



The field: tasks, pasts, futures

Uskali Mäki

To cite this article: Uskali Mäki (2021): The field: tasks, pasts, futures, Journal of Economic Methodology, DOI: [10.1080/1350178X.2020.1868766](https://doi.org/10.1080/1350178X.2020.1868766)

To link to this article: <https://doi.org/10.1080/1350178X.2020.1868766>



© 2021 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group



Published online: 20 Jan 2021.



Submit your article to this journal [↗](#)



Article views: 246



View related articles [↗](#)



View Crossmark data [↗](#)

The field: tasks, pasts, futures*

Uskali Mäki  ^{a,b}

^aDepartment of Political and Economic Studies, University of Helsinki, Helsinki, Finland; ^bNankai University, Tianjin, China

ABSTRACT

The paper sketches a story about how and why the field of economic methodology / philosophy of economics emerged (as a further step in specialization), and how it has evolved intellectually and institutionally. It considers the field as an institutionalized form of higher-order reflection on the discipline of economics and suggests that such reflection, also in its pre-field form, has recurring triggering conditions (e.g. alleged failures in economics, fundamental controversy, launch of new research style) and functions (e.g. criticism, defense, programmatic statement). It lists topics of inquiry that derive from the concerns economists and others have about the discipline (as a modelling discipline and a policy science). It offers consolation to those who worry about effectively addressing academic economists as the primary audience, suggesting there are other valuable audiences (such as philosophers of science and policy makers). It gives examples of how critical conversation will ensure progress in the field.

KEYWORDS

Philosophy of economics; economic methodology; research field; functions of inquiry; audiences of inquiry; debate and progress

Second-order reflection

Whenever there is some significant, institutionalized, and intellectually intriguing or perplexing human activity in place in society, it is regularly accompanied by a higher-order activity that has the task of reflecting on the lower-order activity. Just think of music, sports, politics or business as such lower-order activities. These activities unsurprisingly are associated with higher-order activities such as music theory and music criticism, public commentary and research on sports and politics, and business journalism and business studies.

Science is another extremely significant institutionalized social activity, sufficiently weighty and puzzling to invite higher-order scrutiny by fields such as history, sociology, psychology and philosophy of science. When we are talking about the philosophy and methodology of economics, we are talking about such higher-order activity.

Just like music criticism and political commentary, the second-order activity of philosophy and methodology of economics seeks to ask and answer questions about the target activity, in this case disciplinary economic inquiry. These are questions that the lower-order activity itself does not customarily or regularly ask and answer in its ordinary mode. Due to this relative lower-order silence there is much in the activity that tends to remain hidden or implicit. It is the task of these higher-order reflections to disclose what is hidden and to make explicit what is implicit, and so to

CONTACT Uskali Mäki  uskali.maki@helsinki.fi

*This paper draws on talks given in Cape Town in 2015, Kyoto in 2016, and Helsinki in 2019. Thanks go to the respective audiences for useful conversations.

deepen and systematize our understanding of the structure and functioning of the lower-order activity. Some of the higher-order activity offers diagnostics and normative assessment of the failures and successes (by whatever normative standard) of the lower-order activity, and some of this is developed into principles and prescriptions for how to do it right, or better. Naturally practitioners of the lower-order activity also occasionally engage in higher-order reflection on their own activities, both descriptive and normative. This has also been the case in economics.

Strata

Within economics, the inclination to engage in higher-order reflection seems to vary with disciplinary stratum (rank, echelon, tier, class, or whatever you want to call it). I am entertaining the idea of the economics profession divided into roughly three categories in this respect.

The inclination tends to be weaker amongst the large and relatively dogmatic “middle class” of economists doing normal science, loyal to the dominant paradigm, the currently conventional way of doing economic research. These economists are busy with their ordinary business of inquiry and education, and have no time nor incentive to do much of anything extra as part of their professional activity, such as systematic methodological reflection.

The inclination is stronger amongst the leading high-rank economists, those with broader visions and less constrained creativity, and often a sense of responsibility for the discipline’s future. This elite group includes many Nobel Laureates and other leading scholars. Just to drop a few names from the recent decades as examples, we can list Hicks, Solow, Allais, Sen, Buchanan, Arrow, Coase, Williamson, Akerlof, Schelling, Vernon Smith, Krugman, Leijonhufvud, Hahn, Hendry, Rubinstein, Thaler, Romer, Rodrik. They typically care not only about their own research and career, but more broadly about the discipline’s ways, and they have the courage and capacity to exercise the concern.¹

The inclination to engage in higher-order reflection also tends to be stronger among the profession’s “underdog” featuring members of schools such as Marxists, Austrians, Post-Keynesians, Old Institutionalists. Their interest rests on two motives at least. They are critical of the “mainstream” of economics in some rather fundamental way, and they need an account of why the mainstream is flawed. And given that they constitute a relatively small minority in the discipline, they need a self-justification that would offer grounds for why their approach is indispensable and why they are doing fine as serious scientific inquiry. Methodological reflection serves both functions.

