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SPECIAL ISSUE IN HONOUR OF MARK BLAUG

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Editorial: special issue in honour of Mark Blaug

Mark Blaug used to begin his history of economics course with an old Greek proverb: “the fox knows many little things, but the hedgehog knows one big thing”.¹ He would then say that one could characterise most thinkers in the history of economic thought as either a fox or a hedgehog. As a student, I found that this character-driven view illuminated past economists’ theories in a fresh way and brought home to me the distinctiveness of their approaches. Nevertheless, after reading his work, attending his course, listening to him at seminars, and becoming familiar with his ideas on the history and methodology of economics, one question has always remained unanswered in my mind: was Mark Blaug a fox or a hedgehog?

If one focuses on his choice of research topics, one might be tempted to describe him as a fox. He wrote essays and articles on almost every subject in economics, and could take some credit for launching new ones, such as the methodology of economics, the economics of art, and the economics of education. He was always incredibly well-informed and up to date about the academic literature in all areas of economics. Indeed, he was well-known for reading voraciously and widely, not only within economics, but also in fields as varied as philosophy, political sciences, physics, biology, history, art, and literature. And while some foxes might have to sacrifice a grasp of detail for the sake of comprehensiveness, Mark’s pursuit of the economic ideas that interested him was far from superficial.

Yet from another perspective, he could be described as a hedgehog. There were a number of central issues that he returned to repeatedly and studied intensively (and almost obsessively) throughout his career, for instance, Ricardian economics, general equilibrium theory, the normative character of economics, and falsificationism in economics (see Blaug 1958; 1992 [1980]; 2002; 2003; 2007). Considered separately, Mark’s approach to these topics was quite fervent and dogged towards a

¹ The oldest source of the maxim seems to be the 7th century B.C. poet Archilochus. In the 20th century the proverb was popularised by Isaiah Berlin’s celebrated essay *The hedgehog and the fox* (1993 [1953]).
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well-defined goal. On this light, as a determined and indefatigable pursuer of certain issues, Mark shows the features of a hedgehog.

The ambivalence between the fox and the hedgehog in Mark’s personality can also be seen in his approach to the history of economics. In his Economic theory in retrospect (1997 [1962]), Mark set out to provide a history of economics from the mercantilists up to John Maynard Keynes, a project that combined ambitious breadth (the mark of a fox) with an unusually narrow focus on theoretical analysis alone (the mark of a hedgehog). He was so well learned on each of the topics of his chapters, that the book reads as if each chapter was written by a devoted specialist on the respective topic. The book has been enormously successful through its many revised editions, and it seems to me that Mark’s carefully judged ambivalence between comprehensive reach and single-minded purpose is part of what makes it a masterpiece.

Interrogating the theoretical coherence of the work of historical (and current) thinkers was characteristic of Mark’s confrontational style of scholarship. He relished the intellectual thrill of rigorous scholarly debate and was rather good at it. Yet he was never only interested in winning. Rather, he used such debate as a method of inquiry, not only to challenge his opponents’ ideas, but also to test and improve his own. As an observer of the history and method of economics, he was also attracted to other people’s academic controversies as places where the gaps and weaknesses in conventional accounts were exposed to the most vigorous scrutiny and critique.

So it is not surprising that much of Mark’s work can be organised and studied in terms of tensions or debates. Many of the topics in the history of economics that first interested him were famous scholarly conflicts, such as the Cambridge-Cambridge capital controversy, and the imperfect competition debates of the early twentieth century (see Fountain 2007). Likewise, in the methodological arena, Mark was attracted to heated intellectual disputes, such as between the Kuhnian and Lakatosian approaches to the philosophy of science, the historical and rational reconstruction views in historiography, and the defenders and critics of mathematical formalism in economic theory.

Mark’s love of debate was reflected in his combative style, not only in his published writings, but also in his public addresses and conference interventions. As his wife, Ruth Towe (2013), has also pointed out, Mark’s academic contributions were often worked out and developed by probing others’ arguments and claims, provocatively
challenging established views and seeing what developed, or in ‘collaboration’ with a sparring partner of sufficient intellectual calibre. Yet, while the full onslaught of Mark’s deeply informed challenges could be a rather ferocious spectacle, I was always impressed by his ability to maintain the best of friendships with those he fiercely disagreed with.²

I have been talking of Mark’s combative intellectual style and his attraction to debates, conflicts, and tensions in economics, because the articles included in this special issue of *EJPE* dedicated to him are heavily concerned with such themes. More specifically, they all deal with at least one of Mark’s favourite academic debates in the methodology and history of economics:

1. The debate about the positive or normative character of welfare economics (and of economics in general).

2. The role of formalism in economic theorising (or the tension between mathematical rigour and practical relevance).

3. The proper approach to the history of economics (or the tension between rational and historical reconstruction).

In the first article, D. Wade Hands reviews one of Mark’s recurring topics: is the ‘new’ welfare economics positive or normative? Can such an area of economics be independent of ethical commitments, as some economists claim? Hands explores this subject with reference to the exchange that took place between Mark and Pieter Hennipman at the beginning of the 1990s. His account of their debate sheds light on the implicit assumptions of both authors and thus helps readers understand why it never came to a clear-cut conclusion. Hands ends by elaborating on his own contribution to the debate by, as he puts it, adding what Mark would have had to say to win his case that welfare economics is unavoidably normative.

In her article, Sheila Dow focuses on Mark’s concern that mainstream economists have become much more occupied with mathematical formalisation than with the empirical testing of their theories. Dow comments on Mark's Popperian/Lakatosian methodology in the light of recent developments in economics and experimental results, such

² I witnessed this firsthand after a seminar at EIPE in which Mark and Deirdre McCloskey had had a long and fiery argument during the session. Not five minutes later, they could both be found chatting over a glass of beer and laughing together as old friends will.
as those questioning the validity of the axioms of rationality and optimising behaviour, which are part of the ‘hard core’ of mainstream economics. Dow then provides a methodological appraisal of whether the ‘new’ behavioural economics research program is progressive or degenerative in Popperian/Lakatosian terms.

In the third article, John Davis sets out to explain the evolution in Mark’s historiographic method from a clear endorsement of rational reconstruction to his later reconsideration of the merits of historical reconstruction. Davis explores two issues: on the one hand, the tensions that result from the application of the economics of scientific knowledge to the study of economics research; and, on the other, the move to understanding economic phenomena in terms of path-dependencies, combined with an understanding of competition as a process rather than as an end-state. Davis argues that acknowledging these issues clarifies to a great extent Mark’s disenchantment with rational reconstruction and his turn towards historical reconstruction.

In an almost complementary piece to Davis’s article, Harro Maas refers to a brief correspondence between Paul Samuelson and Mark to clarify the latter’s evolving position on how to practice the history of economic theory. Maas argues that Mark’s intellectual development and the historical context in which he was working explain his changing position on the use of rational and historical reconstructions. Interestingly, Maas illustrates the main point of his essay with an historical reconstruction of Mark’s approach to historiography.

In his contribution, Uskali Mäki considers Mark’s campaign to make economic science more relevant to real-world practical concerns. Mark’s normative methodology proposes adopting a falsificationist approach to economic scientific practice, as well as assessing the progress of economic theory in terms of a trade-off between practical relevance and mathematical rigour. According to Mäki, Mark’s methodological prescriptions in favour of a more ‘realist’ economics are not ‘realistic’ in that they are neither systematically spelled out nor viable. After questioning whether there is in fact a necessary trade-off between rigour and relevance, Mäki argues that Blaug’s intuitions can be developed into a more realistic account by bringing in two further topics: economic modelling and the institutions of academic research.

The sixth and closing article, by Jack Vromen, focuses on Mark’s views on evolutionary economics. Vromen notices that, on the one hand, Mark had a positive attitude towards evolutionary economics, while
on the other hand, he saw no significant merits in the highly abstract theoretical forms of economic modelling which have become extensively used in this field. Vromen reviews Mark’s position in relation to Milton Friedman’s views on the selection mechanism produced by market competition, and in relation to the problems of excessive formalism. Then Vromen questions—like Mäki—whether the alleged trade-off between rigour and relevance is inevitable. Vromen illustrates his argument using Richard Nelson and Sidney Winter’s theory of economic evolution, which combines the core ideas of an evolutionary approach with an abstract and formal methodology.

With the publication of this special issue, *EJPE* aims to make its own modest contribution to the celebration and commemoration of Mark Blaug’s scholarly life and legacy (see also Shaw 1991; Boumans and Klaes 2013). We hope that the articles composing this issue will trigger new interest in Mark’s work among scholars who are unfamiliar with it. And also that readers already familiar with his work will enjoy reconsidering some of his old contributions in the light of new methodological developments, as portrayed by the six very distinguished authors and former colleagues of Mark who write in this issue.

Let me close with a brief final comment on the fox-hedgehog conundrum. Mark had the ability and aptitude required to explore a wide range of intellectually exciting topics and debates, but at the same time the way he explored them was by a passionate and resolute pursuit of the right answer. Finding some difficulties in assigning Tolstoy to the right category, Isaiah Berlin (1993 [1953]) concluded that the Russian writer was a fox by nature, but a hedgehog by conviction. In a somewhat similar vein, Mark cannot easily be described only as either a fox or a hedgehog. I would say that he managed to achieve a thriving balance between the pluralistic interests and convictions of a fox, and the underlying monistic fervour and focus of a hedgehog.

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Mark Blaug on the normativity of welfare economics

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Abstract: This article examines Mark Blaug's position on the normative character of Paretian welfare economics in general, and also specifically with respect to his debate with Pieter Hennipman over this question during the 1990s. The article also clarifies some of the confusions that emerged within the context of this debate, and provides as a conclusion some additional arguments supporting Mark Blaug's position, which he himself did not provide.

Keywords: positive and normative economics, Pareto optimality, welfare economics, ethical and methodological norms, Blaug-Hennipman debate

JEL Classification: A13, B21, B31, B41, D60

Without norms, normative statements are impossible. At some point welfare economics must introduce ethical welfare functions from outside of economics. Which set of ends is relevant is decidedly not a scientific question of economics (Samuelson 1952, 1103).

In welfare economics one is engaged in ethical counseling on the economic aspects of social states (Bergson 1954, 247).

In short, there is no such thing as “value-free welfare economics” and, indeed, the phrase itself is a contradiction in terms. To say that something is an improvement in “welfare” is to say that it is desirable, and persuasive statements of this kind necessarily involve ethical considerations (Blaug 1978, 626).

This article will re-examine Mark Blaug's position on the normative character of Paretian welfare economics. Section one explains Blaug's position and its relationship to the views of the founders of the new welfare economics in the 1930s and 1940s. Section two examines

AUTHOR'S NOTE: I would like to thank John Davis and two anonymous reviewers for helpful comments on earlier versions of this article.
Blaug’s argument in more detail through the lens of his debate with Pieter Hennipman (Blaug 1993; Hennipman 1992, 1993): a (sometimes rather heated) exchange in which Hennipman argued that Paretian welfare economics was (or at least could be) strictly positive economic science, while Blaug argued it was inescapably normative. Providing an overall assessment of the debate will prove to be impossible, because the authors were often talking at cross-purposes and defining key terms in very different ways. However, it is still possible to better understand the two positions and to identify the relationship between the presuppositions of the two economists and their stance on the normativity question.

After the examination of Blaug’s position in the first two sections, the last section turns to clarifying some sources of the miscommunication in the Hennipman-Blaug exchange as well as to suggest some additional arguments that Blaug might have made in his response to Hennipman and to other defenders of the strictly positive interpretation of new welfare economics. The three main goals of the paper are 1) to clarify Blaug’s normative reading of Paretian welfare economics and situate his arguments within the broader literature on the ethical and methodological foundations of welfare economics, 2) to identify some of the origins of the communication problems apparent in the Blaug-Hennipman debate, and 3) to try to add some additional arguments supporting Blaug’s interpretation that he himself did not provide.

BLAG ON POSITIVE AND NORMATIVE IN THE NEW WELFARE ECONOMICS

Blaug’s argument for the normativity of welfare economics was presented in two of his most popular books: his history of economic thought textbook *Economic theory in retrospect* (1987 [1962]), and his survey of economic methodology *The methodology of economics* (1992 [1980]). His arguments were repeated and expanded in various places (1987 [1962]; 1990a; 1998) and he was also the author of an important historical paper on the first and second fundamental theorems of welfare economics (2007). His basic position remained the same in all of this follow-up literature: i) Paretian welfare economics is necessarily normative; ii) contra Lionel Robbins and others, its normativity does not prevent the legitimate use of welfare analysis in economic science; and yet 3) it is important to maintain “the positive-normative distinction as far as it can be maintained” (Blaug 1998, 373).
Before delving into the details of Blaug’s position, it seems useful to review the history of the new welfare economics. Welfare economics has traditionally been defined as the (micro)economic theory that provides tools for the evaluation of various economic policies, institutional arrangements, and allocations of economic resources (outcomes). Since throughout the history of economics the most carefully analyzed institution for the allocation of resources has been the competitive market, there has always been a close connection between welfare economics and the idea that “in some sense perfect competition represented an optimal situation” (Samuelson 1947, 203).

Focusing on mainstream post-classical views, welfare economics experienced two periods of relatively stable equilibria: the “old” hedonistic-utilitarian welfare economics of the early neoclassicals and turn-of-the-century British economists like Alfred Marshall and Arthur C. Pigou, and the “new” welfare economics associated originally with Vilfredo Pareto, but formalized and stabilized by economists such as Abram Bergson, Oscar Lange, and Paul Samuelson during the period 1935-1955. Lionel Robbins’s influential *An essay on the nature and significance of economic science* (1935 [1932]) did not make a direct contribution to the new welfare economics, but his critique of the older approach set the stage for the new theory by arguing persuasively against the possibility of making purely scientific interpersonal utility comparisons.¹ This change in welfare economics was of course associated with the ordinal revolution, the move from cardinal to ordinal utility within consumer choice theory during the 1930s.²

One of the key organizing principles of the new welfare economics was of course the concept of a *Pareto optimal* (PO), or economically efficient, allocation of resources: an allocation from which it is impossible to make one person better off without making someone else worse off. Since the possibility of making one person better off

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¹ Key foundational texts for the new welfare economics include: Bergson 1938; Lange 1942; and Samuelson 1947, chapter 8. However, the ideas were popularized in a number of books originally published during the immediate post-World-War-II period such as: Graaff 1968 [1957]; Little 2002 [1950]; and Myint 1965 [1948]. Samuelson provided a definitive summary statement of Bergsonian welfare economics many years later, see Samuelson 1981.

² Although the exact relationship is much more complex and tension-laden than generally recognized: tensions clearly exhibited in the debate surrounding the Robert Cooter and Peter Rappoport paper on this topic during the mid-1980s. See Cooter and Rappoport 1984, and 1985; Davis 1990; Hennipman 1987; Little 1985. For discussions of the historical and philosophical complexities of the ordinal revolution, see Hands 2009, and 2010.
without making someone else worse off implies the existence of a potential Pareto improvement (PPI), a PO allocation is thus one from which there exists no PPIs. In the new welfare economics the relevant vehicle for the evaluation of whether an allocation is “better” or “worse” for a particular individual is his/her ordinal utility function (or the associated well-ordered preferences) from modern demand theory. The two main theoretical results of the new welfare economics were the first and second fundamental theorems which linked the concept of a PO allocation to the Walrasian competitive equilibrium (CE). The first fundamental theorem states that every CE is PO: that a CE is sufficient for an efficient allocation of resources. The second fundamental theorem states that any PO allocation of resources can be achieved by a combination of CE and some set of lump-sum transfers (taxes and/or subsidies).³

Although the first and second fundamental theorems demonstrate the relationship between CE and OP allocations, they do not alone answer the traditional question about the socially optimal allocation of resources or the associated institutions or policies. The problem is the non-uniqueness of efficient allocations. Even in a simple two-good two-agent pure exchange model there are an infinite number of PO allocations (given by the contract curve). Yes, each could be supported by some CE price vector, and yes, every CE price vector is associated with one of these efficient allocations, but that does not alone identify the socially optimal allocation. For that, the new welfare economics employed Bergson’s idea of a social welfare function (SWF): a function that assigns a level of welfare (W) to each of the relevant states of the world based on the social/ethical values of the relevant society.⁴ In its most general form, SWF is given by:

³ For a detailed history, see Blaug 2007. For a detailed discussion of the philosophical foundations of these two theorems, see Hausman and McPherson 2006.
⁴ The importance of Bergson’s contribution to the new welfare economics is captured nicely in a quote from Samuelson (1981, 3):

As I write, the new welfare economics is just over four decades old. This subject, in its essentials as we know it today, was born when the 24-year-old Abram Bergson—then still a Harvard graduate student—wrote his classic 1938 Quarterly Journal of Economics article. To one like myself, who before 1938 knew all the relevant literature on welfare economics and just could not make coherent sense of it, Bergson’s work came like a flash of lightning, describable only in the words of the pontifical poet:

Nature and Nature’s laws lay hid in night:
God said, Let Newton be! and all was light.
where the x's are various states of the world. Although the most general form of the SWF in (1) is the conceptual starting point, most work in the new welfare economics was based on the more restricted case of an individualistic social welfare function: one that “respects” individual valuations. Given the standard characterization of individual 'i’—his/her ordinal utility function \( U^i(x^i) \)—the individualistic social welfare function becomes:

\[
W = w[U^1(x^1), U^2(x^2), \ldots, U^J(x^J)] \text{ with } \frac{\partial w}{\partial U^i} > 0 \text{ for all } i, \tag{2}
\]

where each \( U^i \) is well-behaved and exhibits neither envy nor altruism. This is the form of the social welfare function that has been most discussed in the literature over the years, in part because it corresponds with how most economists think about individual agents and in part because it captures the profession’s (individualistic) intuitions regarding what ought to count within welfare economics, but also because it facilitates the derivation of necessary conditions for social welfare maximization in terms of Pareto optimality: “as a criterion for a maximum position the condition that it should be impossible in this position to increase the welfare of one individual without decreasing that of another” (Bergson 1938, 326). Notice that such a social welfare function will necessarily make (ordinal) interpersonal utility comparisons; it is the ability to make such comparisons that allows maximization of the SWF to identify the social optimal (identify the optimal allocation among the infinite number of PO allocations along the contract curve). As Samuelson explained:

“[… we have seen that it is not possible to deduce a unique equilibrium unless we have more to build upon. This is only as it should be, for intuition assures us that there cannot be an optimum position which is independent of the exact form of the W function […] Without a well-defined W function, i.e., without assumptions concerning interpersonal comparisons of utility, it is impossible to decide which of these points is best. In terms of a given set of ethical notions which define a Welfare function the best point on the generalized contract locus can be determined, and only then (Samuelson 1947, 243-244).”

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5 I employ the compact symbolism employed in Samuelson 1977.
Of course it is always possible to move beyond the general individualistic SWF in (2) to a third level of welfare concretization, to obtain specific results based on the value judgments of a particular society or group of individuals. This could be done by specifying either more restrictive functional forms or explicit functions for the w(·) as well as each of the U_i(·)s and solving the maximum conditions for that particular case. For example, if one takes w(·) to be additively separable and takes each U_i(·) to be a cardinal indicator of each individual's hedonistic utility, one would have the traditional utilitarian social utility function associated with Bentham and the early British neoclassicals.

In this way, Samuelson and others argued that the new welfare economics provided a general framework for welfare analysis that was not dependent on any specific set of ethical commitments, while at the same time accommodating the various ethical views present in the previous literature as particular instantiations of the general analytical framework. It should be noted that this is all quite consistent with the general approach to economic analysis presented in Part I of Samuelson's *Foundations* (1947): characterize the economic problem in terms of a constrained optimization problem, put enough additional structure on the relevant functions to obtain first order conditions for the general problem, and finally move to explicit functions or functional forms to get specific results for particular applications (in this case for particular ethical values). Also notice that when specific value judgments are included, they are the value judgments of the relevant social community or decision maker. The argument that taking the value judgments of the relevant community as data or background information is scientifically just fine—even though injecting your own value judgments into the analysis is not—goes back to at least Max Weber and was consistently endorsed by Robbins and others (see Mongin 2006, 276). As Bergson explained in his original paper:

> In general, any set of value propositions which is sufficient for the evaluation of all alternatives may be introduced, and for each of these sets of propositions there corresponds a maximum position. The number of sets is infinite, and in any particular case the selection of one of them must be determined by its compatibility with the values prevailing in the community the welfare of which is being studied. For only if the welfare principles are based upon prevailing values, can they be relevant to the activity of the community in question (Bergson 1938, 328).
So given all this, what was scientific/positive and what was normative/ethical within the new welfare economics according to its founders like Bergson and Samuelson? Obviously, the social welfare function is explicitly ethical—its purpose is to make interpersonal welfare judgments—but that did not mean that welfare economics was not a legitimate part of economic analysis. The argument was that this is no different than what is regularly done in other areas of economic analysis such as consumer choice theory. The economist takes the “tastes” (the utility function) of the consumer as given and these tastes reflect “values”, but they are the values of the consumer and not necessarily the values of the economic analyst. As Samuelson put it in *Foundations*:

> It is a legitimate exercise of economic analysis to examine the consequences of various value judgments, whether or not they are shared by the theorist, just as the study of comparative ethics is itself a science like any other branch of anthropology. If it is appropriate for the economist to analyze the way Robinson Crusoe directs production so as to maximize his (curious) preferences, the economist does not thereby commit himself to those tastes or inquire concerning the manner in which they were or ought to have been formed (Samuelson 1947, 220).

For Samuelson, the fact that welfare economics—at least welfare economics that employs a SWF—involves value judgments does not prevent it from being a legitimate part of economic analysis, but it does mean that the results produced by welfare economics are not empirically “meaningful” in the positivistic sense employed throughout *Foundations*. As he says:

> It is only fair to point out, however, that the theorems enunciated under the heading of welfare economics are not meaningful propositions or hypotheses in the technical sense. For they represent the deductive implications of assumptions which are not themselves meaningful refutable hypotheses about reality (Samuelson 1947, 220-221).

Again, this is entirely consistent with the analytical framework of *Foundations*: the mathematical machinery facilitates economic

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6 As Samuelson put it, the SWF “is supposed to characterize some ethical belief—that of a benevolent despot, or a complete egotist, or ‘all men of good will’, a misanthrope, a state, race, or group mind, God, etc.” (Samuelson 1947, 221).
analysis—deductions and theorems from various assumptions—but the cognitive status of the theorems so deduced, whether they are empirically meaningful or not, depends on the empirical content of the underlying assumptions: “By a meaningful theorem I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions” (Samuelson 1947, 4). The new welfare economics involving a SWF is valid and useful economic analysis, although not strictly positive economic science under Samuelson’s definition of empirical science.

It is difficult to compare Blaug’s argument that welfare economics necessarily involves value judgments (discussed below) with the role that Bergson and Samuelson assign to value judgments in the new welfare economics. The problem is that Blaug and the founders of the new welfare economics focus on different parts of the theory. For Bergson and Samuelson welfare economics necessarily requires interpersonal utility comparisons, and therefore a SWF, and that is where the ethics enters into the analysis. In other words, what makes a particular piece of economic analysis “welfare economics” is the social welfare function and the question of whether the new welfare economics involves value judgments reduces to the question of the cognitive status of the social welfare function itself; Bergson and Samuelson were relatively silent about the cognitive status of the various parts of economic theory involved in welfare economics other than (or prior to) the SWF, such as the concept of a Pareto optimal/efficient allocation, the contract curve, and the associated fundamental theorems. Implicitly it seems they considered the cognitive status of such concepts to be the same as that of consumer choice theory and the other parts of economic analysis that new welfare economics is associated with, but these parts of economic theory raise much more general methodological questions than the question of value judgments specific to the new welfare economics.

This is not the case for Blaug. In fact Blaug has very little to say about the SWF. For Blaug, welfare economics is the use of Pareto optimality and the associated fundamental theorems to analyze various questions about economic institutions and microeconomic policy, and that often has nothing to do with a SWF, but rather involves the direct application of PO, PPI, and the fundamental theorems. After Bergson and Samuelson, most economists agreed that SWF-based welfare economics was normative and necessarily involved value judgments,
but for Blaug that was not the important methodological question. The important methodological issue for Blaug was the cognitive status of the concept of Pareto optimality itself, and by implication, the parts of welfare economics such as the first and second fundamental theorems that were based on Pareto optimality. This was a part of the new welfare economics that most mainstream economists, following Pareto, considered to be positive economics and devoid of any value judgments. As Blaug explains:

Pareto asserted, in his now famous statement of the conditions of optimality, that perfect competition would automatically maximize *collective ophelimity* [...] in the sense that no reallocation of resources could make anyone better off without at least making one person worse off. As far as he was concerned, this was a proposition of pure economics, which was completely independent of any ethical value judgments (Blaug 1992 [1980], 122).

It was this aspect of Paretian welfare economics, the concept of efficiency and the associated fundamental theorems—“the quaint notion of the ‘new’ welfare economics that propositions about ‘efficiency’ are somehow value-free, while propositions about ‘equity’ are necessarily value laden” (Blaug 1978, 626)—that concerned Blaug, not the SWF that housed the normativity for Bergson and Samuelson.

Blaug made a multi-pronged attack on the claim that the Paretian welfare economics of the first and second fundamental theorems was, or could reasonably be made into, strictly positive economic science. Perhaps his most direct argument was that the concept of Pareto optimality itself involved value judgments. He noted three separate ways in which the Paretian concept of efficiency is value-laden.

[...] the concept of a Pareto-optimal allocation of resources is predicated on three assumptions which are undeniably judgments of values: (1) that every individual is the best judge of his own welfare; (2) that the social welfare is defined only in terms of the welfare of individuals; and (3) that the welfare of individuals may not be compared (Blaug 1978, 626).

Blaug’s interpretation of statements (1) and (2) seems to be correct as long as one identifies a socially optimal allocation with a socially desirable (or, even more ethics-laden “good”) allocation. The statement that a more desirable allocation of resources is one with more welfare as judged by the relevant individuals and only the relevant individuals
clearly involves value judgments. The presumption that efficiency is (perhaps even ethically) desirable is certainly suggested by the profession’s traditional rhetoric—an allocation where one person cannot be made better off without making someone else worse off is called “efficient”, not “horrible” or some other term with a negative connotation—but as we will see in the next section, not everyone involved in the debate surrounding the new welfare economics would attach such meaning to efficiency. On the other hand, (3) might be considered an empirical statement—for example, if utility is a mental state measurable by modern neurophysiological scanning techniques then it might be empirically testable—but it could also be a value judgment, although not an ethical value judgment. As discussed below, Blaug himself, following Ernest Nagel, makes a distinction between methodological judgments and (ethical) value judgments (Blaug 1992 [1980], 114), and (3) seems to clearly be a judgment about the methodological limitations of our scientific tools, not an ethical judgment.

The second of Blaug’s arguments stems from his understanding of philosophy of science as a normative enterprise. Economists, following Robbins (1935 [1932]), Milton Friedman (1953), and others, have traditionally equated “normative” with “ethical”—equating what “ought to be” with what “ought to be on moral grounds”—and while ethical normativity is one kind of normativity, it is certainly not the only kind. Norms involve rules and action-guiding principles; they are prescriptive, but not all prescriptions prescribe that which is moral. Philosophy of natural science has traditionally been a normative discipline specifying what scientists ought to do in order to be good scientists (to find truth, or to save the phenomena, or to uncover the hidden causal forces, or what have you), so when Blaug the economic methodologist says that economists should make bold conjectures and subject them to severe empirical tests, he is making a normative claim. This understanding of the tight connection between scientific practice (what “is” in science) and methodological norms (what scientists “ought” to do) has often led Blaug to talk down the strict dichotomy between positive and normative that economists have long endorsed (if not always practiced) and this in turn adds another pathway for value judgments to enter into welfare economics.\(^7\) Since “Science as a social enterprise cannot

\(^7\) For a general discussion of the positive-normative dichotomy in economics, see Hands 2012a.
function without methodological judgments" (Blaug 1992 [1980], 114) methodological value judgments like (3) above are bound to be involved in even the most scientific welfare economics. Of course, this is an argument that is not restricted to the new welfare economics, or even welfare economics in general; it applies to all economic (and all other) science.  

Blaug also makes several arguments for an even broader interaction between positive/facts and normative/values in welfare (and other) economics. The facts of the matter often have an important impact on our moral (and other) normative evaluations and thus these normative appraisals may be much more amenable to rational criticism and reasoned revision than traditionally supposed. If so, the concept of economic efficiency could involve value judgments, even moral value judgments, and yet be subject to scrutiny and revision by reason and evidence. As Blaug explains:

We have overstated the case in suggesting that normative judgments are the sort of judgments that are never amenable to rational discussion designed to reconcile whatever differences there are between people. Even if Hume is right in denying that “ought” can be logically deduced from “is”, and of course “is” from “ought”, there is no denying that “oughts” are powerfully influenced by “ises” and that the values we hold almost always depend on a whole series of factual beliefs (Blaug 1992 [1980], 115).

