Part IV

A HISTORICAL PERSPECTIVE ON ECONOMIC PRACTICE

The modern discipline of economics is dominated by a mainstream project whose contributors mostly insist that methods of mathematical-deductivist modelling be everywhere utilised. Few attempts to justify this stance are made. Meanwhile, this mathematising project appears not to be especially successful, certainly not more so than rival approaches and traditions. Here, then, we have an explanatory puzzle. Why or how did economics get into this situation, and how does it persist? Specifically, how has the mathematising project managed so to dominate when we might have expected a programme with no obvious explanatory advantage over others to play a more modest role? This (contrastive) question is addressed in the final chapter of the book.

Prima facie *it does seem likely that historical, political and cultural forces will bear significantly on any explanation. And I find that this is indeed the case. Unfortunately, there exists relatively little serious research into the cultural-political history of those aspects of our discipline here in consideration. This feature of the current situation is in itself somewhat curious, given that the question before us surely represents one of the most pressing and challenging in the history of modern economic thought. In any case, being mindful of the noted state of affairs, and acutely aware that there is much more to be said than I can relate here, I offer the final chapter of the book very much in the spirit of a first step. That having been admitted, I emphasise that I nevertheless believe the account set forth does identify a fundamental component (at least) of the explanation of the puzzle before us.*

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AN EXPLANATION OF THE MATHEMATISING TENDENCY IN MODERN ECONOMICS

The phenomenon to explain

How are we to account for the rise to, and continuing, dominance of the modern mathematising project in economics? Not, I think, in terms of its successes in illuminating the world in which we live. For the evidence is that these are few and far between. Indeed, the modern mainstream project, viewed as a scientific or explanatory endeavour, is not in too healthy a state at all, and unclear even as to its own rationale. Certainly, this is the view of many of its leading spokespersons (see e.g. Rubinstein 1995; Leamer 1983; Chapter 1 above). And away from the mainstream, even outside the economics academy, the perception that (as an explanatory endeavour) the project is faring rather poorly is widely held indeed (see e.g. Parker 1993; Howell 2000).

Perhaps this critical reception sometimes errs on the side of underestimating what has been achieved. But there are certainly no clear grounds for supposing this project has contributed more to advancing social understanding than the numerous traditions it has either displaced or with which it (nominally) competes. And this is the pertinent consideration here. I know of no argument, evidence or reason to suppose that this mainstream tradition has been more explanatorily successful than, say, the (old) institutionalist or evolutionary tradition spawned by Veblen, Commons and others, the post Keynesian tradition building on the insights of Keynes in particular, the Austrian tradition building on the likes of Menger, Mises and Hayek, the Marxian tradition, feminist economists, social economists, and so on. To the contrary, in previous chapters I have provided reason for believing the potential for successes of the heterodox traditions (and not just the level of insight so far achieved) is rather greater.

The situation, then, is somewhat puzzling. For not only is it the case that these heterodox traditions are marginalised within the economics academy, but the degree of their marginalisation is remarkable. In my experience rarely do university lecture courses or set textbooks even acknowledge the existence of alternative projects or traditions. If we look beyond economics there appears to be no other discipline with a mainstream tradition that enjoys anywhere near so great a degree of dominance (or so little relative explanatory success).

So however we look at the situation, it seems that we have here an interesting phenomenon to explain. We have a surprising contrast that we need to account for. How *has* this mathematising project risen to such a position of dominance in modern economics, and managed to maintain this position over a longish period of time, when (given its lack of relative explanatory successes in particular) it would have been reasonable to expect it to fare no better (to say the least) than other projects being pursued? This is my contrastive question here, and I want to set out at least a sketch of an answer, to identify (what I suspect is) an essential part of the total story.[1]

An explanatory first step

I have advanced a partial explanation before (which I further ground below). Specifically, I have previously identified an impetus to the noted situation, the one I believe to be the most significant (Lawson 1997a; 1997e). This is the enormous, almost uncritical, awe of mathematics in modern Western culture. This impetus is a cultural phenomenon pervasive in society at large. The idea that mathematics has a significant role in so many spheres is deeply embedded in our cultural thinking. As Morris Kline summed up his findings in the preface to his *Mathematics in Western Culture* written almost half a century ago:

> In this book we shall survey mathematics primarily to show how its ideas have helped to mold twentieth-century life and thought. The ideas will be in historical order so that our material will range from the beginnings in Babylonia and Egypt to the modern theory of relativity. Some people may question the pertinence of material belonging to earlier historical periods. Modern culture, however, is the accumulation and synthesis of contributions made by many preceding civilizations. The Greeks, who first appreciated the power of mathematical reasoning, graciously allowing the gods to use it in designing the universe, and then urging man to uncover the pattern of this design, not only gave mathematics a major place in their civilization but initiated patterns of thought that are basic in our own. As succeeding civilizations passed on their gifts to modern times, they handed on new and increasingly more significant roles for mathematics. Many of these functions and influences of mathematics are now deeply imbedded in our culture.
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> (Kline 1964: viii)

Indeed the influence of mathematics is now so deeply ingrained within our culture that many people appear to suppose that anything stated in mathematics must be correct, whilst for things to be correct, reliable, insightful or scientific (or at least conferring of scientific status), they must be stated in mathematics. For so many people it seems to be simply an unquestioned and unquestionable matter of faith that if a field of study is to be scientific or accorded status as a knowledge-producing activity, or otherwise regarded as serious, it must take a mathematical form.

This certainly is the view that pervades modern academies of economics. In fact, in the writings of modern mainstream economists, mathematical modelling is even synonymous with the idea (it is considered to comprise the totality) of 'theory', as I detailed in Chapter 1. Further, the belief that mathematical formalism is necessary to science or serious study is accepted even by those mainstream economists aware of the failings of the project (Kirman 1989: 137) as well as by those attempting seriously to change the scope of the discipline (see Sen in *Le Monde*, 31 October 2000). Further, it appears even to seduce many of those who prefer to think of themselves as heterodox. And if for many the belief that mathematical formalism is essential is just too ingrained to be easily shaken off, the thought that formalism could actually be deleterious to understanding is beyond comprehension. Many like Hahn simply reject the latter possibility as 'a view surely not worth discussing' (Hahn 1985: 18) as we noted in Chapter 1. To most economists, mathematical formalism is simply essential to serious substantive theorising.

The point though is that economists are merely copying, borrowing and reproducing 'scientific norms' of the wider community, norms that over time, with the successes of mathematics in numerous spheres, have become embedded in our background ideas about how things work. Certainly, the evidence is compelling that the staying power of the mathematising project in economics owes something to the way mathematics is perceived in our wider culture.

Why, amongst the social disciplines, has mathematics taken such a strong hold in economics in particular? Actually there is evidence of its increasing 'encroachment' elsewhere too. But there is a widespread perception both that the measurable phenomena of the social realm mostly fall within the field of scope of economics, and also that anything that is measurable can likely be treated mathematically. If, as I am suggesting, it is a cultural norm that anything that can be treated mathematically should so be treated, it is not surprising that tendencies to mathematise have put in a stronger showing in economics than in other areas of social theorising.

I might emphasise, before going further, that in making these observations I do not wish to belittle mathematics in any way. To the contrary, I too am enthralled by its elegance and power. But it is possible to recognise the latter without thereby concluding that mathematics be promulgated uncritically and without limit. I am simply drawing attention to what I see as the current uncritical reception (or granting of scientific authority to the perpetrators) of anything mathematical. This, as I say, is an orientation that is embedded in Western culture at large, and in the habits, norms, conventions and power structures of the modern economics academy in particular.

A further puzzle

My thesis to date, then, has been that it is this culturally based idea of science (or serious study) as necessitating mathematics that drives the mathematising project on in economics. Although the fact of this widespread perception of a link between mathematics and all serious thought, does throw light on how the mathematising project persists despite its dearth of successes, it is clear that some problems remain for the thesis advanced. The account so far given is, at best, highly incomplete. For the rise to prominence of practices concerned with mathematising the study of social phenomena occurred only in the twentieth century. Yet, as Kline observes, the cultural embracing of mathematics to which I refer was evident long before this time. And this holds true in parts of the world with amongst the longest and strongest traditions in economics. In France, in particular, the cultural impact of mathematics has, since the Enlightenment at least, been very powerful. Yet even in France it is only in the twentieth century that the attempts to mathematise economics have risen to dominance.

From the viewpoint of the thesis I have been putting forward, then, an important puzzle which remains to be resolved is why, if the cultural norm I identify (that mathematics is essential to serious research) is really so significant, the mathematical project in economics did not rise to prominence at an earlier stage, at least in a country such as France. If, as I am suggesting, the idea, or scientific convention, that mathematics is essential to serious study, is so attractive or difficult to resist, and clearly so easy to copy or imitate, why is it only recently that it has met with the widespread reception it now enjoys? Why, furthermore, did the mathematising project eventually rise to dominance when (and where) it did, given there were no notable breakthroughs in its ability (relative to that of competing projects certainly) to illuminate at that time (or since). Or, to look at the puzzle from a different angle, how can my thesis account for the fact that the project of mathematising the subject does now survive as the dominant approach, given that the place of mathematics in Western culture has not always brought this result?

