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MD Interview

AN INTERVIEW WITH ROGER GUESNERIE

Interviewed by

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Columbia University

September 2009

Roger Guesnerie is a prominent economic theorist. Among his many honors and distinctions, he is a Foreign Member of the American Academy of Arts and Science and an Honorary Foreign Member of the American Economic Association; he has been a President of the European Economic Association and of the Econometric Society. Currently a Professor at the College de France, he has made fundamental contributions to second-best theory and public economics, information economics, and rational expectation theory.

Guesnerie was one of the founding fathers of second-best theory. His contributions to public economics, addressing issues such as nonconvex economies and especially taxation in a second-best context, remain absolute references on these topics; more recently, he has provided an in-depth analysis of issues related to climate change and global warming from a public economics perspective. Regarding contract theory, his work with Jean Jacques Laffont has considerably extended our understanding of the properties of the basic models, as well as the set of tools that economists can use to address these problems. Finally, Guesnerie's work has been influential in the critical reassessment of rational expectation theory that took place over the last decades. On one hand, he has been a prominent figure in the development of so-called "sunspot" models, which reconcile the Keynesian notion of "animal spirits" with the formal requirements of rational expectations. On the other hand, he has provided path-breaking discussions of the *theoretical* relevance of rational expectation theory, based on a detailed assessment of the *coordination* between individual expectations that is implicit in the concept. In some classes of models, Guesnerie argues, rational expectation equilibria constitute a natural outcome, because coordination can be achieved either through "eductive" reasoning (for which common knowledge of individual rationality is sufficient) or as the product of simple evolutionary or learning dynamics (and in general in both ways). In other cases, however, such arguments do not appear to support the rational expectations solution, and its relevance is therefore much

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FIGURE 1. Hanoi (July 2006) after a lecture on global warming.

less obvious. On a more institutional side, Guesnerie has created the doctoral program "Analyse et Politique Economique" at the Ecole des Hautes Etudes en Sciences Sociales; besides this program, in 1989, several related research centers in Paris merged into Delta, which Guesnerie chaired between 1992 and 2000, and then into the Paris School of Economics (PSE), at which Guesnerie is Chairman of the Board.

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This interview with Roger Guesnerie gives an opportunity to discuss further the current situation of economic theory and economics in general, as well as to compare the European and American traditions in economics.

Keywords: Second Best, Optimal Taxation, Contract Theory, Public Economics, Global Warming, Rational Expectations

PERSONAL

Pierre-André Chiappori: You were a student at Ecole Polytechnique, which trains mostly engineers, top civil servants, and firm managers. How did you become an academic in general and an economist in particular? When did you decide to become an economist?

Roger Guesnerie: Ecole Polytechnique is one of the French "Grandes Ecoles" created by the Revolution. It was initially aimed at providing scientifically skilled civil servants, which then meant engineers but for the time being with very demanding scientific background. They were serving in the technical fields of State intervention (public work, transportation, mining), but also as army officers (in the artillery) and military engineers. Now, as you mentioned, the balance of careers of former students has changed and firm managers have replaced army officers! But the School has always produced scientists, physicists, mathematicians, and even some of the most eminent of the nineteenth century (Cauchy, Poincaré, . . .). It has also a tradition in economics. The tradition was built upon the familiarity with the economic problems faced by a state engineer. Jules Dupuit's story is a prototype: he was a very active engineer, building roads and canals, and was naturally curious of the utility of public works. His understanding of the problem led him to challenge the prevalent and somewhat opposite views of H. Navier (the man of the Navier–Stokes equations) and J.B. Say. Hence came the "Dupuit's surplus." But his example inspired others: Dupuit became the pioneer figure of a group, called "les ingénieurs économistes," which was recognized by the Anglo-Saxon economic thought as a coherent group; one can mention Colson, Divisia (the man of the index), Roy (author of the well-known identity in consumption theory), Massé—to cite only deceased members. Most of them and all those I quoted had graduated from Polytechnique. So coming to economics was not an eccentric choice in my situation.

Pierre-André Chiappori: Do you feel you are a product of this school? More generally, as a student in Paris in the mid-sixties, what were the important influences that shaped your thinking about economics?

