

HERAKLES POLEMARCHAKIS INTERVIEW WITH WEBPONDO
July 29th, 2003

Webpondo: We will start with the heavy questions and at the end start getting a little bit more personal. The first few questions are related to the paradigm of General Equilibrium. Carlo Benetti has argued that, despite its unrealism, the General Equilibrium Model (GEM) has become the point of reference of efficiency and market success, necessary for the understanding of the failures of realities that generate inefficient outcomes. According to Benetti, this normative method of neoclassical economics has eliminated any theoretical competition, since the other approaches to economic analysis, like Keynesian rigidities, turn out to be particular cases that can be described as market failures. What would you have to say about Benetti's argument?

Herakles Polemarchakis: I agree; I just don't find it objectionable. I don't know if Benetti means this as a simple statement of facts or whether he considers the situation of the science as sad and objectionable. I don't want to go as far as saying that general equilibrium (and by general equilibrium I mean the way it is practiced) is value-free: this would be an absurd statement. Nevertheless, it is a **flexible** model and it is correct that Keynesian arguments, among others, can and should indeed be addressed in a general equilibrium framework. Now, whether there are issues that general equilibrium cannot encompass or address, there may be. I'm not sure what they are, but an example or a discussion of, say, departures from laissez faire, that general equilibrium is incapable of handling would be very useful and very welcome I would have expected Benetti to put more emphasis on the absence of dynamics as a weakness in general equilibrium, but he does not in this quote.

W.P: There are two criticisms by Benetti that we want to highlight. According to him, it is hard to understand the actual consensus on the general equilibrium model as a good, abstract representation of market success of the price system and there is no theory of the price formation in the model. According to this argument, the general equilibrium model is not a good normative point of reference, invalidating all the results that have arisen from it through the normative method

that we described in the previous question. Do you agree in general equilibrium's lack of a theory of prices and its implications?

H.P: I don't understand the statement that general equilibrium does not have a theory of price, but if by that Benetti means a theory of **price formation**, this is true: general equilibrium is not a theory of price formation. Another valid point that Benetti makes, if I understand him correctly, is to ask whether it is a **fact** that market economies perform better than other forms of economic organizations. It is important to realize that no empirical study makes such a point. It is casual empiricism that, at the present, market economies seem to perform better than alternatives, but this does not make it a general fact, a law of economics. This is an issue that deserves serious empirical study, not by simple, casual observation. Furthermore, indeed Benetti is correct in saying that the theory of general equilibrium is a way of understanding, of looking at an economy: the claim that general equilibrium proves that market economies work better, is incorrect.

W.P: Continuing with more specific criticisms, Benetti argues that far from its pretension of being an abstract representation of the market success without the presence of institutions, the general equilibrium model has actually two major institutions that are necessary for its development. That is, the exchange institutions and the auctioneer. To this extent, the General Equilibrium Model represents much institutionalized economies. Can you tell us your opinion?

H.P: The model of general equilibrium is an abstract representation of a market economy; it is **not** an abstract representation of **market success**. And the abstraction is hardly institution-free. There are extraordinary institutions that are implicit in the formulation of general equilibrium: think only of the capital market, the number and complexity of financial contracts that are priced. The question is whether the institutions that general equilibrium often takes for granted are the appropriate or the relevant institutions; and Benetti's claim is that barter economies or centralized exchanges with an auctioneer may not be. Note, however, that, one cannot accuse general equilibrium of not having a theory of price formation and, at the same time, say that it presumes an auctioneer which is an absurd institution. One criticism or the other: either there is no theory of price formation, no auctioneer, and no dynamics of convergence or computation of prices; or, there is a theory of price formation, the auctioneer. I agree

that the theory of the auctioneer is absurd. But I don't think that anybody takes the fiction of the auctioneer literally, or even seriously. Of course this may be a change, following the failure of general equilibrium theorists in the 50s and 60s to obtain convergence results with an auctioneer. By now, the auctioneer is of no relevance. The fiction of the auctioneer is helpful, for pedagogic purposes, in explaining the logic of the general equilibrium formulation; it makes the presentation easier.

W.P: It seems that at the heart of this discussion is the testability problem in GE. Could you explain the readers what is the testability problem in economics and what progress has been and must be made along this direction?