In short, what explains the *relative fortunes over time* of the mathematising project in economics? If belief in the power of mathematics, along with its necessity to all serious study, has long been an embedded feature of our culture, and is as important and persuasive as the evidence suggests, why have the fortunes of the project not been the same throughout? Why in particular has the mathematising project in economics fared significantly better after the start of the twentieth century, when we might have expected it to fare no differently than before? Here we clearly have a further contrastive phenomenon to account for.

The nature of the expanded explanatory thesis

I want to suggest a development of the earlier explanation that can account for the noted puzzle. In parts at least the explanatory story advanced might reasonably be construed an evolutionary one, incorporating elements analogous to a Darwinian mechanism of natural selection. Indeed, the fact that the project of mathematising economics rose to prominence and survives without at any stage demonstrating itself to be more explanatorily successful than its numerous rivals, immediately suggests that elements of a natural selection evolutionary process may be in play. For a central and great Darwinian insight is that a subset of members of a population may come to flourish relative to other members simply because they possess a feature, which others do not, that renders them relatively suited to some local environment. The question of the intrinsic worth of those who flourish most is not relevant to the story. I shall argue that natural selection mechanisms of this sort are indeed a part of the explanation of the varying fortunes of the mathematising project in economics. But at the same time I shall indicate that such evolutionary mechanisms are *no more than a part of the story*. The episode also helps indicate that borrowing from biology, where relevant, is likely to contribute only partial insights at best.

Evolutionary explanation

In order to illustrate what I understand by 'natural selection', and to indicate why survivors of a natural selection process (typically) do not warrant being regarded as laudatory in any sense, it may be useful, briefly, to recall an example from biology. Consider the case of the varying fortunes of spotted grey and dark moths against an environment of UK industrialisation. Prior to the nineteenth century the spotted grey was more common than the dark moth. When resting on the lichen covered trees in their habitat the spotted grey moth was effectively invisible to birds, unlike the dark moth which was easily spotted against the light coloured trees and eaten. With nineteenth century industrialisation, however, pollutants killed the lichen on the trees in certain areas and rendered the bark of trees in the relevant vicinities a dark colour. Both types of moth continued to rest on trees. But with the spotted grey now more easily recognisable to birds, there was a shift in the relative proportions of the two populations from the spotted grey towards the darker variety. In a sense the pollution-darkened barks protected the darker moths from the danger of the moth-seeking birds.

Darwin provides similar examples:

> When we see leaf-eating insects green, and bark-feeders mottled-grey; the alpine ptarmigan white in winter, the red-grouse the colour of heather, and the black-grouse that of peaty earth, we must believe that these tints are of service to these birds and insects in preserving them from danger.
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> (Darwin 1859: 84)

Notice, however, that although the tints or colours in question may indeed be of service to their possessors, the main natural selection mechanism works neither by way of the variety generation (here genetic mutation) conditions affecting the environment, nor by way of the environment conditions affecting those of variety generation (mutation). Rather, the central causal mechanism in question involves certain environmental factors bearing differentially on (i.e. 'selecting' amongst) the independently produced variety at the level of the individual. In our example, the noted environmental factor selects not at that level at which mutations in types of moth are possible but rather at the level of individual moths. And through such a natural selection mechanism a matching of (surviving) individuals and environment emerges. This is a matching which is no part of anyone's design. It explains why it is so often the case that nature has the appearance of design where it puts in an appearance at all.

Now a significant feature of this process, to return us to the point of the discussion, is that certain individuals are found to fare better than others just because they are of a type, or possess a trait, relatively suited to their local environment, not because they are successful in any wider or absolute or more laudatory sense.

As I say, the explanation I want to advance of the rise to, and continuing, dominance of the modern mainstream or mathematical project in economics (in the face of the failure of the latter to provide any obvious display of relative explanatory superiority) takes much the same form. The way in which the project has been received over the last two hundred years or so, has been related to shifts in the relevant local environment in some way. Changes in the nature of its reception have had little to do with changes in the project's relative explanatory merit or performance.

The natural selection model

Put differently, the possibility I want to examine here is that there is a general process or model of change, one which is well illustrated by biological examples or tokens, but which has social manifestations as well. Specifically, I want to suggest that such a general model can indeed be identified, and that one social manifestation of it is the history of modern economics, or at least of (significant aspects of) its (currently mainstream) mathematical component.

Let me then proceed, at this point, by abstracting out essential components of the more generalised natural selection evolutionary story. The argumentation here will be brief, although it should be sufficient. However, for the reader wanting more detail the basic model is elaborated at greater length in Chapter 5 above.

First of all, in any model capable of incorporating a natural selection mechanism there has to be *variety* in a relevant *population*. The natural selection evolutionary account is one in which, within a population, individuals with a particular trait come to dominate or flourish largely because either

(i) the particular trait is a newly emergent one and found to fit relatively well to the environment into which it is 'born', or
(ii) the particular trait was always present but environmental conditions shift (independently) towards those which in some relevant sense favour the trait in question.

If each individual of a population possess the exact same traits there is no basis for change as evolution via natural selection. If evolution is to be continuous, there must be a continuous source of variation within a population.

Second, if individuals with an environmentally (relatively) apt trait or characteristic are to come to dominate in a population over a period of time, there must be a mechanism whereby the characteristic in question (colour or whatever) is *reproduced* from one generation to another. Following Dawkins (1976; 1978) I shall call an item whose structure is replicated a *replicator*.

Third, there must be a mechanism whereby individuals with different aspects interact with their environments. Without such interaction there could be no mechanism whereby a particular subset of individuals is *selected* in the sense of being found to fit or survive better than others within this environment. Notice I am referring here not (or not just – if we are to capture a natural selection story) to interaction between variety generation (mutation) conditions and the environment, but to interactions between the environment and all the developed individuals within it. Following Hull (1981) I shall call the mechanism for this the *interactor*. All aspects are essential for an explanation along evolutionary lines.

The PVRS model

Let me label an abstract model which supports these features a population-variety-reproduction-selection or PVRS model (as I say, a lengthy discussion of this model is provided in Chapter 5 above). Clearly for the model to capture a natural selection story where *a posteriori* fit is not (wholly) a product of design, it must be the case that the V (variety generation) and S (environmental selection) conditions are largely independent.

Now to suggest that such a PVRS model can have relevance not only in the biological realm but also in the social, is not to suppose that all aspects of the manner in which such a model may be concretised in the biological realm carry over to the social domain. Indeed, if the model has relevance at all to the social realm, it will be concretised quite differently in the latter than in the biological realm.

Most clearly, any processes of innovation, reproduction, interaction and selection as occur in the social realm can be achieved only through the mediation of human agency. Social systems are neither self-reproducing nor naturally produced. Rather, reproduction of the social system results from capable and purposeful human beings going about their daily business, interpreting their everyday tasks and the pertaining social order in very definite ways.

A second major difference between the two realms (that will be reflected in the form of any PVRS model developed) is that any variety generation and selection conditions will be more, or more often, interconnected in the social domain than in the biological. Although much of what occurs in the social realm is unintended and perhaps misunderstood, intentionality is far more significant in the social than natural domains.

I refer to a PVRS model which constrains variety generation (or mutation) and selection conditions to be strictly independent of each other as a strict, or polar (or neo-) Darwinian version of the model. Alternatively put, it is the PVRS model with purely Darwinian features. It is this particular polar model, or close approximations to it, which are often thought to have most relevance in modern evolutionary biology. Certainly it is the version of the PVRS model which best illuminates the natural selection mechanism in which I am here interested. For this version of the model makes it clear that order, a fitting of individual and environment, or part and whole, can emerge even where variety generation and environmental conditions are totally unrelated.

Of course, it is possible to specify versions of the PVRS model that do not conform to the polar Darwinian conception. I refer to a PVRS model which allows environmental selection conditions (S) to feed back into the process of variety generation (V) as a feed-backward or S-to-V model.[2] An example conforming to such a model for the social domain is any situation in which market research and its results, or other anticipations of environmental conditions, are fed back into the variety generation process.

Further I refer to a PVRS model where (conditions or mechanisms affecting) the variety of traits (V) causally influence the selection conditions (S), the feed-forward or V-to-S model. An example here is a situation in which advertising, or indeed any form of persuasion, is used to 'manipulate' the environment of selection.

I mentioned it only briefly above, so let me emphasise that the version of the PVRS model that I refer to as the (Darwinian) natural selection conception is one in which V (variety generation) and S (environmental selection) conditions are independent, at least to a significant degree. Thus I do not restrict it to the polar-Darwinian version, but to any in which the environment, and the factors or traits on which the latter comes pivotally to bear, are to a significant extent independently determined.

In the social realm, of course, it is to be expected that to the extent that the evolutionary or PVRS model has relevance at all, it will never be purely or polar-Darwinian (which would entail that human practices and differentiated survival rates are autonomous of human intentionality); nor purely feed-backward, i.e. backward-*determining* (the functionalist mistake of the modern mainstream); nor purely feed-forward, i.e. forward-*determining* (voluntarism or putty-clay environment). But if it is to be expected that feed-forward and feed-backward mechanisms will each have some role, in a world that is complex, holistic and incompletely understood, such as ours, we should not be surprised if, in any PVRS situation, a Darwinian natural selection element is found to be significant on occasion.

