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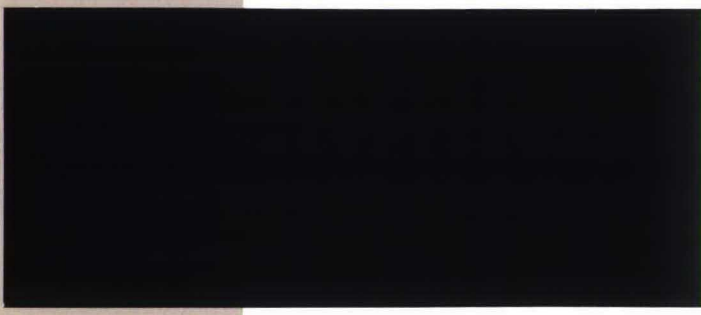
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**THE PREHISTORY OF  
RATIONAL EXPECTATIONS**

by Hugo A. Keuzenkamp

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# THE PREHISTORY OF RATIONAL EXPECTATIONS

Hugo A. KEUZENKAMP  
July 1989

(draft, comments welcome)

Center for Economic Research  
Tilburg University  
Box 90153  
5000 LE Tilburg, The Netherlands

## Abstract

In contrast to what is commonly believed, the first article on rational expectations was not written by Muth, neither did it appear in 1961. The true priority goes to a neglected and forgotten article by Tinbergen, published in 1932. The two articles have amazingly much in common. One similarity is that neither of them caused an immediate breakthrough in economics.

In this paper, I sketch the early developments of expectations analysis in economics, show how different research programs progressed or halted, and how different (technical as well as philosophical) problems hampered progress. The paper also describes in short some later developments, how different research programs converged to the Rational Expectations Revolution in Macroeconomics.

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## 1. Introduction

Revolutions rarely come unannounced. The October Revolution in 1918 Russia was preceded by the uprising in 1905; the Communist revolution in China had its roots in the Boxer war. The Industrial Revolution has its seeds in the agricultural changes of the late Middle Ages. Similarly, scientific revolutions have a history that normally start long before the decisive changes take place.

In Economics, the revolutions that people like to call revolutions are scarce. The marginalist revolution, the Keynesian revolution and recently the Rational Expectations revolution are the few examples one can give.

The *Rational Expectations Revolution In Macroeconomics* (see eg. Begg 1982) is a remarkable development in recent macroeconomic theory. Remarkable, firstly, because of the force with which this revolution changed macroeconomics in the seventies and eighties, and secondly, because the basic idea is so simple and natural that it is amazing that this Revolution didn't take place much earlier. It is on the question: why took it so long for this revolution to take off, that I will concentrate my analysis.

A short history of The Revolution usually starts with John Muth's *Rational Expectations and the Theory of Price Movements* (1961). This was basically a paper on microeconomic price theory. The step to macroeconomics was made roughly a decade later, by the 'revolutionaries' Lucas, Sargent and Rapping.

This history is well known and described in different studies (see eg. Begg (1982), or Klamer (1984)). It is much less known that a discussion on 'rational expectations' in macroeconomics existed long before the work of Lucas et. al., and even before Muth. This is one reason for writing a paper on the 'prehistory' of rational expectations. This study then may also shed some light on the reason for the rather slow take off of this revolution. Another reason is to see how different research programs of the thirties went along and integrated.

The paper is organised as follows. Section 2 gives a short description of the Interbellum literature on expectations formation. Section 3 presents the first rational expectations model, formulated by Tinbergen

in 1932. Section 4 goes into more detail to describe the contribution of early statistical research. Section 5 deals with the Austrian economists, as Morgenstern and Hayek. Section 6 pays attention to Keynes and his disciples. Section 7 sketches the steps to Muth, in particular the development in game theory and topology. Section 8 reads Muth's article in the light of the preceeding, and finally section 9 shows how progress led to the integration of different lines of thought that existed in the thirties.

## 2. Different Programs in the Thirties.

In the following sections I will describe different approaches to the analysis of expectations as they existed in the prewar years. Before studying each of them separately, it is useful to summarise them and sketching the context of these approaches.

Until the Second World War Vienna was a very important place for economics, with economists as Hayek, Mises and Morgenstern and philosophers like Popper setting the stage. Vienna was the place were the *anti-predictivists* were dominant. Particularly Morgenstern was active in attacking the idea that economics should be used for predicting the future.

This attack was partly directed to the use of so called barometers for prediction, or what Koopmans later dubbed the 'measurement without theory' approach. Statisticians at various places tried to predict stock market prices, but also analysed their own (lack of) success in this. Their tool was not yet multiple correlation, but in another context, i.e. estimating demand curves and the like, this technique started to be used. Tinbergen was the first and, until some thirty years later, only one who made the link between dynamic theory, expectations and uncertainty, and probability theory. This lead him naturally to a model of rational expectations, and raises two mysteries: firstly, why no one today knows of his contribution, secondly, why his contemporaries did not pick up his idea.

Then there is economic theory in England. The phrase 'it is all in

Marshall' does not really apply to the analysis of expectations in economic theory. As Shackle (1967, p. 6) notes, "Marshall, as always, was with the angels, but he did not blow this particular trumpet very loud". Perhaps the first effort among the English economists to deal explicitly with expectations was made by Keynes, in the *General Theory*. Apart from its well known importance as a book on disequilibrium economics, it contains interesting remarks on expectations, and particularly on the relation between uncertainty and disequilibrium.

Another economist who contributed to the literature on expectations and information was, after leaving Vienna, Hayek. His perspective was very different from Keynes's: the problem that kept Hayek busy was what he called the division of knowledge and its relation with *equilibrium* instead of disequilibrium. This he found much more intriguing than Smith's well known division of labour.

