1. Approaching economic evolution

1.1. The contours of an evolutionary research programme around 1950

The study of economic evolution presupposes a Copernican turn of perspective *vis-à-vis* standard forms of economic analysis. Instead of dealing with an economic system which is only adapting to exogenous change, we study an evolving, i.e. self-transforming[1] economic system. This fundamental shift of perspective is emphasised by Schumpeter, a pioneer of the study of economic evolution. He tells us that at an early point in his professional life he studied Walras' standard form of economic analysis and discovered that it implied the idea of an economic system which '[i]f it changes at all, it does so under the influence of events which are external to itself'; in other words, 'economic life is essentially passive and merely adapts itself to the natural and social influences' (Schumpeter, 1937/51, 159). This basic approach to economic life was much respected by Schumpeter who praised Walras as the greatest of economists. However, he did not subscribe to the Walrasian perspective. He tells us that he

> ... felt very strongly that this was wrong, and that there was a source of energy within the economic system which would of itself disrupt any equilibrium that might be attained. ... It is such a theory that I have tried to build and I believe now, as I believed then, that it contributes something to the understanding of the struggles and vicissitudes of the capitalist world and explains a number of phenomena, in particular the business cycle, more satisfactorily than it is possible to explain them by means of either the Walrasian or the Marshallian apparatus. (Schumpeter, 1937/51, 160)

Today we can learn much about the radical change in perspective from Schumpeter. But he would have been the first to admit that there is a long way from an evolutionary perspective and a personally coloured theory to a viable form of evolutionary economics. On the way to what may be called a new evolutionary economics, we need much more than a 'dialogue' with Schumpeter and other representatives of what may be labelled old evolutionary economics.[2] In the present book it will be argued that:

A viable new evolutionary economics is characterised by
(1) a population perspective,
(2) an empirical orientation,
(3) a mix of an algorithmic and a fully formal approach,[3] and
(4) a 'dialogue' with older, verbal studies of economic evolution.

This proposed description of the emerging new evolutionary economics may suitably be explored initially in a specific historical context, namely the period around 1950 which is the year Schumpeter died. Here we have the chance of studying the transmutation of the study of economic evolution as well as of immediately opening the dialogue with Schumpeter. In the latter respect we shall confront one of Schumpeter's last contributions where he presented the empirical orientation of evolutionary economics in a provocative and thought-provoking form which leads to a broader consideration of the relationship between facts and formalisms. In relation to the transition from old to new evolutionary economics we find in the same period an early, non-evolutionary but provocative sketch of an algorithmic approach to economic mechanisms in a paper by Goodwin, which can be used at a starting point for a sketch of this approach. Finally, we also find Alchian's rough and thought-provoking version of the population perspective from 1950.

1.1.1. The empirical orientation as Schumpeter's final thesis

In the history of economic thought and analysis Schumpeter has a paradoxical role: on the one hand, he is probably the sole writer who gave the issue of what we now call economic evolution the pivotal place in his creative analytic work;[4] on the other hand, he is closely connected to the marginalist revolution and to the crowding out of evolutionary perspectives by his agreement to set the standards of economic analysis so high that they cannot be met by evolutionary analysis. Throughout his intellectual career he was tormented by the conflict between his evolutionary pivot and his emphasis on conceptual clarity and mathematical tools of analysis. This is emphasised by him in a couple of letters he wrote in the early 1940s. In the first letter he points out that at an early point in his intellectual career he 'worked out a theory of economic evolution into which personal observation, historical studies, and theoretical work enter in proportions which it is difficult to define.' (Schumpeter, 1991a, 229) In the second letter he states that

> ... there is nothing in my structures that has not a living piece of reality behind it. This is not an advantage in every respect. It makes, for instance, my theories so refractory to mathematical formulations. They can never be so cut and dried as Keynes' schema is; but there are compensating advantages, and one of them is that so many people have told me ...: 'Yes, this is so. I know that from my own experience and observation'. (Schumpeter, 1991a, 230)

However, according to his own criteria of scientific excellence, such an acknowledgement is not enough for a successful theorist. Therefore, Samuelson is right when he points out that

> ... though much of the world regarded Schumpeter as the very essence of an economic theorist, he regarded himself as in a sense a theorist *manqué* [an unsuccessful theorist]. When singing the praises of more exact methods in our beloved science of economics,

> Schumpeter claimed he was entitled to do so with a better right since his own work was primarily not in the airy heights of mathematical theory. (Samuelson, 1981, 3)

The reason for this lack of success is mainly due to his chosen area of study rather than to any lack of brightness and analytical skills.[5] Actually, the problems of formalisation of evolutionary arguments and the heavy emphasis on personal and historical investigation is also found in the relevant parts of the works of other economists like Marshall (1890/1961, book 4; 1919) and it is a central characteristic of Darwin's (1859/1964) work on biological evolution.

Such an area did not fit the main trend towards professionalisation of the economics discipline which was strongly supported by Schumpeter: 'One of the many ironies in his life is that his ardent support of mathematics in economics drove his students away from the fields of intellectual endeavor that made his own work so significant, and produced many results that he considered sterile.' (Smithies, 1951, 14) On this background, it is not difficult to understand why the old Schumpeter wanted to present his intellectual testament in historical rather than in mathematical form, even if the latter was his basic ambition (dating back to Schumpeter, 1906/52; 1908).

