

An Interview with Frank Hahn on the Occasion of His 80th Birthday

Conducted by Marcello Basili* and Carlo Zappia*

Frank Hahn received his Ph.D. from the London School of Economics in 1951. After lecturing at the University of Birmingham, he moved to Cambridge as Lecturer at the Faculty of Economics and Politics and Fellow of Churchill College (1960-66). Having obtained a professorship, he taught at the London School of Economics (1967-72) and then returned to the University of Cambridge (1972-92). On his retirement he was appointed Professore Ordinario at the University of Siena, Italy. Here he directed the doctoral programme in economics (1990-1996). Hahn is now Emeritus Professor at Cambridge.

The interview that follows mostly deals with Hahn's view of economics. Hahn kindly accepted to discuss both the theoretical aspects concerning his contribution to the development of general equilibrium theory and the methodological issues inherent in his viewpoint that theoretical models can prove useful in the study of actual economic phenomena. A few notes related to his period at the Economics Department in Siena close the interview. The interview was conducted in Cambridge in January 2005.

*Some of the themes discussed in the interview are the subject matter of two papers written by Hahn in the early 1990s: "Autobiographical Notes with Reflections" (in M. Szenberg (ed.), *Eminent Economists: Their Life Philosophies*. Cambridge: Cambridge University Press, 1992), and "An Intellectual Retrospect" (in Banca Nazionale del Lavoro Quarterly Review, 1994: 245-58). As regards Hahn's views on the meaning of economics, his most interesting work is probably "In Praise of Economic Theory" (in *Money, Growth and Stability*. Oxford: Basil Blackwell, 1985).*

A website introducing Frank Hahn's work can be found at the following address: <http://www.econ-pol.unisi.it/hahn/>

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Question: You have been called the “perennial renegade”, being virtually the sole neo-Walrasian member of a Keynesian department. What was your feeling about the Department of Economics when you first arrived in Cambridge?

Answer: In 1960 people at Cambridge Economics Department were mostly left-wing. And the intellectual content of the left-wing in Cambridge in those days was technically non-existent. They themselves were Sraffians more than Keynesian, and in part inimical to Keynes. Luigi Pasinetti for instance, a charming man, but he interpreted Keynes wrongly. Keynes was a neoclassical because of how he dealt with the real wage system in the General Theory. But then he introduced money and therefore he was correct not to accept Say’s law. But Say’s law is of course true if there is no money. There is no question about it: if there is no money, and I offer my labour then I must demand something else. But of course it is not simultaneous, and this is where Keynes departed from neo-classical economists. There had to be dated goods, and so on. But we all know the answer to that.

Q: When one reads your papers of the late 1950s and 1960s without taking into account that there were the so-called left-wing Keynesians, many of your themes seem to be Keynesian.

A: Oh yes. I remember Hayek asking me whether I am a Keynesian and I said sure! By the way, Keynes made great mistakes. The trouble is that he was a practical man and he was not very happy with theory, and also by the way that although he studied mathematics he was not a good mathematician. And I think that to be a good theoretician you have to be good at mathematics.

Q: As regards the general equilibrium research programme, when did you start working on it, when did you start collaborating with people like Arrow?

A: Well, I do not recall when I started collaborating with Arrow, but the point is that I became interested in general equilibrium theory quite early on. I was visiting professor at Berkeley and I had published an article in *Econometrica* on the dynamic stability of general equilibrium in 1958. So I was very interested in it from the beginning. You know, people think of me as a general equilibrium theorist!

Q: Regarding the development of general equilibrium theory, which is the topic you contributed that you consider to be more relevant, which the more fruitful idea among the ones you worked on: stability, transaction costs, the role for money and expectations?

A: I think the “missing markets”. I realised that there could not be an argument about the mechanism for making expectations relevant if there were a

complete array of markets. If certain insurance markets and markets for future delivery are missing, in the economy there can be trading at every date, and this requires agents to formulate expectations.

