Can Biology Inspire Economists?

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In his survey paper, Elias Khalil has done a commendable job of showing what economists can learn from biology. While imports from one scientific discipline into another can be highly productive, there is always the risk that they will lead to dead ends. For this reason, it is highly useful to have the kind of "field guide" that Khalil provides, which catalogs biological ideas that can put economists on some good leads, while also pointing out possible blind alleys.

Nevertheless, one cannot help being reminded by Khalil's list of "all the possible areas of research" of that passage from Borges quoted by Michel Foucault which mentions "a certain Chinese encyclopedia [in which] it is written that animals are divided into: (a) those that belong to the Emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs..." and so on [Borges 1964, p. 103]. Does Khalil really mean to say that his list exhausts all the possibilities? Since one cannot discern a system behind his list, it is hard to see how it could be shown to be complete. In my comments, I will discuss two potentially useful influences of biology on economics which Khalil does not mention.

Khalil is quite right to examine various metaphors that have been used to relate economic and biological phenomena, since a principal means by which scientists obtain new ways of thinking about phenomena is by making analogies. Analogies across sciences however are not the only way that one science can influence another. One science can get from another science its whole view of the form scientific theories and explanations should take, how they ought to be verified, and how evidence for them should be collected. This is what happened with economics; it adopted physics as its model. While the fact that economics gets from physics its basic model of rational choice — rationality as constrained optimization — together with much of its mathematics is well known, the fact that it gets its very view of what it is to be a science from physics, and that this may have undesirable consequences, is less appreciated. This can be illustrated by considering a paper of Philip Mirowski, who is the observer of the current state of economics most associated with the argument that economics imitates physics to an excessive degree.

In his paper "Mathematical Formalism and Economic Explanation", Mirowski notes that "There is nothing obvious about the definition of human rationality as the maximization of an objective function over a stationary field": "the quest for a natural economic metric", the existence of which is required for there to be such an objective function, turns out to be "futile" (as Mirowski demonstrates) [1986, pp. 188, 233]. But economists do not need such a natural, objective metric in order to be able to theorize about exchange. Instead, they can make use of "strong symmetries upon the processes of trade" which economic actors themselves impose because they "have a very real need to make causal claims about their activities in the economic sphere... In physics, one links causal states with effect states by imposing the restriction that both sets of states possess the same energy. In economics, one causally links the antecedents with the consequences of an exchange by imposing the restriction that both states possess the same value."

The way to rigorously study these restrictions is by employing the mathematical theory of groups [pp. 233-34].

There are two ways in which this argument inappropriately takes physics as a model. First, Mirowski does not appear to take sufficient cognizance of the difference between the phenomena studied by economics and those studied by physics. There are two ways in which mathematics can be of value in science: it can be used to describe fundamental principles of nature, and it can be used to conveniently summarize complex phenomena. To give an example of the latter, one can imagine a biologist finding a relationship between certain structural properties of a class of viruses and the probability that a virus from that class will infect a given kind of organism. Even though such a statistical relationship exists, no one would suppose that the actual mechanisms underlying viral infection are mathematical: what matters is the shape of the molecules at the viruses' surface and whether these allow the viruses to bind with cell membranes, and other such chemical
properties of cells and viruses involved in the mechanisms of viral infection — none of which are conveniently, much less solely, describable in mathematics. Now, as Richard Feynman has observed, “the strange thing about physics is that for the fundamental laws we still need mathematics” [1965, p. 36]. No matter how hard people have tried to find mechanisms underlying fundamental physical laws such as Newton’s laws of motion, we are still left simply with the mathematical laws themselves.

