

Expectational coordination failures and Market outcomes' volatility¹.

Roger Guesnerie,

Paris School of Economics et Collège de France.

¹ This text develops ideas that have been presented, in several occasions, and for example at the « conférence Jean Jacques Laffont » of the 2010 Congress of the l'Association Française de Science Economique, or at invited lectures, (in the series «food for thought : on a post mortem of the financial crisis “ of the European Bank of Investment and in the Columbia conference, “Micro-foundations for macroeconomics”). Related ideas have been presented and discussed in many seminars, workshops, in Marseille, Stanford, New Dehli, Bombay, Aix en Provence, Warwick.... I am grateful to participants for their comments and suggestions, and in particular to K. Binmore, M. Cingolani, P. Sen.

Résumé : La première partie de ce texte revient sur la vision économique standard de la coordination des anticipations, vision que les événements récents conduisent à remettre en cause. La seconde partie passe en revue les directions existantes de réflexion critique sur l'hypothèse d'anticipations rationnelles. La troisième partie montre comment l'évaluation critique de l'hypothèse, dans une logique parfois qualifiée de « divinatoire », change radicalement notre regard sur trois problèmes : le rôle économique de la spéculation, l'efficacité informationnelle des marchés, et last but not least, la capacité des agents à horizon long à anticiper l'avenir. La quatrième partie souligne les acquis et les défis de la réflexion

Abstract : The first part of this text comes back on the standard economic viewpoint on expectational coordination, a viewpoint that the recent events have challenged. The second part reviews the existing directions for a critical assessment of the rational expectations hypothesis. The third part shows how such a critical assessment, along the lines of the so-called “eductive” learning approach, radically modifies our view of three key problems : the economic benefit of speculation, the informational efficiency of markets and, last but not least, the ability of long horizon agents to anticipate the future. The fourth part stresses the established basis as well as the future challenges of the discussed lines of thinking.

Codes JEL : E1, E30, E32, E6

Introduction.

The question of the stability of a market system has been a recurrent subject of debate since the beginning of the 19th century, (which is usually viewed as the starting point of economics as a field). “L’offre crée sa propre demande » (« supply creates its own demand »): this formula, a jewel of digest argument, is supposed to capture the essence of Jean Baptiste Say’s analysis (1803). It expresses the basis of the strong confidence of some of the early protagonists of the debate in the “systemic stability” of markets. But others strongly disagreed: Jean Baptiste Sismondi, and the « catastrophist » school, and its most famous adept, Karl Marx, were waiting for crisis and possibly the “final crisis” of the capitalist system. In the middle, Léon Walras thought that the argument provided by Say was unconvincing, but developed an alternative analysis supporting rather than dismissing the optimistic conclusion. The skepticism about the « systemic stability » of the market has reappeared after the 1929 crisis and Keynes forcefully argued for Government intervention to fight markets’ instability.

At the beginning of the present millennium, the mainstream economic theory had apparently fully rallied Say’s optimism. According to Robert Lucas, our knowledge had improved so that the « central problem of depression prevention has been solved ». Was not the « great moderation », as suggested by Ben Bernanke’s argument, a proof of the adequacy and efficiency of the present macroeconomic and monetary policies? But the argument reflected an appraisal of the facts that was intellectually and geographically biased (see Reinhart-Rogoff (2008)).... Facts, such as crisis, bubbles, are stubborn, and their stubbornness challenges the system of explanations that came to dominate. We have to admit: neither the volatility of financial markets, nor the logic of time of crisis is satisfactorily assessed in the best of our present models.

Many economists, not all, would agree that the recent crisis has raised questions about the present state of economic theory; a subset would probably agree that the reading of the events through the standard glasses of the dominant theories signals some kind of an “economic theory failure”. Going somewhat further, I belong to the circle of those who believe that economic theory has a central responsibility in the occurrence of the financial crisis, which has triggered the present economic crisis. The bottom line is that economists have provided a too optimistic view of the working of the financial markets, (the repeated emphasis put by many specialists on the « efficient market hypothesis » is a most spectacular illustration of this bias). Such an over-optimistic view has within a group - the practitioners of the financial sector, already prone to self satisfaction and reluctant to regulation - triggered the deployment of uncontrolled imagination. The previous acceptance of exaggeratedly optimistic conclusions, either in finance or macroeconomics, has reflected an uncritical acceptance of modelling principles, which were incorrectly given the status of unquestionable axioms. The question of what went wrong with the standard economic knowledge in general and with its

modelling principles in particular, is however likely to suggest a variety of answers. Let me sketch three of them.

