An Interview with Uskali Mäki

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This interview is organized in parts. Several years ago, we decided to carry out an interview with Professor Uskali Mäki. The previous President of SIAME prepared questions focused more on the field of methodology rather than the arena of economics. This had to do not only with the strong shared background of both interviewer and interviewee as philosophers, but also because the methodology of economics is a relatively new area of scientific enquiry, with barely half a decade dedicated to its understanding. The answers to these questions can be found in the first section.

The second part focuses on questions arising from economic theory and is followed by issues of methodology and epistemology. The questions were formulated by academics with principally an economics background. These questions may seem rudimentary if seen through the lens of methodology of economics and its syntaxis, as the latter did not form part of core economics. However, thanks to the breadth of knowledge of Professor Maki, it was possible to discuss economics conjectured analysed in the long history of theorizing regarding issues of logic, modelling, fiction and non-fictions, as well as tools to connect theories and non-theories of the very wide arena of what we call the discipline of economics.

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Epistemology matters? For whom? Why?

There is a sense in which epistemology is ubiquitous in our daily lives, including those of practicing economists. We adopt and hold beliefs about various matters, we revise and reject some of them while sticking to others. We judge the reliability of our sources of information, including other epistemic agents as our informants. Indeed, knowledge is a thoroughly social product, and epistemic trust in our informants and the relevant social configurations is essential for knowledge production. We read newspapers and we build on the research results produced by our peers. We rely on various our methods of acquiring information and checking our beliefs. Sometimes we attain our epistemic goals, sometimes we fail to do so.

Now the things above are epistemic matters, and they indeed are an ineliminable part of human life. Epistemology is about epistemic matters, it deals with the general principles that underlie such matters.

We seldom spell out the principles on which we rely when going about in our epistemic lives, but the principles are there and operative, and sometimes it will be useful to make them as explicit as we can. Economists are usually not trained to articulate and justify the principles they rely on, so it is fortunate that we have philosophers and methodologists around to do this for them, and, even better, with them.

Epistemic activity is laden with uncertainties deriving from various sources, and these uncertainties are particularly pressing in disciplines such as economics that deal with dynamically complex subject matters. Good epistemology should be alert to these uncertainties and be prepared to develop principles for managing them. This requires analyzing the epistemic capacities and limitations of the theories and methods used in economics, including the sources of possible error and misjudgement given the ontic properties of its subject matter and the institutional structure of economic inquiry.

Throughout its history, economics has struggled with striking a balance between the goals and expectations of inquiry on the one hand, and the actual capabilities and accomplishments on the other. Whenever the there is a mismatch, disappointment and frustration results, and doubts are cast on the usefulness and scientific status of economics. This is a chronic issue, and there is no other way of settling or relieving it but to engage in a systematic epistemological scrutiny.

What do you think about the perspectives of Feminist Economics? Which aspects of the economic practice (if any) does it illuminate?

This can be taken to mean several different things, and on many interpretations, a conditional case can be made in favour of feminist – or female - perspectives. Perhaps the most innocent, almost trivial, idea is that female perspectives are important for identifying research topics that have been neglected in economic inquiry, but that have enough human or social significance to warrant scientific attention.

It is less innocent to argue that there is some sort of male bias in the contents of economic concepts and theories (such as the concept labour), and that these should be replaced or supplemented by previously neglected contents that manifest female perspectives. This is of course one of many possible biases, be they related to gender, culture, ideology, or academic careerism. Both scientific progress and the appropriate cultural role of science require that such biases be made transparent and be corrected if needed.

One can construe the issue also as a special case of a larger epistemological issue at a fairly high level of abstraction: how do we best guarantee that the knowledge claims made in economics get the facts right? One could appeal to general principles of plurality in defending female perspectives, provided these are relevantly different from other perspectives. These perspectives – whether gendered, cultural, theoretical, methodological, or what have you – should be multiplied (within some limits…) so as to improve the capacity of an epistemic endeavor like economics to be aware of its powers and limitations. *Longino* I see this is related to your next question.

