The challenge Samuelson never answered – the stability of general equilibrium

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Introduction

This paper tries to focus on one of the most taboo areas of modern economics – the stability of general equilibrium. This question is not highlighted in economic text-books, it is not a topic in the main-stream literature. Typically the lack of stability is admitted by neo-classical economists at best when challenged directly. Very often the answer is evasive – and that is no surprise. Stability of Walrasian Pareto optimal general equilibrium cannot be shown. The fact is that that both before and after Debreu's had perfected the proof of a unique static equilibrium in 1959 the general equilibrium paradigm had been fundamentally challenged on this point. Well-know economists like Hayek (1948), Kaldor (1952), Kornai (1971), Morgenstern (1972), Haavelmo (1974) and Fisher (1983) pointed out that without any theory of how to get to this point from a disequilibrium point this Arrow-Debreu model was of very little use. This challenge is not taken up by Samuelson in his Whig history, nor in the rest of his voluminous contribution to neo-classical economics too my knowledge. But to face squarely the challenges to one's paradigm is the hall-mark of real science. The fact that the stability of general equilibrium is so taboo points to another taboo aspect of neo-classical economics: that it's pivotal point – general equilibrium - is scientifically very weak since it is indeed utterly unrealistic. But the concept of perfect "competition" is normatively very strong. I will that the founding fathers of neo-classical theory did not want such an utterly unrealistically theory, but that was the price you have to pay to "prove" the efficiency of perfect competitive markets. But this show that neo-classical theory is an ideology – not a scientific theory.

The lack of stability – the core question of interpretation

There is no lack of mathematical language in the literature on stability, but there is a real lack of interpretation, of a plausible story of general equilibrium comes about. This problem is admitted, but not discussed by Debreu (1959) who writes in the preface, that the proof of the existence of an general equilibrium needed "the axiomatic form of the analysis, where the theory, in the strict sense, is entirely disconnected from its interpretation". Or in other words – the model does not saying anything about reality at all. But the fact is that Debreu's result has had an enormous ideological influence – it is regarded as undisputed mathematical proof that a market economy give the most efficient use of resources. Debreu's proof has withstood the test of time, but so has the lack of realism, the lack of real scientific interpretation of the Arrow-Debreu model.

This means that when Samuelson held his "Whig history" presidential address in 1987 – there was certainly no reason – for a real scientist - to celebrate the scientific victory of neo-classical theory. On the contrary – if sticking to the standards of real science – he should have taken up the question of stability; that is the obvious contradiction between this utterly static model and the dynamic reality of capitalism.

It is important to note that all the neo-classical founding fathers had been "wrestling with time"[1]. Marshall - Walras – Lindahl – Hicks – all of them saw static analysis just as a starting

[1] To borrow the title of Currie and Steedmans excellent book on this issue

point. Which they never managed to get beyond – at least a strong indication of the "mission impossible character" of showing that general equilibrium is stable.

As mentioned above, there is no lack of mathematical sophistication, neither by Samuelson nor by other neo-classical readers, but a lack of a good and convincing story. This is discussed in a small article by Haavelmo (1958 in Norwegian, 1974 in English) "What can static equilibrium models tell us?" which contains no maths, and for that reason is very useful to highlight the fundamental problems with static equilibrium models. As we will se below Haavelmo's conclusion is that "The system's dynamic motion" cannot be treated "as no more than an appendix to the static model" and that "the solution of the general equilibrium model" does *not* "show what will actually happen in a freely competitive market system."

Samuelson on the stability of general equilibrium

Let it be clear that I use as a representative of the neo-classical tradition in the same way as Mark Blaug does in his "After Samuelson – who needs Adam Smith?" – as a symbol. But Samuelson did get the Alfred Nobel Memorial Prize in Economics for economic dynamics as one of his major contributions. Saumelson has been a very productive economist and has over 500 scientific publications, books and articles. I have used the table of contents of the "Collected Scientific articles of Paul A. Saumelson" – looking for titles that might indicate that the stability of general equilibrium could be analysed in that article. I have also gone through Samuelson's "Foundations" and his introductory text book "Introduction to Economic Analysis". I will use the latter – which of course builds upon the "Foundations" – since it is more accessible to the non-mathematical reader – and since it have had an enormous influence on economic thinking ever since the first edition.

Samuelson first treats the problem of stability in three closely related articles published in Econometrica. The first in April 1941 with the promising title "The stability of equilibrium: comparative statics and dynamics". The second article in January 1942 with the title: "The stability of equilibrium: linear and non-linear systems". The third article has the title "The relation between Hicksian stability and true dynamic stability". All three articles show Samuelson's impressive mathematical skills. There is only one problem. The articles do not solve the problem of stability in an economic sense. Despite a discussion of various meanings of dynamic stability, despite analysis of the mathematical conditions for the stability of some models, especially Hicks in "Capital and Value", despite the introduction of the "correspondence principle" – there is no "convincing story". Samuelson do not tell how markets get to the equilibrium prices we no longer use the Walrasian groping (tâtonnement) or an auctioneer. In a way there is a negative proof that these articles do not solve the problem – that would have made the subsequent efforts of in particular F. M Fisher meaningless. None of those economist that challenged the general equilibrium paradigm even mention these articles – and neither Haavelmo[2] nor Kornai would have any problem with the mathematical parts – which are the main part of these articles. What is lacking here is real economic reasoning.