Now, if the processes studied by chemistry and biology are not fundamentally mathematical, why should one expect the case to be any different with economic processes? After all, the latter are produced by the machinery of the human mind, a highly complex system which, being the product of natural selection, is likely to be extremely jury-rigged, rather than characterized by a few compactly describable principles. It may be that the economists who introduced differential calculus into economics thought that the law of exchange is a “fundamental law” of the economy exactly analogous to the fundamental laws of physics. But why should we take mathematical economic theory that way? It may well be that one can productively use mathematics in economics to summarize certain kinds of phenomena, in the same way that the natural sciences use mathematics to study complex phenomena — not fundamental laws. And indeed, even if such mathematical models as general equilibrium theory do not provide as complete and deep an understanding of economic phenomena as their originators had hoped, it cannot be denied that these models can be practically useful, and serve to point out interesting regularities that one observes in the economy. If physicists such as Feynman are correct in saying that physics is unique in its fundamental laws being mathematical — and if one studies a few scientific disciplines to any extent, it is hard to disagree with them — then the way to go beyond such models is not by searching for a different mathematics, but by doing what natural scientists do when they try to understand complex phenomena: investigating the qualitative properties of the mechanisms producing those phenomena. Since it is by the latter kind of investigation that one expects to discover the true, fundamental principles underlying those phenomena, one is not bothered, the way Mirowski is, that one’s quantitative, mathematical models are not without peculiarities and not universally applicable. Mirowski’s arguments against neoclassical theory and his call for a “reconstruction of [mathematical] economic theory” make sense only if one does take economics to be like physics: as searching for fundamental mathematical laws.

A second way in which Mirowski takes physics as a model for economics is the following. He writes that economic actors “impose strong symmetries upon the process of trade” (which is what makes group theory applicable) because they “have a very real need to make causal claims about their [economic] activities”. Thus, for example, if A has an apple and B has a banana and they trade, the way A understands this trade, since he must think in causal terms, is that his apple “caused” the banana which is in his possession after the trade, in the sense that the values of the banana and the apple are equivalent (as they must be for the trade to be legitimate, which it must have been, since both A and B were willing to enter into it). Now, I doubt very much that people actually think this way when they trade with one another: they do not have to posit any kind of equivalence between the items traded for them to see a trade as legitimate. What makes a trade legitimate is that both parties enter into it willingly, without coercion — nothing more. Furthermore, it is very doubtful that economic actors “have a very real need to make causal claims about their activities”. Social action, as commonly understood by sociologists and philosophers, requires actors to make certain assumptions about each others’ actions in order for these actions to make sense. One of these assumptions is that actors are free and autonomous entities with subjectivity, who select their actions not as the result of some deterministic, causal process, but on the basis of deliberating about reasons. The bringing in of causal modes of thinking into social interaction would bring to an end social interaction as we know it (or at the very least make it extremely impolite). Interestingly, Mirowski develops his conception of economic exchange as a reconstruction of Marx’s treatment of it. As is well known, one of the reasons Marx gave for his “dialectical materialism” being scientific was that it was materialist: it dealt with objective, material matters of fact like those studied by the physical sciences. According to Marx, to non-trivially attribute economic actions in a capitalist setting to human subjects is to fall into the ideology of idealism. In this respect, neoclassical economics is much less imitative of physics than was Marx: the utility model is a formalization of subjectivity (in so far as the latter involves choice), and subjectivity is certainly outside of the world studied by physics. In his move towards reconstructing the Marxian theory of value, Mirowski ironically shows a greater predisposition toward holding up physics as an ideal to be emulated.
than do the neoclassicals: according to him, economic actors — since they think in causal terms about their interactions — and not just economists, think like physicists.

