

# Mäki on Economics Imperialism

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**Abstract:** This paper reviews Uskali Mäki's epistemic analysis of economics imperialism formulated in terms of the science unification ideal and the three constraints on imperialism he develops. It then examines the phenomenon of 'reverse imperialism' associated with the influence of other fields on economics especially since 1980, and advances a core-periphery model of the identity of economics as a field made up of a collection of different research programs. The discussion returns to Mäki's constraints framework to re-evaluate the 'economics' imperialism of individual research programs, and evaluates what we learn from Mäki's three constraints. The paper concludes with a brief comment on the deductive top-down nature of Mäki's epistemic model, and outlines a more historical approach to the subject of disciplinary imperialism that provides an alternative concept of constraints on research program extensions in terms of the idea of 'resistances and accommodations.'

**Keywords:** economics imperialism, epistemic account, 'reverse imperialism,' core-periphery model of economics

**JEL codes:** B20, B41

## 1. Introduction

Of Uskali Mäki's many excellent contributions to the philosophy of economics, arguably one of the most important is his research on the phenomenon of economics imperialism. Other scholars have also discussed economics imperialism, but whereas this has often focused on particular historical episodes, schools, and individuals, Mäki's attention has been both broader and deeper in his effort to think systematically about interdisciplinarity in the social sciences generally, as well as between the social sciences and the cognitive and biological sciences. Indeed to that end in 2006 he founded and has since further developed a new research program at the University of Helsinki, 'Trends and Tensions in Intellectual Integration' (TINT), which investigates issues of unity and disunity in and around the social sciences in their contemporary context. The guiding principle behind this program is the idea that interaction and communication between different sciences is not a uniform, even sort of process, but one that rather involves tensions and conflicts which can best be studied and understood using such standard philosophy of science and economic methodology concepts as model, explanation, mechanism, and unification.

Recently, Mäki has published a comprehensive statement of what he believes economics imperialism involves, in the process laying out a framework of analysis he has developed over a number of years (Mäki, 2008). This framework deserves careful attention and discussion, especially in our current time when it appears that interdisciplinarity is playing a greater role in the development of economics than it has in the recent past. He also makes an important statement about what this paper is meant to do which serves as something of an invitation to discuss its conclusions.

It is important to understand *what this paper does not seek to do*. It does not offer any historical account of economics imperialism, but is supposed to be compatible with a number of different historical accounts concerned with its origins, actual trajectories, and stages of development. Neither does the paper attempt to explain the phenomenon of economics imperialism, it rather seeks to develop tools that could be employed in its evaluation (Mäki, 2008, 353).

That is, the strategy of the analysis is not inductive, involving building up an account of economics imperialism from a set of acknowledged episodes. Rather, with the goal of producing a wider and deeper analysis, the paper aims to proceed in more deductive fashion to produce an account of economics imperialism that is "*neutral with regard to any particular ideas concerning the contents of any particular economics* that behaves imperialistically (*Ibid.*, 354). Disciplinary imperialism in general, he argues, is a more common sort of phenomenon than many recognize, and we accordingly run the risk of not understanding it, both in general and in connection with economics imperialism in particular, if we build up our account of it from one individual discipline's history – and then often only in terms of one episode in that history. Thus Mäki concludes: "The paper is about the philosophical – rather than economic – foundations of

economic imperialism, or any intellectual imperialism that is inclined toward explanatory expansion” (*Ibid.*).

The invitation this makes, then, is for us to ask how well Mäki’s analysis does when applied to reasonably well-recognized accounts of imperialism, especially of course to economics imperialism, the case he employs. That is, how does his top-down more deductive type of account capture what the historical record might be thought to show? To answer this question I will take another look at the much-debated topic of economics and imperialism, but not in the traditional form of economics imperialism. Rather I look at the subject through the lens of history’s recent twist on the subject in the many transformative effects that other disciplines have had on economics since the 1980s in the development of a whole new set of research programs within economics that bear the imprint of these other disciplines, and have produced approaches in economics that significantly depart from the postwar neoclassical economics paradigm (Davis, 2006, 2008, 2009, 2011). With this further ‘data point’ in mind, I will then ask whether the Mäki deductive type of argument produces a successful account of the phenomenon of disciplinary imperialism.