In the following I will first discuss Tinbergen's work. Next comes the statistical-empirical literature coming from agricultural economists and stock market analysts. Keynes's scattered theoretical remarks are analysed subsequently, followed by the works of Morgenstern and Hayek.

### 3. Tinbergen and The Introduction of Rational Expectations

One of the most remarkable episodes during the prehistory of rational expectations is Tinbergen's contribution: out of nothing he introduced by far the most sophisticated analysis of expectations formation of his time. His 1932 paper, *Ein Problem der Dynamik*, is the first that explicitly uses rational expectations, but it disappeared under the dust of history. The fact that the paper is written in German is one of the probable reasons why no attention was paid to this paper in the anglo-saxon literature. But also a paper in the first volume of *Econometrica*, in which some of the ideas in the German paper are repeated, did not shock the world of quantitative economists. The papers were completely forgotten in 1961, when Muth reinvented rational expectations.

Tinbergen started with the formulation of a stochastic dynamic optimization problem (his doctoral thesis dealt with optimizing problems

in physics and economics). Note that Bellman's Principle of Optimality was not yet invented, stochastic dynamic optimisation techniques were not available. New concepts were required, and Tinbergen made an effort to fill the lacune.

The first concept that Tinbergen introduced was the *time horizon* ('Gesichtsfeld') and a degree of time preference (Böhm-Bawerk's 'perspektivische Verkleinerung'). In principle, Tinbergen agreed, the time horizon could be unbounded, but for simplicity, a finite horizon was assumed, denoted by  $\tau$ .

The second important concept was *expectations* ('Erwartungen'). Of course, this concept was not new, but the way Tinbergen formulated it certainly was. As Tinbergen noted, in a dynamic problem there is the inherent problem that some future variables are unknown to the economic agents. Therefore, they need to form expectations on, e.g., future prices and harvests. For simplicity, Tinbergen assumed that these expectations are equal among different individuals. The next, most interesting assumption, is that these expectations are *rational* ('vernünftig'):

"The in my opinion essential characteristic of 'expectations' is not yet eliminated: that is that they don't have to become reality when new facts that were, and had to be, unknown until that moment, have an influence on these expectations. Therefore, we will go a step further, and assume also, that these expectations are 'rational', i.e. are consistent with the economic relationships" (1932, p. 172; the latter sentence reads in the original: "Wir werden sogar noch einer Schritt weiter gehen und auch annehmen, daß die Erwartungen 'vernünftig' sind, d.h. mit den wirtschaftlichen Zusammenhängen übereinstimmen.").

Compare Muth: "I should like to suggest that expectations, since they are informed predictions of future events, are essentially the same as the predictions of the relevant economic theory. (...) we call such expectations 'rational'" (Muth 1961, p. 4). Tinbergen continues:

In certain cases - which probably will be the most fruitful ones for analysis - one can replace these 'expectations' by economic-theoretical deductions (*durch wirtschaftstheoretischen Deduktion*), certain constants or real variables. For example, in case of a random variable, the rational expectation is the mathematical expectation (*so ist die vernünftige Erwartung die mathematische*) and therefore a certain constant. Another example is a variable that is the realisation of a certain law, to some degree of approximation. This expectation can be replaced by a series, in which the current value of the variable and its derivatives with respect to time are used." (p. 172)

And compare Muth again: "The hypothesis can be rephrased a little more precisely as follows: that expectatons of firms (or, more generally, the subjective probability distribution of outcomes) tend to be distributed, for the same information set, about prediction of the theory (or the 'objective' probability distribution of outcomes)." (Muth, op.cit. p. 5).

The language used by Tinbergen remarkably resembles that of Muth. Especially if the *Wirtschaftstheoretischen Deduktion* is translated by 'the relevant economic theory', which is obviously what is meant by Tinbergen.

Before being able to introduce his model, he needed a third concept: lags (*Verzögerungen*) coming from natural and technical factors. The problem analysed in Tinbergen's simple model then takes the following form. An individual makes an economic *plan* for his demands and supplies, given (1) a utility or profit function, (2) the time horizon, and (3), the price expectations. The plans of individuals can be conflicting or mutually inconsistent. This results in excess demand vectors,  $X(p)$ . The equilibrium condition for period  $t$  is defined as  $X_t(p_t) = 0$ , i.e. excess demands are zero. Therefore, equilibrium *realisations* are mutually consistent.

Tinbergen applied this framework to a simple mathematical model of production and inventory allocation, in which demand was deterministic (for simplicity), and supply stochastic. Again compare the analysis of Muth: his (Muth's) notation is somewhat more pleasant, but the model is nearly identical. Demand is non-stochastic, supply contains an expectatoinal and a random component, market equilibrium is determined by an equality between demand and supply. Muth analyses a model with and without inventory speculation, a slight extension compared to Tinbergen who didn't discuss the model without inventory speculation. To further simplify the problem, Tinbergen restricted the time horizon to two periods, but later on in the article this was generalised to any horizon.

