Part III How well does established theory work

Preface to Part III

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The perspective in which the contributions to this book are generally written is, to different degrees, heterodox as compared with the assumptions, the methods and even the style of most contemporary economic analysis, whether theoretical or applied. Surely, this requires some justification. After all, many of the issues discussed in this volume also have a history of analysis within the standard discipline. And several economists would also be prepared to argue that many other topics discussed here, which currently draw scattered or no attention at all, could be handled within the framework which I shall call, 'neo-classical'. Of course, 'heterodoxy' acquires much more significance, and in some circumstances is even epistemologically required, if one is able to show either that it grows out of the preceding scientific achievements, or that some more radical change in theories and methods is needed in order to account for certain classes of phenomena. The contributions to this section, in different ways, do a bit of both. The broad heading they come under is the question whether a 'neo-classical' approach can be applied to the interpretation of economic dynamics and, at the same time, give reasonable explanations of the processes of coordination among economic agents in non-centrally planned economies. There are different ways to attempt an answer to this question.

First, one can check the analytical power of the results which can be derived from a theory. The more 'unlikely' the properties or predictions that are generated, the more 'powerful' the theory. Karl Popper would say that the power of a theory depends on the notional states-of-the-world or interpretative conjectures that it *excludes*, so that, to give an example familiar to the profession, establishing that an equilibrium exists, is unique, stable and 'is this one' is a powerful theoretical result, while saying that any point in the relevant space can be an equilibrium is dangerously bordering on tautology.

A complementary way of assessing a theory is by checking the boundaries of the sets of empirical phenomena to which it can be presumed to apply, exploring the consequences of relaxing the most restrictive assumptions, and investigating the implicit hypotheses that it makes on the nature of the phenomena that it tries to explain. In the first three chapters of this section both routes are pursued, and the general conclusion that can be drawn, in my view, is that indeed there are extremely serious problems in the use of 'neo-classical' tool-boxes and models in the analysis of dynamic

economies, or even economies that are not *stricto sensu* dynamic but still sufficiently complex.

For clarity, let me outline what I mean by a 'neo-classical theory'. In my view, the 'core' of the *strongest* version of the theory embodies the following hypotheses:

(i) the behaviour of the agents can in general be characterized by substantive rationality (literal maximization of something or approximations to it), or, alternatively, market processes are such as to select the 'maximizers', whether they know it or not;
(ii) the economic system is characterized, in the final instance, by some sort of scarcity;
(iii) the nature of the states toward which the system converges are generally path-independent and behaviour-independent (so that history does not count very much);
(iv) non-intentional and counter-intentional outcomes of interactions, and positive feedbacks (such as, for example, increasing returns) are, at the best, weak;
(v) uncertainty, when it occurs, can be reduced to incomplete information whereby the agents can still behave 'rationally' by generating probabilities with which they make maximizing calculations;
(vi) extra-economic institutions do not count in shaping economic conducts and performances;
(vii) markets embody processes which make them converge to some sort of equilibrium;
(viii) technology essentially consists of freely available information (production possibility sets). This implies that
(ix) the agents are identical (apart from their preferences and endowments).

Of course, only the purest neo-classicist would accept all these assumptions and beliefs as empirical generalizations. Probably the most resilient ones are (i) and (v), or 'rationality'. However, if the theory is robust and sufficiently 'progressive' in its interpretative power, it must also prove that at least some results obtained under the most restrictive circumstances can be 'carried over' to analytical set-ups in which some of the assumptions are relaxed. The arguments of the chapters that follow show that, in fact, this is *not* generally the case.

The Coricelli–Dosi chapter discusses the recent attempts to interpret macroeconomics in terms of equilibrium dynamics of an *implicit* General Equilibrium world. The 'New Classical Economics' has certainly been a brave epistemological gamble to explain macro regularities in the purest neo-classical fashion, without empirical '*ad hocs*', institutions, etc. However, the analytical results do not support the claim: one cannot derive, in general, determinate and 'orderly' properties of dynamic economies even under the most restrictive assumptions about the nature of production activities, using only given (and uniform) technologies, given preferences and also a 'rationality' principle. This is when the multitude of agents is 'compressed' into a single representative agent. Moreover, it can be shown that such a reduction is generally illegitimate if one wants to maintain a microeconomic foundation with many agents, diverse at least in their preferences.

Lippi tackles a similar problem from a different angle and, in his difficult but important chapter, shows that aggregating agents each of which behaves according to very simple and stable rules may indeed produce very complex macro dynamics.

This is a result that applies in general to macro phenomena, and is certainly disruptive for any neo-classical interpretation of macro patterns based on their 'backward projection' onto micro behaviours derived from maximizing decision rules; but it also represents a challenge to the more 'institutionalist' and 'evolutionist' approaches suggested elsewhere in this book. What kind of micro behaviours must (heterogeneous) agents present in order to yield relatively ordered macro regularities? Clearly, especially on the side of production activities, relative 'macro order' must be related to the selection processes discussed in other sections of the volume. However, it must also depend on behaviour-governing rules and institutions. In this respect, Heiner's chapter demonstrates the general plausibility as well as the *superior efficiency* of rule-governed behaviour compared to rigorous attempts to 'maximize' in all those circumstances in which the information-processing capability of the agents is—as is empirically plausible—significantly less than perfect. Remarkably, his proof also applies to those in fact rather simple environments in which 'rational behaviour' in the neo-classical sense is easily representable, equilibria notionally exist, technologies do not change, etc. In this sense, Heiner's chapter is also an important *negative* result about the domain of applicability of standard rationality assumptions and a powerful argument in support of the need for detailed observation-based analysis of behaviour and institutions, such as presented in other parts of the book.

There are, of course, many other domains of analysis on which the positive general conjectures, results and interpretations presented in this volume converge or overlap with the neo-classical approach. Some are mentioned in the chapters that follow and elsewhere in the book. Others have been neglected either for obvious reasons of space or because we considered them to be quite well known.

For example, careful *caveats* on the use of standard General Equilibrium Theory as a positive interpretation of empirical coordination processes have been made by its major contributors, such as Arrow and Hahn, who have also highlighted the theoretical limitations of the adjustment processes needed in such a framework to maintain some sort of economic order.

At least since the early 1970s (cf., for example, Spence) it has been established that even in models consistent with most neo-classical assumptions, initial conditions (and thus, implicitly, history) may count. The importance of history in economic processes has also been emphasized recently by Hahn. This acquires even greater validity if, as often done in the contemporary literature, one allows for various sorts of non-linearities.

Stiglitz (and several others) have intensively explored imperfect information set-ups. Even when agents are represented as maximizers and technology is equated to information, he shows that, in a variety of domains, institutions governing incentive- and information-structures are of paramount importance in determining system performance. A huge literature, based on the theory of industrial organization and game-theoretic analysis, hints implicitly or explicitly at the crucial role of mechanisms of expectation formation, initial conditions, organizational contexts, the nature of the technologies, etc. Moreover, Williamson has explored the implications of information-related and incentive-related market imperfections, producing a quite general theory of economic organization.

The implications that can be drawn from these scattered references, together with those discussed in more detail in this section are, in my view, abundantly sufficient to justify exploring the properties of environments where *none* of the above 'neo-classical propositions' apply—which, incidentally, I believe to be generally the case when the economy is a rather complex and changing structure involving changing technologies. But, if this is so, why hasn't the economic discipline made greater efforts in this direction?

The last chapter of this section by Clark and Juma starts from a similar point of view and asks why economics has not developed as a fully 'evolutionary' discipline. This chapter is an exercise in the history of economic ideas which tries to trace the influence on the economic tradition of some of the major intellectual currents of Western culture. They see these as originating in the natural sciences (Newton and Darwin) and now undergoing a rapid extension in theories of complex systems, disequilibrium dynamics, theoretical biology, etc.

6 Coordination and order in economic change and the interpretative power of economic theory

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> 'I . . . side with Heraclitus in arguing that you could not step twice into the same river, for new waters are ever flowing on to you'. It is the appearance of stability that is illusory; just look a little closer and wait a little longer. We Herclitus types find it difficult to understand what the Ecclesiastes types—who think that 'there is no new thing under the sun'—are talking about, what with the universe expanding, the continents drifting, the arms race racing and the kids growing up. The observed predictive performance of economic models also seems to us to be considerably more consonant with the Heraclitus view than with the alternative [Winter, 1986 p. 428]

Introduction

This chapter discusses the ability of economic theory to interpret dynamic economies, and in particular those undergoing technical change.

In chapter 2 as well as in the contributions to Part V of this book, arguments are presented for an evolutionary approach to economic analysis in which the process of coordination among agents is intertwined with the processes generating change of various sorts. Here we shall address the question of whether such an analysis of dynamic economies can also be undertaken by separating—on theoretical grounds—the *coordination problem* of an economy, typically represented as a stationary system, on the one hand, from the interpretation of *dynamic factors* on the other. That is, can we analyse the problems of static allocation on the safe assumption that they are dynamics-independent? Can we reduce problems of change to exogenously determined changes in the parameters of the general equilibrium model?

A good part of the economic discipline, especially in the post-war period, has essentially explored three theoretical perspectives. First, the General Equilibrium tradition focused upon the *theoretical possibility* of coordination between agents who are uniform in terms of decision procedures (maximization, etc.), but diverse with respect to initial endowments and tastes. The exploration of the existence, determinacy and stability of equilibria under more or less restrictive assumptions (e.g. completeness of contingency markets and information, convexity of production possibility sets, etc.) has been in many respects a fascinating attempt to assess the power of the 'Invisible Hand' in a highly stylized, stationary and perfectly competitive environment defined in terms of some 'fundamentals' of the economy (given technology, given individual preferences and a universal maximizing decision procedure for individual agents).[1]

The other two major analytical perspectives are more directly macroeconomic-based. One of these approaches—recently rather out of fashion—focuses upon 'stylized' aggregate regularities (e.g. the patterns of investment, consumption, etc.) and 'explains' them on the grounds of both the 'fundamentals' of the economy and *ad hoc* (in principle, empirically derived) assumptions about context-specific behaviour, adjustment lags, etc. So-called 'neo-Keynesians' basically share this methodology.

Finally, some current macroeconomic analysis (of which possibly the most fashionable is the so-called 'new classical macroeconomics') attempts to 'carry over' to macroeconomics an *implicit* Walrasian equilibrium microfoundation and somehow 'explain' macro variables solely on the basis of a maximizing principle of behaviour and the fundamentals of the economy.

The principle of rationality and the role of the market as coordination mechanism have been extended to the study of dynamic economies. This programme has mainly constructed models of equilibrium dynamics, i.e. models in which equilibrium holds at each point in time, while their (supposedly General Equilibrium) microfoundations are, so to speak, squeezed into single ('representative') agent formalizations.

Of course, theories do, and *must*, abstract and simplify. Indeed, one of the criteria on which the analytical power of various theories can be assessed is the simplicity and degree of generality of their abstractions. However, one must always ask questions such as: how robust are these abstractions? What are the domains of interpretative applicability of the models? For our purposes, the assessment of the analytical results and perspectives of the neo-classical research programme in relation to the understanding of coordination and change in market economies involves two major questions, namely (a) can a model based only on the 'fundamentals' (given technologies and tastes) and on a rationality principle for individual choice reveal to us some fundamental properties of economic processes which hold irrespective of the (history-bound) specification of, for example, particular behavioural rules, institutions, adjustment processes, etc.?; (b) can one incrementally build upon the basic static model and apply it to the analysis of environments characterized by technical change and, more generally, non-stationarity? For an affirmative answer to these questions and in particular to the latter, one should require that, at least under the highly simplified conditions generally assumed by these models, the theory should be able to (i) generate and explain 'order' at the macro level; (ii) handle micro diversity (otherwise one of the major distinctions between micro and macro would disappear); (iii) define an adjustment process leading to the equilibria studied by the models.

We shall argue that, in fact, the project of building dynamic models with economic content and descriptive power by relying solely on the basic principles of rationality and perfect competition through the market process has generally failed. In order to give economic content to equilibrium 'macro' models, we shall argue, one has to sacrifice, in fact, the decentralization of the decision-making processes of economic actors, which is obviously a fundamental premise of the theory.

Conversely, if one sticks to a 'decentralized' representation of diverse actors, it seems hardly possible to retain any robust analytical result on economic coordination whenever one introduces any sort of dynamics, or even relaxes the most demanding assumptions about the perfection of the markets and information sets (needless to say, any innovative environment is *necessarily* characterized by, for example, asymmetric information, non-perfectly competitive markets, etc.). Thus one goes back to a somewhat Schumpeterian dilemma, namely, can one find a sort of division of labour between General Equilibrium theory—meant to explain static allocative processes—and evolutionary theory—meant to deal with dynamic processes? Such a complementarity is implicit in the 'Classical Defence' of equilibrium models and of 'as if' assumptions on literally maximizing agents (see Friedman, 1953, and, for a critical discussion, Winter, 1986). This conjecture has been recently revived by Lucas (1986). According to this view the realm of economic theory (identified with General Equilibrium analysis) is that of steady states or, in any case, regular repetitive environments; adaptive processes, describing dynamic situations, are instead complex, irregular, disturbed by a vast number of factors specific to individual actors, industries, countries. Economic theory—it is claimed—cannot be concerned with these 'noisy' problems. It remains crucial, however, to show that from such a noise the regular stationary equilibria reflecting the rationality of behaviour and expectations of economic actors are selected.

However, we will show, first, that *general equilibrium models with stationary preferences and technologies and with rational expectations do not in general yield simple and regular outcomes. Second, we will show that in general, rational expectations equilibria are not the stationary state of dynamic processes arising from adaptive rules.*

In the sections that follow we shall discuss the attempt to 'explain' the levels and changes of macro variables on the basis of a direct transposition of micro behaviours (maximization) and the fundamentals of the economy We shall then explore the coordination power of the markets (or lack of it) implicit in neo-classical macro models, and the impossibility of deriving, in general, robust results on macro-order simply on the grounds of the rationality principle, endowments and given tastes. On the contrary, complex dynamics and unpredictability of equilibria may well emerge even

in simple competitive models. Can neo-classical macro models (of the rational expectation kind) be considered the results of adaptive processes? The section 'Rational expectations equilibria and adaptive processes' will discuss why this is not generally the case. Thus one must investigate the characteristics of individual behaviours and of economic environments which account for a relative order in *both coordination and change*. We shall suggest that some of these characteristics can be found precisely amongst those factors underlying the main difficulties of neo-classical theory in dealing with economic dynamics. Indeed, several features that are sources of theoretical problems for the neo-classical view are instead the main ingredients of a positive theory: diversity, heterogeneity of agents, non-linearities, continuous change, and hence non-stationarities, the role of learning, beliefs, the importance of 'history' and of 'contexts' and, situation-specific behaviours, are all fundamental features of the evolutionary, self-organization approach to economic dynamics.

Aggregation, 'representative agents' and competitive mechanisms

According to a taxonomy proposed by Malinvaud (1981), any attempt to place macroeconomic analysis on a solid microeconomic footing should go through the following three phases: (i) the microeconomic study of agents' decisions and, in addition, the interaction among agents and thus the constraint on individual behaviour of other agents; (ii) the aggregation of behaviours, i.e. the study of the macroeconomic implications of micro-behaviours and the deduction from the micro choices of the laws of macroeconomic behaviour; (iii) the comparison of the findings of the theory with empirical data.

It seems to us that the current fashion in macroeconomic theory relies upon the mere direct transposition to the macroeconomic level of the results obtained in the microeconomic analysis of the behaviour of a single agent ('the representative agent'). The phase of 'aggregation' is completely neglected and the study of micro interactions is carried out in a very peculiar way: in fact, as we shall see, very little is left of 'interactions'. This approach implies, in terms of empirical testing, that the observation of empirical macro data is assumed to be directly consistent with microbehaviours; in econometric language, macroparameters are taken to be nothing but the reproduction on a larger scale of microparameters.[2]

Of course, it is generally acknowledged that the mere knowledge of individual characteristics is of little help in predicting the outcome at the level of the whole system. The interaction among agents introduces a qualitative difference between micro and macro behaviours. In that version of neo-classical macroeconomics which calls itself 'new classical macroeconomics', the solution to this problem has to be found in modelling this interaction in terms of competitive equilibria. According to Lucas,

> it is the hypothesis of competitive equilibrium which permits group behaviour to be predicted from knowledge of individual preferences and technology without the addition of any free parameters . . . It is possible, we know, to mimic aggregate outcome of this interaction fairly well in a competitive equilibrium way, in which wages and manhours [Lucas is indeed referring to a competitive equilibrium theory of employment] are generated by the interaction of 'representative' households and firms. [Lucas, 1981, pp. 289–90]

Through a competitive equilibrium model, it is argued, the micro–macro link will be transparent, the aggregate outcome being predicted with precision from the knowledge of individual preferences and technology. As a consequence, there is no need to add any *free parameters* (the famous 'ad hockeries') to the structure of the model based on optimizing behaviour of fully rational agents. This type of model is claimed to be capable of pinning down macroeconomic equilibria which are consistent with both empirical observations and results of Walrasian general equilibrium theory—in particular with welfare theorems.[3]

The central message of such a research programme is that under *laissez faire* a competitive economy, not disturbed by destabilizing policies and/or exogenous shocks, will settle on stationary macro equilibria, resembling static Walrasian equilibria. In this way, not only the normative implications of general equilibrium models would carry over to macroeconomic theory, but the Walrasian approach would acquire an extraordinary descriptive power. What is the theoretical validity of this claim?

First, let us consider the theoretical implications of developing macroeconomic models—as is currently done—on the basis of optimizing behaviours of representative agents. Most of the recent developments in macroeconomics rely upon the simple assumption that economic actors are identical. This assumption is also accompanied by the identification of a macro model with a system with only one good. For an approach which claims to have put macroeconomics on steady microeconomic foundations, these are certainly heroic assumptions. By reducing the set of individuals to a 'representative agent' it is implied that aggregate behaviours are just a transposition of micro behaviours: qualitatively they do not differ. In this way the problem of aggregation of individual behaviours is hidden under the rug. It is a sort of paradox that a 'microfounded' approach to microeconomics, instead of shedding light on the complex nature of the link between individual behaviours and aggregate outcomes, has created so-called macro models as oversimplifications of the general equilibrium system (in fact, a general equilibrium model with only one agent and one good!). Moreover the assumption of 'representative agents' appears to lead to theoretical problems[4].

The difficulties arising in the aggregation of individual behaviours have been recognized in the economic literature since the last century, in the partial equilibrium approach to both the theory of consumption and the theory of production.[5]

In a general equilibrium setting the work initiated by Sonnenschein and developed by Debreu and others points out the important result that

> strong restrictions are needed in order to justify the hypothesis that a market demand function has the characteristics of a single-consumer demand function. Only in special cases can an economy be expected to act as an 'idealized consumer'. The utility tells us nothing about market demand unless it is augmented by additional requirements. [Shafer and Sonnenschein, 1982, p. 672][6]

As a consequence we cannot expect aggregate relations, which we have called macro relations (i.e. relations between aggregate variables), to reproduce on a larger scale micro relations. The link between micro and macro behaviour does not entail a simple enlargement of scale but a qualitative change of perspective. In the general competitive equilibrium framework the descriptive and predictive power of the results on individual behaviour for the aggregate outcome is extremely vague. In the textbook by Varian (1984), the Sonnenschein–Debreu theorem is interpreted as an indication that since the utility maximization hypothesis places no restrictions on aggregate demand behaviour and hence any continuous function satisfying Walras's law can be an excess demand function for some economy, practically *any* dynamical system on the price sphere can arise from a Walrasian general equilibrium model of economic behaviour.[7]

The analysis can be pushed further, and it can be shown that in general the price dynamics involved in the tatonnement story can give rise to a whole family of extremely complex and erratic dynamics (Saari, 1983). Even leaving aside the question of dynamics out of equilibria, it can be easily shown that asymmetries among individuals modify the dynamical representation of equilibria over time.[8]

Let us summarize the implications of the discussion so far. In many respects, the General Equilibrium tradition has undertaken the fascinating task of exploring the interdependencies of a decentralized economy through the axiomatization of 'selfish' individual motives and an extremely parsimonious use of ancillary hypotheses (on adjustment processes, institutions, etc.). Under the assumptions of the theory (which, one must admit, are quite restrictive, on information, competition, etc.), one has demonstrated the *possibility* (i.e. the logical consistency) *of coordination* via the 'Invisible Hand' of the market. This is, in the last resort, the meaning of the existence theorems. However, without further (and somewhat *ad hoc* or observation-based) restrictions, the results are not determinate enough to be, so to speak, 'carried over' to a synthetic macro representation which would hold irrespectively of any specific representation of the underlying characteristics of actual agents (in terms of tastes, distribution of endowments, etc.). These properties, together with the quite heavy restrictions that must be introduced in order to obtain stability and determinacy (local uniqueness) of equilibria in an explicit general equilibrium model highlight, in our view, the boundaries of the set of empirical economic phenomena which the neo-classical research programme, at least in its present form, can interpret. Certainly, the set does not include non-stationary environments, but neither does it include relatively orderly sequences of macro states. At the very least, the latter cannot be explained via a 'reduced form' neo-classical macro model without losing the 'interdependencies' of the market, the autonomy of the agents (in terms of beliefs and expectations) and/or the 'parsimoniousness' in auxiliary assumptions (and thus the generality of the models). We shall now discuss these latter issues.

Decentralization and the coordination power of the market mechanism

It may be enlightening to see how the assumption of homogeneity among individuals which underlies the concept of 'representative agents', extends to the characterization of the uniformity of expectations and beliefs.

We shall consider here 'rational expectations' models. In fact, if expectations are non-rational, aggregation problems become even more serious (e.g. for the purposes of a synthetic representative-agent stylization of macro phenomena, identical agents with different rules of expectation formation are different agents: aggregation necessarily involves situation-specific knowledge of the rules, their distribution among agents, etc.). Even in the 'rational' case, as Frydman and Phelps (1983) put it, 'an instantaneous transition to the new rational expectations equilibrium requires a perceived and actual unanimity of beliefs. Such consensus of perceptions cannot generally be achieved by individual agents acting alone in decentralized markets'. To circumvent the problem of forming expectations of other agents' expectations, the Rational Expectations school assumes that every agent forms his expectations on the basis of the equilibrium model and everyone expects that the other agents form their expectations in the same way. This apparently innocuous assumption is in fact very 'totalitarian' and also in marked contrast with the leitmotiv of the neo-classical theory of individual behaviour, whereby 'individual behaviour is not based on the collective consistency of plans, but on the assumption of individual rationality' (Frydman and Phelps, 1983). Assuming an *ex ante* consistency of plans either contradicts the fundamental task of neo-classical microeconomics aimed at demonstrating how market processes, *ex post*, make consistent the independent plans of agents based only on selfish considerations, or makes the theory plainly tautological in the sense that, in order to demonstrate the existence of coordination amongst agents, it assumes it *ex hypothesi*.

It is not far from the truth to say that the current neo-classical approach to the microfoundation of macroeconomics is based on a representation of the economy as a *centralized* system. The assumption of a 'representative agent' together with the rational expectations hypothesis (with homogeneity of expectations) is tantamount to an *ex ante* collective consistency of behaviours in which the market plays no role. In fact, most of the results of the 'new macroeconomics' pointing to the power of *laissez faire* in achieving desirable macroeconomic equilibria derive from an *a priori* exclusion of the analysis of the relationship—and potential conflict—between individual behaviour and aggregate outcomes in a decentralized economy.

Indeterminacy of equilibria: beliefs and behaviour-dependent equilibria

In order to have any 'positive' meaning, that is, in order to have some interpretative power, equilibria have to be at least *locally* unique. If an equilibrium is not locally unique it follows that there are several equilibria arbitrarily close to it. If this is the case it is impossible to carry out any exercise in comparative statics, and thus the dynamic analysis becomes meaningless (loosely speaking, one has a macro model that simply says that 'anything can happen'). For these reasons, a situation characterized by the absence of local uniqueness is usually defined as indeterminacy of equilibria.

The dynamic extensions of equilibrium models to macroeconomic analysis generally suffer from this indeterminacy.[9] As a consequence, preferences, endowments and technology alone may not suffice to determine the allocation of resources in a dynamic economy, even when perfectly competitive markets exist for all goods. In this context the possibility arises of a critical role for the *beliefs* of the agents as well as for an active government policy. Phenomena such as 'sunspot' equilibria or bubbles may be interpreted as a way of selecting particular equilibria from among the large number of competitive equilibria.[10] These equilibria are characterized by the fact that the allocation of resources depends on beliefs, i.e. on factors which are unrelated to the fundamentals of the economy, and that these beliefs will be confirmed by the equilibrium of the system; in other words, expectations of the agents are self-fulfilling. Note that this result is consistent with the assumption of 'rational expectations' and, unlike 'bubbles', it is also compatible with stability of the equilibria. The implication is that beliefs of agents not only are relevant in determining the equilibrium allocations, but also that—when they are stationary—there are no forces causing the system to move necessarily to equilibria reflecting only the fundamentals (as it is the case for 'bubbles' which, by their nature, will eventually explode). It should also be noted that the multiplicity of equilibria in the above sense in a decentralized economy implies the need for every single agent to form expectations not only about the realizations of economic variables, but also about the expectations of other agents. A decentralized economy is therefore caught in the vicious circle of an 'infinite regress' of forecasts about how others forecast the forecast of their forecast, etc., reminding us of the famous 'beauty contest' discussed by Keynes. Consequently, the power of the competitive economy to coordinate agents' behaviour, as described by these neo-classical macro models, in the absence of external intervention, appears to be very weak.

Beliefs matter in determining aggregate outcomes. Moreover, individual forecasting mistakes—or deviations from optimal 'rational' behaviour—matter, regardless of how small they are. Akerlof and Yellen (1985) show that individual behaviours which are only marginally non-maximizing induce a significant effect on macroeconomic outcomes. Small departures from perfectly 'rational' maximizing behaviour which result in only second-order losses to the individual, will nevertheless have first-order effects on real variables.

Complex dynamics and unpredictability in simple competitive models

Of course, the belief-dependency and behaviour-dependency of equilibria challenge the extreme (or 'pure') neo-classical research programme to provide an account of macroeconomic order without invoking empirically based assumptions about behaviour. They do not challenge, *per se*, the theoretical conjecture that neo-classical macroequilibria (no matter how belief-ridden) can provide a justification for the 'Invisible Hand' which pulls together and provides coherence to a dynamic economy. However, the idea that under *laissez faire* a competitive economy, unaffected by exogenous shocks and destabilizing policies, will settle on stationary equilibria resembling the Walrasian equilibrium, is radically challenged by a growing body of literature.[11] Simple economies—simple in that utility and production functions are 'regular'[12] and the economy is populated by 'representative agents'—in which the environment (technology, preferences, endowments) is *stationary* and *deterministic* show in many, far from special, circumstances an extremely complex dynamic behaviour.[13] The complexity of the dynamics is solely the result of the workings of the competitive mechanism, and is not due to friction and disturbances. It has been shown that the motion of the system may present forms of highly irregular dynamic behaviour which is called deterministic *chaos*.[14]

There are several definitions and properties of chaos which cannot be discussed here. One aspect is, however, of greatest interest to us, namely the fact that chaotic systems are extremely sensitive to initial conditions.[15] As a consequence, the behaviour of the model in the future cannot be predicted from a knowledge of its 'fundamentals', nor can it be predicted from a knowledge of its history. The system is thus characterized by a serious problem of unpredictability.[16] Of course, the result is devastating for the assumption of rational expectations, since it implies that the predictive possibilities of agents are necessarily extremely weak and imperfect even if the environment itself is deterministic. Moreover, the fact that in deterministic and very simple models, with stationary preferences and technologies, the 'rational' (perfectly optimizing) behaviour of agents who also have the gift of perfect foresight leads to such complex and erratic dynamics of the economy is a strikingly negative result. This throws serious doubts on its ability to interpret empirical macrodynamics which inevitably reflect the non-stationarity of technology, tastes, etc.. Finally, note that in these models erratic behaviour is an *equilibrium* phenomenon and not the movement of the system out of equilibrium: equilibrium is actually assumed *ex hypothesi*, while attempts to show how agents 'learn' how to get there have generally failed. We shall now turn to this issue.