Q: When you talk about dynamics you concentrate on the requirements for the adjustment to take place. Which is the most relevant problem concerning the adjustment process?

A: Information, in particular about other prices. I mean, I do not know of the price of butter in Holland. One is certainly ignorant of many things. When I used to lecture, I do not lecture any more of course, I said that what economics has done is to assume away all the problems with economic theory. If you do not have complete markets, you have to be informed of prices and of course not everybody is. You know, you and I expect different prices. Moreover you have to know relative prices. The problem with economic theory is that we are expecting too much of people.

Q: In the paper that you read at the meeting of the Società Italiana degli Economisti in Florence in 1995 you quoted Gilboa and Schmeidler's non-additive approach, may be for the first time, as a relevant contribution to explain how multiple equilibria can emerge in an uncertain environment.

A: I was really impressed by the paper by Dow and Werlang in *Econometrica* where they discussed an example of the non-additive approach of Gilboa and Schmeidler. The way general equilibrium theory went it solved the uncertainty problem by taking steps that it could ignore by assuming that there were full markets which cover all states of nature. And then there is no problem. Any attempt to move forward is worth considering.

Q: Do you think that the non-additive approach is a way forward?

A: Yes, I do. But I think people at Siena Economics Department made too much of it. What Schmeidler does is indeed that he takes account of ignorance, and there is a definite kind of uncertainty on that score different from the one we are used to. But I do not quite know what to make of it. Because, all right it is true, but are there any real long term effects that you can simplify in some way, that you can theorise? I really do not know. By the way, one has to make up one's mind that is very damaging for economists to assume that an agent can predict long term outcomes. For instance, Lucas assumed that things turn out to be organised over time, but this makes nonsense of indeterminacy. Moreover in the long run we are all dead, that's what Keynes said and I think it is a very remarkably insightful remark. I think that, if you want to listen to my view, the world is a sequence of short runs, but nonetheless the short run I refer to is when markets clear, because we do not want to have rationing in the short run.

Q: Scholars not familiar with general equilibrium theory may not be accustomed to the notion of indeterminacy. Would you be so kind to explain what you mean by indeterminacy?

A: It's very important for stability analysis of course. The first point, which is the simplest one, is of course that equilibrium itself is not unique. The second point is the following. Suppose that there are multiple equilibria. You use the property of homogeneity, and you say, well if prices double everywhere then there will be no change. But it is not clear that this is true if you work on a system when reaching equilibrium: if people haven't adjusted themselves to the changing prices you may go to some of the other equilibrium.

Q: You said once "Walrasian theory is not useless but severely incomplete", and also you added "the process of learning and adjusting – indeed history – will need to be brought into the story".

A: That is true. The learning stage cannot be skipped as many macroeconomists do. To use simple textbook economics does not seem to me to be appropriate.

Q: But our question is: is the point you are making in these statements something that you endorsed from the beginning of your career or is it an attitude of the last years?

A: I can't date it, but it is not from the very beginning because I came from mathematics and I was very impressed by the general equilibrium. But pretty early on I had the view that you quoted. For instance, the results on expectations and learning by Margaret Bray made the statement obvious.

Q: We read in one of your papers that selfish interest is not adequate to represent human behaviour. Many economists talk today of trust, reciprocity and so on. Do you think that these may be part of the individual's utility function or relationships among individuals is only a question of reducing costs, information costs, transaction costs and so on?

A: The assumption that humans are driven by greed is a very simplifying assumption. But it works very well. For instance the symmetry of the substitution effects and the possibility of distinguishing the substitution effect from the income effect are interesting outcomes of the analysis. But it is true of course that we get a characterisation of human agency that is not descriptive of any particular person's behaviour. But it is a very good theory, because it is useful. I am not saying that this is what happens in the world. But I am saying that it is a very good assumption because it allows us to get on this job. We had Daniel Kahneman giving the Marshall Lecture recently here in Cambridge and he made a case that we are liable to make mistakes. But now we run the risk of being too realistic about human conditions.

