

The future of general equilibrium^{*,**}

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1 Introduction

My task today is not that simple since if I say what I'm supposed to, namely, that there is a lot of future to GE theory, you can rightly ask what have we been doing all these years. GE would be somewhat in the position of that developing country that has a great future ahead and that, the story goes, will always have such a future... I want to start, therefore, by asserting that we are not in such a position. Our generation of general equilibrium theorists has done much work and there are things that simply do not need to be repeated. They have been done for the ages. I will not give you, however, a canonical list of closed topics. It is a delicate matter, as the list would be different from researcher to researcher. It would depend on tastes and also on various psychological pulls. Logically, the list should include the topics one has worked on, for the reason just mentioned: what have you been doing otherwise? But maybe the temptation to list the topics other people have worked on would prove irresistible. Well, I should stop here before I slide too dangerously on the slope of cynicism.

* *Editor's note:* Andreu Mas-Colell served as first president of the Spanish Economic Association/Spanish Economic Review in 1998. This is the first of a series of contributions from past presidents that we intend to publish regularly.

** This is the text of a lecture given at the Econometric Society meetings held at Boston in January 1994. It was a contribution to a panel session organized by Joe Ostroy on "The Future of General Equilibrium". I have revised it a number of times but it has remained unpublished. Salvador Barberà has suggested to me that it fits a type of contribution he wants to have present at the Spanish Economic Review. Since I have always found Salvador irresistible I let myself be carried by his judgement and thank him. Needless to say, the content, with all its shortcomings, is – for better or worse – all mine. This does not mean that I have not picked ideas from here and there (the air included). I note, with some trepidation, that this is the text of the lecture and so, it is not footnoted or referenced.

My dialectical angle today will be the following. I will argue that the style of analysis that characterizes GE theory characterizes also the central nucleus of economics and that, as a consequence, it has as much future as economics has. With this, I will not have answered my question but, as a good mathematician would, I will have reduced my problem to another one: what is the future of economic theory? A question that is very interesting but that I will not try to answer. I plead lack of time.

It is pertinent at this point to ask what I mean when I refer to the GE approach or to the GE style of analysis. In the literature, we could document practically a continuum of definitions, which is good news for me since I want to remain a bit vague and, so to speak, stylized.

At one end of the definitional spectrum, the GE approach amounts to a simple methodological desideratum asserting that good analytical models must be closed. They must take into account, for example, that disturbances in one market cause disturbances in other markets. I think that, in itself, this definition is too weak, hence too facile to argue the case for GE, since it says little more than a vector of values of the independent variables constitute an equilibrium if and only if it satisfies all the equilibrium equations. A good reminder of the need not to lose track of the definition of equilibrium but not more than that. Still, the view of equilibrium as the solution of a well posed and explicit system of equations has fostered a computational outlook that it is a distinctive feature of the GE style.

There is a more refined version of this methodological view that has a definite content. We would now add that we should not treat as parameters variables which, and this depends on context, are clearly derived. Namely, that we must aim to explain from fundamentals. I shall take these desiderata as another distinctive feature of the GE style.

At the other end of the definitional spectrum GE is a theory with substantial and very specific content: it is the Walrasian theory of perfectly competitive markets. Now, I believe that the Walrasian theory of perfect competition (as elaborated by several generations of researchers) is at the heart of economics and that it has value both descriptively and as that ideal benchmark that, somewhat defensively, GE theorist have traditionally emphasized. Yet, I would not want to commit myself to a reductionist view that, for example, would rule out from GE theory monopolistic competition in the style of Chamberlin. I will take the broader view (I need some breathing space to make my case!) that a key aspect of the style of GE lies in the study of markets, perhaps linked systems of markets, characterized by a low level of strategic interaction among agents. I will be more explicit about this but for the moment let me just say that I would regard mass markets as the leading prototype of such markets.

The previous observations suggest an organization for the rest of my remarks. I will present them as comments to the three style features I have underlined. I will do it in reverse order of appearance. First, I will discuss the place in economics of the analysis of markets with low levels of strategic interaction among agents. Second, I will discuss the methodological desideratum of explaining from fundamentals. Third, I will talk a bit about computation.

2 The role of markets with low level of strategic interaction in economics

The first thesis that I will defend is that the comparative advantage of economics lies in the study of markets. Because GE theory deals with markets this fits nicely with an optimistic view of the future of GE theory.