Rational expectations equilibria and adaptive processes

There have recently been attempts to overcome the difficulties discussed in the two previous sections by suggesting that rational expectations equilibria can be seen as a steady state of adaptive processes. These are ostensibly an attempt to build a bridge between the neo-classical approach and some kind of evolutionary theory (Lucas, 1986). (Note that in this instance 'evolution' only hints at 'adaptation', since there is no 'selection': the economic consequences of 'mistakes' are ruled out.)

However, it can be shown that, in general, rational expectations equilibria *cannot* be seen as stationary states of adaptive processes. To put it differently, dynamic paths determined by adaptive rules do not generally converge to rational expectations equilibria (see Fuchs, 1979). Obviously, examples of models in which adaptive rules and learning processes lead to a convergence to rational expectations steady states may be easily formulated (see also Bray and Kreps, 1987). This convergence, however, is linked to particular structures of the system. A simple example may help in clarifying the point.

Let us take a system in which the rational expectations steady state is not an attractor of the rational expectations dynamics. If rationality of expectations is assumed even outside stationary states, hence assuming instantaneous learning (which is equivalent to neglecting the 'gradualism' of learning processes), the system will not converge to the rational expectations steady state. In this situation, by assuming that expectations outside steady states are formed in an adaptive fashion, the instability property of the steady state can be repaired: the rational expectations steady state becomes stable in the adaptive expectations dynamics. This case is shown in Figure 6.1, which is derived from a slight modification of Lucas's example (Lucas, 1986). Every point on the A-curve is an equilibrium for a given instant t. Any sequence of points $\{q(t)\}$ with $t=0, 1 \ldots \infty$, on the same curve is a rational expectations (or perfect foresight) dynamic equilibrium. Consequently, there is a continuum of dynamic equilibria, each of them indexed to a different initial condition $q(0)$, which the model cannot endogenously determine. We are thus back to the indeterminacy problem

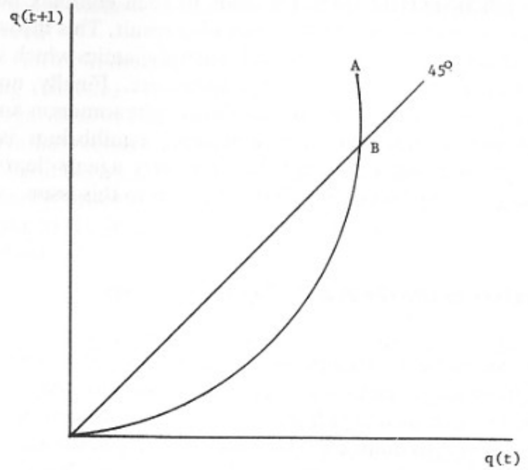

Figure 6.1

discussed earlier. Notice that there are two steady states, the origin and the point B. B is an unstable steady state, but it is the equilibrium point which reflects, so to speak, the 'predictions' of the theory. The locus of equilibrium points described by the curve A is obtained, for instance, from the dynamic solution of an overlapping generations model.[17]

The equilibrium dynamics of the system (see Note 17 for its derivation) is represented by the following equation: $q(t+1)=q(t)^2$. This is obtained by assuming perfect foresight, which allows us to substitute the actual realization to $Eq(t+1)$, that is, the expectation of $q(t+1)$. If we abandon this assumption in favour of an adaptive expectation function we obtain a different dynamic equation. For instance, if the expectation of $q(t+1)$ is based on a weighted average of current and past prices: $Eq(t+1) = q^{a}(t)\, q^{1-a}(t-1)$, with $0 < a < 1$, the dynamic equation of the system becomes $q^{a}(t)\, q^{1-a}(t-1) = q^{2}(t)$, which is equivalent to $q^{(1-a)/(2-a)}(t-1) = q(t)$. By propagating this equation one period ahead we obtain $q(t+1) = q^{(1-a)/(2-a)}(t)$. This equation gives rise to a curve which is a sort of mirror image of the A-curve, since $(1-a)/(2-a) < 1$. The new dynamics is illustrated in Figure 6.2.

The steady states are obviously the same as before; their dynamic properties are, however, inverted; the steady state B is now locally stable. For every initial condition $q(0)$ in $(0,1]$ the system will converge to B. All the dynamic paths reflect the *adaptive* expectation rule and they all converge to the rational expectations equilibrium B. It should be noted that the relation between adaptive processes and rational expectations equilibria depends on the specific structure of the model considered. The same result could have been obtained by changing the functional form of the utility function. For example, with a utility function different from the one underlying the model just discussed we could have achieved a steady state stable in the rational expectations dynamics, but unstable in the adaptive rule dynamics (on these issues see also Grandmont and Laroque, 1986).[18] In such a case the rational expectations equilibrium cannot be seen as a steady state of adaptive processes. In fact the contrary would be true: in a sense, steady *adaptive* expectation dynamics would be the realization of a rational expectation process!

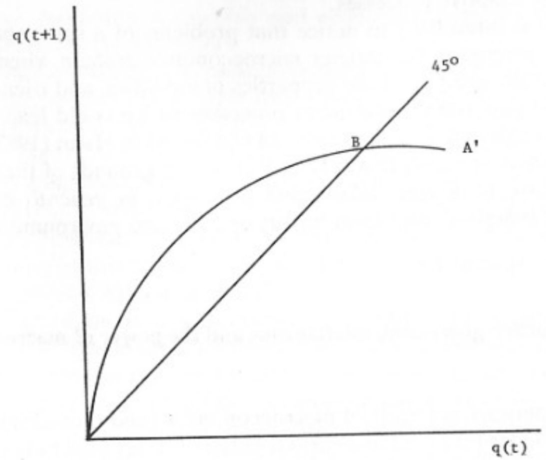

Figure 6.2

In general, the dynamic properties of the system are determined by the interplay between the structure of the system and the expectation function. However, the latter must be considered as a structural feature of the system: a change in the expectation function is in general indistinguishable from a change in utility or production functions. Borrowing a definition used by Azariadis (1983), a variation in the expectation function is 'observationally equivalent' to a change in preferences or technologies.

To summarize, once it is recognized that learning cannot be dismissed even in a rational expectations framework, it follows that one cannot dispose of a characterization of individual behaviour which is also 'situation-' or 'environment-specific'. However, inclusion of these factors into the theoretical picture makes the rational expectations assumption quite irrelevant: the rationality principle together with the nature of the fundamentals of the economy are *unable to determine the dynamic* processes of the economy, nor, in general, even the asymptotes of out-of-steady-states adaptive processes.

Finally, it is interesting to notice that problems of a somewhat similar nature also emerge in the stricter microeconomic domain whenever one goes beyond the analysis of the properties of equilibria and tries to model the (out-of-equilibrium) adjustment processes which could lead there. A recent, thorough and bold discussion can be found in Hahn (1987): 'whatever the form of learning that takes place on the grounds of the information that markets deliver, adjustment processes, *in general*, cannot be represented independently from history-specific and environment-specific conditions.'

Stabilizing, order-generating mechanisms and the power of macroeconomic theory

Most contemporary accounts of macroeconomics (and thus also of aggregate economic 'order') lead to a curious paradox. They generally start with an act of faith in both the 'Invisible Hand' and in the substantive capabilities of individual agents to process information and 'choose' correctly and freely—constrained only by their endowments—and end up with results that show a very crippled Hand, incapable of orderly coordination even in extremely simple environments. Moreover, note that these results are obtained despite an increasing attribution of rational competence and information processing power to individual agents.[19] Certainly, we believe, the attempt to 'explain' macroeconomics solely on the basis of some kind of 'hyper-rationality' of the agents (Winter, 1986) and the (pre-analytical) fundamentals of the economy (i.e. given technology and tastes) has failed.

There seem to be three ways out. First, one may, so to speak, 'stabilize the models' by working one's way 'backward', from the nature of the results one obtains to the restrictions one needs to impose in order to obtain the results one prefers. Just to give an example: the interpretation of the saddle-point properties of rational expectations macro models is quite instructive.[20] The structural model is characterized by a serious problem of instability: for every initial condition different from the one which puts the system on the unique convergent path, the system will diverge from the steady state. The heuristic strategy generally adopted to solve the problem is then to rule out, *a priori*, the instability of the solution, often justifying the choice on the ground that the world is not unstable. In the last instance one imposes stability to the solution (not to the model, which is highly unstable). One may forecast that, for example, the exploration of the conditions under which models generate chaos may lead towards a somewhat similar methodology: that is, use the results precisely to *assume away* these conditions. Frankly, it seems to us that this intellectual strategy is quite close to that scientific fallacy which epistemologists call '*explenans explenandum*': you assume your theory to be true and use your results to prove that it is true. Certainly, following this route no proper refutation is possible and economics becomes more akin to theology than empirical science.

The second strategy is to rediscover the *ad hocs*—auxiliary assumptions empirical character—plug them into a model which maintains a more or less strict neo-classical ascendancy (micro choices based on standard maximization, tatonnement assumptions about adjustments, etc.), and estimate econometrically using these auxiliary assumptions and 'free parameters' (e.g. expectation formation, etc.). This is, loosely speaking, that 'out-of-fashion' approach to macroeconomics mentioned in the introduction to this chapter, which the Rational Expectation school dismissed as theoretically unfounded.

However, there is a general point: in this kind of macroeconomic analysis, as well as in other domains, the results depend very little either on the 'core' behavioural model, based on individual rationality (in neo-classical sense) or on its implicit microeconomics, and very much on the auxiliary assumptions themselves (Simon, 1986), which, to repeat, are of an empirical nature, although generally introduced with the most casual empiricism.

The third approach is, broadly speaking, an 'evolutionary' and 'institutionalist' one. In a way, it starts with similar considerations, thus acknowledging that various institutions *structure* individual decision-making and collective coordination, but *explicitly* considers these institutions as an essential part of the interpretation of the (varying degrees of) order which one observes at the macro level.

An interesting example of the very simple micro 'rules' which order behaviour is shown by Heiner (1983 and Chapter 7 of this book). According to his view, the main source of unpredictability, and also of irregular, chaotic dynamics (see Heiner, 1986), is to be found in the assumption of perfect rational agents on which the conventional neo-classical approach rests. Introducing a form of 'imperfection' of agents, summarized by their 'competence gap' in correctly detecting environmental signals, he is able to show that previously unpredictable actions become regular and consistent with empirical observation of economic behaviour. Notably, Heiner's analysis applies to environments in which individuals *could* notionally be literal maximizers. As mentioned in Chapter 2 and analysed in Dosi and Egidi (1987), there is a wide class of (empirically very plausible) environments in which agents *cannot* adopt maximizing decision processes due to the environmental uncertainty associated with particular kinds of non-stationarity and/or the complexity of problem-solving tasks. Thus, rather general *rules* of behaviour inevitably emerge. Macro models, of course, should embody as their implicit or explicit microfoundations: (a) generalizations of these rules—whenever they are sufficiently stable—and (b) some theoretical propositions about how individual 'rules' aggregate and/or interact in order to produce the 'macro' (observed) patterns.

In a sense, macroeconomics neo-classical models attribute, at the same too much and too little freedom and power to individual agents: too much because their behaviour is totally unconstrained (apart, of course, from the endowed resources) in judging, computing, deciding; too little because, given their preferences, there is only one 'right' thing to do—all possible opportunities are exploited and it is hard to imagine how non-stationarity in technology, etc., can come about, except via some exogenous input.

Precisely the (observation-based) specification of 'structures', which encompass not only 'outside' institutions such as governments but also institutional aspects permeating individual behaviour, appears to be a promising way of avoiding the typical problems of unpredictability emerging from ahistorical, structure-free models. Social habits, routinized behaviour of firms, and contracts are a few examples of structures which delimit the context of individual behaviour and shape adjustment processes (see, for example, Okun, 1981; Kaldor, 1985).

In many respects, several 'positive' (that is interpretative rather than critical) chapters of this volume represent initial contributions to the understanding of these phenomena. Pushing it almost certainly beyond what the author intended, we place a central theoretical importance on the conjecture that 'There are collective norms . . . that are very important in social action. People just do not maximize on a selfish base every minute. In fact the system would not work if they did. *A consequence of that hypothesis would be the end of organized society as we know it*' (Arrow, 1987, p. 233, our italics). Certainly, continuing this quotation, we also agree that '. . . we do not have a good theory of how these norms come into existence' (ibid.). It remains a major interdisciplinary challenge—to economists as well as organization theorists, sociologists, experimental psychologists, etc. However, we *do* have the *beginnings* of a theory of the *process* through which certain behaviours are selected and become dominant: combinations of learning and competitive selection contribute to these processes, as discussed at greater length in other chapters. Clearly, evolutionary/self-organization models focus precisely on these processes.

History, institutions and order in economic change: can they be explained within the neo-classical microeconomic framework?

One rather commonly held belief is that any alternative to the neo-classical research programme would have to be based on highly specific empirical observations and thus would not be very generalisable beyond its original context. Can the neo-classical approach avoid degenerating in this direction? Our foregoing assessment of the recent developments of neo-classical macro theory shows that it cannot. It is indeed recognized that without an explicit consideration of the specificity of economic behaviour depending on contexts, environments, initial conditions, of

learning processes and, we could add, of institutions other than markets, equilibrium models have no economic content or descriptive power. Unfortunately, the lagging belief in the history-free interpretative power of the models sometimes only helps in justifying the most 'cavalier' attitude toward the specification of the structure of the model (for convenience we assume convexity, or infinitely lived agents, or homogenous of degree-one production functions, or risk-neutrality,' etc.), which in turn crucially affects the results.

Certainly, both neo-classically inspired models and that of 'alternative' (e.g. evolutionary/self-organization) models frequently entail a multiplicity of equilibria which may or may not be stable (the former) and a multiplicity of 'evolutionary attractors' and asymptotic states (the latter). What then is the difference in the analytical power between the two, and why should one choose the latter, given that the former derive from a longer established formal tradition.

The first point we want to make is that, equilibrium models *cum* rational agents are simpler, but at the price of a very low economic plausibility of the assumptions.

After all, apples tend to fall down from trees whatever their initial condition, shape, colour, etc. Indeed, it would not be so bad if a theory would allow more than one stationary state for the apples, provided that it specified under what circumstances it went towards either one, but it would certainly be devastating to have a theory that allowed the apples to go in almost any direction.

The story is different for evolutionary/self-organization approaches. There one specifies *economically meaningful processes*, behaviours, initial conditions—in principle, based on observation-related generalizations—and studies the dynamic paths of the system. These paths may often diverge, and there may be a number of asymptotic states (see, for example, Arthur's chapter in this volume). Such a theory would, of course, be highly redundant and unnecessarily cumbersome in all those cases of globally stable equilibria such as falling apples, but it appears to be the more necessary the more 'history matters' and the more the specific features of micro agents matter, too.

The second point is that an evolutionary/self-organization approach can deal more straightforwardly with various sorts of non-stationarities, while 'equilibrium' approaches, as we argued earlier, appear to be ill-suited to the task.

If one believes that a sequential, evolutionary and institutional representation is required in order to account for the values and changes in macrovariables, it is difficult to escape the implication that *its microfoundations, too,* must be in some sense 'evolutionary' and 'institutional', allowing for variety, learning, mistakes, selection and imitation. This, is, of course, one of the main points argued in this book, with particular reference to technology and technical change.

Some conclusions

At a very general level, one tends to observe broad regularities in the values and/or changes of macro variables such as, for example, relatively regular patterns of growth of output per head and capital per head; roughly cyclical, although irregular, movements of employment rates possibly around longer-term trends; relatively steady patterns of income distribution, etc. (for an extensive list of both long-term and cyclical 'stylized facts' drawing on Kaldor and Mitchell, see Simon, 1986). These empirical regularities plausibly hint at some underlying process governing both economic coordination amongst agents (otherwise no regularity whatsoever could be expected to appear) and economic change (for, otherwise, no regularity, however rough, would be likely in the time derivatives). Moreover, there appear to be 'micro' regularities, some of which are investigated in this book, which are related to typical patterns of behaviour of the agents (such as firms, but also recognizable aggregates, such as whole industries or even countries), technologies, the ways they cope or even themselves generate change, their internal structure, and the ways they interact with the external environment. Micro-observations of behavioural rules such as those discussed at length in this book, have, on the one hand, a strong flavour of 'idiosyncrasy', specificity to contexts, periods and institutions. On the other hand, there are phenomena related to quite general features of technologies, competitive environments and organizations, discussed at length in other chapters, which seem to hold, in different forms, across industries and across countries. Overall, the micro picture conveys an impression of marked inter-agent diversity and environmental non-stationarity. Ideally, one would like to find a unified or at least consistent theoretical link between 'macro' and 'micro' phenomena.

In fact, at the 'micro' (or, if one prefers, 'partial equilibrium') level there are two distinct views which compete for the theoretical representation of environments characterized by innovative phenomena. The first view draws quite closely on the way neo-classical economics handles choice, allocation and equilibria in stationary environments.

The other view (call it the evolutionary/self-organization approach), discussed at greater detail in the chapters by Dosi–Orsenigo, Allen, Arthur and Silverberg, takes in many respects an opposite stance and focuses on behavioural diversity, out-of-equilibrium processes, various sorts of externalities, environmental selection and unintentional outcomes of decentralized decision-making. In the former view, order in change comes from the fact that (i) in one and every period the system hits some scarcity constraint; (ii) the environment is 'transparent' enough for the agents to make 'rational' choices; and (iii) there are some processes which, although they have never been specified, ensure the *ex ante* consistency of individual strategies. Conversely, in the evolutionary/self-organization view, relatively ordered patterns of change come from (i) technological and institutional factors which form the basis of expectation formation and orient decision under general conditions of uncertainty and complexity; (ii) selection processes which limit (but do not eliminate) the variety of 'visions' and behaviours of the economic agents; and (iii) the continual generation of new sources of increased efficiency and new product markets.

It is our impression that there is a growing overlap between the stylized phenomena addressed by traditional economic theory—what we called the equilibrium/maximization approach and the emerging evolutionary/self-organization view. For example, within the former approach, various 'micro' and 'macro' models have demonstrated the relevance of beliefs in terms of attained equilibria; extensive form game models hint at the importance of institutions governing repeated behaviours; explicit accounts of market signalling even in simple set-ups highlight the role of initial conditions; theoretical accounts of externalities yield path-dependent models, etc.

This loose convergence in the underlying phenomena which the models address highlights both analytical complementaries and more radical differences in the frameworks underlying these models. Certainly, we agree with Hahn (1984, p. 140) that the study of asymptotic (equilibrium) states of evolutionary environments is also of theoretical importance. It helps in showing under what circumstances such states are actually the attractor of the evolutionary process. Here possibly rests one of the major complementaries between 'equilibrium' and explicitly 'evolutionary' analyses. However, as we have argued in this chapter, one of the few robust results that can be obtained by relaxing some of the most demanding assumptions of the 'unrestricted' equilibrium/maximization model is precisely the *lack of robustness* of its results (in terms of existence, determinacy, stability, and Pareto-optimality of its equilibria).

Conversely, the emerging evolutionary/self-organization approach takes as its 'building blocks' precisely what to neo-classical theory are 'extensions' or 'exceptions', such as externalities, increasing returns, non-stationarity, different and coexisting priors held by agents, complex strategic interactions without dominant strategies, and fundamental uncertainty. It might even be argued that the evolutionary/self-organization approach *predicates* its empirical adequacy on precisely these conditions.

Of course, one could assume that micro diversity, 'noise', mistakes, etc., cancel out in the aggregate by a sort of law of large numbers. Or one may assume that they simply represent empirical imperfections which nonetheless are in some sense 'ordered' by, or tend towards, a (theoretically) much simpler stationary state. Our position, however, is that detailed, observation-based analyses of behaviour, institutions and economic processes are *unavoidable* ingredients of both micro *and macro* theories of coordination and change, which—as the development of post-war economic theory shows—cannot be short-circuited by invoking a 'more general', history-free theory. This is also one of the *theoretical* justifications for many of the contributions to this book.

Notes

1. It might be worth recalling Arrow's view on the epistemological status of General Equilibrium analysis: 'I *do not* believe in the perfectly competitive view of the world, I think the general equilibrium theory is an imaginatively manipulative theory; one can get results out of it. It serves for many purposes as a good approximation for reasons that one does not fully understand. Therefore it is a useful tool for various micro problems. I think it is essential to remember the fact that in some industries there are increasing returns. But if you look at the economy, so to speak, in the gross these exceptions are very small. That is, all these exceptions are small on the scale of the economy. On the whole, what the existence problem has done was to force us to think a lot more rigorously about what it is. That may be the biggest benefit, rather than the existence theorem itself' (Arrow, 1987, pp. 197–8).
2. The following statement by Lucas describes very neatly the 'neo-classical' view of microfoundations: 'If we consider the question: How will a monkey that has not been fed for a day react to a banana tossed into its cage? I take it we have sufficient previously established knowledge about the behaviour of monkeys to make this prediction with some confidence. Now alter the question to: How will five monkeys that have not been fed for a day react to one banana thrown into their cage? This is an entirely different question' (Lucas, 1981, p. 289).
3. Obviously, the analysis of macroeconomic phenomena has forced a revision of the standard Arrow–Debreu model of general equilibrium; in order to deal with issues such as money, public debt, etc., the model has been modified and the main change has been that to make the general equilibrium model a truly dynamical model. An example of this extension, to which we will often refer in the sequel, is the overlapping generations model originally proposed by Samuelson (1958). This requires the issue of expectations to be tackled and the avenue taken has been that of assuming rational expectations, or perfect foresight in a deterministic world.
4. As far as welfare analysis is concerned, a very interesting work by Dow and Costa Werlang (1985) shows that welfare judgements based on the utility of the representative consumer are misleading. Indeed, they prove that it is possible 'that the representative consumer shows an increase in utility when, in fact, every consumer has been made worse off'.
5. See Antonelli (1886); Nataf (1953); Gorman (1953); Eisenberg (1961).
6. Recent contributions have tried to respond to the extremely negative implications of these results for neo-classical theory. It is interesting to note that the restrictions imposed either on the distribution of income (Hildenbrand) or on the shape of the distribution of preferences (Grandmont) 'cannot be deduced from the general hypothesis of individual rational behaviour alone' (Hildenbrand, 1986). They are indeed based on empirical facts. Obviously this avenue—although interesting for its empirical implications—is not a solution to the weaknesses of the neo-classical view, based as it is upon a 'deductionist approach' which derives general results dependent only on primitive assumptions about the economy and not on *ad hoc* hypotheses justified in terms of empirical observations. Other attempts to solve the aggregation problem, such as the one by Grandmont (1985), are not applicable to general equilibrium models because they assume that income is independent of prices. Even after some qualifications indicated in recent works (see Balask, 1986), it is true that in a general equilibrium model, in order to satisfy the assumption of a 'representative agent', extremely strong restrictions have to be imposed.

7. Interestingly enough, if we consider a particular adjustment rule—of the form $p(i)=k(i)z(i)[p]$, with i denoting a particular good, p the price and z the excess demand—in order to obtain stability it is necessary to assume conditions ensuring that the aggregate excess demand function is a single consumer function; therefore, it is necessary to assume away differences among consumers or to impose strong restrictions on preferences and income distribution.
8. See Cass, Okono and Zilcha (1979).
9. See Woodford (1984); Kehoe and Levine (1983). This result is due to the fact that a dynamic economy such as the overlapping generations model can be seen as a general equilibrium system with an infinite number of agents and goods.
10. See Azariadis (1981); Cass and Shell (1983). It should be noted that sunspot equilibria arise even in economies in which equilibrium is unique (see Cass and Shell, ibid.).
11. That is, the utility function is continuous, differentiable, strictly quasi-concave, while the production function is continuous and homogeneous of degree one.
12. See, among others Grandmont (1985a); Benhabib and Nishimura (1985); Reichlin (1986).
13. We take just as an example the model by Pietra (1986). Deterministic chaos is obtained in an overlapping generations model with the following utility and production functions: utility function: $C(t+1) - (1/2)\ L^2(\mathrm{t})$, with $C(t+1)$ and $L(t)$ being respectively future consumption and current supply of labour. Production function (Leontief)): $Q(t+1) = \min\{L, K/\alpha\}$, with $\alpha < 1$.
14. Although often used in the economic literature (Benhabib and Day, 1981, 1982; and Day, 1982, 1983), the definition derived from the Li and Yorke (1975) paper, suggesting that the existence of a cycle of period 3 is a signal of chaotic behaviour, may be misleading, since there can be a stable cycle which can make the 'chaotic' set irrelevant (of Lebesgue measure 0). One can thus define chaos as the case in which all cycles are unstable (cf. Grandmont, 1984).
15. For a definition of sensitive dependence on initial conditions, see Collet and Eckmann (1980).
16. The attractor—i.e. the set where trajectories starting from different initial conditions asymptotically end up—peculiar to deterministic chaos has been denoted 'strange attractor'. The dimension of this attractor is smaller than the dimension of the system and is usually noninteger, making the attractors fractals.
17. For example, an overlapping generation model in which people live for two 'periods', consume when they are old and work in their youth—young people maximize the utility function $U(C,l) = C(t+1) - \frac{1}{2}l^2$, where C stands for consumption and l for labour. Technology is given by the trivial constant

return to scale production function $y(t)=l(t)$. The old receive from a central bank a fixed quantity of money, M, at the beginning of each period. The young solve the following maximization problem: max $U(.)$ subject to $p(t+1)C(t+1)=p(t)y(t)$. The first-order condition of this maximization yields the supply of output of the young at time t: $y(t) = p(t)/p(t+1)$. The old obviously try to get rid of all their monetary holdings. Real demand is thus $M/p(t)$. Equilibrium requires equality of demand and supply, or $M/p(t)=p(t)/p(t+1)$. Defining $m(t)=M/p(t)$, we can rewrite the equilibrium condition in terms of real balances, m: $m(t+1)=m^{(}t)$. Substituting q for m we obtain the equation in the text.