1- The first one does not refer so much to modelling principles but rather to the diversification of modelling. There has been a multiplication of fronts in modern economic research, but the inside progress on each of the front is probably less and less understood from outside, even from neighbour subfields. I have argued elsewhere that the balkanisation of knowledge is more of a problem in an economic (or social science) context, where social action must rely on all dimensions of understanding, than in a scientific field like physics, where applications depend a lot on specifics. From my teaching experience, I have been struck with the fact that, within the field of finance, the communication between let us say the subfields of standard asset pricing, mathematical finance, “informational” finance, corporate finance, the econometrics of finance, etc...was most limited. The difficulty of a getting a synthetical view of the different fronts is however exacerbated when the question of systemic stability, not only of the financial system, but also of the economy as a whole, comes on the scene. Let me stop elaborating on the just sketched idea, not because it is uninteresting but because it brings us away from my main purpose here.

2- A second line of criticism concerns the rationality hypothesis, which has been a quasi-obliged ingredient of economic analysis in the past. Real economic agents do not have the supposed rationality of *homo-economicus*: Paul Krugman has made this point forcefully, for example in his New York Times chronicles. In fact, the fast development of behavioural economics in the last twenty years has stressed the limits of the standard concepts of rationality: behaviour under uncertainty, time consistency, symmetric treatment of losses and gains, to quote a few research chapters, have been the subject of critical reconsideration. Hence the just evoked criticism would not point so much to a failure to analyse the limits of standard rationality, but rather to a failure to focus on the consequences of the reconsideration, let us say in finance and macroeconomics².

3- The “economic theory’s failure” stressed in the present paper, which in my opinion is central, concerns expectational coordination. The oscillations of the assessment of expectational coordination in our models reflects major hesitations: either, as in most of the past before the 50’s, expectations are modelled as exogenous, (this is the viewpoint taken until recently in the temporary equilibrium approach to dynamics), or from the mid 60’s they have become more and more “right on average”, this is the essence of the rational expectations hypothesis, which is the main subject of discussion here. The first option, taking expectations as given, can be, at best, a preliminary step in a more complex analysis and markets expectations have to be explained and not taken as exogenous data. But the ability of decentralised systems such as markets to produce “good” coordination³, good in the sense of

² With noticeable exceptions such as Bolton-Scheinkman-Wiong (2006)

³ There are different senses in which rational expectations are “good”: first, agents are right on average and hence make decisions that are individually optimal, second, the social outcome is satisfactory, since for example, the “equilibrium of plans prices and price expectations” stressed in next section is Pareto optimal. In

the rational expectations hypothesis, has also to be explained. And it may or may not be possible to produce reasonable explanations. In the latter case, one has to conclude that markets are likely to fail to coordinate expectations in a satisfactory way, (satisfactory in the sense of the rational expectations hypothesis, but also, in many other senses of the word). As argued in the previous footnote, we are then faced with a potential “expectational coordination failure”, the analysis of which is the subject of the present paper.

The first part of the paper comes back very briefly on the post second world war history of economic thought. It recalls the rise of the Rational Expectations Hypothesis in the field of formalized economic theory.

The second part reviews the attempts at a critical assessment of the Rational Expectations Hypothesis that have developed particularly since the beginning of the eighties.

The third part of the paper will illustrate how a critical approach, and in particular the one associated with my own research, may trigger a drastic change in the understanding of problems, and in particular in the evaluation of economic policies. I will stress three examples, i.e. the economic role of speculation, the so-called informational efficiency of markets, and the ability of long horizon agents to predict the future.

The fourth part will conclude by coming back briefly on the presented analysis and stressing the future challenges.

1- The rise of the Rational Expectations Hypothesis in modern theory.

The rational expectations hypothesis, from now REH, says that economic agents have a view of the future which is “basically right”: this view may vary depending on individual information but it is not biased. Stressing that the hypothesis is distinct from the individual rationality hypothesis, is obvious but still worthwhile. The idea that “the rational expectations hypothesis is nothing else than an extension of the rationality hypothesis to expectations” has been a misleading but (unfortunately) extremely popular argument for the REH. In fact, let us say in a large economy⁴, it is “right” (individually rational) to adopt the REH, but only if the other agents adopt it. It is wrong in the other case. To put it in a game-theoretical vocabulary, the rational expectations equilibrium is a “Nash equilibrium”, not “a dominant strategy” equilibrium.

general, although “rational expectations” might not be second best optimal expectations, the failure of the rational expectations hypothesis signals an “expectational coordination failure” and in a sense, a market failure in the usual sense of the word (see however section 4, for a brief discussion of such a market failure).