The conclusions Tinbergen draw from his model with rational expectations do not in any way foreshadow modern insights, such as the differences between anticipated versus unanticipated shocks, Lucas's critique etc. The article did not differ from Muth's in this. But his empirical illustration, presented in the beginning of his article, may have warranted Tinbergen's feeling that current realisations are fairly good forecasts of future realised prices. This conclusion stems from



comparing future prices with realised prices. In 1933 an article in *Econometrica* appeared: *The Notion of Horizon and Expectancy in Dynamic Economics*. In this article, the model of 1932 is shortly repeated, but instead of 'rational', Tinbergen translated 'vernünftig' with the word 'reasonable'. A more or less unrelated section in this article deals with the relation between dividend paid and the 'worth' of stocks. The question that Tinbergen posed was on what factors dividend expectations depend. The conclusion is that "The chief determinant factor (...) is the last dividend paid" (p. 261). Tinbergen concludes that there no "forecasting quality" of stock prices. The results are close to the modern random walk hypothesis of stock prices (see Granger and Morgenstern 1970).

In his later work in building dynamic macroeconomic models Tinbergen never referred to his theory of rational expectations. Expected profit was an important factor in his empirical explanation for investment, but no use was made of a rational expectations theory to defend the use of profit and profit lagged as proxies for expected profit (see Tinbergen 1937, p. 25: "It could be asked whether profit expectations rather than past profits should be considered as determinants of investment. In principle this is no doubt correct, but it seems to me that the chief factors in expectations are the actual profits that have been made."). Other sources for dynamics in his models, such as natural and technological production lags (such as pig breeding or ship building) were analysed extensively in other contributions of Tinbergen, the total of these studies in dynamic issues lead Tinbergen to being confident using different lag structures in his macroeconomic models.

The impact of the article, and its idea, on his contemporaries remained low. This mystery asks for an explanation. One such explanation is that quantitative economists were still hard to find, and few of them read German. There was, however, an English version of some of the notions in *Ein Problem der Dynamik* in *Econometrica*: i.e. the 1933 article mentioned above, but what there was called 'reasonable expectations' was only one issue out of a whole range discussed by Tinbergen, and anyway the analysis was less general. No mention was made of 'mathematical expectation', for example.

Furthermore, apparently the jump from probability theory to economic

expectations and forecasting was too big for Tinbergen's contemporaries. First of all, even among statistical researchers, such as those that will be analysed below, probability theory was a relatively obscure topic. Diagrams and the like were much more generally used than multiple correlation. Those who estimated demand functions were not sufficiently interested in dynamic economics to follow Tinbergen: their own research program was flourishing and interesting enough. And finally, someone like Keynes, who was very familiar with probability theory (although not as much with multiple correlation) objected to the use of mathematical expectations for economic forecasts (see also below). Keynes (as well as Morgenstern) had a fair point: how should one deal with expectations of expectations, and the problem of infinite regress. It was not until the development of game theory, decision theory and the advances in classical and Bayesian probability theory that one could start to provide solutions to such questions. Tinbergen, in some sense, was ahead of his time. He was aware of Morgenstern's critique on economic forecasting (viz. a review, in Dutch, of Morgenstern's book *Wirtschaftsprognose* (1928) in *De Economist*, 1929), but remained unconvinced by Morgenstern's critique thanks to a rather pragmatic attitude. Thirty years later, Muth neglected this issue as well. The difference with Tinbergen is that he could have known better.

#### 4. The Statisticians

During the prewar years statistical research in economics started to become increasingly important. One of the most interesting research areas was expectations, and the first statisticians made an effort to use their statistical methods for economic forecasts.

Among those statisticians there were two groups that can be distinguished. First of all the agricultural ones, who among other things (as estimating demand functions) were interested in analysing the dynamics of agricultural prices. The hog cycle (or pig cycle, and related, the cobweb model) was the key issue for these empirical economists.

The second group consisted of stock market analysts. Good forecasts

could be used for stock market trading, and in fact *Econometrica* gained its existence in 1932 because of the anxiety of the President of an investment counseling firm to improve his predictions of stock market prices (this man was, of course, Alfred Cowles, see Hildreth (1986)).

Interestingly enough, neither of these groups applied *probability theory* to their investigations. Statistics meant presenting data, sometimes summarizing them in 'barometers' and diagrams, but the bridge to probability theory, as it was first built by Tinbergen, remained unused.

An example of the first line of research, the agricultural, is that of Coase and Fowler, who analysed the pig-cycle in Britian. Hanau (1930) did similar work for Germany, and in 1931 Tinbergen analysed a shipbuilding cycle. The pig-cycle was popular among agricultural economists to explain the movements in agricultural prices, in other words, to study dynamics. Coase and Fowler however the implications of the cobweb theorem more carefully than was done before. They rejected the view, "commonly held by agricultural economists (...) that farmers assume that present prices and costs will continue unchanged in the future" (1937, p. 55). A major reason for rejecting the cobweb theorem is that "if farmers acted according to the assumptions of the 'cobweb theorem', the technical conditions are such that a cycle of about two years would arise" (1937, p. 77), whereas the observed cycle was much longer than two years, and furthermore, the period and amplitude of the cycle were found to be unstable.

Another critique they gave is that "if farmers acted in the way postulated by the 'cobweb theorem', the cycle of prices when it had arisen would persist even though the supply and demand curves remained unchanged." (1937, p. 79). This is not how farmers act: it would not be rational to do so (Coase and Fowler don't use the word rational, however). Their analysis was less formal than Muth's, but contains most of his points.

The explanation for the cycle given then by Coase and Fowler was:

"The cycle arises because of errors in forecasting on the part of farmers, and we have shown that it is impossible to find any simple and rigid relationship which will explain the course of the cycle. (...) Since farmers are slow to change their estimates of future prices and since, as we have already argued, farmers cannot be expected to estimate correctly, it follows that errors will persist over a period of some years. Apparently, however, there is a tendency for this period to shorten. One or both of two factors may be operative to bring this

about. One factor is that farmers may learn from experience and thus correct errors more quickly and tend to be more accurate in their forecasting. The other is, of course, that farmers who persistently make bad forecasts will make losses and will tend to turn to alternative occupations". (1937, p. 79).