The discussion is organized as follows. Section two sets out the philosophical framework which Mäki develops emphasizing the three constraints he describes that operate on economics imperialism. Section three reviews a number of conclusions about economics’ interaction with other disciplines I have drawn in connection with my analysis of the recent historical development of the field that might be thought to involve a kind of ‘reverse imperialism’ (from economics’ perspective) or an imperialism of other sciences towards economics. Section four advances a core-periphery model of the identity of economics as a field made up of a collection of different research programs, some in the core and some on the periphery of the field, and uses this to re-describe economics imperialism as an ‘economics’ imperialism associated with the field’s core research programs. Section five returns to Mäki’s constraints framework to evaluate this ‘economics’ imperialism, both forming judgments of the prospects for this reconfigured view of disciplinary imperialism and evaluating what we learn from Mäki’s three constraints. The conclusion here is that in a view of science as being made up of multiple research programs, the constraints Mäki develops are unlikely to ever be satisfied. Section six concludes with brief comment on the nature of the epistemic model Mäki employs to address the historical experience of economics in relation to other disciplines, outlining a recommended alternative approach that broadens this model with a more complicated concept of ‘constraints.’

## **2. Mäki’s epistemic model**

### *The philosophical toolbox*

Mäki’s paper begins with an analysis of the concept of economics imperialism in terms of its epistemic aspects, where this is a matter of how we think about sciences providing explanations.

A foremost consideration when we think about scientific explanation is the issue of scope. John Neville Keynes indeed made the issue of scope central to understanding economics as a science when he emphasized that economics is concerned with certain types of phenomena (Keynes, 1955/1891, 2). More formally, Mäki tells us, we may represent the “scope of theory T as the set of classes of T’s perceived explananda” (Mäki, 2008, 355). Note two things he emphasizes about this characterization. First, it does not imply that the scope of a science is fixed. Rather what is perceived to be included in its “explananda” can change over time. Second, what exactly falls within the scope of a science in terms of its conceptual content is also not predetermined, as is reflected in the changing definitions of the science of economics over time. These are important points, because they allow for sciences to change in what they are about over time, something we know occurs from the history of science. They are also important because they allow for the possibility that a science could be imperialistic and undergo change in the process. Clearly this makes it more difficult to speak about imperialism since not only might imperialistic extensions be ‘captured’ and transformed by the disciplines they invade, but this conceivably could also have transformative reverse effects on the ‘imperialistic’ discipline. I return to this possibility below.

Mäki, then, further sharpens his view of explanatory scope with the concept of consilience. The concept was adopted by the Cambridge philosopher William Whewell (1847), and has more recently been central to the arguments of the American sociobiologist Edward O. Wilson (1998). Roughly speaking, consilience is a unity of knowledge idea that counterbalances the virtue of a theory simply having wide scope. It is not enough that a theory pertain to many sorts of phenomena; a theory also needs to be able to equally explain different kinds of phenomena, or bring them into a single explanatory framework. Mäki emphasizes both the idea of simplification, in the sense that a single theory successfully explains many different things, and the idea of theoretical efficiency, in that such a theory is parsimonious in its explanatory capacity. Under the banner of unification, many practicing scientists and philosophers of science have agreed that the idea of consilience is central to the project of defining individual sciences (e.g., Friedman, 1974; Kitcher, 1981).<sup>1</sup> A science is distinguishable as a distinct intellectual endeavor or independent domain of investigation if it holds together as a (relatively) simple unity of statements about any phenomena. Economic methodologists and philosophers of science who follow the influential scientific research programs approach of Imre Lakatos (see Lakatos and Musgrave, 1970) have used a similar notion – the idea of a theory having a ‘hard core’ which effectively unifies it vis-à-vis other research programs with different hard cores. In the next section I apply the general idea of explanatory unification to explain economics as having both core and periphery dimensions, with the latter determining the boundaries that distinguish economics from other sciences.

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<sup>1</sup> As Mäki points out, it remains controversial among philosophers of science whether explanation and unification should be thought to be tightly linked (Mäki, *Ibid.*, 361; see for example, Kincaid, 1996).

One further aspect of consilience also gets attention from Mäki. Demonstrating consilience is closely related to generalizing a theory across different kinds of phenomena. Yet continued successful exercise of theory generalization often has subjective effects on scientists in that they come to expect further successful generalization of a theory in connection with phenomena where the theory was previously not thought to apply. In effect, scientists become optimistic about a theory's expanding scope, and cease to be surprised by its unexpected reach. This forward-looking posture can be termed full consilience. It is particularly valuable for the topic of imperialism (economics or other fields), because that sort of theoretical movement to other disciplines is very much a matter of having confidence in the successful unexpected application of a theory to new phenomena. Note that this links up with Mäki's general treatment of scope as referring to no specific or fixed content. With this in mind, then, let us turn to his treatment of economics imperialism.