18. For instance, it has been shown that replacing rational expectations by an expectation function based on an explicit learning process does not generally stabilize the economy (Fuchs, 1979). As argued by Grandmont and Laroque (1986), 'one should be very cautious when interpreting the stability results one gets from dynamical rational expectation models in which times goes forward. Taking into account the agents learning behaviour on the transition path, as one should, may reverse the stability diagnosis' (p. 139).
19. One of the authors has discussed this issue in Dosi and Egidi (1987); recent and more classic references whose content we broadly share are Simon (1986) and Winter (1986); for a broad discussion see the special issue of the *Journal of Business* where the latter two references appear.
20. Recall that when a steady state is a saddle point its inset (the state of all points converging to it) has zero Lebesgue measure.

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Imperfect decisions and routinized production: implications for evolutionary modeling and inertial technical change

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Introduction

Elswhere I introduced a theory of reliability to explain how imperfect information *and* imperfect ability to use information influence behavior (Heiner, 1983; 1985a, b; 1986; 1987a, b, d). The resulting analysis implies a close link between the scope of information agents can use reliably and the set of actions they can thereby benefit from choosing. In this chapter I briefly explore implications of this theory for understanding production methods and technical change within firms over time, and the need for explicit evolutionary modeling rather than postulating 'as if' optimization. In contrast with this approach, standard-choice theory assumes that agents use information perfectly, by always selecting actions that maximize expected utility based on observed information. Information may be costly to acquire, but there are no decision errors in using it once it has been observed. Suppose we relax this extreme assumption of no decision errors in responding to observed information. Instead, suppose agents' decision-making competence at using information is not necessarily sufficient always to respond optimally, no matter how difficult or complex their decision problems might be (separate from whether there are any costs of observing information in the first place).

Agents then face an additional dimension of uncertainty because there now exists a gap between their 'competence' at using information and the 'difficulty' of their decision problems (called a C–D gap). Standard-choice theory implicitly assumes that no C–D gap exists. Consequently, it has never investigated the behavioral implications of widening the gap; that is, of varying agents' decision-making competence *relative to* the difficulty of their decision problems. When this happens, agents become progressively worse at imitating optimal decision rules.

These issues are especially relevant to the major themes of this book. For example, the possibility of *non-linear* dynamic processes with bifurcation points between qualitatively different dynamic paths is discussed by Peter Allen (Chapter 5) and Gerald Silverberg (Chapter 24). Brian Arthur (Chapter 26) further discusses how such bifurcations can arise when the probability of adopting technical innovations is *path-dependent* on the history of previous adoptions. Similar themes are also developed by Dosi, Orsenigo and Coricelli (Chapters 2 and 6), namely, the intrinsic dynamic complexity of agents interacting in *non-stationary,* path-dependent environments.

All of these factors argue against the possibility of technical change proceeding in a dynamically simple and stable fashion (which could thereby be easily understood by agents within an economic system). To the contrary, technical change will typically display extremely complex and unstable patterns far beyond the competence of individual agents to design optimal strategies for when and how to adopt new technologies over time (so that a C–D gap for selecting dynamic strategies necessarily exists). Consequently, the modern theory of non-linear, path-dependent and non-stationary dynamics provides a basic explanation for why agents' decisions within a technologically evolving economy will be highly imperfect relative to that typically postulated by standard neo-classical investment and rational expectation models. Kenneth Arrow (1986) has recently voiced a similar theme about the weakness (even logical inconsistency) of rationality concepts in the presence of incomplete markets and imperfect competition.[1]

Moreover, even if we eliminate the sources of dynamic complexity noted above (i.e. no path-dependency, non-linearity, or non-stationarity is allowed), Marco Lippi shows (Chapter 8) that imperfect dynamic decisions can still arise from traditional macro models through the very process of *aggregating* individual micro-behaviors within a larger economy. In particular, he shows that static or quasi-static micro-behavior (with at most short lags of independent micro-variables, but no lags of dependent micro-variables) can aggregate to qualitatively more complicated macro-dynamic relationships (where dependent macro-variables now become noticeably lagged). Consequently, the macro-dynamic properties of an economy can be qualitatively *more* complex than the underlying micro-decision rules whose interaction gives rise to them.

This relationship means that trying to anticipate an economy's macro-behavior may require an understanding of relatively more complicated aggregate dynamic properties than needed to administer successfully the micro-decision rules which generate them. As a result, imperfect micro-agents may be unable reliably to affect macro-relationships by deviating from perhaps even simple routinized decisions (that is, the relatively more complicated macro-dynamics in part explains why micro-agents are only imperfectly reliable at deviating from simple micro-behaviors in ways that might successfully improve their performance). Lippi's analysis thus helps explain why the surrounding macro-environment within which individual agents interact is sufficiently complicated compared to their own behavior so as to require relatively simple micro-decision rules in order for them to compete successfully with each other.

In relation to the above introductory discussion, I wish to analyze the behavioral consequences of agents acting within a dynamic environment of sufficient complexity (through aggregation, path-dependency, non-linearity, non-stationarity, etc.) so that an unavoidable C–D gap exists in their decisions over time. To do so, I initially describe some key concepts and principles of reliability theory applied to production decisions within a firm. Three main topics are then discussed. First, I show how imperfect decisions lead to routinized behavior that is not optimally adjusted to all circumstances encountered by a firm. This is implied even if it is costless to adjust production methods to changing conditions. I then discuss why the analysis implies the need for explicit evolutionary modeling. Finally, I discuss how firms must inertially delay adjustment to new conditions in order to control errors in deciding when and how to appropriately use new technology. Here again, this is implied irrespective of whether there are any costs of physically adjusting production methods to new conditions or information.

Preliminary concepts and notation

Let S represent the set of possible *states* of the world and X represent a set of potentially observed *information* whose individual messages are imperfectly correlated with particular states. The set A denotes a firm's space of choosable *actions*, or *production methods*.

Individual consequences or outcomes result from different pairs of actions and states (a, s) taken from the Cartesian product $A \times S$. Let p denote a given probability distribution over outcomes (that is, a probability measure over $A \times S$), and the set of all such *outcome-distributions* is denoted P. Each particular outcome-distribution $p \in P$ in general depends on the likelihood of different states arising, the likelihood of receiving different messages conditional on given states occurring, and the likelihood of agents selecting particular actions in response to different messages. A distribution p can thus be conditioned on particular states occurring, messages received, or actions selected.

In particular, let $p_{A' \times S'} \in P$ be the outcome distribution p conditional on agents selecting an action from a set $A' \subset A$ when a state from the set $S' \subset S$ occurs (leaving unspecified what potential messages are observed when $s \in S'$ occurs). Similarly, let $p_{A' \times X'} \in P$ be the outcome distribution p conditional on agents selecting an action from $A' \subset A$ when a message from $X' \subset X$ is observed (leaving unspecified the occurrence of particular states which affect the likelihood of agents observing messages from X').[2]

Let $V(p)$ represent a value function which measures achieved performance associated with different outcome distributions $p \in P$; that is, the performance which arises from the statistical relationships between states occurring, messages being received, and agents responding to received messages. V might be a traditional expected utility function or one of the 'non-expected' utility functions of Chew, Machina, Fishburn, etc. (see Machina, 1983). In order to simplify notation, let $V(p_{K \times L}) = V(K, L)$ for any $K \times L$ contained in either $A \times S$ or $A \times X$. Thus, for example, $V(A', X')$ measures performance conditional on agents selecting an action from $A' \subset A$ when a message from $X' \subset X$ is observed; and similarly, $V(A', S')$ measures performance conditional on agents selecting an action from $A' \subset A$ when a state from $S' \subset S$ occurs.

Next, introduce the set β of correspondences from X into A. Each element $B \in \beta$ is interpreted as a decision rule or *behavior pattern* for selecting particular production methods in response to observed messages about output demand conditions, input costs, alternative production technologies, and so on. Particular behavior patterns are in general not optimal in selecting actions only when they maximize $V(\cdot)$ given observed information. Thus for a given $B \in \beta$ and $a \in A$, X_a^B represents those messages in X for which selecting action a is at least as preferred to actions chosen by B. That is, $X_a^B = \{x \in X \mid V(\{a\}, \{x\}) \geq V(\{a'\}, \{x\})$ for any $a' \in B(x)\}$. We can then interpret $\pi_a^B = p(X_a^B)$ as the probability of action a being a 'preferred deviation' from behavior B based on observed information. An optimal behavior pattern, denoted B^*, is one for which there does not exist any preferred deviations from it (i.e. $X_a^{B^*} = \phi$ for all $a \in A$). Consequently, there is zero probability of any preferred deviation from an optimal behavior pattern (i.e. $\pi_a^{B^*} = 0$ for all $a \in A$).[3]

A standard decision model would assume that firms always behave optimally and then investigate the implications of this assumption. Doing so implicitly assumes that firms will optimally deviate from any given behavior pattern $B \in \beta$ which is not itself fully optimal. Suppose instead we allow the possibility of decision errors in deciding when to deviate from a given 'status quo' behaviour pattern, so that a C–D gap exists as discussed in the introduction (due to intrinsically complex system dynamics through aggregation, non-linear relationships, path-dependency, non-stationarity, etc.). Firms will then *not* always deviate from a status quo behavior pattern when it is preferred to do so; and they may sometimes deviate when doing so is inferior to maintaining status quo behavior.

Thus, let $r_a^B = p(\text{selecting } a \notin B(x) | X_a^B)$ be the probability of 'rightly' deviating away from $B(x)$ toward an action a when doing so is at least as preferred based on observed information. Similarly, let $w_a^B = p$ (selecting $a \notin B(x) | X - X_a^B)$ be the probability of 'wrongly' deviating away from $B(x)$ toward an action a when doing so is less preferrred based on observed information.

The ratio $\rho_a^B = r_a^B / w_a^B$ then measures a firm's reliability at deviating from a given behavior pattern B by selecting action $a \notin B(x)$ in response to observed messages $x \in X$. The special use of perfect decisions corresponds to $r_a^B \equiv 1$, $w_a^B \equiv 0$ (or $\rho_a^B \equiv \infty$) for all $B \in \beta$ and $a \in A$ (that is, perfect decisions are infinitely reliable at deviating in any way from any given behavior pattern).

On the other hand, imperfect decisions are only finitely reliable ($\rho_a^B < \infty$) at deviating from a given behavior pattern. The objective is to generalize standard-choice theory by investigating the full range of possibilities permitted by $0 \leq r_a^B$, $w_a^B \leq 1$, instead of imposing the limiting assumption $r_a^B \equiv 1$, $w_a^B \equiv 0$.

To complete the necessary notation, define the net gain and loss due to deviating from a given behavior pattern when preferred messages for doing so are observed as compared to non-optimal ones. $V(B(x), X_a^B)$ is the performance achieved when the firm chooses according to $B(x)$ even though action a is the preferred choice given observed information. $V(\{a\}, X_a^B)$ is the performance achieved by selecting action a instead of from $B(x)$ when a is preferred given observed information. Thus, define $g_a^B = V(\{a\}, X_a^B) - V(B(x), X_a^B)$ as the net 'gain' in performance by deviating from behavior B toward action a when preferred messages for doing so are observed. Similarly, define $l_a^B = V(B(x), X - X_a^B) - V(\{a\}, X - X_a^B)$ as the net 'loss' by mistakenly deviating toward action a when non-preferred messages for doing so are observed.

Imperfect decisions and routinized behavior

The above concepts relate to those introduced in my 1983 paper on 'the origin of predictable behavior…'. That paper showed how imperfect agents may benefit from restricting the size and complexity of their decision space A. In this paper, I introduce a set of possible behavior patterns β and show that imperfect agents will not necessarily benefit from trying to deviate from a non-optimal behavior pattern $B \neq B^*$. However, the present formulation is more general in that a shift from a given non-optimal behavior pattern may not necessarily involve expanding the decision space A. Instead, it may involve a firm's attempt to decide more effectively over a fixed set of actions. In either case, the basic issue is whether imperfect agents will always benefit by shifting from a given decision rule $B \in \beta$ until B converges B^*.

Note that convergence to B^* is automatically implied for agents who are perfectly reliable at deviating from any given behavior $B \neq B^*$ (no matter how subtle or complex the deviations that may be required in order to shift B closer to B^*). On the other hand, if perfect decisions are no longer assumed, what then are the behavioral implications for firms composed of imperfect agents?

To answer this question, two assumptions about potential decision errors are introduced. The first assumption was mentioned earlier; namely, imperfect agents have *finite* reliability at deviating from non-optimal behavior patterns. That is,

A1

$$\rho_a^B < \infty \text{ for all } a \in A \text{ and } B \neq B^*.$$

Note, however, that decision errors would make no difference if the resulting losses became *arbitrarily small* compared to the gains from deviating when actually preferred to a given behavior pattern. Thus, for any potential action a, the loss l_a^B due to mistakenly deviating from an initial behavior pattern $B \in \beta$, is assumed to be at least some positive fraction of the gain g_a^B achieved by selecting action a when it is actually preferred to $B(x)$ based on the observed message $x \in X$. That is,

A2

$$\text{For some } K > 0,\ l_a^B / g_a^B \geq K \text{ for any } a \text{ and } B \neq B^*.$$

The necessity of routines over flexible optimizing: a general result

Let V^B denote the expected performance achieved by always choosing according to a given behavior pattern B. Similarly, let V_a^B denote the expected performance if agents can deviate from B toward action a whenever they so choose. Agents will thus benefit from the flexibility to deviate toward an action a if V_a^B exceeds V^B. The following theorem (developed in the Appendix) shows when this is the case.

Theorem 1 (reliability condition for improving on status quo behavior)

$$\text{For any } a \text{ and } B \neq B^*,\ V_a^B > V^B \text{ if and only if } \rho_a^B > T_a^B \tag{1}$$

where

$$\rho_a^B = \frac{r_a^B}{w_a^B} \quad \text{and} \quad T_a^B = \frac{l_a^B}{g_a^B} \cdot \frac{1 - \pi_a^B}{\pi_a^B} \tag{2}$$

T_a^B determines the *minimum* reliability or 'tolerance limit' (the minimum size of ρ_a^B) that must be satisfied before agents can benefit by sometimes deviating from behavior B toward action a. The inequality $\rho_a^B > T_a^B$ compares an agent's actual reliability ρ_a^B at so deviating with the minimum required reliability T_a^B. *If* ρ_a^B exceeds T_a^B, agents will benefit from allowing the flexibility to deviate toward action a; otherwise they will benefit from always choosing according to B.

Now apply Theorem 1 to an explicitly dynamic setting where different behavior patterns $B \in \beta$ may evolve, and consider the effect of moving toward an optimal pattern B^*. In particular, consider a sequence $B^v \in \beta$ such that $B^v \rightarrow B^*$. Since the latter implies $X_a^{B^v} \rightarrow X_a^{B^*}$ and since $X_a^{B^*} \equiv \phi$ by the definition of optimal behavior B^*, then $B^v \rightarrow B^*$ implies $\pi_a^{B^v} \rightarrow 0$ for all a (that is, the probability of any potential deviation being preferrred to status quo behavior B^v goes to zero as B^v gets closer and closer to fully optimal behavior B^*).

In addition, since $l_a^{B^v} / g_a^{B^v}$ is bounded above zero by Assumption A2, then $T_a^{B^v} = l_a^{B^v} / g_a^{B^v} \cdot (1 - \pi_a^{B^v}) / \pi_a^{B^v}$ must approach infinity as $B^v \rightarrow B^*$ (since $(1 - \pi_a^{B^v}) / \pi_a^{B^v}$ rises without limit as $\pi_a^{B^v} \rightarrow 0$). Consequently, Assumption A1 ($\rho_a^B < \infty$) implies the reliability condition $\rho_a^{B^v} > T_a^{B^v}$ *cannot hold* as $B^v \rightarrow B^*$ (so that trying to deviate from B^v will lower performance compared to not doing so). Thus, at some point *before* B^v converges to B^*, imperfect agents who try to deviate from B^v will have a selective disadvantage within a population of similar agents who always behave according to B^v.

Thus, as behavior approaches full optimality, imperfect agents will no longer benefit from trying to make preferred exceptions based on observed information, even though such exceptions still arise with positive probability $\pi_a^B > 0$. Or equivalently, as the preferred deviations to a given behavior pattern become sufficiently rare, imperfect agents will no longer benefit from trying to deviate from the behavior pattern. Instead, they will benefit from rules and procedures *adapted only to typical or recurrently observed messages.* Behavior adapted in this way (in order to satisfy $\rho_a^B > T_a^B$) constitutes *rule-governed* behavior.

Therefore, when imperfect decisions are combined with evolutionary stability, the generic result is rule-governed behavior; meaning behavior that approximates optimal choices only under typical or recurrently encountered information (so that $\pi_a^B > 0$ in general still holds). The above conclusion is similar to the idea of 'routines' used in Nelson and Winter's (1982) evolutionary models, and other related ideas such as 'satisfying' developed by Simon (1957, 1983). It is also consistent with various intuitive connotations of rule-governed behavior that have been applied to both humans and animals, such as habits, instincts, rules of thumb, cultural rituals, customs, norms and so on. All of these phrases collectively testify that some form of rigidity or inflexibility is a universal qualitative feature of behavior. The reliability condition $\rho_a^B > T_a^B$ enables this basic feature to be formally derived (from 'first principles') by generalizing standard-choice theory to incorporate explicitly the effects of decision errors on behavior.

The need for explicit evolutionary modeling

The inequality $\rho_a^B > T_a^B$ of Theorem 1 is a *diagnostic* condition which tells how performance will be affected by trying to deviate from a given behavior pattern. It does not assume agents are themselves competent to determine when or how it should be satisfied, nor that the very best or 'optimal' methods for satisfying it will necessarily evolve. However, we can still use the condition to analyze agents' behavior even if they have no special competence at applying it themselves, including inability to estimate the probability variables used in the condition (see Heiner, 1985a). (These questions are further discussed in Heiner, 1983, about selection processes sluggishly weeding out inferior performers, 1985a, about the 'tacit' nature of most evolved behavior mechanisms, and 1986, about the 'unintended' development of social institutions.)

In terms of the larger concerns of this book, a key, related conclusion should also be noted. It is simply that once imperfect decisions are considered, there is no longer any necessary feature in the formal analysis to guarantee that only appropriate (let alone 'optimal') decision practices will evolve. At best, conditions like $\rho_a^B > T_a$ can be used as a diagnostic tool to help determine whether deviating from given behavior patterns will be more or less likely to 'filter' through an evolutionary selection process. That is, satisfying or violating $\rho_a^B > T_a$ will respectively raise or lower the probability of survival. But this ('short-run') statistical effect does not guarantee that only 'optimal' behavior will eventually filter through a long sequence of selection trials. Only by combining diagnostic tools like $\rho_a^B > T_a$ with an explicitly formulated selection process can any precise results be obtained.

The latter conclusion is one of the most important implications of the reliability analysis presented above. Namely, *imperfect choice theory implies that evolutionary modeling must be added to any (diagnostic) analysis about the welfare effects of different behavior patterns.* Selection processes can no longer be assumed to generate behavior patterns 'as if' they were optimally chosen. In this regard, it is interesting that one of the major themes about understanding technical change emphasized in other chapters of this book (especially Parts II and VII) is the need for a theoretical framework that incorporates evolutionary processes as one of its essential features. Such a realization is intrinsically connected to analyzing imperfect choice (rather than excluding the possibility of imperfect decisions by hypothesis, as in neo-classical choice theory).

Adjustment costs and evolutionary stability

The above analysis made no mention about costs of physical switching between different decisions or production methods, and indeed remains valid even when such 'adjustment costs' are zero. On the other hand, the presence of adjustment costs is often given as a reason for routinized production methods within a firm. This is especially the case when changing methods requires investment in expensive capital equipment. A similar issue also arises in evolutionary biology, when behavioral rigidity is linked to constraints imposed by 'hardwired' neurological design, internal tissue structures, bodily morphology, and so on.

Suppose we agree that many production, institutional and biological features can impose significant costs of adjusting decisions. However, even granting this assumption, a more basic question still remains. Namely, *would such features evolve in the first place (or remain stable once introduced) if agents could perfectly handle any kind of decision flexibility no matter how subtle or complex their resulting behavior might become?*

In particular, suppose $\rho_a^B \equiv \infty$ for all a and B, but certain features of an agent (such as specialized capital equipment) make it prohibitively costly or even physically impossible to implement potential deviations from an initial status quo behavior pattern B^0. These features may thus deter the agent from deviating from B^0 despite their perfect reliability at doing so. Over the long run, however, such perfectly reliable agents would have a selective advantage in competition with similar agents from any change in their features enabling them more flexibly (or more cheaply) to deviate from any given routinized behavior pattern. This is implied no matter how particular 'adjustment cost lowering' features might be first introduced, even by pure random mutation or accident. Consequently, *any feature imposing high adjustment costs is evolutionarily unstable for perfectly reliable agents.*

On the other hand, the opposite conclusion applies to imperfect agents. The reason is that features creating high adjustment costs may help imperfect agents maintain routinized behavior patterns in the face of continual opportunities to deviate from them (yet trying to deviate would violate the reliability condition $\rho_a^B > T_a^B$). Consequently, certain 'adjustment cost *raising*' features may actually benefit imperfect agents. Thus, from an evolutionary perspective, high adjustment costs may themselves evolve to help imperfect agents regulate potential decision errors (instead of representing an independent reason for why agents typically behave according to routinized patterns).

Asymmetrically imperfect decisions

The general analysis presented above also allows another possibility that parallels a major development of neo-classical theory, namely, asymmetric information. That is, once standard theory dropped the assumption of perfect knowledge, the possibility that agents may have access to unequally or asymmetrically distributed imperfect information arose (instead of all agents having equally perfect information). Similarly, once one drops the assumption that all agents are equally perfect decision-makers, the possibility of unequally or asymmetrically imperfect decisions arises (even for the same person when making different types of decisions, or when deciding under different circumstances). A key example of this relevant to technical change is asymmetric competence across different agents within a business organization, including asymmetric competence at allocating individuals to particular jobs or management responsibilities. Some possibilities along these lines are discussed in Heiner (1987a). Asymmetric competence is also a key part of Pavel Pelikan's comparative institutional analysis in Chapter 18.

Inertially adjusting imperfect decisions and expectations

One of the major issues about technical change is how it develops within firms and spreads between a number of firms. New ideas and technology usually spread much slower than physically possible, even taking account of various adjustment costs of installing new equipment, compatibility of existing labor skills, and so on.

The relatively slow diffusion of industrial technology is part of a larger pattern where agents in general tend to sluggishly adjust away from their prior, 'status quo' decisions. These might refer to production output decisions as well as when and how much to adjust previously chosen output prices. Firms must also decide when and in what way to modify prior decisions about production methods.

Accordingly, this section applies the reliability condition $\rho_a^B > T_a^B$ within an explicitly dynamic setting in which agents must successively revise their decisions over time. It is shown that imperfect agents will not benefit from immediately adjusting their decisions to shifts in environmental parameters. Instead, they will benefit from no response for some positive interval of time after the environment shifts. In a production context, this means that firms will not immediately adopt new technology irrespective of whether there are any adjustment costs of doing so (i.e. even if it is costless to shift toward new production methods with no delay).

The analysis in part builds on work by Akerlof and Yellen (1985a, b) about what they call 'near rational' behavior (meaning non-optimal decisions that produce only 'second-order' losses compared to fully optimal adjustment). I thus begin with a short summary of their analysis.

Let an agent's objective function be $V(a, z)$, where a is a decision variable and z is a vector of decision parameters. The function $a^*(z)$ denotes the optimal selection of a which maximizes V for each given z. For simplicity, let us focus on a single z variable, denoted z, and write $a(z)$ only as a function of z, denoted $a(z)$; where the other z parameters are held fixed as z changes. Similarly, write $V(a, z)$ only as a function of z, denoted $V(a, z)$.

Suppose z initially equals z^0 and the agent has had sufficient time to learn the optimal decision $a^0 = a^*(z^0)$. Then let z start to shift from z^0 and compare two responses: (1) optimally adjusting according to $a^*(z)$; and (2) holding action a constant at a^0 as z starts to shift. Computing the net loss from not adjusting relative to optimal adjustment gives

$$\frac{\partial(a^*, z)}{\partial a} \cdot \frac{\partial a^*}{\partial z} + \frac{\partial V(a^*, z)}{\partial z} - \frac{\partial V(a^0, z)}{\partial z} \tag{3}$$

The first-order maximizing conditions imply $\partial V(a^*, z)/\partial a$ equals zero, so only the last two terms remain. The difference in these latter terms obviously goes to zero at the instant where z starts shifting from z^0 (because $a^*(z) \to a^0 = a^*(z^0)$ as $z \to z^0$). Hence, to a 'first-order' approximation, there are no net losses from not adjusting away from an initially optimal decision. (Consequently, any such losses must be 'second order' in magnitude.) This result might be used to explain why agents will not immediately adjust to shifts in z.

However, this conclusion is still not justified. To see why, consider a *completely arbitrary* response rule to shifts in z, denoted $a^?(z)$, and as before compute the net loss from adjusting according to $a^?$ instead of a^*. Calculations similar to (3) again imply that the first-order losses are zero. Consequently, zero first-order losses by itself gives no justification for singling out *any* particular type of response (such as not adjusting) as the assumed meaning of non-optimal behavior. On the other hand, delayed response to shifts in z can be derived with the reliability concepts developed earlier.

Delayed adjustment of imperfect decisions

The basic idea is that imperfectly reliable agents will not always adjust toward the new optimum level of contingent on z shifting. Let r equal the conditional probability of adjusting a in the right direction toward the new optimum when z shifts from z^0, and w equal the conditional probability of responding in the wrong direction away from the new optimum when z shifts from z^0. The reliability adjustment then equals the ratio $\rho = r/w$.

Note that for notational convenience the r, w, ρ variables are not superscripted with B as in the first three sections above. We are also not interested in any single selection of the a variable in response z, but rather in all those selections which adjust a from its initial value a^0 toward the new optimum $a^*(z)$; and similarly, all those selections of a that adjust it away from $a^*(z)$. To indicate this distinction, r, w, p are also not subscripted with the decision variable a as in the first three sections.

For an arbitrary response rule $a^?(z)$, let $a^w(z)$ represent the average or mean response to z conditional on adjusting in the wrong direction, and $a^r(z)$ represent the mean response to z conditional on adjusting in the right direction. The derivatives $da^w/dz = a_z^w$ and $da^r/dz = a_z^r$ then measure the mean rate of response to z conditional on adjusting in the wrong or right direction respectively.

Let ξ denote the length of time since z has shifted from z^0. The probability that time t is within ξ of t^0 is denoted $\pi(\xi) = p((t^0 \leq t \leq t^0 + \xi)$. The conditional response probabilities r and w may in general depend on ξ; because larger ξ provides more time to learn how best to react to shifts in z. They are thus written $r(\xi)$ and $w(\xi)$, so that $\rho(\xi) = r(\xi)/w(\xi)$.