In view of this apparent tendency even of someone who usually very perceptively points out instances of economists’ undue emulation of physics to slip into ways of thinking that are appropriate only specifically within physics, it would be desirable for economists to have at their disposal a successful science other than physics which could more appropriately serve as a model and inspiration. We believe that biology can serve such a function. Khalil observes that “orthodox neo-Darwinists... view the organism as a black box which facilitates the survival of pre-constituted, selfish genes”, in the same way that neo-institutionalists such as Oliver Williamson understand the firm as “a black box that facilitates the pursuits of [atomistic] agents” [typescript, p. 14]. This gives a highly misleading view of the method that contemporary biologists actually follow. Biologists seem to have achieved the kind of “methodological pluralism” that economic methodologists often advocate for economics [Caldwell 1982; Hausman 1992]. Specifically, they have been able to develop a unified body of theory that recognizes the contingent, historical nature of evolutionary phenomena while also allowing one to use mathematical models based on optimizing assumptions, when these are helpful for convincing one of the soundness of one’s reasoning. Two examples will serve to illustrate this. They are representative instances of two different patterns of explanation that are routinely used by biologists in their day to day work.

Around 20 to 30 million years ago, the group of predatory animals called creodonts, dominant up until that time, began to be replaced by the predatory group known as miacids, of which the modern carnivores are the descendants. Although various explanations for why the miacids came to prevail have been suggested, for instance, that they had larger brains than the creodonts and hence outsized the latter, or that the creodonts were clumsier predators and thus failed to compete, recently a more promising explanation has been put forward. This is based on evolutionary changes in teeth; the cheek teeth (“carnassials”) used for slicing through flesh were slightly more forward in the jaws of miacids, leaving ample space in the rear for mashing vegetables or other foodstuffs. The teeth in creodonts, however, were so placed as to force them into a scissor-like motion of cutting which could only manage a diet of meat. As potential prey-populations fluctuated in number, miacids could adjust their diets to other food sources. [Gittleman 1993]

What this example shows is that while Darwinists do use the principle of natural selection as a universal explanatory principle, analogous to the assumption that agents optimize used by economists, this does not preclude them from “opening the black box” of the organism and examining how different specific characteristics of the design of an organism might have affected its chances for survival under various circumstances that occurred in evolutionary history. It also shows, incidentally, that while evolutionary biologists assume that organisms evolve in the direction of greater adaptation, they do not view this adaptive potential as unconstrained: the basic design of creodonts could not be modified to bring about an adaptation allowing more flexible eating behavior. (Presumably why this was so is that there was no way of modifying the genetic programs guiding embryological development to bring about the requisite change in the placement of teeth in a series of adaptive incremental changes, at least not quickly enough to maintain competitiveness with respect to the miacids.) If the principle of natural selection operates under a kind of “rationality principle”, therefore, it is one of bounded rationality.

Our second example concerns the question of why the sex ratio of almost all species is one to one. Since one male can inseminate potentially thousands of females, while males generally do not contribute to the production of offspring aside from this insemination, having as many males as females might seem wasteful, from the perspective of the species. One can see why the one-to-one ratio holds by employing the kind of general, abstract reasoning favored by economists, once one looks at the problem in terms of the replication of genes. Suppose that a species has evolved so that far more females are born than males. The high proportion of females born would be the result of genes in the population predisposing children to be female. Now suppose a mutation occurred in a member of the species that predisposed its offspring to be male. Since by assumption there are only a few males in the population, these would mate with many females, and this individual would end up having many more grandchildren than its rivals, which produce mainly females. Consequently, its genes will spread rapidly through the population, causing the sex ratio to move toward one half. It can thus
be seen that the only stable situation would be the one that commonly holds, which is that there are about as many males as females [Dawkins 1976]. This can be shown formally, by means of a class of game-theoretic models which deal with what John Maynard Smith has called evolutionary stable strategies: biologists routinely use this class of models to deal with much more complicated problems, in the course of which they obtain testable predictions.