⁴ I refer, for example, to the continuum of agents’ framework which serves as a basis to the analysis of Guesnerie-Jara-Moroni (2011)

The rise of the debate on expectations in economic theory can be dated from the fifties and the sixties. Although the pioneering article (Muth (1962)) deals with a microeconomic subject, the macroeconomic polemics triggered by some so-called Keynesian policies is behind the scene: agents can be fooled once, twice, (implicitly by the Government) but not forever: they will refer, or one has to model them as if, to the “relevant economic theory”. The Relevant Economic Theory Hypothesis will become the Rational Expectations Hypothesis (REH), a more appealing label, (although misleading as I have argued). Then progressively, the REH will take over theoretical modelling, or if one prefers “formalized economic theory”.

Let me illustrate such a rise. Start with the field of general equilibrium, which put emphasis on a formally static model, (Debreu (1959) supporting however an a-temporal implementation of inter-temporal equilibrium. In a truly inter-temporal framework, the economy consists of a succession of spot markets for goods and financial markets at each period, (replacing the futures markets held at the beginning of time of the static theory). The equilibrium becomes an “equilibrium of plans, prices and price expectations” (Radner (1972)), an equilibrium which may be interpreted either as involving perfect foresight or, in the broader sense stressed here, rational expectations. In this general framework, if markets are “essentially complete”, the outcome is efficient and replicates the complete markets solution of the static model (see Arrow (1953), Guesnerie-Jaffray (1974)). In this setting, “incompleteness” or the “inadequacy of price expectations” are sources of “markets failures”.

Also, since Walras, economists are aware that the market, in order to deliver the prices that are solutions of the equilibrium equations, has to be a substitute of a computing machine. According to Walras, the virtues of the market computing algorithm are not obvious and have to be explained (this is the purpose of the tâtonnement theory). In the inter-temporal context, the “equilibrium of plans, prices and price expectations” possibly hides two deus ex machina: spot markets at a given date clear, (this is one of the algorithmic virtues of the market, which normally is subject to verification), but this process relies on an understanding of the next period spot markets clearing, the equations of which are implicitly supposed to be resolved in people’s minds. Hence, part of the market “equilibrations” in the sense of F. Perroux, can be viewed as the product of a kind of collective thinking of the agents. Such a collective thought process helps to resolve the walrasian auctioneer’s problem, the mental activity of the agents mimicking the working of the to-morrow market algorithm.

General equilibrium provides a good reference of the general movement. Most inter-temporal modelling in most specialized fields, either with a general equilibrium flavour, as trade, or with a partial equilibrium focus, (labour markets, insurance,..) will routinely adopt the rational expectations hypothesis.

Modern finance is partially rooted in general equilibrium, and the REH has a clear hegemonic position in the field and underlies its influential theoretical models, for example those which stress the informational efficiency of markets, even in its most critical versions (Grossman-Stiglitz (1980)).

Last but not least, the REH has acquired a hegemonic position in modern macroeconomics. Lucas' dismissal of voluntarist policies takes place in a rational expectations world (Lucas (1972)). The Real Business Cycles models (from now on RBC) sometimes presented as "walrasian", describe long-lived agents who anticipate the whole future, correctly in the sense of the REH. In this world, there are few spot markets adjustments and most adjustments come from expectations and agents are as good as the best theorist at computing the complex equilibrium. New Keynesian models although their denomination refers to Keynes, echo some of the Walras preoccupations concerning the implementation of the equilibrium. Prices are quoted, not tentatively by the auctioneer, but irrevocably within each round of quotation, by firms having some market power. The procedure introduces frictions, which indeed have a Keynesian flavour, but the quotation of prices, which provides a non-walrasian response to the Walras problem, relies on the REH: firms have a correct understanding of the future, in particular of the future flows of quotations by rival firms. "Good" expectational coordination is also taken for granted⁵.

2- Directions for a critical assessment.

If the REH has gained a hegemonic position in formalized economic theory, paralleling the rise of Nash Equilibrium in game theory, it has not been unchallenged. I will review three directions for a critical assessment.

1- In economic theory as well as in game theory, there is an "internal" challenge (internal in the sense that it appears even if you find the assumption methodologically impeccable), a multiplicity challenge. In a game setting, the question takes the form: what happens if there are several Nash equilibria? Rephrased in Muth's defence of the REH, this says: "what is the relevant economic theory when there are several candidates"?

