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## The Turn in Economics and the Turn in Economic Methodology

John B. Davis

Marquette University, john.davis@marquette.edu

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# The Turn in Economics and the Turn in Economic Methodology

John B. Davis

University of Amsterdam and Marquette University

## Abstract:

The article presents the speech "The turn in economics and the turn in economic methodology," by John B. Davis, professor of economics at the Marquette University in Wisconsin, during the "International Network for Economic Method Conference" held in Grinnell College, Iowa on June 22, 2006. Davis discussed the developments in the field of economic methodology as well as the structural change confronting economics associated with the new research approaches from other sciences.

# 1 A TURN IN ECONOMIC METHODOLOGY?

There is now considerable evidence that economics is undergoing significant change in which a collection of new research programs all at odds in important respects with standard neoclassical economics is increasingly dominating the economics research frontier (Davis [14]). These new programs include game theory, evolutionary economics, behavioral economics, experimental economics, agent-based complexity economics and neuroeconomics. All raise new issues for economics, and contest long-held assumptions. Such a development, however, naturally raises questions about the nature and direction of economic methodology. Whereas economics investigates the economy, economic methodology investigates economics (as does the history of economics). Thus a significant re-direction of economics suggests there may be a need for an associated re-direction in the focus and concerns of economic methodology.

Of course change in the economics research frontier *per se* does not hold any special implications for economic methodology, since change in any field's research frontier is always ongoing. Indeed even dramatic change in a field's research frontier need not have any special implications for its philosophy of science. The current change in economics, however, has one remarkable characteristic: *all* the new research programs in economics have their origins in other sciences. Thus whereas economic methodology – as befits its name – has essentially only had economics as its province in the past, it now may need to have as its new domain of responsibility not science in general (then it would be indistinguishable from the philosophy of science), but economics-within-science. In this regard, economic methodology as a field comes relatively well prepared, since its own origins and resources as the philosophy of science for economics lie in large degree outside economics in the history and philosophy of science. From the logical positivism of the Vienna Circle, to Karl Popper, Imre Lakatos and Thomas Kuhn, and from them to the sociology and rhetoric of economics, economic methodology has long come to economics with resources found elsewhere, paradoxically giving it both leverage in the analysis of economics and also sometimes the appearance of irrelevance to many economist practitioners (Davis [12]).

At the same time, however, since its most recent 'revolution' (more on this below) in which methodologists have sought a closer relationship with economics through methodological examination of its standard practices, economic methodology has arguably become more and more home-grown, perhaps even in-grown, so that the one important question I believe the current change in the economics research frontier raises – what are the boundaries and the definition of economics – is not obviously one that economic methodologists are currently inclined to address. Indeed, I believe many if not most would say that Daniel Hausman's diagnosis at the end of the 1980s of economics as a separate (and inexact) science remains largely true today (Hausman [20]). Many no doubt would also append the reinforcing view that economics today remains securely imperialist or expansionist vis-à-vis other social sciences, able to expand into other domains without risk. But if the boundaries of economics are now under challenge in new approaches in economics largely derived from other sciences, and if economics' character as a separate science is now increasingly difficult to justify, then the methodological and historical issues entertained on this general view of the field may obscure for methodologists the new issues that economics now confronts. That is, there seems to be something of a roadblock before us to engaging new methodological issues, which ironically many economists themselves are already confronting, often quite consciously. Thus if at the end of the 1980s, when most of the new research programs began to register their impact on the discipline, economic methodologists were ahead of economists in understanding the nature of the discipline, today it might be argued that the reverse is true, and many methodologists are behind the practitioners, or at least the ones engaged in introducing new approaches into economics.

In what follows, I begin in section 2 by setting out a very brief overview of the development of the field of economic methodology in terms of three 'revolutions' that chart its history as a separate subfield within economics. Section 3 then asks how this history squares with the issue of structural change currently facing economics in connection with the new research approaches derived from other sciences. Section 4 advances an

argument for a specifically historical approach to the field of economic methodology, and suggests that this involves an evolutionary kind of reasoning. Section 5 characterizes the economic methodology appropriate to this historical approach as a 'systematic descriptive pluralism', advances two sets of principles that might provide a basis for this approach, and returns briefly to the evolutionary metaphor. Finally, section 6 makes brief remarks about the role of the concept of identity and ontology in this overall framework.