Economics has tended to treat firms, or other institutions, as black boxes. The modeling approach to them has been to take the simplest shortcut allowing to concentrate on the analysis of markets. In fact, the same observation applies to individuals and the consumption side of the economy. By this blackboxing we have often been guilty of crudeness and of missing essential aspects of reality. But it is important to realize that there was a reason to proceed in this manner: to repeat, it allowed us to quickly gain some understanding of the functioning of markets. The accomplishment should not be underestimated. While it is completely clear that much resource allocation takes place in organizations (firms, or the government, are two examples), one of the lessons of the fall of the central planning societies is that markets are essential to a well functioning economic process and that they are not about to disappear.

Many researchers have, at long last, undertaken the task of filling the black box. There is now an emerging new theory of the firm, and a new theory of the government. I will not claim this for the GE approach, it does not belong to us. But I will also make the point that while it is evident that economists have much to say about organizations I do not share an all-encompassing view of the boundaries of economics. As with political science, economist, endowed perhaps with superior technology (and here I include the perspective of methodological individualism, contract theory, the theory of games, etc) may be the midwife to a major reorientation of the field, or even to the emergence of a new field: organization theory, for example. But I am not sure that once everything is said and done we will end up seeing the theory of the firm at the heart of economics.

This is not to say – a partial retreat from an sweeping statement may be in order – that a look at the entrails of economic agents, firms or consumers, may not have consequences for the analysis of markets. Thus, if psychologists convince us that consumers do not utility maximize but do something else we certainly will want to know the market implications of this. The same can be said of the firm. In fact, it is high time that the general equilibrium market implications, in particular the macroeconomic implications, of the new theories of the firm be exploited. While modern macroeconomics has often adopted views of market competition that go beyond the perfectly competitive paradigm it has tended to stick to the hypothesis of profit maximizing firms (in a context where the hypothesis is weak on theoretical ground). I do not think that this is an ideal state of affairs: there is every expectation that the new views of the firm, hardly summarizable in a hypothesis of profit maximization, may have significant macroeconomic implications.

While I have expressed a doubt about the centrality of organization theory in economics, I will not do the same for the theoretical structure that would be next in a logical conceptual development: markets dominated by the interaction

of a few, very few, economic agents. While I grant that this domain is central to economics I will not, however, claim it for GE theory. Here, game theory reigns supreme.

It is the first time that I meaningfully mention game theory. So let me devote a few words to explain what I think are the basic contrasts with GE. Game theory is, of course, a cousin of GE theory and we know well that there has been a close relationship in the development of the two theories. Both deal with the logic of the equilibrium of interacting agents. We may recall, for example, that the first proofs of the existence of a competitive equilibrium were obtained by using game theoretic tools. Historically, however, there has been an unfortunate phenomenon that has obscured the contrast among the two approaches and has made them appear more similar than they are. An application of Occam's razor led very early to the substitution of the hypothesis that markets are composed by many agents, a hypothesis delicate to formalize, by the simpler hypothesis of price taking. But by doing so perhaps we lost a clear view of what the real environment of the theory, mass markets, was. With price taking it does not matter if we talk about few or many agents. And with few agents and price taking the theory of equilibrium and, say, the concept of Nash equilibrium amount, conceptually speaking, to the same thing.

Be as it may, I view the main contrast between the game theoretic and the GE approaches to economics to be the fact that the interaction among the few are essentially strategic while they are not so in a mass market (I'll try to be more precise on this in a moment). By "essentially strategic" I mean that when considering a move, a departure of a position being tested as a possible equilibrium, for example, an agent has to anticipate and ponder how this will affect its opponents and how they may react (and how they will think that he will react, etc). The richness and the subtlety of game theoretic analysis come precisely in trying to make sense of this strategic play.

I will maintain that, in contrast, there is none of this in the GE approach, that individual actions have no effect on others and that comparatively speaking the decision problems of individuals (at the potential equilibria) are trivial and uncomplicated. I will submit, in addition, that this is a prerequisite of the approach that I'm calling GE, that is that the latter applies, in essence, to the domain of environments of market interaction with a low level of strategic interaction.

As indicated the hypothesis of price-taking leads to this sort of strategic simplicity. But much more generally, the intuition is that interaction in mass markets, where individual agents are insignificant in the aggregate, should be the key factor taking us to strategic simplicity. Intuitive as this is, it is also elusive since it appears to require some anonymity in the interactions, be they bilateral or multilateral, and the anonymity concept is not easy to get at. Perhaps this is a topic to be settled in the next few years.

