

Tony Lawson on Orthodox Economics: Math Fetishism or Ideology?

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Introduction

Tony Lawson has over the years, primarily in Lawson (1997, 2003, 2006) and Fullbrook (2009) developed an analysis of main-stream (orthodox) economics where the main conclusion is that the major defect of orthodoxy is that it is obsessed with *method*, in the sense that the essence of orthodoxy “consists in the insistence that forms of mathematical–deductive method should everywhere be utilised” (Lawson (2006, p. 492)). In this paper I presuppose that the reader is familiar with Lawson’s argumentation and his ultimate aim of “reorienting economics”. The paper is written as a contribution to the effort of reorienting “economics”². I completely share Lawson’s view that orthodoxy³ is in an “unhealthy state”.

The main argument of the paper is that Lawson does not pose the fundamental question of *why* economics ended up “insisting” on a very specific set of mathematical models, namely static, general equilibrium. Lawson does not ask why orthodox economics do **not** “insist” in the same manner on using dynamic models. He does not ask why the orthodox economics do **not** insist on using increasing returns to scale as

¹ Thanks to Julian Wells for commenting on an earlier draft. All remaining errors and short-comings are entirely mine.

² Economics in this paper means the study of the economic aspects of society, not economics as text book economics. Economics in this sense is in no way sharply divided from other social sciences like history, sociology, political science etc.

³ I will use the term orthodoxy for what is often called “main-stream” economics, or “neo-classical” economics. The basic criterion for deciding whether a school of economic thought is orthodox or not is its relation to static general equilibrium as a “first best”. If a theoretical current regards this as a benchmark for theory and consequently for policy, it is orthodox.

the shape of the production function. This is all the more surprising since both dynamic models and increasing returns to scale seem clearly much more appropriate models and concepts to grasp such a highly innovative and dynamic system as capitalism – where increasing returns to scale is the name of the game. Failing to ask such questions; Lawson ends up close to rejecting the use of mathematics as such.

The paper further argues that the reason why the orthodoxy insists on *only* by using the maths of static, general equilibrium is that only in such a model can a series of well-known neo-liberal, ideological “results” be proven mathematically⁴. These results have a very strong impact on policy making. Or in other words – on the one hand orthodox theory is ideologically/normatively very strong and relevant and *realistic*, in the sense that it influences reality. On the other hand it is scientifically very weak, completely unrealistic and irrelevant – since its static concepts by definition are unable to grasp a dynamic market system. So as a tool for scientific investigation it performs very poorly. It did neither predict nor explain the current financial crises when it was a fact. From history we know that for a system of thought to be politically important, but scientifically weak is a clear indication that it is not a scientific theory but an ideology in the negative (everyday) meaning of ideology⁵.

Regrettably Lawson explicitly rejects the point of view that main-stream orthodoxy is an ideology both in Lawson (2006, p. 486-487) and in discussing the analysis of Guerrien (2004) and Kanth (1999). This rejection is even more explicit in his updated and extended reply to Guerrien (2004) in Fullbroke (2009).. This despite the fact that for example Hahn; – whom he quotes extensively – and famous economists like

⁴ In fact only *existence*, but not uniqueness and/or stability can be proved as discussed in the last part of the paper.

⁵ Ideology can also be seen as a positive phenomenon, as a useful, roughly correct model (paradigm) of how society works.

Stiglitz - explicitly points to this ideological function of orthodoxy. In a similar fashion Lawson does not discuss the very strong impact of orthodoxy on economic policy. The conclusion of the paper is that economics should not be reoriented “away from math” towards ontology – but towards dynamics – whether formulated mathematically or not.

The role of orthodoxy in shaping economic policy

It should be beyond doubt that orthodoxy is very important in guiding the development of economic policy since the mid-seventies when Keynesianism faded away. The last three decades policy makers have – almost without exception – seen the “first best” state of general equilibrium as the point to get as close as possible. Everybody is of course realising that we will never reach this harmonious nirvana due to various types of market imperfections and failures. Still it remains the uncontested guiding star for the economic policies of IMF, World Bank, European Commission, OECD and the majority of governments world wide. Not only the policies, but also the rhetoric takes its metaphors, its frame of understanding, from orthodoxy. In short - in almost all realms of economic policy making the name of the game is to get as close as possible to this ultra-static “first best”. The quest for privatisation, for globalisation, for free trade is based on static “market cross” models. Orthodoxy regards taxes, minimum wages and unions as creating inefficiencies. Orthodoxy is the theoretical justification for having competition policies fighting against “monopolies” etc. In short – practically all economic and social policy has as their “scientific” static, general equilibrium theory.

The fact that everybody realises that we live in a second, third, fourth – best world does not diminish the normative power of orthodoxy – on the contrary. The concept of second best has no meaning without a first best, without “believing” in the welfare-theorems of orthodoxy.

In my opinion heterodox economics is not only negatively defined as every school of thought that is not orthodox. Heterodoxy is positively defined as paradigms that allows for dynamics, for real time, for learning, for real uncertainty, i.e. all the phenomena that are part and parcel of a real-life dynamics. The

heterodox schools of thought are not separated from the other social sciences by a watertight (static) compartment – when they have not become victims of (static) “economics imperialism”.

As I will argue below, Lawson has a far too restricted operative definition of “ideology” which is an obstacle to see the importance of orthodoxy as an ideology, i.e. an unscientific basis for policy making aimed at serving – consciously or not – not as the orthodoxy proclaim – the general welfare of society – but certain elite interests.

Although I share Lawson’s opinion that “economics is in an “unhealthy state” considered as a scientific theory, I do not think – despite the reduced self-confidence of orthodoxy due to the current financial crisis – that the project is in “a state of disarray”. The present crisis is just a negative external shock from which orthodoxy most probably will recover, but which those of us interested in establishing economics as a real science should use to weaken its influence as much as possible. Tony Lawson has on the one hand made a very valuable contribution highlighting the scientific weakness of orthodoxy, but in my opinion failed to give a completely correct diagnosis and consequently to point out the correct way to reorient economics. A fairly abstract turn to ontology is not an adequate cure – although it is a good starting point.