### *Expansionism and imperialism*

Here, using the full consilience idea, Mäki makes an important distinction between economics expansionism and economics imperialism, where the latter is a special case of the former specifically involving pursuit of full consilience. Thus we may associate economics expansionism primarily with the simple pursuit of consilience, that is, "a persistent pursuit to increase the degree of unification provided by an economic theory by way of applying it to new types of phenomena" (Mäki, 2008, 359). In contrast, economics imperialism motivated by the goal of full consilience occurs when the unification of a theory is pursued with regard to phenomena which are "located in territories that are occupied by disciplines other than economics" (*Ibid.*, 360). That is, economics imperialism is the product of a changing view on the part of economists regarding the discipline's scope brought about by increased confidence regarding its generality that is productive of a new belief that the theory can also explain altogether new phenomena not within its customary scope. An intermediate case is what Mäki terms non-imperialistic economics expansionism: "expansionism where the new types of explanandum phenomena are located in unoccupied territories, that is, territories unoccupied by disciplines other than economics" (*Ibid.*). Of course whether or not a conceptual territory is unoccupied can be disputed. In any event the more important distinction is between economics expansionism and economics imperialism, and particularly important here, as this third case shows, is whether different disciplines have staked out responsibility for certain domains of phenomena, a matter that Mäki emphasizes is historically and socially contingent upon the way in which fields may happen to have developed.

How, then, are we to see the field of economics, particularly in connection with widely agreed upon examples of the extension of neoclassical economics in the postwar period? Taking Gary Becker and James Buchanan as central figures, they seem to have been largely engaged in economics imperialism in that the domains they addressed were indeed mostly occupied by other sciences and fields. They also appear to have both been motivated by the ideal of theoretical unification, and so it is fair to attribute the goal of full consilience to them. But Mäki's

framework includes a further dimension, namely, that pursuit of theory unification through economics imperialism is always subject to constraints. Three are explained.

### *Constraints*

The first constraint is ontological. To explain it Mäki makes a distinction between two types of unification: derivational and ontological.

Derivational unification is a matter of deriving large classes of explanandum sentences from a parsimonious set of theoretical sentences or inferential patterns. It is based on the derivational capacities of theories. Explanations are construed as arguments. Theories are regarded as logical formulae, possibly devoid of truth-value, serving the task of generating implications and saving the phenomena (*Ibid.*, 363).

Derivational unification, then, largely puts aside whether the extension of a theory actually applies to the world. Rather the emphasis rests on what the concepts and principles of the theory can be interpreted to explain in connection with new phenomena, whether or not those phenomena actually have the character ascribed to them by that theory.

Contrast this to ontological unification.

Ontological unification is a matter of redescribing large classes of apparently independent explanandum phenomena as forms or manifestations of a common system of entities, causes, and mechanisms. It is based on the representational capacities of theories in depicting such underlying systems. Explanations are construed as descriptions of the order of things, or goings on, in the world. Theories are regarded as purportedly true pictures of the simplest mechanisms and processes of the world's workings; phenomena are regarded as manifestations thereof (*Ibid.*, 364).

Here more is involved than in the case of derivational unification, since the elaboration of a theory's theoretical sentences or inferential patterns must address a further factor, that is, whether this also succeeds in capturing "the order of things, or goings on, in the world." Clearly this conception of unification asks significantly more of an imperialist project, since different sciences may be distinct in part because the domains they address involve different orders of things and different kinds of mechanisms and processes.<sup>2</sup> Thus Mäki argues that ontologically grounded economics imperialism is justified while derivationally grounded economics imperialism is not, essentially because the former meets a higher standard.

The second constraint on economics imperialism is a pragmatic and axiological one, and is based on comparing the relative consilience of theories in different disciplines. Consider the following two notions when we compare the consilience of two different theories: subsumption and

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<sup>2</sup> This view is expressed, for example, by Cartwright (1999).

cardinality. In the first case, one theory is more consilient than a second if the second is a proper subset of the first. In Mäki's view, pragmatic constraints typically do not arise here. In the second case, however, one theory is said to be more consilient than a second if the cardinality of the set of classes explained by the first is greater than the second.<sup>3</sup> Mäki's argument here is subtle. While comparing theories' consilience by subsumption is relatively straightforward, comparing theories' consilience by cardinality invites pragmatic constraints in the form of judgments of significance. In effect, we must place weights on the different classes of phenomena we count as being explained by a theory, because some classes concern relatively unimportant matters, even if they are explained by the theory (such as ordinary household chores in explanations of social revolutions).

This becomes yet more complicated when we distinguish between static and dynamic notions of consilience, and further consider the relative rates of expansion of scope of theories over time. Here the need for pragmatic judgment arises in connection with our ideas of progress, since we sometimes need to compare theories making slow progress with respect to significant facts and theories making fast progress with respect to insignificant facts.<sup>4</sup> Thus the general conclusion is that economics imperialism faces yet another constraint with respect to consilience by cardinality. We must accordingly make pragmatic judgments regarding whether a particular economics imperialism exhibits progress according to independent standards of what we think significant.