Although Blaug's various points do not come together to produce a single knock-down argument for the normativity of the new welfare economics, taken in total they add up to a fairly serious indictment of the view that the new welfare economics, at least sans the SWF, is just one of many cognitively equivalent subfields within positive economic science and involves no value judgments. As Blaug summarized his view:

The concept of Pareto optimality and the associated concept of PPIs, should not be confused with the theorems of positive economics. If this implies that economists must give up the notion that there are purely technical, value-free efficiency arguments for certain

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8 As I pointed out many years ago (Hands 1984), Blaug's appreciation of the interaction between methodology and the actual historical practice of science was not always apparent in either his methodological writings or in his work in the history of economic thought. See, for example, our exchange on Blaug's interpretation of the Keynesian revolution: Blaug 1976, 1990a, 1991; and Hands 1990.
economic changes, and indeed that the very terms “efficient” and “inefficient” are terms of normative and not positive economics, so much the better: immense confusion has been sown by the pretense that we can pronounce “scientifically” on matters of “efficiency” without committing ourselves to any value judgments (Blaug 1992 [1980], 127).

**Blaug-Hennipman Debate on the Normativity of New Welfare Economics**

This section will discuss the Blaug versus Hennipman exchange over the normativity of the new welfare economics during the early 1990s. Although the arguments involved in that exchange will be the main focus, the discussion starts a few decades earlier with a paper on welfare economics by G. C. (Chris) Archibald published in 1959. The views expressed in the Archibald paper are frequently repeated by Hennipman—in fact Blaug refers to “the Archibald-Hennipman argument” (Blaug 1992 [1980], 126)—but that is not the only reason for examining Archibald’s paper. In addition, Archibald’s interpretation of the new welfare economics has a special significance because he, like Blaug, was attempting to formulate an interpretation of modern economics (including welfare economics) consistent with Karl Popper’s philosophy of science. This Popperian connection puts a particularly intriguing methodological spin on the differences between the Blaug view and the Archibald-Hennipman view of Paretoan welfare economics.

Archibald’s paper was a product of the LSE staff seminar in ‘Methodology, Measurement, and Testing’ (the M²T seminar) that Richard Lipsey began in 1957 (De Marchi 1988). The seminar was both a product of, and a response to, Robbins’s interpretation of economic methodology. As Neil De Marchi explained in his discussion of the methodological impact of the seminar:

This group—really a palace guard, since many had been students under Robbins and owed their elevation to his influence—sought to recast economic knowledge in falsifiable form and proclaim their independence from the dogma, in which they had been schooled, that quantification is not only difficult but unnecessary.

These may sound like sweeping aspirations, but the goal was actually very specific: to replace Robbins’s *Nature and Significance of Economic Science* as the dominant source of methodological ideas for British economists and to argue for the notion that they alone cannot be a sufficient basis for policy conclusions.
Expressed positively, economics should become a quantified science (De Marchi 1988, 141).

Archibald in particular—who was greatly influenced by Samuelson’s *Foundations*—sought to use Samuelson's analytical framework as the basis for a purely scientific version of the new welfare economics that could restore the field to its respected place within positive economic science after being dethroned by Robbins’s arguments against interpersonal utility comparisons. Robbins made a convincing argument that the old welfare economics had no place in economic science, but if the new welfare economics was necessarily normative—as Samuelson and Bergson had argued for welfare economics involving the SWF—then the new welfare economics would be scientifically no better than the old. On the other hand, if Pareto optimality/efficiency and the fundamental theorems did not involve value judgments, then the new welfare economics (at least sans SWF) would have a rightful place along with the rest of positive economic science.

Archibald’s argument was wide-ranging, but I will focus on two interrelated points that also show up later in Hennipman's papers. The first is that although economists often talk about welfare economics as if it tells us—or policy makers—what we “ought to do” for the social good, there is nothing about the theory itself that requires, or even suggests, that interpretation. As Archibald put it, his argument will not satisfy those who, because they demand of welfare economics that it ‘tell them what to do’, understand by welfare economics a discipline necessarily founded upon value judgments, and therefore assert simply that my use of the term is not theirs (Archibald 1959, 316).

For Archibald, and moving to his second point, welfare economics is really no different than other areas of economics such as consumer choice theory or the theory of the firm. Following Robbins’s definition of economics as “the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” (Robbins 1935 [1932], 16) the economist starts with *given* wants and then “asks how their progress towards their objectives is conditioned by the scarcity of means” (Robbins 1935 [1932], 24). Welfare economics is just an extension of this inquiry into the question of economic efficiency. A PPI allocation is one in which at least one person could be made better off without making someone else worse off.
and that is an inefficient way of satisfying given wants with the available means. If the first fundamental theorem of welfare economics demonstrates that every CE is PO, then it shows that competitive markets are an efficient way to satisfy given wants with scarce means. Why should such a theorem have any less cognitive significance than a well-established area within economic science such as consumer choice theory? As Archibald explains, given Robbins’s definition, it should not:

[…] it is hard to understand why welfare economics should be set apart. If we enquire into the efficiency of alternative arrangements for satisfying given wants, why is a judgment about these wants a necessary foundation for the theorems we discover? [...] The sensible procedure in welfare economics appears quite simply to be this: we take, as an interesting criterion, the choice-system of the individuals, and ask how different arrangements alter the available choices. That we call the choice-criterion an index of welfare is no value judgment or prescriptive implication (Archibald 1959, 317).

As he summarized the argument:

The enquiries we label “welfare economics” are positive enquiries into the effects on certain indexes of alternative arrangements [...] No value judgments need precede the enquiry; [...] and the conclusions have no prescriptive force. The theorems of welfare economics are thus theorems in positive economics; they are concerned with the relationship between given ends and available means (Archibald 1959, 320).

Hennipman repeats both of Archibald's arguments, but also adds some additional criticisms of—and a new twist on—normative interpretations such as Blaug’s. Like Archibald, he admits that economists in fact use welfare economics in normative ways:

The characterization of welfare economics as normative has undoubtedly a considerable descriptive validity. As Blaug points out,

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9 One aspect of Archibald's argument that I will not discuss because it would carry us too far a field is his use of a version of revealed preference theory to characterize preference, choice, and welfare. This topic has recently received a lot of attention, in part because of its methodological use by Gul and Pesendorfer (2008), and raises a number of methodological issues well beyond the task at hand. For criticism of Archibald's particular use of revealed preference, see Mongin 2006, 272-275; and for more general discussions of the relationship between contemporary revealed preference theory and welfare economics, see Hands 2012b; and Hausman 2008, 2012. For a discussion of the relationship between Blaug's methodology and Gul and Pesendorfer’s position, see Hands 2013.
economists do in fact judge how practical problems concerning allocation should be solved (Hennipman 1992, 434).

Similarly, he explains that the “neutral” interpretation of concepts like economic efficiency is just a particular version of an instrumental approach to finding efficient means for achieving given ends. The choice between the “normativist” and the “neutralist” reading of Pareto optimality is “a free methodological choice” (Hennipman 1992, 434).10

The neutral approach takes allocative efficiency as a given end in the sense that it may be a desired objective, without itself endorsing the Paretian value judgments […] In consequence, the positive theory does not aim at offering categorical policy prescriptions, it only gives recommendations that are conditional on the acceptance of the postulated goal […] Propositions of this kind are based on economic judgments […] which, from the policy point of view, are known as instrumental judgments […] This simple scheme definitely refutes the view that welfare economics is necessarily normative because it ‘deals with policy’ (Hennipman 1992, 429-430).

Hennipman also responds to Blaug’s comments on the three assumptions that make Pareto optimality a normative concept (discussed in the previous section) and his remarks are quite similar to those I made above;

Blaug’s description of the first and second assumptions makes sense if it is understood as tacitly presupposing that Pareto optimality is an ethical concept and a favoured policy objective […] however, the third assumption is not ‘undeniably’ a value judgment (Hennipman 1992, 416).

In addition he criticizes Blaug’s presupposition that anything that brings about an increase in welfare is necessarily desirable, by arguing that as a factual matter welfare in economics has traditionally meant just what people prefer (solely the subjective judgment of the individual) and has not been considered desirable in any higher, universal, or objective sense (Hennipman 1992, 420-421).

10 Hennipman also follows Archibald in using the term “essentialism” for the view that welfare economics must necessarily be normative—a pretty damning criticism of a Popperian.
Perhaps Hennipman’s most original criticism is a new twist on Blaug’s normativity argument. Hennipman criticizes Blaug’s view that Pareto optimality is normative because it favors and privileges efficiency over any other standard one might choose to employ in the evaluation of various resource allocations. It becomes not only a normative standard about what ought to be done, but the normative standard.

The real danger of such an effect would arguably arise if, following Blaug, economists were to attach an ethical meaning to efficiency, acclaiming it unreservedly as desirable. This would be most injudicious because while one may regard efficiency in many cases as meritorious, it is not always true that efficiency is “more desirable” than inefficiency. In general its moral value obviously depends on the ends, means and ways of action. One may very well prefer an inefficient to an efficient Gestapo (Hennipman 1992, 422).

Later in the same paper Hennipman offers yet another twist by making the case that since welfare economists must, on his reading of Blaug, know what is ethically good—they must have a “distinctive capability”—and the only “remotely feasible justification” “stems directly from the intrinsic desirability of Pareto optimality” (Hennipman 1992, 435). He then spends five pages criticizing the “normative pretensions” of this “ethical desirability postulate”.

Finally, in his reply (1993) to Blaug’s (1993) comment on his paper, Hennipman challenges Blaug’s notion of methodological value judgments. He accuses Blaug of “semantic novelty” by using, and confusing, two different notions of the normative: ethical and methodological. He argues that Blaug first “defines, in accordance with normal usage, normative as ‘involving ethical propositions’” (Hennipman 1993, 291), but then changes to “ought statements” and “appraising judgments” of a methodological sort. For Hennipman this is “sidetracking the debate onto an irrelevant line” which “evades the problem the whole controversy is about, i.e., the ethical commitment of welfare economics” (Hennipman 1993, 292). Hennipman ends his reply with a “dismal epilogue” where he closes with some rather harsh remarks about the “thankless task” of getting Blaug to “see the light” (Hennipman 1993, 294). Needless to say, Hennipman’s remarks did not find any common ground with Blaug’s position, in fact they seemed to push the two economists farther apart.

Unfortunately, Blaug’s comment on Hennipman’s 1992 paper was only two and a half pages long and seemed to muddy the waters still...
more. For example, regarding the question of the dual meaning of normative—ethical and methodological—Blaug makes both of the following statements in his brief comment: (i) “‘Normative’ economics, however, involves ethical propositions about what is good or bad which can never be in the nature of the case decisively resolved by factual evidence” (Blaug 1993, 125), and (ii) “But methodological judgments are just as normative as value judgments, that is, facts and more facts can never persuade us to abandon them” (Blaug 1993, 128). I believe I understand what Blaug meant in both of these sentences, but it takes some serious reading between the lines. By “normative economics” in (i), Blaug probably meant “what most economists have traditionally considered normative economics to be”, and not “any economics that has a normative component must be ethical”, but as I say, it is not entirely clear. It seems that both Hennipman and Blaug are talking at cross purposes and never find any semantic common ground on which they could clearly agree or disagree. One can of course debate whether “the new welfare economics is normative in the sense of necessarily presupposing ethical value judgments” or whether “the new welfare economics is strictly positive in the sense of not presupposing any normative judgments of any type”. Either one of these is an interesting and important debate, and one in which many economists would come down on both sides, but an exchange—particularly a heated exchange—where neither author is clear about which question is being debated is not only one in which nothing will be resolved, it is one in which readers will not even be clear on the positions of the two authors.

Similar remarks can be made for Blaug’s discussion of the three value-laden assumptions of Pareto optimality. Regarding (1), that “every individual is the best judge of his or her best interests” (consumer sovereignty), Blaug says:

The first of these three postulates is clearly a value judgment in the sense that no observations about consumer behaviour could ever force us to abandon the belief that consumers themselves know best what is good for them. Since value judgments belong to normative economics Paretoian welfare economics is necessarily normative.

Although Blaug and Hennipman agree on very little, they both do seem to believe that “every individual is the best judge of his or her best interest” is equivalent to “consumer sovereignty”, which is ironic, since it is not obvious the two terms mean the same thing. The latter seems to mean that the consumer is free to choose, and the former seems to mean that what they choose is always in their best interest; these appear to be entirely different things.
In one sense, this completes, my case and no more need to be said to vindicate my position (Blaug 1993, 125).

Again, this only seems to confuse the issues. If it is possible to get a consensus on what the expression “good for them” means—say increases the survival of the person's genes—then it may in fact be possible to determine whether a particular individual is consuming that which is good for them or not. On the other hand, if one is assuming the good in “good for them” is morally good, but based on subjective personal ethical values, then the problem is methodological; we observe what they consume but we do not have access to their mental states that would allow us to determine whether what they consume matches up with what they think is ethically good. Finally, if one is assuming a universal ethical good in “good for them”, then the statement is an ethically normative statement in the sense in which Blaug seems to be using the term “normative economics” a few sentences above this quote. So depending on one’s definition of the relevant terms the proposition could be positive, methodologically normative, or ethically normative. It is just not clear.

Similar remarks could be made for his comments on the other two assumptions (2) and (3), but I will not go through the details. The bottom line is that neither Blaug nor Hennipman offered an entirely persuasive defense of their position, and perhaps worse, actually seem to have made the issues, and their positions on the issues, even less clear. If one goes back before this exchange and reads Archibald (1959) and Blaug (1978) one is clear about the two positions; one may agree with one rather than the other, or parts of each, or even support something different than either one, but one understands what the authors are saying about the new welfare economics. After the Hennipman-Blaug debate, that no longer seems to be the case.

**WHAT BLAUG MIGHT HAVE SAID TO HENNIPMAN (AND ARCHIBALD)**

In this section, I will try to identify the roots of some of the communication problems in the Blaug-Hennipman debate and also to offer a few arguments that Blaug might have made in response to Hennipman (and in some cases Archibald), but for whatever reason did not make. There are five comments in total, although the first two overlap to some extent.
The term “normative” is the source of much confusion in this exchange and in other discussions of the foundations of welfare economics. Similar remarks can be made about “value judgments”, but since the case for “normative” is a bit clearer, I will focus on that. Economists have traditionally equated “normative” with “ethical”. As a result of the influence of experimental and behavioral economics, this may currently be changing, but the traditional interpretation of the distinction between “positive” and “normative” in economics has been that positive is about “what is” and normative is about what “ought to be in order to be moral”. As Hennipman noted, in the normal usage of economists ‘normative’ means “involving ethical propositions” (1993, 291). There is a long, as yet unwritten, story about how this came to be within the economics profession, but it is in fact the case, and it leads to numerous confusions. The source of the problem, as noted briefly above, is that outside of economics, that which is “normative” is necessarily norm-guided, but the norms need not be moral norms. They could be norms of rationality, or epistemology, or many other things, rather than morality. Blaug, with his knowledge of normative philosophy of natural science in general, and his commitment to Popperian scientific norms in particular, recognized that welfare economics could be normative without being ethical, and used this fact in his argument about methodological value judgments. But this use of normative is quite alien to most economists and Hennipman’s remarks clearly reflect this. That said, Blaug does not help matters much because he never clearly explains that ethically normative is just one particular instantiation of normative, and often slips into the standard economist usage himself. Some definitional groundwork would have been very useful and it would have decreased the amount of talking at cross-purposes. Blaug’s arguments about the role of methodological norms in strictly positive science are sound, but they are never entirely clear because of the blinders imposed by the traditional way the term normative has been used in economics.

2. Following on the previous point, if Blaug had clarified the diversity of ways in which the term ‘normative’ is used outside of economics, he would have opened the door to an additional argument regarding how a type of (non-ethical) normativity enters into welfare
economics. Both Archibald and Hennipman argued that talking about an efficient allocation in welfare economics was just like talking about the optimal bundle in consumer choice theory. Rational choice theory in general and consumer choice theory in particular is just an application of instrumental rationality—selecting the most efficient means for achieving any given ends. Their argument was that the new welfare economics was no less scientific, and thus no more normative, than the ordinal utility theory. But why should we think that ordinal utility is itself devoid of normative considerations. Does ordinal utility theory tell us “what is” by empirically discovering the underlying utility functions that cause choice behavior? Perhaps there are some specific approaches—revealed preference or imputed valuation—that try to do this, but this is not standard textbook economics. The preferences and utility functions that drive most exercises in choice theory are not given by nature or by the best available evidence—they are posited—and the theoretical exercise simply draws out the deductive implications of that posit. Most of the “given” wants of economics are posited wants rather than found wants, and they are not just any-old posited wants. They are posited rational wants. They are wants embodied in well-behaved, complete and transitive, preferences with sufficient structure to support the existence of an ordinal utility function defined over the entire choice space. So where do these posited restrictions come from? They involve rationality. The posited rational wants are motivated by our normative value judgments about what one “ought to do in order to be rational”. There is a reason that many elementary textbooks call the transitivity assumption “rationality”—it originates in our normative intuitions about the essential nature of rationality. This makes the rational choice foundations of welfare economics, and thus welfare economics, laden with normativity.13 They are norms of rationality not morality, and the presence of such normative influence does not prevent the resulting economic theory from being scientifically adequate, but they are norms nonetheless. This is not going to win over those—perhaps like Hennipman—who only want to debate the question of whether the new welfare economics is ethically normative, but it seems to be a nice way to make Blaug’s point that the new welfare economics is not strictly positive in the way that Archibald, Hennipman, and others have argued.

13 A related argument about rationality and normativity in choice theory is given in Hausman and McPherson 2006.
3. One simple thing that would clarify the discussion would be to decide what “welfare economics” is, before entering into a debate about whether it is normative or positive. It seems that the different economists discussed above have quite different definitions. For Archibald and Hennipman it seems that welfare economics is about labeling various resource allocations as efficient or inefficient. The theory says “X is an efficient allocation” (in the world or in a model) and that is the end of it. Nothing else seems to follow from the fact that an allocation is so labeled. It does not necessarily say that it is better than other allocations, or that the government or anyone else should try to bring X about. Of course they consistently note that adding extra normative judgments about efficient allocations is always permissible, but they are not necessary implications of the analysis—and it is still “welfare economics” even if no such additional normative structure is added. Although it is not entirely clear how Bergson or Samuelson would view the act of labeling efficient allocations, it is clear that just doing so would not be welfare economics; for them welfare economics involves the interpersonal utility comparisons provided by a SWF. Unlike Archibald and Hennipman, Blaug obviously defines welfare economics in the traditional way as the microeconomic theory that provides tools for the evaluation of various economic policies, institutional arrangements, and the allocation of economic resources. And although Blaug’s definition is more consistent with professional practice, it is not necessary to decide which definition is more descriptively accurate. The issue is simply that before one is involved in a debate over whether the new welfare economics is necessarily normative it would be useful to agree on what welfare economics is (in addition to, as noted above, what normative means).

4. Blaug should have responded to Hennipman’s claim that by making Pareto optimality (ethically) normative, Blaug committed all economists to this, and only this, notion of the good or what the society ought to do. This is, well, silly. Pointing out that Pareto optimality involves ethical values only draws attention to the fact that ethical values are involved, and then once recognized, the door is open to consideration of other possible ethical values.\textsuperscript{14} Blaug is not saying that Pareto optimality entails the universal notion of what is good; he is

\textsuperscript{14} I would note this is the general approach of Hausman and McPherson (2006).
simply saying that it entails some notion of the good (basically that the satisfaction of individual preferences is good), and by implication other ethical judgments might be considered. This is a very weak argument by Hennipman and Blaug should have pointed it out in his comment.

5. Finally, I close by returning to the founders of the new welfare economics: Bergson and Samuelson. It seems fairly easy to reconcile Blaug’s view with the views of these founders. After all, they too define welfare economics in terms of the evaluation of policies and institutions, they just do not consider the mere mention of Pareto optimality to be sufficient to make a particular piece of economic analysis into welfare economics. A particular piece of theorizing becomes welfare economics when one adds a SWF: a value judgment that such allocations are a good thing and can be used to defend policies on that basis. Although Bergson and Samuelson are silent about the question of non-ethical normative judgments such as rationality in their writings on the new welfare economics, one could certainly add such non-ethical normativity to their general framework. One could argue, consistent with the case that Blaug seems to want to make, that welfare economics is not strictly positive because the choice theory on which welfare economics rests is laden with normative notions of rationality. This would take care of Blaug’s problem about “the quaint notion of the ‘new’ welfare economics that propositions about ‘efficiency’ are somehow value-free, while propositions about ‘equity’ are necessarily value laden” (Blaug 1978, 626), without committing the basic technical machinery of welfare economics or the fundamental theorems to the charge of being ethically normative. The theorems, as in the view of Archibald and Hennipman, would be methodologically just like consumer choice theory, it is just that it too involves normative—though not ethical—considerations about what one ought to do in order to be rational. Then when one moves beyond the background theoretical machinery to real welfare economics—that which judges institutions and suggests policy—here the ethical considerations would be explicit (and necessary). The new welfare economics—defined as welfare economics has traditionally been defined—would in fact (necessarily) involve ethical value judgments as argued by Bergson, Samuelson, and Blaug. This seems to answer Blaug’s main concerns and make his most important points, and it does so without contradicting anything in the stated positions of either Bergson or Samuelson.
CONCLUSION
This paper has tried to clarify the various points of view in the long-standing debate over the normative character of the new welfare economics: in general and with particular reference to Blaug's debate with Hennipman. The original interpretation of Bergson and Samuelson, as well as the strictly positive interpretation of Archibald and Hennipman, were examined in detail. The exchange between Blaug and Hennipman was also discussed in detail, but a conclusive assessment was not reached because there was such a lack of agreement about the starting point, purpose of the debate, and even the relevant definitions, that both authors ended up often talking past, rather than seriously addressing, the arguments of the other economist. Finally, in section three, five additional points were made to help explain some of the sources of confusion in the debate and also to offer a few arguments that Blaug might have made in his exchange with Hennipman and discussion of Archibald.

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Formalism, rationality, and evidence: the case of behavioural economics

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Abstract: Mark Blaug played a central role in the development of the field of the methodology of economics, alongside his theoretical work and contributions to the history of economic thought. The purpose, in this article, is to focus on his contributions to the topic of ‘formalism in economics’, in relation to his methodological commentaries on the Popperian and Lakatosian approaches to the philosophy of science. In Blaugian spirit, the discussion is related to economic theory and draws on the history of economic thought. The argument focuses on the troublesome interface between theoretical and applied economics in mainstream economics. The article includes, as a case study, an assessment of new behavioural economics in Popperian and Lakatosian terms. The conclusion is that such an appraisal exercise—i.e., whether the research programme is progressive or degenerative—is clouded by the interface between the form of empiricism promoted by Popper and Lakatos and the methodological framework of mainstream economics. No conclusion is feasible independent of methodological approach.

Keywords: behavioural economics, formalism, experimental evidence, economic rationality, empiricism, falsificationism, methodology of research programmes

JEL Classification: A11, B20, B41, D03

Mark Blaug was well known both for his promotion of a Popperian/Lakatosian approach to the methodology of economics and for his critique of mathematical formalism. While his thinking evolved over the years—as you would expect for such a subtle thinker and prodigiously-well-read scholar—these were consistent themes over his long years of leading and contributing to thinking on methodology.

Blaug’s stance directly addressed the emergence of a juxtaposition during the twentieth century between two conflicting trends in
economics: on the one hand the idea that theory should be appraised by reference to the evidence, and on the other hand a form of theory which eluded definitive direct testing. Furthermore, of the two trends it was the second which had become increasingly dominant in economics.

In addition to Blaug’s philosophical interest, this juxtaposition has attracted particular attention in the field of experimental economics and its relations with new behavioural economics. The empirical results of experimental economics at times seem to falsify key elements of pure theory in mainstream economics. Yet, amending theory in order to take this into account, particularly with input from psychology, has run up against the strictures of mathematical formalism.

In the Preface to the second edition of *The methodology of economics* (1992 [1980]), Blaug notes that he had contemplated adding material on new developments in economics, such as experimental economics. But he had decided against this, based on his “disinclination to rush in where angels fear to tread” (Blaug 1992, xii). Still this is what—with some trepidation—is proposed for the present contribution.

The financial crisis has provided new impetus to behavioural economics, in the search for an explanation for events which would seem to constitute massive falsifying evidence to a body of theory which presumed markets to be efficient and equilibrating. In this article, we will consider the extent to which new behavioural economics satisfies the Lakatosian criterion for a progressive research programme, namely the capacity to predict novel facts. More generally, we will consider how far the developing field of behavioural economics addresses Blaug’s critique of formalism in mainstream economics.

In what follows, a brief account will be given of Blaug’s Popperian/Lakatosian methodology in relation to his views on the formalisation of economic theory. We will then explore the tensions which have persisted in mainstream economics between pure theory and applied economics. In considering how mainstream economics has evolved in recent decades—including paying more attention to experimental evidence which seems to shed light on the financial crisis—we will draw on Blaug’s views about economic rationality; a notion which is central to the evolution of new behavioural economics. We will then consider the methodological issues surrounding experiments in economics and how far the use of experimental evidence addresses Blaug’s critique of mainstream economics.
Blaug’s Popperian Methodology

Blaug’s methodological position is summed up by him in the final sentences of both editions of his *Methodology of economics*:

> the ultimate question we can and indeed must pose about any research program is the one made familiar by Popper: what events, if they materialized, would lead us to reject that program? A program that cannot meet that question has fallen short of the highest standards that scientific knowledge can attain (Blaug 1992, 248).

One of the hallmarks of Blaug’s methodology was his espousal of falsificationism. Blaug observed that economists paid lip-service to falsificationism while practising verificationism when seeking empirical support for theoretical conclusions. Blaug’s criticism of the disparity between what economists said they were doing and what they were actually doing was a contribution in itself, to which we will return below.

Blaug was careful not to espouse naïve falsificationism, being well aware of the ambiguities of testing procedures such as the Duhem-Quine problem. Any hypothesis being tested incorporates a collection of sub-hypotheses, both theoretical and in terms of mathematical expression and selection of data. It is therefore difficult to identify what precisely has been falsified by an empirical test. Popper himself had been well aware of these ambiguities, proposing a series of conventions for good scientific practice to discourage ‘immunising stratagems’ which would allow scientists to maintain theories in the face of falsifying evidence. Ambiguities in testing also result from the openness of physical systems with respect to observation. Popper (1982) argued that the process of observation was performative, itself changing the physical world; he gives the example of the drawing of a map of the world, which by being created changes the physical reality the map is designed to represent. Like Popper, Blaug was not a purist: “we want to gain knowledge of the world, even if it is only fallible knowledge” (Blaug 1992, 20). Nevertheless he was adamant that, for theory to be worth having, it had to be able to stand up to empirical evidence, which also meant that it could fall by evidence.

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1 Richard Lipsey recalls that—in conversation—Mark Blaug said he would be happy with a less stringent but more tractable requirement, namely that economists be prepared to specify what evidence would *conflict* with their theories (it being a necessary condition for a theory to have empirical content that it not be consistent with all possible observations).
But the practice of verificationism had been given some methodological respectability by Lakatos (1970), who sanctioned disregard of contrary evidence on ‘infant-industry’ grounds as theory was developed, as well as appearing to sanction the protection of ‘hard-core’ principles from testing. Similarly, Blaug discusses how Popper himself allowed for degrees of corroboration (rather than a dualistic divide between falsification and verification). Blaug saw the subsequent popularity of Kuhnian ideas as an extreme version of this—a complete relativism, without any extra-paradigmatic criteria for appraisal. Blaug (1992, 42-47) was quite explicit that his methodological position was monist—it was possible and desirable to establish one best set of standards for appraisal. He thus lauded Lakatos’s criterion of appraisal: the capacity to predict novel facts (see further Blaug 1991).

THE RISE OF FORMALISM IN NEOCLASSICAL ECONOMICS

Blaug maintained his critique of economics as not following Popper’s proposed conventions for robust empirical testing. But the different problem of the absence of testing altogether increasingly became the focus of this criticism. By formalism Blaug meant the prioritising of the form of a theory over its content. That form need not be mathematical, although that is the formalism most evident in mainstream economics (Blaug 1999, 258). Where Blaug gave the highest priority to empirical appraisal—through which theories might be rejected—formalists normally treat empirical testing merely as something which might be done in principle or—in the extreme—as irrelevant (see Hahn 1981). Blaug (1999) charted the rise of formalism in mainstream economics in the second half of the twentieth century. General equilibrium theory, a particular type of formalism, had come to dominate the discipline. It employed a deductivist mathematical approach, built on a set of axioms concerning rational individual economic behaviour.

During this period, theorising and testing were increasingly being treated as separate activities, in spite of Popper’s proposed conventions (Blaug 1999). Popper had argued for theorising to evolve by a process of conjecture and refutation, i.e., of testing successive narrow hypotheses against facts, with the outcome influencing the formulation of future conjectures. While formalism had encouraged ever more reductionism in general equilibrium theory, the axioms were taken to be self-evidently true. Further, any testing of propositions deduced from the axioms was riddled with the Duhem-Quine problem. If the data appeared to falsify
the theory, what exactly had been falsified? Indeed it was difficult to discriminate between theories empirically, allowing the dominance of formal theory to persist.