## 2 THE THREE REVOLUTIONS IN ECONOMIC METHODOLOGY

It may be too artificial to periodize major stages in the development of economic methodology, and it is certainly overly dramatic to talk about 'revolutions' in methodology, but I provisionally adopt these conventions here to more sharply pose the issue of whether economic methodology is now (again) at a turning point. The legacy of those who call out turning points is to be shown by subsequent commentators to have overlooked the density of change, and have exaggerated the moment. No doubt that applies here as well. Nonetheless I adopt the periodization that follows – a fairly standard one in fact – because I believe it helps clearly distinguish the current state of affairs facing economic methodology.

To have a revolution, there must first be a *status quo*. In economic methodology, though there are anticipations in Classical economics, the *status quo* essentially originates with and runs from J.S. Mill and his followers through J.N. Keynes and the Cambridge School through to the neo-Austrian Lionel Robbins. The 'first revolution' was thus the introduction of the logical positivism of the 1930s into economics largely under the leadership of Terence Hutchison that brought the analytic–synthetic distinction and the question of empirical grounding into economics as a required credential for economics to count as a science. Milton Friedman's instrumentalism and Paul Samuelson's operationalism, advanced in the postwar period under the banner of positivism, reflect the persisting influence among economists of this 'first revolution'.

A 'second revolution' can be associated with the rejection of the logical positivist 'received view' (Suppe 1977, [28]) by Popper and Lakatos and extends to, but also begins to fray with, new directions introduced by Kuhn.[1] Popper and Lakatos for related but somewhat different reasons rejected the narrow form of the logical positivist project in terms of the verifiability criterion, but maintained its spirit in a commitment to demarcating science from non-science through the application of rules, albeit more complex ones. Their attention to the organization and practices of science takes them beyond the ahistorical character of the logical positivism, and puts them on the road to Kuhn's overtly historical scientific revolutions approach, though they retain the idea that progress in science is a coherent notion. Kuhn himself was not a demarcationist, but his ideas about normal science have nonetheless lent his views this quality for many. Normal science within a paradigm ultimately finds itself inadequate to the accumulation of anomalies, precipitating a 'liberating' scientific revolution, which is followed by new normal science. Though the incommensurability of paradigms is contrary to the idea of scientific progress, the language of revolution and the implicit idea that a paradigm's normal science exhausts itself in diminishing returns strongly suggests that a revolution is progressive. Normal science appears pedestrian; revolutionary science appears heroic. If not demarcationist, Kuhn nonetheless implicitly prescribes change in science as an advance, and his most famous example, the Copernican revolution, is indeed universally accepted as scientific progress.

There is a 'third revolution' in economic methodology, somewhat less trumpeted than the first two, but no less influential in redirecting the practice of economic methodology, and especially crucial for understanding the outlines of current economic methodology. Central here is the downplaying of any normative or prescriptive account of economics and a commitment to naturalism as a descriptive scientific methodology – a 'reflection without rules' (Hands [19]).[2] The sociology of scientific knowledge (SSK) and its extension to the economics of scientific knowledge, the rhetoric approach in economic methodology, and an emphasis on the history of science represent the main forms.

One consequence of this latest development is a more micro focus to much recent methodological investigation in the form of case studies (e.g. Backhouse *et al.*[3]; Morgan and Morrison [23]). Partly the motivation has been to provisionally hold to the side philosophy of science categories and preconceptions that have dominated much of the history of economic methodology. Partly the motivation has been – in the spirit of the SSK laboratory studies approach – to describe how economics is actually done by economists.[3] But on both scores – the bracketing of philosophical categories and the emphasis on concrete micro description – any more macro-oriented type of methodological investigation concerning how individual case studies might fit into broader developments in economics, or how these developments might reflect changes in the nature of economics as a whole, has become less a focus.

This is essentially where we stand today. How, then, does economic methodology's current posture measure up to the recent change in the economics research frontier?

### 3 A CHANGE IN THE QUESTIONS?

To begin, note that questions which might arise about the nature of economics as a science, should it indeed be undergoing significant structural change at the present time, are not easily formulated in an economic methodology with only a micro case focus. Nor is an economic methodology at the more macro focus end of the scale, as pursued in the field's 'second revolution', likely to allow us to satisfactorily address the nature of current structural change in economics, if this change involves the impact of other sciences on economics, as Popper, Lakatos and Kuhn were basically concerned with changes within a science taken in isolation from other sciences.