So, to sum up this brief discussion, the Darwinian natural selection model (a PVRS model in which V [variety generation] and S [environmental selection] conditions are to a significant degree independent) promises to be a useful source of redress against those who would see everything that happens in terms only of intentionality or prior design or tendencies to 'normal' or otherwise predictable outcomes. It is a model which counters any other which presumes that all outcomes are optimal in some way, and that this presumed optimality (in a world of rationally calculating individuals) is effectively its own explanation.

It is worth emphasising, however, that in any social explanatory context where the PVRS model does prove appropriate, it is unlikely that a natural (or environmental) selection mechanism acting on the individuals of the analysis will ever constitute the whole of any socio-explanatory story, even if sometimes it is highly explanatorily significant. In other words, if a successful social-evolutionary explanation is possible, it will likely identify modes of interaction between only relatively independent variety generation and selection conditions. Strict Darwinian separation of modes of mutation and selection seem likely to give way to processes of causal interdependency and interpenetration to some degree. Any such explanation, in other words, can be expected to involve shifting patterns of both harmony and tension, of accommodation and rejection, as individuals and ultimately the environment interact in a process of continuous reproduction and transformation. Certainly the possibility that evolutionary tendencies form but part of the story should not be overlooked.

But to recognise this is not to preclude the possibility of a mechanism, analogous to that of Darwinian natural selection, having a role in the social realm, and perhaps even a quite significant one. Whether such a possibility is ever actualised is something that can be determined only empirically. As it happens, I believe that the process I have in mind concerning the eventual rise to dominance and ensuing survival of the mathematising project in economics is just such a case of this kind. Let me now turn directly to the task of explaining this particular phenomenon.

Modern mainstream economics

To recap, given that the formalistic modelling approach to modern economics has not fared noticeably better than the numerous other sets of contributions with which it competes (and even in absolute terms it is hardly a resounding success story) its emergence and continuing survival as a hugely dominant mainstream tradition provides a particularly interesting phenomenon to explain.

Indeed, I believe the history of the modern mainstream, the rise to dominance of formalistic modelling practices and the manner of their 'survival' in this role, constitutes a central chapter in the history of academic economics that remains largely unwritten. The one significant exception to this of which I am aware is the excellent study of the history of general equilibrium economics by Ingrao and Israel (1990), an account that ties in very much with my own reading of the relevant episode in the history of economics. Here I can only give the briefest sketch of certain relevant developments.

Basic components of the social evolutionary story

Now what first of all might be the relevant population of the account I am proposing? What is the population of individuals with a variety of characteristics, some of which will be more favoured than others by specific environmental shifts? The population I have in mind is that of research practices undertaken by those who study social (including economic) phenomena. And the sub-group of population members whose (varying) fortunes I am particularly interested in here, is that set of practices significantly concerned with mathematising the study of social phenomena.

A fundamental component of my account is a recognition that it was not the case that one fine morning in recent times a great economist awoke with the idea of mathematising the discipline, and thereby simply went out and (aided by a culturally embedded belief in the ubiquitous relevance of formalism) quickly achieved this. If this were so, my explanatory account would already be sufficient for my purposes. However such was not the case. Instead, attempts so to formalise the study of society and economy have been under way for a rather long time. Thus such attempts should be recognised as but one set of long existing research practices amongst the variety of practices continually in competition within the population of all academic or serious research practices.

However, it is only relatively recently that practices oriented to mathematising social phenomena have caught on in a significant way, as we shall see. *Prima facie*, then, if an evolutionary explanation is appropriate here it will likely be the version which involves a (relatively autonomous) environmental shift (favouring the mathematising practices already in place). And indeed, I shall argue precisely that the varying fortunes of the mathematising project over time reflect in some significant part (autonomous) changes that occurred in the relevant environment, that we do have something of an evolutionary story of the natural selection sort.

Interactors and replicators

It will already be apparent that the interactors of the account I am proposing are the various research practices concerned with social understanding. But what are the replicators, the entities whose structure is passed on or replicated? I think they take the form of ideas, instructions, edicts or conventional norms. Behind all research practices aimed at social understanding are ideas or norms of some sort, even if they sometimes amount to little more than the notion that 'social phenomena can and should be subject to serious systematic study'. The latter idea, indeed, is obviously widely held and continuously replicated by imitation and persuasion.

Now, in being replicated such an idea can 'mutate' or be slightly modified in many ways. One feasible modification of this particular idea involves substituting the term 'mathematical' for 'serious systematic'. This, of course, is equivalent to combining the original idea with the scientific convention 'mathematics is essential to all serious research including scientific study'. It is feasible that all variations on the original norm give rise to practices concerned with pursuing social understanding. But where the noted mutation is also accepted, only mathematical forms are considered.

Notice that there is no reason to expect an exact match between conditioning norms or ideas (the replicators) and the resulting practices (the interactors). Attempts to mathematise, for example, can be conditioned by slightly varying ideas or norms. Individuals might be guided by the idea 'it is interesting or important to mathematise' rather than the more definite 'for a study to qualify as serious or scientific it is necessary to mathematise'. But given the prevalence of the latter today, and its apparent seductiveness, there is every reason to suppose that it has been equally enticing for a good while, and at least since the time of the Enlightenment.

Of course, nothing stays completely the same overtime. If it is likely that two hundred years ago, say, a motivating concern would have been to determine if, and how, the mathematical modelling of social phenomena might proceed (so that individual contributors were probably imitating each other just in exploring whether it is possible to make any headway), in modern times the practices of mathematical economists are not typically presented as being motivated by a concern to mathematise at all. Rather the mathematical form is mostly accepted in an unquestioning manner, and indeed is typically unacknowledged. And in a similar fashion the resulting exercise is usually presented simply as economics, rather than distinguished as mathematical modelling or some such. Even where individuals take issue with the contributions of others, the implicit conditioning norm 'use mathematics' is rarely challenged and more typically subconsciously copied.

Thus, under today's conditions, with formalistic practices so prominent, it is likely that the 'scientific norms' in question are far more readily (and more subconsciously) imitated and borrowed. Indeed, the mathematisation of economics is currently a rather institutionalised phenomenon. The ways in which (positioned) individuals within any forum or workplace can and do act are significantly influenced by the evolved sets of rules (including conventions) and relations which define their (equally positioned) options and obligations.[3] And so it is within the economics academy, and in particular, with regard to the convention I am here interested in. Unlike, say, two hundred years ago, the 'scientific convention' or edict that 'mathematics is to be used' has become embedded within the institutional structures of modern economics faculties, conditioning the power relations in place, the procedures operative regarding hiring, reproduction of hierarchies, allocation of resources, etc., and so significantly bearing on which practices are encouraged. In short, norms such as that in focus, currently function almost as constitutive 'rules of the game'. Setterfield (1997) makes a similar observation:

> As a profession, academic economics is populated by economists who organize their profession according to certain 'rules of the game' and interact with each other on the basis of these rules. … At present these rules include edicts such as 'the more mathematical an explanation becomes, the better', 'the only relevant sources for citation are recent academic journal articles', 'only mainstream … economists need be heeded', 'only publications in what are internally defined as top journals count', and so forth.
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> (Setterfield 1997: 23)

To recognise all this is, in part, to remind ourselves that the transformational model of social activity is central to all in the social world that takes place (see Chapter 2). All relatively enduring structures, including norms and conventions, not only condition human practice but become reproduced (and transformed) through that practice. This of course, would equally have been the case in earlier times. But the nature of operative social relations, and the manner in which specific rules and conventions would have been reproduced (including imitated) and transformed (including modified) would doubtless have been very different in many respects at different points in time.

So little, if anything, stays unchanged. Even so, if the sorts of considerations here noted are matters always to keep in mind, they do not, in and of themselves, explain the contrastive puzzle earlier identified. They are no doubt relevant to understanding how the socio-cultural system has evolved and made a difference. But the specific (contrastive) question as to why mathematical economics did not reach its current dominant state at an earlier time, as we might have expected at least in certain locations, remains unaddressed. Nor, relatedly, do we have an explanation of the timing of the breakthrough which eventually happened. These and associated matters still need our attention.[4]

Here I want to suggest an explanation of the noted developments. Before I can embark on this, however, I must first ground the specific claim that practices concerned with mathematising the study of society and economy have indeed been long in place, at least in countries like France. For some may doubt that they have been. Yet if they had not then, as I have already noted, the supposed puzzle of the varying fortunes of the mathematising tendency over time would be seen to dissolve straight away. In which case I would need to go no further with this particular explanatory endeavour. Matters, though, are (unsurprisingly) not that simple.

Origins

I do not claim to know where, in the history of those research practices that ultimately gave rise to modern mainstream economics, the formalising tendency first took root. However, it is clear that an important impetus to the process was Newton's success in uniting the heavens and the earth in mathematics. Even Kant came to argue thereafter that a science of society was required, and this necessitated a social-scientific Newton or Kepler to identify the laws of society. And in the euphoria of the achievements of the Enlightenment, indeed, the 'mathematisation' of the social sciences became a major theme of contemporary Western culture.