Rational expectations theory addressed this bifurcation head on by defining all actual states as equilibrium states. Hence, not only did economists engage in empirical estimation and prediction with respect to formal theory, but so, effectively, did economic agents as well. While Sargent in particular struggled to deal with the circularity involved in this idea, the logical issues were never resolved in such a way that the theory would meet the deductivist-axiomatic requirements of general equilibrium formalism as well as the empirical estimation procedures that were internal to the theory and the basis for prediction (Sent 1998). While apparently falsificationist, the approach was never overtly dropped on the basis of its unimpressive prediction record, although it was not pursued further. Nevertheless, rational expectations remain embedded in mainstream macroeconomic theory.

But more recently there has been a different type of confrontation between pure theory and applied work. The rationality principle (constrained optimisation based on the rationality axioms), and thus the formalist structure itself, have been the subject of empirical challenge by experimental evidence. Blaug (1992, 232-233) noted that the anomalies which arose from experimental evidence had been widely dismissed as random perturbations at the micro level. But this is no longer the case. Since then the body of experimental evidence has grown considerably, as has the body of theory in new behavioural economics—to be distinguished from the old behavioural economists (see Sent 2004). A major impetus has been the financial and economic crisis, an event which could reasonably be regarded as an anomaly on a grand scale. It had proved difficult to settle the empirical status of the efficient markets hypothesis and subjective expected utility theory (for reasons encapsulated by the Duhem-Quine problem) but the crisis added weight to those who questioned their validity. In particular, aggregative evidence from financial markets suggested that there were systematic deviations from the results implied by the efficient markets hypothesis, which was founded on the rationality principle (Shiller 2000). These deviations could be explained by psychological factors for which experimental evidence provided support. Akerlof (2002) generalised this approach to behavioural macroeconomics.
Blaug (1992, 233) had concluded that, while only a naïve falsificationist would abandon the mainstream approach purely on the grounds of such anomalies, abandonment was made possible by such alternatives as prospect theory and Herbert Simon’s notion of bounded rationality. The implication was that there might be a new progressive research programme in the making. But there has also been a process of retrenchment (which could be classified as immunising stratagems) as mainstream economists have attempted to explain the crisis in terms of constraints on the operation of an equilibrating market system (restrictions on competition, asymmetric information, and/or distorted incentives). Even among behavioural economists there has been a reluctance to depart significantly from the standard framework. Camerer and his colleagues introduce their substantial behavioural economics reader as follows:

At the core of behavioral economics is the conviction that increasing the realism of the psychology underlying economic analysis will improve the field of economics on its own terms—generating theoretical insights, making better predictions of field phenomena, and suggesting better policy. This conviction does not imply a wholesale rejection of the neoclassical approach to economics based on utility maximization, equilibrium, and efficiency. The neoclassical approach is useful because it provides economists with a theoretical framework that can be applied to almost any form of economic (and even noneconomic) behavior, and it makes refutable predictions (Camerer, et al. 2004, 1; emphasis in the original).

Yet Berg and Gigerenzer (2010, 134) criticise new behavioural economics precisely for retaining the standard framework, highlighting the consequent ‘very partial commitments to empirical realism’. In what follows we consider how far new behavioural economics comes up to Blaug’s methodological empirical realist standards. We consider this question in terms of the tension in mainstream economics between empirical testing and formalism.

**NEW BEHAVIOURAL ECONOMICS**

We have seen that new behavioural economics introduced psychology into economics on the realist grounds that there was evidence of behaviour which deviated from what was assumed in standard mainstream theory, that is, empirical anomalies. But to satisfy Blaug’s criteria we want to see: a) that the response to these anomalies was
theoretical developments which are not just *ad hoc* adjustments, b) the capacity to predict novel facts or provide novel explanations, c) an abductive approach to theorising such that theoretical developments are driven by reference to evidence, and d) an indication of what would cause behavioural economists to reject their own theories. How far does it live up to Camerer and his colleagues' (2004) own promise of generating refutable predictions, that is, to falsificationism?

Although not all of this evidence was experimental, and not all experimental economics feeds into behavioural economics, there is nevertheless a significant overlap between the two (see Sent 2004). This inter-relationship was reflected in the award of the 2002 Nobel prize jointly to Kahneman for his contributions to behavioural economics and to Smith for his contributions to experimental economics. The experimental evidence appeared to falsify either the rationality axioms or the presumption of optimising behaviour which formed part of the hard core of the mainstream research programme.

Conventionally, as part of the hard core, the rationality principle had been regarded as being exempt from falsification; rationality could be regarded as a metaphysical principle. While Popper saw theory as being built on conjectures rather than axioms, he had supported this exemption from testing for the rationality principle, allowing a significant element of commonality between Popper and Lakatos when it came to economics. Blaug (1992, 231) explained such a surprising stance in terms of Popper's lack of understanding of the significance of the rationality principle for economics. Further, he argued that Popper had not appreciated the significance of the auxiliary hypotheses attached to the assumption of rationality (such as full information) which were adopted to make the principle theoretically tractable.

But what does the experimental evidence signify? Some have argued that definitive empirical tests of the rationality axioms are not feasible (Blaug 1992, 231). If tested by means of experiments, the results would be ambiguous because of the Duhem-Quine problem. It would be impossible to test the rationality principle independently of assumptions about the stability of preferences, for example. But over the last decade experimental economics has become increasingly sophisticated in devices (such as double-blind experiments) to ensure that the hypotheses being tested are sufficiently narrow and precise, and the tests themselves so well-organised in efforts to yield clear results, that they can be said to adhere to Popper's proposals for dealing
with the Duhem-Quine problem (see, e.g., Berg, et al. 2005). In particular, efforts have been made in designing experiments to isolate individuals from social interaction in order to observe self-interested individualistic behaviour.

But such a stratagem may be interpreted in terms of too much isolation. The intention is to make the experiments accord more precisely to the theoretical framework based on methodological individualism and the rationality principle, but that means the experiments are not reflecting evidence of actual behaviour in the different framework of reality (see further Hargreaves Heap 2009). If, for example, individuals are in fact other-regarding, then it is not clear how experimental conclusions about isolated behaviour can explain actual behaviour. Some experimental evidence (as in the ultimatum game) indicates an other-regarding aspect of individual behaviour which can be taken as falsifying evidence with respect to the standard rationality axioms, and also as limiting the relevance of evidence based on experiments designed to abstract from other-regarding behaviour. Other-regarding behaviour—in the form of mass psychology or herd behaviour—provides an important behavioural explanation for the financial crisis (see, e.g., Kirman 2011).

Others have pointed to logical problems in interpreting experimental evidence which aims to identify deviations from a rational optimising benchmark. For example, the presumption that agents rationally optimise on information in order to rationally optimise in choice situations has been shown to collapse in an infinite regress (Winter 1964; Cohen and Dickens 2002). More generally, Berg and Gigerenzer (2010) classify rational optimisation as ‘as-if’ behaviour, and call into question the validity of interpreting experimental evidence and results with respect to a framework where significant ‘as-if’ assumptions are retained. Taking prospect theory as an example, they point to its limited departure from the standard framework in that the experiments presume that risks can be quantified and manipulated in a sophisticated way. These presumptions about the capacity for knowledge appear to be logically inconsistent with the behavioural theory that agents employ heuristics in order to cope with cognitive limitations (see Tversky and Kahneman 1974; Kahneman and Tversky 1979). Such limitations were a core element of Simon’s (1955) earlier development of the concept of bounded rationality.
Cognitive limitations are an important feature of behavioural economics explanations for behaviour which appears to be other-regarding even in a methodologically-individualistic framework. But in fact much of behavioural economics retains the individual rationality framework. Thus, for example, individual behaviour may be like herd behaviour, but only in the sense of putting undue emphasis on past trends (Bikhchandani and Sharma 2001). Shiller's (2000) feedback theory uses the rationality framework as a benchmark for classifying such behaviour as irrational. Similarly, instability in the real economy may be seen as the result of financial instability (exaggerated amplitude of asset price deviations) which arises from self-fulfilling beliefs, confusingly dubbed ‘animal spirits’. This literature explains such beliefs in terms of Keynes’s ‘beauty contest model' of expectations formation. But, even if expectations deviate from what rationality would predict, the individual decision-maker is depicted as forming optimal expectations given cognitive limitations. In any case, for some contributors, the nature and role of cognitive limitations are peripheral to the explanation of financial instability. The important explanatory factor is an exogenous disturbance to beliefs, which can just as easily be explained by sunspots (see, e.g., Farmer and Guo 2004). Retaining the basic rational choice framework is given priority over the explanation of actual behaviour.

The input of psychology into behavioural economics also takes the form of specifying unconventional preferences to which rational choice is applied. Thus, within representative agent models in prospect theory, for example, scope is given for unconventional preferences such as loss aversion (Kahneman and Tversky 1979). This explains behaviour which otherwise appears to be irrational. Similarly, heterogeneous agent models may allow for different groups of market participants with different preferences. In particular, non-professionals may be guided by sentiment, while professional arbitrageurs are guided by rationality. Instability may emerge if sentiment drives markets in a particular direction, although arbitrageurs will normally ensure a return to equilibrium (Baker and Wurgler 2007). But, as advocated by Robbins (1932), the source of preferences is not explored; it is taken as given (see further Binmore and Shaked 2007).

**PROGRESSIVE OR DEGENERATING RESEARCH PROGRAMMES**

As Sent (2004) argues, what distinguished new behavioural economics from old behavioural economics is that the reference point for the
former is always the standard rational-choice framework. Where old behavioural economists absorbed the evidence of deviations from the rational-choice model and developed an alternative framework accordingly, new behavioural economists accepted the rational-choice framework as their hard core, but amended its auxiliary hypotheses by modifying models to allow for (limited) cognitive limitations and unconventional preferences (Earl 2010). As Kahneman (2003, 1469) put it: “Theories in behavioural economics have generally retained the basic architecture of the rational model, adding assumptions about cognitive limitations designed to account for specific anomalies”. Further, anything which cannot be explained in terms of rationality is dualistically classified as irrationality (Altman 2004). This is clearly shown by Akerlof and Shiller (2009); the behavioural explanations they offer for evidence which challenges mainstream theory explicitly refer to such behaviour as either ‘irrational’ or ‘non-economic’. The Lakatosian framework thus seems to be successful in providing a good account of new behavioural economics as protecting the hard core rationality principle. But, while this may have helped in communicating new behavioural economics ideas to mainstream economists, it leaves behavioural economics without its own coherent theoretical foundation (Cohen and Dickens 2002).

How well does new behavioural economics stack up in terms of Lakatos’s appraisal criteria of predicting novel facts and avoiding ad hoc adjustments? There has been a range of critiques of new behavioural economics on the grounds that it can provide ex post explanations for behaviour, but falls short on prediction (see, e.g., Binmore and Shaked 2007). Similarly, Cohen and Dickens introduce their argument for an alternative theoretical framework (evolutionary psychology) as follows,

the policy influence of [behavioural economics] is limited by its inability to predict circumstances in which anomalous behavior will arise (other than in those sorts of circumstances in which it has been observed before) or how it will respond to policy changes (Cohen and Dickens 2002, 335).

They then proceed to discuss bounded rationality as an ad hoc adjustment.

Mark Blaug (1992) encourages consideration of whether a research programme is progressive or degenerating by means of comparison. As Backhouse (1991, 412) points out, a novel fact could be understood as a
new explanation of an existing fact. Thus, while the financial crisis as experienced from 2007 was not a novel event, new behavioural economics provided a new explanation. But in mainstream economics it was only novel to analyse financial crises as systemic with reference to expectations formation and decision making. Other approaches already offered this kind of explanation, inviting direct comparison between new behavioural economics and these alternative explanations. It is crucial that these alternative explanations arose from different methodological frameworks, so direct comparison as advocated by Blaug is impossible. Not only are there different criteria for judging what is a novel fact and what is a satisfactory explanation, but different meanings are ascribed to both concepts and evidence (see Dow 2012, chapter 1).

As an example of an alternative explanation, old behavioural economics already had well-developed theories of decision-making based on satisficing rather than optimising and using heuristics in order to address cognitive limitations—most of which is precluded by the mainstream rational optimising framework. Berg and Gigerenzer (2010) draw attention particularly to the incompatibility between the gross substitution assumption of the mainstream framework and the adoption of lexicographic preferences, for which there is substantial evidence. The concept of bounded rationality spawned a rich and complex body of thought among old behavioural economists (Fiore 2011). As another example, post-Keynesian economics already had a macroeconomic theory of financial instability which combined a theory of uncertainty (only partly due to cognitive limitations) with a theory of financial structure (Minsky 1982). This theory could not predict the timing of the financial crisis, but did account for how financial fragility was increasing in the years leading up to 2007 creating the conditions for a crisis. Both approaches are logically consistent. The limitations to knowledge which underpin the core concepts of both bounded rationality and uncertainty are incorporated into an open-system understanding of social systems. Rather than being calculative optimisers, agents cope by adopting heuristics, adopting conventional knowledge, following conventional behaviour in practices and routines (which are not necessarily sensible), and satisficing. In this way, theory is consistent with its ontological and epistemological foundations, which contrasts with the internal consistency criterion within a deductivist mathematical framework.
Nevertheless, can the new behavioural economics research programme be seen as progressive at least within mainstream economics? It addresses anomalies which have been found, not only in experimental evidence but also in more conventional econometric evidence, with new theories. Thus Rabin and Thaler (2001) challenged the subjective expected utility theory with an alternative explanation for risk aversion which accorded more with the experimental evidence. Shiller (2000) had identified excess swings in asset prices compared to what was predicted by the efficient markets hypothesis, and explained them in terms of the psychology of information gathering and expectations formation: undue emphasis on trends, undue attention to media interpretations, and so on. Accordingly he developed feedback models to capture this behaviour (see Shiller 2003).

New behavioural economics includes elements which depart from the mainstream framework, as in some authors accepting limitations to global rationality (for a cataloguing of similarities and differences, see Earl and Peng 2012). Perhaps most tellingly, there is a willingness to pursue non-universal explanations, a feature reminiscent of old behavioural epistemology. Here we find some inconsistency between what new behavioural economists say in terms of adopting the standard framework and what they do. This echoes Blaug’s observation that mainstream economists behave inconsistently with their professed falsificationism. It also echoes McCloskey’s (1983) observation of the disparity between the formalist ‘official discourse’ of mainstream economics and the pluralist, context-specific ‘unofficial discourse’. Lawson (1997) identifies inconsistency too, between the closed-system methodology of mainstream economics (which allows for theorising in terms of constrained optimisation) and any sense of the openness of real social systems. Were new behavioural economists to emphasise consistency with their observations of reality over the internal consistency (and universality) of the rational optimising framework, there would be much more scope for theoretical developments which are not ad hoc adjustments to existing theory. But by retaining the hard core of mainstream economics in the form of the rationality benchmark, even if not rationality itself, new behavioural economics is accepting constraints on its scope for progressive development.
The relative prioritisation of theory and evidence

The insistence on a formalist approach to theory was the subject of Blaug’s (1999) critique of mainstream economics. Nowadays more attention is being paid to evidence. But as I have shown, the way in which the formalist mainstream approach developed, with its benchmark of rational optimisation, constrains the way in which theory can evolve in response to new evidence.² Being a deductivist approach, the axioms are of critical importance, so any modification requires general acceptance (not just local applicability) and feeds through into all theoretical results. Either the behavioural approach defines actual behaviour as rational by redefining the constraints, or the behaviour is redefined as irrational. Then the choice is whether to treat irrational behaviour as stochastic, which again does not challenge mainstream theory, or to theorise and model it. But how can that be achieved other than with a modified set of axioms? And what is more, how can a set of axioms incorporate irrationality?

Modelling heuristics, for example, could be an alternative, however some authors have recently reflected on the challenges posed by such approach (see, e.g., Goodhart 2008; De Grauwe 2010). More generally, there is a problem in trying to incorporate models of irrational behaviour into the general deductivist framework. As Blaug (1992, 233) points out, if the evidence suggests that behaviour departs from rationality in financial markets, then it must be presumed that it does so in other markets. This problem stems directly from mainstream methodology. A Lakatosian would be concerned at ad hoc adjustments such as introducing some constraints on market processes to explain anomalies. But within the mainstream framework what is regarded as ad hoc are theories which have only very localised application:

The enduring appeal of classical asset-pricing theory over the last several decades owes much to its success in forging a consensus around a foundational modelling platform. This platform consists of a core set of assumptions that have been widely-accepted by researchers working in the field as reasonable first-order descriptions of investor behaviour, and that—just as importantly—lend themselves to elegant, powerful, and tractable theorizing.

If behavioural finance is ever to approach the stature of classical asset pricing, it will have to move beyond a large collection of

² Lawson (2009) and Dow (2012) have focused further on the deductivist, mathematical nature of this formalism.
empirical facts and competing one-off models, and ultimately reach a similar sort of consensus (Hong and Stein 2007, 126).

The appraisal of new behavioural economics is thus conditioned by acceptance of the formalist mainstream methodological framework. It is this which challenges the value of partial theories which are not deterministic and drives the new behavioural economics agenda in the direction of ever more general formal theories of behaviour which are amenable to mathematical modelling. Within the mainstream framework, new behavioural economics would be theoretically progressive if it enhanced the existing body of theory by increasing its scope. It would be empirically progressive if it addressed evidence of anomalies and improved empirical prediction. But there is the potential for significant conflict between the two and, within the mainstream methodological framework, theoretical progressiveness is prioritised over empirical progressiveness. While the development of partial theories (feedback theories, prospect theory, and so forth) could be said to be empirically progressive, this is incompatible with trying to fit such theories into a general equilibrium framework deduced from the rationality axioms. As long as new behavioural economics accepts the mainstream framework, therefore, it is likely to become degenerative.

**CONCLUSION**

This discussion indicates that new behavioural economics falls short in Lakatosian terms. It could become a progressive research programme if it evolved through partial theories developed in an abductive interplay with evidence, an approach favoured by Blaug (1999). This is something which already exists in the old behavioural economics, as in Earl, Peng, and Potts's (2007) theory of instability in the housing market due to reliance on heuristics. Yet much of the academic success of new behavioural economics must be down to its self-presentation in relation to the rationality framework (Earl and Peng 2012).

In making a Lakatosian comparative assessment of alternative research programmes, we run up against a meta-methodological problem. Considering Lakatos's prescriptive (as opposed to descriptive) framework once we move beyond mainstream economics is problematic in that Lakatos’s approach itself is closely aligned with the mainstream approach to economics. What constitutes a novel fact (even in the form only of a satisfactory new explanation) and what constitutes an *ad hoc*...
adjustment depends on the particular understanding of the world, interpretation of facts and criteria for good theory (including consistency, as discussed above) which distinguish methodological frameworks. This was a key feature of Kuhn’s discussion of paradigms which was dropped by Lakatos, for whom research programmes were directly empirically comparable. Just as the rationality principle is metaphysical and thus untestable, so too is the whole mainstream framework.

This problem is also evident when we consider Blaug’s Popperian criterion, that economists should specify what evidence would lead them to reject a theory. Since theories are part of the complex structure of research programmes, which embody a particular understanding of and interpretation of reality and of what constitutes good theory, rejection ultimately has to be at the metaphysical level. This helps us understand the resistance by many economists to respond to the crisis by rejecting the mainstream framework (Earl 2010). But for some mainstream economists the crisis has shaken confidence in a research programme that assumes the capacity of markets to stabilise themselves. They are open to alternatives. Similarly, if austerity policies in a recession fuelled a supply-side boom then many Keynesians would lose confidence in their approach and seek alternatives. But within any research programme it is the overall approach which is decisive rather than individual theories. The discussion above has illustrated this in the case of behavioural economics.

Our consideration of new behavioural economics addresses Blaug’s concern that theoretical appraisal be empirical in relation to his concern that too much priority was being placed on theoretical formalism. What we have seen is the blossoming of a relatively new area in mainstream economics which seems to successfully explain the financial crisis. But this is not a new research programme in the Lakatosian sense, since the hard core rationality principle was retained. Constraints on full information and on rational choice are explained by a more sophisticated representation of rationality or else as irrationality. This is a change in the protective belt. Nonetheless, there is a pressure on developing these theories in such a way as to make them more general through greater formalisation, that is, limiting the change to the protective belt by ensuring methodological compatibility with the mainstream.
Genuinely alternative theoretical approaches, such as old behavioural economics and post-Keynesian economics, employ different methodological frameworks, thus constituting competing Lakatosian, research programmes. They adopt different stances with respect to the nature and meaning of theory and evidence from mainstream economics, but that is only relevant to the current argument inasmuch as it raises particular issues with a Lakatosian empirical criterion of appraisal. These different approaches are identified by different understandings of real-world processes and terminology from the mainstream research programme, so they have not been recognised (or indeed are not recognisable) as progressive from the mainstream perspective.

There is no independent way of making judgements about progression or degeneration across research programmes. While a Popperian/Lakatosian framework might encourage the idea of an empiricist alternative to formalism, we have seen that pure empiricism is unsatisfactory. A methodologist cannot be in a position to take an independent view on novel facts and ad hoc adjustments. This is not at all to say that any interpretation is as good as any other, but rather that there is no ultimate independent arbiter and therefore any position needs to be justified. Blaug was notably well-informed about and open to alternative approaches to economics. This is the best position from which to engage in constructive debate as to the merits of different theories and theoretical approaches.

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3 Methodologists have raised a wide range of concerns with Lakatos's framework, to Blaug's (1991) dismay.
Dow / Formalism, Rationality, and Evidence


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Mark Blaug on the historiography of economics

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Abstract: This paper discusses how Mark Blaug reversed his thinking about the historiography of economics, abandoning ‘rational’ for ‘historical’ reconstruction, and using an economics of scientific knowledge argument against Paul Samuelson and others that rational reconstructions of past ideas and theories in the “marketplace of ideas” were Pareto inefficient. Blaug’s positive argument for historical reconstruction was built on the concept of “lost content” and his rejection of the end-state view of competition in favor of a process view. He used these ideas to emphasize path dependency in the development of economic thinking, thereby advancing an evolutionary view of economics that has connections to a Lakatosian understanding of economic methodology. The paper argues that Blaug was essentially successful in criticizing the standard rational reconstructionist view of the history of economic thought in economics, and that this is borne out by the nature of the change in recent economics.

Keywords: Blaug, historiography, Samuelson, economics of scientific knowledge, process-conception of competition, path-dependency, evolutionary view

JEL Classification: A14, B20, B31, B41, Z13

In the face of ideas, many economists are simply philistines, like troglodytes listening to a Beethoven quartet and asking why the four players seem to be unable to bow in unison (Blaug 1994, 18).

Mark Blaug was a highly accomplished and influential historian of economic thought and economic methodologist who re-thought his
views about the historiography of economics late in his career in connection with his increased concern over the declining place of the history of economic thought in the economics profession in the 1990s (Blaug 2001). Indeed he was provoked to reflect again upon the historiography of economics by the stated and implicit views on the subject held by many non-historian economists, which had emerged as their grounds for justifying the expulsion and near exclusion of the history of economics from the doctoral curriculum in most American universities by the end of the decade. Though many non-historians supported or acquiesced in this development for reasons often unrelated to their views about the history of economics—such as their desire to increase the time students devoted to training in mathematics and econometrics—their willingness to sacrifice the history of economic thought effectively led them into arguing that the field had no significant value for economics as a science. This entailed a particular view of the historiography of economics, specifically, a combination of two related propositions: (1) the Whig idea that science always makes progress which renders past knowledge irrelevant to current knowledge, and (2) the view that progress in economics as a science consists in analytical achievements which are by nature strongly separable from their origins and manner of development. Thus to combat the profession’s stance toward the history of economics, Blaug needed to be able to explain why this particular historiographic view of economics was mistaken. But this proved an especially difficult task for him personally, because he had held historiographic views earlier in his career which were not far removed from the positions he had since found he wanted to reject. This paper seeks to explain how he pulled off the move from his earlier to his later view, and thereby set out his final understanding of the historiography of economics as the methodology of the history of economics.

I argue that the key to the change in Blaug’s thinking and critique of the standard Whig (or, as we will see rational reconstructionist) historiography were two connected positions he adopted which concerned the growth of knowledge. On the one hand, he turned economists’ own market theory tools against them in using an economics of scientific knowledge approach to argue that the ‘marketplace of ideas’ was neither competitive nor efficient in regard to the production of economic knowledge. From this he was able to argue that the way in which economics developed as a science inevitably
involved loss of content, thus undermining the steady progress idea associated with the Whig view, and allowing Blaug to define a role for the history of economic thought in advancing scientific knowledge. On the other hand, he adopted a particular conception of competition, namely, that competition is a ‘process’ rather than an ‘end-state’ in order to make a case for saying that economic knowledge is path-dependent—by which he meant that economic knowledge at any one point in time depended crucially upon what had previously occurred along the path economists had until then pursued. This then went against the idea that economics amounted to a succession of separable analytic achievements, and justified an essentially evolutionary view of knowledge and science. This gave historians of economics what he saw as their comparative intellectual advantage, a capacity to grasp the depth and breadth of complex interconnections in intellectual history, and thus an important role in advancing the science of economics.

In this paper I seek to explain the genesis and ultimate nature of Blaug’s historiographic thinking, first, by reviewing his response to the Whig view held implicitly or openly by most non-historian economists in their attitude toward the history of economics—one which in a number of respects he had at one time shared—and, second, by showing how his later evolutionary view of the science of economics led to an altogether different historiographic view which sustained an important role for historians of economics in the development of the field.

The first section of the paper begins by discussing Blaug’s response to standard historiography in terms of the distinction between rational reconstructions and historical reconstructions of the past. In his first major discussion of the subject—recalling his own recent practice as an historian (Blaug 1985 [1962])—Blaug had expressed a fairly strong preference for rational reconstructions, and had also been quite skeptical about whether historical reconstructions were even viable (Blaug 1990). In his second major discussion of historiography, however, he reversed himself, and expressed serious doubts about what rational reconstructions achieve, while also making an argument for historical reconstructions (Blaug 2001). To more fully explain this change in position, the second section lays out Blaug’s economics of scientific knowledge argument that the marketplace of ideas is inefficient, implying that the method of rational reconstruction is inefficient, and then goes on to explain how Blaug used his process view of competition to take an evolutionary view of the history of economics.
as path-dependent. The third section makes two comments on Blaug’s mature view, addressing the role that his ‘lost-content’ assumption plays in evolutionary theory in general, and examining the extent to which his view encounters the problem of reflexivity associated with economics sociology of scientific knowledge explanations. The final section concludes with a discussion of the implications of Blaug’s view for the status of the history of economic thought field in the economics profession, particularly with respect to how historians might make the issue of change in economics central to the defense of their field.

RATIONAL VS HISTORICAL RECONSTRUCTIONS: A CHANGE OF POSITION

Blaug drew on the philosopher Richard Rorty (1984) for the distinction between rational reconstructions and historical reconstructions of past ideas (see Blaug 1990; and 2001). Both approaches—Blaug emphasized—were inevitably types of reconstructions for the reason that (here he cited the thinking of Jacques Derrida and Michael Foucault), “all texts of the past need to be reconstructed because they do not speak with one voice and are never unambiguous” (Blaug 2001, 151). The relevant question regarding the method of reconstructing the past—he rather argued—was “how are we to do so: in the light of all that we now know or as faithfully as possible to the times in which they were written”—where the former counted as a rational reconstruction and the latter counted as an historical reconstruction (Blaug 2001, 151). Ostensibly the difference was that in the former case the present was one’s point of entry, while in the latter case it was the past. Given, then, that each had a certain prima facie plausibility, how was one to decide between the two methods?

In his first discussion of rational and historical reconstruction (1990), Blaug had relied on a distinction between absolutism and relativism—an alternative but related distinction which he had employed in his Economic theory in retrospect (see, e.g., Blaug 1985 [1962], 1-2)—to argue that were important asymmetries between rational and historical reconstructions which tended to favor the former

1 Rorty, whose concern was the history of philosophy, distinguished four genres of historiography: Geistesgeschichten, historical reconstructions, rational reconstructions, and doxographies (Rorty 1984). Mark Blaug considered “‘intellectual history’ or ‘geistesgeschichte’ [...] virtually the same thing as what I call ‘historical reconstruction’” (Blaug 2001, 151), and the difference between doxography and rational constructions “at best a subtle one” (Blaug 1990, 28). His main distinction across his two papers, consequently, was the difference between rational reconstructions and historical reconstructions.
over the latter. First, “we can never forget what we know, so that some version of ‘absolutism’ is implied in every attempt to examine some text of the past”, where by absolutism he meant “the tendency to judge past economic theories by the standards of modern economic theory”. Second, if we take relativism to be the polar opposite of absolutism, then “‘absolutism’ is more defensible than ‘relativism’, particularly as strict ‘relativism’ is logically impossible” (Blaug 1990, 28). He then went on to present Paul Samuelson’s (1974b) unhesitating defense of rational reconstruction as the clearest possible statement of what that method involved (Blaug 1990, 30), and drew up the balance sheet for the two methods in a way that gave mixed but generally favorable support to rational reconstructions, and expressed serious doubts about what historical reconstructions could achieve.