In summary, it is clear that I believe that the GE approach and the GT approach are complementary and not substitutes. They have both territories of application which are natural to them. Most important, there are mixed territories demanding the use of the two methodologies. We may, for example, wish

to analyze by GE methods the mass interactions of families of small group negotiations analyzable by GT methods. As another example, we may have many of certain type of agents and a few, even one, of others. A case in point for a rich interaction of GE and GT models is political economy. We have in the politics of economic decisions some aspects that fit like glove to hand the needs of game theory, in particular well-defined rules. With the latter the analysis has a real possibility to predict. When studying markets the rules are so unspecified that the GT approach more than predict what it does is to experiment on how rules affect outcomes so as to gain some insight on which aspects are crucial. On the other hand, political economy abounds with mass interaction situations (the vote, for example), which in principle are well adapted to GE analysis. It is to be expected therefore that political economy will be an area of substantial cross-fertilization among the two approaches.

3 The explanatory power of fundamentals

The aim to explain from a few basic fundamentals is characteristic of economics, and most particularly of GE theory. Let me particularize and refer here to the classical instance of GE modeling, namely the Walrasian theory of markets with price taking agents. It is something of a cop-out to exhaust the content of Walrasian theory in a purely normative theory. If that was all that GE tried to accomplish it would then be fine not to address the positive aspects. After all, it does not matter if an ideal, normative, reference point is realistic or not: one may still claim usefulness as a tool for thinking about the real economy. But it is clear that the Walrasian theory of markets attempted much more. Namely, it attempted to explain and predict from a few basic ingredients.

To be more precise, Walrasian theory attempts no less than to predict the complete vector of final consumptions and productions using only the fundamentals of the economy (the list of commodities, the state of technology, preferences and endowments), the institutional hypothesis that a price is quoted for every commodity (including, of course, the ones that will not be traded at equilibrium) and the behavioral hypothesis of price taking by consumers and firms. The researches of the last twenty five years tell us that under conditions of convexity the theory succeeds (an equilibrium exists; a continuum of equilibria is pathological) but only partly (we can only assert the typical local uniqueness of the equilibrium prediction rather than its uniqueness).

This tidy picture has been challenged in the last years. It has been noted that expectational factors operating with open-ended horizons may generate self-fulfilling equilibrium phenomena with more ease than one may have suspected. Similarly, externalities, or incomplete markets, or imperfect competition acting perhaps in a non-convex world, make it quite easy to lock the economy in a wide variety of positions. The same lesson has also been taught by game theory.

It should not be expected from a theory that wants to explain from fundamentals to explain if the capital of Spain will be in Madrid or Toledo. On the

other hand, in the grand scheme of things this fact does not matter. A more serious difficulty are the locking-in effects that really matter: inefficient locations, etc. Undoubtedly examples abound. But since today I am in a polemical mood I will add that I am beginning to have my doubts on some of the classical examples. Does the famous example of QWERTY vs. DVORJAK keyboard makes the point it wants to make? With modern technology, and the obsolescence of typing schools, it seems a trivial matter to switch from one to the other. If it is so superior, why does it not happen?

Since I do not myself do empirical work it is safe to appeal to reality. It seems to me that in the matter of determination from fundamentals an appeal to casual empiricism is inconclusive. As with the optical illusion picture where on one moment you only see the old lady and on the next you only see the young lady, so it is with reality: it can appear pervasively dominated by externalities, increasing returns and many other features capable of explaining the locking of the economy in a multitude of positions, or it can look as a majestic display of marginal adjustments pushing the economy towards one, or a few, coherent scenarios. The economics profession has certainly oscillated between the two visions. My view is that it would be exaggerated both to deny the existence of non-convexities and other locking-in effects or to consider that we study the classical, convex case only because we know how to do it (a reason which is, incidentally, less trivial than what we are making it sound). At any rate any extreme view in one direction is certain to bring a reaction in the other.