Then let β represent the set of all *imperfect* response rules $a^?$, in that: (1) the probability of wrong responses $w(\xi)$ is positive (that is, $w(\xi) > 0$ for $\xi \geq 0$, so that $\rho(\xi) = r(\xi)/w(\xi) < \infty$ for $\xi \geq 0$); and (2) agents respond in the wrong direction at a rate that is at least some positive fraction of their responses toward the new optimum (that is, $-a_z^w/a_z^r \geq \lambda$ for some $\lambda > 0$). *Thus β contains any kind of imperfect response behavior that has a positive probability of wrong adjustments that are not arbitrarily small compared to correct adjustments.*

Next, introduce an explicit dynamic structure by writing z as a function of t, denoted $z = z(t)$, where $t = t^0 + \xi$. The net loss at time ξ beyond t^0 due to adjusting in the wrong direction from a^0 (where $a^0 = x^*(z^0)$ and $z^0 = z(t^0)$) is given by

$$l(\xi) = d[V(a^0, z(t^0 + \xi)) - V(a^w(z(t^0 + \xi)), z(t^2 + \xi))]/dt. \tag{4a}$$

Similarly, the net gain at ξ beyond t^0 due to adjusting in the right direction from a^0 is given by

$$g(\xi) = d[V(a^r(z(t^0 + \xi)), z(t^0 + \xi)) - V(a^0, z(t^0 + \xi))]/dt. \tag{4b}$$

Now consider whether agents will benefit from immediately adjusting according to some rule $a^? \in \beta$. The reliability condition of Theorem 1 can be used to answer this question. In the present dynamic context it amounts to determining whether the following inequality holds at the limit $\xi = 0$,

$$\frac{r(\xi)}{w(\xi)} = \rho(\xi) > T(\xi) = \frac{l(\xi)}{g(\xi)} \cdot \frac{1 - \pi(\xi)}{\pi(\xi)} \tag{5}$$

The 'first-order' results discussed just prior to this subsection imply that both $l(\xi)$ and $g(\xi)$ go to zero as ξ goes to zero. However, using l'Hospital's rule for the ratio of such limits and differentiating l and g with respect to ξ imply that the ratio of 'second-order' effects, $(\mathrm{d}l/\mathrm{d}\xi)/(\mathrm{d}g/\mathrm{d}\xi)$, converges to at least $-a_z^w/a_z^r$ (see the Appendix). Since $-a_z^w/a_z^r \geq \lambda > 0$ and $\pi(\xi) \rightarrow 0$ and $\xi \rightarrow 0$, then $T(\xi) \rightarrow \infty$ as $\xi \rightarrow 0$. This in turn implies inequality (5) cannot hold as ξ approaches zero so long as the adjustment reliability $\rho(\xi)$ is finite. We thus have the following result.

Theorem 2 (the necessity of delaying imperfect adjustment)

If $a^? \in \beta$ (where β is the set of adjustment rules satisfying conditions (1) and (2) above), when the adjustment-reliability condition (5) will be violated for all ξ below some positive time delay $\delta > 0$.

Theorem 2 implies that agents will not benefit from immediately responding according to any $a^? \in \beta$. More precisely, this means the probability of adjusting according to any $a^? \in \beta$ must drop to zero *before* ξ reaches zero in order to satisfy the adjustment reliability condition (5). Therefore, agents will in general never benefit from adjusting to new conditions immediately except at the limit where their reactions become perfectly reliable. Note also that Theorem 2 holds with no assumptions about the possibility of 'adjustment costs' (including information costs, transaction costs, etc.). It thus holds even in the limit where there are *zero* costs of adjusting faster to new conditions.

Application to diffusion models and evolutionary self-organization

I here briefly discuss how the preceding results relate to two analytical themes developed in other chapters on technical change and evolutionary competition (for example, Chapters 2 (Dosi and Orsenigo), 5 (Allen), 6 (Coricelli and Dosi), 24 (Silverberg), and 25 (Metclafe)).

(1) Theorem 2 provides a theoretical underpinning for a key part of Metcalfe's model on diffusion. In particular, he introduces the parameters d and f as diffusion coefficients that reflect the rapidity with which users learn new technologies, or users' willingness to switch to new technologies or commodities. The diffusion parameters d and f are not themselves modeled, being instead exogenous to the model. Yet the resulting dynamic properties (especially tendencies toward inertially slowed diffusion) are largely driven by assumed variations in d and f. Theorem 2 explains why d and f are finite without having to assume *ad hoc* 'adjustment costs'. As noted above, imperfect decisions have intrinsically inertial dynamics even with zero costs of physically responding faster, or zero search costs of observing more information.

We can also further analyze when dynamic response errors are likely to increase or decrease, and thereby derive when the d and f coefficients will rise or fall (rather than having to assume exogenous shifts in order to explain different patterns of technological diffusion).

(2) Next consider the 'self-organization' models discussed by Silverberg in Chapter 24 (see also Dosi, Orsenigo and Silverberg, 1986; Silverberg, 1985). These models specify an explicit evolutionary process of competition between firms (that have the opportunity of enhancing their relative competitiveness by sooner or later, or rapidly or slowly adopting new technologies). In many cases there is no determinant 'optimal' dynamic equilibrium trajectory. Still, by using computer simulations it is clear that firms can 'mistakenly' adopt a new technology either too slowly or too rapidly, as well as too soon or too late in fact to raise (instead of lower) their rate of growth in profitability or relative market share compared to other firms. Indeed, the simulations suggest that the appropriate adoption time and rate of a new technology are affected by an extremely complex set of dynamic interrelationships. Consequently, it may be difficult for individual firms always to determine appropriately when and how fast to adopt a new technology.

We can thus use reliability variables to measure the probability of adopting a new technology at the 'right time' and at the 'right speed' instead of mistakenly adopting at the 'wrong time' (either too soon or too late to enhance the dynamic trend in their relative market share) or the 'wrong speed' (either faster or slower than needed to enhance relative competitiveness). The analysis of Theorem 2 can then be extended along the lines of Heiner (1987c) to model the behavioral effects required in order to control the incidence of these dynamic errors in both timing and speed of adoption. Here again, a generic tendency toward inertial diffusion is implied. It can also be shown that if the diffusion dynamics has a minimal degree of non-linearity, then the only way dynamic adjustment errors can be significantly reduced is for agents to slow their rate of adjustment so as to induce relatively more stable dynamic paths (in market share and profitability between firms) than would otherwise occur (see Heiner, 1987d).

Delayed revision of imperfect expectations

Up to now the variable a has been understood as a decision adjusted in response to a parameter from the vector z. However, the preceding analysis is consistent with other interpretations. For example, the variable a might not only refer to 'outward' behavior, but also to 'inward' subjective beliefs about probability or expectation variables. In a production context, moreover, expectations of future investment and technical conditions are important determinants of a firm's current plans either to maintain or alter its existing technology. Consequently, a tendency to delay forming new expectations could be a major additional factor causing firms to delay the introduction of new technology.

Let us consider a simplified illustration of the latter involving imperfect expectations. Suppose agents seek to maximize a quadratic utility function, $-\alpha(a-s)^2$, where $a-s$ is the difference between action a and a state variable s. However, they can only observe a noisy signal x which is normally distributed with mean s and variance σ^2. The state s also randomly varies according to a normal distribution with mean $\hat{s}$ and variance δ^2.

In this problem, maximizing expected (quadratic) utility requires the action a to be set equal to the agent's estimate of s, denoted e. The usual assumption is to assume expectations are perfect (i.e. 'rational expectations') in that e equals the optimal posterior Bayesian estimate of s, as given by $e^*(z)=(\delta^2\hat{y}+\sigma^2x)/(\delta^2+\sigma^2)$; where $z=(s, x, \delta^2, \sigma^2)$. Suppose agents do set a equal to e (so that $a=e$ for each given subjective estimate e), but their expectations are not necessarily optimally related to z. Instead, their expectation of s is formed according to $e^?(z)$. where the question mark refers to an unspecified imperfection in adjusting expectations to shifts in the z variables (analogous to $a^?$ in the preceding section).

Mistaken expectations might arise because agents cannot perfectly detect shifts in the underlying $\hat{s}$, δ^2, σ^2 parameters (so that their beliefs about these parameters are not perfectly accurate), or because they are unable to combine them with the signal x according to Bayes' Rule. Whatever the reason for error, suppose $e^?(z)$ is imperfect in a similar manner to $a^?(z)$ discussed above. That is, there is a positive probability of adjusting e in the wrong direction (away from an initially optimal value $e^0=e^*(z^0)$) at a rate that is not arbitrarily small compared to adjustments in the right direction. Let E be the set of all such imperfect expectation rules.

The assumptions (about quadratic utility, $\alpha(a-s)^2$; normally distributed s and x; and actions set equal to expected s, $a=e^?$) imply agents imperfectly maximize the following expected utility function (see Boyd and Richerson, 1986),

$$V(e(z),z)=-\alpha\left[\left(e^?(z)-\frac{\sigma^2\hat{s}-\delta^2x^2}{\sigma^2+\delta^2}\right)+\delta^2\right]; \tag{8}$$

where $z=(\hat{s}, x, \delta^2, \sigma^2)$ and $e^?\in E$.

We can then use Theorem 2 to imply that agents will always benefit from delaying adjustment (according to any expectation $e^?\in E$) for some positive ξ of time after shifts in the z variables. Thus, unless agents are perfectly reliable at forming expectations, they will never benefit from adjusting them immediately to new messages x, nor to shifts in either information parameters such as σ^2 or state parameters such as $\hat{s}$ and δ^2. Standard-decision theory avoids these results by postulating perfect or 'rational' expectations, so that $e=e^*(z)$ by hypothesis. Once this postulate is relaxed, a wider range of behavioral possibilities (such as a generic tendency to delay adjustment of subjective beliefs) opens up for analysis.[4]

Conclusion

I have argued that production methods within firms and technical change over time both systematically depend on how agents use information imperfectly, separate from whether their information is costly and imperfect. In order to do so, certain reliability principles were developed to analyze the effects of decision errors. These involved the probability of failing to select actions when they are superior to others based on observed information, and the probability of still selecting actions when they are inferior to others based on observed information. Depending on the relative incidence of these errors, agents may or may not benefit from trying to improve on any given behavior pattern by selectively deviating from it.

Imperfectly reliable agents will in general benefit from 'routinized' behavior governed by rules and procedures that prevent flexible response to all conditions encountered by a firm. Such behavioral routines are thereby adapted only to relatively likely and recurrent features of a firm's production environment. Nevertheless, the possibility of decision errors makes behavioral routines stable in an evolutionary setting despite the existence of numerous circumstances (that arise with positive probability) for which superior results can be achieved.

The analysis, however, does not guarantee that only appropriately structured routines will evolve; only that such routines will have a higher probability of surviving in competition with other firms. In order to complete the analysis one must explicitly model how raising or lowering the probability of survival (or altering relative profitability rates) will affect the likelihood of particular types of behavior 'filtering' through a long-run evolutionary process. That is, the principles of imperfect choice imply an intrinsic need for constructing evolutionary models.

Finally, imperfect decisions in the context of dynamic adjustment or adoption of new technologies were discussed. Imperfect agents will in general not benefit from trying immediately to adjust their prior production decisions to changing conditions or novel technology, even if there are little or no adjustment costs of doing so. Consequently, even with zero adjustment costs, the diffusion of new technology across firms will tend to be inertially delayed over time.

Taken together these implications provide a theoretical underpinning for the broader issue of 'evolutionary-modeling' in economics. First, instead of a uniform tendency toward the 'optimal solution' within a given environment, imperfect agents will in general benefit from displaying a (potentially wide) diversity of routinized behavior patterns. Second, such diverse yet routinized patterns will intertially adjust, thereby providing more time for selection processes to weed out relatively inferior behavior patterns. We thus have an explanation for the existence of micro-diversity within a larger system, and of behavioral inertia that gives selection processes time to work. Both of these are recognized as key features in understanding evolutionary systems. Finally, recall from Peter Allen's discussion (Chapter 5) that a system's resilience to external change often critically depends on the existence of micro-diversity within its internal structure combined with inertial features that preserve any existing micro-diversity until external conditions change.

Appendix

Proofs for Theorems 1 and 2 are here presented.

Let V_1, V_2, V_3, V_4 equal respectively $V(B(x), X_a^B)$, $V(B(x), X - X_a^B)$, $V(\{a\}, X_a^B)$, $V(\{a\}, X - X_a^B)$. The definitions of l_a^B, g_a^B then imply $l_a^B = V_2 - V_4$ and $g_a^B = V_3 - V_1$. In the special case of standard expected utility function, the linearity properties of V can be used to expand V^B and V_a^B as follows:

$$V_a^B = \pi_a^B[r_a^B V_3 + (1 - r_a^B) V_1] + (1 - \pi_a^B)[w_a^B V_4 + (1 - w_a^B) V_2]$$

$$V_a^B = \pi_a^B V_1 + (1 - \pi_a^B) V_2.$$

Then subtract these expressions and rearrange terms (also recalling the above definitions for g_a^B, l_a^B) to yield

$$V_a^B - V^B = \pi_a^B r_a^B g_a^B - (1 - \pi_a^B) w_a^B l_a^B \tag{A1}$$

Hence, (A1) implies $V_a^B > V^B$ if and only if $r_a^B / w_a^B > l_a^B / g_a^B \cdot (1 - \pi_a^B)/\pi_a^B$, which is the desired result.

Heiner (1984) proves this result also holds for the recent 'non-expected' utility theories of Machina, Chew, Fishburn and others. The analysis remains valid with full generality of the probability measures which interrelate the sets A, X and S, as well as no restrictions on the size and topological structure of A, X and S.

It remains to prove Theorem 2 about delaying imperfect response to changing conditions. Recall the two properties that define the set of imperfect adjustment rules $a^{?} \in \beta$. They are stated in weaker versions than discussed in the main text, so as to hold only as ξ approaches zero. That is, $w(\xi)$ and $a_z^w(\xi)$, $a_z^r(\xi)$ are continuous functions for $\xi \geq 0$; where, (1) $w(0) > 0$, and (2) $-a_z^w(0)/a_z^r(0) \geq \lambda$ for some $\lambda > 0$.

Property (1) implies $\rho(\xi) = r(\xi)/w(\xi)$ becomes bounded as ξ approaches zero. Thus, condition (5) of the main text will be violated if $T(\xi)$ becomes *un*bounded as $\xi \to 0$. To show this, note that since $\pi(\xi) = p(t^0 \leq t \leq t^0 + \xi) \to 0$ as $\xi \to 0$, then $(1 - \pi(\xi))/\pi(\xi) \to \infty$ as $\xi \to 0$. Thus, $T(\xi) = l(\xi)/g(\xi) \cdot (1 - \pi(\xi))/\pi(\xi)$ is guaranteed if $l(\xi)/g(\xi)$ does not shrink to zero as $\xi \to 0$.

Before showing the latter, note that we have not ruled out the possibility of responding too rapidly toward the new optimum, so that $\partial a^r/\partial z$ exceeds $\partial a^*/\partial z$. If the former derivative is sufficiently larger than the latter derivative, then $g(\xi)$ may become negative before ξ converges to zero. On the other hand, the loss $l(\xi)$ always remains positive from responses in the wrong direction for all $\xi > 0$ so that $l(\xi)/g(\xi) < 0$ if $g(\xi) < 0$.

This case represents an application of equation (A1) above where the corresponding l and g terms are positive and negative respectively, thereby guaranteeing that the net benefit from responding rather than delaying is negative regardless of how close $r(\xi)$ and $w(\xi)$ are to 1 and 0 respectively. This means sufficient *over*-reacting in the right direction can be detrimental even if agents never respond in the wrong direction (i.e. even if $w(\xi) = 0$). Thus a necessary condition to benefit from immediate reacting to shifts in z is that $l(\xi)/g(\xi)$ remain non-negative as $\xi \to 0$ (that is, $l(0)/g(0) < 0$ implies agents will not benefit from responding immediately according to $a^? \in \beta$). If this condition holds then, as noted above, we must still show that $l(0)/g(0)$ is strictly positive to guarantee that $T(0) = \infty$.

To answer these questions, begin by computing the formulas for (4a) and (4b) of the main text to obtain,

$$l(\xi) = \left\{ \frac{\partial V(a^0, z(t^0+\xi))}{\partial z} - \frac{\partial V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial a} \right.$$
$$\left. \times \frac{\partial a^w(z(t^0+\xi))}{\partial z} - \frac{\partial V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial z} \right\}$$
$$\times \frac{\mathrm{d}z(t^0+\xi)}{\mathrm{d}t} \tag{A2}$$

$$g(\xi) = \left\{ \frac{\partial V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial a} \cdot \frac{\partial a^r(z(t^0+\xi))}{\partial z} \right.$$
$$\left. + \frac{\partial V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial z} - \frac{\partial V(a^0, z(t^0+\xi))}{\partial z} \right\}$$
$$\times \frac{\mathrm{d}z(t^0+\xi)}{\mathrm{d}t}. \tag{A3}$$

The common term $\mathrm{d}z(t^0+\xi)/\mathrm{d}t$ can be cancelled from (A2) and (A3), so that $l(\xi)/g(\xi)$ equals the ratio of { }-bracketed terms, denoted $\bar{l}(\xi)$ and $\bar{g}(\xi)$ respectively. The discussion of the main text prior to the section on 'Inertially adjusting imperfect decisions and expectations' implies both $\bar{l}(\xi)$ and $\bar{g}(\xi)$ go to zero with ξ. Thus, differentiate these with respect to ξ to get the 'second-order' net losses and gains from reacting away or toward the new optimum,

$$\frac{\mathrm{d}\bar{l}(\xi)}{\mathrm{d}t} = \left\{ \frac{\partial^2 V(a^0, z(t^0+\xi))}{\partial z^2} - \frac{\partial^2 V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial z^2} \right.$$
$$- \frac{\partial V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial a} \cdot \frac{\partial^2 a^w(z(t^0+\xi))}{\partial z^2}$$
$$- \frac{\partial a^w(z(t^0+\xi))}{\partial z} \left[\frac{\partial^2 V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial a^2} \right.$$
$$\left.\left. \times \frac{\partial a^w(z(t^0+\xi))}{\partial z} + 2 \frac{\partial^2 V(a^w(z(t^0+\xi)), z(t^0+\xi))}{\partial a \, \partial z} \right] \right\}$$
$$\times \frac{\mathrm{d}z(t^0+\xi))}{\mathrm{d}t} \tag{A4}$$

$$\frac{\mathrm{d}\bar{g}(\xi)}{\mathrm{d}t} = \left\{ \frac{\partial^2 V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial z^2} - \frac{\partial^2 V(a^0, z(t^0+\xi))}{\partial z^2} \right.$$
$$+ \frac{\partial V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial a} \cdot \frac{\partial^2 a^r_z(t^0+\xi))}{\partial z^2}$$
$$+ \frac{\partial a^r(z(t^0+\xi))}{\partial z} \left[\frac{\partial^2 V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial a^2} \right.$$
$$\left.\left. \times \frac{\partial a^r(z(t^0+\xi))}{\partial z} + 2 \frac{\partial^2 V(a^r(z(t^0+\xi)), z(t^0+\xi))}{\partial a \, \partial z} \right] \right\}$$
$$\times \frac{\mathrm{d}z(t^0+\xi)}{\mathrm{d}t}. \tag{A5}$$

As before, the common multiplicative term $\mathrm{d}z(t^0+\xi)/\mathrm{d}t$ can be cancelled from both (A4) and (A5). Recall also that responses in both the right and wrong directions begin from a^0, so that $a^r(z^0) = a^0 = a^w(z^0)$ for $z^0 = z(t^0)$. This implies the difference between the first two terms in the curly brackets of (A4) and (A5) both go to zero. It also implies $\partial V(a^0, z^0)/\partial a = 0$ so that the third term in both (A4) and (A5) goes to zero. Thus, the ratio of (A4) to (A5) reduces to the following expression at $\xi = 0$,

$$\frac{\mathrm{d}\bar{l}(0)/\mathrm{d}t}{\mathrm{d}\bar{g}(0)/\mathrm{d}t}=\frac{-\dfrac{\partial a^{w}(z^{0})}{\partial z}\left[\dfrac{\partial^{2}V(a^{0},z^{0})}{\partial a^{2}}\cdot\dfrac{\partial a^{w}(z^{0})}{\partial z}+2\dfrac{\partial^{2}V(a^{0},z^{0})}{\partial a\,\partial z}\right]}{\dfrac{\partial a^{r}(z^{0})}{\partial z}\left[\dfrac{\partial^{2}V(a^{0},z^{0})}{\partial a^{2}}\cdot\dfrac{\partial a^{r}(z^{0})}{\partial z}+2\dfrac{\partial^{2}V(a^{0},z^{0})}{\partial a\,\partial z}\right]}. \tag{A6}$$

To simplify the remaining calculations, let $u=\partial a^{w}(z^{0})/\partial z$, $v=\partial a^{r}(z^{0})/\partial z$, $A=\partial^{2}V(a^{0},\ z^{0})/\partial a^{2}$, $B=\partial^{2}V(a^{0},\ z^{0})/\partial a\,\partial z$. Expression (A6) then reduces to

$$\frac{\mathrm{d}\bar{l}(0)/\mathrm{d}t}{\mathrm{d}\bar{g}(0)/\mathrm{d}t}=\frac{-u(Au+2B)}{v(Av+2B)}. \tag{A6$'$}$$

Let $k=-u/v$, and recall that assumption (1) above implies $-u/v\geq\lambda$ for some $\lambda>0$, so that $k>0$ must hold. Then substitute $-kv=u$ in place of u in (A6′),

$$\frac{-(-kv)(-Akv+2B)}{v(Av+2B)}=k\cdot\frac{2B-kAv}{2B+Av}. \tag{A7}$$

At this point recall that the second-order conditions for maximizing $V(a,z)$ at $z=z^{0}$ imply $A<0$, and in addition the optimum response derivative $v^{*}=\partial a^{*}(z^{0})/\partial z=-B/A$. That latter in turn implies $v^{*}B\geq 0$ (i.e., u^{*} and B must have the same sign). Since $\partial a^{r}(z^{0})/\mathrm{d}z$ responds in the same direction as $\partial a^{*}(z^{0})\partial z$, then $vB>0$ also holds (so v and B have the *same* sign).

Now consider two cases of $B\geq 0$ and $B<0$. If $B\geq 0$ then $v\geq 0$, which in turn implies the *numerator* of (A7) is positive. If in addition $v>-2B/A$, then the *denominator* of (A7) is negative. Since $k>0$, the latter implies (A7) is negative, which violates the necessary condition discussed prior to (A2) and (A3) above. On the other hand, if $v\leq -2B/A$, then the denominator of (A7) is nonnegative, so that (A7) is positive. This in turn implies $l(0)/g(0)>0$ by l'Hospital's rule (which then implies $T(0)=\infty>\rho(0)=r(0)/w(0)$ as discussed prior to (A2) and (A3)).

The above results imply that (A7) ranges between $k>0$ and $+\infty$ for $0\leq v\leq -2B/A$ and between $-\infty$ and $-k^{2}<0$ for $v>-2B/A$.

Finally, consider the last case of $B<0$, which implies that $v<0$ since v and B have the same sign. Similar argument to the case of $B\geq 0$ implies (A7) ranges between $k>0$ and $+\infty$ for $-2B/A\leq v<0$ and from $-\infty$ to $-k^{2}<0$ for $v<-2B/A$.

Thus, regardless of the sign of B, either: (A7) is strictly negative (thus violating the necessary condition discussed prior to (A2) and (A3)); or (A7) is strictly positive (thus guaranteeing $T(0)=\infty$, so that $\rho(0)>T(0)$ is also violated). Hence, in all cases agents cannot benefit from responding according to any $a^{?}\in\beta$ for ξ sufficiently close to zero.

Notes

1. For example, Arrow's concluding remarks (1986, p. S397) include the following: 'The main implication of this extensive examination of the use of the rationality concept in economic analysis is the extremely severe strain on information-gathering and computing abilities…many of the customary defenses that economists used to argue…break down as soon as market power and incompleteness of markets are recognized…the combination of rationality, incomplete markets, and equilibrium in many cases leads to very weak conclusions, in the sense that there are whole continua of equilibria.'
2. For example, in the special case where A, S, X are finite sets, then for each $(a^{0}, s^{0})\in A'\times S'$, we have

$$P_{A'\times S'}(a^{0},s^{0})=\sum_{x}\frac{p(a^{0}|x)p(x|s^{0})p(s^{0})}{p(A',S')};$$

where

$$p(A',S')=\sum_{A'}\sum_{X}\sum_{S'}p(a|x)p(x|s)p(s).$$

Similarly, for each $(a^{0}, s^{0})\in A'\times S$, we have

$$P_{A'\times X'}(a^{0},s^{0})=\sum_{X'}\frac{p(a^{0}|x)p(x|s^{0})p(s^{0})}{p(A',X')};$$

where

$$p(A',X')=\sum_{A'}\sum_{X'}\sum_{S}p(a|x)p(x|s)p(s).$$

See Heiner (1984) for a precise discussion of the above when $p\in P$ are defined over algebras of both countable and noncountable sets A, S, X.

3. Note that it is not necessary that a fully optimal decision exist for each message $x\in X$. Indeed, no optimal strategy in general exists for certain dynamic evolutionary environments (as noted in Allen's and Silverberg's Chapters 5 and 24). The above concepts instead only require a ranking of whether it is preferred or not to deviate from a given behavior pattern $B(x)$ toward particular actions $a\notin B(x)$. We can thus apply the analysis to situations (such as competition between firms in dynamic game theory models) where optimality notions may not be fully determinant. This is briefly discussed in the section concerning inertial adjustment within dynamic evolutionary models.
4. This implication provides a theoretical rationale for distributed lag models of expectation adjustment often referred to as 'adaptive expectations' (see, for example, Cagan, 1956; Muth, 1960; Cooley and Prescott, 1973). Adaptive expectations have been criticized as seemingly 'ad hoc' or 'irrational' in not optimally adjusting to changes in the stochastic properties of an agent's environment (in favor of more recent 'rational expectations' models; see Lucas, 1981; Sargent, 1979). However, once the behavioral effects of imperfect expectations are explicitly modeled (instead of ruled out by hypothe-

sis), a generic tendency to delay expectation adjustment becomes systematically relevant. The opposite tendency toward immediately adjusting expectations applies only near the limit where agents become perfectly reliable at doing so.

The above analysis also accords with recent results by Smith, Suchanek and Williams (1986) concerning the behavior of prices and expectation formation in experimental spot asset markets. The rational expectations hypothesis introduced by Muth (REM) predicts well only after a sequence market periods where asset prices eventually converge to equilibrium patterns. However, the dynamic path leading to such convergence displays systematic adaptive expectation features, as indicated in the concluding sentence of their paper (1986, p. 36), '...the general conclusion [is] that the REM model of asset pricing is supported only as an *equilibrium concept* underlying an adaptive capital gains adjustment process' [emphasis original].

Finally, note that Lucas (1986) has recently suggested that adaptively adjusting expectations may be the essential factor in determining which rational expectation equilibria are dynamically stable.

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8

On the dynamics of aggregate macroequations: from simple microbehaviors to complex macrorelationships

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Introduction

Hyperrational, hypercompetent economic agents have been the heart of most of macroeconomics and macroeconometrics in the last two decades. The menu of alternatives out of which they are assumed to be able to pick up the best one has been enlarged from familiar finite-dimensional spaces to temporal paths going from the current period to the most remote future, namely from t to infinity, while the expected magnitudes needed to evaluate the objective functions usually turn out from optimal processing of all the available information about the past.

