5

AN EVOLUTIONARY ECONOMICS?

On borrowing from evolutionary biology

The allure of an evolutionary economics

The idea of an evolutionary economics based on insights from evolutionary biology is clearly enticing to many modern economists. In the last few years especially, the number of economists attracted to it appears to be significantly on the increase (see, for example, Dopfer 2001; Dugger and Sherman 2000; Hodgson 1997, 1998c, 1999b; Laurent and Nightingale 2001; Loasby 1999; Louçã and Perlman 2000; Magnussen and Ottoson 1997; Nicita and Pagano 2001; Potts 2000; Reijnders 1997; amongst numerous others; and see Witt 2001, for an interpretive survey).

The idea is not a new one. Marshall once famously concluded that 'the Mecca of the economist lies in economic biology rather than economic dynamics' (Marshall 1961: xii). And Veblen inspired many in asking 'Why is economics not an evolutionary science?'. Furthermore the issue has been frequently examined throughout the last century, not least in Nelson and Winter's (1982) *An Evolutionary Theory of Economic Change*.

Even so, many contributors have continued to urge caution, whilst debating the merits of borrowing from biology. For example, Penrose concludes a piece on biological analogies by arguing:

> But in seeking the fundamental explanations of economic and social phenomena in human affairs the economist, and the social scientist in general, would be well advised to attack his problems directly and in their own terms rather than indirectly by imposing sweeping biological models upon them.
>
> (1952: 819)

And Schumpeter (in a passage that has been interpreted as supporting the view that economics should eschew *all* metaphors from physical and natural sciences)[1] writes:

> it may be … that certain aspects of the individual-enterprise system are correctly described as a struggle for existence, and

> that a concept of survival of the fittest in this struggle can be defined in a non-tautological manner. But if this be so, then these aspects would have to be analyzed with reference to economic facts alone and no appeal to biology would be of slightest use.
>
> (Schumpeter 1954: 789)

Others have been more obviously positive, although even amongst protagonists there is often an explicit recognition that borrowing from others, including biology, is not a panacea (e.g. Hodgson 1993). All in all, I think, the jury is still out as to whether a fruitful evolutionary economics based on principles drawn from evolutionary biology is a viable proposition.

The problem with the literature as it stands, it seems to me, is that too little progress has been made on the question of what would justify drawing on evolutionary biology (or indeed on any models first formulated outside the social domain). And the reason appears to be not so much a lack of attention to the details of evolutionary models (though there is often room for improvement here) as an insufficient attention paid to questioning the nature of the social realm to which it is intended that the evolutionary models be applied. Determining the nature of social material does matter, however. The nature of the object of study always bears implications for how it can be studied.

Though seemingly obvious, even trite, this latter insight flies in the face of much, if not most, of modern economics with its continual neglect of explicit ontological analysis. Indeed, modern economics is marked by a widespread committal of the epistemic fallacy. This consists in the view that questions about being can always be reduced to questions about our knowledge (of being), that matters of ontology can always be translated into epistemological terms. This fallacy assumes the form of an expectation that methods can be adopted from any sphere, and/or be of any kind – mathematical, evolutionary or whatever – and successfully applied irrespective of the nature of the object of study (see Lawson 1997a).

Even in the more insightful discussions bearing upon the possibility of an evolutionary economics, questions of ontology have tended to be obscured by a concentration on other matters. The latter have included such issues as whether Darwin consistently proposed one sort of evolutionary theory or mechanism only, the proper interpretation and relevance of the contributions of Lamarck, the nature of frontier modelling in modern biology, and so forth. Whilst these sorts of inquiries have their interest, they easily distract from those more relevant to the question of whether it is feasible in any useful way at all to abduct from biology into social theory.

In any case, whatever the reason for it, questions of ontology have been largely neglected in discussions of borrowing from others, and here my purpose is to help rectify this situation. It is the case that ontological considerations of some sort do already creep in here and there. However,

they rarely do so in a sufficiently explicit and systematic fashion.[2] My limited aim here, as I say, is to contribute to helping redress this situation.

Perhaps it is useful if I anticipate at this point the conclusion I reach below on the worth of borrowing from biology. First let me say that by the term evolutionary I mean not any type of change, but the genealogical connection of all organisms along with an account of life and society regulated by descent with modification (essentially cumulative causation). Thus on my understanding, natural selection is but one evolutionary mechanism. However, by the term evolutionary many economists do seem to mean processes of natural selection. With this in mind, the specific thesis defended in this chapter, and developed on the basis of ontological reasoning, is that there is no legitimate basis for an evolutionary economics as such if

(i) the term evolutionary (in 'evolutionary economics') is interpreted (as economists interested in borrowing from evolutionary biology tend to interpret the term) as denoting a process that in some way conforms to the natural selection model that derives from (Darwinian) evolutionary biology, *and* if
(ii) the phrase 'evolutionary economics' is intended to signal a universal approach to economic analysis (implying that all economic phenomena can be treated as resulting from evolutionary [natural selection] processes).

Rather my thesis is simply that the social world is such that certain social phenomena *can* result from evolutionary processes of this sort, specifically from processes that manifest evolutionary natural selection aspects. Where this is so, an evolutionary explanation of the type in question, in part at least, is clearly called for. But this particular socio-evolutionary model ought not to be universalised *a priori*. Even Darwin thought that natural selection was but one mechanism amongst many regulating life on earth, albeit, in his view, the most important one. Thus in the final edition of *The Origin of Species*, Darwin (1872) writes:

> But as my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work and subsequently, I placed in a most conspicuous position – namely at the close of the Introduction – the following words: 'I am convinced that natural selection has been the main but not the exclusive means of modification'. This has been of no avail. Great is the power of steady misrepresentation.
>
> (Darwin 1872: 421)

As we shall see, in the social realm there is even greater reason to adopt such an open or pluralistic orientation in explanatory endeavour.

In truth, to insist on an 'evolutionary economics' modelled on the natural selection paradigm prior even to identifying the phenomenon to be understood and/or explained is ultimately no better than the modern mainstream's *a priori* insistence upon a deductivist or formalistic economics, that all phenomena be addressed using closed-systems deductivist modelling. The relevance of this particular evolutionary model, as with all other methods or epistemological principles, can be determined only *a posteriori*. And the evidence is that the domain of relevance of this evolutionary model within the social realm is certainly not unbounded. Let me now briefly run through the argument.

The biological and social connection

As I say, recent years have witnessed something of a surge of interest in (the possibility at least of) borrowing from biology. But what explains the phenomenon that some, especially (but not exclusively) heterodox economists appear so optimistic about gaining insight from biological writing, and in particular from theories of biological evolution? Much of the appeal seems intuitive. Certainly, I do not find this optimism well articulated even amongst the best of economic commentators. Although the formulation I proposed above, turning on the matching of evolutionary model or method to social ontological insight, seems simple enough, even the better parts of the literature do not always recognise that the relevant matter to be determined is indeed whether biological achievements provide a useful model for the social realm, given the latter's nature.

Actually, a study of the relevant literature reveals that very frequently several different lines of reasoning are run together. There is, in reality, not one type of connection of the biological to the social but three:

1 the biological as an *existential basis* for social phenomena;
2 the biological in *causal interaction* with social phenomena; and
3 the biological as the source of a *model* for the understanding of social phenomena.

Our understanding of capable human behaviour at any level requires an understanding of biological/social connections along the lines of types 1 and 2. But our primary concern here is actually with connections of type 3. The problem with much of the existing literature, it seems to me, is that these different forms of connection are rarely distinguished, with the consequence that support for type 3 is sometimes thought to be achieved by emphasising connections along the lines of type 1 and/or type 2. This is

invalid. After all, whilst social phenomena and processes are, or include, an emergent surplus from the interactions of human beings (as opposed to being reducible to human beings themselves), it is the case that the physical realm (just like the biological) also provides an existential basis for, and exists in causal interaction with, social phenomena. Certainly if it is thought that connections of types 1 and 2 justify those of type 3, some argumentation is required. This, so far, is noticeably missing.