But, in contrast to what modern rational expectations analysts like to think, "the rate at which farmers learn from experience (...) would appear to be a problem lying rather within the province of the psychologist than that of the economist" (p. 79). Would they have made a different statement, had they known of Tinbergen's effort to analyse expectations? Apparently, they were not familiar with his contribution (no reference is made, neither to the German, nor to the *Econometrica* article). Furthermore, they didn't seem to be very familiar with probability theory, which would have been a necessary condition for an own invention of a theory of rational expectations.

Another study of the cobweb cycle is Ezekiel (1938). In a certain sense this is a step back compared with Coase and Fowler: although Ezekiel referred to their work, he completely ignored their empirical and analytical criticism of the cobweb theorem. Ezekiel even extended the application of the cobweb theorem to macroeconomic analysis:

"Even under the conditions of pure competition and static demand and supply, there is thus no 'automatic self-regulating mechanism', which can provide full utilisation of resources. Unemployment, excess capacity, and the wasteful use of resources may occur even when all the competitive assumptions are fulfilled." (Ezekiel 1938, p. 279).

It is this kind of analysis that is rejected by the modern rational expectations economists, first by Muth. Again, we may wonder why Ezekiel did what he did, and didn't better. He certainly could have known: he referred to Coase and Fowler, the least thing he could have done is to tell why he didn't care about their criticism. Maybe the answer is simple: Ezekiel thought he made a point by showing that disequilibrium resulted from the cobweb. The fact that he observed disequilibrium in economics may have been a validation for his neglect of the earlier critique on the cobweb theorem. This explanation seems to be too simple, but it is hard to think of a better one.

A study that combined agriculture and the stock market, is Dow (1941) from the University of Manchester, who investigated the accuracy of

expectations by looking at the Liverpool cotton futures market.

Dow compared cotton future prices with their final realisations, and noted that both present and future prices are "determined by expectations, which are the best that can be made on the data available at the time" (Dow op.cit., p. 165; compare Muth, quoted above). He concluded that "In the Liverpool cotton market, speculation is not very successful in forecasting prices in the future. On the other hand, it is not completely wide of the mark; changes in expectations cause a readjustment of the system of prices in the market, and these readjustments tend on the whole be in the right direction rather than not." (Dow, op.cit., p. 171). Again a result that foreshadows current research on rational expectations and efficient markets, but again probability theory is not among the tools used by Dow.

Finally, there were the pure stock market analysts. One of them is Karsten, Director of Research of the Karsten Statistical Laboratory. He has written a handbook for the stockbroker: *Scientific Forecasting (its methods and application to practical business and to stock market operations)*. In this book he mainly relied on the techniques of 'barometers' (also used by the NBER (see for a modern account Dominguez et.al., 1988)).

Karsten was fairly optimistic about his abilities to make prognoses, but, as he had to admit (1931, p. 23), "at the present time this investigation can not account for economic changes which are wholly due to the element of mob-, crowd- or herd-psychology". In general, however, Karsten's conclusion is that these psychological factors are relatively unimportant, which leads him to find reason to state that his results support "economic determinism". There is only one exception: the stock market (op.cit., p. 26). Of course it is not surprising that Karsten was moderate with claims about stock market prediction, so shortly after the generally unexpected crash of 1929.

Alfred Cowles contributed a paper to the first volume of *Econometrica*, in which he discussed the question "Can Stock Market Forecasters Forecast?". His answer was rather negative: financial consultancy firms did not forecast better on average than pure chance. Cowles thought that this provided a ground for more research in statistics and prediction, not that the effort would be logical impossible. So, he spend a good

amount of money to support the new Econometric Society, and his "Cowles Commission for Research in Economics".

The kind of work of Karsten and the NBER was vigorously attacked by Morgenstern. Also Popper, using a more philosophical approach, strongly criticised this kind of determinism (Popper 1957). It is to this critique that I will turn now.

## 5. The Austrians: Morgenstern and Hayek

The Austrian School of economics has some fame for its dismissal of the use of mathematics and statistics in economics. It is therefore maybe not surprising that one of the leading economists of Vienna during the thirties consistently criticised the use of statistical techniques for forecasting (by the way, some forty years later he wrote a book with Clive Granger on stock market forecasting, in which the random walk hypothesis of stock prices is reassessed).

The economist we are talking about is Oskar Morgenstern, professor at the University of Vienna and Director of the Austrian Institute for Trade Cycle Research. In his book *The Limits of Economics* (1937, p. 87), he wrote:

"(...) instead of developing economic statistics, aided by the eagerness of the present generation, in such a way as to derive permanent utility from them, the economists have gone to extremes and have betaken themselves to the dangerous field of economic prognosis. I myself was perhaps one of the first among the very few who protested sharply against this abuse of science."

Here Morgenstern referred to his *Wirtschaftsprognose* (1928). In that book, he discussed what Popper later called the 'Oedipus Effect' (Popper 1957, p. 13). This is the effect that a prophecy or forecast may have on the outcome: selffulfilling or selfdenying. According to Morgenstern, this effect is a decisive logical refutation of any aspirations of making economic forecasts. At best, no one listen to the forecast (in which case Morgenstern's critique is invalid, by the way), but at worst people do take into account the forecast, start speculating, and cause erratic and strong disturbances in the economic proces. Popper (also raised in Vienna!), for similar reasons, denied the possibility of social

prediction in his *Poverty of Historicism*.