Thus, for instance, demand for labor at time t, n_t, will be set along with the whole path n_{t+j}, j running from 0 to infinity, the path $\{n_{t+j}\}$ depending on the expected values, from $t+1$ to infinity, of, say, wage and output. Then, by eliminating such expected magnitudes according to some theory of the agents' expectations, one will obtain an equation between observables. Analogous procedures are applied to consumption as a function of income, demand for capital as a function of capital price, price as a function of cost, and so on.

The dynamic shape of the equation obtained depends both on the shape of the objective function and on the hypothesis on agents' expectations. Rational expectations, together with intertemporal objective functions containing terms accounting for the distance between current and desired level of the dependent variable as well as some kind of adjustment cost, yield a rich variety of dynamic equations between observables. One gets usually difference equations, that is equations containing the dependent lagged variable in addition to lagged values of the independent ones. Such dynamic equations have been extensively and quite successfully employed to fit actual aggregated macroeconomic time series.

The microeconomic foundations of macroeconomics briefly summarized above have been criticized from many points of view. It has been observed that the uncertainty faced by economic agents operating in a capitalist economy cannot be adequately represented in terms of a probability distribution based on observed frequencies. Moreover, according to an important theoretical approach, to which this book is strictly linked, agents' behaviors are better described by non-optimal, slowly changing

routines, rather than a continuous process of choice within broad menus. As Nelson and Winter referring to firm behavior have put it:

> As a first approximation...firms may be expected to behave in the future according to the routines they have employed in the past...it is quite inappropriate to conceive of firm behavior in terms of deliberate choice from a broad menu of alternatives that some external observer considers to be 'available' alternatives. The menu is not broad but narrow and idiosyncratic....[1]

In the present chapter we shall not deal directly with the behavioral issue. The problem we shall deal with is the correspondence between microbehaviors and observed relations linking macroeconomic time series. The question is whether we must consider the dynamic complexity of macroequations as evidence that the underlying microbehaviors are complex as well, or we can think that aggregate complexity can derive also, *by aggregation*, from dynamically simple microbehaviors. If the second alternative is shown to be possible, the way is open to challenge neoclassical macroeconomics by suggesting explanations of the available empirical evidence based on the above mentioned routinized microbehaviors.

Of course such an attempt can be undertaken only if we abandon a restrictive assumption upon which most of macroeconomics relies, namely the representative agent. Typically, macroeconomic models accept, as an 'explanation', or a 'microeconomic foundation' for a relationship among aggregate variables the behavior of a *single* maximizing agent, provided the equation derived from the maximization reproduces the main features of the given macroequation. We shall see that a significant difference between micro and macrodynamics emerges whenever one departs from the representative agent assumption with respect to two different aspects: first, there must be differences in the agents' behaviors; second, it is necessary that the independent variables corresponding to different agents do not have the same dynamic behavior, i.e. that different agents face different independent variables.

As for the first aspect—the existence of differences between agents' behaviors—even casual empiricism suggests this is the norm whenever the aggregates are so large as to include, for instance, different industries or different firms within one industry. However, also in the case where more homogeneous agents are aggregated, if one just allows for differences in tastes, available techniques, information-processing competence, on the one hand, and for changing environment, on the other hand, it will be easy to accept different behaviors. Indeed, even the constancy of such differences can be assumed only as a working hypothesis.

As for the second aspect—different agents face different independent variables—an important consequence of the representative agent must be pointed out. The representative agent who decides, say, aggregate consumption y_t, takes as independent variable the *aggregate* income x_t. This entails that the x_t variable, just because it is a result of aggregation over large numbers of agents and goods, does possess a dynamic regularity that could not be attributed to the variables actually faced by individual agents. Individual incomes, as well as individual prices, as well as many individual variables, have very often the aspect of fairly ugly jump processes, rather than the pleasant smoothness of the ARIMA models that have been successfully employed to approximate the dynamic behavior of macroeconomic aggregates.

Now, it must be pointed out that the representation we choose for the individual independent variables does influence the way we theorize about individual behaviors. It is apparent that dynamically regular x_t's variables contribute to making it plausible rational expectations and intertemporal optimization extended to the remote future; while dynamically irregular x_t's may give support to simple routinized behaviors.

In conclusion, assuming the representative agent has two implications: first, behavior is uniform across agents; second, the dynamic features of aggregate variables are illicitly transferred to individual variables. Obviously, non-regular, non-uniform across agents, individual independent variables, upon which the agents base their decisions, are highly plausible features of the economic environments discussed in this book, characterized by various forms of technological and institutional change. However, we shall show, differences between micro and macroequations, emerge even if we simply assume that the x_t's are regular but different across agents while the microbehaviors are constant but different.[2] These assumptions, in spite of the little change they mean if compared with the representative agent practice, have proved useful to get interesting results. The dismissal of regularity and constancy of behaviors should lead to an even deeper understanding of the micro–macro issue.

Lastly, a reference to Ronald Heiner's work on routinized behavior (Heiner, 1983, and chapter 7 in the present volume) is necessary. Heiner is trying to show that

> observed regularities of behavior can be fruitfully understood as 'behavioral' rules that arise because of uncertainty in distinguishing preferred from less preferred behavior. Such uncertainty requires behavior to be governed by mechanisms that restrict the flexibility to choose potential actions, or which produce a selective alertness to information that might prompt particular actions to be chosen. These mechanisms simplify behavior to less complex patterns, which are easier for an observer to recognize and predict. [Heiner, 1983, p. 561].

Now, what we are trying to show is that, as long as observations refer to the aggregates of macroeconomics, observed regularities can be understood by starting with microbehaviors that are much simpler than the observed regularities themselves. In particular, as we shall see, a dynamic regular relation between aggregated data can be consistent with static behaviors of the underlying agents.

As the content of the chapter is rather technical, an overview of problems and results will be helpful both for the reader willing to come to grips with the technicalities and for the reader willing to have only a broad understanding of the results.

Let us consider the following example of a macroeconomic relationship, where y_t and x_t are assumed to be aggregate variables,

$$y_t - \alpha y_{t-1} = a x_t + b x_{t-1} + u_t, \qquad (1)$$

in which u_t is a white noise. As stated above, the most usual explanation of equation (1) consists of the description of a single, intertemporally optimizing, representative agent: from the maximization of his objective function, whose arguments are current and expected values of specified variables, after having eliminated the expected variables on the basis of a theory of expectations, an equation reproducing the main dynamic features of (1) must result.[3]

Thus for equation (1) an explanation will be necessary for the presence of the lagged variable y_{t-1}. *If* $0 < \alpha < 1$, as usual, such an inertial feature is often attributed to an adjustment cost present in the objective function preventing the agent from immediately fixing the variable y_t at its desired level.

Our main analytical point consists in showing that if the representative agent is abandoned, that is if the aggregate nature of y_t and x_t is seriously taken into consideration, then the dynamic form of the relationship linking y_t and x_t can be dramatically different from the form of the corresponding microequations, the former being normally much more complicated. In particular, an equation like (1) can be explained by much simpler microbehaviors. For instance, assume that two agents follow this routine:

$$y_{it} = \Pi_i x_{it},$$

which is a static rule not containing any stochastic term. Assume further that the independent microvariables are generated by the following autoregressive processes:

$$x_{it} - \alpha_i x_{it-1} = v_{it}, \quad 0 < \alpha_i < 1,$$

where the v_{it} are orthogonal white-noise processes.[4] If $\Pi_1 \neq \Pi_2$, $\alpha_1 \neq \alpha_2$ then the relationship linking the aggregates $y_t = y_{1t} + y_{2t}$ and $x_t = x_{1t} + x_{2t}$ has the form (1).

The macroequations we have considered above, demand for labor, consumption as a function of income, price–cost, demand for capital, provide good examples of the aggregation problem we wish to deal with. In these instances: (a) both sides of the equation must be aggregated; (b) in the microequations linking the dependent microvariables to the independent ones different microparameters correspond, in general, to different agents; (c) moreover, the independent microvariables corresponding to different agents are generated by different stochastic processes. We shall show that when (a), (b), (c) occur, as in the case of the above two-agent example, the dynamics of the independent microvariables yields a complication of the macroequation dynamics. In particular, even though the microequations are simple routines such as

$$y_{it}=\Pi_i x_{it}+u_{it},\quad y_{it}=\Pi_i x_{it-k_i}+u_{it},\quad y_{it}=\Pi_i \sum_{s=0}^{m_i} x_{it-s}+u_{it},$$

where u_{it} is white-noise, the aggregate equation has the shape:

$$a(L)y_t=b(L)x_t+c(L)u_t,$$

where u_t is a white-noise process and, if we leave aside special negligible cases:

(1) the polynomials $a(L)$, $b(L)$, $c(L)$ are non-trivial, that is they are of positive degree;

(2) the shape of the aggregate equation is quite general: it is neither a rational distributed-lag model with a white-noise disturbance, that is $a(L)\neq c(L)$; nor a finite distributed-lag model with a ARMA disturbance, that is $a(L)$ is not a factor of $b(L)$;

(3) even though the explanatory microvariables are not Granger-caused by the dependent microvariables, the explanatory macrovariable is Granger-caused by the dependent macrovariable;

(4) the macroparameters depend not only on the parameters of the microequations but also on the parameters of the independent microprocesses; a phenomenon usually explained by rational expectations in recent literature may therefore be due to aggregation as well.

A short restatement of our central thesis will perhaps avoid possible misunderstandings. Within all of the models analyzed below the independent microvariables are generated by dynamic processes: the latter will be the main source of the dynamics of the macroequations. With the sole exceptions of the multiplier-accelerator model of Section 3, the question of the origin of the dynamics of the independent microvariables will not be explicitly examined in this paper: our main aim here is to show that aggregation causes a 'propagation' of the dynamics—which is *assumed* to affect the independent microvariables—to macrorelationships whose micro background is not necessarily dynamic, or is dynamic in a much simpler form. Thus we are not, obviously, denying the existence of dynamic behaviors in the economic system. We deny that the dynamics of an equation linking aggregate variables necessarily and adequately reflects the behavior of the agents underlying those variables.

The chapter is subdivided into the following sections. In Section 1 we shall set out the analytic problem. After a brief account of the simple case in which all agents face the same independent variable, we shall deal with the general case in which to different agents there correspond different, though co-stationary, independent variables. A general solution to the problem of determining the aggregate equation is given. In Section 2 we work out a computable case, namely that where there are only two different agents. In this case the macroparameters can be shown to be simple explicit functions of the microparameters. The above results, (1) to (4), are proved to hold. In Section 3 we analyze two simple prototypes: a price–cost equation, a consumption–income equation in the context of a multiplier-accelerator model. In Section 4 we outline the way the results obtained in Section 2 for the two-agent model can be generalized to the many-agent case. We also show that the dynamic shape of the aggregate equation, though complex in general, is quite unpredictable, not only in the case where the microequations are static but also when the microequations are dynamic. Lastly we briefly review the economeric literature on aggregation and dynamics. We shall argue that even in the few cases in which the problem is mentioned, the lack of analysis leads to understate the dynamics complications arising from aggregation.[5]

1. The problem in general

1.1 The same independent variable for all agents

Throughout Section 1 we shall refer to a consumption–income relationship. This is merely a convenient example in order to familiarize the reader with the problem.

We shall assume that consumers' behavior follows the microequations:

$$a_i(L)y_{it}=b_i(L)x_{it}+u_{it}, \tag{2}$$

where y_{it} and x_{it} are, respectively, consumption and income of the i-th agent, $a_i(L)$ and $b_i(L)$ are polynomials in the lag operator L, u_{it} is a white-noise orthogonal to x_{jt-k} and y_{jt-h}, for any j, k and $h>0$; moreover, u_{it} and u_{jt} are orthogonal processes for $i\neq j$; lastly $a_i(0)=1$.

Furthermore we assume that only the aggregate variables:

$$y_t=\sum x_{it},\quad x_t=\sum x_{it},$$

are available. Under rather general assumptions on the x_{it} processes, partly specified below, it is possible to show that microequations (2) determine completely the *empirical macroequation*[6] or, as we shall also say, the *aggregate equation* linking y_t and x_t. Otherwise stated, there exist (finite) polynomials $a(L)$, $b(L)$, $c(L)$ and a white-noise u_t such that:

$$a(L)y_t=b(L)x_t+c(L)u_t, \tag{3}$$

where $a(0)=c(0)=1$, u_t is orthogonal to y_{t-k} and x_{t-h}, $k>0$, $h\geq 0$, $c(L)$ has no roots of modulus smaller than one. Moreover, $a(L)$, $b(L)$, $c(L)$ and u_t are unique.[7]

Equation (3) may be thought of as the factual counterpart for an investigator starting with a theory referred to a representative agent, deriving from that theory a dynamic relationship between y_t and x_t, and trying to specify, estimate and interpret such a relationship. As we shall see, aggregation tends to dynamize every relationship. Therefore, given the acceptance of the representative agent practice, the explanation of macroequations will be strongly biased in favour of dynamic behaviors on the part of agents.

Before starting with the formal analysis we must point out that in the above definition of the empirical macroequation we implicitly supposed that, on the basis of the theory he is relying on, the investigator assumes as plausible that the relationship between y_t and x_t can be *identified* by the orthogonality of u_t to x_t (in addition to the usual orthogonality of u_t to the past of x_t and y_t; see the conditions below equation (3)). This is a simplifying hypothesis on the investigator's assumptions. We shall maintain it throughout this work, with the exception of the model in Section 3.2.

Let us start with the simplest case: we assume that there is no difference in the dynamic behaviors of the x_{it} variables: $x_{it}=r_i x_t$, $\Sigma r_i=1$, i.e. individual incomes are constant fractions of national income. When such an assumption holds we can immediately write:

$$y_t=\left(\sum r_i\frac{b_i(L)}{a_i(L)}\right)x_t+\sum\frac{u_{it}}{a_i(L)}. \tag{4}$$

While no difficulty arises with y_t and x_t, obtaining the macroequation in the form (3) requires some effort owing to the second sum in (4). Since the solution of the problem may afford insight into more general issues we shall deal with it in some detail.

Assume for the sake of simplicity that the number of agents is 2 and that $a_i(L)=1-\alpha_i L$, $|\alpha_i|<1$. Then the second sum in (4) equals:

$$\frac{(1-\alpha_2 L)u_{1t}+(1\alpha_1 L)u_{2t}}{(1-\alpha_1 L)(1-\alpha_2 L)}.$$

It is not difficult to show that there exists a scalar α and a white-noise u_t such that:

$$(1-\alpha_1 L)u_t=(1-\alpha_2 L)u_{1t}+(1-\alpha_1 L)u_{2t}.$$[8]

We note that once α is determined, for u_t we have:

$$\begin{aligned}u_t=u_{1t}&+(\alpha-\alpha_2)u_{1t-1}+\alpha(\alpha-\alpha_2)u_{1t-2}+\alpha^2(\alpha-\alpha_2)u_{1t-3}+\cdots\\&+u_{2t}+(\alpha-\alpha_1)u_{2t-1}+\alpha(\alpha-\alpha_1)u_{2t-2}+\alpha^2(\alpha-\alpha_1)u_{2t-3}+\cdots.\end{aligned} \tag{5}$$

Starting from (5) it is easily seen that u_t is orthogonal to x_{t-k}, any k, and to y_{t-h}, $h>0$.[9] Thus the empirical macroequation is:

$$\begin{aligned}(1-\alpha_1 L)(1-\alpha_2 L)y_t=&[r_1 b_1(L)(1-\alpha_2 L)+r_2 b_2(L)(1-\alpha_1 L)]x_t\\&+(1-\alpha L)u_t.\end{aligned} \tag{6}$$

First it must be pointed out that (6) is more complicated compared with the corresponding microequations (2). Secondly, going back to (5) we see that the disturbance term u_t, belonging to the aggregate equation, has no immediate economic meaning. As a matter of fact, far from being a sum of simultaneous microdisturbances, u_t is a highly artificial white-noise obtained by using the whole past of u_{1t} and u_{2t}, in addition to u_{1t} and u_{2t} themselves. Moreover, the parameters r_1 and r_2, which are not related to microbehaviors of agents but only to the independent microvariables, influence the parameters of the aggregate equation.

Notice lastly that apart from the last observation the same results obtain if the assumption $x_{it}=r_i x_t$ is replaced by $x_{it}=x_t$.

1.2 *Dynamically different independent variables: a dynamic background*

Let us now drop the extreme assumption $x_{it}=r_1 x_t$; i.e. let us suppose that the share of x_{it} in x_t is not constant. It is intuitively obvious that aggregation of microequations (2) requires the knowledge of the processes generating individual incomes. In other words, precise assumptions on the background of our consumption-income equation are necessary.

We shall base ourselves upon the so-called Slutsky–Frisch approach, that is, we shall assume that the economic system, or the part of it we are interested in, can be adequately represented by a system of linear stochastic difference equations:

$$A(L)z_t=B(L)\xi_t, \tag{7}$$

where z_t is a stochastic vector whose components are variables relative to individual agents, ξ_t is a white-noise vector, all roots of det($A(L)$) are of modulus greater than one. Obviously we are assuming that equations (2) are part of system (7) and therefore that the variables y_{it} and x_{it} are among the components of vector z_t.

The assumption on the roots of det($A(L)$) entails that there exist a stationary solution of (7). The latter is unique and can be written in the following way:

$$z_t=A(L)^{-1}B(L)\xi_t. \tag{8}$$

To this solution we shall always refer below.[10]

Before we go on working out expression (8) let us comment on the general assumptions, i.e. the representation of individual microvariables by system (7). This entails, as a consequence, the co-stationarity of the components of the vector z_t, the latter being variables referred to individual households or firms. Co-stationarity, in turn, means that the covariance structure of those components, both the cross-covariances $\mathrm{E}(z_{it}z_{jt-k})$ and the covariances $\mathrm{E}(z_{it}z_{it-k})$, are constant through time. This is, as we pointed out in the Introduction, a very unrealistic picture of individual variables and must be recognized as the main limit of the present work. However, a background like (7) makes the problem mathematically manageable while it does not appear to be unfair towards the point of view we are trying to criticize.

Moreover, we would argue that if a sizeable dynamic difference between micro and macroequations can be obtained within the co-stationarity assumption, it is likely that even more interesting results could be obtained in a more realistic framework, in which co-stationarity is a feature of aggregated, not of individual variables.[11]

As a further observation on system (7) we note that, in general, the parameters of the matrices $A(L)$ and $B(L)$ and the variance–covariance matrix of ξ_t are non-linear mixtures of behavioral parameters: suffice, by way of example, the case of behaviors based on expectations and the elimination of the latter.

1.3 A general formula for the aggregate equation

Vector stochastic stationary processes like (7) can be given a standard representation, the well-known Wold Representation (W.R. from now on):

$$z_t = M(L)\chi_t, \tag{9}$$

where χ_t is a white noise vector having the same dimension (number of components) as z_t (note that the dimensions of z_t and ξ_t can differ), $M(L)$ is a square matrix whose coefficients are rational functions of L, $\det(M(L))$ has no roots of modulus smaller than one, $M(0)=I$, the identity matrix.[12]

The difference between (8) and (9) lies in the fact that the former is still explicitly linked to the behavioral equations, whereas the latter is a standard representation summing up the correlation structure of the vector z_t. The examples in Section 3 will give some insight into the relationship between representations (8) and (9).

The last step necessary to obtain the aggregate equation consists in isolating the components y_{it} and x_{it} within z_t and aggregating. The resulting vector $(y_t\ x_t)$ is obviously a stationary vector. Moreover, considering its W.R.:

$$\begin{pmatrix} y_t \\ x_t \end{pmatrix} = D(L)\eta_t = \begin{pmatrix} d_{11}(L) & d_{12}(L) \\ d_{21}(L) & d_{22}(L) \end{pmatrix} \begin{pmatrix} \eta_{1t} \\ \eta_{2t} \end{pmatrix}, \tag{10}$$

where η_t is a white-noise two-dimensional vector, $\det(D(L))$ has no roots of modulus smaller than one, $D(0)=I$, we observe that the rationality of $M(L)$ entails that $d_{ij}(L)$ also are rational functions.[13]

Once (10) is obtained we need only some elementary algebra to get the aggregate equation. First multiply both sides of (10) by the adjoint of $D(L)$:

$$\begin{pmatrix} d_{22}(L) & -d_{12}(L) \\ -d_{21}(L) & d_{11}(L) \end{pmatrix} \begin{pmatrix} y_t \\ x_t \end{pmatrix} = \det(D(L)) \begin{pmatrix} \eta_{1t} \\ \eta_{2t} \end{pmatrix}.$$

Then multiply the second equation by a scalar K and subtract from the first:

$$(d_{22}(L) + Kd_{21}(L))y_t = (Kd_{11}(L) + d_{12}(L))x_t + \det(D(L))u_t, \tag{11}$$

where $u_t = \eta_{1t} - K\eta_{2t}$. For any K we find that u_t is orthogonal to y_{t-k} and x_{t-k} for $k>0$, since η_{it} is orthogonal to the past of x_t and y_t, $i=1, 2$. If we put

$$K = \frac{\operatorname{cov}(\eta_{1t}, \eta_{2t})}{\operatorname{var}(\eta_{2t})}$$

then u_t is also orthogonal to x_t.

The aggregate equation is got by eliminating denominators and common factors from the rational functions present in (11).

2. Two agents, exact microrelationships

2.1 Explicit formulas for the macroequation coefficients

Let us return to the process we described above starting from (7) and eventually reaching (11). It is impossible to model the coefficients of the latter as explicit functions of the parameters of (7) or of (9) without making some further assumptions. The following case, obtained thanks to drastic simplifications, appears to be both a convenient paradigm and a useful device to get some insight into the general model.

Let us write equations (2) again:

$$a_i(L)y_t = b_i(L)x_t + u_{it}.$$

We assume:

(a) $u_{it}=0$ for any i, that is the microequations are exact;

(b) the agents are divided into two groups: to the members of each one corresponds the same ratio $b_i(L)/a_i(L)$; let us define: $\pi_1(L) = b_i(L)/a_i(L)$, i in the first group, $\pi_2(L) = b_i(L)/a_i(L)$, i in the second group.

(c) $\pi_1(L) - \pi_2(L)$ has no roots of modulus less than one. In particular, making $\Pi_i = \pi_i(0)$, we get $\Pi_1 \neq \Pi_2$. This assumption is there only for mathematical convenience.

First we aggregate the x_{it} within each group: for $i=1, 2$, $X_{it} = \Sigma x_{jt}$, j varying within the i-th group. (Sometimes, as no confusion will arise, we shall avoid a proliferation of symbols and use x_{it} instead of X_{it} for $i=1, 2$, as though the agents were actually two.)

Secondly, as the vector (X_{it}) is obtained by summing components of z_t, it also will have a rational W.R. (see Note 13):

$$\begin{pmatrix} X_{1t} \\ X_{2t} \end{pmatrix} = \begin{pmatrix} a_{11}(L) & a_{12}(L) \\ a_{21}(L) & a_{22}(L) \end{pmatrix} \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix}, \tag{12}$$

where $a_{ij}(L)$ are rational functions of L, $A(0)=I$, $A(L)$ has no roots of modulus less than one. We assume further that:

(d) the variance–covariance matrix of (v_{it}) is non-singular.

For the vector (y_t, x_t) we have:

$$\begin{pmatrix} y_t \\ x_t \end{pmatrix} = \begin{pmatrix} \pi_1(L) & \pi_2(L) \\ 1 & 1 \end{pmatrix} \begin{pmatrix} X_{1t} \\ X_{2t} \end{pmatrix}$$

$$= \begin{pmatrix} \pi_1(L) & \pi_2(L) \\ 1 & 1 \end{pmatrix} \begin{pmatrix} a_{11}(L) & a_{12}(L) \\ a_{12}(L) & a_{22}(L) \end{pmatrix} \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix}. \tag{13}$$

From (13) the W.R. of (y_t, x_t) can be obtained immediately:

$$\begin{pmatrix} y_t \\ x_t \end{pmatrix} = D(L)\,\eta_t, \tag{14}$$

where

$$D(L) = \begin{pmatrix} \pi_1(L) & \pi_2(L) \\ 1 & 1 \end{pmatrix} \begin{pmatrix} a_{11}(L) & a_{12}(L) \\ a_{21}(L) & a_{22}(L) \end{pmatrix} \begin{pmatrix} \Pi_1 & \Pi_2 \\ 1 & 1 \end{pmatrix}^{-1}$$

$$\eta_t = \begin{pmatrix} \Pi_1 & \Pi_2 \\ 1 & 1 \end{pmatrix} \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix}.$$

Applying (11) we obtain:

$$a(L)y_t = b(L)x_t + c(L)u_t,$$

where

$$a(L) = \frac{\Gamma_1(a_{12}(L) + a_{22}(L)) + \Gamma_2(a_{11}(L) + a_{21}(L))}{\Gamma}$$

$$b(L) = \frac{\Gamma_1(\pi_1(L)a_{12}(L) + \pi_2(L)a_{22}(L)) + \Gamma_2(\pi_1(L)a_{11}(L) + \pi_2(L)a_{21}(L))}{\Gamma}$$

$$c(L) = \frac{\pi_1(L) - \pi_2(L)}{\Gamma}(a_{11}(L)a_{22}(L) - a_{12}(L)a_{21}(L))$$

$$u_t = (\Pi_1 v_{1t} + \Pi_2 v_{2t}) - K(v_{1t} + v_{2t}) \tag{15}$$

$$K = \frac{\mathrm{cov}(\Pi_1 v_{1t} + \Pi_2 v_{2t}, v_{1t} + v_{2t})}{\mathrm{var}(v_{1t} + v_{2t})}$$

$$\Gamma = \Pi_1 - \Pi_2, \quad \Gamma_1 = \Pi_1 - K, \quad \Gamma_2 = K - \Pi_2.$$

Notice that the value of K corresponds to the hypothesis that the investigator is assuming orthogonality between u_t and x_t. As with (11), the aggregate equation is got once denominators and common factors have been eliminated.

2.2 *Some results on the dynamics of the macroequation*

In equation (15) the way the dynamics of the aggregate equation is affected by the dynamics of the independent variables can be explicitly observed. In particular, if $\pi_i(L) = \Pi_i$, i.e. if microbehaviors are static, the aggregate equation dynamics is entirely due to the dynamics of the independent microvariables.

Moreover, a simple inspection of formulas (15) shows the dependence of the macroparameters on the parameters belonging to the microprocesses generating the x_{it} variables, and not only on their 'natural' micro counterpart (that is the coefficients of equations (2)).

Furthermore, unless the microparameters assume special values (see 2.3 below) we shall have $a(L) \neq c(L)$ while the ratio $b(L)/a(L)$ will not be a (finite) polynomial. Thus the aggregate equation is a rather general one. In fact we cannot write it in the rational distributed-lag form:

$$y_t = \frac{b(L)}{a(L)}x_t + u_t,$$

nor in the form of a finite distributed-lag model with a ARMA disturbance:

$$y_t = \pi(L)x_t + \frac{c(L)}{a(L)}u_t,$$

where $\pi(L)$ is a polynomial.