Less than two months before his death, Schumpeter protested against the crowding out of evolutionary perspectives from economics in a way which shocked not only his young colleagues like Samuelson and Goodwin (Swedberg, 1991, 176) but also broader groups of active researchers in economics and econometrics.[6] The event took place at the Conference on Business Cycles arranged in collaboration between university researchers and the National Bureau of Economic Research (U-NBER, 1951). Schumpeter's point of attack in his paper on the 'Historical Approach to the Analysis of Business Cycles' was implicit but clear to all participants: econometrics and mathematics as the all-dominant tools for the march of modern economics. Schumpeter chose to 'let the murder out'—i.e. to spoil his own performance and create an unpleasant and intellectually troublesome state of affairs—at this conference which gathered a good deal of the most talented young economic theorists, statisticians and model builders of the US:

> To let the murder out and to start my final thesis, what is really required is a large collection of industrial and locational monographs all drawn up according to the same plan and giving proper attention on the one hand to the incessant historical change in production and consumption functions and on the other hand to the quality and behavior of the leading personnel. (Schumpeter, 1949/51a, 314)

The purpose of this apparently idiosyncratic proposal of 'detailed historical case studies' (p. 311) was to elucidate the mechanisms underlying much of the cyclical behaviour of economic aggregates. Even the cyclical behaviour of investment is in itself a surface phenomenon and we have to investigate 'the actual industrial process that produces it and in doing so revolutionize existing economic structures.' (p. 312, emphasis removed) The mechanisms of this process had not been studied by Schumpeter's ambitious audience. Actually he thought that

> ... the most serious shortcoming of modern business-cycle studies is that nobody seems to understand or even to care precisely how industries and individual firms rise and fall and how their rise and fall affects the aggregates and what we call loosely 'general business conditions'. (Schumpeter, 1949/51a, 315)

This lack of interest was especially painful to Schumpeter who was the economist who most clearly had emphasised the evolutionary foundations of the behaviour of economic aggregates. Actually, he had little real intellectual influence even if he had high scores in other respects: 'at the time of his death, a citation index shows that Joseph Schumpeter was the scholar most often cited in the whole field of economics.' (Samuelson, 1981, 1) However, while Keynes' main arguments were theoretically interpreted, developed and applied to a degree where no further reference to the *General Theory* was necessary, Schumpeter got a place in the footnotes. He became

> ... placed in the category 'footnote economist', i.e. an economist whose works are mentioned in footnotes but are seldom reported and applied more directly in a theoretical interpretation or further development. (Jensen, 1988, 97, my transl.)

Schumpeter's 'final thesis' about the need for industrial and locational case studies was so radically out of touch with the 'logic of the modern scientific situation' (Schumpeter, 1949/51a, 309) that no short-term results could have been obtained. The profession chose a respectful passing over of Schumpeter's 'uncharacteristic performance' (Samuelson, 1951). The reason was that his proposal seemed hopelessly old-fashioned not only because the results of the case studies would be unsuited for mathematical and econometric treatment but also because his emphasis on evolution and leaders looked like romantic-idealistic anachronisms (Swedberg, 1991, 176).

Today we may say that as a representative of the old evolutionary economics Schumpeter was, in a certain sense, an anachronism but his empirical orientation and his emphasis on the heterogeneity principle (which is probably underlying the suggested case studies) is fully in accordance with modern evolutionary studies. His aim appears to have been to influence the trajectory of modelling work by creating an informal demand specification by means of paradigmatic cases and 'stylized facts' (Kaldor, 1961, 178 f.). At least he emphasised (Schumpeter, 1949/51a, 308) that nothing was further from his mind than to start a new Battle of Methods like the one that had earlier raged between marginalist economists and the historical and institutional schools of economics.[7] Instead the task confronting researchers which follows from his advice is ultimately to explore the mechanisms underlying the Schumpeterian type of business cycle, and here no method has a monopoly. 'Theoretical and statistical analysis is in this task as necessary as is historical work. In fact they are inseparable because there is an incessant give and take between them.' (p. 315).[8] The problem for the old

Schumpeter was that balance between the different components had been disturbed and that theoretical and statistical work proceeded without noticing what to him appeared to be the basic explanatory variables.

This balanced approach to theory-evolution seems to be quite modern but this does not make Schumpeter look like a representative of renewed evolutionary studies. Actually, we have to conclude that even if the *Zeitgeist* had not been against him, his proposal to make historical case studies would probably not have helped much as a stand-alone cure against the crowding out of evolutionary perspectives. What was needed was nothing less than a theoretical breakthrough in evolutionary economics which was, in fact, seriously impeded by Schumpeter's emphasis on the historical complexities of evolution and on the explanation of the behaviour of macroeconomic variables. Even Schumpeter's core agent, the innovative entrepreneur who breaks the spell of old routines, was too complicated to start with. Finally, the initial steps of the breakthrough were also impeded by Schumpeter's sharp critique of the use of biological analogies in the exposition of evolutionary-economic ideas (Schumpeter, 1912/34, 57 f.; 1954, 789). For these and other reasons, Schumpeter must rather be considered as belonging to the old tradition of studying economic evolution than to what may be called the new evolutionary economics.[9] However, as soon as a new start is established, Schumpeter's works reappear as a mine of ideas.

1.1.2. The algorithmic approach and Goodwin's prologue

Of special interest is Schumpeter's life-long and fundamental analytical tool problem which has still not left the evolutionary studies in economics and other parts of social science. Even if he provided a 'herculean labor' (Schumpeter, 1954, v), he was not able to overcome this difficulty and live up to his high standards of formalisation. His readers are lucky that he, as mentioned above, developed ways of compensating for the lack of rigour in his theoretical structure. To some extent he transformed some of the difficulties within his chosen area of study into strengths, not least the personal touch and style as well as a richness of historical evidence. But as a theorist he still had a basic 'supply side' problem: the lack of adequate analytical tools for expressing his evolutionary theory compared to the relative abundance of tools suited for other types of studies. In this context the task that Schumpeter and the economists who occasionally tried to help him (Frisch, Samuelson and Goodwin) had set themselves was next to hopeless: Schumpeter insisted on certain characteristics of his evolutionary scheme which could not be supported by the available mathematics or, rather, the available mathematically skilled economists of his time. Goodwin gives an example:

He [Schumpeter] patiently listened to a series of lectures I [Goodwin] gave on Keynesian-type cycle theories, but he would have none of it. Now, half a century later, I better understand why. To begin with they were aggregative, global formulations, which he rightly rejected, since they quite failed to take adequate account of the continuing, innovational re-structuring of the productive economy. Furthermore the whole class of models of that type are unacceptable because they are linear. He perhaps sensed without clearly perceiving this fatal flaw, since his mathematical aptitude was so limited that he barely understood linear dynamic system, let alone nonlinear ones. (Goodwin, 1988, 6)

Schumpeter's lack of mathematical maturity was probably not as much due to a lack of attempts to conquer mathematical tools but rather to his unwillingness to stick to the non-evolutionary assumptions underlying the application of these tools. As Tinbergen (1951/91, 178, 176) has remarked, Schumpeter 'lived another life' and his 'whole attitude vis-à-vis the setting of the problems and their solutions' was radically different from that of the available mathematicians and econometricians.