In economic models, the multiplicity question is particularly acute in infinite horizon models: take for example the simplest model of this sort, a one-dimensional, one step forward looking model, where the state, at time t , obtains as a function of the expectation of the state at time $t+1$. Typically, such a model has at least a steady state (x^* such that $x^*=f(x^*)$), but also a continuum of perfect foresight equilibria, which may, or may not, remain close to the steady state (in the former case, the steady state is said "indeterminate", in the latter, it is said "determinate"). Also, it has been understood, that besides the "focal solution", (the perfect foresight steady state equilibrium), there could also exist well behaved (stationary) stochastic rational expectations equilibria: indeed, the stochastic stationary beliefs governing "sunspot equilibria" look arbitrary but are self-fulfilling (see (Azariadis (1981), Azariadis-Guesnerie (1982, 1986), Farmer-Woodford (1984), and for a broader approach and an attempt at

⁵ Modern macroeconomic theory, does not rule out however a critical assessment of the REH. For example, the Taylor rule leads to choose a monetary policy triggering a "determinate" equilibrium, a requirement that reflects suspicion towards other inter-temporal solutions, although they also meet the REH (see next section).

synthesis, Chiappori-Guesnerie (1990), and Guesnerie-Woodford (1992)). The lessons drawn from the sunspot literature are fairly general. For example, in a slightly more complicated framework than the one just introduced, one-dimensional, memory one, one step forward looking models, the focal solution is no longer a steady state, but a “saddle path” trajectory, although the logic of multiplicity (of sunspot or non-sunspot type) remains the same so that the investigation triggers analogous results. Adding complexity (see Evans-Guesnerie (2005)) and/or intrinsic noise does not change the flavour of the analysis.

Another line of research falls under the heading of a critical assessment of the REH : it is the line associated with the *global games* literature initiated in the work of Carlsson Van-Damme (1993) and Morris-Shin (1998), (2003). It puts emphasis on the incompleteness of the information held by the agents. The initial message (Morris-Shin (1998)) is somewhat different from, and to some extent opposed to, the message of the sunspot literature. The simple theory of currency attack suggests the existence of multiple equilibria, when a better modelling of the noisy information used by the agents often lead to restore uniqueness.⁶ Taking into account the incomplete information faced by the agents is key ingredient of the global games analysis.

Another line of critical assessment of the REH which is again internal (it accepts the Hypothesis, but puts the emphasis on internal difficulties) is the « *herd behaviour*” literature. With herd behaviour, the RE equilibrium exists, but the outcome depends on details of the starting conditions and is fragile in the sense that the information transmitted may be unreliable (see Banerjee (1992), Chamley (2002)).⁷

2- Let me now evoke lines of criticisms that imply a more basic reconsideration of the REH. I refer to them as “*external criticisms*”. I will start from approaches that may be given a more fundamentalist tone, from those that are of more informal or eclectic inspiration.

It is natural to interpret Muth’s « relevant economic theory” as, in game theoretical terms, a *Common Knowledge* “theory”⁸. The REE is then known to everybody, and everybody knows that everybody knows it etc... The natural, somewhat fundamental, question to be raised is the following: is it the case that Common Knowledge of the world (the logic of interactions) and Common Knowledge of Rationality (the logic of decision) imply Common Knowledge of the equilibrium? If the answer is yes, then there is a sense in which there is a Common Knowledge (from now on CK) relevant economic theory. In my terminology, (Guesnerie (1992), (2005)), the equilibrium is “strongly rational” and globally “eductively

⁶ Furthermore, this unique equilibrium is « eductively” stable in the sense suggested below.

⁷ Part of the literature keeps the REH but put the emphasis on the *imperfect awareness of the state* of the economy by agents. Such imperfect information may occur even when all information is publicly available and agents have limited attention. see Sims (2011), Reis (2006) Woodford (2009)

⁸ See Phelps (2007), on “Equilibrium theory.... This in turn implies that everyone knows this understanding to be common knowledge..”, (foreword to Frydman-Goldberg (2007) p14.

stable” (“eductively” because the assumptions trigger a collective “learning” process that takes place in people’s minds). In a simple variant of Muth’s original model, (Guesnerie (1992), the answer to the question may be positive or negative depending on the characteristics of supply and demand. Interestingly, the answer is closely connected to the convergence of the old fashioned Cobweb tâtonnement (that the collective thought process just alluded to above mimics). As global “eductive” stability along the lines of argument sketched here is very demanding, it makes sense generally to define local “eductive” stability. This line of investigation has numerous applications, a subset of which is reported in my book Guesnerie (2005)).

The line of research of M. Kurz and his co-authors can also be interpreted as a departure from the Muth implicit assumptions of CK. In Muth, the exogenous processes generating intrinsic uncertainty have to be Common Knowledge. Giving up this assumption involves drastic changes in the dynamics of coordination, which involves the so-called rational beliefs (or diverse beliefs) see Kurz (1994), Kurz-Motolese (2011), which are not REH beliefs. More generally, the assumption of a common prior is dubious, a fact that suggest bridges between the rationality criticism and the rational expectations criticism.