In the two-agent model we are examining, the possibility of a feedback from the variables y_{it} to the x_{it} is not ruled out. This is not immediately evident from the W.R. of (X_{1t}, X_{2t}), whereas it would be in the W.R. of z_t or in system (7).[14] However, both when a microfeedback is present and, in the opposite case, the term in the lower left-hand corner of the $D(L)$ matrix is:

$$\frac{1}{\Pi_1 - \Pi_2}[(a_{11}(L) - a_{22}(L)) + (a_{21}(L) - a_{12}(L))];$$

thus, for the aggregate variables a feedback is always present unless, as noted for the other aggregation effects, the microparameters assumed special values.[15]

Finally the aggregate disturbance, being a linear combination of the disturbances belonging to the W.R. of (X_{1t}, X_{2t}), has no behavioral meaning. Nor has it an expectational meaning since microequations (2) are already, by assumption, in a solved form, with respect to possible expected variables.

We conclude this subsection by applying formulas (15) to the example we considered in the Introduction. For (x_{1t}, x_{2t}) we have:

$$\begin{pmatrix} 1 - \alpha_1 L & 0 \\ 0 & 1 - \alpha_2 L \end{pmatrix} \begin{pmatrix} x_{1t} \\ x_{2t} \end{pmatrix} = \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix}.$$

From (15) we get:

$$\left(1-\frac{\Gamma_1\alpha_1+\Gamma_2\alpha_2}{\Gamma}L\right)y_t=K\left(1-\frac{\Gamma_1\Pi_2\alpha_1+\Gamma_2\Pi_1\alpha_2}{K\Gamma}L\right)x_t+u_t,$$

where the coefficients Γ, Γ_1 and Γ_2 are defined as in (15). The coefficients of L in the polynomials are different weighted averages of α_1 and α_2 and are therefore in general different.

2.3 Differences between agents' behaviors and between independent variables

The two-agent exact microequation model lends itself to some considerations easy to extend to non-exact microbehaviors and to more than two different agents.

Needless to say, in order to have any aggregation effect $\pi_1(L)$ must be different from $\pi_2(L)$.[16] For instance, in the case of a consumption equation, if the consumers follow static rules, aggregation effects arise only if there are differences in the propensities to consume. But rather serious aggregation effects arise also in the case where the propensities to consume are equal, provided there are differences in the time shape of the response: for instance, assume consumers can be gathered in two groups, such that for the first we have $y_{it}=ax_{it}$, while for the second $y_{it}=ax_{it-1}$; or, the first microequation being unchanged, for the second group $y_{it}=(a/4)(1+L+L^2+L^3)x_{it}$.

Also in the case of a price–cost equation, if individual prices are set following the rule of a fixed mark-up over the unit cost, then the more different the mark-ups the more relevant the aggregation effect; or the more different the periods of production across firms within the aggregate (assuming firms are charging the mark-up over the historical or the average cost), the sharper the aggregation effect.[17]

Yet it must be underlined that behavioral differences among agents are necessary but not sufficient in order for the aggregation effects listed in 2.2 to occur. As a matter of fact, condition (d) (see 2.1) regards the independent microvariables, not the microbehaviors. Its meaning can be conveniently clarified by showing significant examples on both what it does and what it does not rule out.

First, the case $X_{1t}=rX_{2t}$, i.e. X_{1t} is equal to X_{2t} up to a scalar, is excluded. This, in turn, means that we are ruling out not only that for any i and j, $i\neq j$, we have $x_{it}=x_{jt}$ (up to a scalar), but also the following, much less trivial, case: given an agent i in group 1, there exists an agent j in group 2 such that $x_{it}=rx_{jt}$, and vice versa, given an agent h in group 2, there exists an agent k in group 1 such that $x_{kt}=rx_{ht}$ (r is the same for all agents). For instance, in the consumption–income case, assume all incomes stem from two sources (say two firms), so that either $x_{it}=z_{1t}$ or $x_{it}=x_{2t}$, where z_{1t} and z_{2t} are, by way of example, orthogonal stochastic processes.

Then if the consumers belonging to behavioral groups 1 and 2 are equally distributed within each of the two firms, we shall get:

$$X_{1t}=\frac{N}{2}z_{1t}+\frac{N}{2}z_{2t}=X_{2t},$$

where N is the number of agents.

More generally, in order to have $X_{1t}\neq X_{2t}$ (up to a scalar), it is necessary that the x_{it}, as stochastic processes, be not distributed among individuals quite independently of the $\pi_i(L)$.

With the prototypes contained in the next section we shall try to show how differences in the micro background can make plausible such a non-independence assumption, which is implicit in condition (d) above. Here we limit ourselves to the following two observations.

If the x_{it} processes are mutually independent then so are X_{1t} and X_{2t}. Moreover in representation (12) we have $\text{cov}(v_{1t},v_{2t})=0$. This is the opposite case with respect to the one analyzed in 1.1. In general there will be both common causes affecting all x_{it} variables, even though with differences in intensity and phase, and local or sectoral or individual causes of variation for the x_{it}. Secondly, we observe that the presence of common causes is not inconsistent with a representation (12) for which (d) is fully valid. Consider the following example:

$$X_{1t}=U_t$$
$$X_{2t}=LU_t+v'_t$$

where $U_t=(1-\beta L)v_t$ is the common cause, v_t and v'_t are orthogonal white-noise processes. The asymmetry, that is the absence of a stochastic term affecting only X_{1t}, is for reasons of mathematical convenience. Actually, the W.R. is immediately obtained:

$$\begin{pmatrix}X_{1t}\\X_{2t}\end{pmatrix}=\begin{pmatrix}1-\beta L & 0\\ L(1-\beta L) & 1\end{pmatrix}\begin{pmatrix}v_{1t}\\v_{2t}\end{pmatrix}.$$

Here we see the way a simple phase difference in the action of the common cause can produce a sizeable effect (in as much as a sizeable difference between $\pi_1(L)$ and $\pi_2(L)$ is also present).

More generally, if there are several common causes affecting with different lags and intensities all of the x_{it}, and causes affecting only x_{it} variables corresponding to individual or groups of agents belonging to particular geographical or economic areas, we may write:

$$X_{1t}=\gamma_1(L)U_{1t}+\gamma_2(L)U_{2t}+\cdots+\gamma_n(L)U_{nt}+V'_t$$
$$X_{2t}=\gamma'_1(L)U_{1t}+\gamma'_2(L)U_{2t}+\cdots+\gamma'_n(L)U_{nt}+V''_t,$$

where the U_{it} are the common causes while V'_t and V''_t gather the local, sectoral and individual causes of variation. The existence of differences between the $\gamma_i(L)$ and the $\gamma_i'(L)$, and between V'_t and V''_t entails, in general, a representation (12) fulfilling condition (d).[18]

Lastly, the aggregation effects vanish if the microparameters assume special values. For instance, if $a_{11}(L) = a_{22}(L)$, $a_{12}(L) = a_{21}(L)$ then no feedback arises from aggregation (note that in this case aggregation destroys even an existing microfeedback). The prototypes in the next section will show that such equalities (as the one ruling out the other aggregation effects) are quite unlikely when differences in the micro background are adequately taken into consideration.

3. Prototypes

The following simple models have an illustrative purpose both of the assumptions we adopted and of the results obtained. In order to avoid analytical complications we limit ourselves to equations with only one independent variable. Also for this reason, the models below must be considered primarily as prototypes and have no claim to realism. The reader will notice that while in the first model the dynamics comes, so to speak, from outside, the second model is 'closed' and contains a microfeedback.

3.1 A price–cost equation

Consider an economy in which two consumer goods are produced by means of two imported commodities. Let s_{ij} be the quantity of imported commodity j necessary to produce one unit of consumer good i. Assume further that an index number of consumer prices is available in which the weights are both equal to 1; and that an index number for import prices is also available in which the weights are $s_{11} + s_{21}$, $s_{12} + s_{22}$. With no loss of generality we can assume that $s_{11} + s_{21} = s_{12} + s_{22} = 1$. Thus the index numbers are $p_t = p_{1t} + p_{2t}$, p_{it} being the price of consumer good i, and $c_t = p^u_{1t} + p^u_{2t}$, where p^u_{it} is the price of imported commodity i. Note that c_t is the import price index and the aggregate cost index as well.

Assume now that for price p_{it} we have:

$$p_{it} = \Pi_i c_{it}, \quad c_{it} = s_{i1} p^u_{1t} + s_{i2} p^u_{2t},$$

that is the price p_{it} is obtained by applying the constant mark-up $m_i = \Pi_i - 1$ over the unit cost. For the prices of the imported commodities we assume two orthogonal AR(1) processes:

$$(1 - \alpha_i L) p^u_{it} = \xi_{it},$$

where $0 \leq \alpha_i < 1$, ξ_{1t} and ξ_{2t} are orthogonal white-noise processes.

Moreover, we assume that the two techniques (s_{1i}) and (s_{2i}), being independent, are non-proportional: $s_{11}s_{22} - s_{12}s_{21} \neq 0$; that $\Pi_1 \neq \Pi_2$ and, finally, that $\alpha_1 \neq \alpha_2$. It is easy to obtain the equation linking aggregate price to aggregate cost. First, for the individual costs:

$$\begin{pmatrix} c_{1t} \\ c_{2t} \end{pmatrix} = \begin{pmatrix} \dfrac{s_{11}}{1 - \alpha_1 L} & \dfrac{s_{12}}{1 - \alpha_2 L} \\ \dfrac{s_{21}}{1 - \alpha_1 L} & \dfrac{s_{22}}{1 - \alpha_2 L} \end{pmatrix} \begin{pmatrix} \xi_{1t} \\ \xi_{2t} \end{pmatrix}.$$

The W.R. is:

$$\begin{pmatrix} c_{1t} \\ c_{2t} \end{pmatrix} = \frac{1}{(1 - \alpha_1 L)(1 - \alpha_2 L)} \begin{pmatrix} 1 - ML & QL \\ RL & 1 - N \end{pmatrix} \begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix},$$

$$\begin{pmatrix} v_{1t} \\ v_{2t} \end{pmatrix} = \begin{pmatrix} s_{11} & s_{12} \\ s_{21} & s_{22} \end{pmatrix} \begin{pmatrix} \xi_{1t} \\ \xi_{2t} \end{pmatrix},$$

where, taking $S = \det(s_{ij})$:

$$M = \frac{s_{11}s_{22}\alpha_2 - s_{12}s_{21}\alpha_1}{S}, \quad N = \frac{s_{11}s_{22}\alpha_1 - s_{12}s_{21}\alpha_2}{S},$$

$$Q = \frac{-s_{11}s_{12}(\alpha_2 - \alpha_1)}{S}, \quad R = \frac{s_{21}s_{22}(\alpha_2 - \alpha_1)}{S}.$$

Using (15) the aggregate equation is easily seen to be:

$$(1 - FL) p_t = K(1 - GL) c_t + u_t,$$

where:

$$F = \frac{\Gamma_1(N - Q) + \Gamma_2(M - R)}{\Gamma},$$

$$G = \frac{\Gamma_1(\Pi_2 N - \Pi_1 Q) + \Gamma_2(\Pi_1 M - \Pi_2 R)}{K\Gamma},$$

$$K = \frac{\operatorname{cov}(v_{1t}, v_{2t})}{\operatorname{var}(v_{2t})},$$

$$u_t = v_{1t} - K v_{2t}.$$

Thus a purely static behavior has been transformed by aggregation into a dynamic macroequation. To the signs of F and G and their relative magnitude, and hence to the smoothness of the distributed-lag function implicit in the aggregate equation we shall return in the next section. Here it must be pointed out that the knowledge of the way firms operate does not help dynamically to specify the macroequation, unless the aggregation problem is explicitly taken into consideration. On the other hand, an investigator sticking to the commonly accepted representative agent practice, who were able to correctly specify and estimate the aggregate equation linking p_t and c_t, would seek an explanation for a non-existing dynamic behavior.

Using (15) the reader will easily analyze the consequences of different lags in the response to cost variations due to different periods of production.[19]

The following further example may deserve some interest. Consider a single industry producing one homogeneous good. Suppose there exists a firm acting as a leader, to whom the pricing function is left. The price is fixed according to:

$$p_t = \Pi(s_1 p_{1t} + s_2 p_{2t}),$$

where the cost on the right hand side is, of course, relative to the leader firm, and for p_{1t} and p_{2t} we have:

$$(1-\alpha_i L)p_{it} = \xi_{it},$$

$\alpha_1 \neq \alpha_2$, ξ_{1t} and ξ_{2t} being orthogonal white-noise processes. Now assume that, in addition to p_t, an index of the industry unit cost is available based on the industry averages:

$$c_t = s_1' p_{1t} + s_2' p_{2t}.$$

The W.R. of $(p_t\ c_t)$ is easily obtained from:

$$\begin{pmatrix} p_t \\ c_t \end{pmatrix} = \begin{pmatrix} \dfrac{s_1}{1-\alpha_1 L} & \dfrac{s_2}{1-\alpha_2 L} \\ \dfrac{s_1'}{1-\alpha_1 L} & \dfrac{s_2'}{1-\alpha_2 L} \end{pmatrix} \begin{pmatrix} \xi_{1t} \\ \xi_{2t} \end{pmatrix},$$

and the same happens for the empirical equation linking p_t and c_t. A dynamics generated by aggregation is present if $s_1 s_2' \neq s_2 s_1'$.

3.2 *A multiplier-accelerator model*

In the previous example the dynamics of the microvariables is, so to speak, exogenous to the system. We now insert an aggregation problem into a simple version of the multiplier-accelerator model: the dynamics here propagates from the investment decisions to the consumption–income equation. The model is:

$$C_t = C + h_W W_t + h_P P_t + \varepsilon_t \tag{a}$$

$$I_t = I + a(1-L)C_t + v_{1t} \tag{b}$$

$$M_t = M \tag{c}$$

$$X_t = M + v_{2t} \tag{d}$$

$$W_t = K_C C_t + K_I I_t + K_X X_t \tag{e}$$

$$P_t = Y_t - W_t \tag{f}$$

$$Y_t = C_t + I_t + X_t - M_t, \tag{g}$$

where $a > 0$; ε_t, v_{1t} and v_{2t} are mutually orthogonal white-noise processes. Equation (g) defines the aggregate income as the sum of consumption, C_t, investment, I_t, net exports, $X_t - M_t$. Equation (f) defines profits P_t. Imports are a constant (equation (c)); exports are given by the same constant plus a white noise: the average of the net trade balance vanishes. Investment is determined by an accelerator on consumption (equation (b)); in equation (a) aggregate consumption is obtained as the sum of wage-earners and of profit-earners behaviors. In equation (e) aggregate wage-earnings equal the sum of sectoral wage-earnings: K_C is the labor–product ratio for the consumption goods sector, times the wage per worker; analogously for K_I and K_X. Since we are interested in deviations from the mean, we assume $M = I = C = 0$. It must be pointed out that we are assuming homogeneity for each group of consumers (wage-earners and profit-earners) and for each group of firms (consumption goods, investment goods, exports). Finally, we suppose that the propensity to consume on wages is very high, and very low for profit-earners: for simplicity, $h_W = 1$, $h_P = 0$.

Let us now assume that: (1) only the variables C_t, Y_t, X_t, M_t, I_t are available; (2) an investigator is reasoning on the basis of a theory leading to a dynamic relationship linking agents' consumption to their income; (3) such a relationship is transferred as usual to aggregate data. Thus the problem consists in the specification and estimation of an equation whose general form is:

$$a(L)C_t = b(L)Y_t + c(L)u_t.$$

We assume further that u_t is given a behavioral meaning, that is u_t is interpreted as the aggregate of all those fractions of individual consumptions that are not explained by current and past income nor by past consumption. Moreover we assume that the investigator is aware of equation (g), so that he will not consider the equation $\operatorname{cov}(u_t, Y_t) = 0$ as a correct way of closing the model. From his point of view, a solution can be found by considering the variable X_t. The latter, although correlated with C_t *via* Y_t, is not correlated with W_t (because of the interpretation given to u_t by the investigator). Therefore X_t can be considered by the investigator as a valid instrumental variable.

The model can now be solved. First we obtain by substitution the following representation for $(C_t\ Y_t)$:

$$\begin{pmatrix} (1-K_C-aK_I)+aK_I L & 0 \\ -(1+a)+aL & 1 \end{pmatrix} \begin{pmatrix} C_t \\ Y_t \end{pmatrix} = \begin{pmatrix} 1 & K_I & K_X \\ 0 & 1 & 1 \end{pmatrix} \begin{pmatrix} \varepsilon_t \\ v_{1t} \\ v_{2t} \end{pmatrix}.$$

The stationarity condition is:

$$aK_I < |1 - K_C - aK_I|.$$

The generic aggregate equation is:

$$(1-\beta L)\,C_t = HY_t + u_t,$$

where

$$\beta = \frac{a(H(1+K_I-K_C)-K_I)}{1-K_C-aK_I},$$

$$u_t = g\{(\varepsilon_t + K_I v_{1t} + K_X v_{2t}) - H[(1+a)\,u_t + (1+K_I-K_C)\,v_{1t} + (1+a(K_X-K_I)+K_X-K_C)\,v_{2t}]\},$$

$$g = (1-K_C-aK_I)^{-1},$$

as is easily seen by starting from the above representation of $(C_t\ Y_t)$. Finally, to determine H we multiply the above generic aggregate equation by X_t and take expected values assuming $\mathrm{cov}(u_t, X_t) = 0$. We get:

$$H = \frac{K_X}{1+a(K_X-K_I)+K_X-K_C}.$$

Notice that $\beta \neq 0$ provided $K_X \neq K_I$. Therefore, also in this case, different techniques and microbehaviors yield a dynamic equation in spite of static microbehaviors. The only dynamic behavior in the model regards the investment decisions and is a simple finite distributed lag, whereas the macroequation has an infinite distributed-lag structure.[20]

4. Extension of some results to the general model

4.1 *The macroparameters as analytic functions of the microparameters*

We pointed out in 2.1 that in the general case, that is when we drop the assumptions of only two agents and of exact microequations, the dependence of the macroparameters on the microcoefficients is not easy to be analyzed. Yet elsewhere we have been able to prove that the former are analytic functions of the latter, i.e. the parameters of the aggregate equations are analytic functions of the coefficients of the system (7) (including the coefficients of the variance–covariance matrix of ξ_t).

This entails that if the vector of the microcoefficients of (7) is contained in an open connected set $S \subset R^M$, where M is the number of microparameters, then given an algebraic relationship E among macroparameters, the following alternative holds: (1) E is true for any $s \in S$, or else: (2) the subset of S where E is true is nowhere dense in S (that is non-dense in any open set contained in S), thus negligible.[21]

For instance if $A(s)$ and $B(s)$ are macroparameters and there exists a $\bar{s} \in S$ such that $A(\bar{s}) \neq B(\bar{s})$, then the subset of S where $A(s) = B(s)$ is negligible.

A simple model will clarify the possible use of the above statement. Consider a three-agent model whose microequations are:

$$y_{it} = \Pi_i x_{it} + u_{it},$$

while for the x_{it} we have:

$$(1-\alpha_i L)\,x_{it} = v_{it},$$

where the u_{it} and the v_{it} are mutually orthogonal white-noise processes. The model depends on 12 microparameters. If no further restriction on them is imposed S is the subset of R^{12} defined by: $|\alpha_i| < 1$, $\sigma^2_{u_i} \geq 0$, $\sigma^2_{v_i} \geq 0$, $i = 1, 2, 3$. Now let $\bar{s}$ be any point defined by $\Pi_1 = \Pi_2$, $\alpha_1 = \alpha_2$, $\Pi_2 \neq \Pi_3$, $\alpha_2 \neq \alpha_3$, $\sigma^2_{u_i} = 0$, $i = 1, 2, 3$. Since we have again a two-agent case with exact microequations, the results of Section 2 apply. Thus, in $\bar{s}$ we have $a(L) \neq c(L)$, $a(L)$ is not a factor of $b(L)$, there is a feedback from y_t to x_t. Now, thanks to the analyticity of the coefficients of $a(L)$, $b(L)$, $c(L)$, these results can be extended to the whole S with the possible exception of a negligible subset (obviously containing all points for which $\Pi_1 = \Pi_2 = \Pi_3$).

It must be pointed out first that such an outcome corresponds to what could be expected on the basis of intuition: that is, the aggregation effects should not vanish when the microbackground becomes more complicated. Second, if in the above example there are reasons to assume that $\alpha_1 = \alpha_2 = \alpha_3$, then S is no longer the adequate set for s, nor is $\bar{s}$ an acceptable point. Some of the results on the polynomials $a(L)$, $b(L)$, $c(L)$ do not hold in this restricted set: in particular, $a(L)$ will be a factor of $b(L)$ and there is no macrofeedback.

Lastly we note that some obvious statements about the two-agent model can be easily transferred to the general model. Thus, while in the former the aggregation effects will be, so to speak, proportional to the difference between $\pi_1(L)$ and $\pi_2(L)$ and between X_{1t} and X_{2t}, in the latter the size of the aggregation effects will depend on the dispersion of the $\pi_i(L)$ and on the dependence of the x_{it} microparameters on the $\pi_i(L)$.

4.2 *The shape of the aggregate equation*

One may wonder whether the macromodels generated by aggregation of different agents do possess some typical characteristics of the estimated macroequations. In particular, in a macromodel like (1) (see the Introduction) the coefficient α is usually between zero and one, and is therefore interpreted as deriving from the presence of some adjustment cost. In a model as simple as the two-agent one we set up in the Introduction and worked out in 2.2, if $0 < \alpha_i < 1$ then α is also positive. Yet it must be pointed out that the corresponding lag distribution is not necessarily smooth. As a matter of fact, we have:

$$p_t = \frac{1-\beta L}{1-\alpha L}\,c_t + \cdots,$$

where:

$$\frac{1-\beta L}{1-\alpha L}=1+(\alpha-\beta)L+\alpha(\alpha-\beta)L^2+\alpha_2(\alpha-\beta)L^3+\cdots,$$

so that the result depends on $\alpha-\beta$. If the latter is negative the adjustment of p_t consists in an overadjustment in the first period followed by a monotonic opposite correction. For the actual values of α and β and therefore the possibility of a negative $\alpha-\beta$ see 2.1.[22]

But as soon as we consider a slightly more complicated model, like the example in 3.1, we see that not even the result on the sign of α is guaranteed. The aggregate equation was:

$$(1-FL)p_t=K(1-GL)c_t+u_t.$$

While the coefficient K is definitely positive, since v_{1t} and v_{2t} are positively correlated, the coefficients F and G can have both signs. For instance, if $S>0$, $\alpha_2=0$, then Q and N are positive while M and R are negative. Since:

$$N-Q=s_{11}(s_{22}-s_{12})\alpha_1, \quad M-R=s_{21}(s_{22}-s_{12})\alpha_1,$$

then F is negative if $s_{22}-s_{12}$ is negative. Thus we have the possibility of an aggregate equation with no usual interpretation, arising from simple microbehaviors.

The foregoing result cannot be ascribed to an odd behavior of the independent variable, which is smooth, nor to an odd behavior of the two microvariables (costs): in fact, each one of them is smooth and reacts positively and smoothly to variations of the other (the autocovariance matrix of the vector $(c_{1t}\ c_{2t})$ is strictly positive and almost geometrically declining). Notice, also, that the same is true for the vector $(p_t\ c_t)$.

The problem of an unusual shape for the aggregate model is still present for a model in which the independent variables are generated by the same vector process as above, but the microequations are:

$$(1-\theta L)p_{it}=\Pi_i c^{\mu}_{it}$$

with $0<\theta_1<1$. In the simple case in which $\theta_i=\theta$, $i=1, 2$, on the left-hand side of the aggregate equation we have:

$$(1-\theta L)(1-FL)$$

and therefore possibly a root having the wrong sign.

Moreover, if the microequations are less simple we can have a nontrivial $c(L)$ (the polynomial operating on u_t). For instance, if

$$\pi_1(L)=\frac{0.7+0.1L}{1-\theta L}.$$

$$\pi_2(L)=\frac{0.3+0.3L}{1-\theta L}$$

the p^{μ}_{it} being still generated as in 3.1, we shall have:

$$c(L)=1-0.5L, \quad a(L)=(1-\theta L)(1-FL).$$

Now, it must be pointed out that equations estimated on the basis of finite realizations of the y_t and x_t processes can be no more than approximations of the underlying relationships. Moreover, we should remember that the specification and estimation process usually starts with an equation of the form:

$$\gamma(L)p_t=\delta(L)c_t+\tilde{u}_t,$$

where $\gamma(L)$ and $\delta(L)$ are polynomials of a sufficiently high degree to ensure that the hypothesis of a (approximately) white-noise $\tilde{u}_t$ is not rejected. Thus, if the underlying model is;

$$a(L)p_t=b(L)c_t+c(L)u_t,$$

then the estimated $\gamma(L)$ and $\delta(L)$ represent approximations to the expansions, respectively, of $a(L)/c(L)$ and $b(L)/c(L)$. In our case, the polynomial $\gamma(L)$ will be a truncation of:

$$(1-FL)(1-\theta L)(1+0.5L+0.25L^2+\cdots),$$

If a first-order truncation is chosen, the coefficient of L, namely $-(F+\theta-0.5)$, is not unlikely to be negative.

In conclusion, aggregation yields dynamically complex models but their shape is not easily predictable on the basis of the microequations.

4.3 Aggregation of time series relationships in econometric literature

In the econometric literature of time series the possibility of dynamic complications due to aggregation is only seldom mentioned, and in the few cases known to the present author the model implicitly considered seems to be the simplest one, namely: $x_{it}=r_i x_t$. See, for instance, Nickell (1985, p. 128), Layard and Nickell (1985, p. 66), Sargent (1978b, p. 1016), Hendry *et al.* (1984, pp. 1938–9). By contrast, the possibility of a feedback arising from aggregation—so that implicitly a more complex model is assumed—is noted in Sims (1972, p. 34).

Also the model analyzed by Trivedi (1985)—in which the aggregation problem is explicitly considered—is based on $x_{it}=x_t$ too. Thus knowledge of the model generating x_{it} is superfluous and aggregation is immediate (the difficulties in Trivedi's work lie elsewhere).

Dynamic complications arising with aggregation of microrelationships are analyzed in Granger (1980), who starts with a micromodel already containing the lagged dependent microvariable:

$$y_{it}-\alpha_i y_{it-1}=x_{it}+\beta_i W_t+e_{it}.$$

Granger is not interested in the explicit form of the macroequation linking y_t, x_t and W_t, but in the spectral density of the variable y_t corresponding to distributions of the parameter α_i, where $|\alpha_i|<1$ but there is no α, $0<\alpha<1$, such that $|\alpha_i|<\alpha$ for any i (the number of agents is infinite in Granger's model).

Recently in Stoker (1986), the possibility of a dynamic aggregate equation due to different but static microbehaviors jointly with differences in the independent microvariables is explicitly stated. Yet Stoker limits himself to the dynamic form:

$$y_t = ax_t + \frac{u_t}{1-\alpha L},$$

while, as we have seen, aggregation yields general dynamic macromodels.

