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CONSTRUCTIVITY, COMPUTABILITY AND COMPUTERS IN ECONOMIC THEORY: SOME CAUTIONARY NOTES

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ABSTRACT

An overview of the varieties of mathematics that underpin the functioning of a computer is attempted in this paper. Against the backdrop of such an overview, the question is posed whether, and to what extent, the economist has respected these varieties of mathematics in posing economic questions and seeking answers via it, i.e. the computer. The answers are not particularly edifying for the economist.

1. A PREAMBLE OF SORTS

> Our approach is quantitative because economic life is largely concerned with quantities. We use computers because they are the best means that exist for answering the questions we ask. It is our responsibility to formulate the questions and get together the data which the *computer* needs to answer them. (Stone (1962, p. viii), italics in original)

Enormous developments in the theoretical and practical technology of the computer have made a tremendous impact on economics in general, but also in economic theory in particular. However, I do not think I will be misinterpreting the above observation if I point out that it refers to the *digital computer*. But, of course, there are also *analogue* and *hybrid* computers[1] that can

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[1] Not to mention *quantum*, *DNA* and other physical and natural computers that are beginning to be realized at the frontiers of theoretical technology.

be harnessed for service by the economist[2]—or any other analyst, in many other fields—to realize the intentions indicated by Richard Stone. Indeed, in many ways, the analogue computer should be more suitable for the purposes of the economic analyst simply because, at least as an economic theorist at the microeconomic level, one tends to theorize in terms of real numbers and the underpinning mathematics is, almost without exception, real analysis. The seemingly simple but, in fact, rather profound observation by Stone that I have quoted above captures one of a handful of insightful visions that the ubiquity of the computer has conferred upon the intellectual adventurer in economics. Stone appeals to the raw quantitative economic analyst to respect the language and architecture of the computer in pursuing precise numerical investigations in economics.

However, in general, we, as economic theorists, tend to 'formulate the questions' in the language of a mathematics that the *digital* computer does not understand—real analysis—but 'get together the data' that it does, because the natural form in which economic data appear or are constructed is in terms of integer, natural or rational numbers. The transition between these two domains remains a proverbial black box, the interior of which is occasionally viewed using the lenses of numerical analysis, recursion theory or constructive mathematics. With the possible exception of the core of economic theory,[3] i.e. general equilibrium theory, there has been no systematic attempt to develop any aspect of economics in such a way as to be consistent with the use of the computer, respecting its mathematical, numerical and hence also its epistemological constraints. To be sure, there have been serious applications of concepts of recursion theory and numerical analysis in various disparate, uncoordinated, attempts to many different areas of economics, most particularly game theory and choice theory. But these attempts have not modified the basic mathematical foundations on which the theory of games or choice theory or any other field involving recursion theory or numerical analysis has been applied. The basic mathematical foundations have always remained *real analysis* at suitable levels of sophistication.

[2] Charles Babbage, viewed in one of his many incarnations as an economist, can be considered the only one to have straddled both the digital and analogue traditions. There is a story to be told here, but this is not the forum for it. I shall reserve the story for another occasion.

[3] I should hasten to add another exception: the whole edifying, noble, research programme of Herbert Simon is also a notable exception. But he stands out and apart from the orthodox traditions of economics, where, by orthodoxy, I do not mean just neoclassical economics but all the other standard schools such as the new classicals, post Keynesians, new Keynesians, Austrians of all shades, institutionalists and so on; even the behavioural economists, once spanned and spawned by Herbert Simon, have been absorbed into the folds of orthodoxy.

Suppose, now, we teach our students the rudiments of the mathematics of the digital computer, i.e. recursion theory and constructive mathematics, simultaneously with the mathematics of general equilibrium theory, i.e. real analysis. As a first, tentative, bridge between these three different kinds of mathematics, let us also add a small dose of lectures and tutorials on computable analysis, at least as a first exposure of the students to those results in computable and constructive analysis that have bearings at least upon computable general equilibrium theory. Such a curriculum content will show that the claims and, in particular, the policy conclusions emanating from applicable general equilibrium theory are based on untenable mathematical foundations. This is true despite the systematic and impressive work of Herbert Scarf, Rolf Mantel and others who have sought to develop some constructive[4] and computable foundations in core areas of general equilibrium theory. In this sense, the claims of computable general equilibrium theorists are untenable.

What is to be done? Either we throw away any search for consistent mathematical foundations—the cardinal fulcrum on which general equilibrium theory has revolved for the whole of its development and justification—or we modify general equilibrium theory in such a way as to base it on a mathematics that is consistent with the applicability of the theory in a quantitative sense, or, third, we give up relying on the digital computer for quantitative implementations and return to the noble traditions of the analogue computer, which is naturally consistent with any theory based on real analysis. I find it surprising that this third alternative has not been attempted, at least not since Irving Fisher.