H.P: The testability problem arises from the observation that, in its attempt to achieve a high degree of generality, the general equilibrium specification (and this is one thing we might discuss later: there are two words to general equilibrium: there is “general” and there is “equilibrium”) may be consistent with every possible empirical result. In order to be general indeed, general equilibrium may have constructed a paradigm that could explain everything, could account for everything and, as a consequence, from a simple Popperian perspective, nothing. If there is no possible observation that could refute the theory, then it is not good science. The results that have been proven recently, in a very abstract framework, show that this is not the case: that even if one stays at that level of generality, that does not restrict preferences, beyond the minimum of convexity or monotonicity, or production functions, it is **not** the case that arbitrary observations can be accounted for and rationalized. The basic intuition is related to Samuelson's axiom of revealed preference: if an individual, in a certain configuration of prices and income, selects bundle 1 over bundle 2, even though both bundle 1 and bundle 2 are budget-feasible, reveals a preference for bundle 1 over bundle 2; the axiom of revealed preference states that the individual shall not, in a subsequent choice, reveal a preference for bundle 2 over bundle 1. At the individual level, the axiom of revealed preference is indeed refutable. (Of course, there are difficulties: in order to refute the axiom one must be able to observe individuals with credibly the same utility functions, make different choices, at different points, with different income and prices. But, as long as one accept this, whether the axiom of revealed preference has been violated, whether individuals satisfy this property of revealed preference can be refuted.) Work in general equilibrium theory, starting with the work of Brown and Martzkin, and further

work by Chiappori, Ekeland, Kubler, myself, and more recently by Carvajal, has shown that the axiom of revealed preference carries through to the prices and income distributions that can obtain as general equilibria of rational individuals. In the process of aggregating individuals, in looking at equilibrium prices only, as long as there is sufficient variation in the profiles of income distributions in the data, the refutable implications of the individual axiom of revealed preference do not disappear. There are refutable implications of competitive general equilibrium. It is a different problem, to be addressed next, to come up with qualitative and quantitative specifications of these refutable implications; and it is a different question whether the fundamentals of the economy can be recovered, that is if they can be identified from the observable equilibrium behavior of a group of agents. The observations that are likely to be made by empirical economists are an issue: the work of the theorist is to specify a reasonable set of data that can be used to test the refutable implications which, in a prior stage, he has characterized.

W.P: Has problem of testability been addressed in a dynamic context?

H.P: It has. There is the explicit work of Kubler, which raises the issue on a dynamic, intertemporal framework; and, in a much related literature, the work of Duffie and Costantinides, about the properties of asset prices for arbitrary or general income processes, and also work by Krebs. These do not always lead to the same conclusion, but, in any case, all of them pose the problem in an empirically relevant framework. Of course, in order to make the question empirically relevant, one has to make strong stationarity assumptions about the underlying fundamentals; but this goes without saying: if everything changes, then no repeated observations are possible and the theory, no theory, is refutable, but this is not surprising or interesting.

W.P: Could you explain the readers what the transfer paradox is, the identification problem in economics and what are its economic consequences?

H.P: The transfer paradox, which goes back at least to Mill, Edgeworth and Keynes, among others, is the observation (clearly a general equilibrium phenomenon, because it arises from the adjustments in equilibrium prices following a re-distribution of revenues) that the redistribution of income across individuals is going to affect the

welfare of individuals through two channels: one direct and one indirect. The direct channel is that individuals who gain in revenue should gain in utility while those who lose in revenue should suffer a loss of utility or welfare. That is the direct effect. But the direct effect is not the only effect because there is a simultaneous effect which goes through changes in prices. Individuals transact in markets, and the prices at which markets clear reflect the demand and supply of individuals. If individuals are heterogeneous, a re-distribution of income results in a change in equilibrium prices. An individual gains in utility if prices of commodities that he supplies rise and if the prices of the commodities he purchases fall; in the reverse of the two situations, he suffers a loss of welfare. In a competitive specification, individuals are price-takers, and they do not foresee the effects on prices of their actions and, as a consequence, the direct effect on the welfare of the individuals due to the transfer of revenue may be diminished because of the change in equilibrium prices. This, I believe, was observed by Mill and Edgeworth said something along the same lines. Keynes pointed out that, by the same logic, an individual's gain because from an income transfer may be augmented because of the change of prices. Terms of trade effects may augment or diminish the direct effect of the income transfer. Leontieff, in the early 30s, constructed an example of an economy with two individuals and two goods in which the adverse terms of trade effect was such that it outbalanced the gain from the direct effect and, at the equilibrium following the redistribution of revenue, the donor gained, while and the recipient of income lost in welfare: hence the name Transfer Paradox. This bothered economists, as counterintuitive, and trade theorists in particular. Samuelson, in the Foundations of Economic Analysis, in 1947, dismissed the paradox in a footnote, arguing it can occur only at an unstable equilibrium, in the sense of the Walrasian Auctioneer which we discussed earlier. (Remember this was a time when dynamics was taken seriously, so results that obtained at unstable equilibria would be easily dismissed as irrelevant or uninteresting.) It turns out that the fact that the Transfer Paradox occurs only at unstable equilibria is an artifact of the restriction to two individuals. In an economy with multiple, more than two, individuals of whom some may or some may not participate in the re-distribution of revenue, the paradox may well occur even at a unique and stable equilibrium. Surprisingly, Samuelson thinks that the transfer paradox in economies with more than two people is of no interest; I do not understand why.