Finally, the third constraint Mäki sees operating on economics imperialism is what he calls the epistemological constraint. Since we are concerned with the epistemic character of economics imperialism, we always must ask ourselves whether the claims a theory's proponents make to having explained certain phenomena can be regarded as being confirmed. The difficulty is that confirmation is a complex determination involving many different sorts of considerations. This is reflected in the classic Duhem-Quine problem where a failed test cannot be clearly attributed to the hypothesis at issue, and may be due to any number of issues associated with the auxiliary assumptions made in formulating the test. The moral is that scientists need to be circumspect about the element of uncertainty attached to their claims of confirmation. This constitutes the third important constraint on economics imperialism, especially as imperialism, by comparison with expansionism, involves the more ambitious goal of full consilience, where there seems to be greater opportunity for ambiguity in auxiliary assumptions.

### *Judging economics imperialism*

Where do we find ourselves, then, with this overall framework in place? Mäki's conclusion is that an economics imperialism or indeed any sort of disciplinary imperialism may be justified, but only if we can say that the three constraints he explains are satisfied. If these constraints are

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<sup>3</sup> Cardinality concerns the count or the number of kinds of phenomena that are at issue.

<sup>4</sup> The recent success of Levitt and Dubner (2005) comes to mind.

not fulfilled, we have – what he calls economic imperialism\* – a practice that is hegemonic and arrogant.

This conclusion, then, marries positive philosophical analysis with normative recommendation. As indicated at the outset, it involves a deductive top-down type of argument that generates reasonable principles of analysis, and then proposes their application to particular imperialist episodes. Mäki highlights this in his conclusion. He also allows that his constraints analysis in particular may set too high a barrier, and that “economics imperialism is not given a chance” (*Ibid.*, 377). Thus let us apply the framework to see if it both explains and makes judgments appropriate to economics’ recent experience with a kind of ‘reverse imperialism’ from other disciplines.

### **3. Applying history to philosophy**

#### *Imperialism and ‘reverse imperialism’*

The economics imperialism that has been most widely discussed is that associated with Gary Becker’s extension of rational choice theory to such non-market topics as household behavior, discrimination, crime, and education and James Buchanan and Gordon Tullock’s public choice school similar extension of rational choice theory to political and governmental institutions. These extensions originated in the 1970s, and are still active research programs within economics. Economics since the 1980s, however, has also seen the development of a whole new set of research programs – including game theory, behavioral economics, experimentalism, neuroeconomics, complexity, evolutionary economics, and the capabilities approach – that originated by drawing on other disciplines, producing approaches that significantly depart from the standard neoclassical economics paradigm (Davis, 2006, 2008, 2011). Game theory comes from mathematics, behavioral economics comes from psychology, experimentalism is standard throughout science, neuroeconomics was first conceived of by neuroscientists, complexity modeling in economics was stimulated by physics, evolutionary thinking comes from biology, and the capabilities approach shows the influence of philosophy. Thus when we consider both historical episodes, the relation between economics and other disciplines appears to be a two-way rather than one-way street with influences moving in both directions.

This, however, considerably complicates our picture of economics, since it shows that the field is not a monolithic system distinct in nature from other fields, but is rather made up of a collection of heterogeneous elements or even relatively independent research programs, some originating in economics and some developing within economics by drawing on other fields and disciplines.<sup>5</sup>

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<sup>5</sup> Whether research programs that are perceived as native to economics are indeed so can also depend on how far back one goes in history. Thus marginalist analysis, which most would say is native to economics, has been argued to originate in nineteenth century physics (Mirowski, 1989). Given this qualification, in the discussion here I treat



That is, economics is not all economics; economics includes conceptual elements that have not originated in economics, and are generally seen as representative of other fields. The status of these imports is of course difficult to assess. Some may be thought to have been effectively ‘domesticated’ in the sense that they have been shaped to accommodate the characteristic concerns of economics, and have thereby lost their other-science imprint. For example, game theory analysis of strategic interaction, which originated in John von Neumann’s mathematical theory of parlor games (cf. Leonard, 2010), has been redefined in economics as equilibrium analysis in the absence of competition. But others may retain influences from their originating disciplines that conflict with economics’ dominant approaches, thus changing economics to more resemble other fields. For example, laboratory experimentalism, which has long been a means in many sciences for testing and evaluating theoretical assumptions, was rejected as a legitimate procedure in economics until recently but has now been adopted by many economists, thus changing the nature of economics rather than the reverse (cf. Smith, 1989).

It follows, then, that it is not easy to say when economics *as a whole* is imperialist, since on net it could well be the case that ‘reverse’ imperialism incursions into economics from other fields are as significant as the incursions economics had made into other fields. Perhaps a fall-back position would be to argue that economics as a whole could be thought imperialist when its impact on other fields was believed to be greater than their impact on economics. But surely even this would be a difficult if not risky sort of judgment to make since it would require assessing the impact, meaning, and significance of qualitatively different ideas across qualitatively different sciences. How is one to measure greater or lesser impact in such circumstances?