Evolutionary theory and metaphor

In order to assess the relevance of biological models for understanding social phenomena it is necessary to examine the nature of both social and biological modes of determination explicitly and in some detail. Before doing so, however, it is useful to consider the nature and role of metaphor. Discussions of borrowing from evolutionary biology are, as I say, usually couched in terms of the natural selection metaphor, and I think it is necessary, before going further, to unpack how such applications relate to the current discussion.

Economists' discussions of metaphors, as with the practice of borrowing from other domains more generally, often include a good deal of suspect, if not clearly fallacious, reasoning. I am aware of economists arguing that if we borrow from biology we must take the latest aspect of that theory just because it is the latest. The same is often said to be the case if we borrow from physics. In order for economics to thrive, the claim runs, it is essential to copy from the cutting edge of the hard sciences.

This attitude reflects an error I shall term the *abductionist fallacy*. This is the notion that insights, methods, or theories of one domain of science or human reasoning, let me call the latter the source domain, can be abducted into another, the target domain, without prior consideration of the nature of the latter. The basic fact of the matter is that a particular theory of physics or biology or whatever, even if cutting-edge stuff, has no clear relevance for the social domain if it presupposes a type of material or configuration that is entirely absent from the social realm. If, for example, certain regions of the social realm are not in any way atomistic then no matter how hard-nosed may be recent developments in atomic physics, they have no obvious automatic bearing on the regions of the social domain in question.[3]

For a metaphor or other form of abduction to be recognised as appropriate, something must be known about the target domain. This much is clear. And if the use of a metaphor is to prove successful as a means of illuminating the target domain it must generate new lines of analogous, and other forms of, reasoning in this domain. It will be expected, then, that once the categories in question have been abducted, they will take on their own meanings in the new context, i.e. meanings that are not wholly the same as those they carried in the source domain.

How exactly does metaphor work? I think we now know enough about the primary workings of metaphor (see e.g. Boyd 1993; Lewis 1996; 2000b; Soskice 1985; Soskice and Harré 1982) to appreciate how they facilitate understanding and knowledge development. They do so, in essence, by making connections between two domains which hitherto may not have been recognised as having parallels. And they do so, in effect, by way of revealing that an object or feature in the source domain (the vehicle of the metaphor) and an object or feature in the target domain (the tenor) are both tokens of the same type, or each a concretisation of the same more abstract object.[4]

If, for example, we say John is a pig, we are suggesting there is a more general or abstract class of objects of which John and a pig are both tokens or particular sub-types. In this example the class may be of all creatures disposed to eating in a particular fashion. If we say that Jane is a donkey, we may in fact be meaning to suggest that Jane, like the donkey, is a special case of objects that are slow moving.

In these brief illustrations I use the qualifier 'may' in giving the noted interpretations just because the exact meaning will depend on context. The person formulating the metaphor for Jane may be wishing to imply not that Jane is slow like a donkey but stubborn in the manner that donkeys very often are. When John is described as a pig it may be because he has been sun-bathing and is (like certain varieties of pig at least) pink all over (although for this, references to strawberries and lobsters seem more common). In this particular case the relevant type-class is all pink objects. Use of metaphor capturing one token of this type, i.e. a pink object, signals that the tenor or target object of this metaphor is also a token of this type i.e. is something that is pink. The particular nature of intended abstract conception or type, though, is something which can be determined only from context.

Often, of course, the context will be a general one, so that a wide body of people can interpret the metaphor in the manner its formulator intended. If, for example, it is said that trading is stagnant the general class is presumably that of all things where movement of activity is feasible but hardly happening. If it is said that prices have reached their ceiling the general class is presumably anything that has an upper limit and has reached it.

Metaphor works, then, by connecting objects or aspects, etc., previously regarded as unconnected, by showing them both to be special cases of the same general thing, to be tokens of the same type.

In making this connection, metaphor can indicate a possible model for the target object based on the object in the source domain. It allows us to set up a generic system, using insights from the source domain, which possesses the potential to provide lines of development in the target domain.

If we turn to the category of evolution, specifically, the first thing we can note is that were we to understand evolution quite generally as a form of change or development (as some do seem to) it need not be a metaphor for social processes at all. On this conception social processes simply are evolutionary.[5]

Rather, biology becomes a more interesting source of ideas or resources, including metaphors, once we contemplate the suggestion that some social processes are evolutionary in a more particular sense, and specifically in accord with the idea of natural selection. Here, as we will see in due course, the idea of natural selection *is* a metaphor. The implicit intuition or hypothesis is that certain 'natural selection' mechanisms in the source or biological domain, and aspects of processes in the target or social domain, are indeed both tokens of the same more abstract type.

But here we are jumping ahead of ourselves. My purpose with this slight detour was merely to clarify the role played by metaphor (or what I take to be its primary role). Metaphor as with any form of abduction from one realm to another requires appropriate conditions. As indicated earlier, in order to borrow usefully from biology in social theory, we need to ensure sufficient commonalities between biological and social material. Our discussion of metaphor enables us to reinterpret that need as one for a general model of which there are both social and biological sub-types or tokens.

Advantages of the evolutionary model for social understanding: a preliminary orientation

Now, in embarking on the task of identifying relevant commonalities between the nature of biological and social materials, if any, we do not start from a position of complete ignorance of course. One very obvious (if rarely explicitly elaborated) reason for the prevailing optimism that the study of the biological realm can provide insights of relevance for analysing the social, is that both worlds comprise open (i.e. highly unpredictable) and dynamic systems. In other words, there is a very general class of systems, namely those that are open and dynamic, of which the social and the biological are both immediately recognised as tokens. Let me elaborate.

The nature of social material

Certainly the social world can be recognised as an open, dynamic process. Indeed, according to the transformational model of social activity which I have defended at length elsewhere (Lawson 1997a: esp. chs 12 and 13; and see Chapter 2 above), social reality is found to be not only open and

dynamic or processual, but an emergent realm, dependent upon, but irreducible to, transformative human agency, and comprising material that is structured and highly internally related, amongst other things.

More specifically, according to the transformational model, human agency, practice and social structures (including social rules, relationships, positions, etc.) are interdependent, but ontologically distinct, types of things. The social world turns on practice. In acting we both draw upon structures given to us, and contribute to reproducing and/or transforming them. Just as we usually do not acknowledge the structures we draw upon, so their reproduction or transformation is often unintended. Thus we usually speak with a purpose in mind, which is typically to convey a thought or message to someone. But the rules of grammar we draw upon are unacknowledged, and their reproduction, depending as it does upon our collective speech acts, is usually unintended.

How do social structure and agency interconnect in this transformative process? Key categories here are social positions and social relations, especially internal ones. Two objects are said to be internally related when they are what they are, and do what they do, in virtue of this relation in which they stand to one another. Examples include teacher and student, employer and employee, landlord and tenant, etc.

Now it is typically not individuals *per se* that are internally related but the positions in which they stand. The crew members of a passenger airplane have a range of duties and perks, etc. But they are not attached to the crew members personally. If one resigned and a second person were to take their place the second person would acquire access to the same positioned obligations and so forth as the first possessed. The same is even more clearly true of the passengers. As passengers they have rights and obligations. But as soon as others take their place these rights and obligations, etc., transfer. They do so because they are attached to positions the passengers occupy. All of us, choose, or (perhaps more typically) are allocated to, a multitude of positions (teacher, student, employer, employee, parent, child, European, Asian, old, young, male, female, salesperson, customer) each associated with a range of rule-conditioned obligations, rights, duties and prerogatives, etc., and related to other positions to which our practices are oriented. It is in virtue of our being slotted into social positions that we access social structures, and through acting according to position-related rights, obligations and interests that the social world is continually reproduced and/or transformed.