3- The next line of inquiry is the one associated with the old line of research on real time learning: here agents predict the future using inferences that rely on the past. As time passes, they adapt their forecasts, up to the point where eventually learning is successful in the sense that forecasts and realisations become in line (for a review of such a line of research with applications specially to macroeconomics, see Evans-Honkappohja (2001)). As the “eductive” learning approach, the “evolutive” learning may be viewed as providing a robustness test for a given REE : the test is successful when the learning process converges, unsuccessful when it does not. As the “eductive” approach, the test is only apparently 0-1: in fact “eductive” as well “evolutive” learning may be slow or fast, a fact that affects the plausibility of the equilibrium.

There are numerous connections between “eductive” and “evolutive” learning. Existing studies show that success or failure of “eductive” or “evolutive” learning are affected by the same parameters describing the situation (the connections are particularly interesting and spectacular in the infinite horizon models that have been introduced above, see Evans-Guesnerie(2003), Gauthier-Guesnerie (2005)). And it has been argued that “eductive” learning may be justified as a kind of shortcut to “evolutive” learning (Guesnerie (2005)).

This brief and subjective report shows that attempts at a critical assessment of the REH have been numerous, and have proceeded from different viewpoints. Do they have modified our views on the problems under consideration and for example on related economic policy questions?

I will argue in the next section that they have, or at least that they should have, if the profession had taken enough attention to the messages that have been conveyed.

3-How a critical assessment of the REH in different contexts change the standard (REH based) economic intuition.

I will take three examples that are related, although not exclusively, to the “eductive” learning viewpoint and to my own research. The first one is concerned with the question of the value of new financial instruments or the stabilizing virtues of speculation, the second one with the informational efficiency of the market. Both issues raise key questions in finance. The third example reconsiders the “good” expectational coordination that RBC like models assume and argues that a critical assessment of the assumption suggests that it has very weak foundations: indeed, there are convincing arguments suggesting that in the considered context, expectational coordination is likely to be “bad”.

1- New Financial Instruments: Guesnerie-Rochet (1993) have developed a simple inventory model with two goods and no production which is however, by some aspects, reminiscent of the Muth model. The initial organisation of the economy rests on the existence of storage capacities allowing, through costly actions, the transfer of part of the crop from a period of abundant crop to the next one. One then considers opening a *futures* market for the storable good, allowing people who are unable to store to participate. In both settings, the REH leads to a unique equilibrium. In a sense, the REE associated with the futures market is better: thanks to increased possibilities of insurance, the variance of the crop price is decreased. This reflects, in the considered model, an argument made by Friedman and which has indeed some generality: speculation is stabilizing (Friedman’s informal argument is that speculators sell when the price is high and buy when it is low). The point we make is however that the expectational robustness or plausibility of the REH in both cases, as evaluated from the “eductive stability” viewpoint⁹, decreases when the futures market opens. In other words, the set of parameters of the model for which the REE is stable shrinks when the futures market is introduced. Then, speculation is destabilizing, in a different but legitimate sense, i.e. *in the sense that it makes the RE coordination less plausible*. Naturally, the specific (parameter related) stability conclusions drawn are debatable. However, the model neatly stresses one important insight: the effect, on the quality of expectational coordination, of opening new markets should be a key issue in the discussion of their usefulness.

The same idea was illustrated recently in a different context, based on what I called “evolutive” learning. Brock, Hommes and Wagener (2009) consider a model where the exchange of stocks at time t is based on the anticipation of the price at time $t + 1$. This anticipation is formed from a set of learning strategies, strategies whose distribution among players depends on their past success history. The authors consider two versions of the conditions of exchange. In the first version, the stock market is backed by no other market in the second case, claims contingent to a number of states of nature that determine the dividend stock, can be traded. Their number is however insufficient numbers for “completing” the

⁹ The same would likely to be true with stability criteria based on « evolutive learning », as suggested in the next subsection.

markets. The robustness of the rational expectations equilibrium will be here validated by the convergence of the learning process toward equilibrium, the equilibrium which refers to fundamental values. And convergence properties are summarized by thresholds in the parameters space that determine the bifurcations of dynamical system generated by learning. The conclusions of the article echo that of Guesnerie-Rochet: new markets, desirable from the standpoint of risk spreading, have a destabilizing effect (i.e a dramatic negative effect on the convergence of learning).

Why such analyses, although admittedly special, would not have policy implications? To the least, they suggest that instead of accepting new financial products they do not understand, regulators should put the burden of the proof (the fact that, in particular, that they will not destabilize market expectations) on those who want to introduce new financial products. Note also, that the above studies suggest that the present working of the markets for primary goods and the role played by speculation raises questions on which the standard REH analysis is likely to provide dubious recommendations.