To complicate things further, to make such an argument we would also have to be clear on the native economics pedigree of successful incursions from economics into other fields, that is, whether ideas in economics exported to other fields have had a long tenure in the field or are the result of recent imports from other fields. Consider a hypothetical example of contemporary relevance. Suppose a case could be made that the most successful incursions from economics into other fields in recent years can be associated with the extension of behavioral economics reasoning. Behavioral economics is a subfield within economics based on the application of psychology to the theory of choice behavior. One could argue, then, that the rise of behavioral economics as a subfield in economics is the result of economics’ domestication and framing of its (perhaps selective) imports from psychology, in which case this would be an example of economics imperialism. But one could alternatively argue, were one to believe that economics that has been transformed by its imports from psychology, that rather than being an example of economics imperialism this should rather be seen as a case of a two-stage or indirect route type of psychology imperialism. Thus even the nature of recognized incursions from one field into

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long-standing research programs in economics as native to the field, where ‘long-standing’ in the current context can be restricted to the period from the late 1930s when neoclassicism became dominant in economics.

another are subject to interpretation and dispute. Is it economics producing incursions into other fields or is it other sciences indirectly producing these incursions?

### *Expansionism and imperialism*

We can see this same problem of pedigree in another form if we consider the difference between imperialism and expansionism – whether the extensions of a field are into occupied and contested or unoccupied domains – but now address how this difference may play a role in making economics a heterogeneous rather than monolithic discipline. One way to interpret heterogeneity in a field is in terms of the variety of its research programs. Research programs may acquire a relative independence in terms of the particular questions they address, the key publications relied on, methods of investigation, and view of the real world subject matter they are meant to explain, and yet they may still share principles and ideas with other research programs in the same field. There then might be said to be two dynamic models regarding how this status of relative independence comes about. For comparison let us first focus on changes internal to a given discipline. Thus a research program could acquire its relative autonomy by taking over and transforming an existing relatively independent research programs. For example, financial economics was formerly a more institution-driven type of research program, and then became more a neoclassical form of analysis with the adoption of the efficient markets hypothesis. This might be a case of an imperialism that is internal to the field of economics operating at the level of research programs. Alternatively, research programs could acquire their relative autonomy in an expansionist manner by creating new sub-fields in formerly unoccupied domains within a discipline. The classic example here is Keynesian macroeconomics which in the 1950s developed in a previously unoccupied domain, aggregative relationships in the economy as a whole. This would then be a case of expansionism internal to the field of economics.

Let us turn, then, to the emergence of research programs within a field as a result of incursions from other fields. On the distinction above, those new fields may consequently result from either other fields' expansionism or imperialism. For example, behavioral economics could be interpreted either way, that is, either as a contested take-over of the standard axiomatic choice theory research program in economics by psychology or as generating a new formerly unoccupied psychology-based field alongside the traditional choice theory research program.<sup>6</sup> It is the latter state of affairs and the phenomenon of expansionism that I wish to emphasize, then, because of the further insight it offers us into the heterogeneous non-monolithic nature of a field. The point is that the occupation of formerly unoccupied space within a field allows for a relatively intact other-science presence within that field. This may be the end of the story, or a similar outcome might be that the emergence of a new other-science-based research program

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<sup>6</sup> Thus some behavioral economists talk more about the transformation of economics (e.g., Thaler, 2000), while others (e.g., Rabin, 1998) talk more about the compatibility of psychology and economics.

might lead to the emergence of a parallel non-competing, native research program aimed at providing the discipline's own view of that new field, in which case a formerly unoccupied domain would be occupied by two parallel and relatively non-communicating research programs.<sup>7</sup> This scenario envisions circumstances in which other sciences can take up largely independent residence within economics, pursuing their own other field-based research strategies, in the process making it ambiguous what the reference of 'economics' is. Then, even particular episodes of economics-type imperialism associated with a particular economics research program (such as the Chicago school) cannot be labeled 'economics imperialism,' since 'economics' could be inhabited by other research programs uninterested in and perhaps hostile to this particular economics research program's imperialism. In effect, once successfully inhabited by subfields with other science origins, it ceases to be clear what justifies any particular research program's designation as 'economic' imperialist.

The arguments above based on further attention to the recent history of economics therefore show there to be two problems with the concept of economics imperialism. We are neither easily able to say that economics on the whole is imperialist when we add in other fields' reverse imperialism toward economics, nor are we able to say that agreed-upon episodes of imperialism are evidence of 'economics imperialism' when the reference to that expression comprehends evidently non-imperialist research programs. What this then implies is that we need to explain interdisciplinarity and imperialism in particular at a lower level of aggregation than entire fields or sciences. Mäki is surely right to regard Becker and Buchanan as imperialistic toward other disciplines, and given that they are key representatives of relatively independent research programs, it follows that these research programs ought to be seen as imperialistic as well. But if this is uncontroversial, what are we to say about entire fields? Having disaggregated economics into a collection of relatively independent research programs, and having questioned the designation of the term 'economics,' we seem to be left without a view of the identity of a discipline, which is not consistent with the fact that we generally distinguish between entire disciplines. Indeed, the entire discussion of interdisciplinarity presupposes that we can somehow distinguish between disciplines, even if they are recognized as having heterogeneous contents.

