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FOLEY'S THESIS, NEGISHI'S METHOD, EXISTENCE PROOFS AND COMPUTATION♠

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♠ This refers, of course, to Duncan Foley's remarkably original and brilliant -- and unfortunately little acknowledged -- Yale doctoral dissertation of 1967 (Foley, 1967). In the by now vast orthodox literature on computable general equilibrium theory, and its applied off shoots, I have not been able to detect a single reference to what I may call *Foley's Method* (ibid, pp. 64-73), which could provide -- as legitimately as *Negishi's Method* -- an alternative to the orthodox approach to `computing' Walrasian (Arrow-Debreu) equilibrium prices, using, for example, variants of *Scarf's Method*, (Scarf, 1973).

Abstract

Duncan Foley's many-faceted and outstanding contributions to macroeconomics, microeconomics, general equilibrium theory, the theory of taxation, history of economic thought, the *magnificent dynamics* of classical economics, classical value theory, Bayesian statistics, formal dynamics and, most recently, fascinating forays into an interpretation of economic evolution from a variety of complexity theoretic viewpoints have all left – and continue to leave – significant marks in the development and structure of economic theory. He belongs to the grand tradition of visionaries who theorise with imaginative audacity on the dynamics, evolution and contradictions of capitalist economies – a tradition that, perhaps, begins with Marx and Mill, continues with Keynes and Schumpeter, reaching new heights with the iconoclastic brilliancies of a Tsuru and a Goodwin, a Chakravarty and a Nelson, and to which Duncan Foley adds a lustre of much value.

In this contribution I return to mathematical themes broached in Foley's brilliant and pioneering Yale doctoral dissertation (Foley, 1967) and attempt to view them as a *Computable Economist*[1] would.The intention is to suggest that algorithmic indeterminacies are intrinsic to the foundations of economic theory in the mathematical mode.

JEL Codes: C02, C63, D58, E17

Keywords: Equilibrium existence theorems, Welfare theorems, Constructive proofs, Computability.

[1]I coined the description *computable economics* at the end of the 1980s to characterise the formalization of economic theory and its various closures in terms of the mathematics of computability theory and constructive analysis. More recently I have to adopt the slightly more general description *algorithmic economic theory* (cf. Velupillai, 2011).

1 Introduction

"Consider a two-agent, two-good exchange economy with given initial allocation of goods over the agents.

a). Explain *Negishi's strategy for proving the existence of a Walrasian equilibrium.* Illustrate his procedure in an Edgeworth box diagram."

Duncan Foley's **Advanced Microeconomics** *Examination in Class*, New School University, May 10, 2005, *2nd Question*; italics added

In an interview held on November 30, 2001, at his apartment in New York City, reported in Colander, et.al., (2004), Foley, in answering the Interviewers' specific question to 'tell [them] about [his] thesis', made the interesting and important observation that:

> "My thesis adapted the methods of general equilibrium theory to the problem of public goods. .. It has some results that I still think are neat. I proposed an analog of competitive equilibrium for public goods .... and *proved* that it was Pareto efficient...... . *[T]he method was exactly the same as Koopmans's proof* [in Koopmans (1957)] of the Pareto efficiency of price equilibria. .... In proving this theorem I independently discovered *Negishi's method* for proving the existence of Walrasian competitive equilibrium, which is based on *moving along the Pareto efficient surface* by changing household weights in a social welfare function rather than using a Tâtonnement argument on prices."
>
> Foley, 2004, p. 186; italics added.

I have known Duncan Foley, personally, for over a decade and have, in that period, communicated with him intensively (via the ubiquitous possibility provided by the e-mail) on many topics - both professional and personal, with a clear dominance of the former. No subject occurred with more frequency or intensity than the problems posed by considerations of computability of Walrasian equilibria in their Arrow-Debreu versions and their existence proofs underpinned by non-constructive methods. In the ten years - or so - we have exchanged correspondence on these issues - and the couple of times we were able to meet personally and talk about them - it was clear that there was some 'convergence' towards mutually agreed (and agreeable) positions had been achieved, albeit commencing from vastly different starting positions. I believe Foley came around to understand my conviction - based on absolutely rigorous and completely documented (in the form of fairly respectable mathematical publications, cf., for example, Velupillai (2006) and Velupillai (2009)) - on the uncomputability of Arrow-Debreu equilibria and also the appeal made, in almost all orthodox mathematical economics when generating existence proofs, to non-constructive methods of proof.

For example in an e-mail to me as recently as 11 October, 2010, with its subject denoted by '*Computability of Walrasian Equilibrium*', Foley wrote (italics added):

> I take your position to be that Walrasian equilibrium is *uncomputable* in the price simplex due to the *non-constructivity* of the Bolzano-Weierstrass theorem, *a conclusion with which I agree*[2].