2- Prices and the transmission of information: The fact that markets are “informationally efficient”, the *efficient market hypothesis*, has been given different meanings in the literature. Let me discuss it from the classical work of Grossman-Stiglitz (1980) who propose a very moderate version of the property. I will argue that the critical assessment of the REH permitted by the previously presented approach shows that Grossman-Stiglitz’s conclusions still provide a too optimistic view of the ability of markets to transmit information. In their model, agents receive a private signal about the value of an asset, the average private signals providing a summary statistics of the total information available. Agents send a demand curve to the market organizer. The latter aggregates these individual curves and deduce, given the random and unexplained supply of “noise traders”, an equilibrium price that clears the transaction requests. In a RE (Nash-Bayesian) Equilibrium, agents combine their own information with the information they optimally extract from the price, which reflects the others’ information. Indeed, in this setting, the market sends a “substantial” part of the total information of the society.

Is however expectational coordination robust or fragile, for example with regard to the criteria of “eductive” stability presented above? The answer given by Desgranges (2000), Desgranges-Heinemann (2005), is simple and clear. The equilibrium is “expectationally” stable, or strongly rational, with my terminology, *only when if it does not transmit too much information*, too much being assessed against the individual information. The intuition behind the intellectual logic of “eductive” coordination is easily explained: if the market provides too much information, the actions of agents, which have therefore a strong confidence in the market, hardly reflects their personal information. But the market information is simply the sum of the information individually transmitted and will not be reliable if the agents transmit it sparingly: *trusting too much the market leads to dismiss your individual information and if everybody does that, the market will receive little information*. There is a contradiction between the incentives to transmit information and the confidence that it arouses!

This message was echoed in other contributions on this subject in a different frame (Desgranges-Geoffard-Guesnerie (2003)) or on related subjects (Ben-Porath Heifetz (2006)). Desgranges, Geoffard and Guesnerie (2003) have considered another model used in the literature of the 70's and the 80's on the role of prices in the transmission of information. In this setting, there is a unique REH equilibrium. Again, the aggressive search of information can kill "eductive" stability. In other terms and again, one cannot trust *too much* the market if everybody else trust it *too much*.

This goes beyond the result of Grossman-Stiglitz (1980) that *the strong form of the Efficient Market Hypothesis* is impossible, because if markets were efficient in the sense that prices convey all present and future information nobody would invest resources in acquiring information. Such a *weak form* of the Hypothesis compatible with Grossman- Stiglitz's views is still too much optimistic.

One or two more points should be emphasized:

- The set of equilibria that can be "rationalized" as a (substitute) set of possible outcomes of the interaction when the rational expectations equilibrium ceases to be (locally) stable, can be identified in the Grossman-Stiglitz framework: this set is "thick", and the quality of information that agents can infer from its knowledge, is, in a sense, very poor. Yet in the context thus outlined, agents cannot, in any clear sense, "beat the market." The analysis suggests that the retreat of the most notorious supporters of the informational efficiency towards the position: "informational efficiency holds because agents cannot beat the market" is a real rout.

- In this setting, let us just mention two more points. First, the Grossman-Stiglitz paradox, i.e that agents are not willing to pay for information, (because it will be provided freely by the market) partly disappears. In parallel, the risk premium required for market participation is much higher than the one associated with the REE.

In terms of policy implications, these results shed doubt on the unquestioned faith of many in the preeminence of market prices of assets and liabilities in accounting and regulation.

3- Long lived agents and the prediction of the future. The design of Real business cycles models is unlikely to support any Keynesian-like message: long-lived agents can transfer their inter-temporal wealth, without any constraints other than those relating to market conditions. Short run income has little effect on permanent income, so that the Keynesian multiplier can, at best, be very modest. If one adds the icing on the cake that the agents rationally anticipate that additional tax will be lifted tomorrow to offset the deficit today, the Keynesian stimulus can be stored into oblivion! The modelling options, long horizon, rational expectations, (aside the criticism bearing on the high aggregation level and possibly on price flexibility), rule out a Keynesian therapy. Keynes goes out through the door. In this context however, the neo-Keynesian models introduced an interesting

innovation: firms announce prices for their products so that price adjustments are no longer the product of the *deus ex machina* of the market, (as I said earlier, indirect homage to Walras, even if the adopted solution does not rest on a process of trial and error). As noted above, such models inherit some Keynesian-like properties: activity is variable across periods, with correspondingly alternation of inflationary or deflationary effects, that can be associated with a "Phillips curve". Keynes is back on the floor. But here, as in RBC models, agents have long horizons that allow them to modify as they wish their intertemporal wealth, and they also have rational expectations. So firms announce prices, which is reasonable, but their decision is based on a statistically accurate view of future decisions of their competitors. So back on the carpet is the central question addressed here: what can we say about the quality of coordination of expectations in all the just evoked models?