On this conception, then, the social world emerges as an interrelated network of dynamic totalities, of internally related processes. Practice, as I say, is the key to social being. Social structure depends on human agency, and it is through human practice that specific structures are continually reproduced and transformed. This inherently dynamic and

totalising human agency-dependent process, wherein social structure is both condition and consequence of action, I repeat, is the 'transformational model of social activity'. The central point for the moment, though, is that the social system is found to be intrinsically dynamic and open.

The biological model and mainstream economics

The same, of course, can be said of the biological realm. Indeed, a factor that spurred the development of evolutionary biology was precisely an acknowledgement that the biological realm too comprises dynamic and open systems. Such an assessment did not always prevail.[6] But the view that life on earth is a continuous process of transformation is by now sufficiently widely accepted that I shall not defend it here. Rather, my point is simply that the recognised successes of biological evolutionary models in addressing open and dynamic systems gives some immediate credibility to the idea that biological models can prove of relevance in some way in facilitating the analysis of social phenomena.

Certainly biological models are seen to have an immediate *prima facie* advantage over the competing mechanistic models of modern mainstream economics. For the latter are concerned with basically static or stationary scenarios. At best, these mechanistic models conceptualise change as exogenous shocks to systems (albeit to systems which respond by tending to re-equilibrate), or some such. Mostly, modern mainstream economics concerns itself with identifying positions and set-ups in which agents lack any incentive to change what they do. In comparison, as I say, evolutionary theory was developed to explain an intrinsically dynamic order, to account for processes of relatively continuous change. It is an explanatory theory with a potential purchase on any system recognised as being fundamentally open to the future.

If I appear to be labouring the point here, it is because these parallels between the two spheres, the social and the biological, seem to be less than always fully recognised. Rosenberg, for example, concludes 'that Darwinian theory is a remarkably inappropriate model, metaphor, inspiration, or theoretical framework for economic theory' (1994b: 384). And his reservations boil down basically to one: evolutionary models in biology do not predict well:

> My pessimistic conclusions reflect a concern shared with economists who have sought comfort or inspiration from biological theory. The concern is to vindicate received theory or to underwrite new theory against a reasonable standard of predictive success. Few of these economists have noticed what the oppo-

> nents of such a standard for economic theory have seen, that evolutionary theory is itself bereft of strong predictive power.
>
> (Rosenberg 1994b: 384)

Once we take a serious look at the nature of social material, however, we can see that evolutionary theory's lack of predictive power is no objection at all. Successful prediction presupposes closure, whereas the social system is found to be, like the biological realm, open and seemingly insusceptible to many, if any, scientifically interesting local closures, at least of the causal sequence sort. If the nature of the social realm is such that the successful prediction of social outcomes is unlikely, then to adopt methods premised on the necessity of achieving predictive accuracy is to abandon or ignore insights from ontology, to commit the epistemic fallacy. *Prima facie* the biological model gives *a posteriori* grounds for hope just because the social system is an open, mostly non-teleological, system of the sort with which evolutionary methods can in principle cope. In fact, I suspect it is this particular feature of the biological model that, implicitly at least, accounts for its current attractiveness to heterodox economists.

Natural selection

But this shared concern of the two sciences with open dynamic systems cannot be all of the story if the biological evolutionary model is to prove useful to social science. After all, we already have the transformational model of social activity in modern economics and social theory more widely. Biological evolutionary theory must provide something more if it is to enable social theorising to go further in some context.

As already signalled, that 'something more' with which many economists are interested appears to be bound up with the metaphor of natural selection. In economics there are already plenty of contributions claiming to show how order of sorts in society could come about (solely) by way of conscious (human) intervention or design. Biology deals with situations that equally are ordered in some sense, but where the form of order in question has not been brought about intentionally, i.e. by conscious design. This is a radical achievement undermining the idea of a benevolent prior design in history. And it is this insight, I suspect, that provides the relevant motivation for, and context of, seeking to apply the biological evolutionary metaphor in modern economics.

Actually this is not quite correct. I detect two motivations (at least) to the quest for borrowing from others. The first, the one I am myself interested in here, is a desire to understand and explain social reality, to be realistic, to seek for truth. But I cannot deny (and indeed have already acknowledged) that some contributors give a higher priority to drawing on the theories and practices of cutting-edge naturalistic sciences just

because they are revered for being more naturalistic and/or 'cutting edge'. I return to this motivation towards the end of the chapter. For the time being I concentrate on the issues before us, accepting that the primary goal is social understanding including explanation. This, of course, is something I take largely for granted in arguing for an ontological turn throughout this book.

To return to the central argument, I am concerned here specifically with the relevance to the social realm of the specific Darwinian model of 'natural selection'. Although the task of demonstrating that the characteristics of openness and dynamics are common to biological and social domains is fairly straightforward, that of determining whether insights systematised as the natural selection mechanism in evolutionary biology have parallels in the social realm requires a good deal more work.

A clear route to addressing such matters is to retroduce an appropriate (see below) general model of which the biological natural selection conception can be seen as a token, a general model which can in due course be examined for its applicability to social phenomena. In truth, however, there is no need to defer to a general model at all (even if this is the way metaphor works). All that is necessary is that the essential components are distilled from any biological natural selection example. The question I need to pose is merely whether these essential elements carry over to the socio-economic realm. However, by viewing them as features of a general model of which any biological example is viewed as a token, there is, I believe, less scope for confusion as to what is going on. In any case this will be my strategy here. I will consider a biological example, distil out the components essential to a natural selection mechanism and interpret these as features of a general model of which the specific (biological) example considered is a token. I will examine whether economic tokens of it are also feasible.

As a first step on this path, then, let me very briefly now consider a particular example of the biological model. I know that such examples are familiar enough to many modern economists, especially those working in the (old) institutionalist tradition. But I go through one here anyway partly for completeness, partly to convey my understanding of a natural selection process (there are of course competing understandings and emphases) and partly to keep the discussion focused not only on abstract models but also real-world processes.[7]

A biological example: the beaks of Darwin's finches

A well documented example that will serve my purposes concerns 'Darwin's finches' (so called because they were originally studied by Darwin), a group of finches inhabiting the relatively isolated Galápagos islands (visited by Darwin for five weeks during his voyage on HMS

Beagle). I focus, in particular, on a relatively recent episode in the process of evolution in sizes of their beaks.

The evolutionary episode in question took place on the specific island of Daphne Major between 1973 and 1978. It is an episode that has been documented by various scientific observers living on the island at the time (see e.g. Weiner 1994). Conditions were such that just about all the finches on the island were individually 'known' to these observers.