In an earlier correspondence, on 26 January 2010, this time with the subject of the contents referred to as: *Constructibility of exchange equilibrium*, Foley wrote (again, italics added):

> "I take your point that conventional ("ordinary") economic theory operates uncritically in the domain of real analysis, and that if the problem of exchange equilibrium [that is, a point in or near the contract set] is posed in this fashion finding a point in the exchange equilibrium (Pareto set) is non-constructive. "

To this e-mail, Foley appended an earlier interchange between us on related issues, where he had written me (italics added):

> "My conjecture is that there is a *constructive algorithmic* (and even economic) *method* to find exchange equilibrium (not necessarily Walrasian equilibrium) allocations by allowing agents to find and make mutually advantageous trades. I'm not sure you and I ever reached a meeting of minds on this question, but I'd like to pursue it further."

My response to this important *conjecture* was (italics in the original):

> "Unfortunately, I am unable to agree with your 'conjecture'. To 'find [an] exchange equilibrium', with a 'constructive algorithmic method' (by the way, *if* 'constructive' *then* 'algorithmic'!), means you must first define the domain and range of the agents' decision space 'constructively'. Once you do this, the whole problem of an algorithm becomes trivial. 'Allowing agents to make mutually advantageous trades', means - in algorithmic mathematics - that these 'trades' are in terms of constructive or computable numbers, which they are not in any kind of ordinary economic theory."

It has only gradually dawned on me that Foley has always had in mind – or, perhaps, only in the deep recesses of his fertile mind – '*Negishi's method* for proving the existence of Walrasian competitive equilibrium', which he had

[2] My 'position' is more 'radical', as could be discerned from the results reported in Velupillai (2006, 2009). The uncomputability of Walrasian equilibria is *not only* due to the intrinsic *undecidable disjunctions* in the Bolzano-Weierstrass theorem, but also due to the *methods of proof* used in the Brouwer (or Kakutani) fixed point theorem - and other constructive and computability issues, all of which are discussed in the above two cited papers.

‘independently discovered’ over thirty five years ago, and for which discovery the orthodox mathematical economists have never given him adequate - indeed, *any* - credit. Not even usually sympathetic doctrine historians, specialising in the development of mathematical economics, have - to the best of my knowledge - acknowledged Foley’s pioneering contribution, from this point of view, in his remarkably original thesis. From my personal point of view, it is a matter of lasting regret that the thesis was never published as a book, that could have been used as a text in any serious graduate course in mathematical economics in which refreshing economics was coupled to innovative mathematics. Above all. it could have provied a broader social and political scientific basis for one strand of computable general equilibrium theory.

In this paper I take up the thread, with which I have been weaving a tale, on computability and constructivity in mathematical and computational economics, mostly via ‘conversations’ with Duncan Foley in e-mail interchanges, and try to make explicit my standpoint on *Negishi’s Method*. This is the subject matter of section 3, below, preceded by a section 2 where I outline my more professional indebtedness - one that is common to my own generation of theoretically-minded, mathematically literate and (I hope!) socially and politically enlightened economists - to the vast and fascinating canvasses on which Duncan Foley has been fashioning a vision of economics that is far richer than any kind orthodoxy has been able to devise. The concluding section is a reflection on the lessons that one might be able to extract for a kind of economic theory that is formalised in terms of an empirically useful mathematics.

2 A Personal Preamble

> "If the society as a whole has the *complexity level of a general-purpose computer* [or Turing Machine], it will be *impossible* for any other general-purpose computer to work out its evolution except by direct simulation. To carry out the program of rational explanation of behavior in this context would require positing that each individual agent in society had some way of simulating the potential evolution of a system of interlinked Turing machines. At this point the rational explanation program runs into deep paradoxes of self-reference."
>
> Foley, 1998, p. 40

This pithy characterisation of the conundrums of the ‘rational explanation [research] program’ in economics is typical of the way Duncan Foley has been able to locate one or another of the lacunae in orthodox economic theory, whether mathematically underpinned or not. Essentially, Foley has invoked the celebrated *Halting Problem for Turing Machines* to point out where the ‘rational explanation of behavior’ must fall foul of formal possibilities of evolutionary predictability. As a matter of fact, it is not even necessary – for this failure to manifest itself – to posit ‘that each individual agent ... had some way of simulating the potential evolution of interlinked Turing machines'; it is sufficient to posit that 'society as a whole has the complexity of [one single Turing machine]'. Moreover, even if the 'individual agent had some way of simulating the potential evolution of [one single] Turing machine', it will not be possible to circumvent the consequences of the *Halting problem for Turing Machines.*

I was a graduate student of economics at the University of Lund in the very early 70s. At that time in Lund the writings of the great past economic pioneers of that University were still being assigned to graduate students almost routinely (*in the original languages*: Swedish, Norwegian and German). Even standard graduate courses in Macroeconomics, Growth Theory and Capital Theory had, in their required reading lists, texts by Wicksell and Lindahl, Lundberg and Ohlin and Palander and Landgren. So, it was not surprising that the graduate course in Public Finance was heavily influenced by the Swedish tradition in Public Finance and Taxation – i.e., the tradition emanating from the doctoral dissertations of Wicksell and Lindhal (but also influenced by Leif Johansen's contemporary work at Oslo). We were genuinely fortunate graduate students at Lund.