I will advertise here for a text only available in the form of "discussion paper", co-authored with G. Evans and B. Mc Gough (2010). In this paper, we consider a RBC model, the simplest possible, i.e. deterministic, and we examine the steady state, characterized by a level of capital, stable over time. Under what conditions, is the equilibrium (locally) strongly rational or "eductively" stable in the sense described above? The surprising answer is: never, in this model, viewed with the glasses advocated here the infinite horizon equilibrium has maximum fragility.

Let us indeed consider a neighbourhood of the inter-temporal equilibrium : it must be here a neighbourhood of the entire trajectory, say, for a simple example, a "tubular" neighbourhood: everyone believes that the capital will remain indefinitely in an epsilon neighbourhood of its equilibrium value. If all agents share such a belief (that the system will remain in the tubular neighbourhood), then their individual plans are constrained, and the same is true, by summation, of the corresponding states of the system. For example, the hypothetical initial belief implies that capital will remain the first period, at least for certain values of parameters, inside the tube (we call this property "weak stability"). The initial belief also sets bounds on the path of planned savings at each time by each agent, and thus on the capital stock that would result in each period. But here is the key to the difficulty: within the bounds set by the beliefs, inter-temporal aggregate plans can deviate as much as you want from the tubular neighbourhood initially considered, and this holds true whatever the parameters of the system. The reason is interesting: agents' decisions, either plans or immediate decisions are sensitive to the expectations of interest rates not only in the short run but also in the long run. It follows that the long-term plans are becoming increasingly sensitive to the forecast of interest rates over the entire trajectory and the more, the more distant is the period under consideration. The fact that the long-term plans of the agents are strongly affected by the initial uncertainty implies that the mental process of elimination of dominated strategies is blocked at the first iteration: the hypothetical common belief of the first stage can not generate the common knowledge of equilibrium; the equilibrium is necessarily "eductively" unstable!

This "high tech" story has "low tech" counterparts: beliefs in line with the initial tubular restriction will eventually lead the system to leave the "tube", and the same is true if beliefs

are adapted conformably to standard learning rules. . Without going into the details of the analysis, which later combines an “evolutive” dimension of learning together with an a priori tubular or non-tubular restriction, let us suggest that the analysis puts Keynes back through the window: in this world a key role for government expenditures is to help expectational coordination. But the policy discussion would be premature and clearly goes beyond the scope of this paper.

4- Conclusion on where to go ?

Let me try first to summarize some of the teachings of the previous investigation.

First, the proponents of the REH tend to view it as a modelling axiom, which is equally reasonable across problems and situations. The point made here suggests on the contrary that *the fact that the REH is an equally reasonable modelling option across situations is properly unbelievable*. Should we, as some do suggest, conclude that the REH is uniformly unreasonable and uninteresting?

One cannot seriously claim that it is uninteresting: for every problem, the REH provides a reference description of economic evolution, free of serious expectational mistakes. Its lack of compatibility with observed facts, whenever it occurs, is the source of an intellectual challenge.

Is it uniformly unreasonable? I do not personally think so: there are problems and situations, (situations being characterized by the values of the relevant parameters for the considered problem) for which the REH provides a reasonable description of what may happen. Also quiet times, where the hypothesis provides a reasonable modelling tool, may alternate with turbulent times, where it is no longer the case.

A critical view of the REH does not necessarily mean uniform and complete dismissal. But, as the preceding section makes clear, a critical view of the REH makes a huge difference in the policy discussion. When the REH holds, a policy change does not affect the quality of expectational coordination: the system goes from one REE to another one¹⁰. The argument made here, that policy changes affect the quality of expectational coordination, changes the prospects. It suggests that the evaluation of the change of quality of expectational coordination associated with a policy change is an unavoidable and potentially major dimension of any policy change.

Going beyond the present argument, let us return to the different research strategies for facing the challenge of producing theories that truly face the difficulty of explaining, throughout situations, the quality of expectational coordination, or the extent of expectational mis-coordination.

¹⁰ The multiplicity issue raises expectational coordination considerations, relating with equilibrium selection, and which are policy relevant, and which have been given attention in part of the present literature.

A first strategy aims at checking the robustness of the REH in a given context. The searched conclusion is that, in a given situation, the REE coordination is a priori robust. This is the point of view taken in part of the research presented here, which was labelled “eductive” and has two possible directions of application. The global “eductive” test relies on the “high tech” analysis of a sophisticated mental process and is very demanding. The local “eductive” test, which, in a sense is the local version of the global hyper-rationalistic criterion, has a “low tech” interpretation¹¹: local “eductive stability basically reflects the *elasticity of realizations to expectations*: a small elasticity signals robustness when a high elasticity signals fragility. The standard learning studies can be given similar interpretations; convergence of the learning processes signals the plausibility of coordination, when absence of convergence signals difficulties. In both cases, the “eductive” approach and the standard “evolutive” one, the test is in principle 0-1 one, but in both cases, as already stressed, another less abrupt interpretation is available (since, for example convergence may be fast or slow).