By structural change in economics, then, I mean two things: (1) in an external sense, changes that concern where the boundaries of economics as a whole lie relative to other sciences, particularly when economics' boundaries have been challenged by imports of contents and tools from other sciences; (2) in an internal sense, changes that concern how economics operates in core–periphery terms understood specifically as a matter of there being an orthodoxy and a heterodoxy (or rather heterodoxies). These two concerns are interconnected in that (i) by definition imports into economics from other sciences are initially heterodox; (ii) imports that subsequently become orthodox force a shifting in economics of what counts as orthodox and heterodox; and (iii) imports from other sciences that become orthodox contribute to re-defining economics in the image of other sciences, even should it maintain boundaries vis-à-vis other sciences.[4]

This framework creates a set of what I term economics-within-science questions for economic methodology. In terms of the external sense of structural change, the traditional boundaries of economics are called into question with the import of other science empirical practices such as experimentalism, neuro-imaging, subjective well-being analyses, cross-cultural surveys, etc. These go beyond standard empirical practice in economics as in econometrics, and also challenge the field's historic bias towards *a priori* deductive reasoning. In terms of the internal sense of structural change, the traditional view of what is orthodox has been called into question with, for example, the behaviorist critiques of rationality, Santa Fe-based complexity critiques of the equilibrium concept and evolutionary game theory's abandonment of human beings as economic agents. Were rationality, equilibrium and the individual to cease to be orthodox, what would be?

Turning to economic methodology, I claim that the issue of structural change in economics as I have described it is largely foreign to current thinking, and is so for a fairly simple reason. If we date the emergence of economic methodology as an identifiable sub-field of economics from the time of its 'first revolution', that point in time roughly coincides with the beginnings of the neoclassical dominance of economics in the late 1930s, while all the further 'revolutions' in economic methodology that have occurred since then fall within the historical timeframe in which neoclassicism has generally been identified, albeit loosely, with economics as a whole. Thus, economic

methodology has not been investigated either when there was a dominant paradigm under challenge, or more importantly when a challenge to that paradigm brought the status of economics itself as a separate science into question. Accordingly, there has never been an occasion since the professionalization of economic methodology to ask deep structural questions about the nature of economics as a science. Consider the consequences of this.

When multiple approaches compete for dominance within economics, and none can be identified with the field as a whole, the issue of what characterizes economics and also what counts as orthodox naturally comes to the fore, both because it is in dispute when there are multiple approaches in the field, and because the respective practitioners of its different approaches are inevitably intent on explaining and justifying their competing approaches in methodological terms (as for example occurred previously when neoclassicism rose to dominance in the methodological pronouncements of Milton Friedman, Paul Samuelson and Tjallingis Koopmans). Indeed, the recent emergence of practitioner concern with methodology is arguably one source of methodologists' involvement in case studies, as however unsystematically stated practitioner methodological concerns may be, they nonetheless appear to be methodologically significant to the trained eye. (Nonetheless, the emphasis in the case studies approach, particularly as inherited from SSK laboratory studies research, remains micro in nature and is little concerned with the issues surrounding the nature of economics-within-science.)

How, then, might economic methodology reinvent itself again in the current context when economics' external and internal boundaries are in question? I will suggest two avenues that might be adopted for this purpose: (1) an historical approach to economic methodology; (2) a systematic descriptive pluralism approach to methodology.

## 4 AN HISTORICAL APPROACH TO ECONOMIC METHODOLOGY

What would be specifically 'historical' about an historical approach to economic methodology? Explanations framed in historical terms emphasize processes that involve considerable particularity and generally do not conform to logically well-ordered patterns. Accordingly historical explanations concentrate on tracking and describing sequences of events, and on capturing as much detail as is involved in those processes as possible. I suggest this framework of analysis is especially well suited to explanations of economic methodology in periods of significant change in economics, because of the difficulty associated with assessing different competing strategies of economic explanation that are (i) themselves in a process of development and are (ii) changing at least in part in response to change in other neighboring strategies of economic explanation. Conversely, there is less need to rely on historical forms of explanation in economic methodology when strategies of economic explanation are relatively unchanging and uninfluenced by one another.[5]