Certainly during the period of the Enlightenment the endeavour of mathematising the study of social life was enthusiastically taken up by some. According to Ingrao and Israel (1990), in fact, the

> historiography of philosophical thought has long identified the 'mathematization' of the social sciences as one of the major themes of contemporary culture generated and molded in the rich melting pot of the Enlightenment.
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> (34)

France was pivotal in this development, as I have already briefly noted, especially with regards to those aspects of this history that can now be recognised as the direct lineage of modern economics. Let me, then, indicate something of this French history, and thereby give concrete substance to the claim that the drive to mathematise the discipline is really something that long preceded the widespread acceptance of that project in the twentieth century. Once this is achieved, I will be able to consider the puzzle before us under perhaps its most challenging aspect: why the mathematising tendency fared poorly (in terms of take-up) relative to today's achievements, even in France.

The drive to mathematise economics in France

Most economists are aware of Walras' eventual contribution to the mathematisation of the discipline through his formulating the theory of general equilibrium. But he was neither the first nor the last significant contributor to the mathematisation of the subject, even in France. Any list of French contributors prior to Walras, and influenced by Enlightenment achievements, would include the Physiocrats or Physiocratic 'sect', especially Quesnay (1694–1774). Quesnay supposed that the political and moral basis of society is regulated by an inescapable force established by the creator, or at least taking the form of natural law, a view which underpinned his *Tableau économique* or 'arithmetical formula' of the annual reproduction of the nation's wealth.

If Quesnay is to be added to such a list, he is not the only one. There are others whose contributions were often very significant indeed. For example, Turgot (1727–81), a contributor close to the Physiocrats but not a member of the sect, developed the metaphor of blood circulation in suggesting a connection between the operation of markets and the dynamics of fluids. Dupont de Nemours (1739–1817) argued that because everything happens in the order established by the creator of nature, it is possible to apply physico-mathematical methods to the moral sciences. Condorcet (1743–94) attempted to found a *mathématique sociale*, aiming to achieve an objective science of subjective phenomena formulated in terms of the probability calculus. Achylle-Nicolas Isnard (1749–1803) produced his own table of arithmetic to demonstrate, as a departure from Physiocratic thought, that manufacturing industry, like agriculture, may also generate a surplus, one that accrues to not only landowners but also owners of scarce productive resources. Canard (1750–1833), in works on social mathematics and on political economy, provided (or anyway attempted to provide – some dispute his achievements) the first explicit formulation, and dynamic treatment, of the notion of economic equilibrium, the first application of marginal analysis, and a conception of the connection between the ideas of mechanical and economic equilibrium. Dupuit (1804–66), contributed to the development of general equilibrium theory by providing a mathematical foundation for the idea of the measurability of utility (a quality of the good depending upon the attitude of the economic individual). And Cournot (1801–77) demonstrated how to apply functional analysis to economic phenomena in a manner that required specifying only the most generalised features of the functional forms utilised. He also provided a statement of a supposed law relating the quantity of a good demanded and the latter's monetary price in a single market. And it was Cournot who introduced concepts eventually known as the elasticity of demand and marginal cost, and ideal types of market forms (perfect or unlimited competition, etc.), amongst much else.

Walras (1834–1910) remains the central figure in this early French history, of course, at least in terms of modern-day renown. But we can already see that in formulating a mathematical theory of general equilibrium, Walras was developing the work of others, most especially the contributions of Canard, Isnard and Cournot (although only the latter is explicitly acknowledged by Walras).[5]

I have no need to go into the details of Walras' contribution here, which are in any case well known. At this point I am merely concerned with identifying relevant threads in the early history of the current mainstream. I am wanting to draw attention to the fact that practices concerned with mathematising the discipline of economics have long been under way. And the driving force, the generative motor, was societal culture. This bore as heavily on Walras as on his predecessors more than a hundred years earlier. The historical analysis of Ingrao and Israel indicates well

> how deeply attached Walras was to the main trends in French culture that had inspired the application of mathematics to economics and, in particular, the early development of economic equilibrium theory. Despite his reluctance to acknowledge his precursors … there are numerous passages clearly showing his awareness of belonging to a French cultural tradition inspired by

> a project of applying the Newtonian model of physical and mathematical science to the economic and social sciences.
>
> (Ingrao and Israel 1990: 141–2)

We might note, too, that when Walras resigned from his teaching obligations in Lausanne in 1893, he was succeeded by Pareto (1848–1923), born in Paris but eventually raised in Italy, who was also concerned with the mathematisation of the social world. For Pareto, at least as much as for Walras, an understanding of mechanical equilibrium served as a model for theorising general economic equilibrium. In attempting to construct a rational mechanics of economic behaviour using methods of physics and mathematics, Pareto aimed to give the former the same analytical foundation and empirical grounding as rational mechanics.

Part of the lineage of modern economics is to be found, then, in France's intellectual history. However, although important contributions to the modern situation emerged during this early French episode, none were especially well accepted in their own time (even if Walras was occasionally prone to making extravagant claims to the contrary). Of course, as we now know, the goal of mathematising the discipline, including that of developing a formalistic equilibrium theory, did eventually become widely accepted, even if mathematical modelling methods have never proven to be particularly successful or fruitful as ways of investigating and understanding social reality (see e.g. Hahn 1985). Walras in particular, albeit long after his death, was eventually to achieve the recognition he had, for much of his lifetime, felt he deserved. Samuelson, for example, was to interpret him as the only economist on the level of Newton. And Schumpeter declared him 'the greatest of all economists'. However, before there was to be a widespread acceptance of mathematical economics in general, and of the importance of Walras' contribution in particular, a new methodological framework was to be adopted, and the focus of attention would veer away from France to interwar Vienna, Britain, Sweden and ultimately the USA.

Before examining various relevant aspects of these developments, however, there are other matters to consider. But first, let me reemphasise my objective here. I am proposing a socio-evolutionary explanation of the development and persistence of modern mainstream economics interpreted as the project concerned to formalise social/economic phenomena. To this point I have merely indicated that amongst the variety of practices within the population of methodological practices of economists, endeavours to mathematise the study of social phenomena have long been in evidence. If this, at least in part, is an evolutionary story, I need to demonstrate how the environment has played a role in selecting out the *a posteriori* successful practices (or equivalently, in filtering out those which, at any point of time, were unsuccessful). I emphasise that I do not take a deterministic stance here. Changes in the environment do not have to play such an influential role. My argument is just that, in the case of the rise of modern mainstream economics, it turned out *a posteriori* that they did.

The culture of mathematics in France

In fact, a question I ought really to address at this point is why the euphoria of the achievements of the Enlightenment gave rise to such an impulse to mathematise the social sciences in France in particular. Of central relevance here, I believe, is the Cartesian heritage of this country. Newtonianism was initially wielded as a weapon in the intellectual struggle against Cartesianism (Voltaire 1738). But, as is so often the case in a debate where each side contains insight, the outcome was a project modified very much in the light of criticisms of, and so conforming to, the other. Thus it was, that on emerging from its encounter with Cartesianism, Newtonianism (in its particular guise of a concern with elaborating laws)[6] assumed quite unique features in France, being substantially transformed in line with the opposition. In particular, whereas the empiricist orientation of England gave rise to small-scale empirical research, the French physico-mathematical approach adopted the goal of furthering the mathematical analysis of Newton's laws of physics. Moreover those who accepted this goal were quite successful in their pursuit of it. So much were they so, in fact, that (at a time when the English Royal Society was in decline) the French Académie des Sciences, very much bound up with the development of (this form of) Newtonianism, became established as the leading scientific institution in Europe.

As might be expected, this achievement of French science had knock-on effects in society at large. Science came to be seen as the most prestigious sphere of French life and thereby amongst the most influential. In its mathematical-Newtonian guise it came to be seen as an ideal for all branches of study and for culture more widely, giving an impulse to the idea of a mathematical-scientific approach to the governance of society, and, as a condition for this, to an understanding of its conditions. In their historical overview, Ingrao and Israel summarise the ensuing situation in France as follows:

> [With the successes of the 'French physico-mathematical school'] the scientific intellectual became the model intellectual and the scientific community the model for scholarly communities. In the reformist view of the values and decrepit institutions of absolutism, Newton's scientific philosophy and the model of the scientific intellectual established in France became points of reference for an ideal renewal of the whole of society. In its new

> Newtonian garb, science put itself forward as the *center* of society and the *driving force* of reform, promising new horizons in all fields of knowledge to which the new methods of scientific thought could be applied. This scientistic (in the full and broad sense) vision was thus projected beyond the confines of traditional science, and under the urgent prompting of institutional, economic and social problems – first under the *Ancien Régime* and then during the Revolution – the question of the *scientific* government of society and economy achieved full status also in theoretical terms.
>
> (Ingrao and Israel 1990: 35–6)

The environment: orientations to the mathematisation of social phenomena

If Western culture in general, and French post-Enlightenment culture in particular, held mathematical practice in such high esteem, it is not surprising that attempts to formalise economics took place in such conditions.