Note that these categories do not include the specialized activity of philosophers and methodologists of economics. I will let them enter the stage in a moment.

Triggering conditions

The intensity of the activity is not only correlated with the “class” status of economists (and others) but also with the sorts of situation they are in. The higher-order activities are triggered – inspired, stimulated, bolstered, occasioned – in certain types of condition, rather than evenly in all conditions. Many of these conditions often combine in practice, yet it is useful to identify some of the more obvious cases.

A most typical situation is one of *alleged failure or deficiency* of this or that portion of economics, theoretical or practical, or both. These situations often call for diagnosing such failures or flaws in terms of some higher-order principles, and either excusing the failures or using them for demeaning or reforming economics.

Another common situation arises on the occasion of a serious enough *controversy over theories or methods* in economics. Each party as well as spectators of the debate may be incited to invoke and elaborate their favourite general criteria on which one party’s viewpoints emerge as superior and others as faulty.

A dynamic version of this arises when a new initiative is made to *justify the launch of a novel theory or way of doing research* (such as recently experimental economics, behavioural economics, and neuroeconomics). It must be justified as an improvement on old ways or a useful supplement to them, and in any case as scientifically acceptable.

Yet another possible triggering condition is a *struggle over resources*, university positions, research funding, access to prestigious publications outlets, and the like. These may be between disciplines, between schools of thought within economics, or between old and new approaches within the discipline. Higher-order principles tend to be invoked.

Finally, when there are sufficiently serious *issues with the epistemic, practical and cultural authority* of the discipline, the dispute is likely to involve higher-order dimensions. They deal with the justification of the weight and credibility of economics in the public sphere, its impact on world views and policies.

Functions of reflection

Higher-order reflection is not uniform in its goals and functions. Depending on the particular situation in which it arises, the functions may vary. I will list some of the more typical ones; these often overlap and combine in practice.

One is *criticism*. The critic may invoke some methodological standards in terms of which it is then argued that economics is not a science, thus not worth all the privileges that come with this institutional identity; or it, or some part of it, is bad science, thus not a credible source of policy advice, for example. It can be argued to dogmatically stick to flawed theories and methods, to be sloppy in drawing conclusions, and so on. Theories are not falsifiable, assumptions are inappropriately unrealistic, predictions regularly fail, and so on. All this requires setting higher-order standards and applying them to judging lower-order theories and research practices.

Criticism typically provokes *defence*. If not in the form of outright dismissal (e.g. the popular “you don’t know what you’re talking about”), it may set out to contest the methodological arguments used by the critic, or it may lay out an alternative argument that appeals to some other higher-order principles in support of the existing theory or practice under attack. Friedman’s 1953 argument in defence of profit maximization and perfect competition is a paradigm example of the defensive function.

Another common purpose of higher-order reflection is to put forth a *programmatic statement*, an organized set of principles and guidelines telling how to do economics properly or better, typically aiming to canonize an emerging practice guided by those principles as good practice. Recent methodological pronouncements in support of experimental economics, behavioural economics and neuroeconomics have served this function.

More neutrally, one may just aim at *analytical description* of how economics is done, thereby improving one’s (self-)understanding of the discipline. This may be a matter of analysing the structures and presuppositions of research practices in modelling and experimentation, explanation and prediction, controversy and policy advice, and so on. By comparing economics to other disciplines in these respects, progress can be pursued. This is a category that applies to most of current specialized research in philosophy and methodology of economics. I will next finally let it enter the stage.

Emergence of fieldhood

For there to be regular and recognized higher-order commentary, the lower-order-activity typically must be institutionalized. This does not yet determine whether the higher-order activity is institutionalized as well. We may say that once institutionalized, the activity constitutes a *field* of inquiry. Philosophy and methodology of economics (PE&ME for short) today is a field. This was not always the case.

Before the 1970s, most of the higher-order reflection on economics was supplied by practitioners in economics. Indeed, from the very beginnings of economics (or political economy), its major representatives have been active in providing systematic commentary on the nature – “scope and method” – of the science of economics. Just think of Adam Smith, Senior, JS Mill, Marx, Menger, Jevons, Marshall, Veblen, Robbins, Keynes, Knight, Schumpeter, Hayek, Myrdal, Machlup, Friedman, Samuelson. They were generalists whose interests extended widely enough to encompass methodological self-reflection. We know about their higher-order ideas because they were active in developing and communicating them. Yet they did not collectively contribute to a cumulatively progressing field of inquiry as it did not exist.