Thus, already in the early 70s, I was introduced to Duncan Foley's pioneering work on 'Lindahl prices'. Both his doctoral dissertation (as published in *Yale Economic Essays*, Foley, 1967) and his *Econometrica* paper of 1970 (Foley, 1970) were part of the required reading, together with parts of Wicksell's **Finanzthoretische Untersuchungen**[3] (Wicksell, 1896) and more substantial parts of Lindahl's **Die Gerechtigkeit der Besteurung**[4] (Lindahl, 1919). Foley was placed in the grand tradition of Swedish Public Finance, as he was entitled to.

However, two important caveats should be added to this legitimate claim to a Swedish lineage for Foley's early work on public finance in the general equilibrium tradition. Firstly, the internal debate in Sweden, particularly between Myrdal (1930) and Lindahl (1959) somehow did not get reflected in the details of Foley's thesis, nor in his later Marxist oriented stance on public finance. I state this only because Myrdal's sustained critique of the Wicksell-Lindahl tradition in public finance (*ibid*, particularly chapter 7), would, I am sure, have been handsomely endorsed by the later Foley (1978)[5].

Secondly, Foley's lifelong work on macroeconomic theory has not reflected the possible link one can discern between the public finance of the Swedes –

[3] My copy of this book once belonged to *Östen Unden*, Sweden's acting Prime Minister in the transition regime between Per Albin Hansson's death and the eventually long tenure of Tage Erlander. Unden served also as the Social Democrats' Foreign Minister for a long period under Erlander, but also for a brief period in the mid-1920s. My copy of this Wicksellian classic is dated 'Mars 1907' by Unden, i.e., when he was still only 20 years old!

[4] I am in the happy and privileged position of owning the author's presentation copy to Wicksell's personal friend, Gustaf Steffen!

[5] My first encounter with this line of research by Foley was in late 1977, during my tenure as a Research Fellow at C.O.R.E in Louvain-La-Neuve, during the academic year 1977/78. In the autumn of 1977 I obtained, quite accidentally, a copy of Foley's draft paper on *Marxist Theories of the Fiscal Process*, prepared for an International Seminar in Public Economics, at Namur, in Belgium, in November, 1976. I still own an almost tattered copy of the paper (Foley, 1976)!

particularly in how it determined a very special vision of national income accounting and balance-sheet constructions, as stressed by Hicks in his beautiful *Lindahl Festschrift* contribution (Hicks, 1956) – and the macroeconomic dynamics they developed, largely prior to and independently of Keynes of the *General Theory*. I have tried, over the years, to stress this connection, as a way of suggesting a foundation for macroeconomics in the Swedish tradition in public finance (Velupillai, 1991).

In any case, being a graduate student at Lund was my path towards an introduction to Duncan Foley's pioneering work. Since then, from time to time, my own visions have been broadened and deepened by Duncan Foley's sustained original contributions, in a wide and interesting variety of ways, to various frontiers in economic theory, latterly mostly in macroeconomics, a field that has always remained central in my research agenda and teaching activities.

By the time I went to Cambridge in 1973 and my own interest in Macroeconomics was being fostered by the great Cambridge pioneers of that field (particularly Nicky Kaldor and Richard Goodwin, my PhD supervisors at Cambridge), the *Foley-Sidrauski book* (Foley & Sidrauski, 1971) had also become standard reading for us. Growth theory, capital theory and distribution theory were the exciting topics being discussed at Cambridge at that time and once again I had a Foley work assigned as required reading (the Foley-Sidrauski book).

I teach advanced macroeconomics at the Graduate level in Trento. Last year I began using the finely crafted *Foley-Michl book* on *Growth and Distribution* (Foley & Michl, 1999) as the main supplementary textbook for this course, where growth theory is emphasized[6]. The *Foley-Michl book* is refreshingly modern but is so by showing and grounding the evolution of modern theories of growth in the works of our great classical masters.

I mention it this way to show a remarkable consistency in the way Foley's masterly contributions to economic theory proceeds – at least in the way I have learned from his writings. They are always grounded in the works of our classical and neoclassical pioneers. Unlike much modern work, where it is either a Whig interpretation or, worse, ahistorical stories, Foley's work reminds us that we stand on the shoulders of giants, mostly past masters but often also contemporaries[7].

These aspects are beautifully developed and evident in his '*Schumpeter Lectures*' (Foley, 2003). His mastery of the classical economists for their broad insights, for their magnanimity and, above all, for their visions of what Baumol called 'magnificent dynamics', is unsurpassed in modern economic scholarship, especially – but not only – because of Foley's remarkable mastery of a wide variety of mathematical techniques, in addition to a deep understanding

[6] The main assigned textbook was Lance Taylor's *Reconstructing Macroeconomics* (Taylor, 2004), one of the most refreshingly original alternatives to the orthodoxies of the Newclassicals and the New Keynesians.