In the first four years of this period, fairly lush environmental conditions prevailed on Daphne Major. In particular, the rains fell in the early part of each year allowing seed-bearing plants to grow and attendant insects to flourish. There was thus a plentiful supply of food for birds produced in the early part of each year, and most of the finches were observed to survive the remainder of the year, whatever the conditions.[8] However, after the first week of January 1977 little rain fell for the rest of that year. Throughout this period the total mass of seeds on the island declined, and the average size and hardness of the seeds that remained uneaten increased steadily.[9] Hundreds of finches died.[10] Notably, those finches which survived were the bigger-beaked birds capable of cracking open the larger harder seeds that remained.[11] Mostly these were males, the average female beak (and body) having been smaller. In any case, following the drought the birds which survived and were able to mate were those birds which were distinguished within the original population by having larger beaks. And subsequently the offspring of the survivors were found also to possess big or deep beaks, typically about 4 or 5 per cent larger than those of their ancestors in the population of a few years earlier.[12]

In short, the result was evolution by way of a process of natural selection. The period saw a shift in the environment that 'favoured' (in a relative sense) those finches with larger beaks. Larger beaked finches survived the environmental shift, and because their offspring inherited their parents' (larger) beak size, an evolutionary change was observed in the space of just a few years.[13]

Towards a general evolutionary model

So what are the essential features of the natural selection story here? What components are essential to this biological explanation of the mechanism of change or evolution via natural selection? Alternatively put, which abstract model(s) lies behind and systematises this specific illustration and others like it? What is essential to any (class of) model(s) for which the natural selection process whereby the beaks of finches evolved is a token example? Clearly there are several essential components to include.

A first feature of the explanation to retain is that it deals, at some level, with a *population* of individuals of a particular type (finches) and an aspect of the finch's environment or situation (food). Notice that the latter is indeed only an aspect. Finches need water, air, warmth and a host of other factors to survive. The environmental factor of selective causal influence here is food in the form of seeds in a context where substitutes are hard to come by.

The existence of *variety* of some sort within the relevant population is a second essential feature of this form of explanation. In the case of Darwin's finches the variety included size of beak. For, trivially, in order that finches with larger-than-average beaks were able to perform in a relatively successful manner, there clearly had to be finches with larger-than-average beaks present in the original population. Notice, then, that for a natural selection evolution story, the individuals of the population possess both traits that are essential to their qualifying as members of the relevant populations, and traits which differentiate them within that population.

A third essential feature of the explanation is that a mechanism of *reproduction* (or replication or inheritance) is included as part of the explanation. The story told could not count as an evolutionary one, as an explanation of the rise to prominence of the bigger-beaked finch in successive generations, if size of beak was something that the finches did not reproduce through their offspring. Relatedly, we have a conception of lineage here, a spatio-temporal sequence of entities in which later ones are in some sense descended from, and causally produced by, earlier ones.[14] Although over the period in question earlier generations of finches on the island had smaller beaks on average than later ones, they were still finches. There is a sense in which the bigger-beaked finches evolved out of, and constituted an evolution of, finches.

The specification of a mechanism whereby there is interaction between individual and environment is a fourth essential feature of the explanation. This is a mechanism whereby certain members of the population (with specific features) are *selected*. Following Hull (1981), I shall use the term *interactors* to refer to the entities in which interaction between the environment and the individual occurs. This will typically be a different entity to that which passes on its structure in replication. The latter, following Dawkins (1976; 1978), I refer to as a *replicator*. If a mechanism of gene replication is responsible for the reproduction or replication of finches with certain features, this is insufficient to explain the rise to prominence of birds with that gene. The interaction of the whole organism with its environment is an essential feature of the causal evolutionary process. In particular, the need for the finch to eat (in an environment of seeds as food) is an essential aspect of the story. (In biology the genetic constitution of the replicating individual is referred to as the genotype, the nature of the individual or organism the phenotype.)

A fifth relevant feature of the explanatory sketch is the fact of a degree of independence between the process whereby the variety of traits is produced and the manner in which the environmental mechanism doing the selecting has come about. Such independence is essential if the model is to explain the appearance of order or 'fit' (of beak size and seed size) in the absence of design. Otherwise there is nothing that necessarily distinguishes the explanatory schema from any other as found, for example, in modern economics. Specifically, without independence it can be argued that either trait or environment is produced in order to match the other, so that the puzzle of order in the absence of design is not, after all, addressed.

Notice further that evolutionary change in line with natural selection can come about over a period of time because either, first, a new trait emerges within a relevant population, one that is found to be favoured by the existing environment, or, second, the environment shifts in a way such as to favour a trait that has long been in existence (or through a combination of these two types of development). In each such scenario, however, the *possibilities* turn on the processes generating the traits or variety, and the contribution of the environment lies in its 'selecting' amongst the particular set of features in evidence.

The PVRS model

Given that these identified elements are each essential to a natural selection story (such as illustrated by the evolving size of the beaks of Darwin's finches), all will be part of any abstract model of which the biological natural selection model can, *qua* natural selection model, be viewed as a manifestation or token.

Let me refer to any model that contains these components as a Population-Variety-Reproduction-Selection, or a PVRS, model or system. We must keep in mind that for a process captured by a PVRS model to be one of natural selection, V (variety generation) and S (selection) conditions must be to a significant extent independent. The question is, how independent? In particular, should these conditions be strictly independent of each other, or is something weaker sufficient?

To clarify matters, let me briefly consider three versions of the PVRS model distinguished according to the manner of the relation, if any, between the conditions of variety generation and those of environmental selection.

A PVRS model with variety and selection conditions strictly independent

Consider first a PVRS model in which the mechanisms influencing the variety of traits (V) and selection conditions or mechanisms (S) are strictly

independent. This can be termed the polar, or neo-, or strict Darwinian version of the model.[15] This is the form of PVRS model often thought to have most relevance in modern biology.

In the case of Darwin's finches, the presumption is that the conditions which select out the bigger-beaked finches (the availability of food only in the form of difficult-to-open seeds) are strictly independent of the (genetic mutation) mechanisms bearing on the process whereby a finch with a larger beak first emerged.

The advantage of distinguishing this version of the PVRS model is that it illustrates rather clearly that, and how, a mechanism, the natural selection mechanism, can bring about the appearance of order even in the complete absence of conscious design.

A feed-backward PVRS model

Second, we can distinguish a PVRS model that allows S to feed back to, or causally influence, V. Let me refer to this version of the PVRS model as a feed-backward or S-to-V model. A biological token of this feed-backward version is the Lamarckian model,[16] a conception which (according to the manner in which it is most commonly interpreted) allows the inheritance of acquired characteristics. It proposes that acquisitions or losses, wrought through the influence of the environment, can feed back into the evolutionary process, being capable, in certain circumstances of being preserved in the 'species' (or whatever) through reproduction.

To claim Lamarckian features for the evolutionary development of Darwin's finches, would be to suppose the finches somehow acquired the advantageous feature of a larger beak directly in the process of interacting with their environment, and also somehow passed this characteristic on to its offspring.[17]

A feed-forward PVRS model

Finally we can distinguish a PVRS model which allows V to feed forward and causally affect S. Let me refer to this as a feed-forward or V-to-S version of the PVRS model.

To suppose such a model has relevance to the example of Darwin's finches would be to maintain the mutation conditions giving rise to the larger beaks somehow affected the environment of selection, i.e. the nature, or causes, of the finches' food.

As the example of the beaks of Darwin's finches perhaps illustrates, the feed-backward and feed-forward forms of the PVRS model may have comparatively little application relative to the neo-Darwinian version in the biological realm. Or at least this was conceivably so prior to human

intervention. With human manipulation via genetic modification we certainly find scope for the feed-backward model. And with the intervention of humans to ensure an environment prevails in which a particular desired variety of some species thrives, the feed-forward model also has some force.

But in other realms of the biological world, the polar or neo-Darwinian model is often thought to have most relevance. Variety and selection conditions, as in the case of Darwin's finches, are frequently found to be more or less strictly independent.