I conclude that rational reconstructions are perfectly legitimate, although whether they are illuminating depends on the case in question. As for historical reconstructions, they are inherently problematic. Strictly speaking they are impossible because they presume that the past can be recalled without knowledge of the present; no adult can be expected to recall his childhood as if adulthood had never happened (Blaug 1990, 30).²

Thus at best Blaug was ambivalent, aware from Rorty and others that a case for historical reconstructions might be made, but unclear about how they were to be reasonably done, and thus inclined to defend rational reconstructions as “legitimate” though perhaps not always “illuminating”.

Accordingly, when he set out to rethink his views on historiography in economics some ten years later, having experienced how a general acceptance of the method of rational reconstruction in the intervening time had worked against the history of economics, he found himself in a quandary. “The temptation to choose the first alternative is almost irresistible” (Blaug 2001, 151), he still admitted, but one also needed to see that this led to the unacceptable conclusion, as he had previously put it, that “there is really no point to the history of economic thought; why study what Pigou once contemptuously called ‘the wrong opinions

² A somewhat more blunt assessment of his view at this time was recorded later in his 1994 autobiographical remarks with respect to his historiographical stance in Economic theory in retrospect: “I announced myself an unapologetic absolutist and poked fun at relativists throughout the book. This is not a point of view I now hold, having been upstaged over the years by even more strident upholders of the ‘Whig interpretation of history’” (Blaug 1994, 17).
of dead men” (Blaug 1990, 28). Defending the method of rational reconstruction, that is, would only confirm the negative verdict of non-historians regarding the history of economic thought that it was simply an idle enterprise outside the bounds of economic science. Was there thus a stronger case, he then asked himself, for understanding the historiography of economics as historical reconstruction, despite its seemingly problematic nature? And was there something objectionable about rational reconstructions that he had previously overlooked?

To uncover his earlier unacknowledged hesitations about rational reconstruction, let us step aside from the abstract epistemological arguments Blaug originally advanced regarding the nature of the two methods, and rather look at how he first saw them applied in practice in connection with the exchange between Samuelson and William Baumol, which had led to Samuelson’s characterization of the method rational reconstruction. There in fact we see a different appreciation of historical reconstructions associated with the emphasis which Blaug sees Baumol putting on the perspective of the historical economist. The starting point in the exchange had been Samuelson’s (1971) interpretation and analysis of Marx’s transformation problem. Samuelson’s position was that Marx had simply failed mathematically to simultaneously transform surplus value into profits and labor values into prices. Baumol (1974a) responded to Samuelson that Marx was chiefly interested in the transformation of surplus value into profits (in order to explain the origins of profit in labor exploitation), and that the transformation of labor values into prices had only been of secondary interest to him. So Baumol had argued that mathematical adequacy was not the point in understanding Marx’s transformation problem. Samuelson (1974a) nonetheless replied that Marx had still failed at what a comprehensive transformation required, leading Baumol (1974b) to say, yes, but that it had rather been his objective to determine what Marx had intended to do, implying that this was important for understanding Marx and the transformation problem.

Thus as Blaug understood the exchange, the line drawn between the two was over whether economists’ intentions and objectives were relevant to understanding their thinking, with Baumol arguing, essentially as an historical reconstructionist, that there were advantages

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3 It is worth noting that Blaug was also motivated to reconsider the merits of rational reconstruction by his perception of the unhealthy emergence of formalism in economics, see Blaug 2003.
to having this further information apart from how we might view the past from the perspective of contemporary theory. Samuelson thus found himself arguing not only for mathematical adequacy but also that nothing goes missing when we ignore economists’ intentions and objectives. One could additionally argue on Samuelson’s side that one can never know another’s intentions, and so they cannot be part of an explanation of economic thinking, but this argument was really a back-up defense for the position that economists’ intentions and objectives were simply irrelevant, whether or not we could say what they were. We can see this in the subsequent exchange of correspondence between Blaug, Samuelson, and Don Patinkin (see Samuelson, et al. 1991) where the issue became whether a rational reconstruction could deviate from a historical reconstruction. Samuelson’s prior position (1974b) had been that all there could be was rational reconstruction, but having read Blaug’s account of the exchange with Baumol he found himself on the defensive with regard to whether a rational reconstruction might not be faithful to, or “deviate” from, an historical reconstruction. As he wrote to Blaug: “Let’s accept for the sake of argument that in some instances a ‘rational reconstruction’ can deviate from a ‘historical reconstruction’” (Samuelson, et al. 1991, 144). Thus he allowed that there seemed to be something to historical reconstructions, even if one preferred rational ones and did not know how to proceed with the other. Rational reconstructions accordingly no longer unequivocally ruled the roost. Blaug seized on this in his response to Samuelson, expanding on Baumol’s point that knowing what economists meant to say was important to understanding what they said:

Is [Hermann Heinrich] Gossen a true forerunner of the Marginal Revolution? No, because no one read him. It is not enough to have great ideas, as Schumpeter always said; you have to get them across to your colleagues (Samuelson, et al. 1991, 148-149).

That is, whatever the adequacy, mathematical or otherwise, of Gossen’s thinking, we cannot say he was an early marginalist, because marginalism only emerged as a distinct theoretical approach when it

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4 This was important to George Stigler’s position (cited by Blaug) in contrasting ‘personal exegesis’ and ‘scientific exegesis’ (Stigler 1965).
5 This subsequent exchange was stimulated by Samuelson’s reaction to Blaug’s (1990) reflections on historiography and discussion of the exchange with Baumol. Samuelson widened the exchange to include Patinkin, and their correspondence largely concerned other issues in the history of economics.
was seen explicitly to be such by economists. So knowing what economists think about what they were doing matters to our understanding of what their thinking is.

Thus whereas before Blaug had regarded the method of rational reconstruction as a straightforward, justifiable practice, and had doubts about historical reconstruction, now it was not only clear he thought historical reconstructions had certain advantages over rational ones, and thus must have some sort of coherence, but he had begun to develop doubts about the adequacy of rational reconstructions. Essentially he had reversed his earlier view as a result of his further reflection on Samuelson’s statement regarding what the method of rational reconstruction involved in light of the exchange with Baumol. The issue, moreover, concerned what rational reconstructions left out or omitted as explanations which might be recovered through historical reconstructions. This focus on omission points us towards the economics of scientific knowledge argument Blaug was to subsequently make regarding the growth of knowledge in economics.6

**LOST CONTENT, THE ECONOMICS OF SCIENTIFIC KNOWLEDGE, AND PATH-DEPENDENCY IN THE HISTORY OF ECONOMICS: AN EVOLUTIONARY VIEW**

Sifting through the various reasons that Blaug found had been given by various authors for studying the history of economic thought (in “a painfully defensive tone”), Blaug noted that infrequently mentioned was the benefit of discovering ‘new’ and largely forgotten ideas, a prime example being the rediscovery and rehabilitation of the Pareto

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6 Note that Imre Lakatos, who much influenced Blaug on the subject of the methodology of scientific research programs, had much earlier made related statements, arguing that “any rational reconstruction needs to be supplemented by an empirical (socio-psychological) ‘external history’” (Lakatos 1971, 91). Thus he wrote:

> In this paper I have proposed a ‘historical’ method for the evaluation of rival methodologies. The arguments were primarily addressed to the philosophers of science and aimed at showing how he can—and should—learn from the history of science. But the same arguments also imply that the historian of science must, in turn, pay serious attention to the philosophy of science and decide upon which methodology he will base his internal history. I hope to have offered some strong arguments for the following theses. First, each methodology of science determines a characteristic (and sharp) demarcation between (primary) internal history and (secondary) external history and, secondly, both historians and philosophers of science must make the best of the critical interplay between internal and external factors (Lakatos 1971, 122).

> Blaug, however, does not seem to have cited this text. For a discussion of Lakatos’s view, see Klaes 2003.
optimality concept in the 1930s after more than a quarter century of neglect (Blaug 2001, 148). That the profession was blind to this kind of loss of ideas was due—he then argued—to the widely-held view that

the community of economists represents an approximately perfect market in which new ideas are so efficiently transmitted in a communication network of journals, books, seminars and conferences that there is no loss of significant content. This view of an efficient marketplace of ideas implies that history of economic thought can be safely neglected by modern economists, because what is valuable in the ideas is fully contained in the present curriculum (Blaug 2001, 148).

But this view of an efficient marketplace of ideas was in Blaug’s view as indefensible as it was accepted.

The objections against taking the market-of-ideas as anything other than a stimulating metaphor are so obvious as hardly to require discussion. In particular, markets as arbiters of quality in scholarly (or any other kind of) goods are excessively subject to bandwagon and snob effects (Blaug 2001, 148-149).

The implication of saying that the marketplace of ideas was not perfectly competitive and efficient, then, was that there was always a risk of “loss of content” in current activity as economists followed popular trends. Past ideas that were of continuing value were then regularly discarded, meaning that current ideas were commonly less original and well-founded than generally believed. This was directly contrary to the Whig idea of progress in science and the notion that the advance of economics involved a succession of distinct analytical achievements. The method of rational reconstruction, it followed, was itself an inefficient practice, and this suggested that rather some combination of historical and rational reconstruction was needed to generate an efficient scholarly production process.

Mark Blaug thus took it as a given that the growth of knowledge ought to be understood as an economic process, one that could be reasonably well explained with conventional neoclassical concepts such as efficiency and competition. The problem with the profession’s abandonment of the history of economic science was accordingly that economists had failed to use their analysis to understand their own profession. That is, they did a poor economics of scientific knowledge, and therefore reached the wrong conclusions about the economics
production process, the history of economic thought, and the growth of knowledge in economics. This critique was different from ones advanced by many other historians of economics who commonly emphasized the intrinsic value of historical reflection, and were mostly not inclined to use an economics of scientific knowledge approach. However, this is not to say that Blaug as an intellectual historian was unsympathetic to more traditional sorts of arguments about the nature of knowledge and its development:

Knowledge has multi-dimensional depth as well as breadth, and some of the dimensions of economic knowledge include analysis, data, history, institutions and policy questions. There is a raw kind of conceptual depth where concepts are only understood when they are differentiated relative to other closely related ones so that the extent of one’s knowledge depends on the fineness with which one can differentiate. In different contexts, these dimensions of knowledge vary in importance (Blaug 2001, 150).

But to deliver a satisfactory critique of standard historiography and the method of rational reconstruction, Blaug needed to employ an argument that operated upon economists’ own terrain which they would have to answer but which he believed they could not reasonably rebut. It was one thing, however, to successfully attack rational reconstruction. To go further and advance a positive case for historical reconstruction based on its own merits he needed to go beyond simple marketplace of ideas formulations. This brings us to his argument for a process-conception of competition.

An important emphasis in Blaug’s later research in the history of economics concerned how the concept of competition had been understood in the past, and how the modern view of the concept departed from earlier thinking. The modern view of competition was what he referred to as an end-state conception (Blaug 1997). A good part of the motivation behind the adoption of this conception was its tractability for mathematical analysis. But earlier economists had reasoned in terms of a process-conception which emphasized the conditions and dynamics of competition.

What Adam Smith meant by competition is what modern Austrians call “process competition”. What we nowadays call competition was

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7 This was central to Blaug’s critique of formalist general equilibrium theory (e.g., Blaug 2003).
for him “the obvious and simple system of natural liberty”, meaning an absence of artificial constraints and, in particular, restraints on free entry into industries and occupations. Neither competition nor monopoly was a matter of the number of sellers in the market (Blaug 2001, 153).

Of course contemporary economists might well argue that what Blaug called a process-conception of competition was simply a primitive attempt to understand the formalizable end-state conception, but here Blaug used the process idea to counter this potential response and simultaneously defend the method of historical reconstruction. Thus, returning to the problem of “How one can justify the study of the history of economic thought as a specialization within economics”, he offered what he regarded as his “own knock-down argument”.

It is this: No ideas or theory in economics, physics, chemistry, biology, philosophy and even mathematics is ever thoroughly understood except as the end-product of a slice of history, the result of some previous intellectual development (Blaug 2001, 156).

Here what he says is that any end-state or end-product judgment is by itself incomplete as a representation of the economy, and thus inseparable conceptually from the process or development which precedes and produces it. One cannot, then, rationally reconstruct the concept of competition solely in end-state terms, because, despite their desire to formalize, economists know that competitive end-states are always the result of processes that generate them. It follows that if we see economics as a marketplace of ideas—as he believed economists should—rational reconstructions, as an end-state type of thinking, must always include historical reconstructions as a process type of thinking in order to be complete.

From this conclusion, Blaug went on to characterize his view of the growth of knowledge in economics using a key evolutionary concept: path-dependency.

Economic knowledge is path-dependent. What we now know about the economic system is not something we have just discovered, but it is the sum of all discoveries, insights and false starts in the past […]. There is nothing predetermined about our current theories and if years ago, economics had taken another turn at a critical nodal point, we would today be advocating a different theory (Blaug 2001, 156).
Here Blaug's view of path-dependency is subtle. It is not just that there is always a chain of connection between the past and the present which we must trace out in order to have the whole picture. That chain also comprehends many paths not taken, blind alleys, and false starts, which moreover are from time to time revived and re-explored, thus adding cycles of explanation into the history of economics, and undermining the Whig idea of linear progress. In this way, then, Blaug paints in broad-brush terms the picture of an evolutionary system in which many competing ideas and theories interact with one another, with some prevailing at one point in time and others prevailing at others. Those ideas that lose their temporary advantage may re-appear in the future, or even cease to have any significant further role in economics. Blaug consequently goes well beyond the traditional marketplace of ideas metaphor, while still sustaining the idea that competition is a regulating force in a complex evolving world of interdependent theories and ideas.

Blaug contrasted this vision of the history of economics with a more familiar one held by many economists and perhaps some historians of economics as well.

Conversations with other economists have brought home to me that a widely held impression views the history of economic thought as a sort of intellectual archaeology: it may turn up new manuscripts and documents from time to time, but it itself remains unaffected by these discoveries and, unlike other branches of economics, shows no development or progress over time. This is a totally misleading impression (Blaug 2001, 157).

He proceeded to review a number of recent developments in the history of economic thought demonstrating that the status and meaning of many past doctrines was typically up-ended by recent research. This showed that the rational reconstructionist view could not stand, since how current theory related to the past was always in jeopardy when the past was in continual transformation. The archaeological view, in effect, was thus an expression of the rational reconstruction view that economics is a succession of separable analytical achievements that

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8 Thus on the one hand: “At long last, it can be said that the history of general equilibrium theory from Walras to Arrow-Debreu has been a journey down a blind alley”, whereas on the other hand: “General equilibrium theory, which had been dying a slow death ever since Walras's own death, was revived in the 1930s” (Blaug 2001, 160).
proceeded without effects on past economics, but it was increasingly
evident that the idea of rational reconstruction itself was incoherent,
rather than the reverse as he had originally been tempted to think.

Blaug thus moved his thinking about the historiography of
economics a considerable distance from his earlier views. He also
made a case for the place of the history of economics in the economics
profession that was directly contrary to the conventional view.
Whereas the history of economics was an incidental, leisure activity
in the eyes of most economists, Blaug's argument was that it was
absolutely central to the development of economics, since one could not
say what constituted a scientific advance if one could not appraise new
developments in terms of how they related to past ideas and theories.
Moreover, to complicate matters, what the history of economics
amounted to exhibited change no less than the research frontier of
current economics. Yet needless to say, this argument about the history
of economics or economics as a whole has not carried the day for many
economists. Perhaps this is because it was framed as a defense of
the history of economic thought rather than as an explanation of the
science of economics per se, but in any event it can be argued that
Blaug's evolutionary view of economics may have begun to acquire new
relevance in light of the fact that an increasing number of economists
who are now willing to say that economics research appears to be
evolving in a variety of new directions. Thus it is fair to ask, what does
Blaug's evolutionary view tell us about the dynamic nature of economics
today? In the section that follows, I address this question by offering
an interpretation of his 'lost content' assumption and by commenting
on the problem of reflexivity it raises.

THE EVOLUTIONARY VIEW OF ECONOMICS: TWO ISSUES
Blaug's immediate motivation for saying that economics regularly
loses what turns out to be valuable past content (as in the example of
the rehabilitation of the Pareto optimality concept) was to show that
rational reconstructions are inefficient. Pareto efficient states of affairs,
of course, are those in which gains are unequivocal improvements,
and nothing valued is sacrificed to produce them. Conversely Pareto
inefficient states of affairs are those in which gains come at the expense
of losses, and are consequently not unequivocal improvements, in this
case where the development of new ideas and theories in economics
is at the cost of abandoning earlier ideas and theories that remain valuable.

Thus to argue that economics loses valuable content demonstrates not only that the Whig view of steady progress is mistaken, but that it is so because it fails to satisfy the Pareto standard. Again, Blaug’s strategy was to turn economists’ own tools against them on the subject of their rational reconstructionist view of the history of economic thought.

But in making this fairly conventional argument he committed himself—it seems at least in part unintentionally—to the view that economics as a domain of thinking has a holistic character with evolutionary sorts of dynamics. Consider the following two propositions which are arguably implied by Blaug’s position regarding ‘lost content’. First, since lost content is usually regarded as being ‘correct’ in some way when it is first advanced, then ‘incorrect’ when it is later abandoned, and then once again ‘correct’ when it gets rehabilitated, the standard for what counts as a ‘correct’ idea or theory in economics is clearly changeable. Second, since correctness is relative to the views that are dominant at any one time, that any one set of theories and ideas can move back and forth across correctness implies that there must also be change in what kinds of views are dominant in economics over time. These propositions together, then, suggest that economics operates as a relatively closed discourse which continually recycles relatively durable content, albeit in changing ways. That is, were it not a relatively closed system in this sense it seems unlikely that past ideas and theories would undergo regular recovery and rehabilitation, on the grounds that substantial changes in the nature and character of economic thinking would disrupt the system of connections that keeps ‘incorrect’ ideas in play. Perhaps such a view should be termed a ‘weak’ holism since change in those ideas and theories that are dominant allows more space for bottom-up change in ideas and theories.

That it was not Blaug’s explicit intention to reason in these terms might be inferred from his long-standing commitment to the methodology of Imre Lakatos’s scientific research programs (SRP) approach (e.g., Blaug 1992 [1980]). But in his last statement on the subject he advanced a generally holist view of Lakatos’s thinking, emphasizing that Lakatos

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9 This argument was also advanced in a somewhat different manner by Daniel Hausman (1992).
denied that it was ever possible to judge an isolated theory; what could be judged and appraised were clusters of more or less interconnected theories, and it was these clusters that he labeled ‘scientific research programs’ (Blaug 2010, 113).

Moreover, in regard to Lakatos’s treatment of ‘progressive’ and ‘degenerating’ changes in research programs, Blaug pointed out that whether a given idea or theory was one or the other depended on its relative performance over time. For example, regarding the various assumptions behind the competitive market ‘invisible hand’ theorem he argued:

If they are found to be false, this requires a revision of the “invisible hand” theorem. They may be capable of being accommodated by suitable revision, but only the track record of the research program in the face of possible refutations will establish whether it is a “progressive” or “degenerating” SRP […]. The point is that the question of whether a SRP is “progressive” or “degenerating” has no “once-and-for-all” answer (Blaug 2010, 114).

Thus it seems fair to say that there were evolutionary holist ideas in Blaug’s thinking about the ways in which economics developed and changed over time, despite the fact that he did not emphasize them explicitly or apparently see himself as an evolutionary thinker per se.

This then raises the question of how he saw his own role in regard to the arguments he made about the historiography of economics. Were those arguments, particularly as economics of scientific knowledge arguments regarding the inefficiency of rational reconstructions of the past, to be judged as merely relative to “clusters of more or less interconnected theories” about the historiography of economics, and so perhaps to be seen as ‘correct’ at one time but then abandoned and ‘incorrect’ at another, perhaps to be rehabilitated and recovered again later? This is the issue of reflexivity, or how one’s arguments regarding methodological practice apply no less to one’s own arguments than to those to whom they are directed. It is an issue particularly appropriate to Blaug’s thinking, because he casts his critique of rational reconstruction in economic terms via the marketplace of ideas metaphor, and then uses the ‘lost content’ idea to show that ideas and theories are ultimately relative to changes in dominant thinking in economics. This then invites those who would like to resist his critique of rational reconstruction to argue that he has not advanced any
definitive argument for historical reconstruction. Indeed, if there is a general recycling of ideas and theories in economics as might follow on his view, then one might say that Whig type views of economics historiography should have their day as frequently as Blaug’s preferred view of the history of economics.

One way to address this problem and escape the often paradoxical aspects of the concept of reflexivity is by distinguishing different levels in the application of the concept (Davis and Klaes 2003). A first level of reflexivity involves what may be called an ‘immanent’ reflexivity, where ideas and theories not only refer to objects in the world but are also self-referring. This is the case with the economics of scientific knowledge as a particular approach in economic methodology, since economics is being evaluated in terms of the concepts of economics. A second level of reflexivity involves what may be called an ‘epistemic’ reflexivity, where there is a relationship between a methodologist or epistemologist, Blaug in this case, and the economics of scientific knowledge (itself ‘immanently’ reflexive) such that his own arguments refer not only to the thinking of other economists but are also self-referring, as argued in the previous paragraph. A third level of reflexivity involves what may be called ‘transcendent’ reflexivity, where we ourselves, economists generally, and all those interested in the arguments concerning the role and nature of the history of economic thought look upon Blaug’s historiographic view (the ‘epistemic’ level of reflexivity) and do so in a self-referring way, or in a way that refers back upon us, since we are concerned with the same historiographic issues.

Reflexivity exists at all three levels, then, but its paradoxical quality dissipates as one moves from lower to higher levels. Thus it may seem highly paradoxical in light of the evident circularity to say, at the ‘immanent’ level, that the meaning of ideas and theories in economics derives from the meaning of those same ideas and theories. But this difficulty is at least moderated when the methodologist or epistemologist, who stands outside those ideas and theories, discusses at the ‘epistemic’ level whether, when, and how successfully economic ideas and theories can be used to evaluate the development of the field of economics objectified as a type of scientific practice. Nonetheless there still remains something problematic about ‘epistemic’ reflexivity where, as we saw with Blaug, it is fair game to say that a particular methodology that is grounded in certain ideas and theories ought to be judged by itself, another evident circularity.
Consider, then, how we might look upon Blaug's position in 'transcendent' reflexivity terms. Here we focus not on the ('epistemic') relationship between the particular methodologist Blaug and the views he developed but rather on collections of individuals like Blaug, Samuelson, Baumol, others, and ourselves, who are involved in seeking to explain a type of disciplinary practice, namely the history of economic thought, as it operates in the science of economics understood as one among many kinds of social sciences. Here we place Blaug in this wider social context in which at issue is not just his arguments regarding the status of the history of economics but generally how the social science disciplines function and ought to function. Blaug is still a historian and methodologist of economics, and he still thinks in terms of a relatively closed discourse in which he is expert. But our vantage point is now broader, and the representative case he makes against the view most economists defend regarding the historiography of economics is framed by how sciences generally operate on such subjects as “lost content” and change.

Thus, in light of the fact that Blaug uses economics of scientific knowledge reasoning to evaluate economics on historiography, the reflexive aspects of his thinking need to be recognized and addressed. But ultimately his case for the method of historical reconstruction is more widely based and not really vulnerable to the circularity critique. In the closing section, then, lessons from his case and arguments for the history of economic thought are set out in relation to the issue of change in recent economics.

**History of Economics and the Change in Economics**

As already noted, it is now widely recognized in the economics profession that there has been considerable change in ideas, theories, methods, and approaches of economics in the last decade or more. Perhaps not surprisingly, this has given new impetus to appraisals of past ideas as proponents of new research strategies interpret them in terms of past ideas and theories (e.g., Bruni and Sugden 2007). In one sense, this is consistent with Blaug’s view that the past can be recovered and rehabilitated. It also fits his critique of the archaeological view of the history of economic thought, where he argues that the past is not set in stone and there simply to be uncovered, but is continually being explained anew as continued research leads us to revise our past historical thinking. But to the extent that these recent ‘recoveries’
are rational reconstructions of the past, perhaps with Whig-type motivations, they would be as objectionable to Blaug as views such as Samuelson’s ultimately became for him. Nonetheless, might it be argued, with Blaug’s implicit evolutionary vision of economics in mind, that there are grounds for thinking that economists, especially those who are involved in new research approaches and who often find themselves at odds with dominant views are potentially more open to thinking that ‘correctness’ in economics is relative, and consequently that the history of economics could be valuable to understanding the development of economics as a science? Certainly there are reasons not to be too optimistic on this score, but it seems fitting in a discussion of Blaug’s contributions to the historiography of economics to at least make the case for a more optimistic scenario.

The argument, then, would be that the Samuelsonian idea that past ideas and theories can and ought to be rationally reconstructed is no longer nearly as credible as it was many years ago, that it has become increasingly accepted that historical reconstruction is the proper form of historiography, and, most importantly, that this all shows that economics can lose content seen to be of continuing value. Why should we think these three claims are true? First, rational reconstruction is now arguably seen as a naïve method, one not seriously defended any longer by economists, most of whom seem to have originally only followed the lead of Samuelson, while being less competent than he was in arguing it, and so are today not so much averse to the history of economics per se as just interested in increasing training in mathematics and econometrics. At the same time, whereas a leading historian of economics, Blaug, could entertain rational reconstruction at one point in his career, there are far fewer historians of economics now who would take that view seriously. Second, economists, even when they have no interest in the history of economics, generally accept that historians of economics are expert in their field, and recognize that the field has expanded in a research sense with more publications and journals than previously. Thus if historians with expert status defend historical reconstruction, likely economists generally will take that to be proper method. Third, the main feature of the change in recent economics is a multiplication in research approaches exhibiting considerable incommensurability with one another. Supposing that economists increasingly recognize this, they should then be aware that there are significant gaps in research, as is often manifest
in researchers’ successes in seizing upon unexploited subjects of investigation, and thus be more open to the idea that valuable economic ideas and theories may sometimes be missed, neglected, and sometimes also recovered. In effect, then, Blaug’s inefficiency of the economics marketplace idea is no longer really a very controversial idea among economists.

This optimistic scenario, whereby the view of history of economic thought in the economics profession has changed from what is was previously, unfortunately does not imply that resources will flow to the field or that it will make significant progress in the future in being re-established in doctoral study. As world-wide there are less resources directed to university training in general, fields such as the history of economics which are not at the center of their subjects will continue to face difficult times. But it may be taken nevertheless as a legacy of Mark Blaug’s work as an economist, historian, and methodologist of economics to have made a strong case for the field. And who would have been a better person to have done so than one who admitted he had been wrong about rational reconstruction because it failed by economists’ own measure of policy, Pareto efficiency?

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A 2x2=4 hobbyhorse: Mark Blaug on rational and historical reconstructions

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Abstract: Over time, Mark Blaug became increasingly sceptical of the merits of the approach to the history of economics that we find in his magnum opus, *Economic theory in retrospect*, first published in 1962, and increasingly leaned to favour ‘historical’ over ‘rational’ reconstructions. In this essay, I discuss Blaug’s shifting historiographical position, and the changing terms of historiographical debate. I do so against the background of Blaug's personal life history and the increasingly beleaguered position the history of economic thought found itself in after the Second World War. I argue that Blaug never resolved the tensions between historical and rational reconstructions, partly because he never fleshed out a viable notion of historical reconstruction. I trace Blaug’s difficulty in doing so to his firm conviction that the history of economics should speak to economists, a conviction clearly present in his 2001 essay: “No history of ideas, please, we're economists”.

Keywords: economic historiography, rational reconstruction, historical reconstruction, Whig history, constructivism, economic methodology

JEL Classification: A20, B20, B31, B41

On the 12th of December 1998, Mark Blaug wrote a short letter from his house in Devonshire to Paul Samuelson, in his usual longhand.

Dear Paul,
Are you imposing your 2x2=4 hobbyhorse on anyone, you ask. Yes of course: students. You remain an unrepentant rational constructionist. Nothing wrong with that if it comes to understanding the logic of economic arguments, but possibly, no invariably misleading

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when it comes to historical reconstruction. Yes, I do want to get as close as possible to what 1776 readers thought of Smith—and that's my hobbyhorse!

Best wishes,
Mark Blaug.

Mark responded to a letter of Paul Samuelson one week before in which Samuelson described his own approach to the history of economics as rational-reconstructivist and Whig.

These days for something to be called Whig History is on Monday, Wednesday and Friday a compliment, on the rest of the week a slur. My usual practice, which some might call WH, is to describe a scenario that some readers will agree approximates a scenario that various 1720-1870 writers have commonly contemplated. (Most of them were sometimes incoherent and on different pages seemed to write down partially inconsistent words).

I then work out what must be the properties of that scenario: must be in 1817; must be in 350 B.C. (under the scenario's specifications about homogeneous labour, heterogeneous capital goods, heterogeneous or homogeneous fixed-supply land(s); non collusive, free-entry with imitation; and usually no interesting imperfect competitions); must be in 1998; must be in 2998. [...] It does not matter whether I use 1910 graphs; or 1890 mathematics; or few syllable words in French, English or Choctaw. I am not making a rational reconstruction of Jones ["Of course you do!" Blaug wrote in the margin of the letter]. But later I may compare and contrast what Jones seemed to say about that specified scenario with my description.¹

Samuelson continued to explain that he was pursuing his own intellectual curiosity and was not—like the pedigree of Whig history, the nineteen-century Whig historian Thomas Babbington Macaulay—"imposing" his views on anyone. So what fault was there in his ‘2x2=4 hobbyhorse’?