[7] Lindahl was not long dead when Foley's work on Lindahl prices first appeared; I should like to add that when I myself went to Cambridge for my PhD and was interviewed by Nicky Kaldor his only question to me was: 'Why do you want to come here when you have Lindahl in Sweden?" – this in 1973 and Lindahl had been dead just over ten years! His spirit, as those of the other contemporary and past Swedes, was very much alive in Cambridge at that time.

of mathematical philosophy.

Mentioning Baumol in the above paragraph recalls to my mind the way I finally resolved a perplexity regarding Foley's intellectual work. I had wondered why his work on Lindahl prices did not continue and how the macroeconomic work got done, almost in a parallel fashion and they both, then, led to his political economy outlook on public expenditure, on the one hand, and, on the other, how the monetary-fiscal framework of the work with Sidrauski – still relevant at some frontiers of macroeconomic theory – moved on to issues of growth, distribution and capital theories, often in the Cambridge (UK) tradition. By chance, my interest in the theory of public finance was reignited in the late 80s and I had occasion to read Baumol's book of that period on *Superfairness* (Baumol,1986). There, on p.72, I read as follows:

> "All of [the work on fairness theory] appears to have its roots in Foley's writings. .... Foley reports that he was led to think of the [fairness] criterion after seeing a movie in which Bob Dylan graphically emphasized the importance of fairness issues."

I think I understood then how there was a unified thread in Foley's work in Public Finance in the framework of general equilibrium theory, Macrodynamics, History of Economic Thought and the evolution of capitalist institutions: it was a passionate concern for *fairness* in all its dimensions and ramifications. This is also why I think Duncan is such a fine and knowledgeable all-round economist, with a wonderful mastery of so many aspects of economic theory. Again, his mathematical competence makes it easy for him to master many frontiers of economic theory and keep abreast of developments.

My own work in the last two decades or so has been in that exciting interface between dynamical systems theory and computability and computational complexity theory. In addition to this I have also been concentrating on the relevance of constructive mathematics for formalizing economic theory – particularly microeconomics and economic dynamics. There are few economists I'd rather talk to, converse with, have my works read, for critical comments, than Duncan Foley[8].

Duncan Foley's long *Introduction* to the collected essays of Peter Albin (Foley, 1998), is a masterly piece, linking dynamics and computability theory, via the formalism of *Formal Languages Theory.* It contains one of the most pedagogical expositions of the *Chomsky Hierarchy* for economists. I have never failed, in the last decade or so, to have it in my various reading lists for almost any course of lectures I give. In that *Introduction* – but not only in that one – Duncan's expository skills, backed by vast and deep mathematical knowledge and competence, underpinned by a thorough understanding of economic theory, comes to surface in a most felicitous way.

Foley has, in recent years, been focusing on interpreting economic phenomena from the point of view of varieties of complexity theories. He has also begun to wonder about thermodynamic interpretations of economic processes. They

[8]With the obvious exception of my friend and colleague, Stefano Zambelli.

are not new issues to his fertile mind; but they seem to be taking a new turn, underpinned by new visions. I expect these new directions will bear fruit in the same way his thoughts have led to innovative works, right from the beginning, with those *classics on classics*: Lindahl prices, motivated by issues of fairness.

Thus, it must be the case that fairness is one motif in the humane tapestry Duncan Foley has made available to many of us, when we consider ourselves economists in the humanistic mode. Perhaps I am also on the right track when I believe his recent work and interest in nonlinear dynamics and complexity are motivated by his incessant search for the first principles of what Karl Polanyi felicitously called *The Great Transformation* – the transformation that was brought about by Industrialization to the advanced economic societies.

Somehow, despite these many faceted interests and contributions, and the central motif of fairness, I have always felt that mathematical rigour and computation have played an important part in the way Foley has theorised, even when no explicit formulas, equations or other mathematical hieroglyphics appear in his imaginative writings. In my own decade-long 'conversations; with Duncan Foley these two aspects have been the dominant themes. Perhaps that is why the subject matter of this contribution, to pay homage to Foley, is underpinned by the conundrums of *proof* and *computation.*

3 *Negishi's Method*, Fundamental Theorems of Welfare Economics, Equilibrium Existence Proofs and Computation

> "In proving the existence theorem I independently discovered *Negishi's method for proving the existence of Walrasian competitive equilibrium*, which is based on *moving along the Pareto efficient surface* by changing household weights in a social welfare function rather than using a tâtonnement argument on prices."
> Foley, 2004, p. 186; italics added.