Complexity theory and the complexity vision in economics is quite another matter. On the one hand there are the formal theories of complexity: computational complexity theory, algorithmic information theory (or Kolmogorov complexity), stochastic complexity theory (or Rissanen's minimum description length principle), Diophantine complexity theory and so on. On the other, there is the so-called 'complexity vision' of economics—a vision

[4] I should mention that Douglas Bridges, a mathematician with impeccable constructive credentials, made a couple of valiant attempts to infuse a serious and rigorous dose of constructivism at the most fundamental level of mathematical economics. (Cf. (Bridges (1982, 1991), and two other references mentioned in the latter paper, neither of which have been available to me. They fell like water on a duck's back off the economic theorist's palate.) I should also mention that every single explicit proof in the text of Sraffa's classic book is constructive. I have long maintained that there is no need to recast his economics in the formalism of linear algebra to invoke theorems of non-negative square matrices and other theorems of a related sort to reprove his propositions. (cf. Sraffa (1960)). Indeed, I have maintained for years that one can, in fact, use Sraffa's economically motivated techniques to constructivize some of the nonconstructive methods of proof, for example, for the Perron–Frobenius theorems.

much promoted by the Santa Fe research programme and its adherents, of varying shades, in economics. Both of these approaches to complexity—as a theory in its own right and as a vision for an alternative economic analysis—have had applications in economics, even in systematic ways. Scarf's work in trying to tame the intractabilities of increasing returns to scale due to indivisibilities has led to pioneering efforts at studying the computational complexity inherent in such problems. Scattered applications of algorithmic information theory, as a basis for induction and as a vehicle through which undecidabilities can be generated even in standard game-theoretic contexts, have also had a place in economic theory.

These are all issues that can be subjects for individual research programmes and book-length manuscripts, if one is to survey the literature that has dotted the economic journals. I shall not attempt any such survey within the limited scope of cautionary reflections reported in this paper. My aim, instead, is to try to present an overview of the kinds of mathematics that a computer's functioning is based on and ask to what extent the economist has respected this mathematics in posing economic questions and seeking answers via it, i.e. the computer.

With these limited, circumscribed, aims in mind, the next section is devoted to an outline of the kinds of mathematics that underpin theories of computation by the digital computer. The parallel theory for the analogue computer is the subject matter of my main contribution to this volume (Velupillai (2004)). I do this by superimposing on the standard mathematics of general equilibrium theory some caveats and facts so as to make transparent the non-numerical underpinnings of orthodox theory. The concluding section tries to weave the various threads together into a simple fabric of speculations of a future for economics in the computational mode.

I was motivated to tell a story this way after trying to understand the successes and failures of non-standard analysis in economics. There was a clear, foundational and traditional economic theory framework which, if formalized and interpreted in terms of non-standard analysis, seemed to offer a rich harvest of implications and possibilities for the advancement of economic theory, even in quantitative terms. The latter observation is underpinned by the fact that the practice of non-standard analysis is eminently conducive to numerical exercises without *ad hoc* approximations or infelicitous conversions of the continuous to the discrete. That most ubiquitous of the economic theorist's assumption, *pricetaking behaviour*, cried out for a formalism in terms of the *infinitesimal*; and much else.

But elementary microeconomic textbooks or advanced macrodynamic textbooks were not rewritten in terms of the formalism of non-standard analysis. Endogenous business cycle theorists did not make any attempt to understand that traditional non-linear dynamical systems, long associated with well-characterized basins of attraction, were capable of richer geometric behaviour in the non-standard domain. Intellectual inertia, entrenched concepts, unfamiliar mathematics and, perhaps, even a lack of a concerted attempt by the pioneers of what I would like to call non-standard economics may all have contributed to the lack of success in supplanting traditional mathematical economics. It seems to me that the main source of the inertia is the lack of initiative to learn new and alternative mathematics and a monumental lack of knowledge about the mathematical foundations of the computer and computation. Hence, I thought, after also mulling over the fate of non-standard economics, if a story about computability, constructivity and complexity could be told from within the citadel of economic theory, there may well be a more receptive and less obdurate audience.

2. CAUTIONARY NOTES ON COMPUTABLE AND CONSTRUCTIVE ANALYSIS IN ECONOMIC THEORY

> 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.
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> *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. (Richman (1990))

Why should an *economist* bother about constructive and non-constructive proofs or whether a theorem in economics depends on the use of the law of the excluded middle, or whether the status of the Hahn–Banach theorem, which lies behind the mathematical validity of the second fundamental theorem of welfare economics, is dubious in computable analysis, or question the status of the Cauchy–Peano theorem in proving existence of solutions to ordinary differential equations, or whether it is really necessary to appeal to topological, non-constructive, fixed point theorems in economic theory contexts? My questions are not posed as a methodologist or as an epistemologist, although such questions, when answered, may well have methodological and epistemological implications. I ask these questions as a serious user of the computer in ordinary economic analysis: in simulating policy environments and obtaining usable parameter estimates; in testing conjectures by studying alternative scenarios of analytically intractable non-linear systems of equations; in taming the numerical monsters that can arise in increasing returns to scale technologies caused by indivisibilities that are naturally framed as combinatorial optimization problems; and in many other bread-and-butter issues in elementary applied economics that seeks economic theory and mathematical foundations for its numerical implementations.