This brief correspondence between Samuelson and Blaug summarizes in a nutshell an overarching theme in Blaug's career as a historian of economics. From this exchange of letters, Blaug's unconditional choice for historical reconstructions against Samuelson's defence of Whig history—that Samuelson equates with rational reconstructions—seems clear enough. But looking over his distinguished career, Blaug's preferences had shifted, just as the terms of debate had

¹ Mark Blaug's (unpublished) correspondence was kindly provided by Ruth Towse.
done. Even in this short correspondence different labels abound that are by no means the same thing: rational constructionism, historical and rational reconstructionism or reconstructivism, Whig history. The first edition of Economic theory in retrospect (1962) started with the distinction between an ‘absolutist’ and a ‘relativist’ way of writing the history of economics, and clearly expressed Blaug’s preference for the first. In the fifth and last edition (1996), published two years before his brief exchange of letters with Samuelson, he wrote that “in due course” he had come to have “second thoughts about both the choice between these two viewpoints” and about the terms of choice. Consequently, “relativism” and “absolutism” had become historical and rational reconstruction (Blaug 1996 [1962], xvii), and had prompted him to revise his treatment of some of the controversies in the history of economic ideas.

In this essay I will discuss Blaug’s shifting historiographical position and the shifting terms of historiographical debate. I will do so against the background of Blaug’s personal life history, that is heavily intertwined with the increasingly beleaguered position the history of economic thought found itself in after the Second World War. I will argue that Blaug never resolved the tensions between historical and rational reconstructions, partly because he never fleshed out a viable notion of historical reconstruction, and this despite his later leaning towards such an approach. I will trace Blaug’s difficulties in taking historical reconstructions beyond foot stomping to his firm conviction that the history of economics should be a history of economic ideas that would speak to economists, a conviction he adhered to even as late as his historiographical essay of 2001 that bear the ominous title: “No history of ideas, please, we’re economists”.

BLAG’S WORK AND BACKGROUND

Without any doubt, Mark Blaug was the most important Dutch-born historian and methodologist of economics. Among economists, he is best known for his Economic theory in retrospect, a textbook that went through five seriously altered editions from its first publication in 1962. Samuelson once wrote that at his visits to the libraries of Harvard and MIT he was struck by the ‘revealed preferences’ of students; the pages of Blaug’s book were much dirtier by use than those of Gide and Rist, Roll, Spengler, and even than those of Schumpeter’s The history of economic analysis of 1954 (see Samuelson 1987, 56). Historians of economics will
also know Blaug from his *Ricardian economics* of 1958, the book version of his PhD thesis supervised by George Stigler and Terence Hutchison. Methodologists will know him from *The methodology of economics: or how economists explain* of 1980. Blaug produced an impressive stream of essays for scholarly journals, including his work in economic history, economics of education, and cultural economics. The first volume of the series of anthologies of economic essays—which he edited with Edward Elgar—was devoted to the historiography of economics. That is hardly surprising, since writing the history of economics was his enduring interest.

Mark Blaug was born on the 3rd of April 1927 in The Hague in a family from Austrian-Jewish origin. He grew up in Amsterdam in the 1930s (where he remembered playing with Anne Frank). His father ran a successful business in raincoats that was located at the Keizersgracht in the city centre, close to the (then) headquarters of the daily newspaper of the communist party. His family left the Netherlands in time. Blaug entered the United States via England and started studying economics at a time when the discipline fundamentally changed its direction. Econometric tests of theories on statistical datasets were first introduced in empirical research. Diagrams and mathematics became standard tools in the economist’s toolbox. In addition, the large influx of soldiers in the American universities after the Second World War changed the character of universities and university teaching. These changes fundamentally influenced the way in which Blaug, brilliantly and ambivalently, approached the history of economic ideas in the post-war period.

After being denied tenure at Yale in the early 1960s, Mark Blaug left the history of economic ideas for some fifteen years and moved to London to work on applied research in educational and labour economics. He became acquainted with the work of Imre Lakatos, whose approach to philosophy of science made an impression on Blaug’s own thinking about economic methodology—as witnessed from his *The methodology of economics: or how economists explain* of 1980, an intense exploration of the fruitfulness of Lakatos’s notion of ‘scientific research programs’ in understanding the discipline of economics. He also developed an expertise in cultural economics, and the appointment of his wife Ruth Towse—an outstanding cultural economist—at Erasmus University in Rotterdam, brought him back to the Netherlands.
By the end of the 1990s, he combined part time appointments at Erasmus University and the University of Amsterdam. In Amsterdam he was the third to occupy the chair of the Dutch historian and methodologist Joop Klant, after Neil De Marchi and Mary Morgan. The Amsterdam chair was one of the few self-standing positions in history and methodology of economics and recently fell victim to a severe reorganization of the economics faculty. This is a sure sign of the embattled position of history and methodology in contemporary economics. Be that as it may, Blaug's return to Amsterdam brought him back to the borough where he spent parts of his youth, close to the Hortus Botanicus and the Zoo.

**STUDENT OF GEORGE STIGLER**

Blaug's own history in economics started at Columbia University, where he wrote his PhD thesis on David Ricardo with George Stigler (and Terence Hutchison as second supervisor). Blaug started his dissertation work after receiving an unexpected offer of the Guggenheim Foundation for a research fellowship (“But I did not apply for it!”, “Yes, you did”).

The offer directly followed his forced resignation from his temporary teaching job at Queens College, New York in 1953 after he had signed a student petition in support of his colleague Vera Schlakmann who had fallen victim to the McCarthy committee on un-American activities because she refused to testify. The student-petition that asked for her reinstatement had to be signed by a faculty member that unsurprisingly proved difficult to find. As there was nothing in her teaching or behaviour that Blaug could think of as un-American, he simply could not refrain himself from signing. He did sign the petition in the morning and by 2 pm the college president gave him an ultimatum: either resign voluntarily or be sacked (and blacklisted).

The offer of the Guggenheim Foundation showed that behind the scene people tried to save the careers of promising scholars like Blaug. He rented a small apartment at the back of the British Museum to read in the British Library on a daily basis. He remembered this as one of the best experiences in his life—and a life changing experience. Sitting in the British Library Reading Room, “reading, reading, reading, and taking notes”—that was the kind of life he wanted to live for the rest of his life (see Fountain 2007).

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2 These comments are from an interview with Blaug, see Fountain 2007. Blaug wrongly remembered the offer to be from the SSRC, see Backhouse 2012.
Despite Stigler’s position at the opposite end of the political spectrum (as a student, Blaug had communist sympathies) they got on very well. They shared a love for books, not just as objects, but to read them cover to cover. Both had uncompromising high academic standards and they shared an interest in the history of the field, especially in the work of David Ricardo. They certainly did not share the disdain for the “wrong opinions of dead men” that economists on John Maynard Keynes’s authority attributed to Arthur Pigou. But Stigler’s line in the second issue of History of Political Economy that “one need not read the history of economics—that is, past economics—to master present economics” (Stigler 1969, 217) well expressed the growing consensus that reading in the history of economics for an academic economist is a dead weight loss. And this, despite the niceties Stigler had to say about the intrinsic merits of studying the history of ideas.

Back in the United States, Blaug replaced William Fellner at Yale for his course in the history of economic ideas. The meticulous preparation of this course resulted in Economic theory in retrospect. Written as the textbook for which he is now best known, it did not earn him tenure. The Yale of the Cowles Foundation and James Tobin had lost interest in the history of the discipline, if it ever had any. We learn from Roger Backhouse’s (2001) investigation of subsequent editions of the book how Mark Blaug became increasingly critical of formalism in economics—l’art pour l’art mathematical modelling that went untested—misconceptions about Adam Smith’s most famous phrase, delusions about instantaneous market equilibrium at all times, and how he became increasingly sympathetic towards approaches in economics that aimed at doing justice to the context of the economists’ ideas and to Austrian conceptions of the market as a process (see Davis 2013). Blaug moved from ‘rational’ to ‘historical’ reconstructions, a distinction that he came to prefer over the earlier distinction between ‘absolutist’ and ‘relativist’ histories of Economic theory in retrospect. The labels remained confusing and loaded with different connotations, bearing the marks of the different discourses they came from and we will see that Blaug never succeeded in clarifying them.

**History of Economics in an Age of Modelling**

Blaug’s difficulties in clarifying the approaches to doing history of economics had everything to do with the momentous change of the economics discipline following the Second World War. Before the war,
there was no clear distinction between the history of economic ideas and economic theory proper, and this despite Pigou’s alleged dismissive remark about the wrong ideas of dead men. Especially, but not exclusively, in the Continental economic tradition, to do theory was to engage with the books and essays of predecessors; political economists of the past were as much part of economic discourse as economists of the present. Where a review of the literature nowadays is section one of a research paper, an obligatory rite de passage that serves to situate the paper’s contribution to ‘the field’, after which the real theorizing starts, theorizing in the earlier days was essentially a bookish endeavour. In this earlier approach to theory, there was no neat split between theory and empirical data, rather theory and data were convoluted; economists synthesized past thinking and statistical and other observations to understand the questions at hand (Maas 2011). Books were the medium of thought, history of ideas a natural component of the profession.

It is perhaps important to note that this earlier approach to theory was not considered part of history; for an economist to engage with historical texts did not turn the economist into a historian, of ideas or otherwise. Rather, ideas that were voiced by historical actors were incorporated in contemporary thinking. Indeed, since John Stuart Mill’s struggles with history (see De Marchi 2002), economists have boasted to have a method and principles that set them apart from the historian. The outcome was that an early twentieth century economist could still read Mill’s Principles to think and theorize about the nature of contemporary business cycles, but did not think of himself in any way as a historian. Even once the distinction between theory and data started to settle in, someone like Wesley Clair Mitchell still canvassed past authors for their theories on the business cycles to opportunistically use their ideas to understand present data—e.g., in his Business cycles: the problem and its setting (1927).

After the Second World War such a bookish approach to theory became increasingly obsolete due to the modelling practices that emerged in the various top academic institutes and research institutes in the US. These modelling practices were far from uniform, but coincide in that theory became identified with the explicit statement of assumptions that were then used in a modelling exercise from which

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3 For the most recent account of the emergence of this multifaceted practice in economics, see Morgan 2012. See also Boumans 2005.
conclusions could be derived. Economists increasingly lost the ability of how to approach a bookish text for theorizing. Mary Morgan recently quoted Robert Lucas on his experience in reading Keynes: “You had to have an intermediary to get close to the General Theory. Somebody had to help you get at it” (quoted in Morgan 2012, 221 n8). Such intermediaries became themselves models, such as the small model Hicks developed to understand Keynes. To understand the meaning of a bookish theory became to formally state its assumptions, preferably in mathematical form, and then to logically deduce its consequences.

The practical effect was that economists became engaged with models rather than with the texts of their predecessors. Mirroring the split in Victorian Britain of political economy in economic history and economics that followed on the marginalist revolution, after the Second World War an economist with a passion for books instead of modelling became a historian, and this is precisely how George Stigler—Blaug’s thesis supervisor—defended the history of economics; as an antiquarian endeavour that might help train the fibres of the brain, but did not substantially help build economic theories. For economists, history of economics became the business of antiquarians. The history of economic ideas became only problematic when economists abandoned economic theorizing as an engagement with the ideas of predecessors and replaced this with a modelling approach to theory and an approach to history in which the ideas of predecessors were benchmarked in a vocabulary that modellers could understand and work with.

Mark Blaug geared Economic theory in retrospect of 1962 to the needs of this new generation of economists. Blaug included extensive reading guides to classics such as Adam Smith’s The wealth of nations or Alfred Marshall’s Principles of economics, and he presented the theories of past economists in the vocabulary of contemporary economists. His revised treatment of Keynes for the fourth edition of Economic theory in retrospect of 1985 is a good example. Rather than discussing The general theory itself, which according to Blaug would mean to indulge in endless discussions on ‘what-Keynes-really-meant’ (Blaug 1985 [1962], i), Blaug subsumed Keynes under the chapter heading ‘macroeconomics’ and examined the ‘Keynesian system’ in terms of the Hicks-Hansen income-expenditure model, that is, the IS-LM model—the model every economic student around 1985 would have been familiar with and would have been able to work through.
analytically. Blaug concentrated on the logic of the argument, that is, the logic stated in modern terms.

As indicated, in the 1962 introduction, Mark Blaug explained this approach in terms of the distinction between an ‘absolutist’ and a ‘relativist’ approach to the history of economic ideas. An absolutist approached the history of economics in terms of its theoretical and empirical progress, a relativist valued and explained economic ideas against the changing context in which they emerged. For an absolutist the focus was on assumptions and logical structure, not on ideas in context. According to Blaug, no historian of economics could be considered to fully maintain either an absolutist or relativist position. But the absolutist position was the more attractive. Even though there was the danger of judging old thinkers by modern standards, there was also, Blaug approvingly quoted Samuelson, the reverse danger for the relativist of not recognizing the same substance in the thoughts of past economists, “because they do not use the terminology and symbols of the present” (Blaug 1985 [1962], 1; quoting Samuelson). Blaug even wondered if a relativist position was not self-defeating. How were theories to be judged on their respective merits if they were all a “faithful reflection of contemporary conditions” (1985 [1962], 2). If by context was meant such diverse things as “Zeitgeist, social milieu, economic institutions and philosophical currents”, Blaug concentrated in contrast on the “internal logic of theory” and doing so implied for the historian to “willy-nilly becoming an absolutist” (1985 [1962], 7).

Economic theory in retrospect thus served the needs, if any, of post-war generations of economists for history of economic ideas. Or perhaps one could better say the possibility of such a history in the post-war context. These ideas were treated in terms and in a method that was recognizable to the modern economist; in the modern analytical terms earlier economists were short of, but grasped at. The ensuing model was then investigated on the logical (and less so empirical) claims it entailed. This was an absolutist approach to the history of economics in two ways; first, it supposed there was progress of economic ideas in the direction of the present; and second, it supposed it was possible to isolate the kernel of past thinking that could then be examined on its logical structure. Claiming progress and examining logical structure are of course different things, but for Blaug they were (willy-nilly) connected because criteria for progress were found in the advancement of the logical structure itself. If we consider
logical structure, we can then distinguish between the logical structure of a book or theory in its own terms and its logical structure in modern terms. During the 1990s, Blaug came to consider this last difference as one between 'historical' and 'rational' reconstructions.

The implicit assumption Blaug (and many of his contemporary economists) made in Economic theory in retrospect was that the vocabulary of modern economics was more transparent and therefore an improvement on the vocabulary of its predecessors. Progress was implied in logical structure. Clarifying logical structure could also improve original texts that, otherwise, remained opaque. Examining the logical structure of a theory in its own terms was thus automatically subsumed under the examination of logical structure in modern terms.

A good example is the so-called ‘corn-model’ that Piero Sraffa distilled from Ricardo’s Principles (see Blaug 1985 [1962], ch. 4, sec. 31). Sraffa’s corn-model solved the determination of the rate of profit “in purely physical terms without entering into the question of valuation”. However, “on balance” Blaug considered “that Ricardo never went so far as simply to assume that wages were entirely spent on wheat and that all manufactured products are luxuries which are never consumed by workers”, and these two assumptions had to be made for the corn-model to work. Thus, even though one could distil the corn-model from Ricardo’s writing, this was not the model Ricardo himself had “in the back of his mind”. For that reason Blaug considered Sraffa’s “ingenious argument” a “rational reconstruction”. But this left unexplained how a historical reconstruction of Ricardo would have looked like, and it certainly did not give a reason why a historical reconstruction would be superior to a rational one. To the contrary, Sraffa’s exercise showed the limits of Ricardo’s system.

**History as an Extended Presence**

The star example of an approach to the history of economics that highlights logical structure in modern terms was in fact not given in Blaug’s Economic theory in retrospect, but by Paul Samuelson’s so-called ‘canonical classical model’. Samuelson, like Stigler, was educated in bookish fashion and Samuelson did not tire of emphasizing that he self-educated himself in the mathematics that was scorned by economists at Harvard and Chicago. History of ideas was part and parcel of their disciplinary upbringing and actively pursued by both. But Samuelson approached the history of economics as he approached any other topic:
What is the substantial question one can distil? Formulate that in a small coherent model. Look into the different scenarios that can be derived on its basis (and only look at the interesting scenarios among the many). State your conclusions. Possibly, look back at issues you left out that might complicate or alter your findings. It would make no difference to Samuelson whether his models were concerned with historical predecessors or with contemporary issues.

We can find this approach in his substantial economics papers—think of his multiplier-accelerator model of 1939, his joint paper with Stolper of 1941, his exact consumption-loan model of 1958, to give just a few examples—and in his papers in the history of economics as well. In his so-called ‘canonical classical model’ (1978) Samuelson claimed to have captured the essential elements of the theories of Smith, Malthus, Ricardo, J. S. Mill, and Marx (their ‘cash value’). By concentrating on a world specified with “homogeneous labour, heterogeneous capital goods, heterogeneous or homogeneous fixed-supply land(s); non collusive, free-entry with imitation; and usually no interesting imperfect competitions”, Samuelson (1988, 161) ironed out any substantial differences there might be between Smith, Malthus, Ricardo, Mill, and Marx, but also did not bother if these assumptions could be found in the original texts themselves. After all, he was not making a rational reconstruction “of Jones”. But only creating a scenario that he then would use to see to what extent what Jones had to say about a specific question could be explained with his, that is Samuelson’s, scenario.

“Of course”, Blaug wrote in the margins, Samuelson was making a rational reconstruction. Recreating old texts in the modern vocabulary of the economist became exactly what Blaug considered a rational reconstruction to be. Samuelson self-confidently labelled his approach to the history of economics “Whig history”, and gave a vigorous defence of it in his keynote address to the History of Economics Society in 1987. When Cigdem Kurdas (1988) pointed out there was a difference in treating an author in his own terms and in modern terms, Samuelson ignored rather than confronted the evidence.

Kurdas concentrated on Samuelson’s treatment of Smith. For example, Samuelson’s assumption of constant returns to scale—a standard assumption of modern production theory—was nowhere to be found in Smith’s The wealth of nations and in fact contradicted by the link Smith saw between the extent of the market and the increase in the division of labour. But also Adam Smith’s observation that the
“improvement of the productive powers” of agriculture “does not always keep pace with their improvement in manufactures” (Smith 1976 [1776], 16) was a “far cry from the inexorable downpull of land scarcity in the canonical model” (Kurdas 1988, 18). In response, Samuelson preferred to “keep Whig history honest”. “Debating interpretations” was only an “adversary procedure of research” (Samuelson 1988, 161), and rather than opening cans of worms, Samuelson reaffirmed the assumptions of his canonical model as the only ones relevant for a modern economist.

Thus, Samuelson treated Smith, Ricardo, and other past economists very much as contemporaries, indeed just as (political) economists had done in earlier days, with the important difference that he no longer engaged in an interpretation of their words, but replaced them with the vocabulary and assumptions of the modern economist. Whatever Samuelson might write to Blaug in later years about the indifference of the language chosen for the conclusions derived—graphs of 1910, nineteenth century math, French, Choctaw, English—was merely rhetorical. The true litmus test was the language of the modern economist: homogeneous labour, heterogeneous capital goods, heterogeneous or homogeneous fixed-supply land(s), free entry with imitation, and no imperfect competition. Put that in a small model and see what happens. And this was, as Samuelson wrote in an exchange of 1974 with W. Baumol on the interpretation of Marx’s transformation problem, for him the only way to treat past economists as contributing to the “collective house of knowledge” and not “as a historical deity or oddity”. It was to “appraise” past theories as “a journal referee would treat any serious contribution” (quoted from Blaug 1990, 30).

For Samuelson “within every classical economist, there is to be discerned a modern economist waiting to be born” (1978, 1415). If history wanted to speak to the economist, the logic of an argument was to be explained in the language and the tools of the modern economist, no matter if there might be another way to approach the logic of a text. Again, in his keynote lecture to the History of Economics Society, Samuelson claimed he had captured “in one diagram what is held in common by Ricardo, J. S. Mill, Malthus, Marx, and Smith” (1987, 56). The implication of Samuelson's identification of early twentieth century diagrams, end of nineteenth century mathematics, and one syllable prose of French, English, or Choctaw, did not mean the logic of an argument itself could be historicized, but the contrary; Samuelson turned the history of ideas into something that was time and
place invariant. Consonant with the move in the philosophy of science of the fifties and sixties, theory became general, time and place independent, its logic identical with what Thomas Nagel (1986) nailed down as the “view from nowhere” (see also Hands 2007).

History of economic ideas became the history of—in Kenneth Boulding’s words—an “extended presence”; dead authors might be part of the conversation, even when they did not participate in it “directly” (Boulding 1971, 228). The difference with the earlier mode of theorizing is seated in the subordinate clause. While in earlier days economic predecessors were made to participate directly in the conversation, now they only did so indirectly, in the vocabulary of modern economic discourse. Samuelson contrasted the interests of the “honest Whig historian” with those of the “antiquarians”, who did not contribute to modern analysis, but indulged their attention to the investigation of context and details of original texts that were irrelevant to the logic of arguments.

Contrasting antiquarianism with theoretical relevance meant that to put ideas in context was of no use for the economist. It was Samuelson’s Whig historian, or rational reconstructivist approach that put any other approach to history on the defensive. Implied in Samuelson’s notion of the Whig historian was perhaps not even progress, but irrelevance of any approach that did not contribute to contemporary theorizing. The tables had turned against bookish theorists. “After Samuelson, who needs Adam Smith?”, as Kenneth Boulding put it in his contribution to first issue of *History of Political Economy* (1971). The meeting of the small group of historians in Britain in 1968, where the launch of this specialized journal for the history of economic thought was feared to increase the marginalization of historians within the economics discipline, was exemplary for the uneasy and beleaguered position historians of economics came to find themselves in. But it was modern theory that first defined them as historians, and then, as antiquarians.

**RATIONAL AND HISTORICAL RECONSTRUCTIONS**

In the first edition of *Economic theory in retrospect* of 1962 Blaug had self-confidently chosen sides with modern economics—with the rational reconstructivists (for an ‘absolutist’ instead of a ‘relativist’ writing of economics’ history). There was a practical reason for this, even though I am not sure how heavy this weighed for Blaug. Intended as a textbook, Blaug could not just indulge in the texts of the classics, as he had enjoyed so much in the British Library Reading Room. He had to clarify
their ideas to contemporary economics students, educated in the language of Samuelson’s *Economics* (1948)—if not of the *Foundations* (1947). But Blaug fundamentally considered, just like Samuelson, that the logic of arguments was better articulated in the language of modern economics, and a consistent relativist position impossible to maintain. “Relativism” saw past theories as “faithful reflections of contemporary conditions, each theory being in principle equally justified within its own context” (Blaug 1985 [1962], 2). It is significant these words were still there in the fourth edition of 1985. Around that time Blaug’s sympathies had started moving in the opposite direction towards historical reconstructions. The final edition of *Economic theory in retrospect* (Blaug 1996 [1962]) changed as a consequence, but it was transformed unevenly. Refined historical reading guides were followed abruptly by concepts and diagrams that were, if anything, rational, not historical, reconstructions. We find this same unevenness in his article on the historiography of economics of 1990.

This article was perhaps triggered by the exchange between Samuelson and Kurdas and was published in the successor of the *History of Economics Society Bulletin*, the *Journal of the History of Economic Thought* (JHET). Blaug took an explicit stance against the “cliophobia” of economists. He transferred Richard Rorty’s (1984) distinction of four ways to write the history of philosophy to the history of economic ideas. He identified the first, *Geistesgeschichten*, with contextual history, the second, historical reconstructions, with interpretations of the ideas of past economists “in their own terms”, the third, “rational reconstructions”, as a treatment of past thinkers as “contemporaries”, the fourth, doxographies, as hagiographical writing that should be avoided. Blaug linked these distinctions to his own earlier distinction between “absolutist” and “relativist” approaches to the history of economic ideas. But Blaug missed the opportunity for a more in-depth discussion of these different labels and limited himself to the observation that the distinction between an absolutist and a reconstructivists stance was “a subtle one” (Blaug 1990, 28).

This is remarkable. We have already seen that the absolutist approach of *Economic theory in retrospect* entailed: 1) the belief in theoretical progress in economics, and 2) the reconstruction of the logic of old theories in contemporary terms. For Blaug, 2) entailed 1), and this he labelled, following Rorty, rational reconstructivism. We have already seen that 2) does not need to entail 1), and even if we believe
in theoretical progress, there is no need to reconstruct old theories in modern dress.

This does not mean the only alternative is to use the vocabulary of the past—Blaug’s notion of historical reconstruction. Rather, one could show how concepts, methods, and theories developed over time. It is such an approach to the writing of history that one would have expected to find in Blaug’s work, but that was conspicuously absent. That is, an approach to the writing of history along the lines of Imre Lakatos's distinction between rational and historical reconstructions. From Blaug’s own life history, as from his *Methodology of economics*, we know how impressed he was with Lakatos’s philosophy of science, and so it is a puzzling question why Blaug used Rorty’s rather than Lakatos’s distinction between rational and historical reconstruction to clarify his historiographical views.

In Lakatos’s *Proofs and refutations* (1976) the notion of a rational reconstruction also refers to the logic of arguments, but does not impose this logic in our modern terms. Rather, Lakatos aimed to show how proofs in mathematics emerge from a dialogue of arguments and counter-arguments in which proofs, concepts and procedures are proposed, challenged, and changed over time. Lakatos thus showed that what might appear as an incontrovertible proof *sub specie aeternitatis*, could be reconstructed as having a history. However, a rational reconstruction presents a logical sequence of arguments, it does not present these arguments in their historical order of appearance; Lakatos’s famous classroom discussion was a rational reconstruction of the actual positions that were held historically. This was Lakatos’s distinction between rational and historical reconstructions; a rendering of the logic of the argument versus a rendering of the actual course of events that he famously delegated to the footnotes.

Lakatos’s notion of rational reconstruction enables us to see how historical positions can be reconstructed as an exchange of arguments that may lead to the present, but do not need to be framed in modern vocabulary, as in Blaug’s (and Samuelson’s) notion of rational reconstruction. Lakatos also cut the cake between theoretical substance and context differently than Blaug. For Lakatos, historical reconstructions were about the actual order of historical positions (that might differ from its rational reconstruction). This historical order could (and would) bring in context that would help explain why the historical order of argument differed from its logical reconstruction.
The first four editions of Blaug's textbook worked with a notion of relativist writing of the history of economic ideas that only in the last edition became equated with a narrowed down notion of historical reconstruction. A relativist writing of the history of economic ideas treated the ideas of historical actors in their own terms, but also discussed contextual factors: “Zeitgeist, social milieu, economic institutions and philosophical currents” (1985 [1962], 7). In the fifth edition, the meaning of historical reconstructions was narrowed down to an understanding of a theory in terms historical actors “would have recognized as a faithful description” of their intentions (1996 [1962], 7). This distinction between rational and historical reconstructions left questions of historical context and order orphaned; they were removed from the introduction’s main text to a “note on further reading”, and were, as in the earlier editions, hardly treated at all: “There is little in the chapters that follow about Zeitgeist, social milieu or the personalities of the great economists of the past […] simply because the focus of the present book is on theoretical developments” (1996 [1962], 9).

One could equally say that from Blaug’s rational reconstructivist viewpoint Economic theory in retrospect was not about theoretical developments at all, but rather presented different theoretical ideas from a presentist perspective.

Historical reconstructions were left into a dead-end, because of the impossibility of discussing past theories “as if we can forget what we now know”, but more importantly because it is “literally impossible” to put ourselves in the position of past authors. Thus exactly when Blaug increasingly came to doubt the merits of an absolutist—now rational reconstructivist—way of writing the history of economics in favour of historical reconstructions, he robbed himself of the means of doing so, means that he at least on a Lakatosian reading of the difference between rational and historical reconstructions might have had at his disposal. And thus Blaug’s intervention in JHET on the historiography of economics, and the “cliophobia” of economists, turned into a defeat of historical reconstructivism despite Blaug’s clearly changed sympathies to the latter.

**CHEZ VOUS, SANS VOUS**

Most of Blaug’s JHET discussion of 1990 focused on the difference between historical and rational reconstructions, in which Blaug clearly found difficulty in defending the first and faulting the last. There was
nothing wrong with rational reconstructions, or absolutist approaches to the history of economics if the target was the logic of the argument. “Absolutism” was more defensable than “relativism” (Blaug 1990, 28). “Rational reconstructions are perfectly legitimate” (1990, 30). “It is not easy to see how anyone can deny the value of rational reconstructions as such” (1990, 31). “There is nothing wrong with this as rational reconstruction” (1990, 32). “Once again, the corn model is a valid rational reconstruction of Ricardo but it is probably not a sound historical reconstruction” (1990, 33). And so on.