As an example, take the development of complexity economics. Complexity economics is closely associated with the origins of computational economics in artificial intelligence (AI) theory (cf. Davis [15]). But AI theory has undergone considerable change itself, including many proponents' abandonment of its early commitment to functionalism and the multiple realizability thesis, namely, the idea that cognitive processes can be equally instantiated in any and all different types of hardware. This has produced two rival pathways for computational reasoning in economics, one associated with computable general equilibrium models, as associated with the first Elsevier *Handbook of Computational Economics* (Amman *et al.*[1]), and one associated with agent-based computational economics (Tsfatsion and Judd [29]).[6] The former seek to implement Arrow–Debreu–McKenzie axiomatic general equilibrium rational choice models, while the latter simulate a variety of algorithmic agent decision procedures which need not have standard equilibrium interpretations. To make matters more complicated, note also that a major applied development of complexity reasoning in economics involves network theory. But network theory originates in mathematical graph theory and accordingly draws on foundations quite different from computational reasoning. Further, while proponents of agent-based

computational economics regard network theory as falling within the domain of complexity economics, proponents of computable general equilibrium models do not.

Historians of economics are entirely accustomed to sorting out and describing such divisions, cross-connections and rivalries as these. My argument here is that economic methodologists cannot readily explain and assess methodological reasoning inherent in strategies of economic explanation such as are involved in computable general equilibrium theory, agent-based computational economics and network models without knowing how these strategies have developed and how they have developed in relation to one another. In the example here, we see a contest between two underlying theories of computation and also a contest between computation and graph theory. Assessing their attendant economic explanations accordingly depends on assessing the relative merits of their foundational principles as then applied in the space of economic thinking. There is no one epistemic account of explanation available here that will allow us to make methodological claims about the nature of complexity economics as a form of economic reasoning. In a changing world of rivalry between different approaches, we need to chart the comparative progress of different types of explanation as they develop in response to one another. Indeed, this is what the practitioners themselves are doing, as they judge their relative progress in developing their respective economic explanations vis-à-vis one another.

This view of an historical basis for economic methodology is clearly more readily advanced in a period of dynamic and rivalrous change in economics. But it seems to apply no less to historical periods in which broad change in economics is absent and relative stability exists in the form of dominant approaches, as Phil Mirowski and Wade Hands have shown in terms of the competition and rivalry between different neoclassical approaches and strategies for explaining demand in the transition to postwar economics (Mirowski and Hands [22]). Why, then, have arguments for a more historicized economic methodology not been more common over the recent history of the field, which has generally been seen as applied epistemology?[7] The answer on the argument here is to be found in the historical coincidence between the professionalization of economic methodology and neoclassicism's relative dominance of economics. In a period of comparative calm in terms of change in the discipline, the epistemic claims of an approach commonly identified with the field attained a special prominence that naturally attracted methodologists' attention. Add to this the tendency of a discipline structured around dominant approaches and an orthodox–heterodox divide to actively discourage and suppress attention in non-standard approaches (Davis [13]) and one has further reason to overlook the historical character of economics and the need for a more historical approach to economic methodology.

Let me, then, outline what I believe this argument implies is involved in pursuing a more historical approach to economic methodology. Though the emphasis in this section has been on placing greater weight on the role of the concept of change in economic methodology, here I incorporate my earlier emphasis on the change involving the structural character of economics as a science. Thus, if economics exhibits structural change in both external and internal ways – in terms of its boundaries with other sciences and in the way it constitutes a core–periphery orthodox–heterodox divide – then particular strategies of economic explanation ought to be investigated in terms of both these dimensions. As an organizing framework I suggest an evolutionary view that tracks their development and change.

An evolutionary framework studies the origins and destinations of species in an environment of competition between species. The evolutionary metaphor, of course, has a variety of interpretations, but the emphasis here on an historical approach to economic methodology gives it a more cultural evolutionary and less Darwinian interpretation. The Darwinian logic of species competition nonetheless applies to competing strategies of economic explanation within economics, and the related biological notion of ecological niches applies to the status of boundaries of economics relative to other sciences. How can this framework then be applied to economics in its current historical context?

First, while it is possible to imagine a future in which economics ceases to exist as an autonomous science, I assume this is unlikely, and rather focus on the change in economics' boundaries, that is whether its ecological niche is undergoing change as a result of species imports from other niches. If this metaphor seems contrived, note its methodological purpose. The idea of cross-boundary imports is of the entry and at least temporary presence of something quite foreign to economics; in terms of the new approaches in economics, this concerns all the other-science contents now being placed in juxtaposition to already existing economics contents in the current transformation of the economics research frontier. As befits new imports, these other-science contents are initially heterodox. Thus economics' environment is made more complex by the cohabitation of traditional heterodox strategies of explanation and new research program heterodox strategies of explanation, both alongside a long-standing orthodoxy.