The theoretical origins of orthodoxy

It is beyond the scope of this article to really argue that orthodoxy has its origin in a counter-reaction to economic theories based on a labour theory of value characteristic of Smith, Ricardo, Marx – and even J. S. Mill⁶. It is not a coincidence that Debreu’s final mathematical proof of the existence of a general equilibrium *point* was entitled “A theory of value: An axiomatic approach”. The same goes for Hicks “Capital and value”. Economics is necessarily both descriptive and value-laden – its object of study is what value is, how it is created, what a just and/or efficient distribution of “value” is.

⁶ Both Marx and later for example Isaac Rubin in his “History of economic thought” argue that this is the function of “vulgar” economics.

It is a ironic paradox of the history of economic thought that several of the founding fathers of orthodoxy, among them Walras, Lindahl and Hicks, did **not** want the theory to be as unrealistic as it eventually became in the Arrow-Debreu formulation. But all the way there was a trade-off between realism and a set of desired (neo-liberal, anti-government, anti-union, anti-monopoly results) that always turned out to be more important (to prove) than realism. The desire to show that markets with no political (government) interference, no unions (monopsonists) was the best of all thinkable worlds prevailed. General equilibrium theory is “rigged” or “tailor-made” to show that “free”, perfect markets used the scarce resources the best way, and most importantly that if the ensuing distribution is “fair”, every factor was paid according to its marginal, productive contribution etc. This contradiction between a *scientific*, i.e. critical and realistic description of how capitalism actually works and the desired ideological results is evident already in Walras’ work, as Colliot-Thélène (1979, footnote p. 427) point out, first Walras says that the theory of the pure economy is “the study of laws in some way natural and necessary according to which exchange, production, capitalisation and circulation of social wealth tend to operate under a hypothetical regime of organized free competition”. Then Walras says that “pure economy is the science which proves that perfect competition gives the maximum satisfaction of needs...”

In their study of the development of general equilibrium theory, with the unavoidable title “The Invisible Hand” Ingrao and Israel (1990) use the terms “objective-descriptive” and “utopian-normative”. A central theme of their book is how these contradictory objectives of general equilibrium were already “running through all of Walras work”. As we know by now – it was this latter “utopian-normative” objective that became dominant. It turned out – that only under the most unrealistic, restrictive ultra-*static* conditions could the *existence* of a general equilibrium point be proved. A point that one could – and that one have (mis)used as a theoretical basis for these normative “results”. What was shown by Debreu & Co was the existence of *a* (fix) point – neither the uniqueness nor the stability of this point! The lack of uniqueness and stability means that orthodoxy has no theory whatsoever for showing how you get from disequilibrium to this optimal equilibrium point, which means both in scientific and everyday terms – that this is in fact not a *real equilibrium*, certainly not a *general* one in the sense that the economy will move towards this point from most of the possible states.

The totally static – and thus unrealistic - nature of the “solution”, where neither prices, technology nor “initial endowments” are allowed to change as much as an iota was *not intended*. On the contrary – as shown in Currie’s and Steedman’s (1990) excellent discussion of how the orthodoxy have always “wrestled with time” – Walras, Lindahl, Hicks – all saw the static model as just the first step towards a dynamic model. The same story is told in even more detail by Ingrao and Israel (1990). They underline that Walras “did not fail to point out, however, the need to proceed from static to dynamic analysis as the next stage of

study” (p. 103). Ingrao and Israel concludes that “none of the three analytical problems raised by general economic equilibrium theory - existence, uniqueness and stability – received either solution or clear definition in the *Élements*.” (p. 112).

Why neither Walras, Lindahl, Hicks, Samuelson did not get into dynamics, did get into this obvious next step of creating a real *dynamic* general equilibrium model have fundamental mathematical reasons, which I will in the last part of the paper. But the crucial point is that one has to analyse *why* orthodox economics has static equilibrium has its theoretical foundation when the scientific basis (uniqueness and stability) is still missing. Why use static tools to analyse a system - capitalism – clearly the most socio-technological *dynamic* type of society we if we compared to slavery, feudalism or the “command economy”. . This is the fundamental question that Lawson does not ask.

One could argue that he does so in an indirect way by outlining a set of ontological criteria that a scientific economic theory must meet, like being able to grasp society as an “open system”, with “emergent” properties⁷ etc. but this does not spell out clearly the fundamental divide between dynamic and static models of a capitalist economy. It is beyond the scope of this article to argue this in any detail, but in my opinion it is rather obvious that none of the “usual” textbook results do hold in a dynamic model. Take for example the well-know result that “lump-sum taxes” are the only ones that do not create inefficiency. This result should not be accepted before it is proven in a rather general model, with endogenous technological change, increasing returns to scale and other *fundamental* aspects of a capitalist economy. It should be no problem to show – that precisely progressive taxes will give more rapid growth, more equal income distribution and consequently be the best way to maximize welfare. The same point goes for “comparative advantage” this “sacred tenet” of economic theory according to Krugman (1987, p. 131). As Shaik (2004) show, that real life dynamic competition, gives the direct opposite results of the standard model.

⁷ Fullbroke has an informative discussion of this in his introduction to “Ontology and Economics, Tony Lawson and his Critics”, Fullbroke (2009)

One could take each and every one of the “sacred tenets” and show that they do not hold what they promise, i.e. economic policies do not maximise general welfare, but at best – the welfare of some elites. But in most cases – like the Argentinean crisis – even the majority of the elites are worse off. Regrettably Lawson does not discuss such issues when he – correctly in my opinion – criticises orthodoxy for lack of predictive power.