Why does a mathematician express disquiet over the use or the invoking of the axiom of choice[5] in any mathematical exercise in theorem proving? Kuratowski and Mostowski, in their massive and encyclopaedic treatise on *Set Theory*, gave the standard reason for the general disquiet in mathematical circles over the use of this axiom in proof contexts:

> The axiom of choice occupies a rather special place among set theoretical axioms. Although it was subconsciously used very early, it was explicitly formulated as late as 1904 . . . and immediately aroused a controversy. Several mathematicians claimed that proofs involving the axiom of choice have a different status from proofs not involving it, because the axiom of choice is a unique set theoretical principle which states the existence of a set without giving a method of defining ('constructing') it, i.e. is not effective. (Kuratowski and Mostowski (1976, p. 54, footnote 1))

And they go on from that point, in their text of over 500 pages and hundreds of theorems and lemmas, by marking 'theorems which are proved using the

[5] Compare and contrast the following two views on the status of this axiom in mathematics. The first observation is by two distinguished constructive mathematicians and the second by competent mathematical economist:

> [The axiom of choice] is unique in its ability to trouble the conscience of the classical mathematician, but in fact it is not a real source of nonconstructivity in classical mathematics. . . . The axiom of choice is used to extract elements from equivalence classes where they should never have been put in the first place. (Bishop and Bridges (1985, p. 12))

and

> Although the validity of Zorn's lemma is not intuitively clear, it is demonstrably equivalent to an important *axiom of choice* that is *accepted today by most mathematicians*. (Suzumura (1983, p. 15), second set of italics added)

axiom of choice with the superscript ^{o}' (Kuratowski and Mostowski (1976, p. 54)). Another classic in the foundations of mathematics, the *Principia Mathematica* of Russell and Whitehead, observed the same kind of principle. Why, then, do economists not point out, say with an asterisk (*), those theorems in economic theory that depend on non-constructive reasonings (or appeals to the law of the excluded middle) and, perhaps, with a double asterisk (**) where reliance is placed on uncountable domains of definitions or non-recursive sets of various sorts such that computability or decidability fails—in particular, where claims are made as to the quantitative applicability and relevance of the implications of the theorems, derivable by utilizing the computer? For example, would it not serve the student of computable general equilibrium theory to be warned, with such an asterisk, whenever a particular proposition is derived with the help of undecidable disjunctions, with a cautionary note that the claimed construction is not theoretically sound or reliable? Or when momentous policy conclusions regarding decentralization and its virtues are drawn on the basis of the second fundamental theorem of welfare economics without a warning, with the double asterisk, that the status of such a theorem in computable analysis is dubious, to put it mildly, and its status in constructive analysis is subject to severe constraints on the domain of definition of the relevant variables—prices and quantities in the case of economics.

I do not have any sensible answers to any of these simple questions except that economists are rather cavalier in their use of mathematics and careless in their reliance on the computer. Hence, in these 'cautionary notes', I wish to try to initiate a tradition where we may serve the prudent and the thoughtful economic theorist, who is also acutely conscious that there is a mathematics underlying the activities of the ubiquitous computer, where the pitfalls may reside and how, in some cases, one may avoid them by adopting deft strategies. With this in mind I provide, in this section, some rather basic results from computable and constructive analysis that have bearing upon the use (or, rather, the misuse) of well-known classical theorems, with especial reference to computable general equilibrium theory. The reason for placing especial emphasis on computable general equilibrium theory is that it is the only field in the core of economic theory where there has been a systematic and conscious effort to incorporate both constructive and computable structures in a seriously applicable and positive way, and not only as intellectual curiosa with universal negative results such as undecidabilities, uncomputabilities and unsolvabilities. However, I touch upon implications to other areas in economics, as well.

I think it is too much to expect any single textbook in economics, whether advanced or intermediate, whether exclusively oriented towards mathematical economics or not, to follow the enlightened policies of a Russell–Whitehead or a Kuratowski–Mostowski and to mark with an asterisk or a double asterisk when constructively or computably dubious assumptions are made. Therefore, I shall list a sample of such fundamental assumptions and theorems that have bearings upon economics in its computation mode. By this I mean economics, whether micro or macro, where frequent recourse to computation, simulation and existence proofs are made. This means, for example, at the very *broadest* pedagogical, policy-oriented and textbook level, general equilibrium theory, growth and cycle theory, welfare economics and so on. I shall stick to these three fields in this essay, simply in view of space considerations.