It may be thought a puzzle, then, that such practices failed to win widespread approval within the academy at an earlier time, at least in a country like France. If the culture placed a premium on the reproduction and proliferation of mathematical practices, including in economics, why did they not flourish more in that field in the immediate Enlightenment period, and on the scale they do today? After all, they have since achieved dominance without proving to be especially explanatorily fruitful, certainly not more so than other approaches. Why did widespread acceptance within the academy take so long? Why were the mathematising practices (or the scientific values, codes and norms underpinning them) not more widely copied amongst would-be social-economic theorists? Or where they were copied, why were the results not more influential? Why, most especially, did mathematical economics not become more widely accepted at a far earlier point in time in France, where the Enlightenment impulse to the mathematising ideal was accepted so quickly, and early on became deeply culturally embedded.

The answer, I now want to argue, has something to do with the specific local academic environment in which the mathematising economists were situated. Let me indicate something of the context in which the early post-Enlightenment attempts to formalise the study of social phenomena occurred in France.

In fact, in the period immediately following the Revolution, the academic climate in France was particularly open to ambitious projects of political and educational reform. At this point, the application of mathematics, as opposed to many literary activities, was interpreted as accessible to people from all backgrounds or classes, and so desirable, and social mathematics found some space in the educational system. In particular, the Academy of Moral and Political Sciences of the Institut de France concerned itself in a very significant way with the application of mathematics to the study of society.

But Enlightenment culture not only prompted attempts to mathematise all areas, it also required criteria of verification in all fields. There was a demand that descriptive or explanatory accuracy be demonstrated. From early on, even in France, there was significant opposition from the sciences at large, and from within mathematics especially, to the use of mathematics in areas for which it was considered unsuited. As the early optimism of the Revolution turned to the harsher realism, even to academic intolerance, of the Napoleonic order, there was less emphasis on encouraging certain academic practices for their own sake, or for the sake of those who prosecuted them. Greater emphasis, instead, was put on accepting academic practices for their perceived relevance.

The demand for descriptive or explanatory relevance was to prove, then as now, beyond the means of those striving to mathematise the social realm. And this was widely recognised. Laplace, in particular, came to view the attempt to mathematise the study of social phenomena as an intellectual mistake. He gave some support to the idea at the time of the Revolution. But with further study and reflection, his attitude turned to one of outright hostility. So hostile was he, in fact, that when, with the death of Lagrange, he achieved near supremacy in scientific matters in France, especially at the Institut de France's class of geometry, Laplace set about actively purging what remained of the programme of mathematising the study of society.

In this period, with Laplace's influence large, the scientific world largely lost interest in applying mathematical methods outside of physics. Only the physico-mathematical sciences were accorded any serious status. The project of mathematising the social world continued, of course. But a result of developments in the physico-mathematical sciences was the abandonment of all attempts at constructing an autonomous discipline. Instead, such attempts to mathematise social phenomena as persisted followed the official model as laid down by the physico-mathematical sciences. They became oriented to traditional mathematical tools and concepts and deterministic methods of mechanics. All traces of Condorcet's probabilistic approach, for example, for the time being disappeared.

The impact of Jean-Baptiste Say

In this climate it is perhaps not surprising that the study of social phenomena was undertaken in a largely non-mathematical way. Actually this is too neutral a description. For in resonance with Laplace's views, the study of social phenomena in the nineteenth century became dominated by those who not only mostly abstained from formalistic endeavour, but also actively discouraged it, albeit, in part, for somewhat idiosyncratic reasons.

Jean-Baptiste Say (1767–1832) and the French liberal school he in effect founded, a school that was to dominate the field of social study for most of the nineteenth century, took much the same position as Laplace. Say even made opposition to the mathematisation of social phenomena a central plank of the school's broader philosophy. It is relevant to inquire why. After all, the French liberal school was primarily concerned with particular substantive theories and policies. It is true that Say provided numerous comments as to why realisticness ought to be prioritised over 'algebraic formulas' and the like. Even so, an obvious, and sufficient, orientation to have adopted, was opposition to any dogmatism on the part of others who neglect the real world. On the face of things, there was no obvious reason to make an opposition to all mathematising tendencies a central part of the school's programme. Yet this is what happened. Indeed, as Ingrao and Israel have also observed, this 'rejection of the mathematisation of social science was pushed by Say almost to the point of the idiosyncratic rejection of mathematics *tout court*' (Ingrao and Israel 1990: 60). If the stance taken by Say and others to economics was in keeping with the views of natural scientists, and perhaps contributed something to Say's significant influence at the time, what actually was the reason for Say coming to adopt such a position in the first place?

The story is somewhat complicated, as Arena (2000a) makes especially clear. Although the period 1790–1870 saw the rise to prominence of the French classical or liberal school, with Say as the founder and figure-head, Say's initial project was not to establish a new school at all, but something rather different. His purpose was merely to disseminate the insights of Smith's *Wealth of Nations* in continental Europe, albeit with some extensions introduced for purposes of clarity (Say 1803).

But Ricardo and Malthus adopted similar projects, albeit providing different interpretations of Smith. This introduced a kind of rivalry, especially between Say and Ricardo. Over time this led Say to re-evaluate his own contribution. First he revised upwards the degree of originality of his contribution. He reinterpreted his project not merely as disseminating Smith's writings but also as advancing Say's own scientific discoveries. And eventually he came to argue explicitly for a different approach to that of Ricardo and other heirs of Smith. Although Ricardo did not use mathematics in his contributions, he did adopt a deductivist style of argument. It is a mode of argumentation that would lend itself to easy mathematisation by later mathematical economists, and suffers from the same problems of connecting with social reality as experienced with mathematical methods in economics. Say was keen to be distinguished from the Ricardians, and indeed to be viewed as providing a superior contribution. It was an opposition to Ricardo's deductivist method, in particular, that Say chose to emphasise in this, and which emerged thereby as a central plank of French classical thought. Thus commentaries on mathematics, like the following, invariably connect the mathematising tendency with economists influenced by Ricardo:

> Without referring to algebraic formulas that would obviously not apply to the political world, a couple of writers from the eighteenth century and from Quesnay's dogmatic school on the one hand, and some English economists from David Ricardo's school on the other hand, wanted to introduce a kind of argumentation which I believe, as a general argument, to be inapplicable to political economy as to all sciences that acknowledge only experience as a foundation. By that I mean the argumentation that lies on abstract ideas. Condillac has rightly noticed that abstract reasoning is nothing but a calculation with different signs. But an argument does not provide, nor does an equation, the data that is essential, as far as experimental sciences are concerned, to get to the discovery of truth. Ricardo set it in a hypothesis that cannot be attacked because, based on observations that cannot be questioned, he imposes his reasoning until he draws the last consequences from it, but he does not compare its results with experience. Reasoning never wavers, but an often unnoticed and always unpredictable vital force diverts the facts from our calculation. Ricardo's followers … considered real cases as exceptions and did not take them into account. Freed from the control of experience, they rushed into metaphysics deprived of applications; they have transformed political economy into a verbal and argumentative science. Trying to broaden it they have led only to its downfall.
>
> (Say 1971: 15)

As Arena summarises matters:

> This dissent from Ricardo's method was considered by Say as a fundamental issue and this view was then adopted by most of Say's French Liberal followers, forming therefore one of the crucial components of the liberal theoretical framework in France.
>
> (Arena 2000a: 207)

Certainly, most of Say's followers took his lead in opposing the Ricardian deductive approach, with some of them, especially Wolowski (1848), Reybaud (1862) and Baudrillart (1872) being opposed to the use of mathematics in particular.[7] According to Reybaud, for example, the Ricardians were only out 'to feed principles with equations and give political economy a false air of algebra in order to impress minds who look for deep thinking' (Reybaud 1862: 301)

The details of the rise to dominance of the French liberal school, with its fundamental opposition to mathematical methods, need not concern us here (and are well documented in Arena 2000a: esp. 215–18). The point to emphasise, rather, is that once it achieved dominance this school developed strategies to maintain its position. For example, liberals attempted to control educational institutions that played any role in the teaching of political economy. At some point or other, they carried significant and often total influence in the Athénée, the Ecole Spéciale de Commerce, the Ecole Commerciale, the Conservatoire des Arts et Métiers, and the French Grandes Ecoles, with the peak of their sway culminating, in 1871, in the creation of the Ecole Libre des Sciences Politiques. The liberals also significantly influenced the constitution of scientific societies. They created the Société d'Economie Politique in 1842, and became prominent amongst the members of Académie des Sciences Morales et Politiques after its re-establishment in 1832. And liberals also either dominated, or very significantly influenced, the major journals read by economists. These included *Le Censeur*, *Le Libre-Echange*, *L'Economiste Français*, *Le Globe*, *Le Journal des Débats*, *Le Siècle*, and most significantly *Le Journal des Economistes*. The latter, which was created by the liberals in 1841, defended the liberal viewpoint until its demise during World War II. The effect of all this on the contemporary practices of economics in France is once more well summarised by Arena:

> French liberal economists, however, were jealous of the influence of their approach. Therefore, they built and implemented a strategy for the diffusion of this message. The liberal school thus formed a homogeneous group unified by familial links, friendship and participation in common Societies and Journals. This participation strongly contributed to the diffusion of the liberal central message. It was however decisively reinforced by the strategy of control of educational institutions. This control helped French liberal economists to diffuse their views and act as if they were the only ones who could be considered 'economists', as such. Their cultural, political and social predominance was no longer questionable. Economists who did not accept the liberal views were proclaimed to be 'heretics': they became 'socialists' or 'prohibitionists'; they actually lost their right of belonging to the realm of political economy.
>
> (Arena 2000a: 219)

So important was the liberal school's influence, including its amplification of Say's rejection of attempts to mathematise the social sciences, according to Ingrao and Israel (1990), that 'Say's methodological views were long to weigh upon French culture as an impediment to any further attempt to use mathematical models in economics' (60).