Roger Guesnerie: My decision to come to economics may be viewed as a positive response to the tradition, although, at the time, it reflected more rejection, at least some temporary rejection. Let me explain: I was then at Ecole des Ponts et Chaussées, with the purpose of becoming a civil servant at the Ministry of Public Works (called at the time "Equipment"). I was then at the place that could be viewed as the heart of the school of ingénieurs économistes and my teacher was

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Marcel Boiteux, who although he had not graduated from Ecole Polytechnique—nobody is perfect—was a typical and highly skilled member of the School. I learnt everything about cost–benefit analysis, discount rate, pricing, et But in a sense, I got moderately convinced. At the same time, I had decided to learn political science, at a nearby place called Institut d'Etudes Politiques: teaching there was more impregnated of history, and economics was almost exclusively economic policy. I found it much more interesting at the time. But my job choice was constrained by my civil servant position. Joining a center called CERMAP (Centre d'Etudes et de Recherche Mathématique Appliquée à la Planification)[1] allowed me to compromise and to reconcile my taste at the time for policy-oriented economics with some of my validated skills. CERMAP had been created by Pierre Massé when he was the head of the Commissariat du Plan: essentially, the Center was supposed to do analytical work commissioned by Massé and his collaborators. Here, I got quickly convinced that I could not understand in depth the policy problems which I had found so fascinating in my political cursus without coming back to the analytical reasoning which was at the heart of Boiteux course. I was not necessarily back home, to the home of ingénieurs économistes, but finally back to economics. Indeed, I started working, most intensively from 1967 to 1972, on problems related with the connections between standard economic policy and French-type planning. Most of this work should be termed applied and was essentially published in French. The most noticeable outcome, which was more theoretical, is a paper on the "value on international liquidity reserves." which I still like much!

Pierre-André Chiappori: Is there anything that can be called a European or a French tradition that survives and can be opposed to an American tradition?

Roger Guesnerie: The ingénieurs économistes can be viewed as *a* French tradition, but they are not *the* French tradition. While they became highly visible from outside (thanks in particular to Joseph Schumpeter), they were not prominent in the French academic establishment. The French university remained dominated throughout the nineteenth century and for a large part of the twentieth century by people who had little taste for (and most often little knowledge of) what was done by this group. Is there a European tradition? As for the French, there are several European traditions that are notably different: the Viennese groups, the Scandinavian school, the Oxford/Cambridge tradition, themselves split between different sensitivities. The question, however, remains whether there exists nowadays something like a European sensitivity that would differ from the American sensitivity. I am tempted to answer yes. Our views of the economic world are dominated by some unconscious impregnation of early influences, which have been very different on both sides of the Atlantic. European societies have been dominated, until recently, by a social-democrat political culture that was skeptical of markets and eager to promote state intervention. The American political culture is quite different and this has consequences for the questions we are interested in and the way we formulate them. I often noticed that preferences between equally interesting (or uninteresting!) theories often depend on the background. But things

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are changing and now European often accept the American implicit view of the world; this illustrates both the weakening of the social democrat European tradition and the increasing intellectual domination of the American system of production of knowledge.

GENERAL

Pierre-André Chiappori: Does economic science, as it currently exists, resembles what it used to be thirty years ago, when you were a student? How would you describe the major evolutions since then? Would you think of the evolution as continuous, or do you see new paradigms emerge?

Roger Guesnerie: When I started in the profession, I had a rather limited understanding of the state of economic knowledge. This reflected both an unusual and lacunar economic education—no degree in economics!—and the special view of the world I had inherited from France and Europe. To me, the only serious theory was general equilibrium theory and, after reading Debreu's theory of value, I tended to believe that this was the desirable standard of rigor. In a sense, there was at this time a tension in my mind between an interest for concrete problems and a fascination for the aesthetics of the Bourbakist mood. On the one hand, I have been lucky that my own broadening of the horizon went together with the broadening of formal theory. On the other hand, this gives me little authority to describe the changes in the general landscape. I have observed, from a small distance, several new and important undertakings. Let me stress three of them: first, the development of the ideas of asymmetric information—starting with discussions at the 1974 Stanford IMSSS seminar; second, the takeover of formalized modeling by the rational expectations hypothesis; and third, the game theoretical rewriting of economic theory. We have better concepts, better tools, and better models in most fields.