Thus, one sees that biologists are able to combine a historical treatment of the phenomena that interest them, in which they consider specific qualitative properties of organisms, with formal mathematical techniques where these are appropriate. In fact, one of the things that strikes an economist above all about the writings of biologists is the way that biologists, even if they are dealing with very theoretical questions (i.e., questions of a general nature which relate to the fundamental principles of the discipline), and even if they often use mathematical models, constantly refer to specific empirical findings in their texts. Their practice can thus show economists that Herbert Simon’s admonition to them to look at what happens inside organizations is workable, and need not lead to hopelessly getting lost in particulars.5

How one idea from biology that we have just run across — that the adaptive ability of species may be constrained by the rigidity of developmental programs that were selected for under a significantly different environment than the one they now face — can be imported into the study of the economic sphere is illustrated by some work of William Starbuck [1983]. According to Starbuck, organizations facing crises do not foresee the results of their actions: they misperceive environmental opportunities and threats, impose imagined constraints on themselves, and expect rational analyses to produce good strategies. Organizations create behavior programs to repeat their successes, but these programs turn into some of the main causes of crises. Programs focus perceptions on events their creators believe important, so the programs blind organizations to other events that often turn out to be more important [p. 92; emphasis added].

(We may note in passing the central theoretical role that is played by the program concept here; interestingly, Mayr [1982] has observed that the incorporation of this concept into biology has played a central role in bringing biology into its present mature state, and indeed lies at the center of the modern understanding of what life is.) Starbuck illustrates the detrimental effects that the rigidity of programs can have on organizational behavior by considering the example of the calculator manufacturer Facit AB. That company grew large and profitable while making and selling business machines and office furnishings.... Although Facit made many products, the top managers believed the key product line to be mechanical calculators; they saw products such as typewriters, desks, and computers as peripheral.... Facit concentrated on improving the quality and lowering the costs of mechanical calculators, and it created behavior programs to facilitate production and sale of mechanical calculators.... In the mid 1960s, Facit borrowed large sums and built new plants that enabled it to make better mechanical calculators at lower costs than any other company in the world [and was thus able to grow substantially].... The engineers within Facit concentrated on technologies having clear relevance for mechanical calculators.... Costs were low, service fast, glitches rare, understanding high, and expertise great. But only within the programmed domain!... Although some lower-level managers and engineers were acutely aware of the electronic revolution in the world at large, this awareness did not penetrate upward, and the advent of electronic calculators took Facit’s top managers by surprise. Relying on the company’s information gathering programs, the top managers surmised that Facit’s mechanical-calculator customers would switch to electronics very slowly because they liked mechanical calculators. Of course, Facit had no programs for gathering information from people who were buying electronic calculators.... Actual demand for mechanical calculators dropped precipitously.... With bankruptcy looming, the board of directors sold Facit to a larger firm. [pp. 92-93]

In Starbuck’s analysis, we can see a combination of concrete historical analysis with theorizing of a general nature that is analogous to the general practice followed by evolutionary biology. His analysis can serve as a prototype for a pattern of explanation that could be quite productively employed by economists. It should be pointed out that the kind of organizational
irrationality illustrated by the Facit case is by no means unusual or insignificant. Oversimplifying slightly, one might argue that in the post-World War II period, American automobile companies gradually got out of the automobile manufacturing business and into the automobile selling business [Yates 1983]. (It makes sense to talk this way if one stops analyzing economic activity in terms of highly abstract profit functions, which are somehow optimized, and considers instead the actual programs that organizations use to carry out their activities.) They did so because, since they emphasized marketing from the very beginning of this period, and the strategy was initially highly successful, in accordance with Starbuck’s hypothesis, this is what they came to emphasize more and more. Meanwhile, Japanese companies concentrated on producing well designed and built automobiles, proceeding on the assumption that if their quality is high, they will sell themselves. The consequent poor relative performance of the U.S. automobile industry led to a substantial drop in market share, which in 1988 accounted for 53 percent of the U.S. trade deficit [U.S. Census Bureau 1990]. The phenomenon studied by Starbuck is thus of a high macroeconomic significance.6

From the example just given, it appears that something is missing from Starbuck’s theoretical framework: he sees programs as being specific to organizations, while the case of U.S. auto manufacturers suggests that programs of firms in the same region or country can be largely common to those firms. That programs are largely culture-specific and constant across organizations within a given national culture is suggested also by the following remark of a European businessman (who according to the “standard argument”, must more or less know what he is talking about, since otherwise he would not be in business):

U.S. managers want everything to be simple. But sometimes business situations are not simple, and they cannot be divided up or looked at in such a way that they become simple [by for example applying the accounting techniques and formal decision criteria taught by business schools]. They are messy, and one must try to understand all the facets. This appears to be alien to the American mentality.