[2] Haavelmo (19??) in "The dynamization of the Walras schema" (in Norwegian) is very critical of Samuelsons belief that perfect competition is compatible with constant returns to scale.

In my opinion it is in this context two places where Samuelson discusses the problem of stability in the language of economic reasoning and that is in an article Samuelson, P. A. (1967), "The monopolistic competition revolution", in Kuenne, R.E (Eds), Monopolistic Competition Theory: Studies in Impact, Essays in Honour and in the related and relevant chapters of his text-book. In his "Whig" presidential address Samuelson writes that "Shortly after 1930 economics burst out into new life. At least four revolutions erupted: the monopolistic competition revolution, the Keynesian macro revolution, the mathematization revolution, and the econometric inference revolution." Of these – the "monopolistic competition revolution" is most relevant.

Kaldor writes in his article "The irrelevance of equilibrium economics" that "In fact, equilibrium theory has reached the stage where the pure theorist has successfully (though perhaps inadvertently) demonstrated that the main implications of this theory cannot possibly hold in reality, but has not yet managed to pass his message down the line to the textbook writer and to the classroom. (Kaldor 1972, p. 1240). In Samuelson's case it is the contradictions between the pure theorist and the teacher of economic that characterizes his writing on increasing returns, competition and the stability of equilibrium. That is easier to see if we first take a look at the theoretical challenges.

The challenge from Hayek to Haavelmo

There were after the war, especially in the seventies, several well-know economists that challenged the existing paradigm, by attacking its core metaphor - perfect competition – since the stability of this state was not and could not be proved[3].

The core discussion is of course how to describe, how to model competition. Already in 1948 – parallel to the first edition of Samuelson's "Foundations" Hayek wrote an article "The meaning of competition" where he argues that:

"'... if the state of affairs assumed by the theory of perfect competition ever existed, it would not only deprive of their scope all the activities which the verb 'to compete; describes but would make them virtually impossible' (p. 92).

This line of criticism was put even sharper in Hayek (1967) article "Competition as a Discovery Procedure" where he writes:

> "It is difficult to defend economists against the charge that for some 40, to 50 years they have been discussing competition on assumptions that, if they were true of the real world, would make it wholly uninteresting and useless.
> " (p. 179)

Hayek goes on:

> … indicating the absurdity of the usual procedure of starting the analysis with a situation in which all economic theory curiously calls 'perfect competition'. It leaves

[3] There are of course many well-known critics of the neo-classical paradigm that will I not discuss here. The point is not to give an balanced overview of all the critics, but to describe the intelectual challenge Samuelson was confronted with when he held his "Whig history" presidential address.

> no room whatsoever for the activity called competition, which is presumed to have already done its task. (p. 182)

In my mind Hayek ends up with an empirical correct conclusion which goes contrary to the whole ideological project of general equilibrium, that not only is the economy efficient, but each factor gets a price according to its marginal productivity:

> "The game is, to use up-to-date language, not a zero-sum game, but one through which, by playing it according to the rules, the pool to be shared is enlarged, leaving individual shares in the pool in a great measure to chance. (p. 186)

Some years after Hayek, Oscar Morgenstern was even more explicit, in an article called "*Thirteen Critical Points in Contemporary Economic Theory: An Interpretation*" he pointed out the two totally different meanings of the word competition:

> "Consider "competition": the common sense meaning is one of struggle with others, of fight, of attempting to get ahead, or at least to hold one's place. It suffices to consult any dictionary of *any* language to find that it describes rivalry, fight, struggle, etc. Why this word should be used in economic theory in a way that contradicts ordinary language is difficult to see. No reasonable case can be made for this absurd usage which may confuse and must repel any intelligent novice. In current equilibrium theory there is nothing of this true kind of competition: there are only individuals, firms or consumers, each firm and consumer *insignificantly* small and having *no influence* whatsoever upon the existing conditions of the market (rather mysteriously formed by tatônnement) and therefore solely concerned with maximizing *sure* utility of profit – the latter being exactly zero. The contrast with reality is striking: the time has come for economic theory to turn around and "face the music". (Morgenstern (1972, p. 1164)."

One can of course ask if Samuelson read this article by Morgenstern. If he did so he did not try to counter the proposition that "no reasonable case can be made for this absurd usage".

Just two years after Morgenstern, There was an article in Economic Inquiry[4] by Nobel Laureate Trygve Haavelmo (1974) with the title, "What can static equilibrium models tell us". In this article Haavelmo in his typical low-key manner point to the same fundamental problem of the neo-classical general equilibrium model – its lack of stability. This small, eight page article was first published in a "Festschrift" [5] to Fredric Zeuthen, a famous Danish economist in 1958. 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. Haavelmo's starting point is to

[4] The editor or Economic Inquiry, Axel Lejonhuvud got to know the Norwegian version and got it translated.

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

> "… 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 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. [6] 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

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

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

In my opinion, Samuelson’s articles from the forties – and his correspondence principle – is to a large extent occupied precisely with arguing that “if the static model has a certain form prices and quantities will be drawn to the equilibrium point”. I will below discuss in more detail to which extent Samuelson is “careful in making the claim that the solution of the general equilibrium model shows what will actually happen in a freely competitive market system”. But I will also discuss another important point, even if that was the case – would it be the way we want the markets to work, and my answer, like Hayek (1967) and Baumol (2002) is a clear no. Perfect competition should be called “perfect stagnation” since technology does not change, and it is technological change that really matters for welfare creation – not general equilibrium.