I see nowadays two strands. On the one hand, the rationality hypothesis (*Homo economicus* rationality) is very simple-minded, and is challenged by cognitive science and experimental economics as well as studies on "happiness." Presently, the implications of this change of perspective for our understanding of the economic world are far from crystal-clear, but potentially crucial. The second line is balkanization. The territory of knowledge is split into more and more, I was about to say continents, but countries is a better word, with extremely fuzzy connections. Having being recently faced with the task of providing a broad synthesis of the present state of finance, I have been struck by the autonomy of different subfields: standard CAPM-like pricing shows little connection with mathematical finance, corporate finance is a different world, and asymmetric information, learning, and expectational coordination, not to mention the econometrics of finance, are territories between which there seem to be limited exchange of ideas. After a period of introverted production, focusing on the internal problems of the field (of which the reconstruction of the Walrasian theory, transfigured into the Arrow–Debreu model, is a major outcome), came the period of "one hundred flowers." With surer foundations, exploration becomes more audacious and diverse. . . . If

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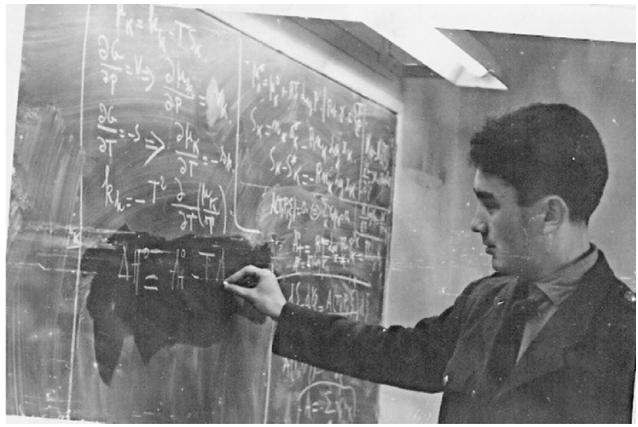


FIGURE 2. At the Ecole Polytechnique (Paris, 1963).

we are optimistic, we see only reasons to rejoice. If we are pessimistic, we may see deconstruction at work, or at least economics becoming a collection of case studies.

Pierre-André Chiappori: How would you compare the university system in Europe and the United States? Are they complements, competitors?

Roger Guesnerie: We would like them to be substitutes, but it is not easy. First note that Europe is diverse and the situation and the analysis cannot be the same whether you think of the United Kingdom or Germany. There are different educational systems, the characteristics of which are products of history. And these systems are more or less adapted to the moment in which they operate. The French system, associated with the coexistence of elite "Grandes Ecoles" and a weaker university system, may not have performed so badly, when compared to the American system and the British one, over long periods in the nineteenth or beginning of the twentieth century. Nowadays, it is partly obsolete and the American model, with a mix of public and private universities and the kind of competition we know, is not perfect but clearly much more adapted than the continental European model to the period in which we live. But educational structures, as all structures rooted in long history, are not easy to change. We are trying to change. For example, in France, recently, we have allowed a private structure to help organizing thematic networks allowing reinforced cooperation between institutions. This provides welcomed flexibility. So we have created the Paris School of Economics and to answer your question, I hope that this new structure will allow us to compete better.

Pierre-André Chiappori: How would you describe the relationship between scientists (and economists in particular) and policy makers? Is it necessary? Can it be fruitful?

Roger Guesnerie: The purpose of scientific activity in economics as everywhere else is to provide a sound view of the world, and in our field as in others

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this is likely to be of interest to policy makers. But obviously, this is not a one-way road. Policy makers raise a number of interesting questions for scientific activity. Indeed, our views are shaped through the complex interaction of our observations and of our mental schemes, from theories and facts, facts being themselves interpreted through some more or less conscious theoretical glasses. And theories, facts, questions on theories and facts evolve together. The confrontation of policy questions and theory questions is unavoidable. However, the dynamics of this confrontation often raises questions. Institutions, in particular global institutions such as the IMF, the World Bank, the WTO, or the DG competition in Europe, need to build some economic doctrine for guiding their activity. For that they consult economists, which is fine. But there is a self-selection mechanism that tends to gather around each given institution a homogenous "epistemic community," the opinion of which is biased with respect to the actual spectrum of opinions of the economics profession as a whole. One problem, exacerbated by the self-selection mechanism, is that it picks up people whose opinions fall more in line not only with the intellectual but also with the nonintellectual interests of the organization they advise.