In the course of the 1980s, a field emerged as a specialized and institutionalized set of experts and activities. A growing network of scholars, in communication and critical interaction, pursued overlapping agendas in higher-order reflection, many of them having this as their main activity – not a minor occasional side activity as in the earlier period. A growing flow of books and articles were published, specialized periodicals were launched (such as *Research in the History of Thought and Methodology* since 1982, *Economics and Philosophy* since 1985, *Methodus* 1989–1993, *Journal of Economic Methodology* since 1993, *Revue De Philosophie Économique* since 1999, *Energeia* since 2000, *Erasmus Journal for Philosophy and Economics* since 2006). Handbooks and companions were published (e.g. Davis & Hands, 2011; Davis et al., 1998; Kincaid & Ross, 2009; Mäki, 2012a). An international organization (INEM 1989) and local centres (such as EIPE in Rotterdam 1997, TINT in Helsinki 2006) were established. A field had emerged and established itself.

Why did the field emerge, why was there this transformation from practitioners’ unorganized side activity to organized and specialized main activity? My hypothesis (publicized in my inaugural lecture in Rotterdam in 1997) is that this was a natural step in the process of increasing specialization. During the long early period, many economists were generalists, able to develop and exercise expertise on, and to contribute to, a broad range of fields and subfields in economics. Some of them used the weakly constrained intellectual space to develop higher-order reflections about the nature of economics. They had the time, incentives and intellectual abilities to do so. Over time, disciplinary constraints grew tighter and specialization progressed further, with the consequence that there was less room – resources and rewards – for activities beyond the narrowing specialisms. Given that economics is quite a significant epistemic institution, it would have been impossible for it to go on without higher-order reflection. Establishing a new research field of specialized higher-order reflection was hence an unsurprising next step in the advancement of division of intellectual labour.²

It seems to me that the intensification of specialization goes quite far in helping explain why many practicing economists are not hyperactive in exercising PE&ME reflection or communicating with PE&ME specialists. There is no special need to invoke their misguided philosophical outlooks or the like.

Who were we?

In the course of the 1970s and 1980s, there was emerging activity in different countries at different scales with diverse degrees of internationally visible output. The Klant Circle (de Klant kring) in Amsterdam and the Club of Critical Economists (KEKLU) in Helsinki, for example, were among the local and internationally relatively less visible collective endeavours. A bulk of internationally significant activity took place at the History of Economics Society meetings in the US; for many of us these conferences provided a regular meeting place, a breeding ground for networking and agenda setting. Popperian and Lakatosian themes were dominant, and they were pursued with little attention given to frontline developments in philosophy of science. Most contributors were employed by economics departments. These included, among others, Larry Boland, Mark Blaug, Neil deMarchi, Warren Samuels, Terence Hutchison, Bruce Caldwell, Wade Hands, Andrea Salanti, John Davis, Jack Birner, and a growing number of many others such as Mary Morgan, Roger Backhouse and

Kevin Hoover. The narrowly methodological viewpoints were challenged by the rhetoric of economics initiative by D. McCloskey and Arjo Klamer, and by broader social studies of science perspectives pursued by people like Bob Coats and Philip Mirowski. Those educated in philosophy of science and employed by philosophy departments were small in number, and they mostly did not share the dominance of Popperian themes. Among these, Daniel Hausman and Alex Rosenberg set out to test and stretch the heritage of logical empiricism; Bert Hamminga imported and applied the structuralist framework of Sneed and Stegmüller; I started moving toward a self-made scientific realism tailored for the peculiarities of economics; there were a few others such as Maurice Lagueux and Margaret Schabas (regarding the approaches of Hausman, Rosenberg and myself, see e.g. Hausman, 2009, 2017; Mäki, 1996, 2009c; Mireles-Flores, 2008; Rosenberg, 2009).

As the field evolved, it has grown far bigger and more international, its gender balance has improved, and its agenda and disciplinary affiliations have expanded.

Labels and affiliations

At the time when the field did not yet exist, it also naturally had no systematically used name. Once the field started taking shape, part of the process was to try to stabilize a label for it, to be used systematically about the new coordinated activity. The label was not only to serve the practical convenience of being able to refer to the activity, but also importantly to fortify and legitimize it. We have also learnt that labels correlate with disciplinary affiliations.

In the early stages of establishing the field, ‘methodology of economics’ and ‘economic methodology’ were adopted (thereby correcting the previous abuse of ‘methodology’ for methods). I think this was partly a rhetorical move in the hope of becoming accepted in economics departments, perhaps of establishing a legitimate subfield *within economics*, next to others like labour economics and microeconometrics. Recall most contributors at that time were employed by economics departments, thus they had their recognition – including employment and promotions – at stake.

‘Philosophy of economics’ suggests another disciplinary affiliation by depicting it as a subfield of *philosophy of science*, next to others like philosophy of biology. This is in line with the development in philosophy of science away from mere general philosophy of all of science and towards its division into subfields each of which deals with philosophical issues in one or another special science. If employed by a philosophy department, the scholar is likely to identify herself as a specialist in philosophy of economics rather than in economic methodology.

