Haavelmo – a low key heterodox?

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Introduction

The aim of this paper is rather modest, it is to discuss the challenge that the work Haavelmo poses to Generel Equilibrium theory, which I argue has been overlooked, by both friends and foes. It is symptomatic that the Nobel Prize press release devoted only the last paragraph out of eleven on the Haavelmo's contribution to a critique of neo-classical theory, despite the obvious fact that after his early work in econometrics Haavelmo, from the late thirties to the late forties, Haavelmo did not devote any research efforts to econometrics.

In the above mentioned press release this fundamental change of focus is described in a rather obscure way, stating that "Once the foundation of probabilistic econometrics had been established, Haavelmo's next important research effort involved attempts to transform various components of economic theory so that the new econometric methods would be applicable. According to Haavelmo, the prerequisite for achieving this purpose were not only additional assumptions about probability distributions, but also in many instances a more dynamic theoretical formulation."

This cryptic statement "undercommunicates" that a central theme in Haavelmo's work was the study of the *dynamic* stability of a economic model. He was fundamentally sceptical to generalize the results from static equilibrium models to such a fundamental dynamic system as a capitalist economy. This paper will use a short, non-mathematical article entiteled "What can static equilibrium models tell us?" written originally in Norwegian in 1958 as a focussing device for a discussion of if – and in what way Haavelmo can be called an heterodox economist. The paper will also discuss why Haavelmo's critique of static equilibrium did not have a wider impact. First of all no response from his peers, and he has not served as an inspiration for new generations of heterodox economists – like for example Schumpeter.

The fact that Haavelmo published most of his later work on economic dynamics only in Norwegian, makes is of course one of the reasons why he have not been more influential. Titles like "Production dynamics", "Dynamic price theory" clearly indicates how central the question of dynamics was to Haavelmo. The paper concludes that both the insights and the

prestige of Haavelmo in the economics profession are useful in the work for a break with the static paradigm, which is the key to bringing economics back into the family of the social sciences. Here Haavelmo with his wide interests in the problems of underdevelopment, of the environment clearly wrote in a non-imperialist way, proud of the tools of economic reasoning, but open to other approaches. It is also necessary for heterodox economics to see the limits of Haavelmo's critique of static equilibrium, which in this author's opinion explains why he has not become a source of heterodox inspiration.

Economics and the other social sciences – the fundamental source of conflict?

The critique of the majority of social scientists – and a large part of the public - towards economists are well-known. They regard economics as to "narrow", to "un-realistic", to "abstract" and "using to much math's", to "atomistic". All these criticisms are correct in a way, but economists can with certain right argue that all modelling involves abstraction, heroic simplification, that maths in themselves are neutral – and sometimes powerful. Far to seldom the criticism is directed towards the fact that the models used are *static*. Which most economists had to admit that they are, but would claim that they although very abstract and "unrealistic"– are real models, or "real Abstraktion" in a Marxian sense, i.e. models that capture the essential aspects of the object of study. But that last claim can be dealt a death blow by asking if the major "results" are can be proved in a dynamic framework? Does the economy converge to this particular equilibrium point if is "disturbed" by new inventions leading to new innovation? An honest neo-classical economist has to admit that this is not the case. So far no neo-classical economists have been able to prove the stability of the general equilibrium as soon as you relax any of the assumptions[1]. This brings us precisely to the question that Haavelmo posed:

"What can static equilibrium models tell us?"

This small, eight page article was first published in a "Festschrift" [2] to Fredric Zeuthen, a famous Danish economist in 1958. It was published anew in an English translation in *Economic Inquiry* in 1974, but did not get any response from fellow economists to my

[1] See for example Fisher (1983) and Currie and Steedman (1990)

[2] Supplement to Vol. 96 of *Nationalökonomisk Tidsskrift, 1958*

knowledge. Haavelmo introduces – in the original – the paper as a tribute to Zeuthen as a patient teacher and devotes the paper to a "… purely *pedagogical* discussion of some rather fundamental problems of economic-theoretical interpretation…". It is precisely because the paper does not use a lot of mathematical notation that it in my opinion so clearly highlights the fundamental problems of static equilibrium[3]. Haavelmo's starting point is to

> "… discuss how fantastically complicated the argument that price and quantity are determined by the scissors [market cross] really is, even if one accepts the most hard-boiled assumptions about market behaviour. […] In its naked simplicity, the well-worn picture of the intersecting curves is still the most important – and perhaps the only – rational foundation that one has to stand on if one wants to believe in the automatism of the free market."