SPECIFIC FIELDS

Second-Best Theory and Public Economics

Pierre-André Chiappori: You are one of the founding fathers of second-best theory. Were second-best issues your first topic in theory?

Roger Guesnerie: My start in theory was rather with first-best, since my first three papers published in English were concerned with general equilibrium and had a clear first-best flavor. The first two appeared in a book edited by J. Drèze, and the second of them dealt with the connections between the Arrow–Debreu world where all goods were exchanged at the outset and the world of sequential markets with what we call now perfect foresight. It explored the idea of "essentially complete markets" capable of providing a one-to-one correspondence between static and sequential equilibria. This has become a leading theme of modern finance. The third one, the first published in an English-speaking refereed journal (*Econometrica*, 1975), was focusing on the first and the second welfare theorem in a nonconvex economy. However, it may be argued that this work had already a second-best flavor, since the most substantial conclusion was that income redistribution and efficiency could not be separated: there exist income redistribution structures that are incompatible with the decentralized attainment of the first-best optimum under marginal cost pricing schemes by the increasing returns sector.

Pierre-André Chiappori: How would you describe the major achievements of second-best theory?

Roger Guesnerie: I think that we have learned a lot from second best, which has shaped in a much better way our understanding of the merits and limits of

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the market. I have tried elsewhere to delineate a second-best culture with three corners.

First point: We have somewhat understood that income distribution and efficiency were not necessarily separated. Again, it was to me the main message of my first *Econometrica* paper, just mentioned above, dealing with nonconvex economies. This absence of separability between efficiency and redistribution has been stressed more recently in new contexts. For example, the literature on education provides examples, supported then by more intuitive arguments where income redistribution may be Pareto improving or deteriorating (which is the basic message I stressed here). This is not anecdotic: this is the rule, not the exception in second-best models . . . and I guess, in the real world.

Second point: The ability of prices to convey the relevant information on social costs is limited, not only by the existence of noninternalized externalities and sector distortions, but because in general, outside the world of the second welfare theorem, prices are no longer proportional to the right shadow values. There are two identified exceptions to that, the case I called "CC-efficiency," in which in a world with taxes, consumption prices do reflect social values, and the symmetric case earlier termed "production-efficiency" by Diamond and Mirrlees, in which production prices have the right properties.

Third point: Do discontinuities in the reaction of the economic system matter for economic policy? The answer here is less crystal clear. J.J Laffont and I have produced a well-documented case where taxing price-makers is plagued by the potential discontinuity of the response of the taxed units, a fact that invalidates standard intuition.

I would also like to stress another point, with which my own research was concerned. It is no longer true that "the more the better." Even if you have strongly increasing returns to size, for example, for financing of a public good, disagreements on the volume to produce can no longer be alleviated by lump-sum transfers, since such transfers rest on non–publicly available information. C. Oddou and I showed that it is enough that people agree bilaterally two by two, which is apparently easier, but still demanding.

All these facts challenge in a different but repeated way what might be called the first-best culture, associated with the second theorem of welfare economics. More generally, besides questioning the founding stones of the first-best culture, as just stressed, the research has considerably improved our understanding of the limits of redistribution, and of the trade-off between costs and benefits of redistribution.

Pierre-André Chiappori: A frequently heard critique of second-best approaches is that they rarely provide clear-cut conclusions; most of the time, the argument goes, second-best theory concludes that "(almost) anything can happen." How do you react to these criticisms?

Roger Guesnerie: In a sense, the situation may be worse, since in some cases, too simple conclusions have been drawn, For example, in the case of redistribution, outside the world of lump-sum transfers, "production efficiency," alluded to above,

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brings us back to the reassuring, but not necessarily well established, separation of tasks: business almost as usual. The simplicity of the teachings is here not well established.

Now, let me come more to the heart of the question. Second best tells us that there are many more reasons to suspect market outcomes than we believed, but in the same way, it says that correcting them may require an understanding of the system better than what we have (that is the theory side), or better data (this is the policy side). Note, by the way, that the lack of knowledge stressed here does not identify with the lack of information of the theory of asymmetric information. The fact that the government has less information than the agents is a founding stone of the second-best theory of redistribution and does not lead to the conclusion that the government should not act, as it is sometimes argued.