Sometimes ‘philosophy of economics’ is used more broadly, to refer to the study of philosophical issues in relation not only to economics but also to the economy. This can be misleading. A philosopher directly investigating the discipline of economics (as in *philosophy of economics*) is often also indirectly addressing issues in the economy, whereas a philosopher directly examining justice, equality, welfare and other philosophical issues in the economy (as in *philosophy of the economy* as we might call it) does not need to (be able to) say anything about accounts of, and research on, those issues by the discipline of economics. ‘Economic philosophy’ might be a more appropriate label for the latter activity (this is a label in use in France). This would be analogous to political philosophy that is not the same thing as philosophy of political science. ‘Economics and philosophy’ is apt for covering the study of philosophical issues in relation to both economics and the economy.

Institutional shifts

Major institutional shifts in PE&ME have unfolded in the last couple of decades or so. One is a shift in disciplinary affiliation from economics to philosophy. This has to do with the educational background and job positions of the practitioners as well as the audiences of their research. A growing proportion of the specialists are employed by philosophy departments and address other philosophers of science, publishing in philosophy of science journals. This shows also in the

use of labels, as a shift from 'economic methodology' to 'philosophy of economics' when practitioners identify themselves. Thus if we ask, by whom and for whom, the shift has been from <by methodologists employed by economics departments for economists and for methodologists and historians of economics> to <philosophers of science employed by philosophy departments for philosophers of science>. This simplifies but captures a portion of actual developments.

A consequence of this shift is the growing requirement of philosophical sophistication (or trendiness of style!) in research as contributors must publish their work in philosophy journals, next to other philosophers of science. Norms and conventions of inquiry alter together with changes in disciplinary affiliation. Yet it seems that nothing very dramatic has happened in this respect, and a strong continuity between contributions with slightly different styles prevails.

It is illuminating to compare philosophy of economics with the rest of the philosophy of the social sciences. Most of the latter is still done by scholars affiliated with various social science departments, while there are not (at least yet) many specialists in philosophy departments working on, say, the philosophy of political science or philosophy of communication studies as distinct sub-fields of philosophy of science. The reasons why philosophy of economics has been welcomed as a subfield of philosophy science may include economics being a relatively more structured and professionalized discipline with high degrees of specialization.³

Another shift has been from mere sets of scattered individuals to the formation of groups and the establishment of longer-lived centres. Even though most of the activity in the field may still be by individuals without the support of a nearby collective, now we also have major concentrations of specialists, such as EIKE in Rotterdam and TINT in Helsinki. The role of such group activities has grown especially in Europe (e.g. The Netherlands, Finland, Britain, France, Spain, Poland) and in Latin America (e.g. Argentina, Mexico). This is paralleled by the activity becoming increasingly international. The importance of cross-country and cross-university networks has also increased together with growing internationalization and specialization.

Multiple audiences

A chronic concern amongst the contributors to PE&ME has been whether they have a receptive audience that is prepared to listen to them. They typically have had a particular audience in mind, namely the one that partly constitutes the subject matter of their investigations: practicing academic economists. Are economists listening? Are they interested in what we do and say? Do they take us seriously? Could we perhaps even make an impact on economics, at least on its self-understanding?

It is obvious that nothing very general can be said about this. Interest in PE&ME varies from one individual economist to another, and at higher levels of aggregation, it varies by field and school and situation; recall what was said above about "class" and triggering conditions. For example, Marxists, Austrians, and institutionalists have, as a rule, been relatively more interested. Young fields in rapid change, characterized by search for novelty and debate, are often more prone to being receptive to PE&ME reflections – just think of recent behavioural economics. In principle, one may expect situations of crisis to be welcoming of PE&ME reflections that provide organized conceptualizations of, and perhaps guidance through, such episodes of relative disarray and perplexity. From this perspective, the post-2008 fiasco was of course disappointing. Many economists started actively philosophizing, reflecting on foundational issues, including the reasons for economics' failure (or reasons for why it did not fail contrary to the appearances) and the need and directions of remedy. They wrote blog entries and engaged in methodological debates as if they never heard of the specialized activity of experts in PE&ME. This was a golden opportunity for PE&ME to establish itself as interesting and useful for practicing economists who were in trouble. For the most part, the opportunity was not ceased, even if there were some pleasant exceptions (such as the symposium in *JEM* 4/2010; and Dani Rodrik's, 2015 book – see *JEM* special issue 3/2018).