I should note that while I do believe that female perspectives may be important in revealing limitations in standard economic theory, there is a further challenge of being clear about the limits of feminist perspectives. Just as there must be limits to economics imperialism, there are likely to be limits to feminist perspectives. It is likely that not all human action is rational choice in some market, and similarly it is likely that not everything is relevantly gendered. Support to female perspectives does not imply support to over expansionist feminist imperialism. What we need is a case by case empirical investigation into these issues.

Pluralism is a vital demand in many communities engaged in heterodox economics. Do you think it is a relevant subject to be philosophically addressed because of its consequences for the advance of the discipline or is it just an ideological and political demand on the part of marginal groups struggling for better conditions?

It is probably both of these at the same time -- and also in historical sequence. Let me explain.

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First the idea of a historical sequence. I recall the time in the 1970s when new initiatives started emerging in economics, many of them in the form of reintroducing old traditions, ranging from Marxism to Austrianism, from institutionalism to allegedly authentic Keynesisms, and so on. They were often accompanied by claims about superiority, while the more standard versions of neoclassical theory were claimed to be on wrong tracks. These claims manifested the confidence with which non-standard economists attacked the standard views and defended their own approach as the best way forward. These attitudes were not always favourable for tolerant comprehensive pluralism.

This story could then be continued with a gradual deterioration of the initial confidence and the emerging difficulties that the advocates of some of the non-standard groups experienced in finding secure positions in the institutions of economics – university departments, journals, etc. A natural response was to retreat to pluralism: let many flowers bloom, so that our flowers will be safe among others! On this story pluralism would emerge as a strategic move in the defense of endangered schools and traditions in economics.

I said 'retreat to pluralism' as part of the historical story, but from a more systematic point of view, pluralism is no retreat at all, but can rather be construed as a progressive principle that is important for the progress of any scientific discipline. In case we are not prepared to say economics has come to the end of all progress, so has made all the progress there is to be made (except for minor theoretical adjustments, further empirical applications, and technical improvements), and in case we believe that some sort of evolutionary structure would be good for progress, a circumscribed pluralism should be among the principles of good science. A key idea is that variety is essential for evolution – selection presupposes there to be a variety of options from which to select.

'Pluralism' can of course mean a number of different things, and it can be justified in terms of various further principles. When pluralism is promoted, it is not always clear which principles are invoked, and how these in turn can be justified.

So regardless of which groups or approaches in economics would be institutionally safe in consequence of the implementation of pluralism in this sense, it can be defended as a principle that any friend of further ongoing scientific progress should endorse.

Behavioral Economics is now an established field of research. And contributions from the social sciences and the role of ethical issues have been growing in the practice of economics. How do you see the prospects of interdisciplinary work in the field of economics?

This is one of the bigger themes that I have been and will be working on, together with others at TINT. We know that major steps towards scientific progress often derive from interdisciplinary interactions of various kinds. These range from metaphors and analogies across disciplinary boundaries, transfer of model structures to neighbouring fields, utilization

of evidence produced in other disciplines, the

Note that a major variety of interdisciplinarity in economics consists in expanding the domain of its application beyond its traditional boundaries. This is the much disputed in economics of imperialism.

If you had to pick up five new promising issues in philosophy of economics, which ones would you choose?

This is a hard question, both because many of the promising research themes are not entirely new, but may be quite old in fact; and because any such list will have to exclude many very promising topics.

The issue of (un) realisticness is going to remain the number one theme also in the foreseeable future. Unrealistic models with unrealistic assumptions will be created and used by economists, and claims will be made both to the effect that they provide informative insights into the real world and that they disastrously distort important real-world facts. Philosophers of economics will be needed to help economists, their clients and critics to understand the issues and to evaluate proposed solutions to them. This is as old as a research theme can be, and much progress has been made on it recently, but new perspectives can be expected to emerge.

Interdisciplinarity is likewise an old theme, but has now been put on the agenda in new ways, reflecting the intensified developments and debates both within the realm of science and in its applications to resolving complex policy issues. This actually divides into at least three themes. The future of economics is being and will be partly shaped by.