It is true, as you say, that the general qualitative lessons stressed above have not crystal-clear operational implications. This reflects the difficulty of the identification of market biases, as just noted, but also probably insufficient effort in the investigation. For example, as I said, the general theory stresses many reasons that the market price system may not provide the right social incentives. But there has been rather scarce theory to identify the biases in classes of relevant situations. Second related example, competition policies give often the impression of implementing first-best prescriptions in second-best contexts. Again, we do not know much, which suggests that we may not have done the required effort!

An entirely different difficulty for the impact of the theory is that the idea of benevolent government, which was at the root of normative theory, has been challenged. This has provided scientific arguments to the distrust of government that underlies the tradition of philosophical liberalism, and one has rightly put emphasis on the need for transparency in political processes. Naturally, the analysis of this issue does not lead to the conclusion that a democratic government should not interfere with markets, as a superficial but widespread interpretation has suggested.

Add to this picture that policy makers want simple ideas (in the seventies, any problem led to the recommendation of creation of a government agency, whereas now any problem is solved by privatization) and there are more explanations than needed for the fact you mentioned.

Pierre-André Chiappori: Regarding public economics, would you say that our perception of the major problems and issues is drastically different from what it used to be thirty years ago?

Roger Guesnerie: Without any claim to exhaustivity, let me focus on three standard chapters of government intervention, as they could be seen thirty years ago: government and production, government and redistribution, and government and social insurance. The questions are long-lived if not eternal: what should the government do on each of these chapters (some people thinking that it should do nothing)? But both the focus and the context have changed. Let me illustrate that point for production and for redistribution. Take the case of production. Direct government intervention in production is shrinking, even in Europe and no less in

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FIGURE 3. Congress of Public Economics (Jerusalem, 1995). Front row, from the left: Yoram Mayshar, David Cutler, Dagobert Brito, Nicholas Stern, Effraim Sadka, and Avinash Dixit. Standing, from the left: Thomas Piketty, Larry Kotlikoff, Peter Diamond, David Frankel, Roger Guesnerie, Jim Mirrlees, Jean Jacques Laffont, Eytan Sheshinsky, Martin Feldstein, and Jacques Drèze.

countries such as mine, despite its Colbertist tradition. Modern public economics in these fields is about regulation, competition, and financial structures. The issues are not easier: for example, if you consider regulation or competition policies and its various intertwined dimensions, price, product differentiation, investments, the interest of the consumer is often difficult to ascertain. And short-term consumers' interests may conflict with medium-term economic advantages. The context also has changed. We understand better that innovation in the long run is the driving force and that innovation is endogenous and has probably complex links with the intensity of competition. The issue is about innovation-friendly structures.

In the case of redistribution and social insurance, the issues for developed countries are about the same, but in the context of globalization and fiscal competition. One may discuss whether the pressure on the low-skilled people's remuneration in wealthy countries comes from trade or technical progress (the most likely answer being that these two factors are not independent), but both redistribution and social insurance issues have now to be considered in the light of the new context. In particular, the location decision of mobile factors is being affected by fiscal competition.

Pierre-André Chiappori: You recently wrote a report on global warming. Would you say that our understanding of issues linked to the environment has evolved during the last decades, and what has been the role of economic theory in this evolution?

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Roger Guesnerie: The progress of the economics of environment antedates the awareness of the global warming problem. Global warming, however, challenges our current knowledge on at least three connected fronts. First, there is the question of the long run and the balance between the ethical views and the pragmatic views of the world; a second important but poorly understood question is the interaction of the market for fossil fuels and climate policies; the third one is the problem of global governance. None of these problems is strictly economic; all have a strong political component. This fact is obvious for the first issue, if one takes politics in a broad sense, including the way we take care of the threat on our spaceship Earth. One issue behind the second aspect is the appropriation of the rent. And the third one is political in the noble sense of the word: the organization of the city. But all solicit economists: generally, it solicits the present software, not pure theory, but its applications.

Let me add two points. On the first question, basically, the issue of long-run discount rates, which has come back to the turmoil of the public debate after the publication of the widely publicized Stern report, the issue is whether you can reconcile what you may call "ecological intuition" and "economic reason." I have argued that the answer is yes, as soon as you have intergenerational altruism (a pure rate of time preference close to zero) and you consider the relative prices effects associated with the nonmarket benefits of environment. Second, the global warming question will likely provide very interesting materials for our understanding of technological progress: which of the presently noncarbonized competing techniques will take over?

