

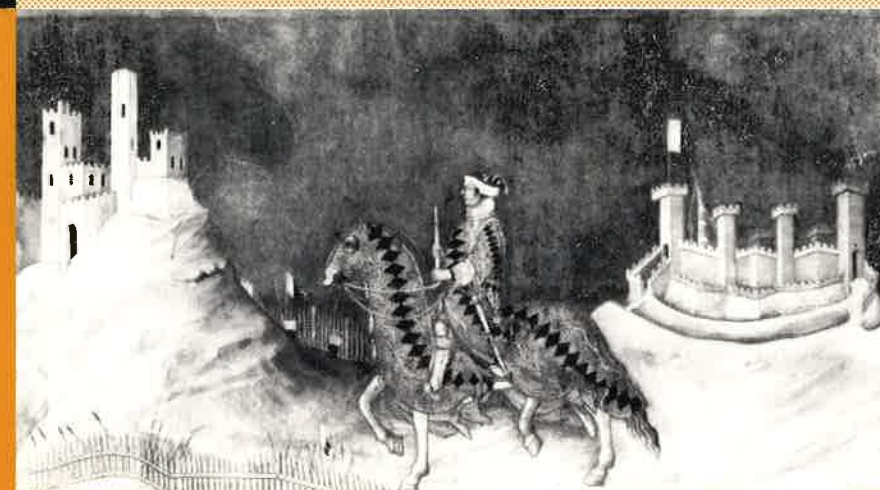
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QUADERNI DELL'ISTITUTO DI ECONOMIA

Massimo Di Matteo

GOODWIN AND THE EVOLUTION
OF A CAPITALISTIC ECONOMY :
AN AFTERTHOUGHT



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Goodwin and the Evolution of a Capitalistic Economy: an Afterthought.¹

I take this occasion as an opportunity to reflect upon what I have been learning from Richard Goodwin over the last few years since he has been associated with the Institute of Economics in Siena. I am interested in economic dynamics and therefore I will limit myself to say something on this topic though I am well aware that Richard's interests are much wider and that I could have learned much more. I will expose some reflections that came to my mind when reading again, on a recent occasion, Harrod's The Trade Cycle and Richard's papers on the growth cycle and on the multiplier-accelerator models (see Goodwin (1982)).

1. It is well known that Harrod was Richard's supervisor in Oxford exactly at the time when Harrod himself was elaborating his dynamisation of the General Theory as a part of his research program (as one would say nowadays) on the evolution of a capitalistic economy. I must say that Richard has been one of the few economists to interpret Harrod's thought correctly at least on the whole. I was led by him to think that Harrod had not a theory of cycles and a theory of growth but that he had a general intuition of the complex dynamics of the system.

The idea is exactly that the system has an endogenous movement but that this movement is of a cyclical nature. It is not correct to dichotomize Harrod into two branches, cycle theory and growth theory. I admit that Harrod's ideas were expressed in a way a bit obscure, cumbersome and sometimes involved: that is why many people got confused by him.

¹ The paper reflects the remarks I made in the discussion which developed at the workshop in honour of Richard Goodwin held in Modena, february 1986. Financial help from the Italian Ministry of Education (fondi 60%) is gratefully acknowledged.

When preparing a set of lectures on cycle theory I went back to Harrod's book and to his later paper (see Harrod (1936) and (1939)) I discovered that Richard's interpretation was fundamentally right. There is a unified vision of the working of the system though there are several ways of treating analytically such a complex phenomenon. In particular there is a certain degree of variability in Harrod's thought over the years but there is also a continuity of analysis between his first book and his last one (see Harrod (1973)).

The basic idea underlying his book is very simple, being the first application of the interrelation of multiplier analysis and accelerator principle to cycle theory. However The Trade Cycle is seldom recognized as the pioneer in the field and instead we have Samuelson's model (1939).

There is a point however where I part company with Richard's interpretation and concerns the review by Tinbergen (1937) of Harrod's book (which is subscribed by Richard). Tinbergen states that Harrod's theory is incorrect since a first order linear model cannot give rise to oscillations. If one reads The Trade Cycle one sees immediately that, though the argument is not carried out in mathematical form, the explanation of the turning points is the result of two non-linearities and it is surprising that Tinbergen failed to see this. Indeed as both Kruger-Glombowsky (1982) and Medio (1979) shew, it is possible to formulate Harrod's argument in a way that it gives rise to a non linear differential equation of the second order which has at least a limit cycle. This is more satisfactory than Samuelson's solution which is a second order linear equation and therefore cannot give rise to persistent oscillations without exploding.

The original argument is that the propensity to save rises in the upswing owing to two factors, namely the rise in the share of profits and, given the distribution of income, the rise in the level of income itself (in accordance to Keynes's ill-starred analysis in the third part of the General Theory). This exerts a deflationary effect on the expansion and is the explanation of the upper turning point. The lower turning point is treated in a symmetric way. It is interesting to note that the procyclical behaviour of profits is due to the introduction of imperfect competition, i.e. of a decreasing demand curve for each firm. It is more difficult to follow the argument with respect to the

movements of the capital-output ratio but the above argument is sufficient to endow the model with self-sustaining cycles.