Second, in addition to characterizing economics as a whole in its niche relative to other sciences, the evolutionary metaphor invites us to examine the different species or explanatory strategies themselves. Using the origins and destinations time references, strategies of economic explanation can be investigated in terms of their origin stories and orientations. Origin stories for traditional heterodox economic approaches tell us, not how these approaches arose, but how they became heterodox (Davis and Sent [16]). Origin stories for the new heterodox approaches in economics on the changed research frontier of the field tell us how other-science approaches have been taken up in economics. But both kinds of heterodox approaches also exhibit destination orientations toward or away from the field's orthodox core, according to whether they make critique of its main propositions central to their development or not. Thus particular approaches in economics can be assessed in their explanatory strategies as they develop in their competition with one another, all relative to the status of economics' niche neighboring other sciences.

What, again, is methodological about this framework of investigation? And why is this not simply a way of carrying out an historical rather than methodological analysis of recent economics? Nothing here precludes our raising all the standard epistemological, ontological and normative concerns that characterize economic methodology. What is different is how those concerns are assessed. Rather than pursuing them one by one in their most abstract form at the bar of philosophy, they need to be seen as parts of packages of explanation, whose contents and relative degrees of importance are continually subject to adjustment in a changing competition between different strategies of economic explanation. Turning to complexity theory again, for example, non-stochastic simulations fail by the standard of empiricism, and recall economics' historic bias in favor of *a priori* deductivist explanations. But non-stochastic simulation also generates explanations of behavior that go beyond econometric data mining in defense of given theoretical preconceptions. Thus what counts as good economic explanation by standard epistemic criteria depends on the comparative merits of both strategies of economic explanation, which can moreover evolve relative to one another. The simple message, then, is that methodology needs to be embedded in the history of economics, and in an economics located within science.

The discussion in this section has been aimed primarily at the question of how economic methodology ought to be pursued. The next section offers a characterization of what economic methodology might be on such an understanding. Here I attempt to give new meaning to an established understanding of methodology as pluralism.

## 5 ECONOMIC METHODOLOGY AS A SYSTEMATIC DESCRIPTIVE PLURALISM

Pluralism, broadly considered, is arguably the approach to economic methodology that has gained the greatest general following in the last decade. However, until recently (cf. Dow [17]; Sent [27]) this has been little associated with change in the economics research frontier, and has been more associated with the argument that there can be no single set of overarching criteria that methodologists might employ to assess economic explanations (Salanti and Screpanti [25]; Samuels [26]). In this section, I build on this earlier understanding of



pluralism and its more recent application to the current change in economics to lay out specific principles which might guide an historical approach to economic methodology as a systematic descriptive pluralism.

The idea that there can be no single set of overarching criteria that methodologists might employ to assess economic explanations is essentially an indirect form of argument,[8] and thus emphasizes what is not the case rather than what is the case. I suggest, then, that elaborating a systematic descriptive pluralism involves adopting the latter focus, where we ask what it is about economic explanations that preclude there being overarching methodological criteria to apply to them. The main answer that the recent change in the economics research frontier implies is that economic explanations associated with the new research programs in economics cannot be judged by a single set of criteria, because those programs originate in different sciences with methodologies that are fundamentally different. That is, though there are certainly commonalities in methodological reasoning across the sciences, those commonalities lie at a high level of generality that does not capture the distinct explanatory ambitions that differentiate sciences. Conversely, were sciences to have essentially the same explanatory ambitions, then they would not be distinct sciences.[9]The dramatic rise in other-science imports in economics since the 1980s makes the internally heterogeneous character of economics especially apparent at the current point in time. But it can be argued that economics – and indeed all sciences – have histories of importing (and exporting), and that the history of science is one of permeable disciplinary boundaries. Economics, because of its tendency to structure itself around dominant approaches, makes its borrowed character less evident when a particular approach is dominant, and this, I have argued, has tended to obscure for methodologists economics' internal conceptual and methodological heterogeneity.