Contract Theory and Mechanism Design

Pierre-André Chiappori: Again, you were one of the early contributors to this literature. What were the goals it initially pursued? In retrospect, would you say that they have been reached?

Roger Guesnerie: I worked on these subjects at the end of the seventies and the beginning of the eighties, first with Jean-Jacques Laffont and then with Laffont and Jean Tirole. I gave up both the joint work and contract theory when I became co-editor of *Econometrica*: maybe it was the wrong choice! However, I remained interested in mechanism design, particularly for understanding the informational basis for "second-best" taxation tools.

To come back on your question, on pragmatic grounds, our understanding of bilateral or sometimes multilateral economic relationships has been much upgraded: the understanding of the asymmetries associated with hidden actions, hidden knowledge, and of the merits and shortcomings of commitment had a great impact on some fields (IO is an obvious example). I suppose that you would not disagree with the fact that the field of insurance has been renovated by contract theory. . . . On general intellectual grounds, the inability of economic theory to deal with the information extraction problem was a major shortcoming: in a sense,

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theory was unable to accurately formulate the one key dimension of the superiority of market solutions vis-à-vis central planning, their better incentive-compatibility properties. On other and somewhat opposite grounds, the widespread belief that redistributive questions could be solved costlessly through lump-sum transfers was clearly dismantled. To me, second best and informational problems are two sides of the same coin. Second best generally comes from informational problems viewed in a general equilibrium context. Contract theory and the theory of information have a lot of applications in partial equilibrium frameworks where they are not called second best. But what I have called the "second-best culture" in general equilibrium has a counterpart in what might be called the "asymmetric information culture" in partial equilibrium contexts.

Pierre-André Chiappori: General equilibrium theory, which was the main theoretical reference thirty years ago (at least in microeconomics), used to convey general messages: the efficiency of a free market, for instance. In comparison, the conclusions of contract theory and mechanism design seem more partial and fragmented; most recommendations, for instance, are model-specific. Do you see this feature as a weakness of the theory?

Roger Guesnerie: The world is less simple than what we tended to believe at some stages. Indeed, a second-best culture or an asymmetric information culture has not replaced the first-best culture: the efficiency of free markets seems, sometimes more than yesterday, the main message that policy makers trust. . . . Is it because such a culture cannot emerge (since conclusions are model-specific)? Or because not much effort has been done in this direction (I am struck for example that there is no fully articulated second-best growth theory, emphasizing the role of central incentives to the production of knowledge).

Pierre-André Chiappori: You have worked extensively on taxation. How would you describe the main achievements of optimal taxation theory? How would you describe its influence on tax reform and government policies?

Roger Guesnerie: My work attempts to describe a polar world in which asymmetric information forbids the attainment of optimality, while generally restricting attention to linear or piecewise linear tax systems. In this polar world, I have been interested in mechanism design, existence and multiplicity of equilibria, tax reform and tax optimization, and political-economic equilibrium based on game-theoretical considerations.

The types of questions that are answered in this setting are Why don't we observe more efficient tax systems (a mechanism design question)? Is the multiplicity problem of general equilibrium theory as acute as in the abstract Arrow–Debreu model? What algorithms are prone to compute tax reforms? Are there qualitative features of optimal taxes? When do prices reflect social values? What are the factors that favor the cohesion of a society composed of heterogeneous individuals whose heterogeneity of interest cannot be alleviated through lump-sum transfers? It is pure theory in the sense of Walras, with little aim at direct applications. On the other hand, "applied theory" has concentrated on the Mirrlees income tax model, arguing that other taxes are useless, a fact true under very special circumstances.

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FIGURE 4. World Congress, Econometric Society (Tokyo, 1995).

I tended to believe that the world with not fully nonlinear taxes, which I have been interested in, was more appropriate to the understanding of actual taxation issues than the standard simplistic nonlinear income tax model. The result that a nonlinear income tax schedule is a summary statistics of a full taxation system (due to Atkinson and Stiglitz) is true only under very specific conditions and becomes somewhat irrelevant whenever the number of characteristics that are required to describe the agents is high (more than one). Again, the theory may not have reached the stage of operational relevance not because it has gone too far, but because it has not gone far enough.

Rational Expectations

Pierre-André Chiappori: Your approach to rational expectations is original. Instead of discussing its empirical relevance (as many have done), you have explored its theoretical foundations and questioned its internal consistency. Can you briefly describe your views?