Haavelmo then repeats for sake of argument the text-book logic behind the supply and demand curve, sellers and buyers acting like quantity adjustors, and goes on:

> "What is then so wrong with the proposition that the 'price will be where the curves intersect each other'? Only this: there is of course, not an iota of information in our behaviour scheme for buyers and sellers about how they themselves would "find the market price". Suppose we let buyers and sellers loose on each other under the presumption that a given market price will rule, and they then find that that isn't the case? What will they do? Even if they where to act quite sensibly, in *no* way whatsoever could their behaviour be deduced just from the information that the supply and demand curve gives us."

Haavelmo then goes on to discuss "the usual and familiar answer … that one would have to formulate the theory dynamically" and replies that what is important is not dynamics per se, but that we have to say something more about "how the market price emerges".

Haavelmo goes on to propose that:

> "The conceptual apparatus of game theory could conceivably be used to construct such a model. But which assumptions should one then make about contacts between sellers and buyers, about their negotiation strategy, about their knowledge of the market, and so on? Here

[3] Similar thoughts, but less developed – and the same starting point with the well-know scissors of supply and demand is found in Haavelmo's review of Bent Hansens book "A study in the theory of inflation", Haavelmo (1951)

> the possibilities are obviously endless. One thing is in any case certain: a vague postulate of "many buyers and sellers will not suffice to determine how this game should proceed."

The industrial organisations literature has indeed borne out Haavelmo's prophecy that the possibilities are endless. The results regarding market structure, prices etc. are very dependent on small changes in the assumptions of the rules of the game, (non) co-operation, repeated games, time horizons etc. [4] He further comments that for the game to be static and at the same time to "reflect practical possible behaviour" the buyers and sellers would have to find the market cross at "their first try". Haavelmo dryly comments that "Presumably we would find that the buyers and sellers taking part in such a game would have to be some remarkably well-informed beings."

Haavelmo then goes on to discuss the usual answer to the difficulties: "just make the theory dynamic". Haavelmo responds: "That answer however, seems to come very close to saying that the demand-supply cross is indeed a fine thing; it's just that it cannot answer any of our questions!" Haavelmo points out that text books that tells a story that when prices are too high they will fall and if they are to low they will rise, but as Haavelmo points out, too high or low in this context "are expressions that are given their meaning by reference to the demand-supply cross" – and it was where they would intersect that was the original problem! Haavelmo rounds up his small article by discussing the development of the general equilibrium model pointing out that after Walras the existence of a meaningful solution has been the focus in recent years and that the demonstration "that such solutions exists under quite general assumptions is considered one of the greatest triumphs in the area of general equilibrium theory. " But he continues:

> "As is well known, that Walrasian general equilibrium model may be assumed to have certain "optimal" properties according to a definition due to Pareto. Seemingly, all that was lacking was a demonstration that the system actually possessed a feasible solution. Since that has now been put in order, all might seem to be well. But there is a problem with the dynamics when the system is found "of its equilibrium point". So far, economic theory has, I think, treated the latter problem with somewhat less respect than it deserves. The system's dynamic motion has been regarded as no more than an appendix to the static model – and appendix of such sort that if only the *static* model has a certain form, prices and quantities will be drawn to the equilibrium point. What has been said above should give reason to be careful in making the

[4] See for example Laffont and Tirole (2001), Norman and Thisse (2000) and Motta (2004)

> claim that the solution of the general equilibrium model shows what will actually happen in a freely competitive market system."

As is seen the key component of this argument is the lack of an analytical framework on the behavioural foundation of micro-economic theory. To address the questions raised by Haavelmo requires an extended theorizing about the behaviours of economic agents in a situation not characterized by equilibrium – of non-equilibrium economic dynamics.