Debreu, in his classic text that codified the modern version of general equilibrium theory, *Theory of Value*, stated in the Preface that 'the small amount of mathematics necessary for a full understanding of the text (but not all of the notes) [of the rest of the book] is given in the first chapter in a virtually self-contained fashion' (Debreu (1959, p. viii)). Let us suppose Debreu cautioned his readers that he was, in fact, choosing one possible mathematics from a world of many possible mathematics, without giving reasons as to why he chose the one he did rather than any other. However, suppose, in the interests of intellectual honesty, that he added caveats to some of the definitions, axioms and theorems, pointing out that they did not hold, were invalid, in other possible, equally valid mathematics—*particularly the mathematics underpinning the computer, computation and algorithms*. What kind of caveats might he have added if the aims had been to warn the reader who had computations in mind? I give a small, but fundamental, representative sample.

The four sections dealing with the fundamentals of limit processes in Debreu's classic are[6] on Real Numbers (§1.5), Limits in R^m (§1.6), Continuous Functions (§1.7) and Continuous Correspondences (§1.8). To these may be added §1.9, culminating in formal separating hyperplane theorems, and §1.10 on the two fundamental fixed point theorems of Brouwer and Kakutani. These exhaust, essentially, all the mathematical fundamentals on which is built the formal equilibrium economic *Theory of Value*. Thus, if I single out the axiom of completeness (§1.5.d, p. 10), definitions of compact, given on p. 15, §1.6.t, continuity, topologically characterized in §1.7, the maximum–minimum theorem (§1.7.h, (4′), and named after Weierstrass on p. 16 by Debreu) and the two fixed point theorems (Brouwer and Kakutani) of §1.10 (p. 26), in addition to the theorems of the separating hyperplanes given in §1.9 (esp. pp. 24–25), as being the fundamental mathematical tools

[6] All references are to Debreu (1959).

on which the whole edifice of general equilibrium theory stands, I do not think it will be considered a wild exaggeration. If these cannot be given numerical or computational content, how can they be useful in formalizing economic concepts and entities with a view to application?

Now, let us imagine that an economic fairy, rather patterned after tooth fairies, appends the following caveats to this chapter, with special reference to the singled-out concepts, axioms, results and theorems.

Proposition 1: The Heine–Borel theorem is *invalid* in *computable analysis*.

The standard, i.e. classical mathematical, statement of the Heine–Borel theorem is

A subset S of $\mathbb{R}^m$ *is compact if and only if it is closed and bounded (cf. Debreu (1959, p. 15, §1.6.t)).*

Proof: See Aberth (1980, §13.2, pp. 131–32).

Proposition 2: The Bolzano–Weierstrass theorem is *invalid* in *constructive analysis*.

A classical version of the Bolzano–Weierstrass theorem is

Every bounded sequence in $\mathbb{R}^m$ *has a convergent subsequence.*

Proof: See Dummett (1977, pp. 14–15).

Axiom 1—Completeness property (cf. Debreu (1959, §1.5.d, p. 10): Every non-empty subset X of $\mathbb{R}$ which has an upper bound has a least upper bound.

Theorem 1—Specker's theorem in computable analysis (1949, pp. 145–58): A sequence exists with an upper bound but without a least upper bound.

Proof: See Aberth (2001, pp. 97–98).

Note, also, the following three facts:

1. There are 'clear intuitive notions of *continuity* which cannot be [topologically] defined' (cf. Gandy (1995, p. 73)).
2. The Hahn–Banach theorem is not valid in constructive or computable analysis in the same form as in classical analysis (Metakides and Nerode (1982, esp. §5, pp. 328–32), (Bishop and Bridges (1985, p. 342)).

3. A consequence of the invalidity of the Bolzano–Weierstrass theorem in constructive analysis is that the fixed point theorems in their classical forms do not hold in (intuitionistically) constructive mathematics (Brouwer (1952, pp. 1–2)).

On the basis of just these, almost minimal, set of caveats and three facts, I can easily state and prove the following two propositions—one, I may call the *grand proposition*[7], and the other, the *non-constructivity proposition*. Keeping in mind the insightful and important observations made by Fred Richman with which I began this section, a 'proof' of the grand proposition will require that I take, in turn, every single one of the economic axioms, definitions, propositions and theorems in the *Theory of Value* and show where and how they rely on one or the other of the above caveats or facts. This, although a tedious task, is easy to do—even to automate. I leave it for an enthusiastic doctoral dissertation by any imaginative and adventurous student. A possible way to state this proposition in its full generality would be as follows

Proposition 3—The grand proposition: The Arrow–Debreu axiomatic analysis of economic equilibrium cannot be computationally implemented in a digital computer.

Proposition 4—The non-constructivity proposition: Neither of the fixed point theorems given and used by Debreu in the *Theory of Value* can be constructivized.