The natural selection mechanism

So with which version of the PVRS model are we concerned here? Notice that although in the case of the Darwin's finches V and S conditions appear strictly independent, the degree or extent to which the strict or polar Darwinian model holds in biology is actually contested. More to the point, all we need here are the insights that relate to the natural selection mechanism. Clearly, the strict Darwinian version best illustrates the workings of the natural selection mechanism. However, the openness of the social system means that even where a Darwinian mechanism is operative it is likely to be but one mechanism amongst many affecting the outcome. Remember we are motivated here by the recognition that in an open changing world, mechanisms can exist (and in the biological realm clearly do exist) that ensure the appearance of order, the matching of part and whole, of individual and environment, even when this outcome is not the product of conscious design. All we need consider here is whether there are 'natural selection' tendencies of this sort at play in the social realm. It is no more necessary that natural selection tendencies be the whole story on any occasion of change or persistence in the social realm than in the biological realm. The question is whether such an evolutionary mechanism is ever in play, whether there exists a tendency for certain selection conditions which are broadly independent of variety-producing mechanisms to bear in any significant way upon the (sorts of) individuals of the population which come (via replication or reproduction) to dominate. The most relevant, or potentially useful version of the PVRS model to investigate further, then, just is any version in which the V and S conditions are at least relatively independent. So the strict or polar or neo-Darwinian version of the PVRS model qualifies as a special case.

Back to social processes

The task awaiting us at this point is precisely to determine whether the biological evolutionary model as conceived here does, or could, have

relevance to social analysis. If we accept that the PVRS model with S and V largely independent, expresses a process which generalises the biological evolutionary model of natural selection in a manner that captures its essential features, we need now to assess whether there is any way in which it is able to be concretised usefully, i.e. be given a meaningful specific interpretation, in the social domain.

In fact, we can again be more definite here. Having accepted the (already elaborated) transformational model of social activity as capturing essential features of social reality, the specific questions we need to address are

(i) how, if at all, does this version of the PVRS model tie in with the transformational model?; and (supposing that it does tie in)
(ii) what might this PVRS model achieve that the transformational model does not already?

The PVRS evolutionary model as a transformational model of social activity

Although the relevance of the latter question presupposes a positive answer to the former, it is nevertheless possible here to address it first. For it is already clear that *if* a PVRS model can add anything to social analysis whilst remaining consistent with the transformational model, this is because the latter, more or less by 'design', is sufficiently abstract as to encompass all (so far) observed aspects of social reproduction/transformation. The latter was determined in the course of developing a general social ontology. But if this transformational model allows that both transformation and reproduction occur, it says little about either the conditions wherein one or the other is likely to dominate, or the specifics of any processes of social reproduction/transformation. The 'natural selection' model then, if appropriate at all, will presumably indicate one specification of the transformational model. It will provide a more concrete account of how reproduction and/or transformation of specific aspects of social structure can happen.

The more fundamental question here, though, is the former one. Given what we already know about social reality, as expressed in particular by the transformational conception elaborated above, does the PVRS model, constrained to conform to the transformational model, carry the potential to illuminate the social realm at all?

As an initial orienting strategy, let me briefly recall the sort of model we are seeking. For a social-evolutionary story we require, at a minimum, some conception of a population of social individuals each with traits rendering them members of that population; a variety generation process giving rise to additional traits which differentiate members within the

population; a notion of a relevant environment; a mechanism whereby individuals with various differentiated aspects are, or can be, reproduced or replicated; a mechanism whereby individuals of the population interact with their environment with different degrees of success; and an account of the process as a whole that conforms to the transformational model of social activity.

Consider, first, some likely candidates for social interactors (the social entities that interact with their environments). What sorts of individuals, if any, are various in their aspects, compete with others in social life, and ultimately are selected by the environment in which they occur? In particular, what answer to this question can be formulated that is in keeping with the transformational model of social activity? Of course, there may be many social scientific tokens. Any evolutionary framework developed here is very unlikely to be unique. Even so, it seems to me that a certain category of social phenomena does stand out more than others as a promising candidate for the set of social interactors we are looking for here. I refer to *social practices*.

Consider, for example, language use, including speech acts. Think of an international conference of academics. Although numerous languages will be spoken by the participants, especially in local restaurants and other off-conference meeting places, in the conference lecture room the practice of speaking English invariably comes to dominate (at least currently). Some participants from countries where English is not the first language, often try valiantly to get the discussion going in their own language, or in a different one to English, in some of the attendant seminars or even the main forum. But for various reasons, including, usually, the sheer number of native English speakers, and because non-native English speakers tend to speak English well, and native English speakers tend to speak other languages very poorly, the practice of speaking in English usually comes to dominate.

Notice that it is specific practices (in this case speaking in various languages) that are the individuals in competition here, not the human individuals *per se*. In my experience, the participants whose first language is not English are often in the majority at both international conferences, as well as in the power echelons of the economics profession, including in the UK and North America. It is not individuals *per se* whose first language is other than English that are squeezed out but the practice of speaking a language other than English in the public forum.

Of course, many young scholars going to an international conference for the first time will recognise this situation, and may, if their first language is not English or if their English is poor, perhaps take actions to acquire competency in speaking it. As such there is likely to be a feedbackwards aspect to the process. But in the main, language-speaking

competencies are acquired independently of the constraining influences of international academic conference practices. Or at least, to the extent that this is so, the development of language practices in these conferences might be interpreted as conforming to the evolutionary 'natural selection' model.[18]

I think this example also indicates a further likely aspect of the most promising social-evolutionary framework of the relevant sort: that the environment of social selection will usually include, and often perhaps consist mainly in, the sum total of other related, including competing, social practices.

In keeping with the transformational model, then, I would suggest that the most promising, or anyway one conceivable, candidate for the social interactor is social practice, and the environment of selection includes all other social practices that are in some way related or connected to that population of practices that constitutes our primary focus. Interaction with the environment just is human interaction.

What sort of thing or aspect might be interpreted as a social replicator, the entity that passes on its structure in replication? The answer that fits most easily with the transformational conception of social activity, I suggest, is social structure, and especially social rules including norms and conventions.

Think again of the practice of language use, say of French. This is governed by the structure of rules, etc., that make up the French langauge. These govern (though do not determine) speech acts. They are also reproduced just through people speaking French. In a manner characteristic of many social practices, this drawing on rules of language in speech acts is sooner or later performed habitually.

Consider as a further example the (methodological) practices which dominate the modern economics discipline. Elsewhere (see Chapter 10) I argue that the twentieth-century rise to prominence of the practices of formalistic-deductivist economics warrants a social-evolutionary explanation along the lines of the PVRS model. Attempts to mathematise the discipline had been in place long before mathematical economics rose to dominance. The interactor here is the practice of attempting to render the study of social phenomena mathematical. The replicator just is the belief or (as some view it) 'convention' or 'cultural norm' (see Chapter 10) that mathematics is a fundamental component of all science and serious study. The rise to prominence of mathematical economics did not reflect any obvious breakthrough in terms of its relative explanatory performance (compared to that of other approaches). Rather, it reflected a shift in the environment of academic practices more widely. Prior to the period in question, realisticness was a goal of all mathematical enquiry. With the turn of the twentieth century, mathematics became disconnected from models of sciences like physics (the mechanics model). Indeed, the need for any kind

of interpretative orientation to mathematics became much reduced as the idea of 'mathematics for its own sake' became widely accepted. This removed the constraint (earlier strongly felt by Walras and others) that mathematical models in economics needed to be realistic, and allowed the mathematisation tendency in economics fuller sway. This change in the academic environment removed factors that previously were selecting against scientific research practices underpinned by the belief or convention that mathematics is an essential component. The shift in question thus allowed the latter mathematical practices more scope to become pervasive and even (as it eventually turned out) dominant. Of course, this evolutionary mechanism was never the whole story (and others are discussed in Chapter 10), but it appears to have been an important element of the relevant historical process nonetheless.

The above example, explored in some depth in Chapter 10, focuses on the mathematising tendency in economics as one set of practices within the population of all research practices. Because the explanatory puzzle is the varying fortunes, and indeed survival, of the mathematising tendency (whatever its form), the emphasis there is on environmental selection.