In 1971 Kornay published his book “Anti-equilibrium”. In the opening chapters of the book Kornai has an exposition of the mathematical preconditions for general equilibrium in the Arrow-Debreu version of which he is fundamentaly critical, due to the utter lack of realism, of real economic meaning. Kornai ends up quoting two famous utterances of Einstein on the relationship between theory and reality:

> ”Physics constitutes a logical system of thought which is in a state of evolution, whose basis cannot be distilled, as it were, from experience by an inductive method, but can only be arrived at by free invention. The justification (truth content) of the system rests in the verification of the derived propositions by sense experiences”
>
> “The sceptic will say: ‘it may be well true that this system is reasonable from a logical standpoint. But it does not prove that it corresponds to nature’. You are right dear skepetic. Experience alone can decide on the truth.
> (A. Einstein, Ideas and Opinions, New York, 1960, pp. 322, 355, quoted by Kornai p. 9-10)

It is symptomatic that one of the authors that Samuelson mentiones in his “Whig” piece Nicolas Kaldor also uses this Einstein quote, because:

> “…the powerful attraction of the habits of thought engendered by “equilibrium economics” has become a major obstacle to the development of economics as a “*science”* a body of theorems based on assumptions that are *empirically* derived (from observations) and which embody hypotheses that are capable of verification both in regard to the assumptions and the predictions.” (Kaldor 1972, p. 1237)

The reason why Kaldor holds that equilibrium economics are irrelevant is due to the "dominating role of increasing returns" and "Once however one allows for increasing returns, the forces making for continuous change are *endogenous* - 'they are engendered from within the economic system'[7]. I shall return to the more detailed arguments made by Kaldor when discussing Samuelson's "The monopolistic competition revolution".

Koopmans – took the challenge of Kornai and Kaldor

That for example Kornai's book "Anti-Equilibrium" and Kaldor's "The irrelevance of equilibrium economics" was seen a challenge is exemplified by Tjalling C. Koopmans article from the American Economic Review in 1974:

> "Is the theory of competitive equilibrium with it? Koopmans starts out by asking: Does the model of competitive equilibrium (the "CE model") in its simplest form represent one useful pure and special case, one valuable foothold for a steep climb? My answer: Yes. " (p. 325)

Koopmans then mentions some "recent criticisms" from Gailbraith, Kaldor, Kornai and Shubik. He notes that Kornai has a "revolutionary" proposal "to start afresh with entirely new approaches emodying other aspects of reality" and that Kaldor "calls for 'a major act of demolition". It is beyond the scope of this article to discuss the arguments that Koopmans put forward in order to substantiate his "Yes". In this context his final remarks are most relevant:

> "I conclude with a few remarks about the finite competitive economy that assumes objective certainty about the outcome of given actions by all agents. There has been an increasing concern in the recent literature with fitting adjustment processes into that model. I attribute this concern to a weakness in the model - not a logical but an interpretative weakness. Optimizing responses of economic agents are simultaneously feasible only if the proper prices are already known to them. But these prices must somehow themselves be the result of these same responses. Thus there is something circular in the description of agents. The market participants must be endowed with extrasensory perception (if acting simultaneously) or with supernatural premonition (if acting successively). (p. 327)

For me it is hard not to see the fact that the model wants the agents to be price takers and price makers at the same time is a logical defect, but of course if one could give a logical interpretation of this "dialectical" relationship that makes economic sense that's the key issue. What is really interesting here is the way Koopmans sums up this. He contrasts to types of models:

a) A process that does not approach an equilibrium (under constant technology and preferences) can be of great interest in itself.

b) An equilibrium that is not approached by any process that starts from a different initial state is of no interest in itself.

Or to put it another way – non-equilibrium processes are of interest, equilibria where stability cannot be shown has no interest. Koopmans does take the challenge – he recognizes the attacks on the core hypothesis of his paradigm, but says that he is still not convinced that the old paradigm cannot be modified to accommodate the attack. He – as shown in a) and b)

[7] Kaldor quotes from Allyn Young "Increasing Returns and Economic Progress", (Yong 1928)

above outline - what are the criteria for further fruitful development of "competitive equilibrium economics".

Fisher – the loyal attempt to prove stability

In 1983 Franklin M. Fisher published "Disequilibrium foundations of equilibrium economics". Writing the book on that topic was proposed to Fisher by Frank Hahn – one of the major general equilibrium theorists. As far as I know – this is one of the last serious attempts of investigating the condition for stability. In the introduction Fisher notes - as Haavelmo – the tendency to regard "the system's dynamic motion has been regarded as no more than an appendix to the static model", but like Haavelmo, Fisher emphasizes that:

> "Yet disequilibrium theory and, in particular, stability theory are of basic importance to economists. The proposition that the equilibria of economic models are not only stable but that convergence to a neighbourhood of equilibrium is achieved relatively quickly2 turns out to be a necessary foundation for the equilibrium analysis of economic theory. If stability theory is unsatisfactory, then that foundation is lacking. The proper conclusion is then either that the foundation must be soundly laid or that the structure based upon it must be drastically altered." (p. 2)

One of the reasons for this is because:

> "Much of what economists have to say about the results of competition, the usefulness or lack thereof of governmental intervention, and the role of the price system is based on propositions about general equilibrium. These are the propositions rigorously formulated in modern times as the central theorems of welfare economics concerning the relations between Pareto optima and competitive equilibria. These propositions, which may be the single most important set of ideas that economists have to convey to laypeople, implicitly assume that general competitive equilibrium is statble and, indeed, that convergence takes place relatively quickly. If this were not so, welfare comparisons of equilibria would be largely irrelevant since what would matter would be comparison of the relatively "transient" behavior of alternative systems including alternative forms of market organization. (p. 9)

In my opinion this is a statement basically saying that without a proof of stability most of the major results of neo-classical economics will turn out to not be based on science, which means that it is primarily an ideology. Or to use Fisher's words "In this sense, stability analysis is of far more than merely technical interest. It is the first step in reformulating the theory of value" (p. 16)

Fisher is able to prove asymptotic convergence, but with such restrictions that it is rather clear that this does not really solve the fundamental stability problem. Basically because Fisher proves this on the condition that there are what he calls "no favourable surprise", no new production methods, no new raw materials, no improvements in enterprise organisation or marketing procedures. As Fisher formulates it "The lesson is clear. Following Schumpeter, we cannot suppose it to be true that an economy in which new opportunities constantly arise will converge to equilibrium…" (p. 90). Secondly – the generality of the stability proof is such that "we know nothing of speeds of adjustment". This means that even if one could be sure that there was no continuous stream of "favourable surprises" – without knowing the speed of

convergence, one cannot "justify analyzing economies as though they were in equilibrium without showing that convergence is rapid. There are other aspects of Fishers model that casts serious doubts on the value of the asymptotic convergence. The number for firms is fixed, they issue shares before the starting date, but cannot do so afterwards. Incomplete markets are not taken into consideration and each agent has at any point in time a unique optimal plan.

But whatever one thinks of these limitations, there is no doubt that Fisher seriously attacks the fundamental problems head on. A major weakness of his whole approach is that he treats technological change as an "exogenous shock" (p. 4), the "favourable surprises" are not seen as part of the dynamic driving force of capitalism – profit maximization.

Samuelson on competition and stability

Samuelson's article "The monopolistic competition revolution" which he alludes to in his "Whig" speech starts out promising: "

> "My purpose here is to discuss some of the theoretical reasons why perfect competition provides an empirically inadequate model of the real world. This forces us to work with some versions of monopolistic or imperfect competition. Chicago economists can continue to shout until they are blue in the face that there is no elegant alternative to the theory of perfect competition. If not, ihe proper moral is, "So much the worse for elegance" rather than, "Economists of the world, unite in proclaiming that the Emperor has almost no clothes, and in pretending that the model of perfect competition does a good enough job in fitting the real world." (p. 134)

One of Samuelson's subtitles is "The breakdown of perfect competition" and later we learn that "Increasing returns is the enemy of perfect competition. And therefore it is the enemy of the optimality conditions that perfect competition can ensure" and that "As long as the specialisation is still being limited by the division of labour, competition cannot be working perfectly.

But Samuelson's criticism of Marshall show that he aims at dividing the world in one sphere where perfect competition is relevant and one where imperfect competition rules. As Samuelson writes : Unfortunately, because of his unwillingness to make sharp distinctions between perfect and less-than-perfect competition, Marshall managed to set back the clock both on competitive theory and on the theory of monopoly. (p. 136) … But where Marshall threw off two generations of scholars was in his insistence on having his cake and eating it too. He would try to treat at the same time cases of less-than-perfect and of perfect competition. … Marshall was so afraid of being unrealistic that he merely ends up being fuzzy and confusing - and confused. (p. 137)

Although Samuelson realises that increasing returns are a fact of life, he moves towards one conclusion that:

> "Because the convexity conditions of the modern formulations of competition are rarely met in real life, I propose in this section to state and prove some asymptotic theorems according to which we approach, in the limit as replicable numbers become indefinitely large, an approximation to the convexity conditions needed for competition. (p. 156)

The article is filled with small comments, polemics, digressions which make it hard to read. There is no clear cut conclusion that a break with the perfect competition paradigm is needed. Samuelson concludes saying that "Chamberlin Sraffa, Robinson and their contemporaries have led economists into a new land from which their [Chicago] critics will never evict us." But this new land seems to be a land where Samuelson ends up being "fuzzy and confusing – and confused" because he both want to acknowledge the importance of increasing returns to scale – and keep perfect competition as the core metaphor, as the "first best", as the benchmark for economic efficiency, as guiding star for economic policies in a world where imperfections are everywhere. Marshall was probably confusing trying to analyse both perfect competition and "imperfect" competition together, as varieties of one competitive process. But in my opinion the separation that Samuelson introduces with some very heroic asymptotic assumption that in fact are assuming away the original problem is confusing in that it leaves the question of what happens when "perfect competition" breaks down on a large scale. Because if firms the search for increasing returns, because it increases profits, then increasing returns becomes not an "externality" but part and parcel of the competitive process.