Naturally, the PE&ME community should be better prepared for such opportunities. Yet even if there were no or little success in attracting the attention of practicing academic economists,

there would be no reason for despair. It is healthy to be reminded that most practicing economists may not be well equipped to listen and learn – they are too busy and too specialized. The contents and style of PE&M accounts may make a difference for whether academic economists are receptive; for example, a militant or even hostile criticism will not arouse hospitable emotions and welcoming responses, nor will an overly technical study on some esoteric theme with no recognizable connection with the concerns of practicing economists. Importantly, there are other audiences to address and impress. These include economists and others involved in the policy making machinery, philosophers of science and other science studies folks, other scientists, science policy makers, university administrators, journalists, the general public. Even though there is no uniformly sharp dichotomy between the producers and consumers of academic economics, the point is that the consumers also constitute a valuable audience for PE&ME – they have at least a latent interest in what PE&EM might be able to deliver. PE&ME is a natural contributor to the protection of consumer rights by making the discipline's epistemic muscles more transparent.

If a PE&ME specialist has a wish to make an impact on the subject matter of her inquiries, that is economics itself, then of course it would be nice to establish the economics profession as one's *direct* receptive audience and then perhaps additionally to succeed in persuading those practitioners to change their practices. The chronic worry is that they don't listen, and so we remain toothless and practically irrelevant. But that's not the only possible way to have impact on economics. One may also use the *indirect* route by influencing the perceptions about the nature and credibility of economics among other audiences. These must be audiences whose assessments matter for how economics is received and supported, for whether its worldviews and recommendations are accepted and adopted or questioned and rejected.

A final point, and this is a real concern. Audiences also serve epistemic functions. Audiences are potential sources of relevant information and usefully critical viewpoints. In order for PE&ME to get its accounts of economics right, and its possible errors and misunderstandings corrected, a dialogue with insiders of its subject matter would be strongly advisable. It is in the nature of things that practicing economists – or practitioners of any activity – do not always have a perfect understanding (or at least a perfect capacity to articulate such an understanding) of what they are themselves doing, but it is equally obvious that they have insider information that PE&ME experts would neglect to their own peril. In an ideal world, there would be mutually respectful dialogue – or even better, collaboration – between the two groups.⁴

Topics

Of the issues that have been and will be addressed by PE&ME, some arise directly from within the practices of economics, while some others derive from the agendas of philosophy of science – perhaps inspired by the study of some other disciplines. Often it is both, as when the *concerns* come from economics and the *concepts* come from philosophy of science – or if not available, the concepts must be created. Some of the topics persist, while others are a matter of the fashion or urgency of the day – and many of them come and go and come again, in cycles (For surveys of the topics and developments of PE&ME research, see e.g. Hands, 1990, 2001, 2019; Hausman, 2018; Mireles-Flores, 2018).

The themes dominant at the formative stages of the field, those of Popperian falsifiability and Lakatosian progress, manifested the concerns about whether economics meets the standards of science, or at any rate those of decent science; and they were dealt with in terms of concepts and standards imported from easily accessible branches of philosophy of science of the day (or yesterday). The concerns will not disappear, but the philosophical tools for addressing them will evolve; the Popperian and Lakatosian frameworks have been left behind (see Backhouse, 2012; Mäki, 1990, 2008).

Another more specific chronic concern with a long past and a guaranteed future is equally pressing. What to make of all those curious models with utterly unrealistic assumptions, apparently

depicting worlds that are not part of our actual world? A great deal of progress has been made on this, thanks to progress in philosophical concepts, including those of model itself, idealization and the functions of modelling (see e.g. Knuuttila, 2009; Mäki, 2020b).

On top of being a modelling science, economics has always been a policy science. This involves a variety of concerns, including those of simplicity of modelling and complexity of policy, evidence and causation, the roles of values and the positive-normative distinction, and the larger institutional structure within which research contributes to policymaking – in the contexts of various parts of economics, including macro and behavioural economics. The literature on “evidence-based policy” has recently contributed to understanding some aspects of this whole, but more conceptual and empirical progress will occur on an extended range of issues in the multifarious relationship between research and policy. Among other things, I anticipate the study of rhetoric that was active in the 1980s and 1990s will be revived, elaborated, and put to new uses.

In order to understand the performance of economics as a science (or as something else), its modelling practices (their styles, successes, and failures), and its intricate relations to policymaking, one must understand economics as social activity itself. This requires investigating economics as a discipline with an internal institutional and cultural structure that shapes its functioning. This involves looking into the interdisciplinary relations of economics, both comparing it to other disciplines and analysing the enabling and impeding conditions of bringing economics in contact with other relevant disciplines – such as psychology, sociology, ecology – whether in “imperialist” or “non-imperialist” ways. New conceptual tools are needed for addressing these issues.

Progress on these and other concerns will be made by way of conceptual elaboration and innovation as well as by consulting relevant other disciplines for theoretical insight and empirical evidence regarding the social structure of economics and its external relations. Social epistemology will provide some of the resources, but it too will require further enrichment. Economic epistemology – the examination of science in terms of economic concepts and theories, popular a couple of decades ago – had better make a comeback.