The false identification

One of the reasons why orthodoxy is so hard to get rid of is precisely because most people – including most economists – identify the success of the “Free Market Innovation Machine” (Baumol 2002) with the *static* orthodox theory. They see the static theory as an extreme, but still useful and fundamentally correct model of real, existing capitalism. “It must be a good theory since capitalism is such a success”. This false identification is in my opinion often not combated clearly enough by heterodox critics. Regrettably very many heterodox critics attack secondary aspects of the model, like atomism, perfect rationality and information. These are clearly closely related to the static nature of the model, but can – seen in isolation and given a more commonsense, broader interpretation – be seen as “real” abstractions. Rather heroic ones for sure, but still real abstractions. As Guerrien (2004) correctly notes regarding Lawson’s critique of the standard assumptions: “The problem with this objection is that a neoclassical theorist would agree with it. He would argue that he is considering only a special aspect of human behaviour: the fact that people try to pay less (rather than more) for a given good, or try to get more satisfaction (rather than less) from given resources. They then try to derive or to “deduce” (to use a word that Lawson doesn’t like) certain *ceteris paribus* consequences from this assumption” (Guerrien, 2004). The point here is that it is the *ceteris paribus* (static) nature of the argument that has to be attacked, not that the orthodox models have completely unrealistic assumptions like “infinite lives”. As Guerrien (2004) points out: “This is an approximation that can be accepted: in general when we take a decision concerning present and future, we do not think about death.” Regarding selfishness, Guerrien points out that “who can deny that self-love exists? Even Marx supposes that the capitalists motive is profit and that workers try to get a better life. This is all that is meant when neo-classical theorists assume that people are rational”.

The reason why many still are critical to the orthodox assumptions – even when they can be given a commonsense interpretation – is that they are static. Maybe the best way to highlight this difference is to take a closer look at the difference between the orthodox model of competition (static) and real life “cut throat” competition.

Perfect stagnation vs. dynamic competition

One important way of illustrating the utterly static nature of orthodoxy is to look closer – what Lawson does not do – to the core mechanism of capitalism, competition. As pointed out by for example Hayek (1948) and Morgenstern (1972), what in orthodox jargon is called competition has in fact nothing with real life competition to do. This is because neither prices, technology nor preferences – in fact nothing at all changes. Morgenstern wrote:

“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... (p.1164)

What orthodoxy calls perfect competition state should be called *perfect stagnation* – since nothing change. The term perfect competition should in text-books be replaced with perfect stagnation. I will return to the efforts of Franklin M. Fisher and others to create “disequilibrium foundations for equilibrium economics”, but first of all a few words on the fundamental logical *inconsistency* in the orthodox mental model, which is regrettably not a central axis of attack for Lawson.

On price taking and price setting behaviour

The key logical inconsistency of orthodoxy is that its actual mathematical model is crucially dependent on price *taking* behaviour, while the “story” behind competition dynamics is dependent on price *setting* behaviour. But as pointed out by many – there is no real theory (mathematical or verbal) about how the atomistic agents reach these prices. That process is not a mystery in real life, but it is not at all modelled in orthodox models. The nature of this problem described in the following way recently by a prominent Norwegian neo-classical economist Agnar Sandmo (2006) in his history of economic ideas:

“Most economists, both in Walras’ time and later, have agreed that the market mechanism in general functions in such a way that excess demand make prices rise and excess supply makes prices fall. It has been less agreement about to what extent the tâtonnement process is a convincing theoretical description of this adaption mechanism. The major objection to Walras’ description concerns the role of the auctioneer, for in most markets there are no such institution who decides prices. Consequently it has to be the agents themselves who decides what the prices are going to be, but at this point we get into the paradoxical situation that these same agents are supposed to take the prices as given. It is obviously

illogical to assume at the same time that they take the prices as given and plays an active role in changing them. This points to the fact that it is only in equilibrium that it strictly speaking can be true that everybody regards the prices as given by the market and beyond their control. Another aspect of Walras' market metaphor which is not very convincing is that there are no transactions as long as those prices that are called out [by the auctioneer] are different from the equilibrium prices. In this domain modern economic theory is still groping for good and convincing descriptions of how the market mechanism works. (Sandmo 2006, p. 174, my translation)

In my opinion there is not much scientific value in a theory that is "still groping" for a convincing description of how the market mechanism *actually* works – 150 years later. But in this context the important thing to note is that Lawson is not focussed on lack of *deductive-mathematical consistency*, or "completeness" that we of course must demand from a scientific theory. By being silent on this point Lawson appears to confirm orthodoxy's claim to be mathematically consistent – when in fact it is not. A fact that clearly indicates that it is the normative, ideological use and effects of this "theory" that is important.

The fact that there is not enough information in the "market cross" model has been pointed out long ago by Nobel laureate in economics, Trygve Haavelmo, in a small article with the telling title: "What can static equilibrium models tell us". Haavelmo (1974) points out that:

"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 were 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."

Or to quote one of the authors that Lawson knows in and out – Frank Hahn – who has many passages expressing this simple "price taking" fact, for example: "For instance, the competitive equilibrium gains its interest from the postulate that prices must change in all other states of the economy. But there are many other plausible dynamics and the competitive one suffers from having *no theory of agents who change prices*" (Hahn 1984, p. 3).

Another way to illustrate this is to take the very common myth that there has to be many actors for firms to be price takers, so that no firm can be "influencing" the price by its own amount of supply. But this is again just a small-talk story without any scientific value. If the prices are given, one firm – a monopoly – will

also have to be a price taker⁸. What is more – if the price is given, that is if a firm can sell all it produces to that *given* price – and if there is increasing returns to scale (falling unit costs) then the amount supplied would be infinite. That means that the model has no mathematical solution – as pointed out by Hahn: “But when there are increasing returns it may not be possible to show that there are any logical economic states which qualify as either Arrow-Debreu equilibria or as members of the core. It may also be wrong to think of a very large number of firms.” (Hahn 1984, p. 50).