How, then, does this shift in focus specifically imply a systematic descriptive pluralism? First, if we focus on other-science contents as they come to be elaborated within economics, a static, snapshot perspective that provisionally brackets the dynamics of research programs can be organized around an established literature on the subject of indeterminate rankings. The objective of this literature is to investigate how we compare and rank options that embody different values. Every comparison of any two things raises the issue of whether there is some value they share, or a 'covering value' (Chang [10]). Putting aside the problem of whether covering values are complex or compound, clearly many comparisons cannot be framed in terms of shared covering values. Those items are then seen as being incomparable (Broome [8]). Different from incomparability, however, is incommensurability, or when items being compared share a covering value, but cannot be precisely or clearly compared because of ambiguity in the application of that value (Broome [7]). But there is yet a further problem with making comparisons associated with the concept of incompatibility. Items may be comparable, and they may be commensurable, but they may yet simply conflict with each other, thereby making their ranking problematic for a third reason (Raz [24]).

Combined, then, we have a trichotomy thesis regarding comparisons and rankings of items – incomparability, incommensurability and incompatibility – which can serve as one element in a pluralist program for systematically judging how distinct research programs can be treated as relatively autonomous. The idea of ranking, as it is found in the existing literature on indeterminate choice, can then be seen in the methodology context as the methodologist's strategy of analysis when the materials being evaluated suffer from incomparability, incommensurability, incompatibility, or some combination of these. That perspective needs to be seen as pragmatically reflecting the goals of the evaluator, but lacks any further epistemic foundation. The items economic methodologists evaluate, moreover, are research programs and initiatives within those programs. The investigation is systematic because the trichotomy thesis offers a framework in which the communication and non-communication between different research programs can be explained, but it is also an essentially descriptive enterprise, because there is no ultimate foundation for the perspective and values adopted by the investigator.

Second, we may also add to this static framework a dynamic one that addresses the changing character of economic explanation and also the changing status of other-science content in economics. On the view advanced here, as long as economics retains a relative autonomy as a science, imports from other sciences must be expected to increasingly lose their other-science associations the longer their tenure in economics. I characterize this as a 'domestication' of these other-science contents, and argue that it often involves the translation of these imported contents into tools shorn of much of their other-science substantive association, and thereby more amenable to combination with existing economics conceptual materials (cf. Davis [13]). A parallel and complementary way of understanding the evolution of other-science materials in economics employs a natural history of metaphor type of approach to explain how cross-boundary transfers of ideas have a short-lived metaphorical status as long as they retain their external origins, only to lose that metaphorical status as they become standard (Lagueux [21]). On either view, other-science contents entering into economics are constantly changing in meaning according to how they are assimilated into economics research programs. This assimilation, moreover, reflects the competition between research programs, which itself may larger reflect relationships between research programs in terms of where they lie relative to orthodoxy and heterodoxy. A systematic pluralist economic methodology, then, could add to static trichotomy thesis analysis an account of the different dynamics operating at economics' boundaries as a science, within economics research programs, and between research programs.

To return to the evolution metaphor of the previous section, a systematic pluralist economic methodology describes a complex developmental process that is arguably governed by a reasonably clear set of principles which accounts for the origins and orientations of research programs as particular species competing within an economics ecological niche bordering on other disciplinary ecological niches. The chief principle of pluralism, that there are no overarching evaluative criteria, takes on a meaning here analogous to the idea in evolutionary theory that the standards of fitness are relative to a species' ecological niche. But evolutionary theory also treats an ecological niche as being subject to transformation according to the competition within it. Thus there are no independent, objective standards in evolutionary theory to judge species or niches. What explains the historic appeal of evolutionary theory, whether in its classic Darwinian form or its various subsequent explications, is the success with which an ongoing process is explained in terms of a relatively small number of principles associated with variation and selection. A pluralist economic methodology, then, can both escape epistemological foundationalism by adopting this developmental perspective, and yet proceed in systematic fashion according to its application of principles that explain research programs/species variation and change.

## 6 CLOSING REMARKS ON IDENTITY CONDITIONS

A loose end in the analysis above concerns the question of how we know when we have a distinct research program or indeed even an entire distinct scientific discipline. The discussion above presupposed that research programs can be distinguished from one another, though it also allowed they can change relative to one another, leaving open the very real possibility that they may combine with other programs or even simply disappear. What justifies our saying, then, for example, that experimental economics or behavioral economics are distinct research programs? The origins and orientations analysis given above offers only a start on answering this question by providing a framework for tracking these programs as distinct programs through change. Similarly, economics' own status as a relatively autonomous science was assumed in the discussion above, though the existence of cross-boundary content and tool transfers between economics and other fields raises the question of what makes economics a distinct field. If economics changes over time, with its character changing to reflect the influence of this science, now that science from which it borrows, what justifies our saying that economics is a distinct science?