This problem was certainly not unimportant. In fact, a refutation of the logical impossibility of prediction had to wait till after the war (see section 7, below). Morgenstern remained interested by the problem, this interest helped him in developing (with Von Neumann) game theory.

Morgenstern (1937) somewhat differs in its critique on predictivism from Morgenstern (1928). The latter emphasised the *logical* impossibility of forecasting, whereas (1937) seems to direct its criticism against the barometer or NBER methodology: the 'measurement without theory' approach. This seems apparant from the following quote:

"The most serious misuse which has been made of business cycle research are the attempts of 'scientific', detailed 'economic prognosis'. These have been made on the basis of an attitude which is antitheoretical and, consequently, entirely misguided as to the uses of statistics, and have rested on the appeal to a completely mistaken empiricism. It must be emphasized, to avoid misunderstanding at the outset, that the kind of prognosis to which objection is made does not of course include that which is implicitly made when any particular theorem is being applied to a concrete case under the strict assumption of *ceteris paribus*. This kind of prognosis takes place in every science that has any connection with empirical events; it raises no empirical problem as long as it proves possible to isolate the initial conditions and so long as there is assurance that the latter do actually remain constant throughout the period under consideration. In this case it is simply a question of the application of scientific analysis pure and simple: *this situation is the same for all sciences.*"

Another economist from Vienna was Friedrich Hayek. His most interesting contributions to the problem of expectations and information date however from his LSE period.

Hayek (1937) was interested in a problem analogous to Smith's division of labour: the *division of knowledge*. The latter concerns the problem of

"(...) how the spontaneous interaction of a number of people, each possessing only bits of knowledge, brings about a state of affairs in which prices correspond to costs, etc., and which could be brought about by deliberate direction only by somebody who possessed the combined knowledge of all those individuals." (op.cit., p. 49).

Hayek continued

"(...) instead of showing what bits of information the different persons must possess in order to bring about that result, we fall in effect back on the assumption that everybody knows everything and so evade any real solution of the problem."

Hayek was critical of economists who

"(...) stress only the need of knowledge of prices, apparently because - as a consequence of the confusions between objective and subjective data- the complete knowledge of the objective facts was taken for granted. In recent times even the knowledge of current prices has been taken so much for granted that the only connection in which the question of knowledge has been regarded as problematic has been the anticipation of future prices." (p. 49 - 50).

For Hayek, price expectations were just a minor part of the problem of knowledge, the major part being the general question of why the subjective data to the different persons correspond to the objective facts.

These questions are indeed the most interesting ones, and remain even today largely unresolved. Hayek made an important contribution by raising these issues, without even beginning to propose any solution. Morgenstern also was unable to go deeper than pointing to the basic problem of predictivism. The theory of games, to which he turned later, would prove to be of major importance in answering the problems that could not be solved in the thirties. Phelps's theory of islands set into motion a modern research program, heir of Hayek's analysis.

## 6. Keynes and his environment

According to the Post-Keynesian school of economic thought, Keynes's major contribution to economic theory was not the analysis of an economy with fixed prices (as mainstream neoclassical synthesis suggests), but his introduction of uncertainty into economics. Indeed, the *General Theory* contains many references to expectations. A number of these give the impression that Keynes already foreshadowed the Rational Expectations literature and the literature on what we call today 'speculative bubbles'. However, it is very hard to argue that Keynes really had more of a theory of expectations formation than, for example, Karsten. I won't argue that expectations were unimportant in the *General Theory* (as Haberler seems to think, see below), but rather that Keynes's theory of expectations amounts to the statement that such a theory is impossible.

Chapter 12 of the *General Theory* discusses 'The state of long term expectation'. It is full of quoteworthy remarks, I will try to restrain



myself. Of course, one has to start with the famous beauty contest:

"It is not a case of choosing those which, to the best of one's judgement, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees." Keynes 1936, p. 156)

The modern reader sees that Keynes tried to find a fixed point, or, tried to show that such a point was impossible to find in case of speculation. Even today, the literature on speculative bubbles does not provide an exhaustive solution to this problem, Keynes's worry certainly was warranted. But his analysis also deals with the non- speculative case. It is simply *human nature* that would invalidate the impossibility of a mathematical treatment of expectations:

"Even apart from the instability due to speculation, there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than on a mathematical expectation, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as a result of animal spirits - of a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities." (p. 161)

On the next page:

"In estimating the prospects of investment, we must have regard, therefore, to the nerves and hysteria and even the digestions and reactions to the weather of those upon whose spontaneous activity it largely depends.

"We should not conclude from this that everything depends on waves of irrational psychology. On the contrary, the state of long term expectation is often steady, and, even when it is not, the other factors exert their compensating effects. We are merely reminding ourselves that human decisions affecting the future, whether personal or political or economic, cannot depend on strict mathematical expectation, since the basis for making such calculations does not exist; and that it is our innate urge to activity which makes the wheels go round, our rational selves choosing between the alternatives as best as we are able, calculating where we can, but often falling back for our motive on whim or sentiment or chance." (p. 162-3).

Haberler (1937, p. 253) remarks that:

"Mr. Keynes has, of course, much to say on the formation of expectatons and the difficulties and limitations confronting any theory on this subject. But all this is contained in the wealth of remarks, observations and *obiter dicta* which - elaborating, supporting, illustrating and, at times, contradicting and blurring - surround the

main outline of his theory: the dynamic aspects do not penetrate the heart of his theory."