To address this problem, in the next section I briefly lay out a structural view of what makes economics a single discipline, and then use this in the following section to discuss interdisciplinarity and imperialism, revisiting the three constraints on imperialism that Mäki develops.

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<sup>7</sup> Chris Starmer (2000) describes a parallelism situation like this in which evidence contradicting rational choice theory's independence axiom led to a new set of 'conventional' rational choice theories minus the axiom that were developed principally by economists and a set of nonconventional choice theories approaching choice in terms of reference points based on the work of psychologists Daniel Kahneman and Amos Tversky. Though both subfields are in economics, there is little communication between them.

#### 4. Imperialism in a core-periphery model of economics

The model I set out previously (Davis, 2008) transfers the general idea behind Lakatos' hard core/protective belt account of individual research programs to entire fields or disciplines (also cf. Boumans and Davis, 136-8). The simple idea Lakatos advances is that the identity of an individual research program may be understood in terms of its core and periphery elements, where the former is stable and unchanging and the latter is continually adjusted to accommodate explanations of phenomena in terms of that set of core elements. When we apply this to entire disciplines made up of collections of different research programs, we may then take a subset of these programs as occupying the core of the field and another subset of them as occupying its periphery. In the postwar period, neoclassical partial and general equilibrium modeling with constrained optimization analysis has constituted a core research program, joined in the 1980s by the game theory research program which investigates strategic interaction without using the price equilibration framework. These research programs can be further divided into a number of sub-research programs which address particular aspects of price equilibration and game interaction, but all such programs taken together constitute the core of economics that is generally referred to when economics is distinguished from other disciplines. At the same time, there are a variety of research programs in economics that compared to these programs occupy the periphery of the discipline. In contrast to Lakatos' view, these do not function as a protective belt where core principles and concepts are explored in applications. Rather these research programs are peripheral in the ordinary meaning of the term; they pursue questions and issues removed from core concerns and often at odds with core assumptions. In addition, they help sharpen the identity of the field in terms of core programs making it clear what in economics is not part of the core.<sup>8</sup>

Among these programs in the early postwar period up to 1980 are those traditionally labeled heterodox (as compared to the designation core programs have as orthodox): old institutionalist economics, Marxist and radical economics, Post-Keynesian economics, Austrian economics, social economics, feminist economics, ecological economics, and others. Since 1980, however, behavioral economics, experimentalism, neuroeconomics, complexity, evolutionary economics, and the capabilities approach have all become active research programs in economics, though like the traditional heterodox research programs they depart in important ways from the principles central to the core research programs that characterize the field. They thus now also occupy the periphery of economics (despite the fact that many economists working in these

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<sup>8</sup> I argue in Davis (2008) that this opposition between core and peripheral research programs plays an important role in setting the boundaries of economics from other disciplines. Since the peripheral research programs often share conceptual elements and principles with other disciplines, their place on the periphery of economics makes clear what core economists believe is not economics. The intensity of the opposition between orthodox and heterodox economics in the early postwar period can accordingly be attributed to an especially strong desire on the part of economics profession to define its difference from other social sciences.

programs have prestigious university positions, and are sometimes accordingly referred to as ‘mainstream’<sup>9</sup>).

This core-periphery framework, however, is a dynamic one, and as the history of economic thought demonstrates what counts as core and periphery research programs has changed over the history of the discipline, thus implying that the identity of the discipline as associated with its core research programs also changes over time. A distinct possibility, then, is that a discipline’s change in identity is the result of the effects of expansionism/imperialism on the part of other disciplines towards it. Consider two cases. First, game theory, with its origins in mathematics, now occupies the core of economics. Since the form of analysis of strategic interaction it involves is quite different from the form of analysis of strategic interaction in more traditional market-based oligopoly theory (Cournot and Bertrand type models), and since economists still employ those older types of models, it seems fair to say that here we have an example of parallelism, or the presence of two similar but largely non-communicating research programs, one with origins outside of economics and the other with native origins. This consequently is a case of mathematics expansionism vis-à-vis economics. Second, and in contrast, behavioral economics, on some interpretations is seen as rival to standard axiomatic choice theory, and as having developed so as to replace it. At the current point in time, though it seems to still be a research program on the periphery of economics, its place in much recent research makes a case for behavioral economics occupying the core of the field in the future. Were this to occur, this would be a case of psychology imperialism. Thus considering game theory and behavioral economics in a core-periphery model of economics, we see that the field can change its identity over time as a result of the impact of other disciplines upon it.