However, Schumpeter might have had much use for the simple algorithmic approach to the description of economic mechanisms which Goodwin published shortly after Schumpeter's death in the short paper on 'Iteration, Automatic Computers, and Economic Dynamics' (Goodwin, 1951/82). This paper had no immediate relevance for evolutionary economics (and Goodwin has later shown few signs of embracing the population perspective) but he is suggesting that an algorithmic approach to the description of economic mechanisms is an important task in itself. The mechanism in question is the equilibrating mechanism of the Walrasian economic system. But as we shall see, the algorithmic approach is also very helpful by increasing our ability to describe in a clear and flexible way different evolutionary mechanisms. This was exactly what Schumpeter was, in my opinion, searching for from an early point in his life as an economist. Schumpeter's starting point was a methodological analysis of marginalist economics where he emphasised its strictly non-evolutionary nature and suggested a complementary, more-or-less evolutionary research programme (Schumpeter, 1908). On this background, Schumpeter became less interested in Walras' theorems on the characteristics of an economic system in general equilibrium than in an attempt to contrast the mechanisms of innovation with Walras' loosely formulated ideas of the equilibrating mechanism of the economic system. Furthermore, it was of crucial importance to describe the equilibrating mechanism in a simplified manner in order to make possible to insert the complexities implied by rule-changing behaviour into the model. Therefore, the perspectives which were more or less clearly raised in Goodwin's paper suggest an important starting point.

Let us in a modern programming language restate parts of the lecture that Goodwin could have given to Schumpeter. The starting point is Walras' famous words about the equilibrating economic mechanism:

Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping, and, before the goal is reached, it has to renew its efforts and start over again, all the basic data of the problem, e.g. the initial quantities possessed, the utilities of goods and services, the technical coefficients, the excess of income over consumption, the working capital requirements, etc. having changed ... like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. (Walras, 1874/1954, 380)

Goodwin (1951/82, 103) remarks that 'Walras's conception of and term for dynamic adjustment—*tâtonner*, to grope, to feel one's way—is literally the same as that of modern servo theory.'[10] But Walras was not clear as to the character of his picture: sometimes it seems as if it is the economist who gropes for a solution to a system of equations by using the iterative algorithm, sometimes it seems as if we are dealing with an actual economic process where the agents acts in real time. The confusion is serious since Walras is basically dealing with a mathematical method but we may turn it into an interesting question about the actual economic mechanism. This at least is what Goodwin suggests and to some extent explores by means of Wiener's cybernetics study of servo-mechanisms (goal-seeking mechanisms). At that time computer language was not yet invented, but it is not a large jump to try, in the present book, to express the idea in modern terms.

Let us start with a totally naive version of the *tâtonnement* process led by an auctioneer who starts by proposing a trial price vector ($\overline{P}_0$) and then checks whether the quantities of the m commodities supplied and demanded by the n agents would actually clear all markets, if transactions were allowed to take place (i.e., whether $\sum_j D_{ij}\left(\overline{P}\right) = 0$, for $j = 1, \ldots, m$). We, therefore, may formulate a 'blind' trial-and-error process in the form of the following algorithm in a semi-formalised programming language:

Naive Tâtonnement:
procedure (agents, *i*, with commodities, *j*)
  forbid exchange between *i*'s;
  repeat
    let $P_j := \text{random}\left(\Re^{+,0}\right)$, for all *j*;
    invoke $D_{ij}\left(\overline{P}\right)$, for all *i* and *j*;
  until [$\sum_i D_{ij}\left(\overline{P}\right) = 0$, for all *j*];
  allow exchange between *i*'s;
end;

This algorithm will (probably) never stop unless we define a minimum error which is neglected by the economic agents. Even then the probability for finding a solution is incredibly small for a complex economy (see section 4.1.2 on problem complexity theory). There are, however, well-known algorithms (or heuristics in the Artificial Intelligence sense) to make things easier, provided that the excess demand function for each market is well-behaved. We only indicate a simplified version of this algorithm in order to give the flavour of the argument including the Walrasian servo-mechanism, i.e. $\Delta P_j = \sum_i D_{ij}\left(\overline{P}\right) > \varepsilon$:

Smarter Tâtonnement:
procedure (agents, *i*, with commodities, *j*)
    forbid exchange between *i*'s;
    let $P_j := \text{random}\left(\Re^{+,0}\right)$, for all *j*;
    invoke $D_{ij}\left(\overline{P}\right)$, for all *i* and *j*;
    while [$\sum_i D_{ij}\left(\overline{P}\right) > \varepsilon$, for some *j*] do
        let $P_j := P_j - \lambda \sum_i D_{ij}\left(\overline{P}\right)$, for all *j*;
        invoke $D_{ij}\left(\overline{P}\right)$, for all *i* and *j*;
    allow exchange between *i*'s;
end;

This algorithm will under the given assumptions converge towards a stop condition consisting of a minimum change of one of the quantities during an iteration. In itself it is not very interesting but the semi-formal approach that it represents makes it relatively easy to formulate a large number of questions for further study: Why is it that the prices and not the quantities (or a mix) equilibrate the system? What happens if the mythical auctioneer is removed and out-of-equilibrium transactions are allowed? By which mechanisms can the agents obtain their apparent foresight? What happens if the behaviour of the agents is relatively inflexible and rule-following? Through experiments with algorithms that try to answer such questions, it becomes patently clear that the Walrasian system represents an extreme case of a non-worldly type. At the same time, it is demonstrated that starting from a confrontation with Walras helps to organise the search for more realistic economic mechanisms and that a programming language may help to articulate the different alternatives. The results of the theories of computability and complexity help to evaluate the abstract possibility of an algorithm as well as its tendency to computational explosion.[11] However, through such studies we gradually drift away from the general equilibrium system and begin to see that the search for a single economic mechanism will never succeed.