The identification problem, which we discussed in connection with testable implications and recoverability, becomes a relevant concern precisely because, if you want, the Transfer Paradox may arise. Suppose that it is agreed upon social policy to improve the welfare of a group of individuals, relative to what they would get at a competitive equilibrium without re-distribution of revenues. Then, the question is: what is the appropriate transfer in order to have the desired result of augmenting the welfare level of that particular group? As results from the Transfer Paradox, one cannot be simple-minded about it: and say "I shall transfer revenue to those I want to help, away from those who have agreed to let their welfare be reduced;" according to the Transfer Paradox, this may have the reverse effect. How is a policy maker to know what transfers to implement? Well, if from the market behavior of individuals one can identify their utility functions, then, in principle, he can answer that question. It is also important that the identification problem, as it is posed in connection with the Transfer Paradox, leads to the question whether it is incentive-compatible for individuals to reveal the information that will lead to the identification of their preferences. However, identification, here, requires here only statistical information about the economy: it is not necessary to name individuals. The joint distribution of preferences and endowments, determines the change in equilibrium prices due to the re-distribution revenue and, hence, the information that is necessary to identify the required transfers.

W.P: Let us move to some extensions of the general equilibrium model. Last year David Levine and Bill Zame published a paper in *Econometrica* with the provocative title: "Does Market Incompleteness Matter?" From a theoretical point of view, from the failure of the two fundamental welfare theories under incomplete markets, one would say that they do matter. What is your personal view on this?

H.P: Indeed, the work of incomplete markets and constrained suboptimality has obtained theoretical results whose empirical relevance should be subject of study: it can well be the case that the results turn out to be empirically irrelevant. And recent work on business cycle theory has argued to that effect: incomplete markets do not have quantitative effects of empirical relevance. But, this is not the argument of Levine and Zane: they argue over an infinite horizon, under the crucial assumptions that the rate in which individuals discount the future is small and with no production (I don't think the result would go through with production) market incompleteness, *even in theory*, does

not affect the average welfare of individuals. It's an interesting result which extends the work of Bewley on the permanent income hypotheses: self insurance may be enough for the welfare theorems, but under very strong assumptions. The important question concerning incomplete markets and constrained suboptimality is their empirical relevance, and that is, work that remains to be done.

W.P: Talking about incomplete markets, the results by Constantinides and Duffie and Krebs, which you mentioned before, suggest that in a dynamic economy with uncertainty there are no restrictions on the prices of assets. One would think then that the market incompleteness may explain facts such as the famous equity premium puzzle. Do you think this is an interesting application of the theory of incomplete markets?

H.P: The result of Constantinides and Duffie is that there are no restrictions on the asset prices processes, for particular income processes --- which Krebs has, recently, re-assessed. I take this as a negative result, of the kind that one wants to get away from, because it does say that explanation and prediction are impossible in an operative model. When Prescott and Mehra identified the equity premium puzzle they argued that equity premia that are observed are incompatible with market clearing, optimization and **reasonable parameter values**, in particular for risk aversion. I do not see that the equity premium puzzle is addressed by the results cited in the question. The question is whether one can rationalize the equity premia that are observed with reasonable parameter values for risk aversion, in a framework of incomplete markets, as opposed to a complete market framework. If I understand the empirical literature correctly, this is not the case, which is a criticism of incomplete markets along the lines we discussed earlier.