Finally, it is important to keep in mind that economics is far from perfectly uniform, thus the issues that arise as pressing vary from micro to macro, from “theory” to policy work, from econometrics to behavioural economics, from trade to labour, from Austrian to Marxist to institutional to evolutionary economics, and so on. One therefore must diversify the problems set and the tools used for addressing them.

Debate and progress

Criticism and controversy are generally considered engines of intellectual progress. There are different conversational styles and strategies, some of them better suited for helping make progress. Let me list a small sample of debates and developments that I have personally been involved in.

My favourite exemplary case has not included any manifest debate proper. It has rather been a matter of respectful refinement by consecutive contributors. This started with Alan Musgrave’s, 1981 sketch of types of assumptions in economic models. This has been followed up with constructive criticisms (with an emphasis on ‘constructive’) and suggested improvements of the typology and of the descriptions of the types of assumptions (such as negligibility, applicability, tractability, early-step). Musgrave himself did not respond in public, but others carried the baton forward for him (see ; Musgrave, 1981; Mäki, 2000a; Hindriks, 2005; Mäki, 2012b).

In a different category, the extended debate over the philosophical underpinnings of the rhetoric of economics project was not the greatest success of convergence to anything close to a consensus. The principles of virtuous conversation turned out to be hard to observe. Yet at least on my own part I felt I made a lot of progress in improving my own understanding of the rhetoric of inquiry and its philosophical presuppositions and consequences (see McCloskey, 1985; Mäki, 1995; McCloskey, 1995; Mäki, 2000b).

Milton Friedman's "Methodology of Positive Economics" (F53 as I dubbed it) has kept serving as a test case for applying and creating philosophical concepts and readings. The field of PE&ME has made progress by suggesting new readings and criticizing others. We have moved from singular readings of it as a consistently instrumentalist statement (Boland, 1981) to plural readings in terms of realism, pragmatism, falsificationism, social constructivism etc, with each of these being challengeable depending on which passages of F53 one wishes to focus on (see Mäki, 2009a; Reiss, 2012; Mariyani-Squire, 2017). Perhaps in due time we can leave the quarrel about the correct reading of F53 behind, and instead use its confusions mostly for educational purposes.

Looking at economics from the point of view of scientific realism is to ask (among other things) whether the discipline – or its parts, such as its models and methods – has a chance of acquiring truthful information about the real world. This has been the undercurrent in my own research. It has been challenged as irrelevant (Hausman, 1998), and an alternative version of realism has been proposed (Lawson, 1999). Resisting these challenges has provided an opportunity to further refine my version of realism, tailored for accommodating the peculiar features of economics (e.g. Mäki, 2000c, 2011).

My idea of models as means of isolation (involving what I call their "experimental moment") has been applied as such, and it has been challenged. Robert Sugden's interpretation of the idea (Sugden, 2000) was corrected and aligned with his idea of models as "credible worlds" (Mäki, 2009b). Till Grüne-Yanoff's suggested refinements and challenges provide welcome opportunities for further elaborations (Grüne-Yanoff, 2011, 2012). The root function of isolation does not exclude serving numerous further functions, such as those of minimal models, benchmark models, how-possibly models, toy models, and so on (Mäki, 2020a; for a broader picture on models and modelling, see Morgan & Knuuttila, 2012).

Economics imperialism, the expansive application of economic models and methods beyond its traditional disciplinary boundaries, is a more recent theme on the PE&ME agenda. I have developed my account of it in critical conversation with other accounts (e.g. Dupré, 2001; Clarke & Walsh, 2009; Davis, 2012; Rolin, 2018). Together with confronting it with empirical cases, this is the best possible way of refining and enriching the account (see e.g. Mäki, 2009d, 2020b; Mäki & Marchionni, 2011; Mäki et al., 2018).

There are many other debates and developments in PE&ME with their own dynamics. They are engines of cumulative progress characteristic of the field – indeed an indicator of there being a field!

Secure future

The field of PE&ME is going to have a secure future for several reasons. Economics won't lose its *cultural and political importance* any time soon, thus it will keep attracting curious attention. Economics and its parts will keep being variously *contestable*, and settling these contestations will require higher-order reflection. Economics will go through minor and major *changes*, prompting foundational issues that are hard to keep track of and decipher without invoking higher-order principles. The interest in economics as a prime and peculiar social science *within philosophy of science* will survive and grow. Any high quality higher-order reflection on economics, abiding with decent principles of conversation and advancement, is bound to have a bright future.

Notes

1. This is partly based on my own experience over the years. It has been remarkably easy and rewarding to communicate with these economists given their willingness and capacity to engage in higher-order reflection.
2. Recently, we have witnessed increasing specialization also within the growing field of PE&ME. When I started reading the literature in 1971, it was possible to read through virtually everything that had been published in the previous two centuries in just a few years. In the next two or three decades thereafter, it was possible to read almost everything of the growing literature that was published. No more today. The field has expanded

and been divided into overlapping sub-fields with different agendas and networks of communication and collaboration.