[Hayes and Abernathy 1980, p. 68]

Since this remark, although not very flattering, at least has the virtue of matching fairly well similar remarks made more than a century earlier by the respected observer of American mores Alexis de Tocqueville, one might surmise that cultures have relatively stable and robust qualities, and what is more, that specific qualities of a given culture can have significant economic effects. Although mainstream economists have traditionally ignored culture, that may now be changing.7 Does biology offer any concepts that might be useful to economists in this regard?

We believe that it does; this constitutes the second major way in which we think that biology has something to offer economists. Dawkins [1976] has proposed the concept of a program-like culturally transmitted entity meme analogous to genes.8 We believe the meme concept can be helpful by allowing economists to adopt a mode of thinking followed by biologists, in which the latter look at evolutionary problems in terms of selection of individuals or in terms of selection of genes, depending on which is more convenient.9 It may be worthwhile for economists to adopt a similar practice, abstracting from individuals when trying to answer certain questions, and focusing instead on the programs that are largely constant across individuals and which, if slightly different across two populations, may have differences the effects of which are scarcely perceptible on the local level, but when aggregated, may have highly significant macro effects, particularly in “historical” time. For the study of organizations, the meme concept similarly allows one to abstract from the individuals constituting an organization: an organization could be taken to be constituted by the memes specifying the formal rules of the organization as well as the “organizational culture”.10 If one adds some complexity arguments here — organizational members are faced by time pressure which generally makes it difficult for them to determine which actions best serve their own interests, and thus forces them to follow organizational programs with a minimum of reflection — this approach holds the promise of providing a simple solution to the principle-agent problem.

The meme concept in so far as it concerns economics is at this point no more than a suggestive metaphor, and there is no way of knowing whether it will help lead to a useful theory. But with the recent extraordinary economic success of south-east Asian countries, which have apparently entered upon a different path of economic development than that of other countries of the Third World, and with the suggestions one occasionally hears that East Asian societies may have picked up the mantle of being the most economically dynamic societies from Western ones,11 that culture can have a significant impact on the economy becomes increasingly clear. Therefore, economists should
pursue some such line of inquiry. In so doing, they might be encouraged by a possibility that suggests itself in this connection: the prestige of mainstream, neoclassical economics might derive largely from its association with American society (which was after the Second World War considered as a model for the future of all societies), an association that is largely the result of the strong individualist orientation that neoclassical economics shares with that society. If current trends continue, the economics profession may become gradually more willing to depart from a strong individualist orientation.