The issues thus raised concern how we identify research programs and sciences. I phrase this as a matter of specifying their identity conditions, or rather as a matter of establishing the identity criteria we implicitly employ to say when particular research programs and sciences endure (at least for periods of time) as distinct programs. As might be imagined, the investigation of the general concept of identity is a significant task in itself, and indeed goes back to Aristotle's *Metaphysics* ([2]). Aristotle was concerned primarily with the identity of substances, or things, but his and later thinking in the history of philosophy about identity has been applied to any and all kinds of existents we wish to postulate: individual things, persons, collections, events, mental states, discourses, etc.[10] Here, in closing I restrict myself to one comment regarding what I regard to be the importance of this issue to the subject of a pluralist economic methodology.

The kinds of things we need to be able to formulate identity conditions for in economic methodology – research programs and economics as a science – are generically speaking different kinds of discourses. Discourses are a complex type of object whose identity conditions are not easily specified or modelled on the identity conditions of individual things or indeed collections of things.[11] This project seems to me nonetheless to be essential to an economic methodology that is pluralist in character, since, as the evolutionary metaphor implies, that methodology assumes that irreducibly different types of things exist. Thus, because a pluralist economic methodology takes the concept of existence seriously, it has ontological reasoning as an important component. This, I believe, represents a largely overlooked dimension of pluralism, which has arguably been obscured by its recent origins in the debate about overarching epistemic criteria. But, if research programs and sciences are at some level, or for periods of time, irreducible to one another (and if we believe we ought to reject an epistemically driven unity of science philosophy approach to understanding the sciences), then the problems of existence and identity must come to the fore in connection with discourses. Here I leave this topic for future investigation, and simply assert that it constitutes one important way in which a descriptive pluralism needs to be systematic.

## ACKNOWLEDGEMENTS

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## Notes

### Footnotes

1. *This revolution also produced the founding of economic methodology as an identifiable sub-field of economics largely as a result of Mark Blaug's systematic delineation of its problems and history (Blaug 1992, 1980). Also instrumental to this development was Bruce Caldwell's discussion of the revolution (Caldwell 1994, 1980).*
2. *Wade Hands' careful and exhaustive discussion of this revolution (Caldwell, 1980, 1994) and Larry Bolands' examination of the foundations of economic methodology (Boland 1982) and the naturalistic turn in economics is the definitive account (Hands 2001).*
3. *Thus reflecting Hausman's ([20]) understanding of economics as a separate science.*
4. *I put aside here whether the import process might lead to a breakdown in economics' organization of itself in terms of an orthodox–heterodox divide, but argue in Davis ([13]) that economics is unlikely to adopt the weaker distinction between conventional and unconventional approaches found in many other sciences.*
5. *The distinction here is that between weakly interactive and strongly interactive systems (cf. Batten [4]).*
6. *As evidence of the competition between the two approaches, consider Leigh Tesfatsion's comment. Tesfatsion recalls that at a 1996 UCLA workshop she, Rob Axtell, Charlotte Bruun, Axel Leijonhufvud, and others in attendance 'discussed naming the field "agent-based economics." Consequently, this is why I called the website that I developed in late 1996 the "agent-based economics website." However, I soon discovered*

that many analytical microeconomists felt they were already doing "agent-based economics" simply by means of having a utility maximizing consumer agent! So I changed the name of the website to the "agent-based computational economics" (ACE) website to try to indicate that we were referring to something quite distinct from current mainstream economics. This is the name I still use for the website [<http://www.econ.iastate.edu/tesfatsi/ace.htm>] today' (Tsfatsion, personal communication, 5 May 2006).

7. Thomas Kuhn's influence and science studies approaches are the exception.
8. The reductio form is, 'if there were such criteria, we could make such-and-such an argument; but we can't make this argument; therefore there cannot be such criteria'.
9. John Dupré ([18]) makes much the same type of argument in connection with his rejection of a unity-of-science conception for the philosophy of science.
10. Within economics, the most investigated identity problem concerns the nature of the firm. Because firms are made up of many agents, it is not obvious why they should be seen as single entities. However, multiple selves analysis makes the identity of individuals themselves an equally important issue, requiring that we ask why they should be seen as single entities.
11. I have offered a partial account of identity conditions for discourses in terms of their boundaries with one another in Davis ([11]).

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