The farewell to econometrics

First off all, why did Haavelmo abandon econometrics? I think the following quote from "Economic analysis. A study in the theory of economic evolution" is sufficient to explain why:

> "As another example, consider econometric research upon production functions. We want to find the effects upon output of changes in the factors of production. But our 'purely economic' theories are built to fit a market where, if all the assumptions we make are approximately true, there should not in any case be very significant changes in input and output. The variations that we observe in such a market are mostly the results of the market failing satisfy exactly the assumptions of the theory! How could we expect to verify a theory from such data? (Haavelmo (1954), p. 5)

The same point of view is repeated in Haavelmo's presidential address to the Econometric Societey in December 1957:

> "…we find a much larger volume of theoretical literature studying effects of all kinds of hypothetical partial variations in prices, interest rates etc. but these variations are often *impossible because they are contradictory to the general deterministic model* within which the variations are supposed to take place. The problem shows up clearly when we try to do quantitative work on the equations of such models. Then we are forced to ask why the observed data show variations, or what freedom of variation they can have if the model under consideration is true. (Haavelmo 1958, p. 353, Haavelmo's emphasis)

In the same presidential address say that it is too easy to "as always, complain about bad statistical data. However, I think we may well find part of the explanation in a different direction, namely in the shortcomings of basic economic theory, and in the somewhat passive attitude of many economists to the choice of axioms and economic content of the models we

work on. (op. cit. p. 355) Haavelmo argues that the econometrician should do less "repair work" and that there is a "need for more emphasis on economic theory".

Haavelmo did let deed follow words, after returning to Oslo after the War in 1947, Haavelmo did practically no econometrics. Instead he went into almost all the major fields of economics[5]. This was in no way accidental, Haavelmo retained his view that before econometrics could really progress; economic theory had to change radically. As he said in his Nobel Prize lecture:

> "It is quite obvious that if the theories we build to simulate actual economic life are not sufficiently realistic, that is, if the data we get to work on in practice are not produced the way that economic theories suggest, then it is rather meaningless to confront actual observations with relations that describe something else. If I were asked today for an evaluation of the kind I have mentioned, I would probably use almost the same words, but I would give them a more drastic content. I have had plenty of time to think about the matter since the time when I gave the address [presidential address to Econometric society] that I have just mentioned. (Nobel Prize Lecutre, 1989, in Mäler (1992))

In the same 1957 presidential address he also touched upon the question of stability. Haavelmo pointed out that the more refined the econometrics got, the clearer it became that there was something fundamentally wrong with economic theory, because "on closer inspection it was obvious that the answer to the question of stability would depend on the numerical values of a multitude of economic parameters." (op. cit. p. 351)

The research programme of Haavelmo – dynamic versus static analysis

As can easily be seen from the titles of Haavelmo's books, articles and lecture notes, dynamics are central[6]. There is of course nothing wrong with static models per se, but as Haavelmo repeatedly underlined, they have to prove that they are true special cases of a more *dynamic* model with known stability properties[7].

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[5] For very informative overviews of Haavelmo's contribution to economics, see Moene and Rødseth (1991), Thonstad (1990), for an overview of Haavelmo's "econometric period" Bjerkholt (2001)

[6] See 'Bibliography ofTrygve Haavelmo's Publications 1938-1987," Scandinavian Joumal of Econamics, 1990, 2, 25-30 for a complete list of Haavelmo's scientific publications.

[7] One example being Haavelmo's treatment of international trade, where he writes: "Some problems regarding external trade can be discussed in a static framework, while others demand dynamic analysis. … Logically we should start with dynamics, and discuss statics as special case, but we will do the opposite of didactic reasons, to start with what is analytically simple." Haavelmo (1954)

The Haavelmo's dynamic research agenda is clearly indicated by the titles he uses. He writes "Dynamic price theory" 1951, "Dynamic theory of production" (in Norwegian), "A study in the theory of economic evolution" (1954) or "Economic equilibrium and economic welfare" (1949) or one of Haavelmo's latest works "On the dynamics of global economic inequality" (1980) written after his retirement in 1979. This just to mention a few titles where the word dynamic is part of the title. But in his other areas of interest: international trade, welfare, pollution, expectations, choice of economic system, general market theory, unequal development, distribution – dynamic models and reasoning are always part of the analysis. Haavelmo was indeed not of a polemical nature, but the critical remarks on static models and reasoning was an integrated part of his research programme. He was especially critical of the use of intellectual resources on some topics and not on others. In his analysis of underdevelopment he writes: "We have countries where income per head has doubled or tripled during the last hundred years while in others there has been only slight progress, or stagnation. And yet, there are probably not as many pages of modern economic theory available on these mysteries as there are books analysing short-run equilibria … (Haavelmo (1954), p. 4).