Certainly Harrod's vision was rather different from that of Richard's in many respects. (For a fuller discussion of Harrod's thought on this topic see Di Matteo (1987)). First of all Richard's vision of the multiplier-accelerator mechanism relies essentially on the variability of the investment expenditure rather than savings and, in this respect, may be seen as a complement to Harrod's explanation. But secondly, and more important, Harrod thought that the expectation of a growing demand was sufficient to explain growth whereas Richard wants to take into account not only endogenous factors but also partly exogenous factors such as innovations. And this is no wonder since the influence of Schumpeter on Richard's thought needs no emphasis.

2. Harrod was not interested in the changing structure of the economy as the result and the spring of the development process so that he analyzed in a symmetric way the two turning points as I said before and paid attention only to aggregate variables. Moreover the explanation he put forward in The Trade Cycle involves unrealistic values of parameters.

Richard on the other hand has always been seriously worried by the lack of a convincing explanation of the lower turning point since there is not at the bottom such an obvious barrier as full employment is at the top. By convincing I mean both consistent and realistic. The way in which Richard is reconsidering the problem in recent times is to eliminate the difficulty altogether by having cyclical models with just one built-in non linearity.

One of the consequences of this new approach lies in my opinion in the appearance of a certain degree of indeterminacy which makes the reintroduction of historical peculiarities into economic analysis possible and necessary. This is a point that has been often emphasized but in essence forgotten. Therefore I think that it could be useful to look at the historical experiences to see whether there is something in common among different situations in different countries over the past decades. What I am thinking of is not a piece of economic history which would be a presumptuous enterprise by an economist, but rather a piece of economic history *raisonne* which tries to discover the mechanisms that

operate in the lower turning points. The idea is that at the bottom major structural changes are taking place of a nature which makes the lower turning point wholly different in character from the upper turning point. Can one operate a *reductio ad unum* of this structural changes, for example are they due essentially to major technical and/or organizational innovations? And how these are related to the economic environment? And is there a uniform modification of the behaviour of the principal agents? For example some people think that in different countries the financial structure of the economy undergoes roughly uniform changes in the upswing and in the downswing respectively and that in these modifications are the causes of the cycle to be found.

3. The last point I would like to make concerns the celebrated, and rightly so, paper on growth cycles (see Goodwin 1982)). I become more and more convinced of the relevance of that paper especially in recent times when I discovered what, to the best of my knowledge, is the first application of the Lotka-Volterra equations to economic analysis. Through my friend Roberto Scazzieri I came across a book, Introduzione allo studio della dinamica economica by Giuseppe Palomba (a student of prof. Amoroso) published in 1939.

In this book the author endeavours to show that a self sustained cycle can occur as a result of the interdependence between capital goods and consumer goods. This holds true for any value of the parameters which however, according to Palomba, could be some unspecified function of time. What a difference with the implications Richard saw in the original Lotka-Volterra model as a metaphor of the struggle between capital and labour in the capitalistic economy!

Let us have a brief look at Palomba's model. He builds up a two sector model (consumer and capital goods) and assumes that consumer goods will grow at a rate e_1 (which may depend on long term forces like productivity and labour force growth: Palomba does not specify them but this seems a sensible interpretation). On the other hand capital goods depreciate at a rate e_2 . The latter will be zero in a finite amount of time unless we make additional assumptions. Palomba stipulates that part of the consumer goods output could be allocated to the capital goods sector so that consumer goods will diminish at a rate g_1 and at the same time, through this channel, capital goods can grow at a rate

g_2 . This being the model we have a pair of Lotka-Volterra equations in the variables C_1 and C_2 which represent consumer and capital goods respectively in Palomba's notation. Therefore, for any given initial conditions (unless we are on the centre), there will be a cycle around the stationary solution which is given by e_2/g_2 and e_1/g_1 for C_1 and C_2 respectively. The solution does not imply that net investment stays constant that is what is meant by a stationary state (this of course would be true if $e_1/g_1 = e_2$) but that cycles occur around either a stationary, or a progressive or even a regressive state. It is clear that I am abstracting from population growth in which case the model can be reinterpreted in per capita terms. As one can see the model is rather peculiar, to say the least, since e_1 and g_1 are not levels, but percentages.

4. Reading that part of Palomba's book however induced me to explore more carefully the analogy between the economic system and the biological or ecological world. As it is well known political economy has been borrowing from physics (or parts of it) for a long time. And certainly one cannot say, nor can I, that this has been wrong or unfruitful. Even Marshall who, according to Keynes, was a great admirer of Darwin and Spencer gave a tremendous impetus in the direction of using methods borrowed from physics. One should only think of the concept of equilibrium to be sure that economics did progress in this way.