This view of Haberler is rather objectionable. Indeed, Keynes did not provide a theory of expectations formation, but Keynes suggests that this is even a logical impossibility. The way expectations do enter the heart of his theory is not affected by this. A reading of Keynes's QJE (1937) article, *The General Theory of Employment*, shows that this *fundamental* (not calculable) uncertainty of the future drives his theory of investment and liquidity preference, and therefore the whole *General Theory* (in fact, it is this problem that also is the ground reason for Keynes's objection to Tinbergen's econometric work). It is not the object of this paper, however, to enter the exegetical debate on the 'real' heart of Keynes's theory.

Keynes's hesitation with using probability for economic analysis dates back to the *Treatise on Probability* (1921). In this work, Keynes argued against a frequentist (or aleatory) theory of probability. Instead, he preferred an objective epistemic theory of probability, where probability relates to a degree of belief. From this he derived a *logical* theory of induction. A much more complicated matter was to formulate a *statistical* theory of induction, or inductive correlation. This struggle can be found in Part V of the *Treatise*, on The Foundations of Statistical Inference. Keynes kept on arguing that probabilities cannot be aleatory:

"Statistical technique tells us how to 'count' the cases when we are presented with complex material. It must not proceed also, except in the exceptional case where our evidence furnishes us from the outset with data of a particular kind, to turn its results into probabilities; not, at any rate, if we mean by probability a measure of rational belief." (Keynes 1921, p. 428).

At the end of the *Treatise*, Keynes acknowledges that if stable relative frequencies could be found, than there would be a "remarkable, if undeserved, justification of some of the methods of the traditional calculus of probabilities." (Keynes (1921), p. 468).

In the *General Theory*, Keynes hardly referred back to the *Treatise*. A former pupil of his however filled the gap, and in fact was closer to an analytical treatment of economic expectations than Keynes himself in the *General Theory*. This man was Hugh Townshend. Townshend, after taking a first in math in Cambridge (1912), had been a pupil of Keynes while preparing for civil service examinations in 1914. In the thirties he worked at the British Post Office, where he was involved in short and

medium term forecasting of the Post Office Telephone revenue (see his letter to Keynes in Keynes (1973, p. 247), and the note by the editor of the CW, Moggridge, on p. 235).

The published work of Townshend only consists of a few book reviews in the *Economic Journal* from 1937 to 1939, and one 'note' (really an article) in the *EJ* (1937). Apart from this note and reviews a number of letters from and to Keynes survive. More than Keynes did, he tried to find out whether it is possible to find some constructive theory of expectations formation, given the difficulties arising from the *Treatise on Probability*. A few hints appear in the *Economic Journal* book reviews.

In the review of Shackle's *Expectations, Investment and Income* (Townshend 1938), Townshend criticises Shackle:

"(...) if this is correct, it is fundamental - I do not feel sure that the treatment of an economic expectation as uniquely correlated with a value will stand logical analysis without modification. For example, Mr. Shackle's concept of 'equivalent certainties', ingenious and interesting as it is, seems to me to imply that the weight of evidence subjectively associated with a judgement of probability can be expressed numerically as a probability of a (numerical) probability. The logical objections to this view have been pointed out by Mr. Keynes in his *Treatise on Probability*." (Townshend 1938, p. 523).

The review of Hawtrey's *Capital and Employment* again provides a criticism in the Keynesian mode. First, Townshend describes how Hawtrey assumes that producers perfectly right decisions. Then:

"This, of course, is a condition of equilibrium, from which Mr. Hawtrey, following (in this at least) a tradition common to both the Cambridge and the Austrian schools, proceeds to develop a dynamic theory, 'taking account of disturbances' which clearly include those due to promoters failing to take perfectly right decisions. But it is logically necessary to any such theory that the position of equilibrium should be capable of existing. *If Mr. Keynes is right, there is no unique set of perfectly right decisions.* For the most profitable decision for one producer depends on what others, including consumers, do; and what they do depends on what he expects to happen. There is of course a 'right decision' for any one producer, in the sense of the course which will maximise his profits *if* the others (and the consumers) do what he expects them to do on the best information available to him. (...) The mechanical analogy (e.g. of Virtual Work, which Mr. Hawtrey cites from Jevons) breaks down; as it surely must, if prices are influenced, through-liquidity premium, by mere expectations. (If one must have a mechanical analogy, I suggest the case of the impossibility of applying the general Hamiltonian equations to solve a problem in dynamics where the system includes non-homologous forces!)."

In a letter to Keynes, dated November 25th, 1938, he again wondered whether it would be possible to surmount the logical problem of finding

numerical probabilities for fundamentally uncertain factors. The letter is a response to a letter of Keynes, 27 July 1938, in which Keynes writes:

"(...) a main point to which I would call your attention is that, on my theory of probability, the probabilities themselves, quite apart from their weight or value, are not numerical." (in Keynes 1979, p. 289).

The letter that Townshend writes is interesting from the beginning to the end, but its essence comes through in a few final paragraphs:

"This is the nearest I can get to an analysis of the part played by the factor of confidence in the rationale of interest. I believe that its further logical analysis at a deeper level of generalisation is connected with the part played by the weight of evidence in your theory of probability; but I cannot see just how. The connection in my mind comes about through the contrast with the conditions of the mathematical theory of probability appropriate to timeless games of chance, where in some way conditions *not indefinitely repeatable* are abstracted from. But this is probably unintelligible. (...) All this, of course, leaves open the question whether, as you suggest in your letter, it may not be possible to develop a logical doctrine of equivalent certainties free from the assumption of numerical probabilities, and perhaps of wider than economic application. I am only maintaining that this has not yet been done. It also leaves out of account the element of arbitrariness in judgements of probability, to which you refer. I think that this last, in its economic aspect, really implies a criticism, or at least calls for further analysis, of the basic concept of the economic man, defined as *determinately* motivated by (his) judgements of maximum (in some sense) anticipated profitability." (Townshend, in: Keynes (1979), p. 292-293).