Samuelson's text-book economics

The monopolistic competition article was written in 1967 – long before the "Whig history", but the fourteenth edition of his "Economics" came in 1992 not long after[8]. No chapter is devoted to discussing stability. The word stability as such is not in the index – only "stabilitsators" so the challenge regarding stability is not directly taken up. But of course the question of stability is closely connected to the question of how one theorizes competition, and competition is a thoroughgoing issue – as in any book about economics. The dichotomy of perfect and imperfect markets is still with us – and there is considerably less focus on the "breakdown of perfect competition". Under the subtitle "The invisible hand and "Perfect competition" Samuelson starts out saying that

> "We know that the market sometimes lets us down, that there are 'market failures' and that markets do not always lead to the most efficient outcome. Important market failures, which will be extensively analyzed in this text, include imperfect competition and externalities like pollution. Let's pause to discuss the crucial role played by competition in the market system. Smith himself recognized that the virtues of the market mechanism are fully realized only when the checks and balances of perfect competition are present. What is meant by perfect competition? It is a technical term that refers to a market in which no firm or consumer is large enough to affect the market price. For example, the wheat market is perfectly competitive because the largest wheat farm, producing only a minuscule fraction of the world's wheat, can have no appreciable effect upon the price of wheat. The invisible-hand doctrine is about economies in which all the markets are perfectly competitive. In such a circumstance, markets will produce an efficient allocation of resources, so that an economy is on its production possibility frontier. When all industries are subject to the checks and balances of perfect competition, as we will see later in this book, markets can produce the efficient bundle of outputs with the most efficient techniques and using the minimum amount of inputs. On the other hand, when a telephone company or a labour union is large enough to influence the price of phone service or labour, some degree of "imperfect competition"

[8] This edition was co-authored by William D. Nordhaus, but I will as a shorthand use "Samuelson" for the two authors.

> has set in. When imperfect competition arises, society may move inside its production possibility frontier. (p. 40)

I guess it is not only me who are a little bit puzzled by the use of "checks and balances" of perfect competition, when both friends and foes has pointed out the problem with this concept is just that is a point, that no convincing story of stability has so far been told. "Checks and balances" must give so strong associations of stabilizing forces at work. Smiths "invisible hand" is clearly a dynamic concept closely linked to the question of division of labour, i.e. in the pursuit of profits the capitalist has a strong incentive to create increasing returns to scale by increased division of labour, increased learning by doing, increased use of machinery for the most elementary mechanical tasks. All this is forgotten. On the other hand – the large, strong union as an "imperfection" comes into the picture[9].

> Samuelson also has a straightforward discussion of returns to scale (p. 110 – 111). Elsewhere we learn that "The case of decreasing costs is not an isolated phenomenon. Numerous detailed econometric and engineering studies confirm that a wide range of non-agricultural industries show declining average long-run costs. Given the prevalence of decreasing costs, we cannot be surprised at the existence of imperfect competition in the modern industrial economy. (p. 148)

Clearly Samuelson admits that decreasing costs (increasing returns to scale), but as always treating as something technical – not as connected to the competitive process in a dynamic way, with one major exception – when Saumelson discusses what he calls the "The Schumpeterian Hypothesis" (p. 188 – 189).

The Schumpeterian Challenge

Samuelson's starting point is that:

> "We have encountered a litany of complaints about imperfect competitors. They tend to set prices too high and quantities too low, they may earn supernormal profits, and so on. But we must now turn to one powerful point of defence of imperfect competition. Many years ago, the great Austrian-born economist Joseph Schumpeter (1883-1950) argued that the wellspring of innovation and technological change is found in giant corporations and in imperfect competitors. While it is true that imperfect competitors cause inefficiencies because their prices lie above marginal costs, Schumpeter thought that the innovation produced by large firms would more than offset the losses from too high prices.

The arguments put forward above are of course well known. Samuelson and Nordhaus (1992, p. 189) call it the Schumpeterian hypothesis:

> Because economists had been taught about the evils of monopoly and the wastes of imperfect competition, the bold *Schumpeterian hypothesis* came as a shock. This

[9] This view on labour unions is directly in contrast to the Scandinavian experience where the strong unions have for decades been shaping one of the worlds most successful economies when measured by welfare creation. High and compressed wages stimulates labour saving innovation – and saving labour is the fundamental source of welfare since (labour) time is the fundamental and only cost to society. This dynamic has been formalised in the Rehn-Meidner model.

hypothesis has been subject to careful scrutiny for over four decades. How well have these views survived in the academic market place?". To begin with, most economists grant the basic truth in the Schumpeterian hypothesis. We hardly see our local grocery store or tomato farmer supporting a large R&D establishment. […] Having conceded that tiny firms do little research, many analysts draw the line. Sceptics note that many firms with low market shares have substantial and successful R&D programs. Moreover when John Jewkes and his colleagues traced the history of the most important inventions of this century, they found that less than half carne from the laboratories of large corporations. The importance of small inventors has been confirmed in recent years as major new products seem to arise from nowhere as occurred when Apple Computer launched the microcomputer revolution in the early 1980s.

To summarize, the relationship between innovation and market power is complex. Because large firms have made a major contribution to research and innovation, we should be cautious about claims that bigness is unmitigated badness. At the same time, we must recognize that small business have made some of the most revolutionary technological breakthroughs. To promote rapid innovation a nation must preserve a variety of approaches and organisations". (p. 189)

What is really striking is the static nature of the argument. In my opinion it is fairly obvious that a significant share of the small firms with a really good idea, creating and using increasing returns to scale, will grow big. Microsoft was a small, but due to luck and a courageous business strategy they became a big firm. It could have been Apple[10]. Both firms were monopolists in spe. And finally, if a nation must "preserve a variety of approaches and organisations" why should we then regard these as imperfect, talk about the "deadweight loss of monopoly" etc. To try to refute Schumpeter by arguing that not only big firms do innovate is not at all convincing. Schumpeter did not argue that *only* big firms did innovate. All big firms started out as small and innovative. No firm is born big from scratch. The population dynamics one should expect is precisely a mix of small and big firms at any point in time.