Increasing returns to scale – and increasing theoretical problems

In 1952 Kaldor wrote an article entitled “The irrelevance of equilibrium economics” where the starting point is that increasing returns to scale is an important real economic phenomenon. As was pointed out above the general equilibrium model cannot tolerate increasing returns to scale, it breaks-down. This is not only an important theoretical fact, but also of great policy importance, because it is increasing returns to scale that is the reason why the developed capitalist economies get richer and richer, since unit costs for almost every *industrial* product falling over time, computers being one extreme and well-know example. This is Adam Smith’s great insight of the effect of the division of labour, the specialisation and mechanisation/automatisation of labour – creating increasing returns to scale. Increasing returns to scale is may be even more significant today than in Adam Smith’s days, because any product with a lot of fixed costs – not the least R&D – will have increasing returns to scale. Digital goods and services being an example, where R&D costs (in a very broad sense) are everything and reproduction costs – close to nil. It is also clear that any profit-maximising firm facing a fixed price (horizontal demand curve in equilibrium) would try to reduce costs, to innovate in order to maximise profits. An obvious conclusion is that the static model is *incentive incompatible* with profit maximization behaviour.

⁸ For a more detailed discussion of this point see, Guerrien (2002)

The first firm that made a product and/or process innovation allowing it to lower its price by one cent would take the whole market. Consequently every firm is a potential monopolist the fight for market shares is an eternal Darwinian struggle. If you do not expand – you contract. Every monopolist once was a small firm. The unavoidable conclusion is that the more perfect the markets are, the less friction there is – the sooner a monopoly will establish itself. The bottom line is that “competition creates monopoly – and monopoly will give us more products and lower prices as the *dynamics* of oligopolistic markets unfolds. This is just one of many cases where a static result⁹ – “monopoly means less production and higher price” – actually can easily be shown to be wrong – the opposite is true. Baumol (2002, p. viii) writes:

”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.

As far as I can see Lawson does not point to this fundamental weakness of orthodoxy – that it does not explain growth, innovation etc. since it is totally focussed on *static* efficiency of the allocation of resources – a clear indication that is more apologetic than scientific. The enormous bonuses and fees on Wall Street and in the City of London – are they fair – do they reflect marginal productivity? If we are in – or close to static equilibrium - that should be the case, but are we?

⁹ In Lawson 2009, in a footnote there is a discussion of an article by Steve Keen “Mad, bad and dangerous to know” where Keen argues for dynamic, non-equilibrium models, Lawson comments that “Even Steve Keen’s otherwise excellent recent analysis of the failure of modern economics to address the relevant issues seems to end up supporting a (different) sort of formalistic modelling.. To me Lawson here comes very close to reject all mathematical models –without closer investigation into their substance, their realism etc.

Stiglitz and Hahn on economics as an ideology

In my opinion Lawson does not comment sufficiently on very clear statements from economist like Stiglitz and Hahn on the question of the ideological character of modern economics. In an article entitled “The (confused) state of equilibrium analysis in modern economics: an explanation” Lawson quotes Hahn:

“it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states which they give no reason to suppose will ever, or have ever, come about. It is probably also dangerous. Equilibrium economics ... is easily convertible into an apologia for existing economic arrangements and it is frequently so converted.” (Hahn 1970, pp 88-89)

It is surprising that Lawson does not discuss this direct statement from one of the “savants” of general equilibrium theory. Especially since he uses this particular quotation from Hahn also in “The nature of heterodox economics”, Lawson (2006, p. 487) where he is explicitly arguing against Guerrien (2004). Because if Lawson had agreed with Hahn here – he would have been reoriented towards analysing why and how the theory is so easily converted into apologia – and as I will argue below – there is more than enough evidence that the theory is “rigged” in order to be “easily converted”.

But let us first look at some statements from Stiglitz on the nature of orthodoxy that Lawson does not have in his “arsenal” of quotes proving the unhealthy state of orthodox economics. The first is from Stiglitz’ Nobel Prize Lecture with the very telling title: “Information and the change in the paradigm in economics”:

One might ask, how can we explain the persistence of the paradigm for so long? Partly, it must be because, in spite of its deficiencies, it did provide insights into many economic phenomena. There are some markets in which the phenomena which we have discussed are not important – the market for wheat or corn – though even here, pervasive government interventions make the reigning competitive paradigm of limited relevance. The underlying forces of demand and supply are still important, though in the new paradigm, they become only part of the analysis; they are not the whole analysis. But one cannot ignore the possibility that the survival of the paradigm was partly because the belief in that paradigm, and the policy prescriptions, has served certain interests. (Stiglitz, (2002))

It is no mystery who's interests the theory has served – the elites. It has obviously not served the poor countries, the working poor, not the trade unions ... But Lawson does just that – ignores the discussion of who's interest the theory is serving. Another “famous” Stiglitz quote on the Internet is:

“That such models prevailed, especially in America's graduate schools, despite evidence to the contrary, bears testimony to a triumph of ideology over science. Unfortunately, students of these graduate programmes now act as policymakers in many countries, and are trying to implement programmes based on the ideas that have come to be called market fundamentalism.” (Stiglitz 2002b)

Here Stiglitz is accurately pointing to the role of orthodoxy as an ideology being a mental framework for the development of policy. One can ask why Lawson does not use Stiglitz as a “witness” – after all both Stiglitz and Lawson want a change of paradigm in economics. Could it be that Stiglitz is seen by Lawson as still too math oriented?

Lawson's explicit polemics against the ideology hypothesis

In his article “The nature of heterodox economics” Lawson discusses the hypothesis of “mainstream economics as an ideology”. In particular he addresses two authors, Guerrien (2004) and Kanth (1999). Lawson quotes Kanth's conclusion: “To state the moral: *the entire enterprise of neo-classical economics is rigged to show that laissez-faire produces optimal outcomes*”.