Thus from the beginning of the French classical period to the time of Walras, the relevant academic environment presented difficulties for would-be mathematisers of the study of social phenomena. In French society at large the idea of mathematics as an essential feature of any respectful discipline prevailed. Yet within relevant branches of the academy (the relevant local environment) the reception afforded the would-be mathematisers of social phenomena was continually hostile. For the natural sciences and their mathematicians, this of course did not entail a demotion in the importance of mathematics *per se*, merely a recognition that economics required something different. For Say and his followers, in contrast, there likely was a rejection of the view that mathematics is an essential component of all serious processes of knowledge production. But in either case, attempts to mathematise the study of social phenomena were viewed as misguided and, more significantly, actively resisted.

Still, attempts to mathematise the social sciences continued throughout, as we have seen. Variety in social research practices was always present, and the wider cultural forces ensured that the range of practices followed included at least some of this mathematising sort. But it was always difficult for the would-be social mathematicians. The influence of Laplace, as I say, resulted mainly in a forced concentration on the strictly deterministic approach of mechanics based on methods of infinitesimal calculus. And the fact of the near-total dominance of Say's school within economics, along indeed with, in the late nineteenth century, the growing influence of historicism and institutional analysis, in addition to the scientific community's eventual near-total dissociation from the mathematisation of the social sciences project, rendered any contribution to the latter a somewhat isolating and wearisome endeavour. It was precisely these conditions that Walras himself was to encounter.

The reception of Walras

It is against a backdrop of such forces and developments, then, that we must interpret the reception of Walras' efforts. Not surprisingly, when in 1873 Walras presented his first attempts at formulating a mathematical economics at the Institut de France's Académie des Sciences Morales et Politiques, it was largely met with either disinterest or outright hostility. The economic historian Levasseur was especially critical. In particular, he ridiculed Walras' application of mathematics to social phenomena which, as he saw it, do not lend themselves to such a treatment, concluding that 'one gets a far better idea from thinking than from the author's mathematical formulae' (Levasseur in Walras 1874: 117). Levasseur also warned of the

> danger that lies in the desire to bring together, as a unit, at any cost, things that are complex by their nature, as in wishing to apply to political economy a method that is excellent for the physical sciences but could not be applied indiscriminately to an order of phenomena whose causes are so variable and complex and that above all involve one eminently variable cause that can absolutely not be reduced to algebraic formulae: human freedom.
>
> (Levasseur in Walras 1874: 119)

Other economists proved hardly more charitable in their reception of Walras' formulations.

Thus ignored or dismissed by economists, Walras turned some of his efforts to seeking the approval of physicists and mathematicians. This is not to say that Walras no longer sought the approval of economists as well. But perceiving that mathematics was the dominant and most influential discipline, Walras reasoned that if the mathematicians could be brought on side, the economists would sooner or later follow. But persuading mathematicians that his approach had relevance was no easier than persuading economists. Although some were interested, most were not. Walras, ever the optimist, eventually claimed Poincaré as amongst the more positively inclined. But this was really an exaggeration. In a short letter he sent to Walras in 1901, commenting on the copy of *Eléments d'économie politique pure* that he had recently received from Walras, Poincaré observed:

> at the beginning of every mathematical speculation there are hypotheses and that, for this speculation to be fruitful, it is necessary (as in applications to physics for that matter) to account for these hypotheses. If one forgets this condition, then one goes beyond the correct limits.
>
> (Poincaré 1901)

It is this realist condition, of course, that mathematical economists have been unable fully to satisfy either prior to, or since, this time.[8] Against Walras' *Eléments*, specifically, Poincaré, picking up on features that are still prominent in much modern economic theorising, observed:

> You regard men as infinitely selfish and infinitely farsighted. The first hypothesis may perhaps be admitted in a first approximation, the second may call for some reservations.
>
> (Poincaré 1901)

In truth, after several years of self-propaganda by Walras and often fierce rejections of the idea of mathematical economics by mathematicians, the dialogue between the two groups – mathematicians and those economists keen to formalise the study of social phenomena – became severely curtailed. Ten years into the twentieth century, indeed, it seemed that the goal of extending support for the application of mathematical methods beyond the borders of physics, certainly to the social sciences, was widely (though never universally)[9] regarded as impossible.

Yet despite these setbacks, the story was, at this point, far from over. As we know the mathematisation project in general, and general equilibrium analysis specifically, were yet to rise phoenix-like from the ashes. How could this be? In particular how could this be if, amongst other things, and as I have noted all along, the project was never to achieve much success in terms of illuminating the social world?

A shifting environment: reinterpreting mathematics

A significant part of the answer lies in a shift that occurred in the relevant environment, specifically, in the environment of academic practices within which attempts to mathematise the discipline competed with others. I have already noted how the criticisms of Laplace and others led those economists who continued with the mathematisation project to adopt the model of the contemporary paradigm of physics, basically mechanics. However, at this time, this classical reductionist programme (the programme of reducing everything to the model of physics, in particular mechanics) was itself coming into disarray. With the development of relativity theory and especially quantum theory, the image of nature as continuous came to be re-examined in particular, and the role of infinitesimal calculus, which had previously been regarded as having almost ubiquitous relevance within physics, came to be re-examined even within that domain.

The outcome, in effect, was a switch away from an emphasis on mathematics as an attempt to apply the physics model, and specifically the mechanics metaphor, to an emphasis on mathematics for its own sake. As classical physics itself went into crisis, developments in mathematics were to reduce the dependency of mathematisation projects on physics altogether. Mathematics, especially through the work of Hilbert, became increasingly viewed as a discipline properly concerned with providing a pool of frameworks for *possible realities*. No longer was mathematics seen as the language of nature, abstracted from the study of nature. Rather it was conceived as a practice concerned with formulating systems comprising sets of axioms and their deductive consequences, with these systems in effect taking on a life of their own. The task of finding applications was henceforth regarded as being of secondary importance at best, and not of immediate concern.

This method, the axiomatic method, removed at a stroke various hitherto insurmountable constraints facing those who would mathematise the discipline of economics. Researchers involved with mathematical projects could, for the time being at least, postpone the day of interpreting their preferred axioms and assumptions. There was no longer any need to seek the blessing of other economists or of mathematicians and physicists who might insist that the relevance of metaphors and analogies be established at the outset. A need to match method to the nature of social reality was no longer regarded as a binding constraint, or even a matter of any relevance, at least for the time being. Nor, it seemed, was it possible for anyone to insist (with any legitimacy) that the formulations of economists conform to any specific model already found to be successful elsewhere (such as the mechanics model in physics). Indeed, the whole idea of prior models, metaphors, even interpretations, came to be rejected by some economic 'modellers' (albeit never in any really plausible manner).

If, then, there is, for many, something almost addictive, certainly seductive, about the idea that undertaking serious study requires the application of mathematical formalism, early in the twentieth century this particular 'scientific convention', as a motive for social study, was cut free from its previous leash. Economists could now indulge their mathematical desires, freed from the need to give much by way of realistic interpretation of their contributions as justification, or even (at least in principle) from the need to provide any interpretation at all.

Probably the most famous (though certainly not the only)[10] influential contribution to the formalisation of economics since Walras remains Debreu's (1959) axiomatic treatment of (the existence and uniqueness) of general equilibrium, a contribution that gained its author the Nobel Memorial Prize in economic science. Even today the language and symbolism of Debreu's *Theory of Value* is found in many axiomatic papers. And Debreu's contribution rests for its legitimacy precisely on the claim that axioms are not in need of any interpretation. As Debreu expresses these matters himself:

> *Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations*. In order to bring out fully this disconnectedness, all the definitions, all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics; moreover, the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions: 'in the language of the theory,' 'for the sake of the theory,' 'formally.' Such a dichotomy reveals all the assumptions and the logical structure of the analysis. It also makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretations of concepts.
>
> (Debreu 1959: viii, emphasis added)

If the decline in the classical reductionist programme and the rise of axiomatic mathematics laid the conditions for the eventual proliferation of mathematical economics, advances along these lines came only gradually. And it is perhaps significant that the project of mathematising economics received the greater stimulus at this juncture not in France, with its close links with the classical reductionist programme, but in Austria and Germany, where the new physics, a revised conception of the role of mathematics and a specific emphasis upon axiomatic mathematics, had originated and now flourished. In particular, it was here that von Neumann, Wald, Morgenstern and other mathematicians made their initial contributions. And although approaches such as those of Wald and von Neumann were different in kind, they were later reconciled in the US, where many of the early contributors emigrated under the Nazi threat.