Q: Let us turn to the methodological aspect of your work.

A: I hate methodology.

Q: Yes, this is well-known. But the fact is that you are much quoted by methodologists. For instance your colleague Tony Lawson wrote in his book on realism: "I have focused on Hahn's views of the situation because he is one of the very few contemporary mainstream economists prepared to reflect at all explicitly on what is done". Can we say that you are a representative of the deductivist tradition in economics?

A: I never understood that properly. Using deduction ... I do not see how else to work. This is how humans behave. They use logic. Kaldor of course was criticizing very heavily people like me and so on for building castles in the air, or that we were making science fiction. There is something true in it, but at least we built something.

Q: This still is a big question. For instance, Deidre McCloskey says that economics is a kind of abstract art, but now has turned to an intellectual game.

A: No that is not true. Let me give you an example, which I am afraid reflects very well on me, but one that really impressed me. I was put on a committee of, let us say, fifteen people. It was set by the minister on how much to spend on research in different disciplines. I used all my economics, and I was the best of the group. They asked me not to go away. I was appointed for n years. And they wanted me to stay for $n+1$. It was very easy because nobody had a framework in which to think.

Q: But the problem with the deductivist tradition is how to deal with axioms. You once said: "the assumptions are there to enable certain results to emerge and not because they are to be taken descriptively". But one way to move forward in deductive reasoning is to make less stringent assumptions, and this often means making more descriptively accurate assumptions. Is not this contradictory?

A: Yes, but you see the point: I have found, and I think you have found by now, that the more descriptive you are the less you can do. For instance, I have read quite a bit recently in my illness about quantum mechanics. It is easier to be realistic about the behaviour of sub-atomic particles than it is about sub-atomic members of society. I really think that we should stick with any abstraction which is useful, until it is shown that it leads to a complete mistake. And I do not know whether people like me make more mistakes than people who do not have this attitude. Suppose I were lucky enough to be asked by Toni Blair to give him advice on economic policy: I would said no.

Obviously I would be delighted to be asked, but I could only supply Blair consistency. On the other hand I am confident we would avoid the most tragic mistakes.

By the way: you have not asked me whether I am happy to be an economist. I am not sure that I am. I would have liked to have been a theoretical astronomer, and not because as an astronomer you cannot feel the pressure of reality, but because you can theorise and the reality can catch up with you.

Q: We are close to the end of our interview. Is there a Hahn's school around?

A: No. I mean, I have got followers. It is true that I receive three or four supportive interesting letters each year. And I am happy about that.

Q: What about your period in Siena. Were you "basking in the sun" as one can read in your biographical notes at the New School web site on the History of Economic Thought?

A: Siena was lovely. But I was not basking in the sun, first of all because my wife Dorothy and I discovered that Italian weather can be as bad as English! Siena Economics Department is a very civilised environment. I have to admit that the degree of technical proficiency I found was not as good as I expected. When I was there a number of new *ricercatori* were appointed, and sometimes they were not technically as good as they should be. But all the same Siena was a very nice interlude in my life. There are people I really admired in Siena.

Q: Together with this interview will be published your talk at the Siena Meeting for the foundation of the European Society of the History of Economic Thought, held at the Certosa di Pontignano in 1995. You pointed out a few times your lack of interest in the history of economic thought, and your talk starts from this point. Would you be so kind to summarise, for instance, why, as you once said, a study of what Ricardo meant by value does not engage your interest.

A: Well you see, the point is this. Ricardo was for his times, a clever little man. I can see why people might be interested in him. But I am not. As I said in the talk, I am interested in understanding the world instead of understanding the manner someone else understood it. Ricardo looked for the standard of value, but there is no such thing. I used to have dinner with Sraffa once a month. Sraffa never came clean on that. It simply is a question of a numeraire. And this is why I am not really interested in it.

Q: Is there anything in the talk you would amend?

A: No. Not really. I really was delighted to read it.