Lawson's answer to Kanth's conclusion is the following:

“How is this rigging said to be achieved? One component of the most common strategy is everywhere to stipulate that human beings are rational (meaning optimising) atomistic individuals. A second is the construction of theoretical set-ups or models specified to ensure that (typically unique) optimal outcomes are attainable. This is not yet enough to ‘show’ that the overall economic system is in itself optimal in any way. If the claim is that mainstream economists seek to defend the economic system *per se*, something more is required to guarantee this result. This, it is usually supposed, is achieved by the commonplace construction of an equilibrium framework, the latter being so specified that the actions of isolated optimising individuals somehow (tend to) work to bring an equilibrium about” (p. 487)

The most important point to note here is that it seems to me that Lawson does not grasp the utterly static nature of orthodox, general equilibrium. There is no “tending” in an Arrow-Debreu equilibrium framework, the individuals do not “bring” equilibrium “about”. The individuals are supposed to be in a position where they do not want to change their actions. They are placed in equilibrium so to say by theoretical decree. In orthodox theory equilibrium is a definition, is a mathematical fix-point, not a theory of action, of movement *resulting* in equilibrium.

One should also note the lack of precision of “optimal behaviour”, that is everything from rational expectations, perfect foresight to a very bounded rationality, to constant (sub-optimal) learning how to optimize as best one can in a multidimensional hyper plane where most of the possible maxima is clouded in the fog of uncertainty – and you know you can’t climb them all – so there is clear path-dependency. And the “rigging” of models “to ensure (typically unique) optimal outcomes are attainable” is not that the name of the game? The whole book of Ingrao and Israel (1990) is just a study in how reality is gradually – and totally – eliminated from the models in order to show that “perfect markets” gives a perfectly efficient and therefore efficient and de facto “just” utilisation of the available resources?

Lawson then goes on to argue that most economists do not model with optimal outcomes: “There is no doubt that some economists approach their subject in the manner that Guerrien and others suggest. But most do not. And I worry that portraying main-stream economics as driven by the goal of achieving results in these terms is overly conspiratorial”. (Lawson 226, p. 487). But does not every text-book used today to train new economists show precisely that the optimal characteristics of a market economy is the core topic, is the gospel to be learned? It is not a conspiracy, it is a movement of “true believers”, of economists that are convinced that policies guided by the welfare theorems – will actually create maximum welfare. Lawson correctly describes that at conferences you have to have a model, but that is not any kind of mathematical model, it’s got to be an equilibrium model, preferably the equilibrium should be unique, Pareto-optimal etc.

These latter demands on the type of model flows directly from the conviction that getting as close as possible to the “first best” – will give us the best of all possible worlds. Another example is the use in the policy sphere: of the concept of “market failure” does it have any meaning if the reference point - the benchmark - is not “perfect markets”, i.e. “optimal outcomes”?

Even Hahn says as a comment to Kornai’s “Anti-equilibrium”: “I am now also somewhat more ready to grant that equilibrium may be the wrong, or at least dangerous benchmark.”¹⁰ Which means that even for a fairly insightful sceptic like Hahn it has been and is benchmark, although after being bombarded by Kornai’s arguments he is “somewhat” more ready to discuss whether it is wrong or dangerous *as a benchmark for policies*.

Hahn, who’s complex, multi-dimensional – and from my point of view inconsistent – attitude to general equilibrium theory merits more than one article – is in no doubt when regarding the “usefulness” of the theory: “On the whole my attitude is this: if we did not have the Arrow-Debreu machinery there would be an urgent need to invent it, because it gives us the best base camp for sallies into new territory.”¹¹

In my opinion Lawson’s counter arguments misses the point made by Kanth and Guerrien even more when he writes: “Consider the conclusions of Frank Hahn” – one of them being that “Poignantly, Hahn believes equilibrium outcomes or states are rarely if every manifest”¹². But neither Kanth nor Guerrien – or the vast majority of orthodox economist have never argued that such states are part of reality – they agree

¹⁰ Hahn (1984, p. 487)

¹¹ *ibid.* p. 10

¹² Lawson (2006, p. 487)

completely with Hahn on this point. . The ideological function of orthodoxy is that static general equilibrium is seen as a benchmark for policy, a state to get a close as possible. Everybody knows that the world is not and will not be perfect, there are (nothing but!) externalities, asymmetric information, indivisibilities – all sorts of market failures - but still it guides IMF's policy, still it guides most governments policy.

One problem in this debate is that different authors focus on different aspect and/or meanings of ideology. Guerrien defines ideology as “intuitive beliefs that render them [most people] blind”¹³, not primarily as a set of “dominant ideas” favoured and nurtured by social elites since it serves their (conceived) interests. In my opinion both Guerrien and Lawson are too focussed on secondary aspects of orthodoxy, like the atomistic character of its agents, the implicit denial of conflicting social interests (employers vs. employees), what Guerrien calls “social structure”. Another problem is the focus on “academic” economics. As Lawson puts it:

“My response is to question the supposition that this conception of the efficiency of markets or perfect competition still constitutes a set of beliefs widely accepted by the mainstream community, if it ever did. At least I do not think this conception plays much of a role in motivating what goes on in the economics academy, *which is the focus of the discussion here*. Perhaps some academic economists do hold such views, but certainly not all. Mainstream journals are even open to certain “analytical Marxists” and others who seem happy to criticise the market mechanism. And just as much to the point, the substantive content of mainstream theorising is far wider and more dynamic than a fixed focus on market mechanisms, or on conceptions of competitive equilibrium and claims that they lead to efficiency, and such like” (Fullbroke 2009, p. 166).

¹³ Guerrien (2004) in Fullbroke (2009)

Again this statement of Lawson seems to me countered by the fairly obvious facts about what kind of models, what kind of gospel is thought almost universally, globally as “Economic Theory” – mostly in majestic singular and capital T – as if there were no other “economic theories”, no competing paradigms. That most of the academic research in economics are focussed on the anomalies, the imperfections, the “externalities” does not change this. This must be seen as an attempt to make the theory more realistic. But it is still a fact that “New Growth Theory” could only allow increasing returns to scale on the macro level – since introducing it on the micro level would “ruin” general equilibrium.