But I would like to see also other ways explored such as the analogy between economics and biological sciences, whether they can be helpful especially in understanding the evolution of the economic system over time. There are several ways in which this suggestion can be implemented and Robin Matthews (1984) developed recently his views (not to mention Armen Alchian (1950)). In general the discussion concentrated on the profit maximisation criterium and its validity in real life where defective knowledge is the rule rather than the exception.

It may be interesting to deal briefly with this interpretation in order to clear up some points. The uncontroversial starting point is that maximisation requires complete knowledge, since it is not possible to choose the best action(s) if people do not know them and their

features. This remains true also when people do try to maximise ignoring that in fact they do not know enough to perform a successful maximisation.

Complete knowledge is also consistent with a risky situation according to Knight's definition (see Knight (1921)). Indeed in risky situations we can easily reformulate the maximisation hypothesis provided we know how to attach probabilities to each event. We leave aside the Keynesian point about the impossibility of talking of risk and not of uncertainty in real life.

But what does complete knowledge mean? For our purpose here it has to acquire a precise meaning. It is not sufficient that we know that a certain equation has one (or more than one) solution. We have to know also how to compute it and this can be very difficult indeed, even in our era of computers. In other words proofs of the existence which economists can provide and can leave them satisfied do not offer the complete knowledge we are looking for. This incidentally meets the objection raised long ago by Edith Penrose (1952) when she ironically stated that, if economists know what firms do not, they should be asked to run the firms themselves.

From what we said above it is clear that the two kinds of knowledge could be rather different, the economist being content with an abstract qualitative result, the latter being insufficient for the manager of a firm. Lack of complete knowledge may depend either on the impossibility of computing the best alternative(s) or on uncertainty i.e. from the impossibility of appraising the effects of future events on the choices to be made today. In the latter case acquiring further knowledge may be almost impossible.

If this is the case firms have to choose according to some other criterium which has been the subject of endless investigations by Simon (e.g. see Simon (1982) and others. It becomes now clear that what would have been chosen in one stroke by the maximisation procedure has now to be performed through a process that leads to the elimination of those firms which chose wrongly. It is apparent that in this context the actual procedure chosen is immaterial, whether a routine or a random one.

It has been pointed out with great care by Robin Matthews that the adoption of a procedure other than the maximisation may lead to

results that differ greatly from the results we achieve if the maximisation hypothesis is adhered to. And in my opinion there are elements in the selection procedure of the social world that are different from the natural world and that therefore need careful analysis. In particular what I said so far seems rather appropriate for studying the effects of an exogenous change in the environment over which firms or agents have no influence. In fact this is not the only possibility.

It is a well known feature of any population that individuals do differ from one another under various respects and it is also clear that this differentiation is the vehicle through which selection operates in the face of a changing environment. In other words elements of the population best suited to the changed circumstances survive and others die. Therefore differentiation among firms (or *mutatis mutandis* among skilled workers) may be a crucial element in analysing the evolution of an economic system.

Following Marshall one can think that differences in the techniques used are an example of that but this is far from exhausting the possibilities of differentiation. In addition the organizational structure of the firm becomes an important element in the selection process since the flexibility of the firm is increasingly gaining relevance as far as the response of the firm to variation in market conditions is concerned. It has been shown (among others) by Williamson (1975) that the size of the firm is constrained by two mechanisms. The first is the exploitation of the economies of scale, the second one is the existence of transaction costs. The first tends to rise the average size, the second acts to reduce it since transactions costs are supposed to rise with the size itself. To this we can add a third factor i.e. the differentiation of the product that can work in the same direction as the second force.

As soon as we leave the perfect competition case we can consider differentiation either in an oligopolistic context or in a Chamberlin context. In the first case we have also differences between big firms and the fringe with various consequences on price and growth determinants. In the other one we can remark the following. Schumpeter was well aware that monopolistic competition was to be considered in his scheme (see Schumpeter (1939), chapt.2, sec.F) and that this was a

transitory phenomenon due to disappear during the process of imitation induced by the extra profit made by the innovators.

In this way the analogy with the natural process would be apparent, with the technical progress acting as the generator of differences within the population. But it is clear that the process of evolution would in the limit remove all the differences and this seems at odds with reality unless we postulate, contrary to Schumpeter, that technical progress is continuous. In both the oligopoly and the monopolistic competition cases the outcome of the process of the diffusion of innovation is strikingly different from that following from the competitive case.

Here the analogy with the natural world has to be taken with care because it is not at all clear that the differentiation of product leads to a better working of the selection process. It can be seen that the differentiation can be either a stimulating factor, a vehicle for introducing innovations or a retardation factor through which firms try to resist to the innovator by creating a segmented market.

This is only a sketch of the problems that I think are interesting to study in detail and as I said at the beginning have been prompted by my association with Richard Goodwin even if, in some respects, I departed from his approach emphasizing aspects whose importance he would probably dispute.

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