With this in mind, then, we can return to the subject of economics imperialism – that is, ‘economics’ imperialism – where this is a matter of the imperialism of individual research programs in economics. With the core-periphery distinction, it is possible for an ‘economics’ research program imperialism (or expansionism) to emanate from periphery programs,<sup>10</sup> but since the ‘economics’ imperialism of Becker and Buchanan is commonly associated with the field’s dominant principles (such as the *Homo economicus* assumption), and since these are assumptions of the field’s core research programs, I address imperialism in this more familiar connection. Returning to Mäki’s constraints analysis, then, how ought we to now look at ‘economics’ imperialism?

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<sup>9</sup> See Colander, Holt, and Rosser (2004) for a discussion of the ‘mainstream’ as a sociological rather than doctrinal or conceptual category.

<sup>10</sup> Thus Lawson (2009) suggests that heterodox economists ought to build bridges to other disciplines. This could be seen as expansionist or even imperialist.

## 5. ‘Economics’ imperialism

Recall that Mäki’s approach is an epistemic one in which we evaluate imperialism/expansionism in connection with extension of a theory’s scope and increased consilience. Also, full consilience reflects a change in subjective view on the part of the scientist, whereby the expectations are raised regarding the further extension of the theory in application to subjects and domains where it was not previously thought likely to apply. However, the impulse to expand a theory also faces the ontological, pragmatic/axiological, and epistemological constraints. To address how each of these constraints applies when we focus on a single core research program’s ‘economics’ imperialism, I emphasize in connection with each constraint the consequences of this core program’s being contested by rival or parallel research programs within economics.

The ontological constraint, then, is a matter of generating a unification of a theory across a wider range of phenomena, not simply in a derivational way showing how the logical formulae of the theory could be applied to new phenomena, but in an ontological way where the theory is shown to increasingly represent the “the simplest mechanisms and processes of the world’s workings” (Mäki, 2009, 364). Consider Becker’s Chicago School research program’s imperialism in regard to explaining behavior within the non-market setting of the family (Becker, 1973, 1974). The main argument is that people are rational maximizers, and given different human capital resource endowments across women and men, the sexual division of labor in the household explains in rationality terms the traditional division in which women bear primary responsibility for household labor. Is this analysis likely to capture “the simplest mechanisms and processes” in the workings of households? An early rival game theory account of the household division of labor (Manser and Brown, 1980) uses bargaining theory in cooperative games with imperfect information to produce a rather different view of “the simplest mechanisms and processes” involved. Thus in an economics made up of different research programs, Becker’s imperialist proposal is contested ontologically within the field before we even consider its ontological credentials outside of economics. This not only means, as Mäki argues, that the merits of this particular imperialist project are disputable, but it also means in an understanding of a discipline made up of many research programs, some rival and some just parallel, it is unclear whether any particular research program could ever satisfy the ontological constraint.

The pragmatic/axiological constraint particularly applies when in comparing the relative consilience of two theories we find it necessary to make judgments of significance regarding the weight we place on the classes of new phenomena explained. Some of these new classes of phenomena may be perceived as significant, others as insignificant, and this need to justify significance acts as a constraint on the claims of an imperialist program. Consider, then, the *Freakonomics* book by Levitt and Dubner, another example of the Chicago School’s research program imperialism. The popularity of this book might be thought to imply that its extension of rationality reasoning to a whole variety of phenomena previously little discussed by economists – cheating by sumo wrestlers, selecting names for children, information control in the Ku Klux Klan, etc. – signals a relative success in consilience terms vis-à-vis other research programs in

economics that do not emphasize rationality, such as behavioral economics. But the behavioral economics research program has also turned in an imperialist manner to explaining new sets of phenomena – whether cafeteria managers ought to place desserts last, how pension form applications ought to be written, etc. – in a way that attracted attention in Richard Thaler and Cass Sunstein’s popular book *Nudge* (Thaler and Sunstein, 2008). How are we to compare the relative significance of these two parallel sets of applications when the case either one makes is offset by the other? Mäki is clear that the pragmatic constraint is a difficult one to satisfy. But when economics offers multiple research program imperialisms, it seems all the more difficult to satisfy.