I might note, however, that if we narrow our perspective, the wide-ranging set of mathematising practices within economics can be construed as a population in its own right. And if we were to focus on this population as a candidate example of a selection process in the social realm, the interest would likely fall more on processes of incremental adaption, i.e. on the question of how this project has evolved over time. The likely or candidate replicators now appear to be (or to include) the enduring or core concepts, theories or methods (e.g. supply and demand analysis, general equilibrium, econometric method) underpinning the array of competing substantive contributions that are manifest. Such a hypothesis, though, is not something I shall explore further here.[19]

Disanalogies between evolutionary biology and evolutionary social science

If the above considerations are suggestive of the possibility that, and perhaps indicative as to how, the natural selection PVRS model may be concretised in the social domain, the transformational model is also especially useful as an aid to identifying some significant disanalogies between the biological and social realms.

Most obviously, it is only through the medium of human agency that variation is produced in the social realm, and that reproduction/replication and selection occur. Aspects of social structure may be reproduced by (collections of) individuals both over time, through these individuals repeatedly drawing on them (and adopting practises which presuppose and indeed manifest them), and also across people at a point of time,

through (possibly sub-conscious) imitation, and so forth. Social systems are neither naturally reproduced nor self-reproducing. Rather such reproduction or transformation as occurs is the result of capable human beings purposefully going about their daily lives and tasks, interpreting themselves, their purposes and the social order in very definite ways, and continually interacting with (including copying) others. Although much of what occurs is unintended and perhaps misunderstood, intentionality is far more significant in the social than the natural domains. Human intentional activity is always the medium of both social reproduction and transformation.

The distinctiveness of the natural selection or biological evolutionary model

A further obvious difference between the social and the biological is that, whatever may be the precise relation between variety generation (V) and selection conditions (S) in the biological realm, these sets of conditions are likely more often to be interdependent or connected in the social (if and where they occur in the social realm at all). We need only think of the impact of market research, or indeed of almost any form of forward thinking or planning, to recognise that feed-backward linkages will have some force in the social domain, that selection conditions affect the variety produced. And we need only think of advertising, and then any form of persuasion, including use of power relations, to recognise the relevance of the feed-forward model in the social domain, to see how variety generation conditions can come to affect the environment of selection.

Indeed, and as I have already emphasised, it is precisely because of this contrast between conditions in the biological and those in social domains that reflection on the biological realm, and specifically the natural selection model, proves so useful to social explanation. For such reflection helps clarify the nature of a mechanism whereby order can be produced, whereby a matching of individual and environment (or part and whole) can emerge, that is not at all the result of conscious design.

Of course, just as social processes will rarely conform to the strict or neo-Darwinian model (which would mean that human practices were entirely autonomous of human intentionality – though see the discussion on 'memes' below) so they will not be purely or strictly Lamarckian or backwards-determining (the functionalist-deterministic mistake of modern mainstream economics) nor conforming to a polar feed-forward or forwards determining model (the voluntarist or perhaps environment-as-putty model).

Nor should we expect that, if and where evolutionary features of a social process are identified, they are bound, or necessarily likely, to persist. Even where V and S conditions exist in a social process and are

found at some point to be to a degree independent, it cannot be presumed that one and the same relation between V and S must hold throughout. That is, although environmental selection may have made a difference to the structure of the population over a period of space-time, this in itself is no guarantee that such a selection process will continue. Indeed, the past effects of any such mechanism in the social realm will likely provide a spur to power struggles, or to developments in technology, etc., designed purely to bring such a process under increased conscious control. It all depends on the situation.

Such considerations, then, lead us to anticipate that the natural selection biological model may well prove useful to social analysis. But if so, we can also anticipate that anything which can reasonably count as social-evolutionary explanation of the relevant (natural selection) sort will typically identify modes of interaction or influence between conditions of variety generation and of selection that are only relatively independent. In order to understand a social process adequately it will likely also be necessary to identify patterns of accommodation and rejection, of harmonious reinforcement and tension, between 'individuals' and the environment. The strict separation of both modes of replication and interaction, and modes of mutation and selection, often thought to characterise the biological realm, give way, in the social realm, to processes of greater or more obvious causal interdependency and interpenetration. Indeed, in the end the contrast between the evolutionary and many other social explanatory scenarios may be one largely of degree rather than kind.

Evolutionary explanation as a limited epistemological case

Having, I think, found qualified support for the thesis that borrowing from evolutionary biology carries the potential to be of some use in the social realm, depending on context, I want (like many before me, if not necessarily for the same reasons) also to sound a note of caution. For if there is reason for supposing that successful social-evolutionary explanation remains always a possibility, depending on context, the transformational model which helps us properly to see this also indicates that the evolutionary model is unlikely ever to be the whole story. It may even be a rather small aspect of the total picture.

A fuller story (at the relevant level of abstraction) is provided precisely by the realist transformational model of social activity. Its scope of coverage includes merely developmental (including wholly planned) forms of change, forms of emergence, acts of whim, and so on. Indeed it includes forms of change where, amongst other things, there is either no variety in a population, or no meaningful concept of an identifiable environment playing a selecting role.

One difference between the transformational model and specific versions of the PVRS model is of vital significance here. This is a contrast not between their particular specifications but in the ways in which, in the social domain, the two explanatory models are derived or supported. The transformational model (unlike the PVRS model) has been derived in an *a posteriori* manner to explain highly generalised features of social experience (e.g. the prevalence of routinised forms of behaviour, segmentation of practices followed by different types of individuals, and so forth – see Chapter 2). It has been derived by inferring (by way of transcendental argument) what the world must be like for generalised social phenomena to be in evidence. In other words it has been derived by considering the social realm directly. This contrasts with the manner in which the PVRS model has been derived, which is by way of abstracting from the natural selection model found to achieve explanatory successes in biological science. The relevance of the PVRS model to social analysis is thus always open to question, a matter to be assessed in context. In other words, the sustainable reason for focusing on the 'natural selection' model in the social domain is the possession of some *a posteriori* ground (turning, I have argued, on our understanding of the nature of social reality and seeing parallels between it and the material of the biological realm) for suspecting there *may* be some scope for its successful application to social phenomena.

The danger for 'natural selection' thinking which draws on insights from biology, then, is of universalising *a priori* what is but a particular insight, a set of principles, whose relevance in the social realm is found *a posteriori* to be highly dependent on context. I am not suggesting the PVRS model cannot have relevance in the social domain; indeed I have suggested some likely applications above. Obvious further cases to study for purposes of uncovering an evolutionary story are social processes where structures and practices are found *a posteriori* to be relatively enduring but wherein the outcome is not obviously a success story by any absolute, or even necessarily very wide, set of criteria. Possible candidates for social-evolutionary explanations of this sort are (aspects of) some institutions (i.e. of structured processes of interaction that reveal a degree of space-time durability and are recognised as doing so – see Lawson 1997a: 165, 317–18; this volume, Chapter 8 below), as well as, or including, certain routines and habits, as well as some seemingly locked-in (additional, including technological) structures that will likely be bound up with the development of institutions and/or habits.

However, here I want to emphasise that, promising though such candidates may seem, we have not uncovered grounds for any insistence on, or universalisation of, the 'natural selection' evolutionary model. I repeat that to insist without investigation or argumentation that such an approach is everywhere relevant is to promulgate a reductionist *a priori* methodological injunction, on a par with methodological individualism, or deductivism.

My concern here, of course, is to urge the abandoning of all *a priori* injunctions where this is feasible, and to turn, instead, to trial-and-error experimentation as seems reasonable, but also to any approach which includes, as an essential element, the endeavour to fashion methodological principles in the light of social ontological insights obtained *a posteriori*.