W.P: Moving to Game Theory, last year in a conference in Brazil, Robert Aumann made a sharp criticism to game theory. He argued that since the work of Von Neumann on two-person, zero-sum games, very little that is useful has been said, in the sense that there is nothing that tells the player how to play strategically. What is your opinion on the development of game theory?

H.P: Game theory, very much like general equilibrium theory, has provided clear thinking; to use an expression that Hahn used for Solow's work on growth theory, it "allowed *grammatical thinking*." After all, what was the contribution of Nash in game theory: above all else, was the notion of Nash equilibrium. The mathematics of his proof of existence was not a mathematical innovation: it was an application of a well-known result. The importance of the contribution was the realization that equilibria are fixed points. The major contribution of Harsanyi, much like Nash, was not a proof or a result: his major contribution was simply to explain how one should think of strategic situations where individuals are differentially informed. It is the language that allows us to think about a class of problems that is fundamental. I do not do game theory, I only use elementary game theory, and I hesitate to discussing or expressing opinions on the directions in which game theory is going. Nevertheless, one may doubt whether the emphasis on refinements of equilibrium in purely abstract, even definitional, frameworks is of much interest. Also, unlike economics, game theory suffers from the fact that it does not have the fallback of market prices and quantities, empirical facts on which to base intuitions and on which to judge the importance or relevance of results. There is of course an attempt, a recently very popular attempt, for game theorists to acquire the equivalent of prices and quantities that economists have at their disposal, and this is through experiments; but one can doubt whether the behavior of individuals in experimental settings reflects, accurately, how they would behave in actual situations. Furthermore, what is bad about this development is that experimental game theory is treated as an alternative to theoretical or decision-theoretic game theory and this is not the right way to go.

W.P: Herakles, let's play a futuristic game. If you were asked to give a short and informative talk on "The future of economic theory, what to forget and what to pursue?" what would you say?

H.P: A short answer: "What's the right way to do partial equilibrium?" This takes us back to the two terms in "general equilibrium:" "general" and "equilibrium." Unlike the development of general equilibrium in the 50s and 60s, since the adoption of general equilibrium by at least a school of macro-economists, in the 70s, the emphasis has been on "equilibrium," not on "general." Many, willing to consider simplified situations, without any claim to generality, nevertheless, insist on equilibrium, because of the

internal consistency of the specification. If one puts parentheses around “general” and particular situations, this may be the correct way to do partial equilibrium. One isolates a problem to study, only only it is clear what abstractions and simplifications are involved, what variables are endogenous and what are exogenous. Popper in his book “The Poverty of Historicism.” makes which is, is precisely a statement against general equilibrium: there is no point in stating general laws of history, because general laws of history cannot be refuted, and so, from Popper’s point of view, they’re of no relevance. Nevertheless, the experimental method for the empirical verification of laws that apply to partial settings is what Popper advocates and that’s what I mean by doing partial equilibrium: addressing specific problems.

There is another major problem that requires attention: this is the notion of equilibrium in intertemporal settings. Some criticize backward induction even for individual or strategic decision-making: centipede games are supposed to prove that backward induction leads to absurd conclusions, which are both intuitively unconvincing and experimentally, apparently, contradicted. I have no problem with backward induction at the individual level or when two individuals play a game; but at the level of competitive equilibrium, where individuals only look at prices and based on prices they make their decisions, the fact that equilibria are solved for by backward induction by the auctioneer, to use Benetti’s arguments again, is truly unconvincing. So, the issue is how to specify equilibrium in a dynamic setting There were attempts by Hicks and later by Grandmont with temporary equilibrium, but these attempts were very unsatisfactory because they closed the model in ad hoc fashions and did not at least try to argue that the comparative statics are to some extent invariant with respect to the arbitrary closure of the model. What I would like to see is a model where individuals base their decisions on what happened in the past and so does the auctioneer: in a dynamic setting individuals do base their actions in period T on what happened from period zero to period T, with or without uncertainty; the person who does not do this is the auctioneer, who computes equilibrium prices, and that I think this is a fundamental weakness of the notion of dynamic equilibrium.

W.P: What about ideas like dynamic recursive equilibrium? Isn't that a good notion of dynamic equilibrium?