So far, we have more or less been following Goodwin. Now we may ask whither this exercise leads. Some might consider this series of studies as a gradual approximation for an evolutionary analysis of economic life. But the underlying heuristic rule is rather peculiar: Instead of guiding theory-evolution by a preliminary version of the searched evolutionary mechanisms by visions, preliminary versions of mechanisms and paradigmatic examples or 'stylised facts', we move forward by looking backward. The research agenda is set by the problems of the Walrasian system rather than by our goal of finding schemes of evolutionary analysis. This appears to be a hopeless strategy for evolutionary economics. Instead we should turn it around by formulating a new goal or 'demand specification' for the mechanisms for which we are searching. Or we may, as the Walras-worshipper Schumpeter might have remarked, look at the next page in Walras' great work where inflexibilities and sudden disturbances of economic life are more important:

It can happen and frequently does happen in the real world, that under some circumstances a selling price will remain for long periods of time above cost of production and continue to rise in spite of increases in output, while under other circumstances, a fall in price, following upon this rise, will suddenly bring the selling price below cost of production and force entrepreneurs to reverse their production policies. For, just as a lake is, at times, stirred to its very depths by a storm, so also the market is sometimes thrown into violent confusion by *crises,* which are sudden and general disturbances of equilibrium. (Walras, 1874/1954, 380 f.)

Analysing this kind of economic process appears to have been one of the driving forces behind the young Schumpeter's (1912) creation of his own theory of economic evolution (*wirtschaftlische Entwicklung*) and his later attempts to substantiate it (Schumpeter, 1939). Here the task is basically to find an evolutionary underlying the shifting periods of prosperity and depression:

... our mind will never be at rest ... until we have assembled in one model causes, mechanisms, and effects, and can show how it works. And in this sense, whatever we may object, the question of causation is the Fundamental Question, although it is neither the only nor the first to be asked. (Schumpeter, 1939, 34)[12]

The importance of the algorithmic approach is that it helps to depict the basic mechanisms which connect cause and effect in an evolutionary process whose concrete path is essentially irreversible. Thus while this approach has basically helped to emphasise the limits of the Walrasian approach, it has a much more positive effect on Schumpeter's research programme. If we may envisage 'Schumpeter with a computer',[13] we recognise that he has obtained a very different expressive power with respect to the 'Fundamental Question' than the real Schumpeter before the computer age. To him new problems will emerge while some of the old problems will simply vanish. Some of these problems will be explored in chapters 3-5 where we shall deal with 'Artificial Economic Evolution'

1.1.3. The population perspective as introduced by Alchian

The transformation from old to new forms of evolutionary economics may be said to have started in the year that Schumpeter died. The programmatic announcement of central themes in new evolutionary economics was made in the paper on 'Uncertainty, Evolution, and Economic Theory' (1950/93) by Alchian, an economist who seems to have been outside Schumpeter's sphere of influence. This now-classic paper—which appears to be based on analogies between biological and economic evolution—presents a revision of neoclassical economics with new assumptions like imperfect information and rule-bound behaviour. Economists have traditionally assumed that economic agents make rational choices or selection among behavioural alternatives. Instead we should 'treat the decisions and criteria dictated by the economic *system* as more important than those made by the individuals in it.' (Alchian, 1950/93, 67) In this new perspective the object of analysis is a heterogeneous set of firms which are subject to a process of selection. In other words, Alchian introduces explicitly into economic analysis what we now call 'population thinking'.

The idea that population thinking is a central characteristic of evolutionary analysis has most clearly been developed in evolutionary biology (see Mayr, 1976, 26 ff.) where this mode of thinking confronts non-evolutionary 'typological thinking'. To think in terms of typologies means to consider the differences between basic types and their concrete instances as something which should be ignored in order to focus on the true essence of the phenomena. For researchers who think in terms of 'populations', things are quite different: the 'typical' characteristics of a population of economic agents are just abstractions while reality is characterised by varying behaviour. More important: this variance is the fuel of the evolutionary process; to ignore it is to retreat to a Platonic world of non-evolving 'ideas'. This retreat is appealing from a purely logical point of view but it makes it impossible to deal with the empirically observable gradual evolution. The same is the case when evolutionary syntheses are crowded out in order to promote logical simplicity. To consider all economic agents as perfectly informed and rational is to consider them as representing a homogeneous type which cannot evolve. Their type is a 'lightning calculator of pleasures and pains' (Veblen, 1898/1961, 73) which responds with varying behaviour to different parameter settings. On the other hand, if their behavioural characteristics (e.g., with respect to degree of profit maximisation) are varying and 'sticky', then there is room for evolutionary processes.

In Alchian's population perspective we consider a large number of different agents and, therefore, we tend to abstract from the processes of individual decision-making or at least to consider them in a highly simplified form. In the most simplified form, we have a Darwinian-like trial-and-error process. Alchian illustrates some parts of such a process through the example of the behaviour of a population of travellers:

> Assume that thousands of travellers set out from Chicago, selecting their roads completely at random and without foresight. Only our 'economist' knows that on but one road are there any gasoline stations. He can state categorically that travellers will *continue* to travel only on that road; those on other roads will soon run out of gas. Even though each one selected his route at random, we might have called those travellers who were so fortunate as to have picked the right road wise, efficient, foresighted, etc. (Alchian, 1950/93, 69)

This set-up is made to demonstrate that totally random behaviour may be filtered by a selection mechanism in a way which makes the observable behaviour look as if it is informed and well-thought-out. Our omniscient economic researcher is shocked by the many attempts to conclude on the appearances and to rationalise the behaviour of the travellers. But he is able to explain and, if he is informed, to predict changes in the behaviour:

If gasoline supplies were now moved to a new road, some formerly luckless travellers again would be able to move; and a new pattern of travel would be observed, although none of the travellers had changed his particular path. The really possible paths have changed with the changing environment. All that is needed is a set of varied, risk-taking (adoptable) travellers. (Alchian, 1950/93, 68 f.)