Proof: This is a simple consequence of the constructive invalidity of the Bolzano–Weierstrass theorem.

What, then, are we to make of grand, quantitative, policy prescriptions emanating from applications of the two fundamental theorems of welfare economics and, even more pertinently, of the results and use of computable general equilibrium theory? I suggest that a large barrel of salt be kept within reaching distance of anyone being subject to quantitative homilies, based on computations that rely on these two classes of models. Let me illustrate this cautionary attitude slightly more formally for computable general equilibrium theory.

[7] I hope the reader is able to infuse an appropriate dose of humour at this point lest the irony is mistaken for megalomania!

Walras, Pareto, Irving Fisher, Hayek, Lange, Marschak and others were concerned with the *computational implications* of the solutions of a general economic equilibrium model and each, in their own idiosyncratic and interesting ways, suggested methods to solve them. Walras, Pareto, Hayek and the Lange of the 1930s had, obviously, an interpretation of the market as an *analogue* computing machine. The *digital* computing machine, as we know it today, even though conceived by Leibniz and others centuries earlier, came into being only after the theoretical developments in recursion theory that resulted from the work of Gödel, Turing, Church, Kleene and Post. However, it is almost entirely Scarf's single-handed efforts that have made the issue of computing the solution of an existence problem in general equilibrium theory meaningful, within the digital computing metaphor,[8] in a numerical and economic sense. Scarf took the Arrow–Debreu version of the general competitive model and, imaginatively exploiting the implications of the equivalence between the Brouwer fixed point theorem and the Walrasian economic equilibrium theorem, proved by Uzawa in 1962, made the equilibrium 'approximately' constructive.

Scarf's methods and framework are based on mathematical structures and computing tools and concepts of a kind that are not part of the standard repertoire of an economics graduate student's education. Constructive mathematics, recursion theory and computational complexity theory are the main vehicles that are necessary to analyse computable general equilibrium models for their computability and computational efficiency properties. By providing enough of the essentials of computability theory and constructive mathematics to ask interesting and answerable questions about the computable general equilibrium model and its computable and constructive meaning, one enters the citadel with the Trojan horse of computable general equilibrium theory. The strategy of adding constructive and computable analytic caveats and facts to an otherwise orthodox textbook presentation of standard real analysis as a supplement or an appendix to a presentation of general equilibrium theory, the way I have suggested above, is another vantage point.

There are two aspects to the problem of the constructivity and effectivity of computable general equilibrium models:

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[8] The two traditions were demarcated with characteristic clarity by Scarf himself in his essay for the Irving Fisher birth centennial volume:

> In *Mathematical Investigations in the Theory of Value and Prices*, published in 1892, Irving Fisher described a mechanical and hydraulic **analogue device** intended to calculate equilibrium prices for a general competitive model. This chapter takes up the same problem and discusses an algorithm for a **digital computer** which approximates equilibrium prices to an arbitrary degree of accuracy. (Scarf (1967, p. 207), bold emphasis added)

- the role of the Uzawa equivalence theorem;
- the non-constructivity of the topological underpinnings of the limit processes invoked in the proof of the Brouwer fixed point theorem.

For over half a century, topological fixed point theorems of varying degrees of generality have been used in proving the existence of general economic equilibria, most often in varieties of Walrasian economies as, for example, is done in Debreu (1959). Uzawa's fundamental insight was to ask the important question whether the mathematical tools invoked in, say, the Brouwer or Kakutani fixed point theorems were unnecessarily general.[9] His important and interesting answer to this question was to show the reverse implication, and thus to show the mathematical equivalence between an economic equilibrium existence theorem and a topological fixed point theorem.

In other words, the Uzawa equivalence theorem demonstrates the mathematical equivalence of (for example) the following two propositions.[10]

Proposition 5—Brouwer fixed point theorem (Debreu, 1959, §1.10.b (1), p. 26): If S is a non-empty, compact, convex subset of $\mathbb{R}^m$, and if f is a continuous function from S to S, then f has a fixed point.

Proposition 6—Walras's (equilibrium) existence theorem (Uzawa's formulation 1962, pp. 59–60): Let there be n commodities, labelled $1, \ldots, n$, and let $p = (p_1, \ldots, p_n)$ and $x = (x_1, \ldots, x_n)$ be a price vector and a commodity bundle, respectively. Let P and X be the sets of all price vectors and of all commodity bundles:

$$P = \{p = (p_1, \ldots, p_n) \colon p_i \geq 0, i = 1, \ldots, n, \text{ but } p \neq 0\}$$

$$X = \{x = (x_1, \ldots, x_n)\} \text{(i.e. commodity bundles are arbitrary } n\text{-vectors).}$$

[9] As Debreu noted:

> [The theorem] that [establishes] the existence of a price vector yielding a negative or zero excess demand [is] a direct consequence of a deep mathematical result, the fixed-point theorem of Kakutani. And one must ask whether the . . . Proof [of the theorem] uses a needlessly powerful tool. This question was answered in the negative by Uzawa (1962) who showed that [the existence theorem] directly implies Kakutani's fixed-point theorem. (1982, p. 719)

[10] Paradoxically, even 40 years after Uzawa's seminal result and almost as many years of Scarf's sustained attempts to make general equilibrium theory amenable to computations, I know of only one textbook presentation of the Uzawa equivalence theorem and its ramifications. This is the pedagogically superb textbook by Ross Starr (1997, esp. ch. 11).