The argument "that the innovation produced by large firms would more than offset the losses from too high prices" is also strange for looking at this dynamically it is quite clear that IBM had a much better price/performance ratio than the manual methods that IBM out competed. The same store repeated itself when first mini-computers and then personal computers came onto the scene. The price/performance was significantly better – so there is no "deadweight loss" to offset. The reduced prices meant that more computers were produced; firms reaped increasing returns to scale etc. – contrary to the myths of neo-classical economics about the effects of "monopoly".

Rivalry or competition?

As one could guess, given the utter static nature of "perfect competition" which should be called perfect stagnation, because that is what it is, Samuelson have to introduce another

[10] In the late eighties Apple charged to high prices, they did not fully understand (as many in the software business) understand the importance of lock-in, i.e. of network effects. Paradoxically, by charging their customers the "real" value of their product, which was clearly superior. But that meant that Apple lost the battle of the operating systems.

concept in order to describe what most people call – an in reality is – competition. Samuelson introduces the notion of "rivalry"?

> Note that the existence of imperfect competition does not preclude intense rivalry in the marketplace. Imperfect competitors are often fighting to increase their market shares. Intense rivalry should be distinguished from perfect competition. Rivalry encompasses a wide variety of behaviour, from advertising that attempts to shift out one's demand curve to inventing better products. Perfect competition says nothing about rivalry, but simply denotes that every firm can sell all it wants at the prevailing market price. (p. 163)

And one may ask in what way does the process of "rivalry" relate to general equilibrium? Does "inventing better products" move us back from the production possibility frontier? Or is it precisely this aspect of capitalism that like Hayek argues makes competition a non-zero-sum game?

Or as Baumol (2002, p. viii) formulates this:

> "My central contention here is that what differentiates the prototype capitalist economy most sharply from all other economic systems is free-market pressures that force firms into a continuing process of innovation, *because it becomes a matter of life and death for many of them.* The static efficiency properties that are stressed by standard welfare economics are emphatically *not* the most important qualities of capitalist economies.

The key point here is of course the fact that perfect competition has nothing to do with competition at all. The theory of perfect competition describes *static* equilibrium, but does not describe how to get to that equilibrium, or how stable that equilibrium is. My proposition is that perfect competition is not only a non existing state due to the extreme lack of realism of the model, but that it is an *unstable*, *unsustainable* state. Nobody wants it, neither firms nor society. For the firms it means reduced profits or bankruptcy. For society it means less innovation, less welfare. Whenever markets are having characteristics that one might think of as characteristic of perfect competition, the firms will – if at all possible - actively take action to change that situation, by technological innovation, product differentiation, by obscuring information, by various forms of cooperation. A lot of what we observe in real life markets is strategies the firms apply to avoid getting in a situation where some of the conditions of perfect competition are being satisfied. The combination of these strategies leads to various outcomes. A discussion of the welfare effects of these strategies is beyond the scope of this paper. The point is that perfect competition – by its unstable nature – *cannot be a benchmark* for the real existing market structures or for the optimal economic policies.

What has happened after Samuelson came out of the closet?

At the time of writing it is twenty years since Samuelson held his presidential address and one might ask what has happened since then. The simple answer is – not very much, i.e. one the one hand the problem of proving stability has been emphasised even stronger. On the other hand most economist, including founding fathers of the modern general equilibrium model like Arrow continues to treat this as a non problem – or as a problem that is solved. Let me illustrate this with four sources. First of all Alan Kirman (1989). "The Intrinsic Limits of Modern Economic Theory: The Emperor Has No Clothes". Kirman focuses on the usual

scientific demand that a theory should be able to generate verifiable propositions. Kirman writes:

> "A second point is that general equilibrium lacks any result as to the stability or uniqueness of equilibrium that can be derived from the standard assumptions on the endowments, production possibilities and preferences of individuals. As Morishima (1984) says:
>
> > 'If economists successfully devise a correct general equilibrium model, even if it can be proved to possess an equilibrium solution, should it lack the institutional backing to realize an equilibrium solution, then that equilibrium solution will amount to no more than a utopian state of affairs which bears no relation whatsoever to the real economy.'" (p. 127)

Let's first of all note that Morishima comes to exactly the same conclusion as Haavelmo does, which is also Kirman's conclusion. Also worth noting is that Kirman specifically argues that the demand for "micro-foundations" for macroeconomics – Kirman quoting Solow (1986) – "was a demand that macroeconomics should be built on Walrasian foundations". The mental model and the equilibrium terminology used in "macro" economics that last twenty years invariably has as underlying assumptions uniqueness and stability. Kirman dryly comments that it "should be clear by now that such assumptions have no theoretical justification."… and that "most economist are not even concerned over the sea-worthiness of the vessel which they are sailing". A real scientific paradigm would of course not let such a challenge of being a emperor without clothes be unanswered. But this seems to be the case – clearly confirming the hypothesis that we are dealing not with a scientific theory, but with an ideology. An indication is that in a recent working paper[11] by Donald W. Katzner (2004) with the telling title "The Current Non-Status of General Equilibrium Theory". Katzner confirms Kirman's finding that