It is far beyond the scope of this article to analyse Haavelmo's the substantial contributions in various fields of economic, but that he – in his humble way – had the ambition to show the need for and the analytical strength of dynamic models is in my opinion beyond doubt. Let my try to illustrate this with some quotes from his major works. One of them is clearly "A study in the theory of investment" (1960). In the opening chapter "Survey of Problems" Haavelmo writes:

> "As I tried to dig deeper into this problem, seeking to clarify the implications of the per-unit-of-time dimension of investment, I found that the problem was intimately tangled up with the notion of capital as a factor of production, and, in a more general way with the field of *dynamic production theory*. Here I found myself involved in a section of economic theory where it seems that the literature is very scarce. (p. 13)

As a didactic device Haavelmo did very often accept as a starting point "the most hard-boiled assumptions" of neo-classical theory – only to show that this created irresolvable inconsistencies, because you jus cannot grasp a dynamic reality with static concepts. Starting from these hard-boiled assumptions Haavelmo in his "Concluding remarks" on investment theory writes:

> "The "demand" in this case is for a stock of capital, whereas the supply is a flow. In this dimensional difference between the demand and the supply side lies the main problem of an equilibrium theory of investment. Here lies, perhaps, also the key to an understanding of why actual processes of investment, in the short run, may be wild and, perhaps, unpredictable. (p. 215)

The need Haavelmo felt for paradigmatic change – from static equilibrium models to dynamic models – is perhaps most clearly expressed in his discussion of Keynes. Let there be no doubt that Haavelmo was a traditional social-democrat with an intellectual openness. He never regarded capitalism as the end of history. Given his left-wing inclination it is may by surprising how critical he was of Keynes from a theoretical point of view. The following quotations are from notes taken from Haavelmo's lectures in the spring semester of 1977. Letting others make extensive notes from his lectures was Haavelmo's usual way of getting his thoughts down on paper. The note-takers where very loyal to the essence of Haavelmo's thinking and Haavelmo always were in dialog with the note-taker on the final text – sometimes allowing extensions, new examples etc. In these lectures entitled "The Keynesian Revolution" Haavelmo writes:

> "Instead of starting directly with the problem, how the high level of unemployment – around 30 % - should be explained, Keynes had a tendency to start with the problem of how such a level of underemployment should be explained in a neo-classical model. […] "The economic system that the neo-classical models simulated seemed to many to have such desirable characteristics that one wanted to go back to this point. The economist let their wishes influence their view of how the economy worked: since the classical[8] world was good, the classical model had to be a good description of the observed economy. In that case one could not avoid the question how one in this scheme could get 30% unemployment. […] The capitalism of today and of the thirties are characterized by a large part of production is used for investment, which demand growth if the produce is going to be sold. Keynes focused on a dynamics demanding phenomenon which could not be thrown light on by static exchange market model of the classical type. […] In reality he [Keynes] came close to building a separate theory for employment and investment … , but since he in contrast to Wicksell did not build

[8] Haavelmo several times in footnotes states that he uses the terms classical and neo-classical for static models.

an explicit dynamic theory, he was always in a logically dangerous zone, and that created a lot of confusion.

There is no doubt that Haavelmo politically was "Keynesian", that he wanted a more fair income distribution, was completely pragmatic about subsidies and deficit spending. But methodologically he was always critical of static models trying to explain dynamic phenomena.