Even these changes may be misinterpreted: the correspondence between changes in behaviour and the economic environment appears to suggest that the travellers are able to learn and keep their acquired characteristics. But, according to Alchian, this is really a case of 'Lysenkoism', i.e. mistaken Lamarckism where a Darwinian account would have been correct. On the other hand, it should be remarked that Alchian's emphasis on a random mechanism in the process of problem solving at the population level is extremely wasteful: most of the travellers have to walk back to Chicago but some solve the problem of getting away from the city. It is also very demanding with respect to the irrationality of the agents: if all travellers are learning not to use the non-supplied roads by doing the first experiment, then nobody will explore the new road where gasoline is present in the second experiment. This version of the system is characterised by a lock-in situation where no long-distance travelling takes place. The presence of an absorbing state for the system demonstrates that we are not facing a full analysis of an open-ended evolutionary process and this may be the reason why Alchian did not explore a Lamarckian mechanism of learning by doing and the transmission of acquired characteristics.

When Alchian deals with economic agents rather than imaginary travellers it is important to ask for the 'criterion by which the economic system selects survivors' (Alchian, 1950/93, 67). The obvious answer is that the criterion is the level of realised profits of the individual firms: 'those who realize *positive profits* are the survivors; those who suffer losses disappear.' (*ibid.*) Like in the case of Darwinian natural selection, we should not presuppose that the success or survival of individual firms are based on full information about the rules of the game and about the other agents in it. The question of success is decided *ex post* rather than *ex ante*: 'Among all competitors, those whose particular conditions happen to be the most appropriate of those offered to the economic system for testing and adoption will be "selected" as survivors.' (pp. 67 f.) Thus, by apparently minor modifications of the standard economic account Alchian arrives at 'a vastly different analytical framework—one which is closely akin to the theory of biological evolution.' (p. 74) Within this framework we can give

> ... accounts for observed uniformity among survivors, derived from an evolutionary, adopting, competitive system employing a criterion of survival, which can operate independently of individual motivations. (Alchian 1950/93, 73)

The framework for developing such explanations is in principle independent of the biological analogy (Alchian, 1953) but it is by means of this analogy that it gets its appeal (Friedman, 1953) as well as its adversaries (Penrose, 1952). By suggesting that the 'economic counterparts of genetic heredity, mutations, and natural selection are imitation, innovation, and positive profits' (Alchian, 1950/93, 74), whole trains of thought are started in interested readers. But the really interested reader soon runs into trouble which primarily relates to the fact that there is no real analogy between 'genetic heredity' and 'imitation'. The introduction of the possibility of 'imitation' makes the firms much more adaptable than they would be in a Darwinian process of 'survival of the fittest'. Actually, the firms may innovate and imitate more rapidly than the competitive process is selecting the 'fittest' and in this case the outcome will not be an evolutionary process but a state of behavioural chaos. They may also be so slow in changing relative to the functioning of the selection process that the system becomes characterised by a non-evolutionary stasis (only slightly disturbed by exogenous change). Unfortunately, Alchian does not demonstrate how long-term evolutionary processes may take place between the Scylla of behavioural chaos and the Charybdis of behavioural stasis. For this reason his programmatic announcement of an evolutionary renewal is radically incomplete and it might be reabsorbed in non-evolutionary modes of thinking.

These limitations of Alchian's approach suggest the premature character of Friedman's early attempt to accommodate neoclassical economics to Alchian's ideas (or *vice versa*) in his famous paper on 'The Methodology of Positive Economics' (1953). Here Friedman simply presupposes that the process of selection is strong and fast enough to remove the non-adapted types of behaviour:

> Let the apparent immediate determinant of business behavior be anything at all—habitual reaction, random chance, or whatnot. Whenever this determinant happens to lead to behavior consistent with the rational and informed maximization of returns, the business will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the addition of resources from outside. The process of 'natural selection' thus helps to validate the hypothesis [of profit maximization]—or, rather, given natural selection, acceptance of the hypothesis can be based largely on the judgement that it summarises appropriately the conditions for survival. (Friedman, 1953, 22)

This argument, which has convinced generations of economists, reflects that the verbal account of an evolutionary process easily leads to a neglect of central assumptions. Winter (1964, section II; 1971/93, section III; 1975, 96-99) has provided a list of some of the implicit assumptions beneath Friedman's argument which lead to serious questions—at least if one has Winter's realistic view of scientific explanation: Are we facing 'habitual reaction' which may allow the process of selection to take place or 'random chance, or whatnot' where we cannot assume that a successful mode of behaviour will be upheld? Can we assume that the firms with the right (profit-maximising) behaviour are the ones which actually expand? Is the process of variety-creation sufficiently strong to exclude long-term persistence of suboptimal modes of behaviour? How fast is the process of selection of the optimal behaviour?

To Friedman's instrumentalist view of scientific models, such questions do not appear to be particularly relevant since theories are not expected to be realistic, as long as they produce correct predictions about the future. However, from the viewpoint of the development of a new evolutionary economics they are quite central. Actually, they led Winter to the creation of much more satisfactory outlines of evolutionary economics based on the population perspective than Alchian had delivered. In these outlines (Winter, 1964; 1971/93) we see the beginnings of a synthesis between Alchian's study of the selection mechanism, Simon's work on behavioural routines and Schumpeter's ideas on innovative behaviour; this synthesis will be explored in chapter 4. But even today the problems raised by Alchian's introduction of population thinking into the realm of economic analysis have not been fully explored.

1.2. Surveying evolutionary economics[14]

1.2.1. Crude definitions

The difficulties of evolutionary-economic theories are, not least, due to their attempt to account for the *endogenous* transformation of the knowledge applied in economic systems. More specifically, the problems spring from the attempted explanation of the ever-changing diversity and adaptedness of the decision rules, commodities, production methods, and organisational forms of economic life. The endogenous explanation of these facts (the 'data' of standard neoclassical analysis) will necessarily focus on the mechanism of the evolutionary process: given an outcome of a supposedly evolutionary process, we try to point out the mechanism which has brought about this outcome or result. The necessity of this mechanism-oriented approach to explanations (Elster, 1983; 1989) is based on the fact that novelty plays an essential role in evolutionary processes. Shackle has emphasised that this characteristic of evolution excludes standard modes of scientific explanation:

> Whether evolution, irreversible, non-repetitive change, can in logic lend itself to analysis is open to doubt. Science depends upon regularities, upon repetitions of path, structure or association, and evolution in its long-range effects is the negation of repetitive

phenomena. If there can be a theory of evolution of any subject-matter, whether biological or social, it must lie in mechanism, in the basic principle which allows or directs an explicatory development. (Shackle, 1965, 187 f.)