In a final reaction, Keynes (1979, p. 294) writes:

"I think it important to emphasise the point that all this is not particularly an *economic* problem, but affects every rational choice concerning conduct where consequences enter into the rational calculation. Generally speaking, in making a decision we have before us a large number of alternatives, none of which is demonstrably more 'rational' than the others, in the sense that we can arrange in order of merit the sum aggregate of the benefits obtainable from the complete consequences of each. To avoid being in the position of Buridan's ass, we fall back, therefore, and necessarily do so, on motives of another kind, which are not 'rational' in the sense of being concerned with the evaluation of consequences, but are decided by habit, instinct, preference, desire, will, etc. All this is just as true of the non-economic as of the economic man."

Keynes, this is clear, would not easily accept a rational expectations hypothesis in economics. His pupil, Townshend, was a bit more open minded but he was unable to provide a solution to the logical problem of finding numerical probabilities of probabilities. Techniques were a major constraint to analysis in the thirties.

One point still is interesting to discuss. A constructive theory of expectations formation existed, i.e. Tinbergen's model. What would Keynes and Townshend have thought of this model? Neither of them ever referred to *Ein Problem der Dynamik*, or to the English article in *Econometrica*. Keynes's attitude to Tinbergen's econometric work is all too well known, however. He was horrified. He had a number of reasons for his objections, not all of them equally valid. One point, however, was fundamental, and can be related to his ideas in the *Treatise on Probability*. That is his doubt, cited above, that economic time series represented stable relative frequencies: "the main *prima facie* objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment" (Keynes 1973, p. 316). In a certain sense, Keynes even anticipated Lucas's critique, in accusing Tinbergen that he destroyed the tools of the economist. A model should be used to *change* the nature of the data, the economy, and therefore never can represent a part of a stable relative frequency.

Townshend was probably completely unaware of Tinbergen's work, as may be expected from an amateur economist. But it may be that Townshend would have been more impressed by tests (provided by Tinbergen in his econometric work) on stability of a model, and by the original rational expectations idea of Tinbergen. These notions remain entirely speculative, though.

## 7. Game Theory and a Fixed Point Theorem

Two developments, or new inventions, enabled the next generation of economists in their analysis of expectations. The first is due to Von Neumann and Morgenstern: *The Theory of Games and Economic Behaviour*, published in 1944 (Von Neumann already had an article on game theory, in German, in 1928). The second development, not completely unrelated, was the advance in mathematical techniques (many of them existed already for some time, but it took time before economists started to make use of them).

- Townsend, Hugh (1937), Review (of R.G. Hawtrey: *Capital and Employment*),  
*Economic Journal*, p. 321 - 326
- \_\_\_\_\_, (1938), Review (of G.L.S. Shackle: *Expectations, Investment and  
Income*), *Economic Journal*, p. 520 - 523

Tinbergen's: his use of probability theory is much more subtle, and he provides an interesting comparison of different dynamic models. His contribution remains immense. But, like Tinbergen, Muth ignored some important problems, on the other hand, there are other items that Muth did not neglect: for example by providing theoretical support to Coase and Fowler, and criticizing Ezekiel.

But now the points Muth neglected. Firstly, he made only one reference to Grunberg and Modigliani, by noting that "A 'public prediction', in the sense of Grunberg and Modigliani (1954), will have no substantial effect on the operation of the economic system" (1961, p. 5). Modigliani and Grunberg however *only* show that correct public prediction is possible if the possibility of correct private prediction is accepted (see Grunberg and Modigliani 1954, p. 478, where they also anticipate, in a footnote, Lucas's critique). Muth more or less took for granted that this does not give any problem, he was not bothered by any game theoretic problems. Also, Hayek's preoccupation with diffuse knowledge is not discussed. Of course, an exhaustive treatment of all these topics would have been far from feasible for one article by one person. An avalanche of papers was waiting, but first some years had to pass by. As McCloskey (1985) shows, the citations of the 1961 article numbered 5 in 1966, 3 in 1967, 2 in 1968 and 1969 and only then started to become more frequent. At that point of time, Lucas and his cooperators took over the banner and caused the final take off of the Rational Expectations Revolution.

## 9. Epilogue: The Convergence of Research Programs

In the preceding sections I sketched the developments in analysis of expectations from the '30s onwards. Different programs developed, but to a large degree they ignored each other. Tinbergen, in his somewhat technical and pragmatic approach, didn't pay attention to philosophical problems, such as the mere *possibility* of prediction and existence of rational expectations. This was a focal point for Keynes, who denied such a possibility. Also *learning*, something that attracted Hayek's attention, is a neglected issue in Tinbergen's work.

But conversely, neither Keynes nor Hayek refer to the empirical work as

done by the statisticians. Arguments in Coase and Fowler that refer to learning were ignored. Keynes's arguments do not show up in Hayek's work, neither seemed Keynes to be interested in the troubles of his Austrian colleague.

The introduction of new techniques made new developments possible. For example, economists started to become familiar with topology and game theory in the late forties. Savage (1954) gave an impulse to probability theory and decision under uncertainty. And other ideas and techniques became available. They were needed to solve some of the old problems. In this sense, Tinbergen was too early with his rational expectations. Muth appeared not much less ignorant than Tinbergen on issues like these, however. There is no reference to any game theoretical argument in his writing. Learning is not necessary in his model (similar to Tinbergen: the agents *know* the economic model).