3. One small indication of this is evident in the composition of the 16 volume set of handbooks in philosophy of science published recently by Elsevier. One full volume was devoted to philosophy of economics next to philosophy of physics, chemistry, biology, cognitive science, engineering and so on.
4. A recommendable and replicable format was applied at an International Economic Association conference in 1996, with six issues in economics addressed by both practitioners in the respective fields of economics and specialists in methodology. See Backhouse et al. (1998).

Disclosure statement

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

Notes on contributor

Uskali Mäki is professor emeritus at the University of Helsinki, directing TINT – Centre for Philosophy of Social Science; and a visiting professor at Nankai University, China. In 1995–2006 he was professor of philosophy at the Erasmus University of Rotterdam in the Netherlands, directing EIPE [Erasmus Institute for Philosophy and Economics]. He is a former editor of the *Journal of Economic Methodology* (1995–2005), and former Chair of the International Network for Economic Method. His current research is mainly on the philosophy of economics, on models, scientific realism, interdisciplinarity, and social aspects of science.

ORCID

Uskali Mäki  <http://orcid.org/0000-0001-9103-1133>

References

- Backhouse, R. (2012). The rise and fall of Popper and Lakatos in economics. In U. Mäki (Ed.), *The handbook of philosophy of economics* (pp. 25–48). Elsevier.
- Backhouse, R., Hausman, D., Mäki, U., & Salanti, A. (1998). *Economics and methodology. Crossing boundaries*. IEA Conference. Macmillan.
- Boland, L. (1981). A critique of Friedman's critics. *Journal of Economic Literature*, 17, 503–522.
- Clarke, S., & Walsh, A. (2009). Scientific imperialism and the proper relations between the sciences. *International Studies in the Philosophy of Science*, 23(2), 195–207. <https://doi.org/10.1080/02698590903007170>
- Davis, J. (2012). Mäki on economics imperialism. In A. Lehtinen, J. Kuorikoski, & P. Ylikoski (Eds.), *Economics for real* (pp. 203–219). Routledge.
- Davis, J., & Hands, D. W. (2011). *The Elgar companion to recent economic methodology*. Edward Elgar.
- Davis, J., Hands, D. W., & Mäki, U. (Eds.). (1998). *The handbook of economic methodology*. Edward Elgar.
- Dupré, J. (2001). *Human nature and the limits of science*. Clarendon Press.
- Grüne-Yanoff, T. (2011). "Isolation is not characteristic of models". *International Studies in the Philosophy of Science*, 25, 1–19.
- Grüne-Yanoff, T. (2012). Mäki's three notions of isolation. In A. Lehtinen, J. Kuorikoski, & P. Ylikoski (Eds.), *Economics for real* (pp. 96–111). Routledge.
- Hands, D. W. (1990). Thirteen theses on progress in economic methodology. *Finnish Economic Papers*, 3, 72–76.
- Hands, D. W. (2001). Economic methodology is dead – Long live economic methodology: Thirteen theses on the new economic methodology. *Journal of Economic Methodology*, 8(1), 49–63. <https://doi.org/10.1080/13501780010022839>
- Hands, D. W. (2019). Economic methodology in the twenty-first century (so far): Some *post-reflection* reflections. *Revue de Philosophie Économique*, 20(2), 221–252. <https://doi.org/10.3917/rpec.202.0221>
- Hausman, D. (1998). Problems with realism in economics. *Economics and Philosophy*, 14(2), 185–213. <https://doi.org/10.1017/S0266267100003837>
- Hausman, D. (2009). Laws, causation, and economic methodology. In Harold Kincaid and Don Ross (Eds.), pp. 35–54.
- Hausman, D. M. (2017). Philosophy of economics: A retrospective reflection. *Revue de Philosophie Économique*, 18, 182–202.
- Hindriks, F. (2005). Tractability assumptions and the Musgrave-mäki typology. *Journal of Economic Methodology*, 13, 401–423.
- Kincaid, H., & Ross, D. (Eds.). (2009). *The Oxford handbook of philosophy of economics*. Oxford University Press.