From this viewpoint the important thing is to develop the tools for a realistic study of the mechanism of economic evolution and at the same time to shift away from the instrumentalist methodology of economics with its concentration on prediction of future outcomes and its neglect of realistic mechanisms (propagated by Friedman, 1953).

Evolutionary-economic studies have a synthetic character largely because the mechanism underlying economic evolution is very complex; this mechanism should be thought of as a synthesis between different (sub)mechanisms rather than as a single mechanism whose parts can be considered as black-boxes. A basic task is to show how an evolutionary process can be synthesised from these individual mechanisms. The major difficulty is due to the fact that the mechanisms are normally considered to be related to the domains of different scientific disciplines: the preservation and transmission of rules and norms have traditionally been related to sociology but are also studied by, e.g., institutional economics; the mechanisms of variety-creation are studied by psychology and social psychology, but also as interdisciplinary innovation studies which include a heavy economic component; the mechanisms of selection have especially been studied by standard economics, both as the competition between economic agents and as the agent's choice between different alternatives; the mechanism of segregation or closure is an area of industrial economic dynamics as well as of sociology, etc. The attempted integration of these diverse mechanisms in the study of evolutionary processes clearly represents an ambitious and risky synthesis. Representatives of the different specialities tend to emphasise 'their' mechanism at the cost of other mechanisms. But it is the synthesis between different theories rather than the contributions to the detailed understanding of the individual mechanisms which is the core factor of evolutionary economics. This combination of different mechanisms may even be taken as a definition:

> *An evolutionary-economic explanation* is an explanation of a fact of economic life by reference to previous facts as well as to a causal link which (immediately or in reconstructed form) may be shown to include
> (1) a mechanism of preservation and transmission,
> (2) a mechanism of variety-creation,
> (3) a mechanism of selection,
> and which includes or may be enhanced by introducing
> (4) a mechanism of segregation between different 'populations'.

The emergence of an evolutionary process presupposes that none of the individual mechanisms becomes too dominant. If preservation dominates, the result is a stasis of economic knowledge, while a dominance of variety-creation leads to non-deterministic chaos. We need certain assumptions about the economic agents in order to generate a system of such agents who can change their behaviour in an irreversible manner through a self-generated process. Such an assumption-oriented delineation of evolutionary economics tends to be more restrictive than the mechanism-oriented definition and we can only hope to reflect a limited part of the studies within the realm of evolutionary economics. Still, it is useful to produce a loose list of:[15]

Typical assumptions and characteristics of evolutionary-economic explanations

1. The agents (individuals and organisations) can never be 'perfectly informed' and they have (at best) to optimise locally rather than globally.
2. The decision-making of agents is normally bound to rules, norms and institutions.
3. Agents are to some extent able to imitate the rules of other agents, to learn for themselves and to create novelty.
4. The processes of imitation and innovation are characterised by significant degrees of cumulativeness and path dependency but they may be interrupted by occasional discontinuities.
5. The interactions between the agents are typically made in disequilibrium situations and the result is successes and failures of commodity variants and method variants as well as of agents.
6. The processes of change occurring in a context described by the above assumptions and characteristics are non-deterministic, open-ended and irreversible.

This list may look as if it has been constructed to contradict the standard neoclassical assumptions. At present it should, however, be seen as yet another indicator of the difficulties of evolutionary economics: it is extremely difficult to deduce mathematical results from such a basis. Instead the list emphasises the empirical orientation of evolutionary economics. But this empiricism cannot be upheld in all cases and the theoretical development of evolutionary economics will often presuppose much stricter and less realistic assumptions.

This list of assumptions and characteristics also raises another serious question: to what extent is it helpful to search for *typical* evolutionary explanations? If we apply population thinking to the subject of the development (or evolution) of evolutionary-economic theory itself, it may—at least in certain periods—be more helpful to emphasise the variety or diversity of evolutionary explanations. Even if the ultimate aim is to create a modern evolutionary-economic synthesis, any premature lock-in of the theoretical development may be very harmful. The quest for crude definitions is, however, unavoidable. In particular, there is a natural tendency to neglect the internal differences within evolutionary economics in order to make it easily comparable to dominant modes of economic analysis, especially neoclassical economics.

This tendency is clearly found in the presentation of the characteristics of evolutionary-economic explanation in contradistinction to neoclassical-economic explanation in Nelson and Winter (1982, chs. 1, 2, 3, 6, 8, 15 and *passim*). Their strategy of exposition helps to bring out some of the programmatic claims of evolutionary economics and it faces squarely the fact that the basic scientific training of most of the students and researchers in the area is based on neoclassical modes of explanation. However, the complexities of the comparison between evolutionary and neoclassical economics presuppose simplifications with respect to the two entities which are being compared. The most obvious simplification is the concentration on ‘textbook neoclassicism’ rather than ‘research-paper neoclassicism’ (see Winter, 1991) but more serious simplifications are based on the tendency to avoid the differences and difficulties within evolutionary economics in order to ease the comparison with neoclassical explanation.

1.2.2. On the modern history of evolutionary economics

The crowding out of evolutionary perspectives (which is reflected in Schumpeter’s ‘final thesis’) is an interesting but little studied aspect of the history of economic thought and analysis. This crowding out of ‘the Mecca of the economist’ was certainly not intended by Marshall (1898, 43),[16] as little as it was by Adam Smith, Marx, Menger, or Schumpeter.[17] On the contrary, they saw their work as first approximations to a fuller understanding of the evolution of economic life and they gave very diverse suggestions of how to approach economic evolution: Smith (1776/1922) gave hints about the interplay between division of labour, dynamic economies of scale and accumulation of capital (see Young, 1928; etc.); Menger (1871/1981; 1883/1985) insisted on dealing with difficult subjects like the changing quality and diversity of economic goods as well as the non-designed emergence of institutions like money through a process of trial-and-error (O’Driscoll, 1986); Marshall (1890/1961, book IV and appendix H) emphasised external and internal economies of the ‘representative firm’ in his long-term analysis of supply, which should later be enriched by an explicit analysis of the rise and fall of unequally endowed firms in their struggle for life (Thomas, 1991); Schumpeter (1912/34; 1939; 1942/87, part II) presented a dualistic vision and analysis of economic evolution as a struggle between on the one hand innovative entrepreneurs and bankers and on the other hand rule-based economic agents.