Against these sentences, Blaug’s well-intended defence of historical reconstruction looked rather bleak. Blaug referred to the discussion between Baumol and Samuelson on the transformation problem, where the balance of arguments was in favour of the rational reconstructivist. Baumol’s “historical reconstruction” of Marx was no doubt “correct”, but showed that “Marx was only straining to accomplish the impossible”, as became clear “with a twinkling of the eye” (Blaug 1990, 29) with Sraffa’s rational reconstruction. A historical reconstructivist acted like an econometrician fitting a regression line through obscure and contradicting statements of past authors (1990, 30-31), but Blaug missed the opportunity to point out that on a Lakatosian notion of rational reconstructivism, the obscure and contradicting statements of past authors offered the resources to reconstruct the development of the logic of arguments. Nowhere did Blaug come much further than defending historical reconstructions as an expression of the ideas of past thinkers “in their own terms”, but what value added such writing had over those of rational reconstructions in contemporary terms remained unclear. Indeed, on Blaug’s own account historical reconstructions bordered on the antiquarian.

Samuelson, just as Blaug, never afraid of a fight, jumped on Blaug’s short essay in an exchange of letters between Samuelson, Patinkin, and Blaug. This quickly drifted off into a discussion between Samuelson and Patinkin on whether Samuelson “validly” summarized Cassel’s *Fundamental thoughts in economics* of 1925 in a small model, using Cobb-Douglas production functions and Leontief’s “homogeneity postulate”. As with his ‘canonical classical model’, Samuelson was imposing the vocabulary and assumptions of the modern economist on past authors. Despite Patinkin’s reservations against Samuelson’s “loose constructionism”, he engaged in a discussion of technicalities on Samuelson’s proposed model.
The distinction between rational constructivism, and rational reconstructivism went unnoticed, and it is interesting that Blaug, in the reply to Samuelson with which I started this essay, equally slipped from rational reconstructions to rational constructivism. This last phrase expresses much better what the absolutist or Whig writing of the history of economic ideas is all about. Samuelson was simply imposing a different text; the text of modern economics. In the published correspondence in JHET, Blaug, the historian, was eventually squeezed out of a discussion that had started with an exchange between Samuelson and Blaug. Chez vous, sans vous as the French snapped at the Dutch on the occasion of the signing of the Treaty of Utrecht in 1713 that ended a long century of wars in Europe.

**NO MARCH TO PROGRESS**

Blaug’s orphaned notion of historical reconstruction left the historian un-armoured. This can be witnessed from a note in the *European Journal of the History of Economic Thought* of Sraffian Rodolfo Signorino on Blaug’s historiographical views. Was a historical reconstruction “a more or less faithful description of what [past thinkers] had set out to do” (Signorino 2003, 329; quoting Blaug), one that gave an account of their ideas “in their own terms” (Blaug 1990, 28)? Signorino could easily point out that giving such an account was turning historical reconstructions into an “empty box”. There was no way in which one could circumvent the problem of multiple interpretations, of the baggage an interpreter brought to a text, or—the greatest problem of all—to establish what an earlier thinker “really thought”.

On the other end, Signorino signalled that Blaug identified rational reconstructions (just like Samuelson) with Whig history—history written “as a march of progress from past errors to present truths”, while an analysis of the logic of an argument—either in the original or in modern terms—in no way entailed such a progressivism. Blaug’s grudging counter-note evaded an answer, but I think one of the reasons that Blaug came to prefer the label of rational reconstructions over that of an absolutist writing of the history of economics is because he had lost faith in economics’s march to progress, as witnessed best from his acerbic attacks on the increasing formalism (and hence practical irrelevance) of economic theory. But in his reply to Signorino he did not distance himself from the idea of progress in economics. Blaug limited himself to repeating that the only thing “we as historians of ideas ought
to be aiming for”, was historical reconstructions “despite the fact” these were “strictly speaking impossible” (Blaug 2003, 607), and turned to arguments ad hominem by speculating on what was “really motivating Signorino’s over-the-top attack”, namely Blaug’s 1999 attack on the Sraffians (2003, 607). But this did not bring Blaug’s own polemical arguments in favour of historical reconstructions beyond mere foot stomping.

Blaug’s answer to Signorino was clearly not his best showing. If historical reconstructions were “strictly speaking” impossible, Signorino’s criticism was simply legitimate and pertinent. But Blaug’s narrowing of historical reconstruction to what historical actors “really meant” was not the only alternative available and neither was his presentist notion of rational reconstruction. Earlier, I briefly discussed the possibility that Blaug might have seized upon to develop the notion of rational reconstruction in a Lakatosian direction. Let me, as a conclusion, consider two alternative ways for practicing historical reconstructions.

**IDEAS IN CONTEXT**

The two alternatives I will consider were present at the conference of historians at the end of the sixties, embodied in two persons that one may situate at two ends of a scale with ideas on one side and context on the other: Donald Winch and Bob Coats. Put otherwise, Winch and Coats embodied different notions of “context”: intellectual and social. Reading the research agenda Bob Coats outlined in the first issue of *History of Political Economy*, “context” was “social context”; Coats more or less established (historical and contemporary) studies of the sociology of economics.

Yes, it made a difference whether political economy was practiced in the Common Rooms of Oxbridge Colleges, in the London Clubs, in German princely administrations, in Select Committees, in Vienna coffee houses, in academic institutions, in parliaments or Think-Tanks. Despite its weaknesses, Marion Fourcade’s recent *Economies and societies: discipline and profession in the United States, Britain, and France, 1890s to 1990s* (Fourcade 2009) is a good example of history of economics writing that aims to examine how different institutional structures shape different styles of economic practice. Fourcade wrote a historical

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reconstruction of economics that examined the institutional structures of the profession Blaug had relegated to the “notes on further reading”. In George Akerlof’s words on the back of the book she “demonstrates irrefutably that economists are as much influenced by where they are located as by their supposed adherence to ‘scientific method’”. Fourcade’s book is all about the institutional structures that economists, such as Samuelson, but also historians, such as Blaug, considered peripheral to an understanding of the theories and methods of economists.

Perhaps for that reason Fourcade’s book is looked at with distrust by an older generation of historians of economics that shares Blaug’s predilection for an understanding of the logical structure of theories and ideas. But also scholars of a younger vintage, such as Ivan Moscati (2008, 86), emphasize the importance of logical structure over institutional context. Indeed, according to Moscati, it is only in the texts where the action is. His fear is that a focus on context makes us lose all connection with the economics discipline. And what then are we going to teach our students: no ideas, please, we’re historians?

Donald Winch stands for the alternative notion of “context” I want to consider here, intellectual history, just as Quintin Skinner, one of the editors of the renowned Cambridge series ‘Ideas in Context’. With his meticulous charting of the intellectual influences on Robert Malthus and dissection of Malthus’s reasoning, Winch exemplifies an approach to the history of economics that comes closest to Blaug’s answer to Samuelson, “to get as close as possible to what 1776 readers thought of Smith” or to try to “see the past as much as possible as the past saw itself” (Blaug 2003, 607). Winch analyses Malthus’s texts in terms that were available to him, and in relation to the economic-socio-political problems he faced and perhaps even tried to influence, and so for those who read Malthus.

The book of Blaug that comes closest to this is without any doubt his first one, the result of his daily visits to the old British Library Reading Room, “reading, reading, reading” and taking notes, *Ricardian economics* that had as its subtitle: ‘a historical study’. If Blaug ever made a historical reconstruction, it is in this book. He examines Ricardo’s work in terms available to himself, and then concludes on the textual and contextual evidence available that the *Wirkungsgeschichte* of Ricardian economics was only limited—much more limited than most economists in the 1950s in search for Giants on whose shoulders they could stand, would have been (and were) willing to admit.
Marion Fourcade considers herself an economic sociologist, not a historian of economics. Donald Winch is an intellectual historian who does not think of himself as economist. Their work is different from that of the economist. Even though economists may be interested in their work, as Akerlof shows interest in that of Fourcade, or Amartya Sen in that of Winch, such an interest does not earn one a job in an economics department, as it did not for Blaug in the early 1960s. The job article has to be an economics article, in a top mainstream journal, addressing an economists’ audience first. Nowadays, it certainly should not be a book. But Mark, the historian, did consider himself an economist first, he wanted to talk to and with economists, he considered the economics profession his first biotope. He considered his reading jury not historians, but economists.

Mark Blaug was, at the end of the day, interested in the arguments of economists, but also remained a man of books in a profession that was increasingly alienated from them. Already in his review of Economic theory in retrospect, H. D. Dickinson (1965) rightly saw the “division between head and heart” that Blaug never solved. History was for Blaug history of ideas, not of social context or research practices. Economists after Samuelson, including Blaug, would automatically take an examination of ideas as a logical, not as a historical exercise, and that came to mean using the logic of contemporary economics.

That determined, in my view, Blaug’s ultimate ambivalence on the merit and meaning of historical reconstructions once modern economics had excluded a direct engagement with historical texts as part of its approach to theory, except as a hobbyhorse. As an economist, Blaug was all too well aware there is no place for history in economics. The title of Blaug’s (2001) perhaps most programmatic essay exactly captured this: ‘No history of ideas, please, we’re economists’.

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**Harro Maas** is an associate professor at Utrecht University. He published widely in history and methodology of the economics, from the early modern period to the econometric revolution in the Interwar period, and especially in the history of political economy in Victorian Britain. His book on one of the founders of modern economics, William Stanley Jevons (Cambridge University Press, 2005) was awarded the Joseph J. Spengler prize by the (American) History of Economics Society. *Economic methodology: an historical introduction* will appear with Routledge in February 2014. With Mary S. Morgan (LSE), he co-edited the History of Political Economy (HOPE) annual supplement of 2012 *Observing the economy: historical perspectives*.

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Mark Blaug’s unrealistic crusade for realistic economics

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Abstract: Mark Blaug’s normative methodology of economics is an attempt to articulate certain intuitions about how economic science could be improved by making it more “realistic”. I discuss two such articulations, one in terms of falsificationist principles, the other in terms of an alleged trade-off between relevance and mathematical rigour. My conclusion is that Blaug’s methodology is itself unrealistic, both descriptively and normatively. His (well intended) methodological prescriptions for the improvement of economics are not based on a systematic, consistent, descriptively adequate, and normatively viable account. I suggest that Blaug’s intuitions can be developed into a more realistic account by incorporating the analysis of two further topics: economic modelling and the institutions of academic research.

Keywords: economic methodology, falsificationism, realism, realisticness, relevance, rigour, economic modelling, institutions of inquiry, Mark Blaug

JEL Classification: A11, A14, B31, B41

I first met Mark Blaug in May 1984 in Pittsburgh at the History of Economics Society (HES) meetings. I had read his Economic theory in retrospect (1978 [1962]) and The methodology of economics (1980), as well as many other of his smaller works. I had admired his mammoth knowledgeability as a historian. His fast reading skills and his capacity to store information were exceptional, and this showed in his confident interventions in conference sessions. Obviously, he knew that he knew. He was a superman as a producer and reproducer of historical information.

While my admiration for Mark as a historian was strong (even though not being a proper historian myself, I could not really judge), I was far less impressed by his methodological work. He must have sensed this. In the early 1990s, I was in London for a workshop, and
Mark invited me for lunch in a pizzeria next to the British Library. After some frank and friendly conversations, he suddenly asked, “Why don’t you ever cite my work?” Years later, after the second edition of *The methodology of economics* (1992 [1980]) had appeared, he gave me a copy with his hand-written inscription (dated 21-08-1997): “Since this is a book which you have never seen, I thought that you might like a copy—bedtime reading!”

The presupposition of Mark’s question—i.e., that I never cite him—was not quite true. In 1990, I had published a paper on economic explanation in which I stated that very little had been written on the topic before, and added a footnote commenting that the subtitle of his 1980 book—“how economists explain”—was misleading as there was nothing about this topic in the book. The book was on the epistemology of theory assessment, and not on the practice of explanation in economics.

Our academic and personal relations were respectful and increasingly warm. For a decade, we collaborated (together with Roger Backhouse and Kevin Hoover) as the editorial team of the *Journal of Economic Methodology*. In 1997, I invited Mark to be one of the speakers at the conference on “Fact or fiction? Perspectives on realism and economics” organized on the occasion of my inauguration at Erasmus University Rotterdam. An extended version of his article “Ugly currents in modern economics” (Blaug 1997) was then included in the volume *Fact and fiction in economics* (Blaug 2002). Some years later, Mark joined us in Rotterdam as a visiting professor, to teach history of economics to the students in our programme at EIPE (Erasmus Institute for Philosophy and Economics). He also regularly attended the weekly EIPE research seminar that I coordinated, greatly enriching its lively discussions. We visited each others’ homes (theirs in Leiden, ours in Rotterdam) and had wonderful conversations.

Yet, it remained true that I did not cite him frequently. That this bothered him was fairly understandable. Mark was after a better economic science that would be more realistic, one that would be more relevant to real-world concerns. But so was I! It was obvious that we

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1 Some years before, in the early 1990s, I had invited Mark to contribute to one of the methodology workshops I organized for the Finnish Doctoral Programme in Economics. In the course of those years, several other activists in the field had joined the workshops, including Bert Hamminga, Maarten Janssen, Dan Hausman, Tony Lawson, Jochen Runde, and Roger Backhouse. Mark's response to the invitation, a real letter written with his beautiful handwriting in real ink, was that he would be too expensive to us. I was amused. This was his humour.
shared many broad intuitions about economics and good science. Why then did I not use his work more often? Now that I think of it, the reason probably was that he did not have a systematic and elaborate methodological theory that I could have considered an appropriately “realistic” theory about economics, both descriptively and normatively. In the following brief remarks, I will start explaining and justifying this answer, mainly focusing on Mark’s ideas about falsifiability and relevance. I will then outline two themes that I believe are of central importance for elaborating the intuitions that Mark and I shared, namely: economic modelling and the institutions of economic inquiry.

**ELEMENTS IN BLAUG’S CAMPAIGN: FALSIFIABILITY AND RELEVANCE**

Caricaturing a little, Mark Blaug was a historian of economics who turned into a methodological preacher. His dissatisfactions with much of economics primarily were—or at any rate manifested themselves as—complaints about the procedures and principles that he believed guided economic inquiry. The outcome was bad science, he thought, because the theories economists regularly produce and use tend to be empirically unfalsifiable and—or—practically irrelevant. Blaug appealed to methodology as part of a project of helping economics perform better as a science. Indeed, his use of methodology was predominantly normative. The descriptive component in his normative campaign has mainly consisted of pointing out failures to conform to the principles of good science. It is perhaps somewhat paradoxical that a historian of economics would take on the role of a preacher in his methodological work, (mostly) prescribing rather than (mostly) describing the disciplinary activities of his subject.

Blaug ran a methodological campaign for a more realistic economics, but he did not consistently rely on a realistic methodological theory, or on realistic methodological arguments.² ‘Realisticness’ and ‘unrealisticness’ are (themselves manifold) properties that can be ascribed both to economic theories and to their meta-theories. Blaug’s meta-theoretical claims about economics often were descriptively unrealistic in that they tended to oversimplify and exaggerate, thereby ignoring important facts about scientific inquiry. This fortified the

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² Having a realistic meta-theory about economics is different from having a realist meta-theory. It is not clear whether Blaug could be justifiably characterized as consistently espousing philosophical realism even though I believe he wanted to be so characterized.
normative unrealisticness of his prescriptions about how to do better economics, making them unachievable as requirements for scientific practice and its assessment. In my view, normative methodology is just fine, but in order for the normative assessments and prescriptions to be adequate and effective, one needs to develop a deep descriptive understanding of the structure, presuppositions, and preconditions of the practice that is being normatively evaluated and regulated.

While Blaug made many sharp and intuitively agreeable observations about economics and its deficiencies, he neither adopted nor built an elaborate methodological theory that would have justified and organized these observations. There was no (whether stable or progressively evolving) meta-theory, or a coordinated set of such theories grounding his critical observations in a consistent manner. This may sound strange given that he regularly appealed to Popper and Lakatos and their versions of falsificationist methodology. But first, there indeed were two (or more) different versions of falsificationism, and it seemed the versions were often conflated, and were invoked inconsistently. And, second, he used other methodological arguments—such as those based on the alleged trade-off between rigour and relevance—that were not based on, or even consistent with, falsificationism or perhaps any other systematic meta-theory.

Instead of engaging myself in a thorough and detailed scrutiny of Blaug’s methodological writings, I will focus on two of his favourite arguments and give very brief and selective comments. One of the arguments is on falsifiability, the other is on relevance. Blaug claims that economics fails insofar as it lacks these two properties—and that it often does. In both cases, I suggest that the broad underlying intuitions are on the right track, whereas the specific elaborations are flawed.

**Falsifiability: when theory and facts conflict at two levels**

Blaug believed that good science first develops theories that are falsifiable (in that they entail predictions that can be in conflict with observable evidence), and then attempts to falsify such theories. He accused economics of failing to be good science in this sense. Economics would become a proper science, or a better science, by revising its practices so as to better abide with falsificationist prescriptions. Here is a passage that summarizes the core ideas:

That economists rarely practice falsificationism only demonstrates the need to preach falsificationism day in and day out, always
assuming that falsificationism is in fact practicable in economics and that the history of our subject displays some instances of it (Blaug 1992b, 57).

There is a general background intuition here that I find agreeable. It would be good for economists to be more welcoming of—and responsive to—criticism that questions their theoretical and other ideas, whether minor or major, including criticism that appeals to empirical evidence. The balance between dogmatism and responsiveness to criticism should be critically reconsidered.

What invites special attention in the passage quoted above is the relationship between two ideas, namely between the descriptive claim that “economists rarely practice falsificationism” and the normative proposition that there is “the need to preach falsificationism day in and day out”. This is a version of the familiar tension or discrepancy between theory and the facts. Any such tension can be resolved in two ways, broadly speaking. Both ways seek to align the theory and the facts by removing the discrepancy. One either revises the theory or modifies the facts (or both—to make the set of options complete). These options are familiar in the case when economic theory is perceived to be in conflict with the evidence (or with the world itself). Blaug would generally choose the former option: to revise the economic theory. This is indeed advised by his falsificationism. Yet in the case of methodological theory, his choice was different: he would rather change the facts of economic inquiry while sticking to the methodological theory. The way to change the facts of research practice is to preach the theory of falsificationism. This latter choice (to change the current style of economic inquiry), Blaug hoped, would force economists to be more inclined to make the former choice (to change economic theory) when economic theory and the facts were in conflict.

A necessary condition for judiciously preaching falsificationism “day in and day out” is, as Blaug puts it, “always assuming that falsificationism is in fact practicable in economics”. It must be possible to practice falsificationism for it to make sense to preach it. If it could be substantiated that falsificationism is practicable, the next question would be to ask whether practicing it would be recommendable (and it is possible to argue that it would not). But we do not need to ask the latter question about recommendability if we have answered the previous question about practicability negatively. As we know, the assumption about practicability is questionable—many would say it is a descriptively
unrealistic assumption. If this is correct, then preaching falsificationism would be normatively unrealistic as it would recommend the pursuit of a utopian goal, a goal that cannot be attained, not even with a tolerable degree of approximation.

There is no need to repeat the investigations and debates of the 1980s and early 1990s here (see, e.g., Hands 1992). Among the conclusions was that falsificationism is impracticable in economics. This can be summarized by referring to the Duhem-Quine challenge and the impossibility of relieving it by an effective epistemic control for the inescapable ceteris-paribus clause involved. One can argue that falsificationism is even less practicable in economics than, say, in experimental physical sciences (adding that it has not been accepted as a philosophy of the latter either). Basing testing on predictions—especially its ambitious versions—is harder in economics and is further complicated by the anti-predictivist arguments implied by certain fashionable economic theories themselves.

Even if falsificationism as a specific methodology cannot be sustained, I still think it is important to defend something like it—something that accommodates the important intuitions that Blaug tried to articulate in flawed falsificationist terms. The generic supportable idea is that economic theories and models should be so formulated and used that they can be made accountable to the facts. Forces such as dogmatism, formal elegance or ideological appropriateness should not dominate the acceptance and rejection of economic theories. Blaug himself considered other specific ideas in this spirit. I will discuss one of them next.

**Relevance and its alleged trade-off with rigour**

In the later years Blaug talked less about falsifiability and more about “relevance”, another property of theories that he considered desirable. This gives another sense in which a theory can be realistic. Blaug believed that there is another quality that is in conflict with it, namely what he calls “rigour”.

There is a trade-off in economics (and elsewhere) between rigor and relevance: the more we achieve deductive certainty in our arguments, the less likely it is that we will achieve socially and politically relevant conclusions (Blaug 2009a, 219).
Again, I find the underlying intuitions here quite agreeable. Economics should provide helpful services to solving the important problems of the humankind, and sometimes this would require (putting it very intuitively indeed) less activity on the blackboard and more in the field (while not denying the essential importance of the role of the blackboard). However, things get much more complicated as soon as we start looking at the details of such statements.

My immediate comment on Blaug's statement is that making bold claims about such a general trade-off requires defining the concepts of 'rigour' and 'relevance', with a little bit of more rigour. Unfortunately, it seems that Blaug may have applied a second-order version of the alleged trade-off: as if he wanted so badly to make relevant methodological claims about economics that the attempted relevance of these claims was traded-off for the rigour of the arguments. Let us see what he says.

Blaug briefly discusses some meanings of 'rigour', but he only deals with the kinds of mathematical rigour that he believes characterizes economic inquiry (Blaug 2009a, 220). This is unfortunately narrow as it ignores other kinds of rigour, such as rigour in the analysis of concepts and data, and rigour in inferences from models to the world.\(^3\)

As for the meanings of 'relevance', Blaug says even less, close to nothing. In order to assess his thesis, we need to do a little more. It is useful to start with a general notion of relevance that makes it explicit that relevance is a relational property. We might say, for example, that some X is relevant to some Y, which can be taken to mean that X has or might have some sufficiently significant consequences for Y. Here we may take X to designate a theory or model, or a stream or approach in economics. In principle, Y can be anything, provided it is perceived as somehow worthwhile as a goal or purpose. Note that this way of conceiving of relevance is normatively neutral in two ways. First, it permits the consequences of X for some Y to be either beneficial or harmful. Second, it permits the desirability of Y to be contestable. I think these are important features for the notion of relevance to be of use when assessing economics. At the same time, the limitations of the concept can be seen more readily.

Blaug's opening statement of the trade-off that was cited above includes a more specific version of relevance, specifying the Y: his definition talks about “socially and politically relevant conclusions”\(^3\)

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\(^3\) At one point he says that the rigour of game theory is “only epistemic rather than purely mathematical” (Blaug 2009a, 224), but it remains unclear what this means.
suggesting that for him Y consists of social and political purposes. In line with this interpretation, making it a little more specific still, a piece of economic inquiry is irrelevant if it involves a “failure to draw any policy conclusions” (Blaug 2009a, 221).

The same article puts forth other ideas of what relevance and irrelevance are, and it is obvious that these ideas do not boil down to one and the same thing. One of them suggests that purpose Y is “understanding” the real world: Blaug criticizes Sraffian economics for being “irrelevant to our understanding of the real world” (Blaug 2009a, 221). While this could be taken to be consistent with the generic idea of irrelevance as failure to have “socially and politically relevant conclusions”, this is not the same thing as the more specific “failure to draw any policy conclusions”.

We can find a further sense of relevance in the same article. In this sense the relevance of game theory is “undeniable since it is concerned with interaction between instrumentally rational individuals and that is almost a definition of the domain of social science” (Blaug 2009a, 224). This is rather striking as it implies, among other things, that any highly imaginary other-worldly (game theoretic or whatever) toy model satisfies the desideratum of relevance insofar as it contains ideas about “interaction between instrumentally rational individuals”—regardless of how fictional these may be.

Yet another notion of relevance is suggested by the claim that a piece of economics is “irrelevant to the major concerns of modern economists” (Blaug 2009a, 221). An appeal to such concerns as a mark of relevance is somewhat curious given Blaug’s complaints about the economics profession not being sufficiently concerned with socially and politically relevant issues. If the concerns of contemporary economists are misguided in such a way, then it should seem odd to criticize a stream of economics for being irrelevant to those concerns. Things become even more curious as we next read this:

Relevance is not a matter for individual opinion but a social judgment of the community of professional economists. We assess that judgment by inspecting the professional literature, by citation counts, or by any of the other methods of bibliometrics. [...] it is the nearest approximation we have to a supreme tribunal of social science (Blaug 2009a, 221).
Blaug then appeals to this tribunal against Sraffian economics. This actually suggests a version of relevance, namely relevance to getting one's work published and widely accepted, or recognized, measured in terms of citations. Now it seems obvious that much of standard economics that Blaug finds dubious is relevant just in this sense. This version of relevance may be used to speak against one target of criticism—here Sraffian economics—but it does so at the price of inconsistency by speaking in favour of other streams in economics that Blaug otherwise finds problematic.

I might add a couple of exploratory remarks on this framework of rigour and relevance in relation to falsificationism. First, there does not seem to be any necessary connection between a theory being falsifiable and that theory being policy relevant, such that being more falsifiable would go together with being more policy relevant. It is conceivable that a policy irrelevant theory is closer to being falsifiable than more ambitious policy relevant theories. Relevance and falsifiability seem independent from one another. Second, one may wonder if there is a connection after all, via rigour, but not what Blaug might have wanted. On the one hand, he thinks deductive reasoning is one of the marks of rigour supposedly in conflict with relevance, but on the other hand, his favourite falsificationism is a deeply deductivist doctrine. Third, saying that “if economics is to be practically relevant, there must be some concrete truths in which we can place confidence” (Blaug 1994, 119) is hard to reconcile with the falsificationist doubt about confidence in truths. Perhaps a resolution to these misgivings comes from the realization that no consistent falsificationism is being endorsed by Blaug: “It is high time that economists re-examined their long-standing antipathy to induction […]” (Blaug 2002, 50).

I will have a little more to say on the issues of relevance and rigour later, but let me make two further observations now. First focusing on a narrow version of the alleged general trade-off between mathematical rigour and practical relevance—or “policy relevance”—can be easily questioned (or falsified!) by pointing out counterexamples. Such examples include research serving practical goals such as building a skyscraper or sending a spacecraft to Mars. These practices require a great deal of mathematical rigour, so in such cases rigour is not at all in conflict with practical relevance, but is instead required for relevance. If one were to argue that there are major qualitative differences between these cases and economics, some detailed arguments would be needed.
Regardless of whether such arguments are forthcoming, it seems that economics itself provides representative examples that speak against the alleged trade-off. Being mathematically rigorous should in no way as such be an obstacle to drawing policy conclusions—just think of Robert Lucas and the new classical macroeconomics, or new Keynesian macro for that matter. It is a separate issue that those conclusions are empirically and ideologically contestable.

The second observation breaks away from the above narrow interpretation of rigour in the alleged trade-off. I am myself inclined to agree that mathematical rigour has become overemphasized in economics (a claim that would require a lot of qualifications to be sustained), but I would insist on immediately adding that *many other kinds of rigour are underemphasized*. It is not advisable to waste a good concept, that of ‘rigour’, by giving it an all too narrow meaning and confining it to mathematical economics. In broader senses of the term, I think *economics needs more rather than less rigour in order to be more relevant* to the most important real-world issues.

In conclusion, I do not presently see much reason for believing in a general trade-off between rigour and relevance. If there are no good such reasons, then the very idea should be abandoned.

**WIDENING HORIZONS: MODELLING AND INSTITUTIONAL CONSTRAINTS**

I will next suggest that we need to augment our horizons to be in a position to properly assess Blaug’s intuitions and arguments, and to see how his worries about economics could be dealt with in a more rigorous and less unrealistic manner. To get rid of an unrealistic methodology that is out of touch with real scientific practice, we need to look at what really happens in economics. And we need to expand our conceptual toolbox needed for understanding the complexities of actual scientific inquiry. I will offer a very brief and selective outline, bringing in just two themes: scientific modelling, and the institutions of inquiry.

**Implications of model-based inquiry**

Blaug’s methodological campaign might be understood as being built on a hypothetico-deductive image of economic inquiry. Good science is a matter of deriving predictions from hypothetical theories and law-like statements, then comparing the predictions with empirical data, and finally inferring to the acceptability of the theories and law statements. The Popperian version of this requires looking for negative falsifying
evidence and not accepting theories as anything more than as not-yet-falsified. At no point is inductive inference supposed to be exercised.

This image of science has no prominent role for models. But models do play a prominent role in economic inquiry. Neglecting the issues of modelling and the challenges they pose for methodological scrutiny is one source of unrealisticness in Blaug’s methodological statements. Accommodating models and understanding how they function will have important implications for how we should judge economics. Models can serve a variety of different purposes, and for each type of purpose, they are to be assessed differently. So for example, straightforward predictive testing is not the way to go except for models that are built with the goal of prediction in mind (while keeping in mind that ‘prediction’ itself can mean a number of different kinds of thing).

For some models it does not matter much at all that their “predictions” have little match with the data—just think of my favourite, Johann Heinrich von Thünen’s (1966 [1826]) highly idealized model of agricultural land use and its ‘prediction’ of concentric rings that regularly fail to match with actual land use patterns. These models have been built for other purposes, such as that of a “minimal model” depicting—and it is hoped truthfully depicting—a causal mechanism that contributes to the phenomena to be explained, yet does not influence the phenomena alone but together with other mechanisms and conditions (von Thünen’s simplest model isolates a mechanism that contains transportation costs and land values as functions of distance; see Mäki 2011). Or they may be used for exploring possible causal scenarios, in search for plausible how-possibly explanations—such as in the case of Thomas Schelling’s (1969) segregation models. These models are imagined systems that isolate a limited set of causal factors and that are described in terms of apparently false idealizing assumptions. It would be inappropriate to try to test either kinds of model simply by their assumptions or predictive implications so as to result in a possible falsification.