Notes
1. See, for example, Mead [1934]; Habermas [1984-87].
2. One might add that while no completely successful formalization of rationality is possible, it does seem fairly clear that to the degree that rationality can be formalized—and no matter how rational or irrational one thinks people are, the rationality concept must be central to one's understanding of economic processes—the utility model is the "correct" one. Mirowski's critique of neoclassical economics has always been associated with a downplaying of the rationality concept. Of course, this downplaying is related to his search for a "materialist" grounding for the theory of value in the paper under discussion.
3. And all contemporary biologists are "orthodox" neo-Darwinists, in their day-to-day work, if not in their polemical reflections about the explanatory strategies used in their discipline. Indeed, one might question the usefulness of applying the label "orthodox" here. The concept of orthodoxy suggests that a discipline is confronted with some scandalous anomaly, which is suppressed in public discussions within the discipline, if not repressed in the minds of its practitioners, and that the obstinate refusal to face this anomaly head-on is preventing progress. Few educated observers would wish to deny that Darwinism has been an immensely successful research program, or fail to concur that it does not suffer from for example what Bernard Gellner has called the problem of "eternal return" in the social sciences, that the same ideas keep getting reintroduced in different languages (e.g. as McCloskey has noted, "monopolistic competition... keeps getting reinvented [1991, p. 14]). In fact, the very term neo-Darwinism reflects the progressive nature of the discipline: the prefix serves to distinguish Darwin's own theory from the neo-Darwinian synthesis, which by combining Darwin's theory of natural selection with Mendelian genetics and the discovery that DNA is the medium of inheritance, was able to explain the mechanism of the variation required by his theory, which Darwin merely presupposed. (The analogous outcome in economics would be a scenario in which neoclassical economists were long preoccupied by the problem of how optimization is possible, finally to have Herbert Simon demonstrate how organizations are able to optimize by means of implementing certain routines.) If one actually looks at criticisms of neo-Darwinian "orthodoxy", one finds that they usually involve strategies such as the "demolishing" of straw men. For instance, one of the main faultfinders, Stephen Jay Gould, has recently written that "Darwin's vision may prevail in the here and now of immediate adaptive struggles. But if we cannot extend the small changes thereby produced into the grandeur of geological time to yield the full tree of life [so as to make what he calls the doctrine of 'extrapolationism' workable], then Darwin's domain is a limited corner of evolutionary explanation.... [I]f mass extinctions are true breaks in continuity, if the slow building of adaptation in normal times does not extend into predicted success across mass extinction boundaries, then extrapolationism fails and adaptationism succumbs" [Gould 1992, p. 53]. As Daniel Dennett has commented on this passage, there is no reason "why any adaptationist would be so foolish as to endorse anything like 'extrapolationism' in a form so 'pure' as to deny the possibility or even likelihood that mass extinction would play a major role in pruning the tree of life, as Gould puts it. It has always been obvious that the most perfect dinosaur will succumb if a comet strikes its homeland with a force hundreds of times greater than all the hydrogen bombs ever made" [Dennett 1993].
4. For different views on the proper role of the optimality assumption in evolutionary reasoning, see Dupré [1987].
5. Even if one remains at a strictly general level, biologists have something to teach economists. Thus while evolutionary biologists usually work at the level of the organism or the gene and talk about how their attributes lead to "macro-level" effects relating to reproductive success, they can switch to a lower level and talk about the molecular mechanisms giving rise to genetic variation, the embryological mechanisms which create constraints on evolution (although these are as yet not well understood), and so on, all while remaining in the same unified system of concepts. They thus possess what one may call a good theoretical map. I have argued elsewhere that economics needs such a map, but does not yet have it [Viskovatoff forthcoming].
6. Since at any rate General Motors had relatively easy access to huge amounts of capital—as is illustrated for example by its acquisition of Electronic Data Systems—another partial explanation for the relative shortsightedness of U.S. manufacturers—their relatively high cost of capital and lack of long-term, committed investors—is not very relevant in this case.
7. For instance, Kenneth Arrow has recently observed that "It is quite clear... that culture affects economic performance" [Arrow 1987, p. 237].
8. Dawkins introduced this concept mainly to show that evolutionary phenomena are the consequence of the fact
that genes are what he calls "replicators", as opposed to being somehow the result of genes being "contained" in a specific chemical medium, and that one can imagine replicators that are not based in chemistry at all. He did not mean to suggest that the analogy between cultural and biological evolution is an especially good one. In this, we can agree with him.

9. As it happens, genic selectionism will always be applicable, while there are some phenomena that one cannot understand in terms of selection of individuals. For a balanced treatment of gene selectionism versus individual or group selectionism, see Sterelny and Kitcher [1988].

10. We thus disagree with Khalil when he says that taking the "meme's-eye point of view" in biology "parallels the [individual-] reductionist view of the firm in neoclassical theory of production" [typescript, p. 15].

11. See, for example, Gellner [1992, p. 142].

References