Roger Guesnerie: Again, the rational expectations hypothesis is a central construct of modern economic theory that has taken over formalized economic modeling, in a powerful move starting from the sixties. It is an interesting question of intellectual history to understand why and how, and why at this time, the

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FIGURE 5. With Jean Jacques Laffont (left) (Dead Sea, May 1998).

takeover took place and how it paralleled the rise of Nash equilibrium in game theory—two conceptually identical constructs. The point is that the hypothesis is not the "extension of the rationality hypothesis to expectations" as early influential propaganda suggested; it is an unavoidable but debatable reference.

Over the last ten or even twenty years, the hypothesis has been subject to intensive testing from different directions: game theory has trembling hand equilibrium, others have looked at the possible consequences of incomplete information for multiplicity and argued that it favored expectational coordination, learning theories try to assess the plausibility of the hypothesis by looking at whether the real-time learning trajectory converges to the RE solution, etc. My own approach to the question again attempts to compromise between some pure theory appeal for foundations and the concern for economic intervention. Both the method ("eductive learning") and the scope ("standard models of economic theory") reflect, although to a different extent, these two concerns. Eductive learning echoes some basic and most abstract ideas of game theory, namely rationalizability: the fact that an equilibrium is the consequence of common knowledge of the model or of rationality. But it can also be interpreted in an unsophisticated way. For instance, a locally eductively stable equilibrium is just an equilibrium for which you can find a nontrivial neighborhood having the property that if everybody believes that the state of the system is in this neighborhood, then it will surely be. Then it is a simple robustness test, although it implies the more sophisticated property that infinitely iterated elimination of non–best response strategies of the agents, starting from this set, leads to the equilibrium. The scope is on models in which individual

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agents are small, a fact that allows us both to develop systematic tools and to cover a wide spectrum of economic situations: partial equilibrium, general equilibrium, simple models of finance (transmission of information), dynamics, etc. Also, the teachings are not contradictory to those of evolutive learning, in the sense that the analysis often stresses the same factors of expectational coordination, but, in my view, in a more elegant and efficient way.

Pierre-André Chiappori: Some of your criticisms of the RE hypothesis are strong. In particular, you point out that in some situations, the assumption can be supported neither by an eductive argument, nor by a learning or evolutionary approach. What are the practical consequences of this view? Should we discard RE in those situations?

Roger Guesnerie: We certainly write models in which the RE fails most reasonable stability tests. This is true whatever the theory you take for testing. Naturally, giving up the world of rational expectations is a bold move for theorists of our education. It is not necessary a bold move for everybody. Many people coming from physics have explored worlds of active atoms that are not guided by forecasts. They are not aware of the frightening fact, which economists know, that the theories they are building cannot be true. In circumstances that allow repetition, a non–RE theory can only be wrong: if it were right, agents would learn from it to make forecasts on the future that would be contradictory with what the theory assumes. Hence, we face some paradox: stick to RE, even when the argument for that is extremely weak, or write false theories! The paradox disappears if you believe, away from the axioms of philosophical determinism that have shaped our discipline, that the predictability of economic situations may differ according to the moment and situations.

Just a final remark on this question: I tend to believe that expectational coordination is one of the blind point of economic theory, and the recent "crisis" has, if anything, revived the concern.

CONCLUSION

Pierre-André Chiappori: Where do you think economic theory is heading? What are the main challenges theorists face? In which fields is progress most needed?

Roger Guesnerie: The extent to which economic future is predictable by economic agents is debatable, if you believe what I just said on expectational coordination. However, the progress of knowledge is truly unpredictable. There are several directions in which present research may deeply change our views of the economic world. A better understanding of the working of our brain will have consequences for the way we think on many problems. Studies on happiness may also redirect attention on dimensions of social activities and a better cost–benefit analysis of our economic organization. On the other hand, I expect progress to be made from a multiplication of somewhat local investigations, documented both by

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theories and facts. Concerning the systemic questions, they have not disappeared and progress in our understanding of expectations and computational virtues and shortcomings of markets is not hopeless and the recent crisis suggests it is particularly urgent. Also, we face many other real life challenges, such as global warming or the impact of globalization, that need to trigger more reflection.