- Knuuttila, T. (2009). Representation, idealization, and fiction in economics: From the assumptions issue to the epistemology of modeling. In M. Suarez (Ed.), *Fictions in science. Philosophical essays on modeling and idealization* (pp. 205–231). Routledge.
- Lawson, T. (1999). What has realism got to do with it? *Economics and Philosophy*, 15(2), 269–282. <https://doi.org/10.1017/S0266267100004016>
- Mäki, U. (1990). Methodology of economics: Complaints and guidelines. *Finnish Economic Papers*, 3, 77–84.
- Mäki, U. (1995). Diagnosing McCloskey. *Journal of Economic Literature*, 33, 1300–1318.
- Mäki, U. (1996). Two portraits of economics. *Journal of Economic Methodology*, 3(1), 1–38. <https://doi.org/10.1080/13501789600000001>
- Mäki, U. (2000a). Kinds of assumptions and their truth: Shaking an untwisted F-twist. *Kyklos*, 53, 303–322.
- Mäki, U. (2000b). Performance against dialogue, or answering and really answering: A participant observer's reflections on the McCloskey conversation. *Journal of Economic Issues*, 34(1), 43–59. <https://doi.org/10.1080/00213624.2000.11506243>
- Mäki, U. (2000c). Reclaiming relevant realism. *Journal of Economic Methodology*, 7(1), 109–125. <https://doi.org/10.1080/135017800362266>
- Mäki, U. (2008). Method and appraisal in economics: 1976–2006. *Journal of Economic Methodology*, 15(4), 409–423. <https://doi.org/10.1080/13501780802507735>
- Mäki, U. (Ed.). (2009a). *The methodology of positive economics. Reflections on the Milton Friedman legacy*. Cambridge University Press.
- Mäki, U. (2009b). MISSING the world: Models as isolations and credible surrogate systems. *Erkenntnis*, 70(1), 29–43. <https://doi.org/10.1007/s10670-008-9135-9>
- Mäki, U. (2009c). Realistic realism about unrealistic models. In Harold Kincaid and Don Ross (Eds.) pp. 68–98.
- Mäki, U. (2009d). Economics imperialism: Concept and constraints. *Philosophy of the Social Sciences*, 39(3), 351–380. <https://doi.org/10.1177/0048393108319023>
- Mäki, U. (2011). Scientific realism as a challenge to economics (and vice versa). *Journal of Economic Methodology*, 18, 1–12. <https://doi.org/10.1080/1350178X.2011.553372>
- Mäki, U. (Ed.). (2012a). *Handbook of the philosophy of science: Philosophy of economics*. Elsevier.
- Mäki, U. (2012b). The truth of false idealizations in modeling. In P. Humphreys, & C. Imbert (Eds.), *Models, simulations, and representations* (pp. 216–233). Routledge.
- Mäki, U. (2020a). Puzzled by idealizations and understanding their functions. *Philosophy of the Social Sciences*, 50(3), 215–237. <https://doi.org/10.1177/0048393120917637>
- Mäki, U. (2020b). Notes on economics imperialism and norms of scientific inquiry. *Revue de Philosophie Économique*, 21, 95–127.
- Mäki, U., & Marchionni, C. (2011). Is geographical economics imperializing economic geography? *Journal of Economic Geography*, 11(4), 645–665. <https://doi.org/10.1093/jeg/lbq021>
- Mäki, U., Pinto, M. F., & Walsh, A. (Eds.). (2018). *Scientific imperialism: Exploring the boundaries of interdisciplinarity*. Routledge.
- Mariyani-Squire, E. (2017). Critical reflections on a realist interpretation of Friedman's 'methodology of positive economics'. *Journal of Economic Methodology*, 24(1), 69–89. <https://doi.org/10.1080/1350178X.2017.1279743>
- McCloskey, D. (1985). *The rhetoric of economics*. University of Wisconsin Press.
- McCloskey, D. (1995). Modern epistemology against analytic philosophy: A reply to Mäki. *Journal of Economic Literature*, 33, 1319–1323.
- Mireles-Flores, L. (2008). Realism from the 'lands of Kaleva': An interview with Uskali Mäki. *Erasmus Journal for Philosophy and Economics*, 1(1), 124–146. <https://doi.org/10.23941/ejpe.v1i1.16>
- Mireles-Flores, L. (2018). Recent trends in economic methodology: A literature review. *Research in the History of Economic Thought and Methodology*, 36A, 93–126. <https://doi.org/10.1108/S0743-41542018000036A008>
- Morgan, M., & Knuuttila, T. (2012). Models and modelling in economics. In U. Mäki (Ed.), *The handbook of philosophy of economics* (pp. 49–87). Elsevier.
- Musgrave, A. (1981). Unreal assumptions in economic theory. *Kyklos*, 34(3), 377–387. <https://doi.org/10.1111/j.1467-6435.1981.tb01195.x>
- Reiss, J. (2012). Idealization and the aims of economics: Three cheers for instrumentalism. *Economics and Philosophy*, 28(3), 363–383. <https://doi.org/10.1017/S0266267112000284>
- Rodrik, D. (2015). *Economics rules*. Oxford University Press.
- Rolin, K. (2018). Scientific imperialism and epistemic justice. In Uskali Mäki, Manuela Fernandez Pinto and Adrian Walsh (Eds.) pp. 51–68.
- Rosenberg, A. (2009). If economics is a science, what kind of a science is it. In Harold Kincaid and Don Ross (Eds.) pp. 55–67.
- Sugden, R. (2000). Credible worlds: The status of theoretical models in economics". *Journal of Economic Methodology*, 7(1), 1–31. <https://doi.org/10.1080/135017800362220>