Economic explanation is also an old theme, but we don't seem to know much about it. Curiously, Mark Blaug's The Methodology of Economics (1980) is subtitled "How economists explain", but the book does not deal with this theme at all (as I noted in a paper on economic explanation in 1990). So the challenge now is to take Blaug's promise to the heart. I would say we are now in a much better position to make progress on this topic than Blaug was. Among other things, this is because recently there has been so much useful progress in the philosophical inquiry into scientific explanation.

When economics meets audiences other than practicing economists, such as students and policy makers in teaching and the institutions of economics education, such as in standardized courses, the global textbook institution will necessarily force the use of economics in methodology policy.

My final hunch is that the rhetoric of economics will make a comeback. Since the early 1980s, Deirdre McCloskey and Arjo Klamer made exciting openings, but their project remained

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uncompleted.

As I said, many important issues will be excluded by any list of this length. These include the kinds, sources and uses of evidence simulation.

Naturalism is now firmly established in current philosophy of economics. Don't you think that it jeopardizes its critical approach? What would it mean to be critical of theoretical economic practice for a discipline that is consciously engaged in recovering the rationality of such a practice?

These are of course very large and hard issues, but let me just say a couple of things. First, I am not awfully happy with the phrase, 'naturalism'. What exactly is supposed to be *natural* which the 'ism' is about? It is not the natural of 'natural sciences', nor is it the natural of 'natural state of affairs' or some such.

It is also not always clear what precise idea is intended to be conveyed by the unhappy phrase. I can see too useful but different ideas.

One is doing philosophy of science in an empirical manner, adopting its issues from actual scientific practice, and checking its claims by appealing to evidence pertaining to such practice. Any philosophy of economics done in this way would be empirically informed about its targets and therefore would also be of more interest to practitioners of economic inquiry, ceteris paribus.

The implications for normative assessment are obvious. One needs to know the target of one's criticisms – or perhaps praise - in order to hit it. There has been all too much economics bashing that is not appropriately based on a sufficiently articulated and informed image of its target. This has been counterproductive, resulting in an establishment of a standard strategy of evasion on the part of economists: criticism can be justifiably ignored simply because it is uninformed.

I think all of economics needs to be critically examined all the time, but bad philosophical criticism has done harm both in promoting those standard evasionary practices and in damaging the reputation of the philosophy and methodology of economics.

Another idea behind the unhappy phrase refers to using the contents of scientific theories to inform the contents of philosophical accounts of science. If we take science to be a matter of certain institutionalized ways of using the cognitive capacities of the human brain (or something of that sort), then philosophical accounts of science are appropriately informed by the best scientific accounts of human cognition and social institutions.

As to the implications for normative assessment, here I see the key to lie in plurality and the accompanying pluralism. There is a plurality of scientific resources in any given discipline,

let alone in a multitude of disciplines, and this plurality can and should be utilized for the purposes of informing relevant parts of the philosophy of science. As I pointed out in "Economic epistemology: Hopes and horrors" (2005), economics itself does not convey a single message, but rather numerous possible messages about how science is and should be organized. As you start adding further resources borrowed from, say, anthropology, evolutionary biology, cognitive science, and political science, the various perspectives can be used to supplement and cross check one another. No discipline is perfectly uniform and coherent, and any collection of disciplines is even less so. either clash or reinforce one another

In regard to using economics for informing the contents of philosophical accounts of economics and normative assessments of it, the extra bonus here is that this has the potential of helping to reach the audience of practicing economists and perhaps to engage them in responding to the critical appraisals.

Supposing that the distinction between orthodox and heterodox economics is clear enough, which piece of orthodox economics deserves philosophical criticism? And what would that criticism be?

I don't think there is a clear distinction between orthodox and heterodox economics – and I don't think these are well-understood or always very useful categories. But independently of this, the recent and ongoing crisis provides some obvious pointers.

Regarding the recent turn from a theory-centered to a model-centered epistemology, do you think it's a really promising avenue for the field or just a fad?