In the same way the “analytical Marxists” are certainly not seen as a challenge, because their method is also static, general equilibrium. No price changes, no endogenous technological change, a totally uniform rate of profit etc. That the *verbal* “story” is a different one does not change this fact. It is neither a question of not “criticising” the market since the static general equilibrium model can easily be seen as a planned economy with the central planning office as the Walrasian auctioneer . This fact is elegantly used by Stiglitz in his “Wither socialism” as a pedagogical way to point to the information weaknesses of the *static* general equilibrium model. Main-stream economist are very clear (and correct!) in pointing out the “information problems” of “market socialism” – but the fact is that all this critique also hits static general equilibrium with full force, since basically – mathematically it is the same model.

The point is that the divide between heterodox and orthodox is not fundamentally a political one. The Austrians – symbolised by Hayek – are very pro-market, very pro-capitalism, very free market oriented, but they are methodologically dynamically oriented. They see the innovating entrepreneur as a fundamental driving force in capitalism. They see the income differences justified not by some marginal productivity, but as a reward for being innovative, being entrepreneurial.

The fact that most heterodox currents – as described by Lawson (2006) – are more or less left wing only shows that “establishment” is – for historical reasons *locked in* to static general equilibrium theory. But they could switch to Austrian theory or modern Schumpeterian/evolutionary theory. If the elites agreed to go for evolutionary theory as “Economic Theory” there would for the economic elites be a big gain in realism, explanatory power – but also – given the “path” (theory) they did choose – a big loss in “normative” power. In evolutionary theory Government intervention is not *fundamentally* inefficient – on

the contrary – since markets are not viewed as self-regulating. The same goes for free trade, progressive taxes, strong unions, state provision of health, education, small income differentials etc.

The latter type of policies one can say archetypically characterises the Nordic countries. It is not accidental that recently there has been established a “centre of excellence” at the department of Economics at the University of Oslo precisely to study this Nordic paradox. That is – to do all the “wrong” things for decades and despite that prosper – decade after decade. As one can read from the centres web site under the title: “Against all odds: the Nordic welfare states”:

“Low income disparity, high taxes and generous welfare benefits are the perfect recipe for economic disaster - in theory. But the Nordic welfare states are prospering with exactly that formula. Our theory is that there is more than one approach to achieving strong economic results, not just the market-based model with US-inspired institutions at its core. The Nordic economies substantiate this, so perhaps traditional economic theory needs to be re-evaluated against them. [...] Economic theory can expect a serious challenge!”¹⁴

One cannot fail to note that “economic theory” – still is in singular. It is in my opinion rather obvious that it is *only* to a static “optimal” model with its traditional “results” that the Nordic experiences constitute a theoretical challenge. It is no problem in a dynamic growth model to show that there is a lot of *positive* feedback from high wages creating high aggregate demand for industrial goods – giving room for even higher wages. Did not Henry Ford pay his worker “extraordinarily” high wages so they could afford to buy the car they were producing. High wages gives a greater stimulus to innovate, to mechanise etc. The same goes for state intervention - also a lot of positive growth feedback. But then of course – you are constantly

¹⁴ <http://www.rcn.no/servlet/Satellite?c=Nyhet&pagename=sff%2FHovedsidemal&cid=1224067014133&p=1224067001881>

far from perfect “competition” static equilibrium! That is - the economy is fortunately very far away from perfect *stagnation!*

But these – rather obvious – political aspects of orthodoxy are not focussed by Lawson and that means that Lawson has to search for an explanation for the “obsession with methodology”. Denying that the orthodoxy is an ideological-political phenomenon he turns to two types of explanation – math fetishism and psychology. Psychology is needed according to Lawson to explain “why the mathematising project *remains dominant in economics in the face of repeated failure*” (Lawson, 2006, p. 170). He suggests “that a psychological explanation is likely required, perhaps focusing on the non-gender-neutral composition and orientation of the typical economics academy”. Lawson points to a recent article by Vinca Bigo (2008) who discusses the difference in the upbringing of boys and girls as a fundamental, “Freudian” explanation for the math fetishism. This explanation – while clearly not being totally wrong - seems to me to be far fetched since the politico-ideological aspect of orthodoxy to me is by far the most obvious reason for it’s dominance. I will consequently focus here on Lawson’s discussion of the role of mathematics in modern economics.

In “Reorienting Economics” there is a whole chapter on “An explanation of the mathematising tendency in modern economics”. This is a complex issue since two arguments are generally correct:

- a) Mathematical tools have been of great help in the sciences, also in the social sciences.
- b) There has been and is an unwarranted respect and belief in mathematical formulations being “better” having more scientific value than “verbal” arguments per se.

In my view Lawson gives too much weight to b), because just as one does not need to be a naïve techno-optimist, i.e. uncritical of all kinds of technological progress to be of the opinion that technological development have improved the human condition, one do not need to turn into a naïve mathematico-

optimist even if one seeks the use of logic expressed in mathematical form in the social sciences. The crux of the matter is – and here I agree with Lawson - that the mathematical tools must be appropriate to the ontological and epistemological nature of the object of study. Since society is dynamic, the mathematical tools must aid analysis of the feed-backs, the non-linearities, the “chaotic” properties of the system. That could be dynamic systems (systems of difference/differential equations) solved by numerical methods as in many sciences, or statistical mechanics¹⁵. There is in my opinion per se nothing wrong when economists looked to Newton, or evolutionary theories in biology etc. The point here is that the choice of mathematical tools is not a neutral, objective one, but unavoidably influenced by the results one wants to prove. As Kanth (1999) formulates it:

“Of course, it would be naïve to imagine the scientific process as a sequence moving in that (logically), schematic, if satisfactory, order, from initial assumptions to final policy; the truth is exactly the obverse: *it is policy that guides the selective choice of assumptions, with ‘theory’ a mere rationalisation of the former.*”

This point is in a way discussed by Lawson (2003) in two passages entitled “The political environment” and “The post-World War II US context” respectively. The latter passage has a long quote from E. Reinert (2000) pointing out how the Cold War and McCarthyism influenced the developments of economic theory. Reinert argues that to counter the demands for “protectionism” and later a “New economic world order” from the Third World countries – the answer of orthodoxy was comparative advantages, factor price equalisation, proving the “optimality” (benefit) of to free-trade for all. As Reinert points out: “The pure neo-classical techniques in which economic harmony is already solidly built into the basic assumptions – providing results like Samuelson’s *factor price equalisation* – was the kind of theory that was ideologically

¹⁵ An example of the use of statistical mechanics in economy is “The laws of chaos”, Farjoun and Machover (1983) analysing the classical Marxian “controversies” by tools taken from physics. Farjoun and Machover are actually forerunners of the econo-physics school.

and politically in demand. We are not suggesting that this kind of theory was created for political purposes. The theories had been there essentially since Ricardo, but the *demand* for this kind of theorising rose considerably during the Cold War, sharpening its focus and message, but conveniently leaving aside the mitigating counter arguments of history ... in this way the ‘technicians’ crowded out the ‘intellectuals’ of the economics profession. (Lawson, 2003, p. 276)

Lawson comments Reinert:

“Clearly Reinert, in drawing attention to the nature of the post-war US context, is focussing as much on the content of the (sorts of) theories that thrived as on their formalistic nature. But the nature of the content is always constrained by the method. And in any case, the arguments about the selection have even more bearing when we focus on the use of technique *per se*, and particularly on those instances in which the construction of (formalistic) structures were held to have no necessary interpretation whatsoever”. (p. 276)

We see here how Lawson turns Reinert’s clear statement of the “ideologisation” of economics almost on its head, from a focus on what content is politically in demand to a question of what *methods* is in demand; arguing that static, axiomatic, general equilibrium fix-point techniques a la Debreu “have no necessary interpretation”. Regarding this “emptiness” there are at least two remarks to make, first that it is true that the Arrow-Debreu model is “empty” in a certain mathematical sense, but that is clearly no major obstacle for “interpretations” that have no basis in the maths. This schizophrenia is clearly expressed by Debreu himself:

“Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations. In order to bring out fully this disconnectedness, all the definitions, all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics; moreover the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions: ‘in the language of the theory’, ‘for the sake of the theory’, ‘formally’. Such a dichotomy reveals all the assumptions and logical structure of the analysis” (quoted after Lawson (2003, p. 272), his emphasis)”

In a normal science where a formal statement might have several “normal”, i.e. interpretations grounded in reality, such a separation is of course allowed. But in the case of orthodoxy this is not so, because what the theory is supposed to say something about – the working of a market economy, the “invisible hand” – it cannot say anything about, due to the static nature of the mathematical model. Or as it was formulated by Haavelmo (1974) at the same time¹⁶ as Debreu (1959) published his “A theory of value”:

“As is well known, the 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 “off 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.”

But orthodoxy lives on this Ibsenian, “vital lie”, this fundamental illusion that the Arrow-Debreu model not only says “something”, it proves a lot of things, especially the “sacred” neo-liberal results¹⁷. But as Haavelmo points out – without dynamics nothing can be said or proved:

¹⁶ Regrettably only in Norwegian, in a “Festskrift” to the Danish economist Zeuten. Later translated into English and republished in *Economic Inquiry* in 1974, which is the version used here.

¹⁷ I do not use the term “pro-market” since the model is not a description of markets at all. Real markets are both creative and destructive and one cannot be one-sidedly pro- or anti- such a contradictory phenomenon.

“As was mentioned, the usual answer to the difficulties that we have discussed is quite simple: just make the theory dynamic. 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!”

The bottom line – as already Walras knew – is that dynamics has to be the next step, that is stability has to be proven. But while Walras could be naïve and believe in *tatônement*, while Hicks could still hope that the problem of “dynamisation” of the model could be solved by even more competent mathematicians than himself – as time goes by and the mathematical results showing the problems (impossibility) of making the theory dynamic – one has to take these results into consideration. The use of maths in this context is very important, because if you – more or less enthusiastically – accept the axioms then you just have to accept the results. No sane person can be opposed to logic. This is the reason why orthodoxy cannot rely on “verbal” reasoning – it does not force the adversary to accept the results – mathematical reasoning does. But that is also why the question of stability of static general equilibrium is a key question.

Stability – the Holy Grail that cannot be found

After Debreu there was a rather intense and generally optimistic effort to prove stability¹⁸. For a description of the maths involved, see Ingrao and Israel (1990). Here I am going to discuss the politico-theoretical implications of the lack of such a proof. My core argument has been that the lack of realism or orthodoxy was not intended or wished, but a strictly logical result of giving priority to the “utopian-normative” (= politico-ideological) uses of the theory, rather than the scientific (objective-descriptive). As Ingrao and

¹⁸ Debreu himself was not part of this effort. Ingrao and Israel (1990) in their last chapter argue that he already knew that this effort would not be rewarded by success.