Negishi himself, reflecting on his youthful masterpiece[9] more than three decades later (Negishi, 1994, p. xiv; italics added), remarked:

> "The *method of proof* used in this essay [i.e., in Negishi (1960)] has been found useful also for such problems as equilibrium in infinite

[9] Foley was not quite 30 years old when 'proving the existence theorem' and Negishi still only 27. In both cases their respective contributions were among the first few formal publications in two outstanding academic careers, each pathbreaking in its own way. These facts alone place in perspective Clower's mature reflection (Clower, 1984, pp. 263/4):

> "Economics is less obviously a young man's game than mathematics, but I doubt that many economists have had a really new idea after the age of thirty-five. I must confess that, one way or another, everything I have done in the second half of my life reminds me (at least retrospectively) of something I did or thought about earlier."

dimensional space and computation of equilibria."

What exactly was Negishi's *method of proof* and how did it contribute to the *computation of equilibria*?

Duncan Foley's pithy characterisation of the difference between the standard approach to *proving the existence* of an Arrow-Debreu *equilibrium*, and its *computation*, by a tâtonnement procedure – i.e., algorithm – of a mapping from the price simplex to itself, and the alternative *Negishi method* of iterating the weights assigned to individual utility functions that go into the definition of a social welfare function which is maximised to determine – i.e., compute – the equilibrium, captures the key innovative aspect of the latter approach. Essentially, therefore, the difference between the standard approach to the proof of existence of equilibrium Arrow-Debreu prices, and their computation, and the *Negishi approach* boils down to the following:

- The standard approach proves the existence of Arrow-Debreu equilibrium prices by an appeal to a fixed point theorem and computes them – the equilibrium prices – by invoking the *Uzawa equivalence theorem* (Uzawa, 1962) and devising an algorithm for the excess demand functions that map a price simplex into itself to determine the fixed point (Scarf, 1973).

- The Negishi approach *proves*, given initial endowments, *the existence* of individual welfare weights defining a social welfare function, whose *maximization* (subject to the usual constraints) *determines* the identical Arrow-Debreu *equilibrium.* The standard mapping of excess demand functions, mapping a price simplex into itself to determine a fixed point, is replaced by a mapping from the space of utility weights into itself, appealing to the same kind of fixed point theorem (in this case, the Kakutani fixed point theorem) to prove the existence of equilibrium prices.

- In other words, the method of proof of existence of equilibrium prices in the one approach is replaced by the *proof of existence* of 'equilibrium utility weights', both appealing to traditional *fixed point theorems* (Brouwer, 1910, von Neumann 1937 (1945-6), and Kakutani, 1941[10]).

- In both cases, the computation of equilibrium prices on the one hand and, on the other, the computation of equilibrium weights, algorithms are devised that are *claimed* to determine (even if only *approximately*) the same fixed points.

Before proceeding any further, I should add that I am in the happy position of being able to refer the interested reader to a scholarly survey of Negishi's work. Takashi Negishi's outstanding 'contributions to economic analysis' are brilliantly and comprehensively surveyed by Warren Young in his recent paper

[10] There is a curious – albeit inessential – 'typo' in Negishi's reference to Kakutani's classic as having been published in 1948. The 'typo' is not 'corrected' even in the reprinted version of negishi (1960) in Negishi (1994).

(Young, 2008). Young's paper provides a particularly appropriate background – together with at least a nodding acquaintance of § II & § III of Foley's Thesis (Foley, 1967, pp.64-76) – to the issues I tackle in this section. It – Young's paper – is especially relevant also because his elegant summary of Negishi's 'contribution to economic analysis' identifies Negishi (1960) as one of the two crucial pillars[11] on which to tell a coherent and persuasive story of what he calls the Negishi 'research program' (ibid, p. 162; second set of italics, added)[12]:

> "To sum up, a number of major research programs can be identified, therefore, as emanating from Negishi's now *classic* papers, that of (1960) [Negishi, 1960] and 1961 [Negishi, 1961], respectively. Negishi's 1960 paper forms the basis for both 'theoretical' and 'applied' research programs in general equilibrium analysis, and his 1961 paper ... has been *almost as influential* in demarcating ongoing research up to the present in the field of imperfect competition and non-tatonnement processes. These papers ... attest to Negishi's considerable influence on the development of modern economic theory and analysis."

However, no one – to the best of my knowledge – has studied Negishi's *method of proof* from the point of view of *constructivity* and *computability*, the issues that were at the centre of my dialogue with Duncan Foley. Young's perceptive - and, in my opinion, entirely correct - identification of the crucial role played by Negishi (1960) in 'both "theoretical" and "applied" research program in general equilibrium analysis' is, in fact, about *methods of **existence** proofs* and *computable general equilibrium* (**CGE**), and its offshoots, in the form of *applied computable general equilibrium analysis* **ACGE**) – even leading up to current frontiers in computational issues in *DSGE* models (cf., Judd, 2005, pp. 52-57, for example).

Before I turn to these issues of the constructivity and computability of Negishi's method of existence proofs and the underpinning of some aspects computation in CGE and ACGE models in Negishi's approach (rather than, for example, in the standard approach pioneered by Scarf, 1973), there is one important economic theoretic *confusion* that needs to be sorted out. This is the question of the role played by the fundamental theorems of welfare economics in Negishi's method of the proof of the existence of a general (walrasian) equilibrium.