Let me add a point. I was struck by the retreat of systemic, or if one prefers, general equilibrium (in a broad sense) preoccupations in the period preceding 2008. This is not surprising, because after the collapse of the soviet system, the collective confidence in the self-regulation ability of a market organization has considerably increased. It is true also that we need to understand millions of local phenomena in a large variety of contexts. However, I am not sure that what has shaped our discipline, i.e., the question of the global understanding of the system, not only of its pieces but of the connections between the parts, again a present key problem at the time of globalization, has become obsolete.

NOTE

1. CERMAP became later CEPREMAP. . .

SELECTED SCIENTIFIC PUBLICATIONS OF ROGER GUESNERIE

Valeurs des réserves de liquidité internationale. *Cahiers du Séminaire d'Econométrie* l4, 1972, 65–93.

Optimality of equilibrium of plans prices and price expectations (with J.Y. Jaffray). In J. Drèze, ed., with the assistance of F. Delbaen, L. Gevers, R. Guesnerie, and D. Sondermann, *Allocation under Uncertainty Equilibrium Optimality*, pp. 71–86. London: MacMillan, 1974.

Pareto optimality in non-convex economies. *Econometrica* 43(1), 1975, 1–29.

On the direction of tax reform. *Journal of Public Economics* 7(2), 1977, 179–202.

Taxing price makers (with J.J. Laffont). *Journal of Economic Theory* 19(2), 1978, 423–455.

General statements on second best Pareto optimality. *Journal of Mathematical Economics* 6(2), 1979, l69–l94.

Second best taxation as a game (with C. Oddou). *Journal of Economic Theory* 25(1), 1981, 67–91.

Effective policy tools and quantity controls (with K. Roberts). *Econometrica* 52(1), 1984, 59–86.

A complete solution to a class of principal agent problems with an application to the control of a self-managed firm (with J.J. Laffont). *Journal of Public Economics* 25(3), 1984, 329–369,

Planning under incomplete information: The ratchet effect (with X. Freixas and J. Tirole). *Review of Economic Studies* 52(2), 1985, l73–l9l.

Sunspots and cycles (with C. Azariadis). *Review of Economic Studies* 53(5), 1986, 725–737.

Stationary sunspot equilibria in an N-commodity world. *Journal of Economic Theory* 40(1), 1986, 103–104.

An exploration of the eductive justification of the rational expectations hypothesis. *American Economic Review* 82(5), 1992, 1254–1278.

Sunspot equilibria: The case of small fluctuations around the steady state (with P.A. Chiappori and P.Y. Geoffard). *Econometrica* 60, 1992, 1097–1127.

Lucas equation, indeterminacy and non neutrality: An example (with P.A. Chiappori). In P. Dasgupta, D. Gale, O. Hart, and E. Maskin, eds., *Economic Analysis of Markets and Games—Essays in Honor of F. Hahn*, 445–465. Cambridge, MA: MIT Press, 1992.

(De)stabilizing speculation on futures markets: An alternative view point (with J.C. Rochet). *European Economic Review* 37(5), 1993, 1043–1063.

Rational random walks (with P.A. Chiappori). *Review of Economic Studies* 60, 1993, 837–864.

https://doi.org/10.1017/S1365100509990782 Published online by Cambridge University Press

The genealogy of modern theoretical public economics: From first best to second best. *European Economic Review* 39, 1995, 353–381.

A Contribution to the Pure Theory of Taxation (Econometric Society Monograph Series). Cambridge University Press, 1995.

Assessing Rational Expectations 1: Sunspot Multiplicity and Economic Fluctuations. Cambridge, MA: MIT Press, 2001.

Short-run expectational coordination: Fixed versus flexible wages. *Quarterly Journal of Economics* 116, 2001, 1115–1147.

Kyoto et l'Économie de l'Effet de Serre. Report and direction of R. Guesnerie. La Documentation Française, 2003.

Do prices transmit rationally expected information? (with G. Desgranges and P.Y. Geoffard). *Journal of the European Economic Association* 1, 2003, 124–153.

Calcul économique et développement durable. *La Revue Economique* 55(3), 2004, 363–382.

Coordination on saddle-path solutions, the "eductive" viewpoint, 2-linear multivariate models (with G. Evans). *Journal of Economic Theory* 124(2), 2005, 202–229.

Assessing Rational Expectations 2: Eductive Stability in Economics. Cambridge, MA: MIT Press, 2005.

https://doi.org/10.1017/S1365100509990782 Published online by Cambridge University Press