Economics and metaphor

Why are reductionist tendencies of the sort I have just noted as prominent as they are in economics? One possible reason for them is the earlier noted belief, seemingly growing in popularity in some quarters, that economics, or the study of social phenomena in general, must borrow wholeheartedly from some other discipline, and, in particular, that it must draw wholesale on metaphors which connect it to other more naturalistic (and especially 'cutting edge' or anyway currently fashionable) developments. This belief is apparently supported by a perception that economics has historically been driven by an attraction for the mechanistic metaphor. The coupling of this latter perception with an assessment that the discipline needs to be brought up to date encourages the idea that the immediate task is to replace the mechanics metaphor with another drawn perhaps from biological, cyborg, or some other science.

Actually, I believe the historical conception of economics as having been driven by mechanistic ideals or the metaphor of mechanics (an orientation often disparagingly attributed in turn to 'physics envy') is largely misleading. The drive to mathematise has always been the more dominant concern (see Chapter 10). It just so happens that the sorts of mathematical methods economists have pursued rest on an implicit mechanistic (essentially atomistic) ontology, thus encouraging mechanistic substantive conceptions.

Further, I think it is important to recognise that when the likes of Marshall, Penrose and Schumpeter make reference to biological models they are concerned not with any necessity to adopt metaphors *per se*, but with the possibility of achieving the goal of a more realistic account of social reality thereby.

Of course, if I am suggesting that it is inaccurate to portray the competition over metaphors as essential to the history or the scientificity of economics, I do not deny that the employment of metaphor has often been, and will continue to be, useful. Rather I maintain only that the usefulness of any particular metaphor to any science is something to be determined empirically, and relates to its appropriateness to the nature of the material under analysis.

Memes and memetics

Still there is undeniably, at present, a wish on the part of some economists to be seen to be abreast of state-of-the art science, or of branches of it most recently in fashion. Now as it happens there appears to be a somewhat imperialistic tendency emanating from elsewhere, and specifically evolutionary psychology, ready to embrace this particular disposition. I refer to the project of memetics. Indeed some readers may wonder why the discussion so far has made (almost) no reference to it. One reason is simply that it is not clear from the literature that very many economists are aware of memetics anyway. So if achieving a short cut in my argument were the goal it is not obvious that reference to memetics would help.

But there are other reasons why I have not connected with the relevant literature before this point. Whilst I believe there may be value in the category at the centre of this project, namely the *meme*, memetics is seemingly most ardently promoted by those who give the appearance (whatever qualifying asides are also tagged on) of seeking to achieve two questionable forms of reduction. The first is a reduction of the natural selection mechanism to the achievements of the 'selfish replicator'. The second is a reduction of the study of society and culture to (aspects of) evolutionary biology or psychology. If this assessment is at all accurate, I do indeed wish to maintain a critical distance. For these tendencies, and particularly the latter, are of just the sort that I have been cautioning against throughout. Let me quickly elaborate.

The term meme derives from the writings of the evolutionary biologist Richard Dawkins, in particular *The Selfish Gene* (Dawkins 1976) and *The Blind Watchmaker* (Dawkins 1986). Having introduced the idea of a *replicator* as anything of which copies are made, i.e. a feature which passes on its structure in replication, and accepting that genes are the replicators of biology, Dawkins asks if there are replicators in other domains. He suggests there are. These are units of cultural transmission or imitation:

> We need a name for the new replicator, a noun that conveys the idea of a unit of cultural transmission, or a unit of imitation. 'Mimeme' comes from a suitable Greek root, but I want a monosyllable that sounds a bit like 'gene'. I hope my classicist friends will forgive me if I abbreviate mimene to *meme*.
>
> (Dawkins 1976: 192)

Memes then are social replicators. In fact they are bits of information which replicate between minds as individuals communicate. Dawkins writes of 'tunes, ideas, catch-phases, clothes-fashions, ways of making pots or of building arches' (1976: 192).

One specific item that may appear to qualify as a meme, so understood, is the already discussed cultural wisdom or belief that for research work to count as scientific (or substantial or serious) it must take a mathematical form. 'Mathematics is essential to science'. This idea, though false as a claim about reality (see Chapter 1), is one that is nevertheless easy to grasp and to believe (especially given the remarkable and continuing achievements of mathematics more widely). Many people (especially those who labour under the two-part impression that mathematics is merely a language, and any language is somehow neutral in scientific work) view the idea (that mathematical methods be always used in science) simply as a (scientific) convention. And as we shall see in Chapter 10, this apprehension of the role of mathematics has long been an element of Western culture.[20]

Of course the idea (or convention) that science requires mathematics is sufficiently abstract to have high fidelity, where accepted. And it is replicated as the practices, methods and theories it conditions are reproduced and transformed over time (and is implicit in, and grounding, the content of textbooks, research papers, lectures and the like). The basic belief or convention involved does not literally copy itself. Certainly it is not copied unaided by human beings. But then nor do genes replicate themselves unaided, certainly not outside the laboratory.[21] Moreover, if replication does depend on human agency, continuity of the kind achieved is not a matter of simple individual volition. The options available to economists are both informed and constrained by the current practices of the academy, as well as accepted canons of knowledge, and curricula. And for mathematical economists the goal is not to reproduce the basic convention as such; mostly the latter is an implicitly accepted belief serving as a means to an end. The perceived goal is a new theorem, or a stable econometric relationship, or some such. But in doing such work and displaying it, the relevant 'copy me' message is communicated to all would-be academic economists nevertheless.

As I say, it is conceivable that we have here an example of a meme. But I am not sure, and I leave it for memeticists to decide. My hesitation in embracing memetics stems, first of all, from a perception that its proponents mostly employ a rhetoric which implies acceptance of the view that the replicator is the prime mover in all that happens. Environmental selection (if it is to happen) 'requires' some reproduced entities to work on. In the case of the finches, for example, reproduction and selection are clearly influenced in an essential way by the nature of the birds themselves (the phenotypes). It is not just down to their genes *per se* (the genotypes or replicators). Dawkins, however, appears often to suggest that, because there is always a genetic contribution to the form, behaviour and reproduction of any phenotype and because the contribution is inherited, the gene is therefore the unit on which selection must

act. Dawkins includes numerous qualifications to such an interpretation in his writings. But the thrust of the argument is clearly that everything of functional importance and complexity is an adaptation fashioned by natural selection working only for the benefit of selfish replicators, that is, in the biological realm, for the 'selfish gene'.

Transposed to the social realm, the selfish replicator become the selfish meme. To quote Dawkins again:

> When we look to the evolution of cultural traits, and at their survival value, we must be clear whose survival we are talking about. … A cultural trait may have evolved in the way it has, *simply because it is advantageous to itself*. … Once the genes have provided their survival machines with brains that are capable of rapid imitation, the memes will automatically take over. We do not even have to posit a genetic advantage in imitation.
>
> (Dawkins 1976: 214–15, emphasis added)

Contributors to memetics appear mostly to accept this perspective, and write of people being victimised by 'viruses of the mind' (Dawkins 1993; Brodie 1996). Thoughts think themselves.[22] Just as, for Dawkins, our bodies are lumbering robots for our genes, so our brains become lumbering robots for our memes, the latter being an evolutionary agent that evolves in accordance solely with its own interests.

Now, on the account defended throughout this book human beings are, amongst other things, intentional subjects. But further, the social realm is not just the result of mental processing by humans. Society is not even made up of people. Rather it is a realm of emergent phenomena comprising social relations of powers, institutions, positions, rules, processes and much more. Culture does not exist only in human minds. What many memeticists appear to lack is serious insight into real social processes, and of how human (intentional) agency and structure interact.