It is generally agreed – and especially by Foley – that the *Negishi method of existence proof* is an applications of fixed point theorems on the *utility simplex*, in contrast to the 'standard' way of applying such theorems to the *price simplex* (cf., Cheng, 1991, p. 138, and above). This fact has generated a remarkable

[11]The other one being Negishi (1961). I am in full agreement with Young important observation that it is Negishi (1960) that is more important, which is why I have added italics to the phrase 'almost as influential', in the above quote.

[12]Even young's comprehensive list of references - 108, in all, in an article that distills the essence of the Negishi 'reserach program' in only 12 papges, manages to forget to refer to Foley (1967).

confusion on the question of which fundamental theorem of welfare economics underpins the Negishi method! For a method that has been around for over half a century, it is somewhat disheartening to note that frontier research and researchers seem still to be confused on which of the two fundamental theorem of welfare economics is relevant in Negishi's method. Thus, we find Judd, as recently as only a few years ago (op.cit, pp. 52-3) claiming, unreservedly, that (italics added):

> "T*he Negishi method exploits the first theorem of welfare economics*, which states that any competitive equilibrium of an Arrow-Debreu model is Pareto efficient."

On the other hand, Warren Young (op.cit, p.152; italics added) equally confidentially stating that:

> In his pioneering 1960 paper, Negishi provided a completely new way of proving the existence of equilibrium, *via the Second Welfare Theorem*. He established equivalence between the equilibrium problem set out by Arrow-Debreu and what has been called 'mathematical programming', thereby developing a 'method' that has been used with much success by later economists working in both theoretical and applied general equilibrium modelling ... ."

Fortunately, Negishi himself returned to a discussion of the 'Negishi method, or Negishi approach' more recently (Negishi, 2008, p. 168) and may have helped sort out this conundrum (ibid, p. 167; italics added):

> "The so-called Negishi method, or Negishi approach, has often been used in studies of dynamic infinite-dimensional general equilibrium theory, and the numerical computation of such equilibria ... . This method is an application of the Negishi theorem (Negishi 1960), which demonstrates the existence of a general equilibrium *using the first theorem of welfare economics*, which states that any competitive equilibrium of an Arrow-Debreu model is Pareto efficient. In other words, a general equilibrium of a competitive economy is considered as the maximization of a kind of social welfare function (i.e., the properly weighted sum of individual utilities, where the weights are inversely proportional to the marginal utility of income."

Negishi is one of those rare economists who is both a scholar of the history of economic theory and one of the most competent general equilibrium theorists and – even if he had not been the originator of the Negishi method – therefore one may feel forced to reject Warren Young's claim[13]!

[13] The puzzle here is that the Young and Negishi articles appear 'back-to-back', in the same issue of the *International Journal of Economic Theory* and the two distingusihed authors thank each other handsomely in their respective acknowledgements!

As a matter of fact, from my *Computable Economics* – i.e., from a constructivist and recursion theoretic - point of view, this conundrum is a non-problem for several reasons. First of all, both fundamental theorems of welfare economics are proved non-constructively and lead to uncomputable equilibria. Secondly, all – to the best of my knowledge – of the current algorithms utilised in CGE, ACGE and DSGE modelling appeal to undecidable disjunctions and are effectively meaningless from a computablity point of view. Thirdly, and most importantly, *Negishi's theorem*[14] is, itself, *proved* nonconstructively. Finally, at the risk of challenging the mature reflections of one of the great and masterly contributors to general equilibrium theory, I would like to point out that there is no meaning that can be attached to *Negishi's method* if applied to '*dynamic infinite*-dimensional general equilibrium theory' – even if such a thing can be defined (or, indeed, has been defined, except in the utterly fictional and non-rigorous world of newclassical claims of dynamic stochastic general equilibrium modelling – i.e., so called DSGE modelling).

There are two theorems in Negishi (1960). I shall concentrate on *Theorem 2* (ibid, p.5), which (I think) is the more important one and the one that came to play the important role justly attributed to it via the *Negishi Research Program* outlined by Young (op.cit)[15].

Proposition 1 *The Proof of the Existence of Maximising Welfare Weights in the Negishi Theorem is Nonconstructive*

Proof. (Sketch) Negishi's proof relies on satisfying the Slater (Complementary) Slackness Conditions (Slater, 1950[16]). Slater's proof[17] of these conditions invoke the Kakutani fixed point theorem (Theorem 1 in Kakuani, 1941), and Kakutani's Min-Max Theorem (Theorem 3, ibid). These two theorems, in turn, invoke Theorem 2 and the Corollary (ibid, p.458), which are based on Theorem 1 (ibid, p. 457). This latter theorem is itself based on the validity of the Brouwer fixed point theorem, which is not just nonconstructive, but also non-constructifiable (cf., Brouwer, 1952). ■

Proposition 2 *The vector of maximising welfare weights, derived in the Negishi Theorem, is uncomputable*

[14] *Negishi's theorem* is one thing; *Negishi's method* is quite a different thing. The latter *should* refer to the 'method of proof', but the vast literature on the issue – admirably documented in Young (2008) – is *not* fee of confusion on this point. Essentially, the 'method' refers to the fact that a mapping is defined, not on the price simplex, but on the 'utility simplex' (as mentioned above with a reference to Cheng, 1991).