H.P: Yes, but it applies to very restrictive settings. When equilibrium is recursive, it blurs the distinction between past and future, and backward induction is less objectionable. But we do want to allow for economies where there is change, and the environment has at least aspects of non-stationarity. Recursive equilibria are useful, but one cannot restrict one's attention to situations where recursive equilibria are guaranteed to exist.

W.P: To end with the technical questions, let us ask you something about the use of mathematics in economics. Often one hears people express concerns about this. To what extent do you think mathematics is the right language to address economic problems? Do you think we are overdoing with the use of mathematics in economics?

H.P: There have been excesses, but the harm done by those who have used mathematics to excess are negligible compared to the damage and the confusion caused by those who have opposed the use of mathematics. There is nothing wrong with the use of mathematics: its good, clear-thinking and, it is the way to do things. However, there is an issue there worries me: because of the level of mathematics that one has to control in order to go through a graduate program or do research, there is a tendency for curricula in economics to lose sight of economics as a social science; and to that extent, mathematics has had a detrimental effect. But this does not have to be the case: it is simply a matter of requiring future economists both to know mathematics and use it correctly, and at the same time to think of what they do as a social science and not as mindless applied mathematics.

W.P: We have few more questions which are just personal. Please don't go mad. You can always say "No, I don't want to answer this". As you might know Colombia has had huge problems with the underground production and commercialization of illegal drugs. I think you know that?

H.P: Yes, I've heard about it.

W.P: Wanting to contribute to the debate, Webpondo has been very curious on the opinion of top scholars regarding the penalization of illegal drugs. What do you

think about this? Just keep fighting the problem with probably better policies or legalize and regulate it?

H.P: *A priori*, I would say legalize and regulate. But there is a problem: I do not know to what extent society can convince people, in particular children, who matters most, that something is harmful, even fatal, which, at the same time, is legal.

W.P: But how about tobacco and alcohol?

H.P: Let us distinguish between “soft drugs,” whose harmful effects are comparable to those of tobacco or alcohol, and “hard drugs.” The previous statement referred to the latter. When it comes to soft drugs, indeed, I would say legalize regulate and inform --- “educate” may be paternalistic. To legalize and regulate would be what I would favor for hard drugs as well, but I am worried about the message getting across.

W.P: Would you like to tell us who have been the most influential people in your career, intellectually speaking?

H.P: In economics, it was Kenneth Arrow. I learned from him, what it is to think clearly and to work from first principles. His paper on the role of securities allowed us to think coherently about the social allocation of risk. Beyond economics, my main interest and source of inspiration has been in politics

W.P: Can you tell us more or less what kind of political problems you are interested in?

H.P: The late 60s and early 70s was a time of a dictatorship in Greece and then political concerns was very well defined: to get rid of an authoritarian, violent regime. Such problems are faced by many countries still today, but many countries have grown beyond them. So, the problems that I find interesting right now, beyond the application of economic theory to policy making, are humanitarian. There are issues that bother me strongly, like the issue of immigration. I wonder if one could “compute” the economic impact, world-wide, of lifting all immigration restrictions. Immigration restrictions bother me as restrictions on the free choice of individuals. I am also thinking about

ways that children should be brought up: if one looks at the available spectrum of parents one realizes that equality of opportunity for children is a hopeless task.

W.P: Do you like the current work on political economy?

H.P: From the bits that I've seen, I am not impressed with it. Asking important questions is not enough; one should also provide interesting answers. When a reader finishes a paper, he should have the feeling that he understood something that he did not understand, had not thought about before; the reader should have the "aha reaction" that Steve Weinberg has identified; a descriptive repetition of an important problem is not enough. Many of the papers that I have seen in political economy suffer from this. Now, that is not to say that political economy is not an extremely interesting subject. By now economics, and this is may to make Carlo Benetti even more mad, is the generic social science: whatever is interesting and well done in the social sciences is economics. And there are important issues in the interaction between economic and political considerations and fascinating work to be done.

W.P: OK, you're about to leave tomorrow, actually. Do you want to say a few words on you impressions, on what yo u think of Bogotá. Would you like to come back? How about the people?

H.P: Of course I would like to come back. At the same time, I know next to nothing about the country: I have been here for just a week. What impressed me most was the colleagues I met, at the Bank and at the University: genuinely interested in what they do, doing it seriously and with excitement and not worrying about things that really don't matter.

W.P: Which are those?

H.P: Impressions.

W.P: Thank you very much.