Of course, France itself eventually witnessed significant related developments as well. I have already mentioned the contribution of Debreu. Although Debreu's *Theory of Value* was produced after his move to the US Cowles Commission in the 1950s, Debreu was very much a product of the French Bourbaki 'school' (a group of French mathematicians[11] who argued that mathematical systems should be studied as pure structures devoid of any possible interpretations). It was at the Ecole Normale Supérieure in the 1940s that Debreu came into contact with the Bourbaki teaching. And once trained in this maths, but with his interests aroused by economics, Debreu sought a suitable location to pursue an interest in reformulating economics in terms of this mathematics. It is perhaps not insignificant that his move to the Cowles Commission coincided with the latter's effective acceptance of Bourbakism.

The fine details of the latter and all other developments cannot be elaborated here.[12] My general point, though, is common to most if not all such individual pathways, and can be stated without filling in all the specific links. It is that in the Western academies at least, the constraint of social reality on mathematical modelling was at this point postponed until some 'tomorrow'. And with this being the case, the possibilities for mathematical modelling were, for the time being anyway, restrained almost solely by the ingenuity of the protagonists.[13]

The political environment

But is this shift in the way mathematics became understood and pursued a sufficient explanation of the fact that the formalising tendency in economics came to achieve such a dominant position? The factors so far discussed certainly provide some understanding as to why the cultural perception of the ubiquitous role of mathematics came to play a bigger role in influencing developments within the economics academy at a certain point in the twentieth century. They also help explain why before that time the mathematising tendency was constrained from playing a greater role, at least in France where a significant role was most to have been expected. However, it is not clear that the environmental shift described is sufficient by itself to account for the phenomenon that, from the mid-twentieth century onwards, the mathematising project has become quite so dominant in economics. Is the seductiveness of doing things mathematically sufficient to explain its successful take-up? Or did aspects of the relevant environment move in further ways that not only unleashed, or freed up, the mathematising tendency, but actually advantaged the formalising endeavour relative to other forms of research practice? I think the latter happened as well.

The post-World War II US context

This freeing up of mathematics, this removal of the burden or constraint of having to fit with reality, was indeed a reasonably generalised phenomenon. Nevertheless, to understand subsequent worldwide developments in the post-World War II period, it is necessary to appreciate that, and why, this decoupling of mathematics and (the study of) reality (allowing the promotion of the former unhindered by the constraint of conforming to the latter) enjoyed an especially warm reception in US economic faculties.[14] For it has turned out that the US has had the resources to dominate the post-World War II international academic scene in economics (as indeed it does in so many other disciplines).

Why was the US so receptive to this decoupling? A significant feature was a shift in the political environment. In particular the emergence of McCarthyite witch-hunts in the context of the Cold War significantly affected the developments in which we are interested. In this climate, the nature of the output of economics faculties became a particularly sensitive matter. And in such a context, the project of mathematising economics proved to be especially attractive. For it carried scientific pretensions but (especially when carried out in the spirit of the Bourbaki approach) was significantly devoid of any necessary empirical content. The group most feared or resented by the McCarthyites were the intellectuals (Reinert 2000). The formalising project with its technicist emphasis, often to the exclusion of almost any critical or reflexive orientation, was clearly extremely attractive to those caught up in the situation. This was especially the case not just for insecure or fearful university administrators, but also for the funding agencies of US social scientific research (who were especially important in this period – see for example Coats 1992; Goodwin 1998; Yonay 1998).

In making these observations I am not, of course, suggesting that those who contributed to the formalising project in economics did so opportunistically to pander to this demand for non-controversial stances. Indeed, it is an essential part of my thesis that this formalistic project was already long established. It had a tradition in the US as elsewhere, especially since the 1930s (see for example Yonay 1998), and most clearly after the establishment of the Econometrics Society. Those who pursued the mathematising project were no doubt motivated only to improve the project's academic performance and intellectual legitimacy. My argument, rather, is that, during this period, various relevant environments, including the political environment of the US, swung in a way that favoured the formalising project. And as always there were enough people around, or attracted by the North American situation, who were enamoured of formalising practices. These just happened to be the economists who benefited most from swings in the political environment.[15]

In fact, historians of the US have long argued that McCarthyism and the Cold War was decisive in the growth of anti-intellectualism in the US in the twentieth century[16] (see e.g. Hofstadter's [1963] *Anti-Intellectualism in American Life*; or Bloom's [1987] *The Closing of the American Mind*). My point here is simply that this environment impacted on the economics faculties as elsewhere, and was doubtless conducive to the spread of economics as mere technicist manipulation. Reinert (2000) reaches a somewhat similar conclusion:

> McCarthyism and the Cold War created a demand for a kind of economics that the mechanical versions of neo-classical economics and Austrian economics could both provide. The neo-classical utopia of market clearing harmony and factor price equalisation was an important counterweight to the communist utopia and its omnipotent state that promised to wither away.
>
> In this context the 'intellectuals' became a nuisance. The 'intellectuals' had historical and political qualifications and modifications to the clear message of an absolute superiority of the unmollified market economy. American pragmatism under the pressures of the Cold War degenerated into expediency and anti-intellectualism. History – also US history – cluttered the message of the near 'evilness' of state interventions under all circumstances and in all contexts. Removing economics' previously solid foundation in the humanities pried open for the rule and dominance of the mechanical models: clear conclusions, but conclusions which in their pure and undiluted form are only valid in a world devoid of diversity, of friction, of scale effects, and of time and ignorance. …
>
> The pure neo-classical techniques in which economic harmony is already solidly built into the basic assumptions – providing results like Samuelson's *factor-price equalisation* – was the kind of theory that was ideologically and politically in demand. We are not suggesting that this kind of theory was created for political purposes. The theories had been there essentially since Ricardo,

> but the *demand* for this kind of theorising rose considerably during the Cold War, sharpening its focus and message, but conveniently leaving aside the mitigating counter arguments of history. … In this way the 'technicians' crowded out the 'intellectuals' of the economics profession.
>
> (Reinert 2000: 29)

Clearly Reinert, in drawing attention to the nature of the postwar US context, is focusing as much on the content of the (sorts of) theories that thrived as on their formalistic nature. But the nature of the (potential) content is always constrained by the method. And in any case, the arguments about the environment of selection have even more bearing when we focus on the use of technique *per se*, and particularly on those instances in which the construction of (formalistic) structures were held to have no necessary interpretation whatsoever (also see Morgan and Rutherford 1998).[17]

Let me briefly take stock. I have argued that the formalising tendency has been in play long before the twentieth century, albeit meeting with little success in the area of formalising the study of society. In the early to middle twentieth century, however, that project's fortunes, in terms of approval rating, started to improve remarkably. This, however, occurred not as a result of any improved explanatory performance relative to that of any competing projects (or even in absolute terms). Rather it was the climate of its reception that shifted. Fundamental here are changes in the way mathematics became interpreted, and in the criteria according to which mathematical reasoning in any sphere is considered justified. And, in the US especially, there were relevant shifts in the political environment as well.

Of course, a multitude of factors not considered here will also have played a role in shaping eventual outcomes, or at least in shaping the manner in which things happened. No doubt, as I have already acknowledged, the life paths of specific individuals will have made differences, often fortuitously. And one especially significant development in the midst of all this was the emergence of cheap computing facilities, allowing the speedy development, initially of econometrics, and later of computer simulation models and the like. Indeed, the war effort likely induced a range of technical developments which facilitated the post-World War II mathematising project.

However, I do not need to recount the precise steps whereby, in the changed and changing, more conducive, environment, mathematical economics came to be accepted and indeed grew to become dominant. Nor do I really want to. For I am not suggesting a deterministic account, that what happened had to be. My aim is merely to indicate that, as it turned out, the environment of other relevant practices often had a very significant bearing in the determination of which practices in economics were, or were not, able to survive comfortably enough to flourish. Although there was no inevitability about anything that happened, it is clear, I think, that the changes in the environment made a significant difference, that the account of them sketched above has significant evolutionary-explanatory power. If the environmental shifts which occurred did not determine the outcome, they did serve to make what in the end happened more likely.

The drive to mathematise the study of social phenomena has for a long time been a dominant force in Western culture, a force that has been manifest in the academy. However, prior to the twentieth century, this drive was essentially constrained within the academy (or at least within parts of it that I have, with reason, focused on here) by the more dominant local view that research practices ought to be relevant to the object of study, that reality ought to constrain the analyses prosecuted. With the re-conceptualisation of mathematics in the early twentieth century, this constraint of reality on the mathematising project in the social sciences was lifted. Thus unconstrained, and aided by shifts in the political environment, a cheapening of computing power and other factors, the project came to achieve a spell of dominance, a spell that still continues.

An important point, here, from the perspective of establishing a Darwinian natural selection story, is that the conditions responsible for the noted shifts in the environment had little to do with the conditions generating the variety of research practices which economists followed. The conditions of variety generation and environmental selection are largely independent.

Feed-forward and feed-backward mechanisms

The topic of this illustration does warrant further comment at this point, however. For although the axiomatic approach allowed a postponing of the day when the axioms and assumptions were to be given a realistic interpretation, it was always expected that the day of reckoning would eventually come. Yet we are still waiting. Illuminatory successes, as noted throughout and detailed in Chapter 1, are hard to find. How, then, after more than half a century of the 'new' approach to mathematics, is modern mainstream economics managing to survive, despite its unhappy record in providing social illumination?