> "In particular, no acceptable analysis of the uniqueness and global stability of competitive equilibrium had yet been found and, hence. Even with respect to tâtonnement dynamics, a full and theoretically satisfying explanation if price determination in a competitive economy, i.e., an explanation of the means by which market-equilibrium prices are determined from all appropriate out-of-equilibrium positions, could not be given." (p. 1)

Katzner still holds that it is worthwhile to work inside the Walrasian paradigm because "the general equilibrium n construction as originally set out by Walras is a significant component of the theoretical basis of, in its most idealized form, capitalism – an arrangement that, for better or worse, orders much of our modern world. Public discussions in the media, in political arenas, and elsewhere that relate to general equilibrium in part or in toto are commonplace. For economists to turn their backs on such matters is to disassociate themselves from an important aspect of current affairs to which they have a professional obligation to contribute." Against Katzner one could argue that capitalism is in no need of a theoretical basis. If needs a theory arguing that it is the best of all possible systems is another matter, which I will not pursue here.

The point I am heading at is that in 2006 there was published an important collection of articles with the title "Samuelsonian Economics and the Twenty-First Century"[12]. Of especial interest in this context are relation between the foreword is by Kenneth Arrow and a small

11 The working paper in an extended version is also found in Katzner (2006)

four page article by Franklin M. Fisher entitled "Paul Samuelson and the Stability of General Equilibrium". Arrow remembers that changing focus from statistics and mathematics to economics he started reading Econometrica where "I encountered several papers by Paul, particularly those defining and applying the concept of stability of economic equilibrium. […] Like Paul, I had profited greatly by happening on J. R. Hicks's Value and Capital. This work gave, like no other since Walras, an overview of the economic system, including, most importantly, its time dimension. (p. xi) Arrow describes Samuelson's "very wide knowledge of mathematical systems used to describe natural phenomena" which "… underlay his pioneering and piercingly clear statement of the meaning of stability of competitive (or other) equilibrium in a world of many commodities and the relativity of the definition to a dynamic system of adjustment." (p. xii) Finally Arrow notes that it is in a way paradoxical that Samuelson – having placed Keynes on par with Smith and Walras as economists – have produced practically no "serious studies of income analysis along Keynesian lines". The explanation given is remarkable, "I think on the whole the evidence is that Keynesian theory, for better of worse, is not a model rich in the kind of implications that competitive equilibrium theory has led to in the hands of a master like Paul." It is significant that Arrow does in no way reveal for the reader that the stability problem is still unresolved. He does not say that Samuelson solved it, or that he answered the fundamental challenges regarding stability. Arrow's expressions are notoriously vague, "like defining and applying the concept of stability of economic equilibrium". Arrow – more than almost anybody else – should be very well aware of the state of the art regarding this. And whatever one think of Keynes, in my opinion the Keynesian analysis of capitalism is a much richer and realistic description than anything based on Walras[13].

Especially since one of the articles in the book is concerned precisely with Samuelson and stability. Fisher starts his essay by stating that:

> The study of the stability of general equilibrium is not a popular indoor sport among present-day economists. Yet, the lack of a fully satisfactory stability analysis is a gaping hole in microeconomic theory. In particular, the First and Second Welfare Theorems on which so much policy depends are theorems about the efficiency properties of general equilibrium. If general equilibrium is not satisfactorily stable, then the usefulness of those theorems is in question. Further, to assume that the economy is always at or near equilibrium is to beg the question of why that is so, and to fail to notice that relative prices do change in fact. (p. 142)

This judgement comes from an economist that on the same page states that: "Despite the fact that stability theory has yet to reach a satisfactory conclusion, it remains my hope that the twenty-first century will see more attention paid to this area and considerable progress made." But as the "challengers" pointed out, the fundamental problem – as Fisher himself formulates it - is that "We are dealing with a competitive economy, in which all participants take prices as given. But, as Arrow, among others, aptly remarked, if everyone takes prices as given, how do prices ever change?" This means that as long you do not have a substantial (real-world) theory of how firms are *setting* prices, how they *innovate* in order to avoid situations when

12 Szenberg et al. (2006)

13 I think Haavelmo's judgement is a very correct one, that Keynes when treating "dynamics demanding phenomenon" with static concepts "was always in a logically dangerous zone, and that created a lot of confusion." – and opened up for the total watering out of Keynes insigths after a decade or two in main-stream economci.. Haavelmo (1977).

prices of fairly homogenous goods can be easily compared by the buyers – you cannot make a mathematical model of the dynamics of capitalism. On this particular point Fisher's view of Samuelson's contribution is not very positive: "Samuelson pointed out that the modelling of disequilibrium behaviour over time was essential, but also suggested an adjustment equation that has little or nothing to do with such behaviour." (p. 144). As discussed above Fisher came to the conclusion that the stability proof he himself developed ""depends on a strong assumption about individual perceptions generally – an assumption that new favourable opportunities do not arise as sudden surprises in the course of the adjustment process." (p. 144). But such "new favourable opportunities" are the same as an competitive advantage, and firms day and night try to create them. Such activities is nothing exogenous, on the contrary they are core, endogenous activities of any firm.

The fact that Arrow can gloss over the lack of any proof of stability of general equilibrium given by Samuelson or any other main-stream economist, when an article in the same book written by an expert on that issue writes an article in the same book, just for the n'th time goes to show how utterly incapable the neo-classical school is in treating this issue seriously.