Let the excess demand function $x(p) = [x_i(p), \ldots, x_n(p)]$ map P into X and satisfy the following conditions.

(A) $x(p)$ is a continuous mapping from P into X.
(B) $x(p)$ is homogeneous of order 0, i.e. $x(tp) = x(p)$ for all $t > 0$ and $p \in P$.
(C) Walras's law holds:

$$\sum_{i=1}^{n} p_i x_i(p) = 0 \qquad \text{for all } p \in P$$

Then there exists at least an equilibrium price vector $\bar{p}$ for $x(p)$, where a price vector $\bar{p}$ is called an equilibrium if

$$x_i(\bar{p}) \leq 0 \qquad (i = 1, \ldots, n)$$

with equality unless $\bar{p}_i = 0$ $(i = 1, \ldots, n)$.[11]

Now, in Uzawa's equivalence proof there is a 'construction' of a class of excess demand functions satisfying conditions (A), (B) and (C) above. A particularly clear and pedagogically illuminative discussion of this construction is given in Starr (1997, pp. 137–38). This construction does not place either computable or constructive constraints on the class of functions from which they are built. This, compounded by the constructive invalidity of the Bolzano–Weierstrass theorem, makes Uzawa's equivalence theorem neither constructively nor computably implementable in a digital computer. Hence any claim that computable general equilibrium theory is either computable or constructive is vacuous.

What then, are we, to make of the following assertion:

> The major result of postwar mathematical general equilibrium theory has been to demonstrate the existence of such a [Walrasian] equilibrium by showing the applicability of mathematical fixed point theorems to economic models.
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> . . .
>
> The weakness of such applications is [that] they provide non-constructive rather than constructive proofs of the existence of equilibrium; that is, they show that equilibria exist but do not provide techniques by which equilibria can actually be determined. . . . The extension of the Brouwer and Kakutani fixed point theorems in [the] direction [of making them constructive] is what underlies the work of Scarf on fixed point algorithms . . . (Shoven and Whalley (1992, pp. 12, 21))

[11] There is a minor misprint in the original Uzawa statement at this point, where $\bar{p}_1$ is written instead of $\bar{p}_i$ (Uzawa (1962, p. 60)).

This is where that large barrel of salt to dip into will be quite useful—plus, of course, knowledge of the caveats and facts given above. For further discussion, details and implications of the non-constructivity and uncomputability inherent in the Uzawa equivalence theorem and Scarf's work on fixed point algorithms I refer the reader to Velupillai (2002).

An even more dangerous message is the one given in the otherwise admirable above-mentioned text by Ross Starr (1997, p. 138):

> What are we to make of the Uzawa Equivalence Theorem? It says that the use of the Brouwer Fixed-Point Theorem is not merely one way to prove the existence of equilibrium. In a fundamental sense, it is the only way. Any alternative proof of existence will include, *inter alia*, an implicit proof of the Brouwer Theorem. Hence this mathematical method is essential; one cannot pursue this branch of economics without the Brouwer Theorem. If Walras failed to provide an adequate proof of existence of equilibrium himself, it was in part because the necessary mathematics was not yet available.

Obviously, Ross Starr and others advocating a one-dimensional mathematization of equilibrium economic theory have paid little attention to even the more obviously enlightened visions of someone like Smale, who has thought hard and deep about computation over the reals (cf. for example (Blum *et al.* (1998)):

> We return to the subject of equilibrium theory. The existence theory of the static approach is deeply rooted to the use of the mathematics of fixed point theory. Thus one step in the liberation from the static point of view would be to use a mathematics of a different kind. . . . Also the economic equilibrium problem presents itself most directly and with the most tradition *not as a fixed point problem*, but as an equation, supply equals demand. *Mathematical economists have translated the problem of solving this equation into a fixed point problem*.
>
> I think it is fair to say that for the main existence problems in the theory of economic equilibria, one can now bypass the fixed point approach and attack the equations directly to give existence of solutions, with a simpler kind of mathematics and even mathematics with dynamic and algorithmic overtones. (Smale (1976, p. 290), italics added)

So, what are we to do? I do not think the situation as far as computational underpinnings for economic theory, even for equilibrium economic theory, is as desperate as it may seem from the above discussions and observations. After all, that great pioneering equilibrium theorist, Irving Fisher, untrammelled by the powers of the digital computer and its mathematics, resorted in a natural way to analogue computing. However, if we, in an age dominated by the digital computer, want to preserve the edifice of orthodox equilibrium theory, and also want to make it computable or to constructivize it, then there are several alternative routes to take. The obvious way, the Irving Fisher way, is of no use since our starting point is the dominance of the digital computer. The alternative ways are to harness one of the theories of real computation, say the kind being developed by Smale and his several collaborators, or the approach of Pour-El and Richards and so on. My own suggestion is the following.