Israel point out these live more and more separate “parallel lives” (p. 290). This schizophrenia is especially clear when it comes to stability. Or in the words of Ingrao and Israel:

“An ideological standpoint that regards the market as possessing the virtue or intrinsic property of combining subjective behaviour harmoniously cannot content itself with simply knowing that a final state of equilibrium exists. It has to show that the economy is capable of attaining this state spontaneously, that the system’s variables of state – i.e., prices – vary and adjust in such a way as to arrive at a vector of equilibrium prices. Otherwise, one would be forced to acknowledge that market forces are not capable of leading the market itself to equilibrium and that Smith’s “invisible hand” wavers Sisyphus-like around the actually existing equilibrium without having the strength to push the economic system into it. “ (p. 331)

The key word here is of course ideological, and although Lawson quotes and draws upon Ingrao and Israel he seems to ignore such clear statements regarding the ideological character of the orthodox enterprise. This is even more striking in the case of Hahn. Ingrao and Israel write:

“Formal analysis of the theory most explicitly proclaiming ‘the ability of a competitive system to achieve an allocation of resources that is efficient in some sense’ would be incapable of showing how the system can attain equilibrium except ‘by decree’, i.e., by directly *imposing* a system of equilibrium prices in a way that would be possible only in a centrally planned economy. This view would be tantamount to dropping the claim that the market possesses all the wonderful properties attributed to it and thus incompatible with the standpoint referred above.”

The authors have a footnote to this passage saying: “Hahn is certainly the most consistent and explicit representative of this school of thought (see Hahn 1982a, b). In my opinion Hahn is more schizophrenic, but one of Hahn’s egos is this one.

Ingrao and Israel also has a very illustrative description of the fact that even the “mathematicians” are not value free in the case of one of the expert contributors to the post-Debreu work on uniqueness and stability Smale, “a mathematician rather than an economist” according to Ingrao and Israel. This “mathematician tackling the theory of general equilibrium with a completely free mind, with none of the traditional ideological prejudices that influence the attitudes of so many researchers more intimately occupied with the theory”. But this “pure” mathematician is also crystal clear on the character of the enterprise:

“After all of this is said, equilibrium theory will eventually stand or fall, depending on its truth as an important idealisation of actual economic systems or as a model with values of justice, of efficient distribution and of utilisation of resources. As a normative theory, I find great merit in its decentralisation features...”

The point here is not that one should seek a un-engaged, value free science. Very often the best science, the most interesting and above all relevant results come from socially and politically engaged scientists. The function of science is to critically test the realism of paradigms and hypothesis. The problem for orthodoxy is just that even on the most abstract mathematical level – it is a failure. Giving again the floor to Ingrao and Israel:

“While no agreement has yet been reached as to the implications concerning uniqueness, those concerning global stability (i.e. the market’s ability to attain equilibrium) are unquestionably negative”. So far, this difficult state of affairs has rarely been faced up to with the necessary coherence.” (p. 361)

The reason why these mathematical results have not been faced squarely cannot in my opinion be “insistence that such methods” should be used always and everywhere as Lawson argues. There are no longer any “mathematical” reasons for clinging to the paradigm, but there are clearly ideological ones. In addition comes of course personal prestige, the prestige of the economics profession. But the latter are clearly of a secondary nature.

Curiously enough Ingrao and Israel does not discuss one major attempt at laying the “Disequilibrium foundations of equilibrium economics”, to quote the title of Franklin M. Fisher’s book from 1983. This book is significant in that it tries to get rid of one of the major unrealistic features of static general equilibrium – that trading is not allowed to disequilibrium prices – another feature of the orthodox model that most people and many economists are not aware of. Fisher also tried to incorporate another central feature of capitalism – innovation, what Fisher called “a favourable surprise”. Fisher was also (like Hahn sometimes) concerned that “if comparative statics is to be useful, the adjustment process must not only be rapid and thus unimportant in terms of real time, it must also be unimportant in terms of its effect on equilibrium. In the present state of our knowledge, there is no basis for the belief that this is the case” (Fisher, 1983, p. 216). I guess that it comes to no surprise to the reader that Fisher’s attempt to introduce

some realism, to dynamise the static general equilibrium was unsuccessful. But that was Fisher anno 1983. It is relevant in this context to take a look at a recent assessment by Fisher of the state of the art regarding stability. Fisher introduces his assessment by stating:

“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.”

Fisher – like most others – and unlike Lawson – sees the political implication of both the mathematical tools and the (lack of) mathematical results. When discussing Samuelson’s contribution to finding a solution to the question of the stability he goes directly to the heart of the matter:

“The problem with it [Samuelson’s dynamic price equation] however, is that, appealing as it is, we have no good reason for believing in it. This can be described as follows: 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?”

Fisher – although rather pessimistic – has still not given up all hope. He concludes by saying: “Still if progress is going to be made, economists cannot refrain from considering such issues. One cannot understand the working of the Invisible Hand by examining only situations where the Hand already has done its work”.

But not only main-stream economists should be concerned by such issues. Also the heterodox critics should be familiar with the total failure of the Walrasian, main-stream enterprise to succeed on its own *mathematical* terms.

Conclusion

The basic hypothesis of the paper is simple – that the reason for orthodoxy’s lack of realism, its math fetishism – can fundamentally only be explained by the very important politico-ideological role that the theory has in every day economic policy making. Policies that – in my view - mainly serves the status quo and its economic elites. The obsession with method, with mathematical models is nothing but a reflection of the ideological need to (mis)use peoples rational and natural respect for logic, for maths, to get them to accept certain “truths” about a market economy.

To fight against the use of maths in general in economics is in my opinion both a bad and a lost cause, also the subset of “deductive” mathematical tools is to general a concept. One has to explain why orthodoxy painted itself into in to the sterile, static corner where it now is locked-in. It is important to highlight the mathematical fiasco of orthodoxy *on its own terms*. A fiasco that can basically only be explained by the politico-ideological misuse of the static general equilibrium model. Orthodoxy is not mathematically rigorous – it has a dynamic story and static maths.

That means that the task is to reorient economics towards dynamics. These dynamics will probably more often be formulated verbally as is the case with Marx, Schumpeter, Keynes – or by means of systems of dynamic (time dependent, path dependent equations, of taking the form of (econometric) simulations. This seems to me to be a much more fruitful direction to go for critics of orthodoxy than insisting on a turn to a correct but far to general and abstract ontology.

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