The limits of Haavelmo's research agenda

In my opinion there are very many dynamic insights in Haavelmo's work and heterodox economist would benefit from his insights on whole range of important topics. But there is in my opinion there is one rather surprisingly underdeveloped area: the dynamics of competition itself. To my knowledge Haavelmo deals with this topic at length only in two lecture notes. The first is from 1973, "Production dynamics". The starting point is a problem given to graduate students at their final written examination in the spring 1972: "To what extent can production theoretical approaches be used to explain the current tendency of firm mergers". As one can easily imagine the students were not theoretic well armed to answer this in Haavelmo's words "dynamics demanding" question. The lectures on "Production dynamics" was clearly intended to change that and there is as usual a lot to learn from Haavelmo's "pedantic" – to use his own characterisation – way of analysing. But there is clearly in my opinion something missing. Haavelmo restates the "old and famous phrase, called Proudhon's statement, which says that competition has a tendency to kill itself […] in the end we get monopoly and competition fades away" (p. 88). But in his explanation of this phenomenon Haavelmo does not see technological change as something driven by competition itself, that without the *endogenous* creation of new products firms looses in competition. Haavelmo does neither see the tendency towards greater plants (= increasing returns to scale) as a necessity created by competition itself. This, despite insights that runs counter to the conventional wisdoms of the benefits of many firms in the market. Haavelmo writes inter alia: "We are seeing that if a single producer which is small in the market finds a smart improvement of technology which he manages to keep for himself, he will certainly benefit from that", but he does not follow this insight to its logical end, i.e. that each firm is a monopolist in spe, that the best way to get a monopoly is to drive the competitors out of the market by a better and cheaper product produced by increasing returns to scale. It is beyond the scope of this paper to give a nuanced assessment of the limits of Haavelmo on this point. Relative to other

economists at that time – and still – Haavelmo goes deeper into this – taboo area than most economists before and after[9]. In his recent book Baumol (2002)[10] claims that besides Marx, Engels, Schumpeter and a few economic historians, no economists have clearly formulated the relation between competition, monopoly/oligopoly and welfare creation. In short that competition creates monopoly/oligopoly. This is the reward for being innovative. Since it in most cases builds on increasing returns to scale, prices are lower and volume of production higher – totally contrary to the traditional myths about the "monopoly case" of economic text books. There is also in Haavelmo – as in most other economist writings – an implicit assumption that there is anything of the beneficial or evil mechanisms that we normally connotes with competition in "perfect competition"[11]. Real competition is essentially all the things firms do to avoid "perfect competition", i.e. that homogenous products are compared using price as the only decision variable.

Conclusion

In the title I posed the question if Haavelmo was a low key heterodox, and in my opinion he clearly was heterodox in his rejection of static models as *the* tool of economic analysis. On the contrary he worked very intensively with dynamic models – without a personal computer that could churn out numerical solutions, investigate chaotic phenomena etc. But Haavelmo is very instructive in that he points to the static nature of "perfect competition" as its Achilles' heel, not primarily the unrealistic perfect nature of the preconditions. Haavelmo was certainly low key as a person, shy, not polemical at all. He called himself pedantic, his student and later colleague Bjerkholt calls him "Socratic" – and I think that is a better characterisation. But he was also low key in another sense; his critique of the static nature of the neo-classical models was always accompanied by statements that they contained valuable insights etc. And this points to the limits of Haavelmo's critique. He did not analyse thoroughly the dynamics of the capitalist competitive process. My feeling is that he did not do that because he never - although he clearly saw the insufficiency of static models, he never discussed if it general equilibrium should be considered to be more an ideology, than a scientific theory. He saw dynamics as something more advanced – not contrary to static models. In a mathematical

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[10] Baumol is clearly overlooking the evolutionary economists, see Fagerberg (2004) for a more balanced point of view.

[11] See Morgenstern (1972, p. 1164) for a excellent discussion of the misleading use of the word competion

sense this is of course true, but in the history of economic ideas one can clearly argue that neo-classical theory did get its extreme static – and "anti-dynamic" nature, not because its founding fathers wanted it to be so utterly divorced from reality, but because that is a mathematical necessity if you want to prove mathematically the well know neo-liberal "results" that you need against unions, deficit spenders, environmentalists, supporters of more egalitarian wages etc. etc.

In fact general equilibrium is a theory of *perfect stagnation* - not competition - since nothing changes, neither prices nor technology. From a scientific point of view it is impossible to use such a static theory to analyse a phenomenon as dynamic as capitalism. So what can static equilibrium models tell us? The answer is according to Haavelmo – close to nothing. Dynamics are needed.

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