But the result of these uncoordinated suggestions was not the emergence of a more integrated approach. Instead the result was exactly opposite of their intentions, as if the economic profession was led by an ‘invisible hand’. Many of the attempts to eliminate the paradoxes, find the core arguments, clarify and axiomatise had as the first victims the hints about the evolutionary process, like increasing returns, representative firms, industries and commodity types, profit-creating entrepreneurship, etc.[18] In all cases the evolutionary attempts could be seen by main-stream economists as representing sources of confusion, as misfit parts of economic analysis; in the language of fairy tales they were the ugly ducklings in the yard of economics. The marginalist revolution, the Keynesian revolution and the post-war formalist revolution each have made their contribution to the crowding out of evolutionary-economic offspring. As a result, the multiform old evolutionary economics had largely disappeared in the first decades of the post-war period. Even the notion of 'evolutionary economics' which was first used by Veblen in his famous article 'Why is Economics Not an Evolutionary Science?' (1898/1961, 76 f., 79) was almost forgotten, except as a seldom used synonym for institutional economics in the tradition of Veblen and Commons.

The failure of many different openings toward the study of economic evolution cannot be blamed solely on external factors like the dominance of the neoclassical paradigm in the economics profession.[19] This kind of explanation of the crowding out tends to draw the attention away from fundamental difficulties which are intrinsic to such a study. Instead we should emphasise that such difficulties are probably sufficient for any scientific observer to cast serious doubts about the feasibility of an evolutionary-economic research programme. It will suffice to mention a few problems: the very limited predictability of the outcome of evolutionary processes which appears to block the falsification of evolutionary theories (Popper, 1972); the synthetic or impure character of the evolutionary mechanism which forces evolutionary-economic theories to transgress the borders of different social-science disciplines and creates an impression of eclecticism; the historical or empirical orientation of evolutionary explanation which hinders any clear-cut demarcation line toward business history and the do-it-yourself economics of practical economic life. Such difficulties are probably sufficient to explain the crowding out of evolutionary-economic perspectives. They relate to a mode of thinking which is difficult to learn, suggests messy 'dialectical' concepts (Georgescu-Roegen, 1971), blocks convenient modelling strategies, and blurs border-lines to other disciplines. Had it not been for the huge potential explanatory power of a successful theory of economic evolution, the difficulties would probably have been sufficient to scare away all researchers from the evolutionary field.

Today we see clear signs that the 'dark ages'[20] for the evolutionary perspectives are over. One sign is the still more frequent citing of the different types of old evolutionary economics (relating to Menger, Marshall, Veblen, Schumpeter, and others). Another sign is the rapid spread of economic studies emphasising the structural similarities between biological and economic analysis with a special emphasis on evolutionary issues (surveyed in Khalil, 1992; see also Rosser, 1992). A still more important sign is the emergence of studies of economic and related technological and institutional phenomena which are based on an explicit and clear concept of an evolutionary process, a concept which was not really present in the old evolutionary economics. Such studies may be classified as instances of what I shall call the emerging new evolutionary economics. In other words, the new evolutionary economics is here defined in a preliminary way by its application of an explicit and clear concept of an evolutionary process.[21]

The first applications in economics of a well-articulated idea of an evolutionary process are found in Alchian's (1950/93) and Winter's (1964; 1971/93) programmatic papers (see section 1.1.3), but the real change with its focus on evolutionary mechanisms has taken place in the last 10-15 years. A landmark in this respect was made when Nelson and Winter rewrote several earlier articles and formulated a research programme in their book on *An Evolutionary Theory of Economic Change* (1982).[22] This work showed the possibility and importance of a new start, although not all agreed with their particular strategy based on the assumption that the 'verbal account of economic evolution seems to translate naturally into a description of a Markov process—though one in a rather complicated state space.' (p. 19) But this way of formulating their synthesis made easy the next step, namely the translation of the Markov process to computer models and simulations which allowed a treatment of intricacies of evolutionary mechanisms not imagined by, e.g., Alchian. In this way Nelson and Winter succeeded in formulating a family of models where firms are not only 'naturally selected' by the economic system; the firms are also influencing their own destiny by modifying their own behaviour through processes of search for and selection of new modes of behaviour. Thus Nelson and Winter provided constructive proofs of the existence of relatively interesting evolutionary-economic models. However, very few researchers have followed Nelson and Winter's work directly. (See chapter 4)

Another beginning which is more important in relation to general economics is related to new developments in relation to game theory. Models which appear to be well-suited for the study of evolutionary processes have been created, e.g. in the form of repeated games of players with limited information and calculating capabilities, with procedural behaviour and learning from past experience, etc. (see Aumann, 1987, 468 f., 478; Kreps, 1990a, ch. 6) In this area there has been an interaction between economic and biological applications, not least relating to the concept of an evolutionary stable strategy, i.e. a strategy which when dominant in a population cannot be outcompeted by an alternative strategy (Smith, 1982). We now have an important literature on the properties of evolutionary games in economics (Sugden, 1989/93; Friedman, 1991). However, one of the most innovative examples of how to apply iterated games in both theoretical and experimental ways is found in Hirschleifer (1982/93; Hirschleifer and

Coll, 1988; 1992) and especially in Axelrod's work on *The Evolution of Cooperation* (1984; Axelrod and Dion, 1988). In the latter case the different strategies are tested in an iterated Prisoner's Dilemma while they are generated either by different game theorists (who participate in computer tournaments) or by the computer itself by means of so-called genetic algorithms. (See chapter 5)

An important issue in the analysis of evolutionary-economic processes is how history matters (path dependency). That this is the case appears obvious for any consideration of the facts of economic evolution. But it is not easy to treat this intuition systematically. Attempts to do so have especially been made by Arthur (1988; 1993; Arthur *et al.*, 1987/93) and David (1985/93; 1993). They demonstrate by means of stochastic processes how the development of a system with an initial variety of possibilities can show a lock-in which blocks further adaptation. The empirical standard example is the process which led to the emergence of the QWERTY keyboard even if for decades it has been known to be a suboptimal solution to the typing problem. More generally, the evolutionary process is seen as taking place in a 'landscape' where it is bound to find locally rather than globally optimal solutions. This result seems to substantiate the idea of an evolutionary process which normally follows the trajectories created by the constraints of given paradigms but which occasionally shows more basic alternatives (see sections 2.5-2.7).