Models are artificially constructed objects that are examined in place of real systems in the wild. The tricky issue—the ultimate methodological issue in economics—is about how these two systems are related to one another. It is useful to draw a distinction between the study of the properties of models on the one hand, and the study of how these models are related to the real world and perhaps policy concerns, on the other. It is tempting to suggest that the examination
of models is a rigorous activity, while relevance is a separate issue that would be checked by examining how models relate to real-world concerns. But this would be too simplistic (given what we have said about the notions of rigour and relevance above). The study of models can be directly relevant to a variety of purposes (such as isolating real effective causal mechanisms and conceiving real possibilities and impossibilities), while one may also want to study how models relate to real world concerns to be more rigorously conducted than it often tends to be.

Consider a representative example. Many theoretical models in economics provide an account of how things might possibly go in the world, without yet implying or involving any claims about how they actually go. Economists examine these models without systematically linking them with empirical data. One might suspect that such how-possibly models have no relevance to policy concerns, but this would be too hasty. Even how-possibly models can be policy relevant. Again Schelling’s (1969) segregation models are a case in point (see Sugden 2002; Aydınonat 2008; Grüne-Yanoff 2009; Mäki 2009). They can be examined on a blackboard, on a checkerboard, in a computer simulation, without empirical research on actual cases of segregation. The racial versions of these models undermine the commonsense belief that racial segregation in urban housing markets necessarily results from discriminatory racial attitudes among individuals by pointing out that it is possible for segregation to arise without such attitudes. This discovery has consequences for the array of policy strategies that should be considered, so the models are clearly policy relevant. Naturally, choosing the right policy instruments in each particular case requires empirical enquiry about the detailed actualities of those cases, but this is not the only realm of policy relevance. Nor should the focused examination of models be the only realm of rigour.

So how should one articulate Blaug’s valuable intuitions while acknowledging the importance of modelling in economics? The distinction between surrogate modelling and substitute modelling might be helpful (Mäki 2009). Surrogate models are objects that are directly examined in order to indirectly learn about their targets in the real world, while substitute models are examined for their own sake, with no such further goal in place. Substitute models are the easy playgrounds that substitute for possible hard-to-access real-world targets, while surrogate models serve as tools for the hard task of
preparing epistemic access to such targets. In both cases the study of models without immediate and full attention to how they link to real targets is an entirely legitimate activity, but only in the case of surrogate modelling would the research community in due time take further steps in establishing those links. Naturally, given such a characterization, the distinction between these two sorts of modelling cannot be sharp, and contestable boundary cases cannot be avoided—but this as such does not distort the messy complexities of scientific inquiry (while all too neat meta-theoretical concepts might).

It remains to be seen whether these concepts, together with some heuristics, perform better than does Blaug's failed dichotomy between rigour and relevance. Performing better here amounts to being descriptively more adequate and normatively more feasible. Surrogate modelling would be a matter of exercising rigour both in examining the properties of models and in examining how they connect with some real world targets, and the latter connection would help guarantee relevance to real world concerns (that would not in all cases involve direct policy relevance). Substitute modelling would at most involve rigour in the study of models, and would lack real world relevance.

**Preaching and institutional design**

Another extension we need in our horizons deals with the institutions of economic inquiry. Otherwise we will not fully understand the actual practices and conventions of economic inquiry, nor will our normative assessments and prescriptions have much force, and thus our methodology will remain unrealistic, both descriptively and normatively. Nothing in science happens in an institutional vacuum, whether testing or modelling, or judgements of rigour and relevance, and institutions often make a difference. Furthermore, the institutions of inquiry are neither historically constant nor completely uniform across scientific disciplines. Without due attention to these institutions, any preaching will fall on deaf ears.

It is also possible that without a systematic understanding of the institutional dynamics of scientific research one may unintentionally promote trends that one would otherwise find unfortunate. Consider Blaug’s campaign for more practically relevant economics in relation to the new actually emerging regime of societal accountability in science
MÄKI / BLAUG’S UNREALISTIC CRUSADE

(“Mode 2” and all that). Might his campaign be used for supporting the current trend toward reducing the autonomy of academic science and bringing it into closer contact with the practical demands deriving from politics and business?

In its falsificationist mode, Blaug’s normative methodology comes in three waves. The first-order falsificationist rule asks economists to make their theories falsifiable, and to try to falsify them with negative evidence. Second, in response to failures to do so, Blaug puts forth the second-order fortifier addressed to economists: “try harder!” The third wave is the instruction addressed to methodologists, asking them “to preach falsificationism day in and day out”.

Now if falsificationism is impracticable, waves two and three cannot have the consequences the preacher intends. Preaching makes sense and may become effective when the endorsed rule or principle is practicable, but, especially if it is difficult to practice, mere preaching may be inadequate. Institutional facilitators are needed too. Indeed, much depends on what economists would call the incentive structure embedded in the industrial organization of inquiry.

As a perceptive observer, Blaug naturally had an eye for these facts. He refers to “a well-established professional culture that values technical facility above all else” (Blaug 2002, 44) and “a veritable professional treadmill with a built-in momentum that feeds continually on the pressure to publish in prestigious journals in order to gain employment in prestigious institutions” (2002, 45). Yet in response to his own question, “What to do?” (2002, 44), he focuses on individual economists’ behaviour in potentially breaking away from “the dominant fashion for economics papers” (2002, 45). It would be too much to ask younger scholars to risk their careers, so the hope must be laid on “the older members of the profession to show the way with empirically relevant research grounded in the attempt to confront outstanding policy issues” (2005, 45). If the desired change indeed depends on the initiative of individual economists, then the proper task for methodologists is to preach, and to preach for a change in individual behaviour.

Some gentle preaching addressing individuals is often just fine, but without ideas for a re-design of the institutions of economics, preaching may become just a matter of ineffectively scratching the surface without

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On “Mode 2” of knowledge production in the sociology of science, see, for example, Nowotny, et al. 2001.
lasting effect. A more realistic campaign should go deeper and develop a plan for a structural change in the relevant institutions. It would be more realistic both in the descriptive sense of providing an adequate account of the cognitive and institutional preconditions of economic inquiry and in the normative sense of providing possibly effective recipes for reform. This requires a rich framework of concepts beyond those of testing and prediction, rigour and relevance, and the like. Economics itself, as a subject dealing with optimal social organization, may contribute to this framework.

CONCLUDING REMARK
As I mentioned in the introduction, my reason for not citing Mark’s work frequently was that I did not see an elaborate and progressive methodological theory that I could accept or consider as having a promising future. In the 1980s, I was among the few (together with others like Dan Hausman, Bert Hamminga, and Alex Rosenberg) who did not adhere to the Popperian and/or Lakatosian framework and agenda in relation to the methodology and philosophy of economics (see, e.g., Rosenberg 1986; Hausman 1988; Mäki 1990, 2008; and on the rise and fall of this framework, see Backhouse 2012).5

I thought the most important issues regarding the connections of economics with the real world were much more complicated than can be resolved or even envisaged from within the Popperian and Lakatosian frameworks. In the course of the 1980s, I had developed elements for a different account that I believed would be more adequate for addressing these issues. These included notions such as ‘isolation’ and ‘idealization’, and the account kept evolving so as to become an account of modelling. In this project, I learned a great deal by examining 19th-century economists’ writings. These included J. H. von Thünen’s Isolated State (1966 [1826]), whose highly “unrealistic” land-use model I interpreted as an intendedly true account of a simple mechanism that is isolated by employing false idealizing assumptions.

Mark happened to be an expert on von Thünen. It must have been already in the early years of the 2000s when he once attended a seminar at which I summarized my reading of von Thünen. Mark’s reaction was

5 Note that there is a difference between sharing a more or less Popperian/Lakatosian framework and agenda on the one hand, and accepting Popperian/Lakatosian principles and prescriptions of good science, on the other. In the course of the 1980s and 1990s, methodologists and historians of economics gave up the latter more easily than the former; see Mäki 1990.
somewhat dramatic, the verbal side of it put more or less in these words: “Oh what a fool I am! How come that never occurred to me!” I thought that was a truly Popperian reaction (Popperian-the-doctrine rather than Popperian-the-man), even though it was understandable that within the Popperian framework, the chances of reaching the insight were not so good simply because the required conceptual resources—those of idealization and isolation—were not given the prominent role they deserve.

On some other topics too, Mark was quite responsive to challenging evidence and arguments, and was prepared to revise his views accordingly. These included his understanding of Milton Friedman's 1953 essay on which he moved away from Popperian and straightforward instrumentalist interpretations (Blaug 2009b). In these situations he acted like a good meta-methodological or second-order Popperian (in some generalized broad sense). Had he lived longer, he would probably have revised his first-order Popperianism (in a more narrow sense) and the argument from rigour and relevance more than he actually did. And we would have had a chance to learn from him even more than we actually did.

REFERENCES


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Competition as an evolutionary process:
Mark Blaug and evolutionary economics

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Abstract: Mark Blaug and I agree that if there is a realist interpretation of economic behavior to be discerned in Friedman (1953), it is to be found not in Friedman’s belief that the profit motive overrides other possible motives, but in his belief that a selection mechanism is working in competitive markets. Our joint sympathy for evolutionary economics is largely based on a conviction that the conception of competition as a dynamic evolutionary process is rather plausible. We disagree, however, on two issues: first, how important the evolutionary conception was for Friedman’s overall argument; and, second, whether we can learn something about the real world from rigorous formal analytical models. In this article, I explain and argue for my position on these two issues, and use Nelson and Winter’s (1982) theory of evolutionary economics to support an illustrate my argument.

Keywords: evolutionary economics, institutions, competition, theory of the firm, realism, formalism, abstraction, theoretical modeling

JEL Classification: A12, B25, B41, D21, D80

One of the things Mark Blaug and I shared was a sympathy for evolutionary economics. In his ‘Disturbing currents in modern economics’ (1998), Blaug explicitly claims that the (at the time) recent work in evolutionary economics was one of the most hopeful and fruitful developments in economics. In the same vein, after my Economic evolution (Vromen 1995)—in which I gave Richard Nelson and Sidney Winter’s (1982) evolutionary economics a prominent place—I have continued to work on contested topics and issues related to Nelson-and Winter type of evolutionary economics. Although some of my work is critical, it is based on the presupposition that these topics were

1 This is an extended version of his better-known essay ‘Ugly currents in modern economics’ (Blaug 1997a).

AUTHOR’S NOTE: My thanks go to Marcel Boumans, Matthias Klaes, Luis Mireles-Flores, and three anonymous referees, for helpful suggestions. The usual caveat applies.
interesting enough to be subjected to critical scrutiny. Thus Mark Blaug and I were in broad agreement on the idea that evolutionary economics (narrowly understood) is a promising alternative to orthodox economics.

Our views diverged, however, on our assessments of specific attempts to connect evolution and economics in a meaningful way. For instance, Blaug failed to see any merit in evolutionary game-theoretic analyses, whereas I believed that such analyses could be fairly illuminating. In this article I focus on this difference in our perspectives. Blaug's dislike of game theory tout court (and not just of evolutionary game theory) was clearly related to his scathing critique of the dominance of “ugly” formalism in mainstream economics. Consequently while he referred favorably to the general evolutionary approach to economics, he disliked the highly abstract theoretical forms of economic modeling. At the end of the article, I have a few positive things to say about formal modeling in economics and in relation to the “formal theorizing” part in Nelson and Winter's (1982) approach to evolutionary economics. I claim that to understand why Nelson and Winter thought that formal models were needed in their account is instructive to clarify the differences between Mark's views and my view about the merits of formal modeling.

**FRIEDMAN (1953) ONCE AGAIN**

Before moving to discussing Mark Blaug's endorsement of evolutionary economics, I first want to refer to the pièce de resistance par excellence for economic methodology in the twentieth century: Milton Friedman’s ‘The methodology of positive economics’ (1953). Like so many others, Blaug struggled to come to grips with this paper that resonated with many economists, but that was highly contested by most economic methodologists. What was appealing to economists seemed to be exactly what put off methodologists and philosophers, namely the idea that economists did not need to care about the realism—or ‘realisticness’, as Uskali Mäki (2009) would put it—of their assumptions.

Here, I focus on the parts of the essay in which Friedman exposes what Mark Blaug called “the Alchian thesis”—and what I have called “Friedman's selection argument” (Vromen 1995). Paradoxically, this thesis—which Friedman clearly used to defend “orthodox” economic theory against what he believed were mistaken critiques—was one of the main sources of inspiration for Nelson and Winter's (1982) “unorthodox” evolutionary economics. More precisely, I concentrate on
how important this thesis was in Friedman’s defense of “orthodox theory” and on how this thesis relates to a possible realist reading of Friedman (1953; henceforth F53). These are two issues that Blaug and I seem to have disagreed on. Unfortunately, Blaug and I have not had the chance to continue our discussion about such matters. I take writing this paper as an opportunity to go one last round in our always cheerful and inspiring conversation. Like most of us, Mark loved to have the last word. Unfortunately for him, in this case, that cannot be the case.

On the importance of the Alchian thesis for a realist grounding of F53

The relevant passage that I want to highlight comes from an essay by Blaug (2009) devoted to F53.2

One of the most memorable things in F53 is the notion that competition is an evolutionary selection mechanism weeding out businessmen who fail to maximize returns. Jack Vromen (this volume) dismisses the importance of this argument in F53 because Friedman expounds rather vaguely what I once called the Alchian thesis and follows it almost immediately by the “countless applications” paragraph cited earlier. However, without something like a Darwinian selection mechanism, Friedman’s frequent appeal to as-if reasoning lacks any grounding in a commonsense realist interpretation of economic behavior. This is a crucial point in the essay at which it does matter whether we read it as an exercise in the philosophy of realism or in the philosophy of instrumentalism because, as Uskali Mäki rightly observes in chapter 3 of this volume, we may argue that businessmen act as if they only maximize profits (but of course they do many other things) or that they act as if they maximize profits (but that they really don’t). I side with Mäki and against Boland in this (Blaug 2009, 351).

There are several issues at stake here that I want to comment on. Let me start with immediately getting one thing out of the way. I never dismissed the importance of the selection argument as such. I always felt it is one of the more intriguing ideas in F53 that market competition involves an evolutionary selection mechanism weeding out all firm behavior that fail to make profits. Indeed, the notion that competitive markets select for positive profits is also one of the leading ideas in

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2 Unfortunately, there is not much textual evidence to draw on, neither in Mark Blaug’s writings nor in (the relevant passages in) F53. I realize this leaves room for different interpretations from the ones I defend here. Nevertheless I hold that my interpretations are better supported by the available textual evidence than the ones I argue against.
Nelson and Winter's “unorthodox” evolutionary economics, and one of the reasons, I submit, that Blaug and I have both sympathized with it.

Blaug is right, though, that in my essay (Vromen 2009) I did call into question that Friedman's selection argument is very important for Friedman's overall argument in F53. Textual evidence clearly suggests that Friedman thought that another sort of evidence is more important for boosting his confidence in the “businessmen maximize returns” assumption. This evidence is to be found in the “countless applications” paragraph Blaug is referring to. In this paragraph Friedman argues that

An even more important body of evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted (Friedman 1953, 22-23).

Blaug (1980) finds it quite disappointing that Friedman does not come up with even one single example of this evidence. Blaug might have a point here. We may find Friedman's “countless applications” argument deficient in this respect. But, whether we like it or not, Friedman does explicitly state that he thinks the “countless applications” argument is more important than his selection argument. Thus, although I do not totally dismiss the importance of the selection argument in F53 in Friedman's own view, I do argue that it is less important than Friedman's “countless applications” argument.

Let me next concentrate on two claims that Blaug makes:

1. In F53, the only grounding of as-if reasoning in a commonsense realist interpretation of economic behavior is provided by something like a Darwinian selection mechanism.

2. Uskali Mäki is right in arguing that we can read F53 as an exercise in the philosophy of realism. In particular, Mäki is right that Friedman's reasoning that businessmen behave as if they maximized profits should be read as saying that the profit motive, even though it is not the only motive behind firm behavior, is a real and forceful motive—and not, as Larry Boland (1979) purportedly suggests, that the profit motive is not a real motive underlying firm behavior.

I am basically in agreement with the first claim. But I have my doubts about the second. On the face of it, the second claim appears to be at odds with the first one. How can the profit motive provide a realist
grounding of Friedman’s as-if reasoning if it is something like a Darwinian selection mechanism that provides the only realist grounding of Friedman’s as-if reasoning? The profit motive and something like a Darwinian selection mechanism seem to be two rather different things. Indeed, I believe that the two are profoundly different. I also believe that if there are elements of realism to be found in F53, they relate to a selection mechanism and not to the profit motive.

This is what Friedman has to say about as-if reasoning in connection with the assumption of profit maximization:

[...] under a wide range of circumstances individual firms behave as if they were seeking rationally to maximize their expected returns (generally if misleadingly called “profits”) and had full knowledge of the data needed to succeed in this attempt; as if, that is, they calculated marginal cost and marginal revenue from all actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal. Now, of course, businessmen do not actually and literally solve the system of simultaneous equations in terms of which the mathematical economist finds it convenient to express his hypothesis (F53, 21-22).

Note that there is a good deal more here at the right hand side of the “as if” clause than just the profit motive. The profit motive is simply the motive to make as much profit as possible. Let us agree that the profit motive only covers the “[...] seeking [...] to maximize their expected returns” part in the above quote. What Friedman adds to this is the assumption that firms do so rationally and with full knowledge of the data. He adds this assumption to make sure firms succeed in their attempt to make maximum profits. What this highlights is that a belief that firms are led only by the profit motive would by itself not be sufficient for ensuring that firms succeed in making maximum profits.

Phrases like “profit-maximizing firms” are ambiguous. They can refer to one of the following two sets:

(1) The set of firms whose only (or overriding) motive (or aim, or goal) is to maximize profits.

(2) The set of firms making maximum profits.

Statement (1) only says something about the motives (aims, goals, or intentions) of firms. It states that firms are led by the profit motive. It remains silent on the extent to which these firms are successful.
Firms trying to maximize profits may fail. Statement (2) only says something about the magnitude (or size) of the profits firms actually make. It remains silent on the motives firms might have. It is possible that firms with other (or more) motives than the profit motive make maximum profits. Thus we can say that statements (1) and (2) are not equivalent. It is even possible that the two sets do not overlap at all. Only if we add the assumptions that firms in the first set are perfectly rational and avail of all the knowledge needed to be successful do the two sets coincide. This is what Friedman seems to intimate in the passage just quoted.

Note that Friedman does not say anything about the reality and relative strength of the profit motive here. This is true not only for the passage just quoted, but of F53 in its entirety. It is simply not clear whether Friedman believes that, compared to other motives the profit motive is the most forceful one driving firm behavior. To be sure, Friedman is not denying anywhere that the profit motive determines firm behavior. But the textual evidence to support this hypothesis is lacking. There is textual evidence, though, suggesting that Friedman believes that this issue is not important for the point he is trying to make. For Friedman appears to claim that the motives of businessmen do not matter. Such textual evidence can be found in the passages immediately following the one just quoted. Friedman argues that although it is obvious businessmen do not actually execute the sort of marginalist calculations ascribed to them in the maximization-of-returns hypothesis, their behavior might nevertheless be accurately described (or predicted) by the hypothesis.

The relevant context is provided by the so-called marginalism controversy (Mongin 1992, Backhouse 2009). Antimarginalists sent out questionnaires, found that no businessman based his decisions on the magnitudes of marginal costs and marginal revenues, and concluded from this that the hypothesis had to be rejected. Friedman flatly denies that the antimarginalist findings about how businessmen make their decisions provide a relevant test for the hypothesis. The only relevant test for the hypothesis, Friedman argues, is whether the hypothesis correctly predicts the decisions actually made by businessmen.

Let the apparent immediate determinant of firm behavior be anything at all—habitual reaction, random choice, or whatnot. Whenever this determinant happens to lead to behavior consistent with rational and informed maximization of returns, the business
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will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the additional resources from outside. The process of “natural selection” thus helps to validate the hypothesis—or, rather, given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for survival (F53, 22).

What is important—Friedman seems to argue here—is not the motives or determinants of firm behavior, but whether firms succeed in making maximum profits. The latter is particularly important because firms that fail to make maximum profits are not likely to stay in business for long. Furthermore, it cannot be maintained that only firms led by the profit motive succeed in making maximum profits. As Alchian (1950) emphasizes, if there is pervasive uncertainty, then making maximum profits can be a matter of luck rather than the outcome of pursuing maximum profits. In the passage about as-if reasoning quoted above, Friedman suggests that in the absence of perfect rationality, full information, and full foresight there is no guarantee that firms attempting to maximize profits succeed in their attempt. In sum, Friedman seems to maintain that the profit motive being the overriding motive behind firm behavior is neither a sufficient nor a necessary condition for the predictions of the maximization-of-returns hypothesis to hold true.

I conclude that there is no textual evidence in F53 that supports Mäki’s realist rereading of Friedman’s as-if reasoning with respect to the maximization-of-expected-returns hypothesis (i.e., Blaug’s second claim presented above). Friedman’s acceptance of the hypothesis is not based on his belief that the profit motive is the overriding determinant of firm behavior. Mäki might be right that Friedman believes that the profit motive is the overriding determinant of firm behavior, although the textual evidence for this is inconclusive. But this issue is irrelevant to the argument Friedman is making. Friedman argues that even if the profit motive were not the overriding determinant of firm behavior, we might still be confident that the predictions of the hypothesis holds true. In this argument, Friedman’s belief that there is selection for

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3 Although this is not the place to dwell on it, it has to be noted that there are also significant differences between Alchian’s (1950) and Friedman’s (1953) selection arguments. Whereas Friedman makes stark claims about firm level behavior, Alchian more cautiously makes claims about tendencies at the level of industry behavior. See, for example, Kay 1995; Vromen 1995.
maximum profits—rather than his (alleged) belief that the profit motive is the overriding determinant of firm behavior—takes center stage.

**Selection as a realist grounding of Friedman’s as-if reasoning**

This brings me back to Blaug’s first claim. I agree with Blaug that a belief in something like a Darwinian selection mechanism provides the only “realist grounding” of Friedman’s as-if reasoning in F53. If there is a belief in the existence of a crucial mechanism to be found in F53, backing up Friedman’s confidence that the maximization-of-returns hypothesis predicts firm behavior fairly well, it is not the belief that the profit motive overrides all other motives. It is the belief that there is a selection mechanism working, weeding out all firm behaviors that do not realize maximum profits. Arguably, not one but two beliefs are involved here. The first is that there is something like natural selection working in competitive markets. The second is that this mechanism gradually eliminates less profitable firm behavior. As Friedman puts it: acceptance of the maximization-of-returns hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for firm survival. If there is a necessary condition to be found in F53 for the hypothesis to be acceptable, it is the conjunction of these two beliefs, rather than the belief that the profit motive dominates other motives in determining firm behavior.

The two candidate forces here, the profit motive and selection, may not be mutually exclusive. Both may be active in shaping firm behavior. For a while, it was implicitly assumed in the philosophy of social science—inspired mainly by the work of Elster (1979; and 1983)—that behavior is produced either by forward-looking mechanisms or by backward-looking mechanisms. Rational choice theory was supposed to refer to a forward-looking mechanism: someone’s expectations in conjunction with his preferences (and constraints) are assumed to determine one’s behavior. By contrast, evolutionary theory was supposed to refer to a backward-looking mechanism: some evolutionary forces (such as notably selection) working on actual, realized...
consequences provides the negative feedback loop linking the consequences to behavior displayed in the next period (or generation). It was implicitly assumed that the two mechanisms rule each other out: behavior is produced either by a forward-looking mechanism or by a backward-looking mechanism. The analogue of this with regard to Friedman's maximization-of-returns hypothesis would be that firm behavior is produced either by the profit motive or by selection. But on closer inspection, this alleged opposition (or exclusion) of mechanisms might be a spurious one.

The biologist David Sloan Wilson (2012) rightly observes that selection is an ultimate cause and the profit motive is a proximate cause. Explanations in terms of ultimate causes and explanations in terms of proximate causes need not conflict with one another. Rather, they can complement each other in a more complete understanding of behavior. Wilson is referring to the important distinction in biology between ultimate and proximate causes (Mayr 1961; Tinbergen 1963). Mayr (1961) points out that a seemingly unequivocal question such as “Why do warblers in New-Hampshire migrate in Autumn?” allows for at least two correct and compatible answers. One answer is that ancestors of these warblers that did not migrate were selected against in the past. This answer crucially refers to natural selection, the paradigm case of an ultimate cause in evolutionary biology. Yet another equally correct answer is that it is the drop in temperature or the decrease in daylight that causes the warblers migrate south. Such changes in the external conditions of present-day warblers—together with the physiological processes that they induce in warblers—make up the proximate causes for the warblers' migrating behavior. Unlike ultimate causes, such as natural selection, that impinged on ancestors of the present population of warblers a long time ago, proximate causes impinge on (and are part of the functioning of) members of the present population of warblers.

In Mayr's case of the migrating warblers, it is clear that the ultimate and proximate explanations offered do not rule each other out. Something similar also seems to be the case with firm behavior. Two different—yet compatible—answers can be given to the question of what makes firms behave the way they do. One is that it is selection for profits, as an ultimate cause, that eliminated all behavior but the present behavior; another answer is that it is the profit motive, as a proximate cause, that leads firms to behave the way they do. Both forces might be involved in the production of firm behavior.
The twist that Friedman gives to the distinction is that ultimate and proximate causes might work concurrently, rather than one after the other. In the case of the warblers, it is assumed that natural selection worked long time ago, in the ancient past. Natural selection trimmed the set of ancestors of present-day warblers, which are assumed to have inherited the behavior-producing proximate causes from their reproductively successful ancestors. Friedman by contrast takes selection to be working here and now, on the current set of firms. Or, rather, he assumes that a process of selection has just run its course, resulting in some stationary end-state whereby all firms that remain maximize profits. This does create some sort of competition between the two forces, not in the sense that only one of them could possibly be involved in the production of firm behavior, but in the sense that only one of them ensures that firms make maximum profits. As I hope to have made clear, for Friedman it is selection that is ultimately causally responsible for the latter. Firms that are solely led by the profit motive might fail to make maximum profits because they lack the rationality, information and foresight required to ensure that their attempt to make maximum profits succeed. Conversely, another implication of Friedman’s twist is that if we assume from the start that all firms are led solely by the profit motive and that they all avail of the full rationality, information and foresight needed to succeed, selection is pre-empted. In such an idealized world, selection would not produce any change, since it would have no causal work to do.

But does it really entail realism?
What the foregoing discussion suggests is that although the “countless applications” argument is the most important argument for Friedman himself, it does not provide the only reason for Friedman to accept the maximization-of-returns hypothesis. If it would, it could be concluded that Friedman is a full-blooded anti-realist. If Friedman’s confidence in the hypothesis were based solely on “the repeated failure of its

Friedman is (in)famous for having given a peculiar twist to Popper’s methodology of falsificationism, by arguing in F53 that the more significant a theory, the more unrealistic its assumptions (see F53, 14). This is the so-called F-twist that many methodologists have commented upon (e.g., Musgrave 1981). The twist I identify is a different one.

In evolutionary epistemology, pioneered by Campbell (1960; 1974) and Popper (1972), it is similarly argued that what is ruled out by an evolutionary approach is not intentional, purposeful action as such, but only intentional, purposeful action with providence (see Cziko 1995).
implications to be contradicted” (F53, 22), then we could say his acceptance of the hypothesis would be based solely on its empirical adequacy. Following van Fraassen (1980), however, the fact that Friedman does mention a belief he entertains about an unobservable mechanism (and its consequences) as support for the hypothesis, is enough to conclude Friedman is not a full-blooded anti-realist. At the risk of repeating myself, the belief is not one in the dominance of the profit motive—as Mäki argues—but a belief in the functioning (and in some consequences) of a selection mechanism.

A merit of my realist rendering of Friedman’s defense of the hypothesis is that it allows us to treat the three examples that Friedman discusses on a par. In preparing his selection argument, Friedman successively discusses two examples: “the density of leaves around the tree” example and “the expert billiard player” example. The second example—which was already discussed in a previous essay by Friedman and Savage (1948)—is meant to show that the fact that expert billiard players do not make lightning calculations does not provide a good reason to reject the hypothesis that expert billiard players make lightning calculations (and have the knowledge necessary to make almost perfect shots). The first example is meant to bring out that the hypothesis “that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives” (F53, 19) should not be rejected on the ground that leaves do not deliberately seek anything at all. Friedman makes abundantly clear that he sees all three examples as analogous or parallel. In all of these examples he believed that there is some selection mechanism working: only those (positions of) leaves/billiard-players/firms survive that behave as if they were deliberately, consciously, and flawlessly maximizing some goal-function. The claim that leaves, billiard players, and businessmen do not actually maximize in this sense does not provide sufficient reason to reject the hypotheses that they do maximize some goal-function.