Finally, recalling the Duhem-Quine problem, the epistemological constraint requires that we always be circumspect about the difficulties associated with claiming theories are confirmed when they include many auxiliary assumptions. This is particularly a difficulty when we think in terms of full consilience, since there the subjective posture of the theorist changes, perhaps encouraging imperialists to abandon the caution they previously exercised in seeking to apply their theories and models where they formerly thought they did not apply. Note, then, that when we see a discipline as being made up of many research programs, differences between these programs are often a matter of auxiliary assumptions. For example, some economists reject laboratory experiments as a means of investigation because they do not believe that controls can be put in place in the lab on human behavior. This issue, however, often turns on ambiguities connected to the framing of protocols given to experimental subjects. Thus whether laboratory experiment results are meaningful could be a matter experimental design. Mäki is surely right, then, that the confirmation problem and the epistemological constraint it produces is a serious problem for imperialist strategies. What we may add to this is that a plurality of different types of research programs within a discipline can increase the role auxiliary assumptions make in theories, since research programs typically make different kinds of auxiliary assumptions in developing their theories and models. Thus Mäki’s epistemological constraint seems yet even more binding.

## 6. Concluding comment

The conclusion of the last section is that when we look at economics as being made up of many different research programs, Mäki’s three constraints on disciplinary imperialism – or rather on individual research program imperialism – seem unlikely to ever be fulfilled. Thus imperialism is never justified, or what I have termed ‘economics’ imperialism is always what he calls an “Economics Imperialism\* ... [an imperialism] based on an *economics hubris*” (Mäki, 2009, 375). But this seems too strong a conclusion. Though the idea of ‘imperialism’ is naturally offensive to many because of its political connotations, it nonetheless seems right to say that if a theory occupying a particular domain is not a good theory, then better theories that aim to take over that domain ought to be promoted, whether this occurs in an interdisciplinary setting or

within disciplines. Thus we seem to still need to be able to explain how a science or research program expands at the expense of another. Alternatively, then, I suggest that the strategy of approaching imperialism in terms of the idea of constraints on extensions of a theory may itself be problematic, and that accordingly we may need another sort of epistemic model to explain growth and conceptual change in science. Let me briefly suggest a rationale for one different sort of approach by reframing Mäki's approach.

Mäki's epistemic model – based on a theory's consilient extension of scope subject to constraints – can be seen to be a version of instrumental rationality's constrained optimization algorithm. Imperialist/expansionist sciences or research programs essentially seek to maximize an objective function that functions formally much like a utility function. Just as we can say that a single unique utility function exists when the preferences that underlie it are well-behaved (or exhibit completeness, reflexivity, transitivity, and continuity), so the unification ideal similarly rules out conflict between a science's hypotheses and theoretical initiatives – effectively makes certain they are well-behaved – that would preclude its representation as a single consistent project. But if one cannot postulate that a science or research program effectively operates with a single objective function – for example, the 'maximization' of the rationality hypothesis in the Chicago School research program – then the whole language of constraints becomes problematic. That is, if there are many competing initiatives within a science or research program, then any given constraint would affect them each differently, and this would have mixed effects on the science or program taken as a whole.<sup>11</sup>

A view alternative to this one is that any science or research program operates with many goals, some or even many of which may be inconsistent with one another, so that development of a science or research program is subject to changes in direction and sometimes apparent reversals as the priority of these different goals changes over time. This, for example, seems to be shown by the longer history of the Chicago School economics research program (Emmett, 2010). How might we understand changes in priority of a science or research program's different goals? One way to proceed is to modify Mäki's concept of a constraint to represent sciences and research programs as dynamically managing a changing environment of shifting barriers – what we might call a "dialectic of resistance and accommodation" (Pickering, 1995). In this way, constraints are seen instead as provisional barriers that may be circumvented and navigated rather than always count as clear limits on a science or research program's development.

This, however, is not the place to begin systematically investigating this proposal, and thus I will close by simply suggesting how this model might have particular advantages for explaining the development of science when we represent disciplines as being made up of many heterogeneous research programs. Thus, as all inhabiting one discipline, the members of a collection of heterogeneous research programs are both different and yet still related. This implies that cross-

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<sup>11</sup> For two accounts of science without the unification ideal, see Cartwright (1999) and Dupré (2001).



fertilization and borrowing between them is always possible, and consequently provides them with a certain amount of flexibility in their paths of development. In such circumstances, should a ‘constraint’ appear to be binding on a particular research program, that program might alter its perceived agenda by drawing on related programs’ apparent success in addressing that ‘constraint.’ Such shifts might even be expansionist or imperialist should they move a program’s agenda into another theoretical domain (unoccupied or occupied). Science development seen in this sort of way, of course, is likely to leave us with fewer clear lessons about the nature of interdisciplinarity and imperialism, since change on this view is all too piece-meal in nature to produce many general conclusions. But it may also be the case that the development of science simply does not provide us with the sort of big lessons that we might like to have. Accordingly, while Mäki’s top-down more deductive type of account gives us a valuable framework for opening up the important subject of the relations between sciences – as well as between research programs within sciences – I vote for a more historically specific type of analysis that seeks to enlarge his framework by paying close attention to the history of the sciences, and in the case at hand the history of economics.

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