Another issue is how to treat the extremely complex coevolution of different commodities, production methods or institutions. Here it is necessary to have a more distant approach to evolutionary processes than in the case where every economic decision is treated explicitly. However, in the new evolutionary economics there has been a tendency to have too much evolution and too little 'ecology', too much variety and too little structure. Instead the interaction has often been left to non-evolutionary economists. The first step to change this situation has been to reconsider the huge literature on the diffusion of innovations within an evolutionary framework (Metcalfe, 1988). The next step is to deal with several interacting populations which are partly competing, partly oriented towards different static or dynamically changing 'ecological niches'. Here the work has hitherto been dominated by business economists and organisation theorists (like Hannan and Freeman, 1989). The problem at this level of analysis is not only the complexity of the interactions but also the lack of economic theories on commodity types and other 'meso-economic' concepts. Given such a theory, there is a possibility transforming the results of modern evolutionary-ecological biology in a way which helps to systematise older evolutionary-economic ideas of growth poles and development blocks (Dahmén, 1950/70; 1988) and recent ideas of national systems of innovation (McKelvey, 1991; Lundvall, 1992; Nelson, 1993). (See chapter 3)

Many other studies of relevance to the emergence of new evolutionary economics could be cited. First, we find studies emphasising structural similarities between biological and economic analysis. Such studies help to transfer to economics the know-how of coping with the general methodological difficulties of evolutionary studies. Especially, they help to explore the surprisingly difficult population perspective (and the related heterogeneity principle) which underlies much of evolutionary economics. Second, there exists a huge empirically oriented literature on the economics of technological and institutional change, which is more or less clearly related to the new evolutionary economics. Basically, evolutionary economics is an empirical science which deals with the intellectual reconstruction of the real mechanisms of economic evolution; its models must be explored and developed in interaction with well-defined areas of empirical investigation. Unfortunately, the work on *Technical Change and Economic Theory* (Dosi *et al.*, 1988) is unique in the way in which it brings together empirical and 'grounded theoretical' work with the more abstract attempts to develop new evolutionary economics. Third, there is the rapidly expanding literature on Menger, Hayek, Schumpeter, and others. Especially, the concept of an evolutionary process is used to give a new account for the history of evolutionary-economic theory in retrospect (to paraphrase the expression of Blaug, 1962/85). All these studies are important. However, the emphasis of the present progress report on the conceptual and formal tools reflects a central tendency in the literatures as well as a central thesis of the present book.

Seen from a modern point of view the major problem with the old evolutionary synthesis is that it is built of out-dated elements and that it is so complex that it is nearly impossible to master intellectually. We see, e.g., that 'the intellectual coherence and power of thinking about Schumpeterian competition have been quite low, as one would expect in the absence of a well-defined theoretical structure to guide and connect research.' (Nelson and Winter, 1982, 29) The research agenda for the new evolutionary economics has been dominated by a search for such a 'well-defined theoretical structure' and thus by a wish to overcome the short-comings of the old evolutionary economics. The introduction of such systematic reconstructions of evolutionary processes helps to bring the latent ideas of the old evolutionary economics into clear consciousness and thus to overcome some of its intrinsic problems. The relatively approach is, so to say, acting as a midwife for the birth of an evolutionary paradigm in economics (and other social sciences)—especially by facilitating the clear articulation of the surprisingly difficult concept of an evolutionary process.

This thesis about the central role of a relatively formal approach to the study of economic evolution is apparently diametrically opposed to the lesson from the history of the crowding out of evolutionary perspectives; experience suggests that a further formalisation may tend to close the niches for alternative thinking. At best one might suspect that the resulting new evolutionary economics will be a highly distorted version of the pre-paradigmatic ideas of the old evolutionary economics. The result would be yet another form of crowding out, this time of the ‘magnificent dynamics’ (Baumol, 1951/70, 8 f., 21) of economic evolution as studied by Schumpeter and others; they would be substituted by limited and boring forms of evolutionary analysis. The present book demonstrates that there are good reasons for this expectation but it also shows that the necessary but painful transformation to a paradigm-based new evolutionary economics may be eased by tools which mediate between, on the one hand, the informal and empirical approaches and on the other hand the fully mathematicised analysis of evolutionary processes. Actually, some of the studies which have been cited represent such a mediation rather than a fully formal approach. They are more or less representative of a flexible algorithmic approach to evolutionary economics.

However, even in the presence of much theoretical flexibility, it is a major problem that the relative success of the new evolutionary economics has, until now, been obtained by redefining the goals of evolutionary economics in a way which tends to increase its isolation from historians and policy makers (David, 1993). In this sense, much of the evolutionary-economic work has some similarities with the mathematical modelling of biological evolution in the 1930s, which was only understood by very few researchers but nevertheless became a major element of modern evolutionary-biological explanation. In this comparison we are still waiting for something similar to the ‘modern evolutionary synthesis’ between practically oriented naturalists and theoretically-experimentally oriented geneticists (as described in Mayr and Provine, 1980). It is one of my central theses that the recent evolutionary-economic modelling may be developed into a part of a similar overall, modern synthesis between descriptive and theoretical studies of the diversity and adaptiveness of economic life. The algorithmic rethinking of major contributions to the new evolutionary economics should be seen as stepping stones on the way to such a modern synthesis. In this perspective is important that the ‘dialogue’ between new and old evolutionary economics is not necessarily bound to take place at a theoretical level. The old evolutionary economics was to a large extent an empirically oriented endeavour (see, e.g., Marshall, 1919; Schumpeter, 1939). Thus the ‘dialogue’ may also help to create a balance and a fruitful but difficult interaction between theoretical and historically oriented work and thus to promote a modern evolutionary-economic synthesis.