An analogy between aggregation and rational expectations models has been pointed out in the present work: in both cases the parameters belonging to the independent processes contribute to the determination of the parameters of the equation linking observables. In the case of rational expectations a noticeable effort has been made to implement tests on joint hypotheses (rational expectations plus maximization of intertemporal functions) by making the above dependence explicit. Yet such attempts are confined to the representative agent. The existence of an aggregation problem is recognized, for instance, in a paper by Sargent devoted to testing for rational expectations in a demand for labor equation (see Sargent, 1978b, p. 1016, footnote). Sargent appears to believe that the problem could easily be dealt with. Actually, in his case, if one accepts that all firms face the same variable for the wage-rate, which is the only independent variable in Sargent's model, a test taking explicitly into account aggregation could be constructed along the lines of 1.1.[23]

However, when the assumption of identical independent variables across different agents is no longer acceptable even as a working hypothesis, the problem is much more difficult to deal with. If we consider, by way of example, the case in which the variables are consumption on the left- and income on the right-hand side, the construction of a test for, say, permanent income under rational expectations is incomparably more complicated.[24] In fact, the parameters of the aggregate equation depend on the parameters of the disaggregate model of incomes and, thus, the knowledge of the aggregate income parameters is no longer sufficient: a test for rational expectations could not even be set out without some idea of a disaggregate model for individual incomes.

The whole issue of testing theories is beyond the scope of the present work. We only observe that aggregation will obviously make it difficult to implement any test also for theories grounded on simple routinized, rather than maximizing, behaviors.

Concluding remarks

We have shown that serious consideration of the aggregative nature of the macrovariables can cast grave doubts on current interpretations of the dynamic form of estimated macroequations and of their parameters. As we have seen, the dynamics of the independent microvariables propagates to the macroequations, the latter turning out with a general dynamic shape, even though the microbehaviors are perfectly static or quasi-static. Moreover, aggregation is likely to introduce feedback relationships, whereas no feedback is present at the micro level. Finally, the macroparameters are not invariant with respect to change in the parameters belonging to the stochastic microprocesses generating the independent variables the agents are facing.

Yet it must be pointed out that the macroequations obtained by aggregation do not necessarily possess some of the most typical characteristics of estimated macroequations: as we saw in 4.2, if the aggregate macroequation has the following form:

$$(1-\alpha L)y_t = K(1-\beta L)x_t + u_t,$$

it is not unlikely that α is negative, contrary to what normally happens. We also saw that such an odd shape for the macroequation is not the outcome of unlikely cases for the microprocesses generating the independent microvariables. Nor does the outcome necessarily change if the microequations have autoregressive polynomials such as $(1-\alpha_i L)$ with $0<\alpha_i<1$.

Further research on this problem could be carried out along the lines followed above. However, we conjecture that not much progress could be made within the narrow limits of the Slutsky–Frisch approach we adopted to model the dynamics of individual behaviors, not any development within those limits would be very interesting in our opinion. On the contrary, as we argued in the Introduction, in order to come to grips both with micro–macro problems and microbehaviors, the issue of individual microvariables and individual behaviors should be re-examined by dropping the dynamic regularity assumption, as well as the constancy of individual microbehaviors, and trying to model non-regular dynamic environments on the ground of specific information corresponding to economic problems. Thus we get back to one of the main issues raised by this book, namely the need of microfoundations of macroeconomic regularities much more embedded in the analysis of the institutional context in which the agents operate and of the actual decision-rules that they follow. We hope that the present work can give a contribution to the statement of the problem, just by showing how an important difference in dynamic shape between micro and macro can arise even under a very slight departure from the common practice of macroeconomics, in which the problem of the micro–macro correspondence is ignored altogether.

Notes

1. Nelson and Winter (1982), p. 134. See also, with reference to the pricing problem, Coutts *et al.* (1978): 'A useful analogy to the behavioral problems of

a firm would be to say that firms have (metaphorically or literally) developed computer programs over the years in order to cope with the stresses and challenges of the environment in which they live. The typical computer routine for pricing is very simple and not responsive in an optimizing way to fairly frequent environmental shocks (p. 98).'

2. More precisely, we shall assume that the x_t's are stationary stochastic variables, either after the subtraction of a regular trend or up to the application of a suitable difference operator.
3. To get an idea of the extent to which the problem of aggregate dynamic is identified with the problem of the dynamic behavior of a single agent, see among others, Nerlove (1972), Sims (1974) and, for a recent review, Hendry *et al.* (1984), pp. 1937–40.
4. That is, v_{1t-k} is orthogonal to v_{2t-h} for any k and h. We recall that orthogonality for two stochastic variables means that their covariance vanishes.
5. The proofs of some mathematical results we shall refer to are contained in the unpublished paper by Lippi (1986), available on request from the author.
6. 'Empirical lag distribution' is used in Nerlove (1979), p. 167, for the lag distribution corresponding to the aggregation of unobserved components (see Section 6 below).
7. See Lippi (1986), Lemma 1.
8. The unknowns are α and σ_u^2, the variance of u_t. As for the equations we have only to write down the variance of $(1-\alpha L)u_t$ and the covariance of it with $(1-\alpha)u_{t-1}$, thus getting two non-linear equations. It is possible to show that there exists a solution with $|\alpha| \leq 1$, $\sigma_u^2 \geq 0$. This is a particular case of a general result, enabling to deal with any number of agents and any degree for the polynomials $a_i(L)$, $b_i(L)$. We shall return to this question in 1.3.
9. Since u_{it} is orthogonal to x_{jt-k}, any i, j, k, and as u_t is a linear combination of u_{it}, $k \geq 0$, then u_t is orthogonal to x_{t-k}, any k. The orthogonality to y_{t-h}, $h>0$, is easily found by considering the expression of y_{t-h} resulting from (4).
10. The other solutions of (7) contain terms like $\alpha\beta^{-t}$, where β is a root of the determinant of $A(L)$. The above assumption on the modulus of the roots of $\det(A(L))$ can be relaxed to include roots of modulus equal to one. These latter have been excluded for simplicity of exposition.
11. Let us stress the distinction, already made in the Introduction, between easily treatable non-stationarity, as represented for instance by the presence of unit modulus roots in $\det(A(L))$, and more radical forms of non-stationarity.
12. The rationality of $M(L)$ is a consequence of z_t being generated by system (7), thus having a rational spectral density. See Rozanov (1967), Chapters 1 and 2, and Hannan (1970), pp. 61–6. This is the general result we refer to in Note 8.
13. Actually, from the rationality of the spectral density of z_t the rationality of the spectral density of $(y_t\ x_t)$ follows easily.
14. If there were a microfeedback from the y_{it} to the x_{it} then the parameters of the W.R. of $(X_{1t}\ X_{2t})$ would not be invariant for variations of the parameters of equations (2). When speaking of a microfeedback we include both the case in which for each agent i the variable y_{it-k} enters the determination of x_{it} for some $k>0$, and the case in which y_{t-k}, or subaggregates of it, determines all the x_{it}.
15. We are still assuming $\Pi_1 \neq \Pi_2$ and therefore $\pi_1(L) \neq \pi_2(L)$. If $\pi_1(L) = \pi_2(L)$, the representation (14) degenerates. Notice also that the feedback disappears

if $a_{11}(L) = a_{22}(L)$, $a_{12}(L) = a_{21}(L)$. Such a possibility, to which we shall return in 2.3, should not be confused with the case in which $X_{1t} = X_{2t}$ (up to a scalar) as stochastic processes. In the latter case (13) would degenerate, that is the variance–covariance matrix of v_t would be singular.

16. If $\pi_1(L) = \pi_2(L)$ our model degenerates and equations (15) are no longer valid. Yet by letting $\pi_1(L)$ and $\pi_2(L)$ tend to the common limit $\pi(L)$, while assuming that the limit of
$$\frac{\pi_1(L) - \pi_2(L)}{\Pi_1 - \Pi_2}$$
is finite, $b(L)/a(L)$ tends to $\pi(L)$ while u_t tends to zero.
17. The period of production is here defined, according to Coutts *et al.* (1978), as 'the length of time between the first purchase of the input used for the production process and the sale of the finished product' (p. 37).
18. There is an intermediate case between (d) and the case analyzed in 1.1. It is possible that $X_{1t} = X_{2t}$ (up to a scalar) is false but the variance–covariance matrix of v_t in (12) is singular: it is sufficient to assume, for instance, that $X_{1t} = v_{1t}$, $X_{2t} = v_{1t} + av_{2t}$. Such cases do not appear very important. However aggregation is nearly as easy as in 1.1.
19. See Note 17.
20. Alternatively, if the equation linking C_t to Y_t were interpreted as deriving from a relation between C_t and expected values of Y_t, u_t could be interpreted as resulting from the difference between expected and actual values. See, for instance, Hendry and Ungern-Sternberg (1981). Lastly, no difficulty arises, as the reader can verify, if the model is closed by $\mathrm{cov}(Y_t\ u_t) = 0$.
21. See Lippi (1986). Actually, it is not necessary that S is open. The assumption that S is contained in the closure of an open connected set is sufficient.
22. The possibility of non-smooth lag distributions as an outcome of rational expectations is noted in Sims (1974), p. 314. The lag distribution Sims refers to is the orthogonal projection of p_t over the present past and future of c_t; thus it differs from ours, which implies only the past (on this point see 4.3 below). However, our result could easily be obtained for the bilateral projection.
23. It must be pointed out, however, that some assumption on the distribution of the vector of microparameters among firms is necessary in order to deal with the finiteness of the sample size. Either a small number (as compared with the sample size) of different firms or a continuous distribution depending on a small number of parameters must be assumed.
24. However, for a test of permanent income under rational expectations based on the representative agent, see Sargent (1978a). In this case the aggregation problem is not even mentioned.

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9 Evolutionary theories in economic thought

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Introduction

The stand one takes on epistemological issues—i.e. on what we expect theories to tell us and therefore on how we judge their respective merits—is never an easy one to explain. Nor is it something that academic researchers, especially professional economists, have ever been encouraged to explore in detail, possibly because such an exploration is almost certain to threaten all sorts of established positions—ideological, cognitive, economic, bureaucratic—which have not been reached without some considerable expenditure of effort (and other resources) and which have often become enshrined in established institutions (like university departments) that have seldom been noted for their propensities towards rapid cognitive change. There are, of course, enormous advantages inherent in such intellectual inertia since it is doubtful if disciplined scholarship could take place at all under more anarchic conditions.

And yet every so often such conditions do not only arise, they are clearly necessary as an essential feature of the advancement of knowledge. We do not understand precisely how this process functions. It is certainly not only through some explicit confrontation between 'fact' and 'theory' (or through the sheer weight of Kuhnian 'anomalies'), but rather also as a result of some more subtle set of mechanisms which impress upon intellectual communities that something is wrong, that existing theories just do not work and that attempts to make them work, to re-establish the viability of existing theory, increasingly leads to intellectual clumsiness and 'baroque' explanations.

However, we do know that an important factor influencing adherence to particular theoretical positions lies in the character of underlying metaphysical positions, positions which are often not fully appreciated by practising scientists themselves. In his Lowell Lectures, brought out as *Science and the Modern World* in 1926,[1] A. N. Whitehead made this point in terms of the following broad propositions:

1. Intellectual thought in any field is always conducted through a process of abstraction whereby 'reality' is expressed in terms of specific entities and their relationships to each other.
2. This process of abstraction both excludes what is felt to be unimportant for the analysis and gives the entities specific characteristics.

3. Provided these abstractions are used carefully—i.e. constantly confronted by experience—then they have great scientific usefulness. Where, however, they get taken for reality itself they will have a profoundly deadening effect on scientific development.
4. It is the job of philosophy (and philosophers) to make sure that this does not happen by constantly reviewing the nature of the abstractions made within any given context both conceptually and empirically.

However, it is all too easy to get caught in the grip of one's abstractions. According to Whitehead,

> We all know those clear-cut trenchant intellects, immovably encased in a hard shell of abstractions. They hold you to their abstractions by the sheer grip of personality. [Nevertheless] . . . the disadvantage of exclusive attention to a group of abstractions, however well-founded, is that, by the nature of the case, you have abstracted from the remainder of things. Insofar as the excluded things are important in your experience, your modes of thought are not fitted to deal with them. You cannot think without abstractions; accordingly, it is of the utmost importance to be vigilant in critically revising your modes of abstraction. It is here that philosophy finds its niche as essential to the healthy progress of society. It is the critic of abstractions. A civilisation which cannot burst through its current abstractions is doomed to sterility after a very limited period of progress.[2]

What follows is an exploration of the history of economic ideas with a view to understanding better the underlying 'influential metaphysics' and how dominant ways of thinking emerge and evolve. These are major ideas and 'visions' which organize the development of scientific models, the nature of the theoretical 'abstractions' made, the choice of variables, the 'facts' that are held to require exploration, and the heuristics guiding 'scientific progress'. One can discern many examples of such 'visions' which have influenced the ways economists have tried to model the temporal growth of economic systems and whose characteristics may be seen in dialectical terms.

For example, a 'short-term' perspective stresses the properties of the economic system *given* an unchanged parametric structure (in the neoclassical formulation, the 'givens' are the so-called 'fundamentals'—technologies and tastes). To a considerable extent the short-term perspective coincides with 'static' analysis. Conversely, the 'long-term' perspective centres on dynamics and changing structures (including, of course, technologies, institutions, beliefs and behaviours). A second example, which is discussed at length in various chapters of this volume, lies in the dichotomy between an 'equilibrium' and an 'evolutionary' perspective. Such a dichotomy is partly related to the relative emphasis on the *result* of a process compared to the process itself. However, it is in fact more than that, and in the history of economics the former vision has often implied an almost exclusive attention to the existence and properties of equilibria and equilibrium paths neglecting completely the question of how precisely the system gets there. On the other hand, the 'evolutionary' perspective places attention on a careful definition of the relevant processes (selection, innovation, etc.).

A third dichotomy on which this chapter is mainly focused is the tension between 'mechanistic' and 'organic' visions of the behaviour of economic systems. Essentially, the 'mechanistic' view entails the belief that the 'whole' can be simply described in terms of properly quantifiable and stable 'equilibrium' relationships amongst its constituent 'parts' (the 'watch' metaphor, quoted also in Allen's chapter, is a useful example). On the contrary, the 'organic' vision argues that the 'whole' cannot be reduced in this way, but rather the properties of complex systems evolve from non-linear interactions among its components and 'disequlibrium micro-states'. It is also a vision consistent with Prigogine's theory of dissipative structures in chemical systems, whereby crucial systemic properties emerge as a result of micro fluctuations and coupled dynamics between (disequilibrium) processes at the level of individual parts.

It is our intention to explore aspects of the evolutionary tradition in economics with a view to understanding why it is that the discipline has not become an evolutionary and 'organic' one but has instead become increasingly mechanistic throughout the twentieth century. Beginning, briefly, with the early classical economists it is possible to trace a strong (albeit sometimes implicit) 'organic' tradition in the history of economic thought, particularly with Marx and institutional thinkers such as Veblen. However, from Marshall onwards the prevailing approach has become progressively dominated by the notion of equilibrium even where it engages with evolutionary ideas. It is only relatively recently that several economists, including some represented in this volume like Freeman and Perez, Nelson, Dosi and Orsenigo, and Silverberg, building upon the work of Schumpeter, have begun to develop a more systemic view of the nature of technological and economic change.

It is at least arguable (cf. also Allen, chapter 5) that the biases towards quantification, reductionism and equilibrium behaviour associated with early thinkers such as Bacon, Descartes, Galileo and Newton, have had a considerable (if unconscious) influence on the minds of professional economists right from the very beginning. But, of course, while the fountainhead itself, classical physics, began to change course during the nineteenth century towards a more 'organic', 'indeterminate' perspective on natural events, precisely the opposite is the case with professional economics.

Classical antecedents: from Smith to Marshall

Evolutionary views of socio-economic development in general, and technological change in particular, are not a recent academic enterprise. Their antecedents are to be seen in the work of the classical economists. For example. Darwin's work was partly inspired by reading Malthus' essay on human population and according to Schumpeter 'the terms static and dynamic were . . . introduced by John Stuart Mill. Mill probably heard them from Comte, who, in turn, tells us that he borrowed them from the zoologist de Brainville.'[3]

Darwin's *Origin of Species* consolidated a long tradition of evolutionary thought. But the application of Darwin's theory to economic development was impeded by three main factors. First, limited knowledge on evolution and human behaviour opened the way to arguments mainly by analogy; such arguments are often fallacious. Second, social change was not obviously gradualist and therefore the theory was not particularly consistent with the observations of social historians (especially of the Marxists). Third, the rules of the hard sciences (especially Newtonian physics), combined with the Cartesian philosophy of nature as automata and the Baconian appeal to empirical rigour, had become a legitimate view of reality. And economics mostly adopted a mechanistic world-view.

Classical economists did not know as much as we do today about evolutionary concepts.[4] However, they recognized the dichotomy of static and dynamic systems. But this recognition was influenced more by mechanical dynamics than by organic evolution. It is in this context that the dynamics of Mill and Smith can be understood. Much of Smith's use of the terms 'equilibrium', 'laws of motion' and 'scientific objectivity' are drawn from Newtonian physics. Smith, as well as other early economists, is fascinated by the 'order' of the economic system and an obvious analogy is the 'order' of the physical world, generated irrespective of the 'intentions' of the individual units (which, of course, in the physical world are not there at all). The economic sphere could be seen as a microcosm of the celestial arena: forces of supply and demand, guided by the invisible hand, would generate a balance despite, or better *because* of, individual selfishness as market forces 'gravitate' in the right direction. The metaphor of economic order as a 'gravitation process' also underlies Ricardo's theory value. Of course, the physical metaphor with 'forces', 'gravitation', 'natural' (i.e. equilibrium) prices, distributive shares, etc., is not the only possible one. Somewhat earlier than Smith, Mandeville, in his *Fable of the Bees*, describes the economic order with a seemingly biological metaphor, the beehive, whereby coordination is generated by division of labour and specialisation. However, the 'biology' of the metaphor does not go much beyond the nature of the example. What, in fact, Mandeville wants to stress is the mechanics of an ordered interlocking among different economic functions.

Smith was obviously not at home with biological metaphors. He stressed that, unlike animals, human beings had specific attributes which enabled the division of labour to emerge: the ability to truck, barter and exchange. These abilities could be brought into a common stock 'where every man may purchase whatever part of the produce of the other man's talent he has occasion for'.[5] But not for animals:

> The strength of the mastiff is not . . . supported by either the swiftness of the greyhound, or by the sagacity of the spaniel, or by the docility of the shepherd's dog. The effects of those different geniuses and talents . . . cannot be brought into a common stock, and do not . . . contribute to the better accommodation and conveniency of the species.[6]

Starting from selfish goal-motivated individuals raises the question of the social assessment of collective outcomes. The 'forces' of supply and demand may well 'order' the general mechanics of the system (the Invisible Hand properties) but are these outcomes socially desirable? One must recognize that in Smith there is a tension between the institutionalist influence of the early Scottish social philosophers (Ferguson, Stuart, etc.), revealed particularly in his *Theory of Moral Sentiments*, which pushes him to 'investigate' the nature of the social and moral context in which the market system is embedded[7] on the one hand, and the natural optimality of the 'celestial harmony' from the Newtonian metaphor, on the other. Certainly, one does not find in Smith the acritical optimism of contemporary Pareto-optimality theorems, but it is also true that neo-classical economics can reasonably claim to have Smith among its ancestors. Relatedly, the 'individual'—in his abstract autonomy—was, and still is, at the centre of the economic universe.[8]

Darwin's influence on economic thought is particularly interesting in the context of the development of Marx's concept of technological change. When Marx first read Darwin's *Origin* in 1860, he wrote to Engels that 'although it is developed in the crude English style, this is the book which contains the basis in natural history for our view'.[9] But later, Darwin and his followers became victims of Marx's hostility. There are two main reasons for this. First, the Malthusian content of the theory (on populations and within-population selection) was inconsistent with Marx's own ideological position. He saw Malthusianism as an apologia for the establishment and Engels asserted that Darwinism was more scientific without its Malthusian content. Second, Marx and Engels contended that their conception of history as a series of class struggles was much richer in content than the 'weakly distinguished phrases of the struggle for existence'.[10] Marx rejected the application of Darwinian views to socio-economic evolution with their implicit gradualism, preferring a Hegelian approach which allowed also for sudden ruptures and 'quantitative change that becomes qualitative change'. However, he adapted Darwinian concepts to his analysis of technological change.[11]

His view of socio-economic evolution involved transition from one mode of production to another. These transitions resulted from internal antagonisms or conflicts which resolved themselves in a new synthesis where the ultimate transformation (for example, from feudalism to capitalism) was seen as a dialectical leap. This is clearly inconsistent with Darwin's evolutionary gradualism, though not with modern notions of punctuated evolution. Indeed, Marx was committed to the overthrow of the political system and therefore any appeal to gradualism was not welcome. And Darwin was equally uninterested in his revolutionary ideas.[12] But despite this hostility towards Darwinian concepts, Marx consistently used biological or organic metaphors in his analysis of socio-economic transition in general, and technological change in particular. Technology evolves from crude designs to more refined manufacturing systems that benefit from scientific disciplines:

> The power loom was at first made . . . of wood; in its improved modern form it is made of iron . . . It is only after considerable development of the sciences of mechanics, and an accumulation of practical experience that the form of a machine becomes settled entirely in accordance with mechanical principles, and emancipated from the traditional form of the tool from which it emerged.[13]

This evolution occurs in a social and economic environment. Both the technology and the environment influence each other:

> Social relations are closely bound up with productive forces. In acquiring new productive forces men change their mode of production; and in changing their mode of production, in changing the way of earning their living, they change all their social relations. The handmill gives society with the feudal lord; the steam mill, society with the industrial capitalist.[14]

In this process, the role of individuals adds little to the broader pattern of evolution: 'A critical study of technology would show how little any of the inventions of the eighteenth century are the work of a single individual.'[15] Marx equates the development of technology to that of organs in species:

> Darwin has directed attention to the history of natural technology, i.e. the formation of the organs of plants and animals which serve as the instruments of production for sustaining their life. Does not the history of the productive organs of man in society, or organs that are the material basis of every particular organisation, deserve equal attention?[16]

His view of technological change is akin to the co-evolution of species in a given ecosystem and their mutual transformations. The tone is clearly evolutionary.

As simple tools evolve, they are adapted to the requirements of particular applications and used by specific workers.

> In Birmingham alone 500 varieties of hammer are produced, and not only is each one adapted to a particular process, but several varieties often serve exclusively for the different operations in the same process. The manufacturing period simplifies, improves and multiplies the implements of labour by adapting them to the exclusive and specific functions of each kind of worker.[17]

This functional differentiation, according to Marx, creates a combination of simple instruments that forms one of the material conditions for the existence of machinery. Here we see evolutionary theory applied to technical change. But where does technical change come from? Marx himself reveals the source linking technological change with the division of labour. Darwin's law of variation:

> As long as the same part has to perform diversified work, we can perhaps see why it should remain variable, that is, why natural selection should not have preserved or rejected each little deviation of form so carefully as when the part has to serve for some one special purpose. In the same way that a knife which has to cut all sorts of things may be of almost any shape; whilst a tool for some particular purpose must be of some particular shape.[18]

Marx recognized that technical evolution continued long after the machinery had been installed, a fact that underscores the evolutionary nature of technological progress. As noted elsewhere, he paid particular attention to the role of working experience, or the accumulation of disembodied technical change. But he also anticipated modern studies of technical change in the capital goods sector by pointing to plant-level technical improvements: 'When machinery is first introduced . . . new methods of reproducing it more cheaply follow blow by blow, and so do improvements which relate not only to individual parts and details of the machine, but also to its whole construction.'[19] He was able to blend Darwin's notions of random mutation with Hegelian dialectics to provide a methodological analysis of technical change that is unparalleled among classical thinkers. Studies which have ignored this fact have missed the vital interactions and feedbacks between technology and social change and have erroneously viewed Marx as a technological determinist. These studies have also alluded to some imagined ambiguity in Marx's analysis of technological change. The perceived ambiguity is a result of confusing the role and position of technology in the various transitional stages along the path of socio-economic evolution.[20] In fact, Marx tries to capture what other authors in this book would call the 'matching' or 'mis-matching' between technological systems and forms of social organization, labelling it as the 'contradiction between productive forces and the relations of production'.

The main problem with Marx is that he recognized the significance of evolutionary factors but returned to a sort of 'long-term equilibrium' world-view in his prognosis for future social systems. In Marx's world, the socio-economic system has been experiencing moments of extreme fluctuations, of class struggles, but will tend towards a stable end-state, governed by sociaist principles—classless societies in which the sources of fluctuations and struggle are eliminated. Society settles into an equilibrium as the underlying social laws that Marx sought to lay bare prevail over individual action and inter-group conflict.

A different type of ambivalence is manifested by Marshall.[21] For Marshall the 'Mecca of economics lies in economic biology rather than economic dynamics'.[22] He argued that economics was like biology because they both dealt with 'a matter, of which the inner nature and constitution, as well as the outer form, are constantly changing'.[23] For Marshall the subject-matter was 'human beings who are impelled, for good or for evil, to change and progress'.[24] However, although he advocated the use of biological concepts, his own work paid only token allegiance to the approach. Much of his *Principles of Economics* is non-evolutionary except for the sections which deal with industrial organization and the division of labour where he draws on the concepts of survival of the fittest and the physiological view of human behaviour. He sees large-scale industries as trees of the forest which grow, compete for light and water, lose vitality, grow old and die; except for 'vast joint-stock companies, which often stagnate, and do not readily die'.[25] Interpreting his views in terms of contemporary biology, one could say that he conjectured (but did not demonstrate) that both short-term and long-term equilibria are the 'hill-climbing' results of selection processes.

Marshall's evolutionary views also differed from Marx's in terms of their 'gradualist' content. In Marx we find cumulative transition mediated through class antagonism which reaches a critical moment and makes a dialectical leap, a revolutionary overthrow of one class by another. In contrast Marshall adopts Darwinian gradualism: 'Economic evolution is gradual. Its progress is sometimes arrested or reversed by political catastrophes: but its forward movements are never sudden . . .'[26] Nevertheless, both Marx and Marshall (and implicitly unlike Smith) agree that the contributions of individuals add only little to the cumulative changes which have been in the making long before them. Thus

> any inventor, or an organizer, or a financial genius may seem to have modified the economic structure of a people almost at a stroke; yet the part of his influence which has not been merely superficial and transitory, is found on inquiry to have done little more than bring to a head a broad constructive movement which has long been in preparation.[27]

Marshall's Darwinian metaphors led him to visualize some form of selection equilibrium in the growth of the firms. He states that

> a business firm grows and attains greater strength, and afterwards perhaps stagnates and decays; and at the turning point there is a balancing or equilibrium of the forces of life and decay . . .[28]

But although such balances appear dynamic, Marshall did not abandon the Cartesian–Newtonian world-view. For a volume of the foundations of economics must 'give a relatively large place to mechanical analogies'.[29] Later, he offered an economic methodology under which mechanical analogies would be used in the early stages of economic development and biological explanations would take over in later stages.