The big advantage Muth had, compared with Tinbergen, is that he was not as much ahead of his time, for game theory, Bayesian learning and statistical decision theory developed quickly in the postwar years, and with some lags, entered economic theory. The conditions for the revolution were not ripe in 1932, they were hardly ripe in 1961, but they were ripe at about 1970. This is the moment that the different research programs were able to meet each other, and to start the cross fertilisation that resulted in models like presented in Frydman and Phelps (1983).

One issue, worth studying, remained undiscussed in this paper. How well does the history of rational expectations fit within notions of philosophers of science, such as Lakatos's Research Programs, progressivity, growth; or Kuhn's account of scientific revolutions? This question is worth a separate paper, that I hope to write in due time.



## References

- Begg, David K. H. (1982), *The Rational Expectations Revolution in Macroeconomics*, Philip Allan Publishers, Oxford
- Coase, R.H. and R.F. Fowler (1935), Bacon Production and the Pig-Cycle in Great Britain, *Economica*
- \_\_\_\_\_, (1937), The Pig Cycle in Great Britain: An Explanation, *Economica*, p. 55 -82
- Cowles, Alfred (1933), Can Stock Market Forecasters Forecast?, *Econometrica* 1, p. 309-324
- Dominguez, Kathryn M., Ray C. Fair and Matthew D. Shapiro, Forecasting the Depression: Harvard versus Yale, *American Economic Review* 78, p. 595 - 612
- Dow, J.C.R. (1941), The Inaccuracy of Expectations (A statistical study of the Liverpool cotton futures market, 1921/2 - 1937/8), *Economica*, p. 162 - 175
- Ezekiel, Mordecai (1938), The Cobweb Theorem, *Quarterly Journal of Economics* 52, p. 255-280
- Frydman, Roman and Edmund S. Phelps (1983), *Individual Forecasting and Aggregate Outcomes ("Rational Expectations Examined")*, Cambridge University Press
- Granger, Clive W.J. and Oskar Morgenstern (1970), *Predictability of Stock Market Prices*, Heath Lexington Books
- Grunberg, Emile and Franco Modigliani (1954), The Predictability of Social Events, *Journal of Political Economy* LXII, p. 465 - 478
- Haberler, Gottfried (1937), *Prosperity and Depression*, 4th edition, Atheneum 1963
- Hanau, Arthur (1930), *Die Prognose der Schweinepreise*, Sonderheft 18, Vierteljahrshefte zur Konjunkturforschung, Institut für Konjunkturforschung, Berlin
- Hayek, Friedrich A. von (1937), Economics and Knowledge, *Economica* p. 33 - 54
- Hildreth, Clifford (1986), *The Cowles Commission in Chicago, 1939-1955*, Springer Verlag, Berlin
- Karsten, Karl (1931), *Scientific Forecasting (its methods and application to practical business and to stock market operations)*, New York,

- Greenberg Inc.
- Keynes, John Maynard (1921), *A Treatise on Probability*, Vol. VIII of The Collected Writings of John Maynard Keynes, St. Martin's Press, New York
- \_\_\_\_\_, (1936), *The General Theory of Employment, Interest and Money*, Vol VII of The Collected Writings of John Maynard Keynes, MacMillan 1973
- \_\_\_\_\_, (1937), *The General Theory of Employment*, in *Quarterly Journal of Economics*, reprinted in Keynes (1973)
- \_\_\_\_\_, (1973), *The General Theory and After, Part II: Defense and Development*, Vol XIV of The Collected Writings of John Maynard Keynes, MacMillan
- \_\_\_\_\_, (1979), *The General Theory and After, A Supplement*, Vol XXIX of The Collected Writings of John Maynard Keynes, MacMillan
- Klamer, Arjo (1984), *The New Classical Macroeconomics (conversations with new classical economists and their opponents)*, Wheatsheaf Books Ltd.
- McCloskey, Donald N. (1985), *The Rhetoric of Economics*, University of Wisconsin Press
- Morgenstern, Oskar (1928), *Wirtschaftsprognose*
- \_\_\_\_\_, (1934), *Das Zeitmoment in der Wertlehre*, *Zeitschrift für Nationalökonomie*,
- \_\_\_\_\_, (1935), *Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht*, *Zeitschrift für Nationalökonomie*,
- \_\_\_\_\_, (1937), *The Limits of Economics*, W. Hodge and Co.
- Muth, John F. (1961), *Rational Expectations and the Theory of Price Movements*, *Econometrica* 29, p. 315 - 335, reprinted in Robert E. Lucas and Thomas J. Sargent (1981), *Rational Expectations and Econometric Practice*
- Popper, Karl (1957), *The Poverty of Historicism*, Routledge & Kegan Paul, 1984
- Savage, Leonard J. (1954), *The Foundations of Statistics*, Dover 1972
- Shackle, G.L.S. (1967), *The Years of High Theory (Invention and Tradition in Economic Thought 1926 - 1939)*, Cambridge University Press 1983
- Tinbergen, Jan (1932), *Ein Problem der Dynamik*, in: *Zeitschrift für Nationalökonomie, III. Bd., 2. H.*
- \_\_\_\_\_, (1933), *The Notions of Horizon and Expectancy in Dynamic Economics*, *Econometrica* 1, p. 247 - 264
- \_\_\_\_\_, (1937), *An Econometric Approach to Business Cycle Problems*, *Impasses Économiques*, Hermann & Cie, Éditeurs

- Townsend, Hugh (1937), Review (of R.G. Hawtrey: *Capital and Employment*),  
*Economic Journal*, p. 321 - 326
- \_\_\_\_\_, (1938), Review (of G.L.S. Shackle: *Expectations, Investment and  
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