Conclusion: stability - the challenge that cannot be met

In a historical perspective the marginalist/neo-classical theory came as a reaction to the "classical" labour theory of value. From Walras to Debreu there is an increased mathematical rigour, but at the cost of a total lack of realism. The intuitions about how market prices where formed had to be exorcised in order to prove all the neo-classical and neo-liberal truths about state intervention, taxes, comparative advantages, how bad unions are etc. etc. The lack of realism is not a "defect" – it is a necessary consequence of the ideological nature of the neo-classical paradigm. Its raison d'etre is not scientific, but ideological.

That a major figure in neo-classical economics like Samuelson after some sterile mathematical exercises in his early career never takes up the gauntlet thrown by his peers like Kornai, Kaldor and Haavelmo clearly indicates that there is only one solution to this problem, to build economics on a dynamic foundation. The starting point is not only that perfect competition does not exist, the starting point must be that it is a state in the state space that competitive firms do all the can to get away from – and that that this is the core of real price/performance competition where innovation plays a crucial part – and that it is this *dynamic* process that is welfare enhancing. It is not the allocation of scares goods that is interesting, it is dynamically overcoming scarcity.

References

Baumol W. J. (2002): The free-market innovation machine: Analysing the growth miracle of capitalism, Princeton University Press, Princeton and Oxford.

Currie, Martin and Steedman, Ian, (1990): *"Wrestling with*

Fisher, F. M. 1983. Disequilibrium foundations of equilibrium economics. Cambridge: Cambridge University Press.

Hayek, F. A. (1948): "The Meaning of Competition", in Hayek, *Individualism and Economic Order*, University of Chicago Press

Hayek, F. A. (1978)" Competition as a Discovery Procedure," in New Studies in Philosophy, Politics, Economics, and the History of Ideas. Chicago: The University of Chicago Press, 1978.

Helpman, Elhannan and Krugman Paul R. (1993), "Market Structure and Foreign Trade", MIT Press.

Haavelmo, Trygve (1974), *"What Can Static Equilibrium Models Tell Us?",* Economic Inquiry, 1974, vol. 12, issue 1, pages 27-34 (Original in Norwegain 1958)

Haavelmo, Trygve (1977), "The Keynesian Revolution", Memo, Institute of Economics, Univ. of Oslo

Koopmans, T. C, (1974), "Is the theory of competitive equilibrium with it?", American Economic Review, 64(2), 1974

Kornai, Janos (1971), Anti-Equilibrium: On Economic Systems Theory and the Tasks of Research

Makowski L, and Ostroy J. M (2001), "Perfect competition and the creativity of the market", Journal of Economic Literature, vol. 39, no 2.

Mäler, Karl-Göran , Nobel Lectures, Economics 1981-1990, Editor, World Scientific Publishing Co., Singapore, 1992)

Kaldor, N. (1972), "The Irrelevance of Equilibrium Economics", 1972, Economic Journal

Donald W. Katzner, 2004. "The Current Non-Status of General Equilibrium Theory," Working Papers 2004-10, University of Massachusetts Amherst, Department of Economics, revised Aug 2005. http://ideas.repec.org/p/ums/papers/2004-10.html

Katzner, Donald W. (2006.) An Introduction to the Economic Theory of Market Behavior : Microeconomics from a Walrasian Perspective. E. Elgar

Alan Kirman (1989). "The Intrinsic Limits of Modern Economic Theory: The Emperor Has No Clothes", The Economic Joumal, 99 {Conference 1989}, 126-139

Metcalfe, J. S, (1998): *Evolutionary Economics and Creative Destruction*, Routledge, London.

Morgenstern, Oscar (1972), “Thirteen Critical Points in Contemporary Economic Theory: An Interpretation”, Journal of Economic Literature, Vol. 10, No. 4 (Dec. 1972, 1162-1189)

Morishima, M. (1984). ‘The good and bad uses of mathematics.’ In Economies in Disarray (ed. P. Wiles and G. North) Oxford: Basil Blackwcll.

Nelson, Richard R., “Capitalism as an Engine of Progress,” Research Policy, 19, 1990, pp.61-87.

Samuelson, Paul A. (1943),Dynamics, Statics, and the Stationary State The Review of Economics and Statistics, Vol. 25, No. 1, pp. 58-68

Samuelson, Paul A. (1941) “The Stability ofEquilibrium: Comparative statics and dynamics”, 1941, Econometrica.

Samuelson, Paul A. (1944) “The Relation Between Hicksian Stability and Tme Dynamic Stability”, 1944, Econometrica.

Samuelson, P.A (1967), "The monopolistic competition revolution", in Kuenne, R.E (Eds), Monopolistic Competition Theory: Studies in Impact, Essays in Honor of Sir Edward Chamberlein.

Samuelson, Paul A. (19??), Collected Scientific Papers, five volumes, 1966-86. Stiglitz (eds.)

Solow, R. (1986). ‘What is a nice girl like you doing in a place like this? Macroeconomics after fifty years.’ Eastern Economic Journal, Vol. 12, no. 3, pp. 191-8.

Stiglitz, Joseph E. (2002), ”Information and the Change in the Paradigm of Economics”, *The American Economic Review*, Vol. 92, No. 3, June 2002.

Young, Allyn, (1928), ”Increasing Returns and Economic Progress”, Economic Journal, 1928, pp. 527-542