A more demanding, although attractive strategy is to search for a general alternative to the REH modelling. Going along this line of approach does not necessarily mean a rejection of the just sketched “robustness” line of investigating: the alternative might be close to the REH in cases where the REH construct produces robust outcomes, But this line is clearly much more ambitious: the objective is to stress outcomes which are not REE, but in a sense “generalized economic equilibria”¹².

Note first that the “eductive” approach, presented here, provides the basis for such an alternative theory, where the “generalized economic equilibria” would be identified with the “set of rationalizable equilibria”. Indeed, in some cases, they have been identified¹³ and provide an alternative view of plausible economic outcomes. Other alternatives have been proposed in the literature, as the “diverse beliefs” equilibria of Kurz (1994) or the “imperfect knowledge economy” (IKE) of Frydman-Goldberg (2011), that I cannot discuss in depth here.

Let me however stress that the task is formidable. Out of the disciplined world of the REH, there is a “wilderness” of non-REH worlds, (if I may borrow from Sims’ metaphor of the “wilderness of bounded rationality”). Let me finally stress four reasons for such a wilderness.

- First, one has to disentangle the difficulties of predicting, on the one hand, exogenous or “intrinsic” uncertainty - the states of the world, which are not affected by actions- and on the other hand, strategic (or “extrinsic”) uncertainty, i.e the agents’ actions. These two problems are not similar, although they may be connected. Note that in the basic models I have

¹¹ For example, it does not suppose, in its less sophisticated interpretation, even that agents “know the model”.

¹² Note that standard learning studies may stress the possibilities of different outcomes to learning, although this is difficult to view them as generalized economic equilibria.

¹³ See Guesnerie-Jara-Moroni (2011) for the characterization in models with strategic complementarities or substitutabilities

sketched in the previous section, there is no “intrinsic” uncertainty. The focus is on the difficulty of predicting the others’ actions, a problem that the REH axiomatically resolves. Note also that it has been argued convincingly that in our economies, a number of basic “structural” parameters are changing in a way which is not correctly assessed by the agents. The consequent absence of “stationarity” in the data raises legitimate but different concerns on the difficulties of coordination. Naturally, the two difficulties may interact : if it is difficult to predict the others’ beliefs on changing “intrinsic” uncertainty, it will be difficult to predict their actions, even if predicting their actions conditional on beliefs is possible, which may or may not be the case.

- Second, the risk for theory is not emptiness but overflow. Once mis-coordination (in the sense of the existence in the economy of a variety of views of the future) of expectations is allowed, one is forced to recognize that there are myriads of ways for this to happen. The theory must face the challenge of assessing such a variety, while putting plausible limits on this variety (if not, it would not have any predictive power).

- This brings to the third point: the theory under consideration must face the challenge of being, in some sense, “a relevant economic theory”. There are different meanings to that, one of them being that if the theory becomes known by its actors, and if the situation is enough reproducible, it should remain true, when reproduced. Many possible stories on the working of the stock markets, which involve for example competition between learning schemes, cannot fit such a requirement; if the story convincingly shows that one of the learning schemes is superior, then why would the others be still competing in the future? Naturally, this question makes sense only if the future looks enough like the past. The question of the degree of similarities of situations is clearly behind the scene¹⁴.

- The fourth point is that there are many fascinating normative questions behind the reconstruction of an alternative viable theory. For example, the REH leads to view the market’s coordination of expectations as “good”, good in the sense of being unbiased, but good also to some extent from the social welfare viewpoint¹⁵. Outside the REH territory, the question of the possible role of government for improving the coordination of expectations comes back on the scene¹⁶. In any case, the question raised in the third part of this paper, i.e the question of the effect of policies on the quality of expectational coordination will remain unavoidable and more difficult to resolve.

¹⁴ I have discussed this problem (Guesnerie (2000), in my inaugural lecture at Collège de France. In a very different way, the “black swan” argument (Taleb (2007), raises the question of the recurrence of rare situations.

¹⁵ Although, in the absence of completeness what is “socially good” is not always crystal-clear.

¹⁶ Although I do not claim that there is an obvious answer: there may be a Hayekian line of argument claiming that the government cannot improve on market coordination even when it is bad. The market failures may go with a government failure. Note however that P. Massé’s views of the merits of French planning stressed almost uniquely its expectational coordination virtues. (“une étude de marché généralisée”, Massé (1978))

Further behind the scene is the question of the philosophical determinism which has shaped the development of natural sciences but also of economics as a social science. Let me suggest, in order to conclude, that our knowledge, to be more relevant may have to become more modest and closer to the standards that some sociologists assign to their field. (Passeron(1991)).

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