Above (and in Chapter 10) I suggest that the enduring dominance of formalistic deductive economics may involve natural selection tendencies. Here, then, I am acknowledging the possibility that this process (specifically the selection of practices which presuppose the replicator belief or convention 'mathematics is essential to science') constitutes an example of memetics. But if this is indeed thought to be the case, I must emphasise that in attempting to explain how the convention in question is reproduced I do not enquire (or write of) what this convention does for itself, or even what we do for it (which is seemingly the set of questions posed, or stance taken, by most memeticists). Rather I take for granted that, amongst those individuals who seek to mathematise the study of social phenomena, a central motivation is the attainment of improved understanding or scientific advance, or at least gratification in the form of scientific status. In other

words, my argument is that the pursuance of mathematical economics is caused *not* by a self-interested, or selfish, parasite in our minds, but by (understandably) mistaken assessments of the nature and goals of science on the part of its human protagonists. It is because the proponents of mathematical economics are often mistaken in their views concerning the necessity of mathematics, and can be shown to be so, that it is worth engaging with them in order to effect a change (or at least to influence those thinking of joining in with the mainstream project).

Herein, then, lies one reason why I have not linked into the literature on memes from the outset (and am still cautious about doing so). If I have misinterpreted the intentions of memeticists here (and I do acknowledge the numerous tagged-on qualifications that are to be found in the relevant literature, here and there), I suspect the problem lies as much in the way the project is mostly presented as in this reading.[23]

But actually, there is a yet stronger reason for my hesitancy here. This is the universalising and reductionist orientation taken (and even trumpeted) by many of those who contribute to the memetics programme. Whatever insight there is in the literature on memes, and I believe there is a good deal[24] (and I do not accept all the criticisms made of it),[25] a major problem is the propensity of many of its proponents to treat the approach from the outset as one that has universal bearing (even prior to any agreement amongst those working in the area over whether memes have been shown to exist – see for example the competing views of Robert Aunger and Susan Blackmore, both found in Aunger 2000 [chs 11 and 2 respectively]).

Darwin coped with abstract units of inheritance because he had a phenomenon to explain. But modern memeticists are not in the same situation. They lack not only a clear account of details of any proposed memetic explanation, but seemingly also a developed sustainable understanding of the nature of society and culture which they wish to account for. Certainly, nowhere do I find anything closely resembling the transformational model discussed above. Nor are social-ontological elaborations much in evidence at all. The driving force of the project, rather, is an apparent desire to reduce the whole of the social sciences and cultural studies (whatever the nature of the 'objects' the latter do study) to a form of evolutionary thinking.

Perhaps it is not surprising, then, to find of the contributors to a much heralded edited volume on memes (Aunger 2000), that those most committed to the memetics project turn out to be biological in inclination, whilst those most opposed to it have a background of working more in psychology and/or social theory. It is certainly noteworthy that the quest for evolutionary understanding of the social world through using the category of memes has been systematised (by Dawkins) as 'Universal Darwinism', a heading that readily conveys the impression

(whatever may be the strict definition)[26] that the endeavour is not modal, i.e. concerned with seeking *a posteriori* successes, but categorical, signalling an *a priori* thesis about the scope of relevance of the model. It is in this fashion, too, that we can appreciate Dennett's idea of 'universal acid'. It is formulated in his *Darwin's Dangerous Idea* (Dennett 1995), a book which has contributed significantly to a diffusion of the memetics idea. The 'dangerous idea' in question is an abstract algorithm, sometimes called a replicator dynamic. It consists in repeated iterations of selection from among randomly mutating replicators. Couched in such terms, a specific Darwinian evolutionary process, that of natural selection, is interpreted as an entirely general phenomenon characterising not just biological material (such as DNA) but any other kind as well, allowing application of the Dennett's algorithm to anything:

> Darwin's dangerous idea is reductionism incarnate, promising to unite and explain just about everything in one magnificent vision. Its being the idea of an algorithmic process makes it all the more powerful, since the substrate neutrality it thereby possesses permits us to consider its application to just about anything.
>
> (Dennett 1995: 82)

If memetics, at least as perceived by these contributors and others, evades the charge of genetic determinism, it does so only by embracing a universalist stance on socio-cultural evolution. That is, in (correctly) rejecting the idea that evolutionary biology or genetics can explain everything, memeticists encourage the view that biology can explain the natural world and memetics can explain the rest.

As Susan Blackmore, who has perhaps contributed more than anyone to popularising the memetics project (see for example Blackmore 1999a; 1999b; 1999c; 2000a; 2000b), observes:

> The new vision is stunning … because now one simple theory encompasses all of human culture and creativity as well as biological evolution.
>
> (Blackmore 2000b)

In the end, it is difficult to avoid gaining the impression that, so far at least, this 'stunning' feature, this potential to facilitate a theory of everything, is actually the central drive of, and dominant explanation of much of the growing support for, the current memetics project.

My own position, defended throughout this book, is that a surer path to understanding turns on an avoidance of *a priori* universalising, where feasible, and the determining of relevance *a posteriori*. Certainly I welcome

cooperative interdisciplinary endeavour concerned to explore the *scope* of evolutionary, including natural selection, mechanisms. But this is quite different from agreeing in advance that socio-cultural study, including economics, should be reduced to evolutionary psychology and/or biology.[27]

Tailoring to context

To draw the chapter to a conclusion, and to return to its central argument, the basic thesis I defend is that the borrowing by economists from others can benefit from a turn to ontology. Once ontology is brought into the picture it is conceivable that little disagreement of substance will be found to remain amongst many of the imputed protagonists to the debate over the legitimacy of borrowing from evolutionary biology.

When Schumpeter argues that social phenomena 'would have to be analyzed with reference to economic facts alone and no appeal to biology would be of slightest use' (1954: 789), we can interpret him as saying that the relevance to the social realm of the (abstract PVRS model which generalises the) biological-evolutionary model can be determined only by examining directly the nature of (the relevant aspects of) the social phenomena in which we are interested. If so Schumpeter's remark need not after all involve him in the view that economics should eschew all metaphors from physical and natural sciences.

And when Penrose writes that

> in seeking the fundamental explanations of economic and social phenomena in human affairs the economist, and the social scientist in general, would be well advised to attack his problems directly and in their own terms rather than indirectly by imposing sweeping biological models upon them.
>
> (1952: 819)

this again need not be at odds with borrowing from biology. For the point being made here is merely that the external imposition of models and metaphors, without any consideration of their potential relevance to the social domain, is likely to be unhelpful.

The evolutionary natural selection model is merely a construct that carries (now *a posteriori* grounded) potential for insightful social analysis. The call to look to such evolutionary processes, where appropriate, has modal status only. The reasoned stance is to determine the relevance of any specific evolutionary claim by examining it in context. Where the interest is with natural selection mechanisms specifically, it is important to recognise that the PVRS model (i.e. the PVRS model with S and V conditions significantly independent) is not the biological natural selection

model *per se*, but an abstract conception of which the biological model is a token. However, the manner of its *a posteriori* usefulness in the biological realm (i.e. the fashion of its success as a biological token) can, given what we know of the model and of the nature of the social domain, be suggestive of leads to be followed and investigated, but never imposed, in the social realm.

Two contentions are central to my argument. The more specific one is that borrowing from evolutionary biology, or indeed from anywhere, needs to be carried through in a manner informed by the perspective on the transformational model of social activity. The more general contention (of which the former is a special case) is that, in borrowing from other disciplines, economists can benefit from a commitment continually to shaping and reshaping theories or models in the light of insights obtained (and continually updated) concerning the *nature* of social reality. Indeed, I conjecture that problems of the sort that sometimes plague the discussion in question will mostly be seen quickly to dissolve once an ontological turn is effected in economics, with a greater take-up of the realist social theorising.