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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

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One of the things Mark Blaug and I shared was a sympathy for evolutionary economics. In his 'Disturbing currents in modern economics' (1998),[1] 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).

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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

[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

> 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.[3] 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

[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.[4] 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.[5] 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

[4] To some extent the idea that the two forces mutually exclude each other is suggested by Nelson and Winter (1982) who contrast their own view that firm behavior is produced by rigid routines with the view put forward in "orthodox economics" that firm behavior is the result of flexible choice. Note, however, that Nelson and Winter do not deny that firms are led by the profit motive (Nelson and Winter 1982, 4); they only do not assume from the outset that all firms succeed in making maximum profits.

[5] My own discussion of evolutionary mechanisms (see Vromen 1995) was also very much in this spirit.

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.[6] 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.[7]

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

[6] 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.

[7] 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.[8] 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.

[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,

[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.[10] 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.[11] 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-

[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 Safarzyńska 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.[12] 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

[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.[13] 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.[14] 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

[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.[15] 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 to 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.

[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.

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