[15] To demonstrate the *nonconstructive* elements of Theorem 1 (ibid, p.5), I would need to include almost a tutorial on constructive mathematics to make clear the notion of *compactness* that is *legitimate in constructive analysis*. For reasons of 'readability' and 'deeper' reasons of aesthetics and mathematical philosophy, I shall refer to my two main results as'Claims' and their plausible validity as 'Remarks', and not as 'Theorems' and 'proofs', respectively.

[16] Slater (1950) must easily qualify for inclusion in the class of pioneering articles that remained forever in the '*samizdat*' status of a *Discussion Paper*!

[17] I should add that the applied general equilibrium theorists who use Negishi's method to 'compute' (uncomputable) equilibria do not seem to be fully aware of the implications of some of the key assumptions in Slater's complementary slackness conditions. That Negishi (1960) is aware of them is clear from his *Assumption 2* and *Lemma 1*.

Proof. A straightforward implication of Claim 1 ■

Discovering the exact nature and source of appeals to nonconstructive modes of reasoning, appeals to undecidable disjunctions and reliance on nonconstructive mathematical entities in the formulation of a theorem is a tortuous exercise. The nature of the pervasive presence of these three elements – i.e., nonconstructive modes of reasoning, primarily the reliance on *tertium non datur*, undecidable disjunctions and nonconstructive mathematical entities – in any standard theorem and its proof, and the difficulties of discovering them, is elegantly outlined by Fred Richman (1990, p. 125; italics added):

> "Even those who like algorithms have remarkably little appreciation of the thoroughgoing algorithmic thinking that is required for a constructive proof. This is illustrated by the nonconstructive nature of many proofs in books on numerical analysis, the theoretical study of practical numerical algorithms. I would guess that most realist mathematicians are unable even to recognize when a proof is constructive in the intuitionist's sense.
>
> *It is a lot harder than one might think to recognize when a theorem depends on a nonconstructive argument. One reason is that proofs are rarely self-contained, but depend on other theorems whose proofs depend on still other theorems. These other theorems have often been internalized to such an extent that we are not aware whether or not nonconstructive arguments have been used, or must be used, in their proofs.* Another reason is that the law of excluded middle [LEM] is so ingrained in our thinking that we do not distinguish between different formulations of a theorem that are trivially equivalent given LEM, although one formulation may have a constructive proof and the other not."

Finally, it is easy to demonstrate that Foley's theorems in section III of his pioneering thesis (Foley, 1967) are proved nonconstructively. For example, this is straightforward in the case of the Theorem in paragraph 3.29 (see 72, where Brouwer's fixed point theorem is invoked). It is less straightforward to demonstrate the upper-semicontinuity of the mapping on the simplex K (pp. 69-70). The theorem of paragraph 3.22 is, on the other hand, easily shown to be nonconstructive due its appeal to the Bolzano-Weierstrass theorem (p. 69). Similar considerations apply to the theorems of section II (but there separating hyperplane theorems are invoked, rather than fixed point theorems – but the former are no more constructive or effective than the latter, unless defined on very specially structured normed spaces).

These are further reasons to pay close attention to Richman's carefully spelled out constructive thoughts. For, a supreme economic theorist like Duncan Foley, who also happens to be mathematically able, could use words like 'construct' (for example, bottom p. 60) in an otherwise wholly nonconstructive setting and not suspect that 'proofs are rarely self-contained, but depend on other theorems whose proofs depend on still other theorems. These other theorems have often been internalized to such an extent that we are not aware whether or not constructive arguments have been used, or must be used, in their proofs.'

In any case, Foley's candid 'confession', in his interview of November, 2001 (Foley, 2004), quoted in the opening lines of this paper, should have made all this obvious. This is because he acknowledges that the method he used *'was exactly the same as Koopmans's proof* [in Koopmans (1957)] of the Pareto efficiency of price equilibria'. This, I believe is a reference to the pedagogically excellent, although mathematically deceptively simple, exposition in chapter 2 of Koopmans (1957). The methods used by Koopmans, a combination of fixed point theorems and separating and supporting hyperplane theorems, are intrinsically nonconstructive and uncomputable. Moreover, the whole method adopted by Koopmans in this beautiful exposition rules out Diophantine considerations – to which I now turn, in conclusion.

4 Diophantine Conclusions

> "I have argued here as forcibly as I can that it is a serious error to indict mathematical thinking or the use of mathematics per se as the source of these [specific] flaws [in macroeconomics and finance]. *What economics needs is not more or less mathematics and statistics, but mathematics and statistics better adapted to its problems and its limitations*. In the long run the discipline of economics will be shaped as much by its sociology and the philosophy of science and scientific interchange that commands its consensus as by particular methods or theoretical approaches."
>
> Foley, 2010, p. 16; italics added.