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n principle, modelling is an effective way of dealing with complex subject matters, so no wonder it is being used so extensively in a variety of research fields such as ecology, , -- and economics.

If it is fashion, it is largely a useful one. But on the other hand, it is in the nature of fashions that they first come, and then they go. It is hard to see that modelling as a major style of inquiry would go away. New methods and principles of modelling are likely to evolve, but modeling in general is likely to stay. If this is correct, then modelling is not a fashion.

At the same time, it is naturally very important to keep in mind that whatever methods one uses for investigating complex subject matters, the risks of failure are considerable. Progress in modelling – in the methods and principles of modelling and in models themselves – consists in developing more effective ways of acquiring information about the world and in controlling for the uncertainties that are involved. My feeling is that, presently, too little

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attention is paid to identifying and managing the uncertainties. Failures in modelling are sometimes hard to diagnose and to remedy, and therefore a deficient line – or even style – of modelling may survive all too long.

Now modelling is often conceived as an activity of constructing elaborate mathematical systems. This activity is a coin that has two sides. On the one hand, this makes many aspects of models and modelling transparent and controllable; on the other hand, other aspects – more substantial aspects – of models may remain hidden or without sufficient attention, and so their critical evaluation may become harder.

What is the relationship between economic models and reality? What can they tell us about real systems and how they do?

This is of course one of the biggest and most important issues in economics and its philosophy. Unfortunately, it is not among the easier ones. It is not surprising therefore that a variety of views have been offered and that not all of them have tremendous merit.

As an economics student some time ago, I was so deeply puzzled by just these issues that they have shaped my academic career ever since. Here I can briefly highlight just a few key ideas that I have developed.

One idea is that you cannot infer from the falsity of the assumptions of a model to the falsity of the model. In order to establish any connection between them, one first needs to identify the relevant components of a model and to ascertain what functions they serve in any particular modelling exercise. This is why I call my approach a "functional decomposition approach". On this view, for example, some assumptions just have to be false in order to provide useful services for the whole modelling exercise, including pursuing the acquisition of true information about the target phenomena.

Another and related idea is the strategy that was inspired by Alan Musgrave's 1981 paper. Assumptions are not always formulated with full transparency about the intended – or appropriate - claims to be made when using them. The simple formulations one finds in the expositions of economic models often mislead, so they need to be replaced by other formulations that reveal those intentions, or at least what would be appropriate to intend. It is these latter versions that should be assessed for the services they provide for the purposes of modelling. So instead 'transaction costs are zero' we should consider the truth of 'positive transaction costs are negligible for the purposes for which this model is used', and so on.

Yet another idea that I have proposed is that the relevant notion of truth for dealing with models is one of relevant truth. This is a nice way of incorporating pragmatics into my generally scientific realist outlook. I take models to be models-of and models-for (while acknowledging the ambiguity of these expressions). Models are models of some targets, and they are for some purposes. Purposes may be specified in different degrees of detail. We may

distinguish between, say, explanation, prediction, and design as among the possible purposes, but these can be made far more detailed, such as prediction of some variable with a specific degree of precision regarding value and timing, for example.

Relevance is a function of purpose, while truth is a matter of how the model is related to its target. Relevant truth combines the two. A model is not made false by not truly representing all aspects and details of its target; it can be relevantly true if it truly represents just a tiny fraction of those aspects and details, and does this in just such a way that the set purposes are being served.

This is related to one further idea. It is a popular belief that very simple and idealized models are further from the truth than more complex and richer models. On this view, one gets closer to the truth by relaxing idealizing assumptions and by enriching a model by adding further factors and relationships. So de-isolation by de-idealization (some call it "concretization") would be the route to the truth.

My view is that there are so many kinds of truths about the world that can be pursued and achieved that such a conception is all too narrow. I have defended the view that even the simplest and most idealized versions of models may, if they succeed, convey true information about some interesting aspects of the world. A minimal model, as it is sometimes called, might be a true representation of the structure and characteristic way of operation of a causal mechanism.

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