To this point I have focused very much on the role of the environment of all practices serving to select or reject those of mathematical economics. Of course, once any project has achieved a certain level of dominance the opportunity may well exist for its agents to affect variety and selection conditions in its favour. And if and where this occurs, we must recognise that the natural selection model is limited in its explanatory contribution, or at least that the degree of dependence between conditions of variety production and environmental selection is relatively high.

We have already seen, for example, how the dominance of Say's school made it very difficult for the early mathematising project to gain proper consideration, or even to get started, and how the influence of Laplace made it difficult for any endeavour that did not conform to the standard model(s) of physics. These are possibly best viewed as cases of the feed-backward version of the PVRS model having some relevance, of selection conditions likely affecting the variety in play.

There are also numerous historical examples whereby the feed-forward version of the PVRS model is appropriate, of variety-generating factors influencing, or at least being brought to bear on, an attempt to influence the selecting environment as well. One such is Walras' well known attempt to publicise his own approach. He appealed not just to Poincaré, but to almost any economist or (more often) physical scientist or mathematician of influence, who might find an interest in it. As Ingrao and Israel note:

> An examination of Walras's published correspondence provides confirmation of the turning point reached in 1874. It was precisely in that period that he began an intense promotional campaign largely through his letters in an attempt to open channels of scientific exchange and possibly to win pupils and create a number of 'Walrasian schools'. His method was to establish networks of correspondents in various countries (Britain, the United States, Germany, Austria, Italy, and France). His greatest efforts were, as usual, directed to his home country and now in particular to the scientists, while not neglecting his traditional relations with economists. A brief glance immediately reveals where he found listeners and where not, where interest was sometimes followed by disappointment. While the German-speaking world proved fairly indifferent to mathematical economics, greater interest was displayed in Anglo-Saxon circles, albeit only amongst economists. In this sphere, his most important exchanges were with Jevons and Edgeworth, and both brought disappointment and difficulty.
>
> (1990: 148)

And if a century ago, possibilities for new approaches to mathematising the study of social phenomena, or for influencing the environment of selection, were rendered difficult, today the boot is on the other foot. In modern times it is the traditions that maintain realisticness or social illumination as the primary goal that mostly fail to receive a sympathetic hearing.

In other words, I think it is fair to say that, within the modern economics academy, there are instances where this mathematising project, now the mainstream tradition, maintains its position of dominance by closing off lines of intellectual competition, where it manipulates conditions both of variety generation and environmental selection. During the period of the dominance of mathematical economics, for example, we have tended towards a position where university lecture courses in faculties of economics in many countries cover little more than methods of formalistic modelling (especially at the postgraduate level), where most journals regarded as prestigious have acquired gate-keepers who effectively bar non-mathematical expositions, where appointments and promotions in academic economic departments and the like are heavily biased in favour of (econometric, micro- or macro-) modellers, and so on. I do not suggest that this is done with ill intent. The ways of proceeding regarded as standard or proper, along with the reward system supported, merely reflect the values that the dominant group of the day have come to accept.[18]

In my experience mathematicians, philosophers and other social scientists who are aware of the situation of modern economics side heavily with heterodox criticisms of the (concentrated emphasis on the) mathematising tendency within economics (though this often means they [erroneously] regard it as not a serious subject, even in potential). However, the mainstream of modern economics preserves itself in a situation of significant isolation from other disciplines. And until recent times, at least, such a situation has appeared sustainable. To the uninformed, the mathematical emphasis gives an aura of technical sophistication that is perhaps intimidating, esoteric, something to be left in the hands of economist experts,[19] certainly culturally accepted and admired; whilst the degree to which the project dominates the modern discipline encourages the response that surely so many people (most mainstream economists) cannot be wrong.

Yet nothing stands still, especially in the social realm. The McCarthyite period is past. Certainly in many countries there is nothing resembling it in place. Further, the impetus gained to the mathematising project from recent advances in computer technology appears to be petering out. In these circumstances we might expect the emergence of forces working to change the academic balance in the direction of prioritising realisticness, despite the mainstream's hold on positions of power. And significant changes do seem to be happening. Whilst the heterodox groups persist in making significant contributions, other tendencies are in train. Student enrolments in economics faculties are currently in decline in many parts of the world (see for example Abelson 1996; Chote 1995; Kirman 2001; Parker 1993; Pisanie 1997). This has certainly coincided with the growth of business schools, and a reorienting of departments of human geography, sociology and the like, which now provide opportunities for people to teach and study aspects of life considered to be economic without the constraint of it all having to be carried out in a formalistic fashion. It seems likely, certainly possible, that such pressures will lead to a more pluralistic reorientation sooner or later.

Overview and further questions

I hope that I have by now covered enough ground to indicate that the rise to prominence of the mathematising project in economics conforms (or has aspects which conform) to a significant degree to the (Darwinian) evolutionary model, to the natural selection metaphor. It is indeed a success story for the practices concerned in terms of their eventual rise to, and continued, dominance. But, it does not appear to be a story of relative success by any wider or more laudatory criteria. In fact, if measured against the criterion of progress in knowledge and understanding of the social realm, many observers, as we have seen, continue to conceive modern economics as something of an unfortunate episode.

The example discussed here illustrates that any social process that does manifest evolutionary tendencies of a 'natural selection' sort will almost inevitably be one of continual accommodation and resistance, attraction and rejection, fit and mismatch, harmony and disharmony of subject and object or of 'individual' and environment, as changes in each interact with the other, as new practices emerge, and selections and selecting environments adjust. The social evolutionary process, then, will inevitably be one of shifting, slipping and sliding.

There can be no presumption that any *a posteriori* underlying direction of longer-term change is necessarily irreversible, of course. We are sometimes encouraged to think of the development of life on earth, including the emergence of human beings, or of developments in some branches of knowledge as, by and large, stories of irreversible progress. But there is no reason to suppose that all evolutionary episodes conform to such examples if so interpreted. Reversals of fortune are always possible. Such a reversal, of course, is precisely the outcome many heterodox economists are attempting to facilitate in the context of modern economics. The aim is so to reorient the discipline. It is to reinstall the goal of explanatory adequacy, even of truth, as primary once more, as part of a process of seeking a more pluralistic forum.

Of course, reversals in fortune are not unheard of even in the biological realm. Indeed they are rather common. I referred earlier to the varying fortunes of spotted grey and dark moths in the UK in the nineteenth century. In particular I noted how, with nineteenth-century industrialisation, pollutants killed the lichen on the trees in question and rendered the bark a dark colour, leaving the spotted grey at a relative disadvantage compared to dark moths because more easily recognisable to moth-eating birds. With the increase in pollution control in the twentieth century, however, lichen is again growing on trees in relevant areas, and I understand that once more the dark moth is on the decline relative to the spotted grey.

The opportunities (noted at the end of the previous section) for students to study economics without the constraint of reducing all to formalistic modelling, in business or management schools, departments of human geography, sociology and the like, and the openings equally provided for researchers more interested in social illumination, may mark an analogous case of re-switching in the environment of academic economic practices. Recently, students at some of the elite schools in France have begun a protest against the excessive mathematisation of the modern economics discipline, a protest that appears to be drawing significant support worldwide (for an overview see Kirman 2000; and especially Fullbrook 2003, forthcoming). Perhaps it will all make a difference.

However that may be, the evolutionary model does seem capable of providing a framework for understanding certain significant aspects of developments in the modern economics academy. Of course, the explanatory sketch provided here, though an extension of an argument found in previous contributions, remains (like any explanatory account) somewhat partial. Indeed (and again as with any explanation) new questions are thrown up by the answer(s) suggested. For example, why did the mathematising tendency not take off in a bigger way in the other branches of social science? Is the fact that most measurable social phenomena are regarded as 'economic' sufficient to explain this? And why in the last fifty years especially, have *specific* forms of mathematical economics (and not others) taken off, and why have they taken off when they have? For example, why has game theory risen to prominence only relatively recently, given that the basic principles were developed rather a long time ago?

What, in turn explains the explanation supported here? Specifically, what explains the *attractiveness* of conventions or edicts of the 'use mathematics' sort? Is the enduring place of mathematics in Western culture, with its very significant effect on the aspirations of modern economists in particular, solely due to the continuing successes of mathematical methods in numerous disciplines other than economics? Or is there also a deeper psychological explanation, turning, perhaps, on a fear of accepting the openness of society (and indeed of reality in general), the consequent fact of pervasive and fundamental uncertainty, and so the limited scope for predictability in life and thereby for control over what happens?[20] And, if the latter putative psychological mechanism is at all contributory, there arises the interesting supplementary question as to whether, as some suspect[21], its influence is significantly gender-differentiated.

I postpone setting out my own answers to questions such as these to a further occasion. But, whilst most answers to questions can generate yet more problems or puzzles to be resolved, the truth is that the historical documentation and explanation of the mathematising tendency in economics is a task that largely still lies in the waiting.

Here I have merely provided a sketch of what I believe is one set of explanatory ingredients in the history of modern economics. But it is an important set in that the features noted seem, in some significant part, to account in a coherent way for the varying fortunes of the mathematising project overtime, including its relatively recent rise to prominence, and indeed continuing dominance, in the absence of any obvious measure of relative success over and above the fact of its current widespread acceptance. Moreover, this explanatory coherence is achieved in a context where, currently, it is difficult to find, or easily imagine, any convincing alternative explanatory story.