As always, Foley's thoughts on formalisation in economics are both perceptive and prescient. To understand the intrinsic problems and limitations of economics, without first transmogrifying them into a stunted mathematical formalism, is the art of good theorising. The great classical economists, some of the later neoclassicals, almost all the Austrians – even including Schumpeter among them – did not need any mathematical formalism at all to theorise imaginatively and derive momentous policy conclusions. Surely, this is true of Keynes, the immediate post-Wicksellian Swedes – I have in mind, in particular, Lindahl, Myrdal, Hammarskjöld and Lundberg – and the first generation Chicago economists, too. That a master mathematical economist expresses such an enlightened, almost visionary attitude to the place of mathematics in economics should be taken seriously by the modern economic theorist with his or her penchant for mathematising everything in sight indiscriminately.

I find this particular vision that Foley enunciates intellectually most congenial and fully coherent with my own research program in economics. I have argued for over twenty years that the kind of mathematics we should use should be dictated by, and adapted to, the 'problems and limitations' of the natural domain of economic data, the nature and constraints of economic decision making and the social and political constraints on them that make the search for determined and determinate solutions to constructively provable intractable problems a vain and chimerical pursuit.

However, like Foley, I do not associate such a vision with any less need for mathematics – but a need for a different kind of mathematics and, in my case, I have tried to make a sustained case for what may now be called *Diophantine Economics*. Mathematical formalisms of economic entities – data structures, institutions, behaviour, etc., – must respect the Diophantine nature of their domains of definition. This constraint, when respected, leads to the natural algorithmic indeterminacies of *Diophantine decision problems*[18]. Constructive and computable mathematics are a natural framework within which to frame Diophantine problems. The constructive and computable indeterminacies discussed in the previous section are elements in the research program on Diophantine Economics I am trying to develop (cf., Velupillai, 2011).

In this connection I may ask the following question. Lindahl's *Thesis* is a book of a little over 200 pages. It has exactly two diagrams (on p. 89 & on p. 159). The only mathematical symbolism in the book is confined to the contents of exactly one footnote (ppp. 90-920). How, then, can we assume that Lindahl's intense and deep economic theoretical reasoning, backed by impeccable doctrine-historical scholarship and wide empirical knowledge of the public finances and taxation, underpinned by ethical arguments and constitutional constraints imposed by one kind of democratic arrangement, can be formalised by one kind of mathematics? It is here that I think Foley's wise precept to seek out 'mathematics and statistics better adapted to [the problems of economics] and its limitations' becomes highly relevant.

The dichotomy between *proof* and *computation* that is characteristic of orthodox mathematical economics has led to the many unwarranted claims and propositions of applied general equilibrium theory – not to mention policies underpinned by the fundamental theorems of welfare economics. Mathematical economics of almost any variety is intrinsically non-algorithmic and, *a fortiori*, non-Diophantine. One unfortunate reason for this is the enforced dichotomy between proof and computation. Orthodox mathematical economics is seriously deficient in the epistemology of computation and the philosophy of mathematics, also partly due to this unnatural dichotomy.

In Diophantine Economics there is no such dichotomy.

Mathematical political science and social choice theory did not consider the Arrow Impossibility Theorem a shackle on mathematical theorising. There is no need for the economic theorist to consider the natural indeterminacies, undecidabilities and uncomputabilites of Diophantine decision problems any less

[18]In diophantine economics orthodox optimization paradigms are replaced by the more general framework of diophantine decision problems, where emphasis is placed on problem solving, finding methods to solve problems and classifying the algorithmic difficulties of such methods. It is squarely within what I have come to call *Classical Behavioural Economics*, that which was pioneered by Herbert Simon, underpinned by a model of computation (in Simon's case, even if mostly implicitly, it is the *Turing Model of Computation*). Some of the formal definitions of the above concepts can be found in Velupillai (2009A, 2010).

constraining on mathematical theorising in economics.

One of the most stunted use of the notion of a *social equilibrium* is the one by that doyen of mathematical economics, Debreu (1952). I have rarely seen a better, more enlightened, more imaginative use of the phrase social equilibrium than the one by the young Foley, in his extraordinary doctoral dissertation's concluding pages (Foley, 1967, pp.92-3; italics added):

> "The study of societies at the present time is a study of *social equilibrium*. This important concept can be defined only *vaguely*, as a state where the interaction of the millions of individuals in a society produces certain relatively constant aggregate features. ... Economic and political equilibria are special aspects of general social equilibrium, which includes the relative constancy of religion, law, family, and all the features of existence without which we could not recognize social life at all."

As economists struggle to encapsulate moral and ethical constraints in their formulation of behavioural and institutional hypotheses, mediated and humbled by the vicissitudes of history and its contours, the kind of social equilibrium Foley, with characteristic imagination, conceived almost half a century ago, becomes relevant almost with a vengeance.

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