On Mäki’s realist rereading we are forced to treat the maximization-of-returns hypothesis and the leaves.seek.sunlight hypothesis as dissimilar. As we saw, Mäki argues that the maximization-of-returns hypothesis allows for a realist reading, because Friedman believes in the actual predominance of the thing he puts on the right hand side of the as-if clause: the profit motive. By contrast, the leaves.seek.sunlight hypothesis does not allow for a realist reading, Mäki argues, because no one believes that leaves deliberately seek anything at all. Since the
forces cited after the “as if” are not real in the leaves-seek-sunlight hypothesis, this hypothesis could only be understood in a non-realist, fictionalist way. Hence, Mäki is forced to put aside “the density of leaves around the tree” example (see Mäki 2009, 105, 107).

On Mäki’s realist rereading Friedman’s text and argument entails yet another incoherence; one that disappears with my reading. Mäki seems to assume that if there is no belief that the forces or mechanisms cited after the “as if” are real, realism is out of the window. Only if there is a belief that the forces are real, a realist reading would be possible. I think such a belief is neither a necessary nor a sufficient condition for realism. A belief that the forces cited after the “as if” are real is not necessary, because acceptance of a hypothesis can be based on beliefs in mechanisms that are not cited after the “as if” phrase. I argued that it is a belief in a selection mechanism that backs up the hypotheses in all three examples discussed by Friedman. A belief that the forces cited after the “as if” are real is not sufficient either. Friedman probably believed that the profit motive is the strongest motive, but this belief did not ground his acceptance of the maximization-of-returns hypothesis.

**Nelson and Winter's (1982) theory: a more substantive form of realism?**

One might object that the sort of realism I am invoking here is very meager or weak. It amounts to no more than that there is a belief in the efficacy of some specified “underlying” unobservable mechanism on which acceptance of some hypothesis is at least partly based. Evidence is not required to support this belief. Moreover, it is not required that the mechanism is explicitly referred to and accounted for in the hypothesis itself. The belief in the mechanism might only pop up, for example, in an informal defense of the hypothesis.

A more substantive and stronger sort of realism would insist that if there is such a belief in some mechanism, and the mechanism is not cited after the “as if” in the hypothesis that is defended, then the mechanism must be explicitly referred to and accounted for in a new, altogether different hypothesis. Following Koopmans (1957) and others, this is what Nelson and Winter (1982) and other evolutionary economists demand. Nelson and Winter share Friedman's (and Alchian's) belief that there is a selection mechanism working in competitive markets, weeding out firms that fail to make profits.

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8 Note that this is also not required in Mäki’s realist rereading.
Firms that happen to make positive profits can grow and expand, they argue, while firms that suffer losses are forced to contract. They disagree with Friedman about what place and status this belief should be given in a satisfactory economic theory, however. Nelson and Winter argue that what serves as a background belief in Friedman's defense of the maximization-of-returns hypothesis should serve as a starting point (and a cornerstone) for an altogether different evolutionary economic theory.

One of the things Nelson and Winter investigate is whether Friedman's selection argument is valid. Is Friedman right to hold that only firms that make maximum profits survive the selection mechanism? Taking Friedman's first belief that there is a selection mechanism working in competitive markets as a starting-point, they probe the tenability of Friedman's second claim that only profit-maximizing firms survive the mechanism. Nelson and Winter argue that a more formal and rigorous analysis is needed than the informal “appreciative” theorizing of Alchian and Friedman to explore the additional assumptions that have to be made to validate Friedman's second belief (1982, 141). Nelson and Winter come to the conclusion that Friedman's second claim is by and large untenable. Firms that happen to make maximum profits may be the only survivors. But this is by no means guaranteed. Indeed, starting with Winter (1964), Nelson and Winter present a plethora of reasons why non-profit-maximizing firms might survive. One of them is that the set that economic “natural selection” trims need not include firms making maximum profits. If firms making maximum profits are not part of the anterior set, selection will not (indeed, cannot) lead to a population (a posterior set) with only profit-maximizing firms.

Nelson and Winter argue that selection is not the only real and important mechanism working in competitive markets. Another crucial mechanism is search: firms are not believed to passively undergo the force of selection, they actively search for better routines if they fail to make satisfactory profits. This search takes the form of trial-and-error learning. As long as operating routines yield satisfactory profits, firms will tend to stick to them. But as soon as operating routines cease to do so, firms will start looking for better ones. This bounded-rationality type of learning, which Nelson and Winter explicitly associate with Herbert Simon's (1955) work on satisficing, clearly differs from the fully rational type of Bayesian updating learning that prevails in orthodox economics.
Nelson and Winter's idea that search is guided by second-order routines also bears close resemblance to Simon's emphasis on the role of heuristics in search and discovery-processes. Nelson and Winter advance their ideas about “failure-induced search” as an explanation of when and why innovations tend to occur. Following Schumpeter, evolutionary economists like Nelson and Winter speak of endogenous technical (or technological) change.

I conclude that Nelson and Winter’s evolutionary theory, which is partly based on the appreciative theorizing implicit in Friedman's selection argument, allows for a more substantive realist reading than the maximization-of-returns hypothesis Friedman is defending with his argument. The reason is simply that the belief in the efficacy of selection does not function as a background belief to support acceptance of a non-evolutionary theory—as in the maximization-of-returns hypothesis—but is put center stage in an overtly and explicit evolutionary theory. Nelson and Winter’s evolutionary theory studies the working and effects of selection, the force which is believed by all to be real and important in shaping aggregate behavior.

WHY BLAUG WELCOMES EVOLUTIONARY ECONOMICS

So much for realism and F53. Let me now turn to Blaug’s endorsement of evolutionary economics. It is clear that Blaug considered evolutionary economics to be one of the most promising new developments in economics. It is not so clear, however, what exactly made evolutionary economics so attractive to him. There simply does not seem to be much textual evidence to go on. All I have been able to find in Blaug’s writings are passages like the following two:

I would welcome more of the ‘New Institutional Economics’, ‘evolutionary economics’, neo-Austrian economics, or call it what you will, with its emphasis on bounded rationality, norms of behavior, and evolving processes (see Langlois 1986; Witt 1993; and Hodgson 1993). I am not alone in sensing that the days of end-state theorizing are over. Books like Nelson and Winter (1982), with its radical use of computer simulation models of firm behavior, or Penrose (1980), a recently reissued classic study of the growth of firms over time, as leading examples of a renewed interest in the dynamics of the “invisible hand” (see also Klein 1977; Brenner 1987; Best 1990). The end product of these developments will be a different brand of economics from what we are used to. So long as we continue to demand the standards of rigour that we have come to
accept from the highly stylized, locally tight, choice-theoretic models of mainstream microeconomics, we will never explore an alternative to end-state competitive theory. Empirical science frequently proceeds on the untidy basis of what is plausible rather than what can be formally demonstrated beyond any shadow of doubt (Blaug 1997b, 257).

Among the most hopeful, and I believe most fruitful, developments in economics is the recent growth of evolutionary economics in books like An evolutionary theory of economic change (1982) by Richard Nelson and Sidney Winter, Inside the black box (1994) by Nathan Rosenberg, and a series of papers by Richard Lipsey, leading to a forthcoming major work on technical change and growth. The style of all these works is less rigorous, less enamoured of precise results, and less inclined to thought experiments employing logical deduction than we are accustomed to from reading mainstream economics. But they more than make up for that by their continuous reference to real-world questions in close touch with empirical evidence (Blaug 1998, 31).

In passages like these, I submit, three sorts of considerations can be discerned that seem to have made evolutionary economics attractive for Blaug. One set of considerations concentrates on the sorts of questions, or issues, which are addressed in evolutionary economics. According to Blaug, evolutionary economics addresses real-world questions and competition as a dynamic process. They are taken to be more practically relevant than the arcane and sterile issues studied by orthodox economics. The second set of considerations regards the contents of the theories and models advanced in evolutionary economics. This relates to the assumptions made in evolutionary economics to address real-world questions, for example, the assumption of bounded rationality. These assumptions are taken to be more realistic than the ones made in orthodox economics. The third set of considerations has to do with the style of theorizing in evolutionary economics. This style is argued to be less obsessed with analytical rigor and to be more attentive to empirical evidence than orthodox economics.⑨

Blaug seems to have regarded questions of long-term growth and of technological change to be especially relevant. This is indeed what the then forthcoming book, which in the meantime has been published,

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⑨ I realize that the distinction made here is not clear-cut. For instance, the conception of competition as a dynamic evolutionary process could also be regarded as part of the contents and has also consequences for the style of theorizing. I nevertheless think the distinction is helpful for my purposes in this essay.
Economic transformations (Lipsey, et al. 2005) is all about. And Blaug seems to have regarded questions that general equilibrium theory tries to answer, such as the existence and uniqueness of multimarket equilibrium, as irrelevant questions from a practical perspective. Likewise, he also chastised general equilibrium theory for its futile conception of (“perfect”) competition as an end-state. By contrast, he praised evolutionary economics for its much more plausible conception of competition as a dynamic process. And, indeed, we do find attempts to study the basic mechanisms of competition as a dynamic evolutionary process in the “recent” books that Blaug refers to. As we saw, selection is one of the basic mechanisms driving aggregate change, the other one being search. As Lipsey and Chrystal (2007) argue in their textbook under the heading of “Competition as an evolutionary process”, in which they closely follow Blaug’s (1978 [1962]) discussion of Hayek’s and Schumpeter’s Austrian view on competition, a great deal of competition in markets takes the form of competition in innovation. Blaug’s Austrian leanings seem to be apparent in his predilection not just for the sorts of issues addressed in evolutionary economics but also for its contents.

About these contents we can also be brief. Evolutionary economics discards the “choice-theoretic models in mainstream micro-economics”, as Blaug argues. In particular, it rejects the idea that firms maximize profits. As we saw, it is not that evolutionary economics deny that firms are primarily motivated by profit. On the contrary, this is affirmed. It is rather that they deny that firms have the perfect information, foresight, and rationality that would ensure they succeed in making maximum profits. Lipsey (2012) argues that especially when it comes to decisions about how much to spend on research into technological change, there is a lot of uncertainty. Following Frank Knight, Lipsey distinguishes uncertainty, when the odds of things happening is unknown (and cannot be calculated), from risk, when the odds can be known. In situations of uncertainty, Lipsey argues, two equally well-

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10 Contrasting the questions that evolutionary economics and general equilibrium theory (GET) address in this way might be a bit unfair. There are attempts to address long-term growth, starting from the framework of GET to be sure (such as DSGE models), but these treat technological change as exogenous random shocks (rather than as endogenous change, as in evolutionary economics). Furthermore, presumably Blaug would have found these attempts to be lacking in terms of contents (their allegedly unrealistic assumptions) and style (their obsession with analytical rigor).

11 Evolutionary economics does not seem to give a prominent place to the “norms of behavior” Blaug speaks of.
informed maximizing agents may make different choices. And until the results of both choices are known, there is no way of deciding who made the better choice. Instead of fully rational maximizing (or optimizing) firm behavior, evolutionary economists assume trial and error learning, where search is guided by heuristics.

What Blaug says about the different styles of theorizing is more open for debate. For one thing, it is not obvious that evolutionary economics is in closer touch with empirical evidence than mainstream (or orthodox) economics. Perhaps all Blaug means to say here is that the contents of evolutionary economics are more in line with casual or anecdotal evidence, so that it is more plausible, than mainstream economics. That seems incontrovertible. But when it comes to putting models in evolutionary economics to strict empirical tests, things are not so clear. Even proponents and advocates concede that evolutionary economics has been lacking so far in this respect. Furthermore, not all evolutionary economists would agree that their models are less rigorous than the models in mainstream economics. Starting with Nelson and Winter (1982), evolutionary economists have been building models of their own, often but not always involving computer simulations (as Blaug observes; for a useful overview see Safaryńska and van den Berg 2010). Not all the models are analytically tractable models that allow for the logical deductions Blaug describes. But they do all impose a form of rigor and evolutionary economists do not want to compromise on the precision of their results.

We can only speculate about the relative weights Blaug attaches to the three sorts of considerations. It is also not clear whether, and if so how, he thinks that the three sorts of considerations are independent of each other. Blaug seems to believe that the practical relevance and the empirical adequacy of theories are closely related. He seems to believe in particular that theories that are out of touch with empirical evidence cannot possibly be practically relevant. But why would this be so? It seems a theory that is empirically inadequate might still address clearly practically relevant issues such as economic growth or economic recessions and crises (see also Hodgson 2013). Especially if the theory succeeds in picking important forces, mechanisms and/or factors, it is not obvious that we should find the theory lacking. Suppose the theory is empirically inadequate because it assumes that the influence of other forces, mechanisms and/or factors is negligible and suppose further
that that assumption is most of the time but not always warranted. It is not obvious that such a theory is lacking.

Perhaps Blaug believes that empirically inadequate theories cannot possibly be practically relevant because he equates practical relevance with policy relevance. Perhaps he believes that reliable policy recommendations cannot be based on theories that are somehow out of touch with reality. If so, I think there still is an ambiguity here. As F53 and the ensuing debate over it shows, theories can be out of touch with reality in their assumptions or in their empirically testable implications (or both, of course). As I just suggested, theories that have plausible assumptions (such as bounded rationality) might still have implications that are disconfirmed by the data. Conversely, theories that have empirically implausible assumptions (such as perfect rationality with perfect information and foresight) might nevertheless have implications that are confirmed by the data. To cut a long story short, I take it that Blaug believed that mainstream economics has assumptions that are clearly out of touch with reality and that mainstream economists do not engage in serious empirical tests of its implications. It seems that rather than arguing that mainstream economics has a poor predictive record, Blaug is arguing that mainstream economists simply do not care so much about serious testing the implications of their theories. It seems that he also held that the combination of both renders them virtually useless for policy purposes.

**Analytical Rigor Versus Practical Relevance: An Inevitable Trade-off?**

A theme pops up here that preoccupied Blaug in his later work: the relative weighting of analytical (or logical) rigor and practical relevance (see also Backhouse 2013; Hodgson 2013). It is clear that Blaug felt that in mainstream economics the relative weighting is extremely lopsided. Mainstream economics is seen by Blaug as being obsessed with formal rigor, but also as being almost totally devoid of practical relevance. Blaug (1997a) explicitly states that for mainstream economics analytical rigor is everything and practical relevance is nothing. At times it seems Blaug believed there is an inevitable tradeoff between analytical rigor and practical relevance: increasing the one always goes at the cost of

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12 Vernon Smith (2008) argues that standard economic theory has been shown to rather accurately predict aggregate behavior in anonymous impersonal market settings, for example.
decreasing the other. You cannot have a theory that excels in terms both simultaneously. According to Blaug, evolutionary economics strikes a better balance between the two than mainstream economics. In comparison to mainstream economics, evolutionary economics sacrifices a bit of rigor, but that is more than compensated for by a gain in practical relevance (and empirical adequacy).

Indeed, Blaug seems to have thought that much of mainstream economics is irrelevant for practical purposes because of its obsession with formal rigor. What is not entirely clear is whether Blaug believed this holds for formal modeling in general (i.e., for all sorts of formal models), only for particular kinds of formal modeling, or only for particular kinds of formal modeling in combination with particular assumptions (and contents in general). The first possibility is that Blaug believed that not only analytically or logically rigorous formal models, but all sorts of models work at the cost of practical relevance. The second possibility is that Blaug believed this only holds for analytically or logically rigorous formal models. The third possibility is that Blaug believed that this only holds for analytically or logically rigorous formal models that assume perfect rationality.

There seems to be some textual evidence for all three possibilities. Sometimes Blaug seems to suggest that it is the tidiness and neatness of all sorts of models that prevents models from coming to grips with the messiness of the real world. At other times he seems to argue that the “radical use of computer simulation models” by evolutionary economists suffer less from practical irrelevance than the analytically tractable models of mainstream economics. And yet at other times he seems to suggest that it is the specific combination with the assumptions of static equilibrium and perfect rationality that renders the choice-theoretic models of mainstream economics practically irrelevant.

About the third possibility we can be short: here it is suggested that it is not modeling as such that stands in the way of practical relevance, but the modeling of empirically implausible assumptions. This would be a critique not of formal models per se, but only of formal models with the wrong contents. Not models as such, but the specific assumptions of static equilibrium and perfect rationality in mainstream economics are taken to be the culprit. If instead empirically more plausible assumptions were modeled, Blaug would find no fault in them, even if the models were analytically tractable ones.
A problem with this interpretation is that Blaug never was particularly fond of evolutionary game theory. Evolutionary game theory provides analytical models. It arguably dispenses with the assumptions of perfect rationality and static equilibrium. It replaces the former by the assumption of bounded (or sometimes even zero) rationality. And instead of assuming that populations are always in equilibrium, it (at least in its dynamic versions) examines the conditions under which evolutionary processes converge on equilibria. Evolutionary game theory also (and particularly) analyzes the stability of equilibria, an issue that according to Blaug takes center stage in the process conception of competition he favored (Blaug 1997b, 241). Here we have analytical models based on more realistic assumptions and dealing with the right sort of issues, but that Blaug did not seem to have liked so much.

This leaves us with the first two possibilities mentioned above. There seem to have been things with modeling as such, or with analytical modeling in particular, that Blaug was rallying against. The textual evidence falls short, I think, of settling the issue of whether Blaug had problems with formal models in general, or only with analytical models. Blaug seems to speak approvingly of Nelson and Winter’s (1982) computer simulation models. This would suggest that he has no problems with certain sorts of non-analytical models. But Blaug also suggests that it is the tidiness and neatness of models that he is objecting against; a tidiness that is hard to square with the messiness of reality. The latter seems to hold for all sorts of models, not just analytical models. This would suggest that he has problems with all sorts of formal models.

**ON THE MERITS OF MODELS**

Whatever Blaug might have thought about this, I think there are problems with both positions. My problem with the first position is that models might provide illuminating insights in real-world phenomena precisely because they abstract from some “messy details” in the real world. In their *Guide for the perplexed*, the formal evolutionary theorists

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13 At the same time Blaug seems to have been fascinated by evolutionary game theory. I vividly recall (personal communication) that he followed the debate between Ken Binmore (1998; 2002) and Robert Sugden (2001a; 2001b) over evolutionary game theory and its explanatory potential with great interest.

14 For many modelers, not just non-evolutionary ones, it is just the other way around: simulations are poor substitutes for analytic models (McElreath and Boyd 2007, 8).
McElreath and Boyd (2007, 4) put it as follows: “models are like maps—they are most useful when they contain the details of interest and ignore others”. As the saying goes, only by backing away from the trees we might be able to see the forest. General patterns and regularities (and, who knows, laws) in reality might elude us if we insist that our theories should represent reality in all its messy details. Actually I take this problem to be so obvious, that I cannot imagine Blaug would disagree with it. Perhaps what Blaug meant was that we should not only have highly abstract models in economics, and that there should also be room and appreciation for empirical research in the messy details of the real world. If this is what Blaug wanted to get across, then I fully agree.

I think we can also defend the turn from appreciative theorizing to formal theorizing in Nelson and Winter (1982) in terms of the need for insights in general patterns and regularities at higher levels of analysis. Nelson and Winter distinguish between appreciative and formal theorizing in orthodox economic theory. Whereas the formal theorizing focuses on static equilibria on the assumption that economic agents maximize well-defined goal-functions, appreciative theorizing in orthodox theory tells stories about equilibria might be reached and replaced, assuming that economic agents are gradually groping towards their goals. Friedman’s selection argument is an example of appreciative theorizing. As Northover (1999) points out, appreciative and formal theorizing can also be found in Nelson and Winter’s own evolutionary theory. In the first five chapters of Nelson and Winter (1982) the theoretical backbones of their evolutionary theory are discussed in informal terms. In subsequent chapters formal evolutionary models are presented and explored. It has been observed by many that in the transition from the informal discussions to the formal explorations much is lost in terms of richness in detail. In the first chapters, close attention is paid to what routines in firm behavior are, how they typically operate and what are their main functions, for example, while in the models in later chapters routines are simply represented as decision-rules of firms. This “simplification” in Nelson and Winter’s treatment of routines can be defended, I think, as a necessary step to get a clearer picture of general patterns in the dynamics in industries at a higher, aggregate level. Such a clearer picture can only be obtained by “zooming out” on the messy details of the inner workings of routines (Vromen 2011a).
Blaug is right that most of the models Nelson and Winter develop are computer simulation models rather than analytical models. But Nelson and Winter do not eschew analytical models in general. And they surely do not avoid logical deduction. Nelson and Winter apparently believe something can be gained by doing so. And rightly so, as I shall argue. This brings me to my response to the second position against particular types of modeling described above. Analytical models and valid deductive reasoning starting from simple principles can yield interesting and important insights.

Starting with Winter's (1964) doctoral thesis, Nelson and Winter have been probing the validity of Friedman's selection argument in F53. In particular they have critically examined what I called Friedman's second belief in his selection argument: assuming that there is indeed a process of competitive market selection (similar to natural selection in biology; which I called Friedman's first belief), will this process converge towards an end-state in which all firms behave as if they were fully rational, fully informed profit-maximizers endowed with perfect foresight? Nelson and Winter do not accuse Friedman of wrongly assuming that there is a process of competitive market selection. To the contrary, they believe this is a valuable insight in Friedman's appreciative theorizing that is to be retained in their own evolutionary theory. They accuse Friedman of not critically and rigorously exploring whether his second belief follows from this insight. Nelson and Winter argue that formal modeling is required to carry out such a critical and rigorous exploration. Only then we can identify hitherto implicit (or tacit) assumptions that have to be made for Friedman's second belief to follow logically from his first belief (Nelson and Winter 1982, 141).

Nelson and Winter also develop simple analytical models to explore to what extent aggregate change in industries can be explained on the basis of selection alone. This exploration is not based on Nelson and Winter's belief that selection is the only important or the most important mechanism producing aggregate change. According to Nelson and Winter search is an equally important second mechanism producing aggregate change. The reason why they nevertheless leave out search in these models is that they want to counter the intuition that most if not all of aggregate change is the result of changes in firm behavior. Nelson and Winter want to show that much aggregate change can be explained even if the behavioral characteristics of firms were constant (1982, 9). Even though the latter assumption is clearly unrealistic, they believe this
is useful exercise. For it proves the intuition wrong that aggregate change is always produced by “individual” change.

I think it is fair to say that formal modeling is on the rise in evolutionary theorizing, not just in attempts to account for empirical phenomena but also in attempts to settle foundational issues. With respect to the latter Alan Grafen’s (1999; 2007) project of formal Darwinism is a case in point. An interesting demonstration of what formal modeling can contribute to our thinking about foundational issues in evolutionary theorizing is provided by Henrich and Boyd (2002). The issue at stake is whether the same equation that is often used to analyze biological evolution, the so-called replicator dynamic, can also be used to analyze cultural evolution. Both sides in the debate agree that in processes of cultural change, traits are not inherited genetically (as in biological evolution) but are transmitted socially (through imitation, for example). Critics of the use of replicator dynamic to analyze cultural change such as Dan Sperber (2000) draw the attention to the existence of systematic (so-called content-based) biases in social transmission. Since replicator dynamic assumes that transmission is faithful, this seems to invalidate replicator dynamic as an equation to track processes of cultural change. Henrich and Boyd (2002) show, however, that even if there are strong systematic biases in social transmission, replicator dynamic might still accurately track cultural change. Thus what Henrich and Boyd point out is that faithful replication, although a sufficient condition (if conjoined with other suitable conditions), is not a necessary condition for replicator dynamic to accurately track change (but see Vromen 2011b for a critique).

The common theme here is that our intuitions and our reasoning powers, unaided by formal models (including analytical models), are weak and error-prone. Without the aid of formal models, our intuitions might easily lead us astray. And we might easily overlook tacit assumptions and erroneously think some conclusion follows logically from some premises. It is tempting to draw an analogy with our senses. Unaided by microscopes and other instruments, our senses might be a poor guide to determining what exists in the real world. Similarly, unaided by formal models, our intuitions and reasoning capacities might be a poor guide to determining what conclusions follow from some premises.

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15 Hodgson and Knudsen (2010) use of the so-called Price equation to define “selection” in their project of “Generalized Darwinism” provides another example.
Logically and analytically rigorous modeling is worthwhile only if the premises make sense, it might be argued. If the premises are wildly implausible assumptions, rigorous modeling easily deteriorates into futile and arcane exercises (at least for the purposes of an empirical science). But this shifts the discussion back to the issue of whether the forces and mechanisms modeled are believed to be real and important ones. I have been arguing that something interesting can be learned from analytically rigorous modeling if the premises one starts with are believed to make sense. Even though it is true that in a valid deduction all the information in the conclusion is entailed in the information in its premises, the information expressed in the conclusion might nonetheless be surprising and cognitively significant.

McElreath and Boyd (2007) discuss various ways in which simple analytical models can aid our understanding of the world. One of them I just discussed: simple analytical models can bring counterintuitive results of certain premises to light. McElreath and Boyd argue that counterintuitive results can lead to new theory construction and data collection. Another way is that models can tell us which possible explanations of phenomena are internally consistent and when conclusions follow from their premises. This in turn helps us in narrowing down the set of possible explanations. Yet another way in which models can teach us something is that they facilitate communication. Concepts are often only loosely defined in informal theorizing and verbal reasoning. Formal modeling allows for less vagueness and much greater precision. Finally, formal modeling can facilitate prediction. In formal models it is often much clearer what predictions follow from the model than in verbal theorizing.

Wrapping things up, it is not entirely clear exactly what renders the formal models of mainstream economics irrelevant for practical purposes in Blaug’s view. It might be that Blaug thought that it is the tidiness of these models that makes them out-of-touch with empirical reality, but I argued that the tidiness as such should be no problem. On the contrary, it is only because of their tidiness that models enable us to spot patterns and regularities that otherwise would elude us. Furthermore, this would apply to all models, not only to the ones advanced in mainstream economics. Another possibility is that it is the fact that the models advanced in mainstream economics are analytical models that renders them practically irrelevant. It might be that Blaug thought that the non-analytical simulation models in evolutionary
economics fared better in this respect. I argued, however, that analytical models can serve useful functions for an empirical science. Non-analytical models do not have merits only; they also have drawbacks in terms of (lack of) transparency. Moreover, evolutionary economists (and evolutionary theorists in general) also construct analytical models whenever they can. A last possibility is that Blaug did not oppose analytical modeling as such, but only analytical modeling in combination with questionable assumptions. The problem he had with the analytical models in mainstream economics might be that they assumed things like perfect rationality in individual behavior and the guaranteed existence of static equilibria as the end-state of competition. Analytical modeling based on more realistic assumptions might yield interesting and important insights into the real world. If the latter is what Blaug meant, I am on his side. The same holds for Blaug's insistence that the present imbalance in mainstream economics in the valuation of various theoretical virtues, analytical rigor and precision on the one hand and empirical adequacy and practical relevance on the other, should be restored.

CONCLUSION
What attracted both Blaug and I to Nelson and Winter's type of evolutionary economics is primarily its plausible conception of competition as a dynamic evolutionary process. In this conception, "evolution" stands not only for selection for positive profits in competitive markets, but also for the active search of firms for more profitable production techniques. This search does not take the form of an optimization exercise, defined over well-defined choice options, but of trial-and-error learning. Technological change does not appear as an exogenous shock, as in "orthodox" economics, but as endogenous change, produced from within the economic system and explained by evolutionary economics. Insofar as there is place for notions of static equilibrium in evolutionary economics, static equilibria are seen as possible end-states (or possible stationary states) on which evolutionary processes can converge. Rather than assuming that market economies are always in equilibrium, one of the things evolutionary economics investigates is whether, and if so how, equilibria are reached.

Blaug and I also agree that if there is a realist grounding of Friedman's as-if reasoning in F53, it is in Friedman's belief that market competition entails a selection process. Or, to be more precise, Friedman
bases his confidence in the usefulness of the maximization-of-returns hypothesis on two beliefs: first, the belief in the efficacy of the selection process just mentioned and second, that only firms that de facto realize maximum returns (often called profits) survive the selection process. Pace Mäki (2009), realism in F53 is to be found in these beliefs, not in Friedman's belief that the profit motive is the dominant determinant of firm behavior. The version of realism involved would admittedly be a very weak one, one in which the beliefs that undergird Friedman's acceptance of orthodox economic theory are not explicitly cited in the theory. Given that Nelson and Winter (1982) elevate Friedman's first belief to one of the cornerstones of their own “unorthodox” evolutionary theory, Nelson and Winter's evolutionary theory involves a stronger, more substantive version of realism. In one of their analytical models Nelson and Winter examine whether or not Friedman's second belief is tenable. Blaug seemed to have seen little, if any use in the sort of rigor and precision that analytical models provide. But Nelson and Winter argue convincingly that more analytical rigor is needed to examine the validity of Friedman's selection argument than Friedman himself exhibits in his informal argument, and that this is exactly what analytical models provide. In general, analytical models enable one to better distinguish what implications a particular set of assumptions do and do not have (and, conversely, what assumptions have to be added to back up some alleged conclusion). But—and I am more than willing to give Mark the last say here—for the purposes of an empirical science such formal exercises are only justified if models explicitly cite what are believed to be important mechanisms in the real world.

REFERENCES


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