In addition to the above caveats and facts, suppose I also introduce the mathematically minded student to the following *constructive* recursion theoretic fixed point theorem (cf. for example Moret (1998, pp. 158–59)).

Theorem 2: There is a total recursive function f such that, $\forall x$, *if* ϕ_x *is total, then we have* $\phi_{h(x)} = \phi_{\phi_x(h(x))}$.

In other words, given any total function $f = \phi_x$ the fixed point x of f can be computed through the use of the total recursive function h.

If I then work backwards and rebuild the economic theory formalizations making sure, at each step, that the constituent functions, definitions and elements are all consistent with the applicability of the recursion theoretic fixed point theorem, then, of course, the theorem can be applied to prove the existence of an economic equilibrium which will be as much Walrasian as the Arrow–Debreu formalization—indeed, more so. After all, Walras's times were long before compactness, the Brouwer fixed point theorem, the completeness axiom, the separating hyperplane theorems and so on. Walras and Pareto were writing at a time when Picard's *iteration* and rudimentary *contraction mappings* were the metaphors on the basis of which to understand iteration and equilibrium *processes*. Nothing more complicated than variations on these concepts are involved in the above recursion theoretic fixed point theorem—not even the completeness axiom, let alone compactness, continuity and all the other paraphernalia of the limit processes underpinning real analysis.

It may well be true that 'Walras failed to provide an adequate proof of existence of equilibrium himself'. It may even be true that 'it was in part because the necessary mathematics was not yet available'. But it certainly does not imply, as noted by Smale in a different way, that 'the Brouwer Fixed-Point Theorem . . . is the only way . . . to prove the existence of equilibrium'. Nor does it mean that 'any alternative proof of existence will include . . . an implicit proof of Brouwer's Theorem'. This kind of dangerous claim, even while utilizing the Uzawa equivalence theorem to build the vast edifice of a computable general equilibrium structure that is computably and constructively untenable, compounds ignorance of the mathematics of computation, particularly via the digital computer, and, even more importantly, the world of mathematics itself.

3. WHITHER ECONOMIC THEORY IN THE COMPUTATIONAL MODE?

> The term 'computing methods' is, of course, to be interpreted broadly as the mathematical specification of algorithms for arriving at a solution (optimal or descriptive), rather than in terms of precise programming for specific machines. Nevertheless, we want to stress that solutions which are not effectively computable are not properly solutions at all. Existence theorems and equations which must be satisfied by optimal solutions are useful tools toward arriving at effective solutions, but the two must not be confused. Even iterative methods which lead in principle to a solution cannot be regarded as acceptable if they involve computations beyond the possibilities of present-day computing machines. (Arrow *et al*. (1958, p. 17))[12]

If we are to take these thoughts seriously, and remember Stone's admonishment, then I suggest that we reorient our activities as economic theorists in the computational mode with the following strategies in mind.[13]

- The triple {*assumption*, *proof*, *conclusion*} should be understood in terms of {*input data*, *algorithm*, *output data*}.
- Mathematics is best regarded as a very high level programming language.
- In constructive, computable and (constructive) non-standard analysis, every proof is an algorithm.
- To understand a *theorem* (in any kind of mathematics) in algorithmic terms, represent the assumptions as input data and the conclusions as output data. Then try to convert the proof into an algorithm which will take in the input and produce the desired output. If you are unable to do this, it is probably because the proof relies essentially on the law of the excluded middle. This step will identify any inadvertent infusion of non-constructive reasoning.
- If we take algorithms and data structures to be fundamental, then it is natural to define and understand functions in these terms. If a function does not correspond to an algorithm, what can it be? Hence, take the stand that functions are, by definition, computable or constructive.

[12] I am indebted to my friend and colleague Enrico Zaninotto for bringing this paper and, in particular, this important, early, observation by Arrow *et al*. to my attention.
[13] I have been influenced to formulate a strategy in this way by a reading of Greanleaf (1991).

- Given a putative function f, we do not ask 'Is it computable?' or 'Is it constructive?' but rather 'What are the data types of the domain and of the range?' This question will often have more than one natural answer, and we will need to consider both restricted and expanded domain–range pairs. Distinguishing between these pairs will require that we reject the excluded middle for undecidable propositions. If you attempt to pair an expanded domain for f with a restricted range, you will come to the conclusion that f is non-computable or non-constructive.