> There is a fairly close analogy between the early stages of economic reasoning and the devices of physical statics . . . I think that in the later stages of economics better analogies are to be got from biology rather than from physics; and, consequently, that economic reasoning should start on methods analogous with those of physical statics, and should gradually become more biological in tone.[30]

Marshall's insistence on mechanical analogies probably reflects the influence of the Cartesian–Newtonian appeal to mathematical rigour. Mathematics was only useful to economics if it could throw 'a bright light on some small part of the great economic movement rather than at representing its endless complexities',[31] and where the subject-matter was stable enough in its parameters, or their derivatives, to be reduced to entities that validate the use of mathematics. Marshall's tone was thus somewhat Newtonian. Like celestial bodies, parts change while the whole remains stationary: individuals grow old and die while the population remains the same; grain prices fluctuate with every harvest but the average value of the grain remains stable.

The growing command of mankind over nature changes the character and magnitude of economic and social forces. To Marshall this is analogous with Newtonian mechanics:

> Our planetary system happens . . . to be a stable equilibrium; but a little change in circumstances might make it unstable, might for instance, after a time cause one of the planets to shoot away from the sun in a very long ellipse, and another to fall into it.[32]

For example, the law of supply and demand also takes on at an early stage a clear Newtonian perspective:

> In the earlier stages of economics, we think of demand and supply as crude forces pressing against one another, and tending towards a mechanical equilibrium; but in the later stages, the balance or equilibrium is conceived not as between crude mechanical forces, but as between the organic forces of life and decay.[33]

Thus Marshall manifests a commitment to the mechanical thinking of the day despite his appeal to biological analogies. At the same time, however, since society is not an ordinary combination of inanimate material, we have to revert to an organic view of economic activity. This ambivalence is reflected in his analysis of competition, leading to some confusion over perfect and imperfect competition. His approach was later reinterpreted by the neo-classical school, especially with the now standard formulations of monopolistic competition and imperfect competition.[34]

By the late nineteenth century it is clear that economics was being progressively purged of its organic content, prompting Veblen to ask: 'Why is economics not an evolutionary science?'[35] There are several answers, some of which have been alluded to above. First, biology was still embryonic at the time economics was consolidating itself. Darwin came to the scene a century after Adam Smith and development in the biological sciences was partly retarded by the emphasis on classification rather than on measurement and analysis. But even more important were the efforts made in the eighteenth and nineteenth centuries to adopt the Cartesian–Newtonian world-view to economic analysis.

This became the tradition that economists sought to belong to—the tradition of hard sciences, of irreducible and stubborn facts. The post-Smithian economics relied increasingly on particular forms of abstraction or units of analysis, and the best mathematical minds endeavoured to make the discipline an exact science. This process reached a significant peak with the publication in 1874 of Leon Walras's *Elements of Pure Economics*, whose general equilibrium theory had strong mechanical underpinnings. He visualized 'the pure theory of economics or the theory of exchange and value in exchange' simply as a 'physico-mathematical science like mechanics or hydrodynamics'.[36] The need to make economics an exact science was a strong drive during the period. Walras says that 'the establishment . . . of economics as an exact science is no longer in our hands and need not concern us. It is . . . perfectly clear that economics, like astronomy and mechanics, is both an empirical and rational science'.[37]

It is interesting to note how closely the modern tradition in economics echoes precisely this mechanistic framework. Economic systems are conceived of in terms of *units of production* (firms) and *units of consumption* (households) exchanging commodities and factor services in *markets* and *prices* which reflect the forces of supply and demand. Markets always are predisposed to clear since competition amongst buyers and sellers ensures that prices will *equilibrate* at precisely the point at which there is no excess or deficiency of goods and services in the market place. The system is then 'idealized' to reveal the conditions under which it will function in a 'perfect' way—there are many buyers, many sellers, perfect knowledge of all alternatives, no production/consumption complementarities and so on. Under such an idealized system all economic actors will behave perfectly predictably. The forces involved are those of competition, themselves determined by specific behavioural postulates of a psychological character and by technical conditions. Prices provide all the necessary information to allow the entities (households and firms) to behave optimally and, provided the system is suitably isolated, its internal behaviour can be described mathematically in a deterministic way.

Finally, historic time is effectively abolished in the sense that markets are assumed to clear instantly, there are no transactions costs, and future states of nature are either perfectly known or can have probabilities of their occurrence assigned to them.

Where system conditions are not such as to allow the economic system to behave in this idealized way, economists tend to talk in terms of 'market imperfections'. And, of course, that is what reality is all about. The point is, however, that the idealized system becomes the ultimate reference point against which all real states of the system are judged. In Whitehead's language a set of logical abstractions becomes a normative standard. The idealized system is turned from an analytic device into what nature *really is or should be*. Where the evidence does not seem to support such a view, which is practically all the time, then 'institutions' are routinely wheeled in to explain deviations from the mechanistic ideal.

But the very discipline that set the 'metaphorical' pace for economics started changing its course in the last century. The limitations of Newtonian physics started being felt with the theories of electromagnetism and thermodynamics. The process was later complemented by the theories of relativity and quantum physics, and with the acceptance of these theories Newton started to lose his grip on the natural sciences. It can therefore be argued that conventional economics was well ahead of the other social sciences, but in a misleading direction; even the pace-setter and archetype of the natural sciences, physics, had changed course. Georgescu-Roegen puts it as follows:

> By the time Jevons and Walras began laying the cornerstones of modern economics, a spectacular revolution in physics had already brought down the mechanistic dogma both in the natural sciences and in philosophy. And the curious fact is that none of the architects of 'the mechanics of utility and self-interest' and even none of the latter-day model builders seem to have been aware at any time of this downfall.[38]

Veblen's argument, that if 'economics is to follow the lead or the analogy of the other sciences . . . the way is plain so far as the general direction in which the move will be made'[39] (i.e. the evolutionary route), was not needed. Instead, evolutionary concepts in the post-Marshallian period sought refuge in other theoretical camps and co-existed with cartesian and Newtonian frameworks.

The post-Marshallian era

Although post-Marshallian economic thought was dominated by mechanistic notions, efforts were made to inject some dynamic elements into its content. One of these areas was market competition. Competition was viewed in conventional economics as analogous with Newtonian motions where resources 'gravitated' towards their most optimal pattern of utilization and prices were 'forced' to the lowest possible levels which could be sustained over the long run. Competition therefore guaranteed order and stability in the market just as gravitation did among Newtonian bodies.[40] However, this view did not adequately account for the competitive behaviour of firms. Economic theory was bedevilled by the paradoxical concepts of monopoly and perfect competition: 'Both are situations in which the possibility of any competitive behaviour has been ruled out by definition.'[41] In fact, in the pre-Second World War period, several attempts were made to introduce alternatives to the equilibrium/short-term analysis of economic systems. For example, Sraffa[42] criticized the logical foundations and empirical plausibility of the Marshallian compromise between U-shape cost curves and longer-term, non-decreasing returns. And, of course, since 'scarcities' and decreasing returns are fundamental elements of the neo-classical representation of the economy, Allyn Young's article on increasing returns[43] must be seen as a contribution (unjustly neglected) to the understanding of dynamic economies (implicitly or explicitly, economies with technological change).

Other elements of economic dynamics relate to product changes. In this respect, Chamberlin attempted to reorientate economic theory by introducing dynamic concepts. His analysis sought to synthesize monopoly and competition in a way that is akin to chemical processes[44] in so far as chemical synthesis requires continuous movement and change in which dynamic and static characteristics may be clearly distinguished. Although he remained in the orthodox economic mainstream, his work carried elements of evolutionary thinking: the dominant role of continuous product differentiation and the wide range of product possibilities suggests an implicit evolutionary content.

Although product variation plays a significant role in Chamberlin's model, it is not clear whether technology was to be held constant or not. But since he stressed product variation, it is reasonable to assume that innovation would be important. Indeed, he subsequently admitted that an entrepreneur would need to innovate to break away from the established order of things:

> The appearance on the market of any new products creates pressure in some degree on the markets for others, and when products are variable and determined by profit maximization some of this pressure is bound to be exerted on quality in order to maintain prices which people can afford to pay.[45]

Despite these dynamic aspects, Chamberlin did not seek to recast his theory of competition on an explicit evolutionary forge. This was left to other economists such as Alchian, who sought to replace the notion of explicit maximization with the biological concept of natural selection.

> The suggested approach embodies the principles of biological evolution and natural selection by interpreting the economic system as an adaptive mechanism which chooses among exploratory actions generated by the pursuit of 'success' or 'profit'.[46]

Competitive behaviour among firms, he argued, was not determined by the motive of profit maximization, but by 'adaptive, imitative, and trial-and-error behaviour in search for profits'.[47] Success was largely influenced and reinforced by previous success, not motivation. The fact that successful firms were still in the market was not a result of their 'conscious' profit-maximizing behaviour but rather an outcome of the fact that a whole lot of other firms had been selected out. The situation is clearly Darwinian: '[T]hose who realize *positive profits* are the survivors; those who suffer losses disappear.'[48] Alchian rejects the relative importance of Schumpeter's entrepreneur because even in a world of fools there would still be profits.

However, a limitation of Alchian's analysis is that, while giving a comprehensive assessment of the behaviour of firms in a competitive environment, he does not offer a convincing account of the role of technical change in the process of natural selection. Part of the problem results from excessive emphasis on imitative behaviour to which much of innovation is attributed.

> Adapting behavior via imitation and venturesome innovation enlarges the model. Imperfect imitators provide opportunity for innovation, and the survival criterion of the economy determines the successful, possibly because imperfect, imitators.[49]

He argues that

> Innovation is provided also by conscious wilful action, whatever the ultimate motivation may be, since drastic action is motivated by the hope of greater success as well as the desire to avoid impending failure.[50]

But his view neglects the conditions under which technical change becomes a critical tool for competition in so far as it sets in motion the conditions which call for its constant improvement.

As in neo-classical approaches, Alchian often seems to treat technical change as exogenous to economic evolution. It is merely brought into play for purposes of adaptation to the changing market environment but does not necessarily shape those conditions. Hence it is reasonable to conclude that Alchian did not seek to reframe all economic theory into an evolutionary outlook. Rather he restricted his analysis to firm behaviour by showing the irrelevance of the notion of profit maximization. Indeed, it has been argued (albeit never demonstrated) that evolutionary processes of adaptation and selection lead precisely to those equilibria analysed by neo-classical economists.

Boulding, on the other hand, attempted to restructure economics and bring it in line with ecological dynamics. He conceived micro-economics as a 'study of particular economic quantities and their determination'.[51] Firms or households, in his view, are analogous with economic organisms. He builds a theory which attempts to show the functional relationship between the behaviour of these organisms and the external environment.

> By developing a theory of the interaction of organisms through exchange, micro-economics also develops a theory of the determination of the main quantities of the system—prices, outputs, consumption, and so on.[52]

In a similar fashion macroeconomics is viewed as dealing with the national aggregates of individual quantities.

Boulding sought in ecology those elements which were analogous with existing economic abstractions such as population equilibrium and homeostatic mechanisms. This led him to a 'balance sheet' approach to economic analysis in which he expanded the Marshallian forest to include other organisms. It is a

> complex pattern of organisms, trees, grasses, flowers, birds, mammals, insects, reptiles, bacteria; subsisting, growing, propagating, dying in a maze of complementary and competitive relationships, all founded on the physical environment of earth, air, sun, water.[53]

But this reformulation remained ineffective in explaining economic transitions because it sought simply to replace static 'mechanical' equilibria with 'ecological' equilibria. Concepts such as homeostasis became crucial to the theory because they provided arguments for some form of balancing mechanism among economic organisms.

Economic organisms drift towards some steady-state situation. In his view, there is some state 'of the organism which is organized to maintain, and any disturbance from this state sets in motion behaviour on the part of the organism which tends to re-establish the desired state'.[54] This argument suggests that there are some inherent forces in economic organisms which direct it towards 'a homeostasis of the balance sheet'. The latter, however, may tell us a little of what happens in the structure of industry, but does not explain the dynamics of economic evolution.

The institutionalist tradition

Institutional economics, or institutionalism, provided one of the earliest expositions of evolutionary thinking. Institutionalism was not itself a coherent package of analytical tools, but a diverse collection of critical ideas built on a theoretical and methodological rejection of conventional economics. It revolved around Veblen, Mitchell and Commons, although, as Blaug says, the three economists had little in common:

> Veblen applied an inimitable brand of interpretative sociology to the working creed of businessmen; Mitchell devoted his life to the amassing of statistical data, almost as an end in itself; and Commons analyzed the working of the economic system from the standpoint of its legal foundations.[55]

They were dissatisfied with the narrowness of neo-classical thinking, demanded the integration of other social sciences into economic thought, and rejected the casual empiricism of conventional economics.

Institutionalism was rooted in dissent. In his post-mortem, Boulding sees some elements of suicidal criticism in the movement:

> Veblen is the type of dissenter of the sourest kind, whose weapons are irony and sarcasm and sardonic innuendo, but who . . . almost deliberately brings his own house on his head in the process of general destructiveness.[56]

In his critiques, Boulding generalizes on the basis of other forms of dissent, a type of reasoning which many would find fallacious. What is more important, however, is the fact that Boulding's own image ignores Veblen's contribution to evolutionary thinking in general and to the significance of technological change in particular. As with many critical attacks on the establishment, Veblen did not develop his ideas into a solid and consistent analytical framework, but he left behind interesting insights into economic systems that deserve attention.

He argued that economic activity evolves in an unfolding sequence, but that conventional economics had remained at the stage where 'the natural sciences passed through some time back'.[57] What could then replace the law of supply and demand, the theory of price equilibrium, marginal utility and the rest of the tools in the neo-classical kit? The answer lay in a reformulation of the contextual setting of economics, whose subject matter had to be seen as an unfolding sequence embodying evolutionary realism: 'There is the economic life process still in great measure awaiting theoretical formulation.'[58]

Industry and technology are the motive power behind this economic life process:

> The active material in which the economic process goes on is the human material of the industrial community. For the purpose of economic science the process of cumulative change that is to be accounted for is the sequence of change in the methods of doing things—the method of dealing with the material means of life.[59]

Veblen was writing at the turn of the century when the role of technological change in economic evolution had become apparent, but was largely unexplained. And to him, everyone was unavoidably trapped in the evolutionary sweep of technological advancement.

> Under the stress of modern technological exigencies, men's every-day habits of thought are falling into the lines that in the sciences constitute the evolutionary method; and knowledge which proceeds on a higher, more archaic plane is becoming alien and meaningless to them. The social and political sciences must follow the drift for they are already caught in it.[60]

Veblen emphasized the role of technological change, broadly defined to include both hardware and know-how. He stressed industrial arts to a point that bordered on determinism. The adage, necessity is the mother of invention, was reversed; invention had become the mother of necessity. Technological change was an inherent aspect of social evolution and still took place irrespective of economic factors. However, the issue at hand is the self-propelling dynamism that is accorded to technological change. Veblen often suggested new directions for analysis but left them undeveloped. It is in this sense that the role of technological change in the process of economic evolution had therefore to await the analysis of Schumpeter.

The Schumpeterian heritage

Schumpeter is one of the few economists who both questioned the static underpinnings of neo-classical economics and at the same time suggested an alternative approach. By locating economic transition within the broad context of social change, Schumpeter adopted, like Marx, an evolutionary model in which technological change and the efficacy of the entrepreneur as an innovative agent played the most significant role. However, he acknowledged the importance of Walrasian equilibria as 'ordering mechanisms' of the economy and, especially in his early work, presented a continuous tension between these two aspects of his writings—a tension which he never really resolved (on this issue see also Chapter 2 by Dosi and Orsenigo).

The Schumpeterian economic system carried strong evolutionary notions: 'The essential point to grasp is that in dealing with capitalism we are dealing with an evolutionary process.'[61] He goes on:

> The fundamental impulse that sets and keeps the capitalist engine in motion comes from the new consumer's goods, the new methods of production or transportation, the new market, the new forms of industrial organization that capitalist enterprise creates.[62]

These changes

> illustrate the same process of industrial mutation—if I may use that biological term—that incessantly revolutionalizes the economic structure *from within*, incessantly destroying the old one, incessantly creating a new one. This process of Creative Destruction is the essential fact about capitalism.[63]

In his early work, Schumpeter set out to analyze not the process of evolution itself, but the dynamics which bring it about.

> Not how the economic process developed historically to the state in which we actually find it, but the workings of its mechanism or organism at any given stage of development, is what we are to analyse.[64]

The influence of Walras and Marx can be noted at this metaphorical level in his reference to the 'mechanism or organism' of the economic process. He attempts to blend the two. Interestingly enough, Schumpeter follows Marx's cue by rejecting the hasty generalization arising from the Darwinian 'postulate that a nation, a civilisation, or even the whole of mankind, must show some kind of uniform unilinear development'.[65] He also rejects the Newtonian view of society by asserting that historical 'changes constitute neither a circular process nor pendulum movements about a centre'.[66] Moreover, both Marx and Schumpeter conjecture on the long-term outcomes of the evolution of capitalist economies. For Marx socialism would emerge from the collapse of capitalism, while for the later Schumpeter it would result from its success as investment opportunities shrink and the role of entrepreneurs becomes obsolete.

Schumpeter's theory of economic develoment emphasized the endogenous forces which bring about economic evolution and qualitative change. For economic development to occur, a society has to do more than just adapt to changing market conditions. If

> the phenomenon that we call economic development is in practice simply founded upon the fact that the data changed and that the economy continuously adapts itself to them, then we shall say that there is no economic development.[67]

In the Schumpeterian system, development is understood as 'changes in economic life as are not forced upon it from without but arise by its own initiative, from within'.[68] The transition is both cumulative and sequential:

> Every concrete process of development finally rests upon preceding development . . . Every process of development creates the prerequisites for the following.[69]

His evolutionary theory of development thus transcends the notions of circular economic flows and the tendency towards any general equilibrium. The changes in the circular flow and the destabilization of equilibrium originate in the sphere of industry and commerce (on the supply side), not in the area of 'wants of the consumers of final products' (on the demand side). The shift is not, by definition, minor; it is one

> which so displaces its equilibrium point that the new one cannot be reached from the old one by infinitesimal steps. Add successively as many mail coaches as you please, you will never get a railway thereby.[70]

Schumpeter emphasizes further the evolutionary view of economic change in his Business Cycles:

> As a matter of fact, it is to physiology and zoology—and not to mechanics—that our science is indebted for an analogous distinction which is at the threshold of all clear thinking about economic matters.[71]

He defines economic evolution as the 'changes in the economic process brought about by innovation, together with all their effects, and the responses to them by the economic system.[72]

Hence we have a picture which is both *co-evolutionary* and *far from equilibrium*. The creation of 'economic space' or a market niche leads to the swarming towards new innovations by imitators as the copying or modification of newly introduced technologies become increasingly possible. In the Schumpeterian system such opportunities come in clusters and are unevenly distributed, so that the changes which result from these disequilibria are not relatively smooth, as a Darwinian process would tend to be, but proceed in jerks and rushes. Nevertheless, it is still possible to locate their epicentre.

In every span of historic time it is easy to locate the ignition of the process and to associate it with certain industries and, within these industries, with certain firms, from which the disturbances then spread over the system.[73]

However, as the capitalist system matures Schumpeter visualizes a situation where investment opportunities vanish and the entrepreneurial function becomes obsolete, forcing the economy into near-equilibrium 'socialist' practice.

> Technological progress is increasingly becoming the business of teams of trained specialists who turn out what is required and make it work in a predictable way. The romance of earlier commercial adventure is rapidly wearing away, because so many more things can be strictly calculated that had of old to be visualised in a flash of genius.[74]

Finally Schumpeter delivers his ultimate prognosis:

> Since capitalist enterprise, by its very achievements, tends to automatise progress, we conclude that it tends to make itself superfluous—to break to pieces under the pressure of its own success.[75]

This return of the economic system to a near-steady state associated with socialist organization suggests that the appeal to stable or near-stable systems that has characterized the post-seventeenth-century intellectual tradition influenced Schumpeter's thinking just as it affected Marx's.

Schumpeter's work, however, forms a significant starting point for the analysis of non-equilibrium economic structures and has been built upon by a number of modern economists, as discussed in more detail by Dosi and Orsenigo (Chapter 2) and by Silverberg (Chapter 24). While in our view there are still strong strains of 'mechanistic' thinking permeating this work (for example, his fascination with Walrasian theory as the description of ideal equilibrium conditions of the economy), nevertheless Schumpeter remains a major source of inspiration for those economists who begin to engage more directly with socio-economic complexity in theory building. For example, the work of Freeman and Perez, Silverberg and Arthur illustrates quite clearly (see Chapters 3, 24, 26) the systematic and non-equilibrium dynamics of modern industrial production.

Some concluding points

In this chapter we have provided a survey of some of the 'classic' historical literature in evolutionary economics. While not complete (for example, we have not mentioned the German Historical school which influenced the early American economists), our survey appears to indicate that this tradition, despite giving many useful insights, has never engaged systematically with technological change as a *process*. Instead, most of its practitioners have tended at best to erect an evolutionary canopy over a conceptual structure which remains fundamentally a mechanistic one.

It remains for us, finally, to speculate briefly upon the reasons for this trend. One possible set of reasons is *ideological* in that the appeal of 'private selfishness' mechanically yielding harmony was well suited to match the 'common-sense' justifications of the status quo and also to defend the benefits that derive from it. It is our view, however, that a more important set of reasons is *cognitive*. The elegance, and early success, of classical physics appeared to validate a view of nature in which discrete 'entities' are linked together by 'forces' according to simple laws. However, we now know that although classical physics has been very successful in describing the gross behaviour of inert macro-systems, it has, as Allen points out in Chapter 5, been singularly unsuccessful in explaining living systems at whatever level of size or complexity. Nor does it always explain very well changes of form and structure even in inert macro-systems. Conversely, although their views are still in a minority amongst natural scientists, modern theories which stress the non-linear and self-organizing dynamics of complex structures (associated with Bohm, Haken, Prigogine and others) seem more suited to the essentially systemic nature of technological change.[76]

What is certain, however, is that a fresh look should be taken at many of these matters. Our own view, which we have developed in detail elsewhere,[77] is very much in accordance with that of Allen (see Chapter 5), who argues the need for a dynamic systems approach integrating many disciplines. We believe that by portraying the evolution of new technologies as complex and unstable systems based upon flows of information and guided by socially agreed paradigms, not only is it possible to open up the 'black box' of technical change (normally reduced to an exogenous 'catch up' in standard models); it is possible also to give economic growth and development a content of detailed causation. Much, however, still remains to be done.

Notes and References

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3. J. Schumpeter (1934), *The Theory of Economic Development*, Cambridge, Mass., Harvard University Press, p. xi.
4. With the exception arguably of Malthus. See, for example, N. von Tunzelmann (1986), 'Malthus' "total population system": a dynamic re-interpretation', in D. Coleman and R. Schofield (eds), *The State of Population Theory*, Oxford, Blackwell, pp. 65–95.
5. A. Smith (1961), *The Wealth of Nations*, Vol. I (Cannon (ed.)), London, Methuen, p. 20.
6. ibid., p. 20. Students of entomology would take issue with Smith for not considering the division of labour among bees and ants. In fact, in his day, Linnaeus had already described aphids as ants' cows, recognising the division of labour among insect societies. Houthakker (1956) had provided economic arguments showing that the Smithian notion that the division of labour was limited by the extent of the market was analogous to speciation, or the formation of species among animals. See H. Houthakker (1956), 'Economics and biology: specialization and speciation', *Kyklos*, vol. 9, pp. 180–9.
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8. See A. Hirschman (1982), *Shifting Involvements: Private Interest and Public Action*, Oxford, Martin Robertson.
9. D. Meek (ed.) (1953), *Marx and Engels on Malthus*, London, Lawrence & Wishart, p. 172.
10. ibid., p. 187.
11. We shall return to the Darwinian stand in our review of Marshall.
12. See R. Colp (1982), 'The myth of the Marx–Darwin letter', *History of Political Economy*, vol. 14, no. 4, pp. 416–82, for detailed assessment of the contacts between Marx and Darwin, especially on the myth that Darwin rejected Marx's requests to dedicate *Capital* to him.
13. K. Marx (1976), *Capital*, Vol. I, Harmondsworth, Penguin, p. 505. Marx captured the ultimate organic metaphor in the 'attempt made to construct a locomotive with two feet, which raised it from the ground alternatively, like a horse.'
14. K. Marx (1976), *The Poverty of Philosophy*, Moscow, Progress Publishers, p. 102.

15. Marx, *Capital*, Vol. I, op. cit., p. 493.
16. ibid., p. 493. Marx attempted to develop these ideas in unpublished notebooks. See E. Colman (1971), 'Short communication on the unpublished writings of Karl Marx dealing with mathematics, the natural sciences and technology and the history of these subjects', in N. Bukharin (ed.), *Science at the Crossroads*, London, Frank Cass, pp. 234–5.
17. Marx, *Capital*, Vol. I, op. cit., pp. 450–61.
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54. ibid., p. 26–7.
55. M. Blaug (1968), *Economic Theory in Retrospect*, London, Heinemann, p. 678. For a detailed review of the history of institutionalism since Adam Smith, see J. Spengler (1974), 'Institutions, institutionalism: 1776–1974', *Journal of Economic Issues*, vol. 8, no. 4, pp. 877–96.
56. K. Boulding (1957), 'A new look at institutionalism', *American Economic Review*, vol. 47, May, p. 2. More recently Boulding has changed his position on these issues and has developed a much more organic perspective. See Boulding, K. (1978), Ecodynamics, A New Theory of Societal Evolution, Beverly Hills, London, Sage, and (1981), *Evolutionary Economics*, Beverly Hills, London, Sage.
57. Veblen, op. cit., p. 384.
58. ibid., p. 387.
59. ibid., p. 387.
60. ibid., p. 397. Veblen placed his evolutionary conception in an institutional context thus: 'From what has been said it appears that an evolutionary economics must be the theory of a process of cultural growth as determined by the

economic interest, a theory of a cumulative sequence of economic institutions stated in terms of the process itself' (ibid., p. 393).

61. J. Schumpeter (1943), *Capitalism, Socialism and Democracy*, London, Allen & Unwin, p. 82.
62. ibid., p. 83.
63. ibid., p. 84.
64. J. Schumpeter, *The Theory of Economic Development*, op. cit., p. 10.
65. ibid., p. 57.
66. ibid., p. 58.
67. ibid., p. 63.
68. ibid., p. 63.
69. ibid., p. 64.
70. ibid., p. 64.
71. J. Schumpeter (1939), *Business Cycles*, Vol. I, New York, McGraw-Hill, p. 37.
72. ibid., p. 86.
73. ibid., p. 102.
74. J. Schumpeter (1943), *Capitalism, Socialism and Democracy*, op. cit., p. 133.
75. ibid., p. 133.
76. See, for example, D. Bohm (1980), *Wholeness and the Implicate Order*, London, Routledge; F. Capra (1983), *The TAO of Physics*, London, Fontana, 2nd ed.; I. Prigogine and I. Stengers (1984), *Order Out of Chaos*, London, Heinemann.
77. N. Clark and C. Juma (1987), *Long Run Economics: An Evolutionary Approach to Economic Growth*, London, Pinter Publishers.