Perhaps, informally speaking, we can validate the substantive relevance of the GE outlook by recognizing that there is more than meets the eye on what we regard as “fundamentals” of the economy. For example, in Walrasian theory, the tastes, technologies and, most importantly, the list of commodities (for which, we should not forget, we are assuming the implicit existence of a grand coordinating scheme in the form of price quoting) may not be absolute primitives but rather the historically conditioned “equilibrium” of the non-convex side of the economy. It can be presumed that different aspects of theories of strategic, or of monopolistic, competition, and of the theory of games (e.g. coordination games) would be the ingredients for explaining, perhaps not as sharply, the equilibrium of this non-convex part.

4 Computational issues

It is safe to predict that in the future of economics the computability of economic theory models will play an increasing role. The availability of more and more data to pin down values of theoretical parameters and the computer revolution guarantee as much.

One must say that GE theory has been sensitive to computational issues. From the viewpoint of this test the theory looks good. The body of work of computable general equilibrium has flourished and it constitutes one of the success stories of the GE approach. In this respect, the contrast is notable with, what else?, GT.

The astonishing development of the pure theory of games has not been coupled with a parallel development in the computational front. This is hard to explain since up to a quarter of a century ago game theory was ahead. There may be intrinsic difficulties in carrying through a computation program for some of the more refined refinements, but I think that it has been mostly neglect and it is high time that game theorists put a remedy to this. I cannot think, from the standpoint of GT, of a task more important. There are, let me add, some recent papers, by Wilson and by Koller-Meggido. For the moment they are isolated but it is to be hoped that they are only the beginning of a flood.

Coming back to GE theory, it is evident that the computational side of the theory will not lose importance. One can forecast, but I'm out of my breath here, that CGT will merge with econometrics more than it has so far – I must be influenced by Dale Jorgenson here – and that it will pay special attention to dynamic aspects and to the computation of models with “imperfections”. Some of the computational challenges raised by dynamics are well known. As for the “imperfections” I may mention the recent work of Brown-DiMarzo-Eaves for the case of incomplete markets.

Finally, and since I'm on the topic of computation, I would like to digress and finalize with a few words about simulation vs. theorem proving.

The GE tradition has been distinctive in its emphasis on the need for rigorous proofs and this outlook has ended up permeating much of economics. At the risk of passing for a methodological reactionary I would say that the present and coming wave of simulation, while most welcome and enormously helpful, should not however entirely replace the drive to prove theorems. I do mean the “enormously helpful” part, it is not merely a rhetorical concession. Even if I belong to the group that derived some pleasure from the fact that the proof of Fermat's last theorem has been accomplished without the use of computer, by sheer and unaided intellectual power of the old fashioned kind, I will be the first to recognize that this is not the wave of the future nor should it be.

But, to repeat, theorem proving should still have a role. The importance of theorem proving in economics has derived in good part from the indecisiveness of the empirical appeal as a crucial test of economic hypothesis and theories. Certainly, empirical tests are important in economics but they are not, as a matter of fact, as decisive as they have historically been in physics. In a situation like this the test of logical consistency, theorem proving more generally, plays a substitutory role in enforcing the all important aim of intellectual discipline and orderly development.

Interestingly, I will also mention an article by the mathematical physicists Jaffee and Quinn¹ where they make a similar plea for physics. They observe that in recent years theoretical high energy physics has moved into a terrain (we could loosely code it under the name of string theories) where no empirical test is possible. This is a new world in physics (or so I do understand) and Jaffee

¹ Jaffe, Arthur and Frank Quinn. “*Theoretical Mathematics*”: *Toward a Cultural Synthesis of Mathematics and Theoretical Physics*, Bulletin of the American Mathematical Society, July 1993, Vol. 29, Num., 1, p. 1–13.

and Quinn are concerned that if the empirical test is not substituted by some other type of discipline the theory may, so to speak, spin out of control and be seriously impaired (they point out some interesting sociological facts: one way progress may be impeded is that research areas may be left stagnant in a mess of contradictory statements and half proofs for the simple reason that talented students may not want to be seen performing the unrewarded job of cleaning up somebody else's mess). They propose that the substitute for empirical tests should be the increase of mathematical rigor and the provision of mathematical proofs: mathematical physics as the reality test of theoretical physics.

Economics, in general (but some areas may be exceptions) is not in a situation of not being illuminated by empirical work. It is somewhere midway between the pre-strings particle physics, – the so-called standard model – with decisive empirical tests, and the string particle physics, with no possibility of decisive tests. Consequently, I would hope that the role of theorem proving is kept alive, perhaps not as strong as before, we may have overdone it, but with a substantial presence.