None of these steps is part of the standard repertoire of textbooks, research methodologies or traditions of economic education and practice. No one can contemplate a seriously mathematical, empirical or experimental study of economics without following the above strategy, or some variant of it, if computations by a digital computer are to be a vehicle for modelling, testing and inference.

This strategy, I think, will lead to a formulation of the existence problem as a *decision problem* in the recursion theoretic sense. In particular, we will be more precise in choosing the domain of definition of economic variables and not blindly and arbitrarily let them be the real numbers. I envisage a day when the 'economic problem' will become a decision problem for the solution of Diophantine equations. What kind of theorems could we hope to devise if we follow this strategy? I put it this way because, of course, I expect us, as mathematical economists, to have left *Cantor's Paradise*—not driven away—voluntarily; I expect, also, that we will have abandoned Plato in his caves, contemplating ideal existence, inferred from shadows of the mind. Theorems will be invented, not discovered. When theorems have to come with a recipe for construction, we will also wonder about the costs of construction, costs in the widest economic sense. Thus, economists will have to come to terms with the theory of *computational complexity* and try to go beyond worst-case analysis.

If we take too seriously statements like the following, where the implicit assumption is that 'theorems' are the exclusive prerogative of the Formalists, the Bourbakians and varieties of Platonists, then we may be entrapped in the belief that mathematical economics can only be practised in one, unique, way:

> My guess is that the *age of theorems may be passing* and that of simulation is approaching. Of course there will always be logical matters to sort out, and our present expertise will not be totally obsolete. But the task we set ourselves after the last war, to deduce all that was required from a number of axioms, has almost been completed, and while not worthless has only made a small contribution to our understanding. (Hahn (1994, p. 258), italics added)

The age of theorems of a particular kind of mathematics may be passing; after all nothing has been heard from the Bourbakian stables for quite a number of years (cf. Cartier (1998)). But *not* new kinds of theorems, those based on numerically meaningful assumptions and with computationally rich implications. One of the great merits of recursion theory and constructive mathematics is the explicit awareness and recognition that mathematical concepts are, often, formalizations of intuitive concepts; that in the formalization, which is always *pro tempore*, blurred borderlines are the order of the day. The traditional mathematical economist does not bat an eyelid when accepting the ε–δ definition of continuity even in economic contexts without wondering whether it is appropriate. The recursion theorist or the constructive mathematician is not that cavalier. Two of the great achievements of twentieth-century mathematics were the mathematical formalization of the intuitive concepts of effective calculability and randomness. The recursion theorist is happy to work with theses that capture, again *pro tempore*, the intuitive contents of such terms, honed by centuries of experience. The real analyst is less disciplined. What would the Bourbakian mathematical economist say if I assert that 'topological space' does not necessarily capture the full intuitive content of continuity? What would the computable economist say if I said that the Church–Turing thesis does not capture the intuitive content of effective calculability? Or how would the applied recursion theorist react to a counterexample to the thesis on randomness put forward by Kolmogorov–Chaitin–Solomonoff–Martin-Löf? I am sure the latter two—the computable economist and the applied recursion theorist—will happily adjust their mathematical concepts rather than force the intuitive notions to conform to the formal ones. I am not sure about the Bourbakian or Formalist mathematical economist, who must have known for decades that, for example, the topological definition of continuity is intuitively inadequate, or that the Heine–Borel theorem is invalid in computable analysis, or that the Bolzano–Weierstrass theorem is false in constructive mathematics, but continues (sic!) to plague economic formalism with definitions that rely on them, as if they were indisputable truths. How can such a formalism adapt to the age of the digital computer?

The main message of this paper is very similar in spirit to the epistemological and methodological points made by Tommaso Toffoli many years ago (cf. Toffoli (1984)). Indeed, his paper, substituting 'mathematical economics' for 'mathematical physics', makes my case quite clearly (pp. 117–18; italics in the original):

> Mathematical physics, both classical and quantum-mechanical is pervaded by the notion of a 'continuum', that is, the set $\mathbb{R}$ of real numbers with its natural order-

> ing and topology. . . . How do we manage to specify some constructive way the behavior of a system beset by so many uncountable infinities? . . . [In] modeling physics with the traditional approach, we start for historical reasons . . . with mathematical machinery that probably has much more than we need, and we have to spend much effort disabling or reinterpreting these 'advanced features' so that we can get our job done in spite of them. On the other hand, . . . , we outline an approach where the theoretical mathematical apparatus in which we 'write' our models is essentially isomorphic with the concrete computational apparatus in which we 'run' them.

The difference is that I am not as wedded to the power of cellular automata as fruitful repositories of economic metaphors as Toffoli is for them as vehicles for modelling physics. This is partly because, paradoxically, I believe economic statics remains an important field in its own right, and therefore getting our formal language more appropriate for both dynamics and statics entails getting our mathematical theorizing less *ad hoc*.

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