The Impossibility of an *Effective* Theory of Policy in a Complex Economy

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Abstract

It is shown that for a `complex economy', characterised in terms of a formal dynamical system capable of computation universality, it is impossible to devise an *effective* theory of policy.

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1 Preliminaries on *Complexity* and *Policy*[1]

There is one main theme and correspondingly, one formal result in this paper. On the basis of a general characterization of what is formally meant by a 'complex economy', underpinned by imaginative suggestions to this end in Foley ([6]) and in Brock and Colander ([3], henceforth BC), it will be shown that an *effective*[2] theory of economic policy is impossible for such an economy. There is, in addition, also a half-baked conjecture; it will be suggested, seemingly paradoxically, that a 'complex economy' can be formally based on the foundations of orthodox general equilibrium theory and, hence, a similar 'impossibility' result is valid in this case, too.

I have found Duncan Foley's excellent characterisation of the *objects* of study by the 'sciences of complexity' in [6], p.2, extremely helpful in providing a base from which to approach the study of a subject that is technically demanding, conceptually multi-faceted and philosophically and epistemologically highly inhomogeneous:

> "Complexity theory represents an ambitious effort to analyze the functioning of highly organized but *decentralized systems* composed of very large numbers of individual components. The basic processes of life, involving the chemical interactions of thousands of proteins, the living cell, which localizes and organizes these processes, the human brain in which thousands of cells interact to maintain consciousness, ecological systems arising from the interaction of thousands of species, the processes of biological evolution from which new species emerges, and *the capitalist economy*, which *arises from the interaction of millions of human individuals, each of them already a complex entity*[3], are leading examples."
>
> [6], p.2; italics added.

These *objects* 'share':

> "[A] potential to configure their component parts in an astronomically large number of ways (they are *complex*), constant change in response to environmental stimulus and their own development

[1] I am immensely indebted to Professor Stefano Zambelli of Aalborg University for detailed, valuable, comments and criticisms on an earlier draft of this paper. Despite his sterling efforts, it is inevitable that there are remaining infelicities and inaccuracies, for which I am solely responsible.

[2] I mean by effective the formal sense of the word in (classical) recursion theory.

[3] Note Foley's interesting characterisation of each human individual in the interaction of millions in a decentralized system as a *complex entity*. This is diametrically opposed to the formalism in agent-based models of simple agents interacting to generate self-organized patterns of behaviour. In these latter class of models the formalism chosen for the individual agent abstracts away from all its realistic complex characteristics. A *Turing Machine* formalism is the best way, in a precisely definable sense and consistent with formal economic theory, to encapsulate the full complexities of an individual agent.

(they are *adaptive*), a strong tendency to achieve recognizable, stable patterns in their configuration (they are *self-organizing*), and an avoidance of stable, **self-reproducing** states (they are *non-equilibrium systems*). The task complexity science sets itself is the **exploration of the general properties of complex, adaptive, self-organizing, non-equilibrium systems**.

The *methods* of complex systems theory are highly empirical and inductive. ... A characteristic of these ... complex systems is that their components and rules of interactions are *non-linear* ... .The computer plays a critical role in this research, because it becomes impossible to say much directly about the **dynamics of non-linear systems** with a large number of degrees of freedom using **classical mathematical analytical methods**."

(ibid, p.2; bold emphasis added.)

In a similar vein, Fontana and Buss ([7]) have suggested that 'complexity' in a dynamical system arises as a result of the interactions between 'high dimensionality' and 'non-linearity':

"...[A] fundamental problem in methodology [is that] the traditional theory of 'dynamical systems' is not equipped for dealing with constructive processes[4]. ...We seek to solve this impasse by connecting dynamical systems with fundamental research in computer science ....*Many failures* in domains of biological (e.g., development), cognitive (e.g., organization of experience), social (e.g., institutions), and economic science (*e.g., markets*) *are nearly universally attributed to some combination of high dimensionality and nonlinearity. Either alone won't necessarily kill you, but just a little of both is more than enough. This, then, is vaguely referred to as 'complexity'."*

[7],pp.56-7; italics added.

The suggestions for a formalism of a 'complex economy' in BC are more specifically framed in the context of a discussion of the role of policy in such an economy. I hope the following concise summary is reasonably faithful to the spirit of their approach and encapsulates the more imaginative ideas explicitly. In section 2 of [3], titled '*How Complexity Changes Economists' Worldview*' the authors list six ways in which a 'complexity vision' brings about (or should bring about) a change in the worldview that is currently dominant in *policy circles*. The latter is given the imaginative representative name '*economic reporter*', a person trained in one of the better and conventional graduate schools of economics and fully equipped with the tinted glasses that such an education provides: a worldview for policy that is underpinned by *general equilibrium*

[4] I do not believe this refers to 'constructive' in the strict sense of 'constructive mathematics' of any variety.

theory (henceforth GET) and game theory. These are the bright young things that go around the world seeing Nash equilibria and the two welfare theorems in the processes that economies generate, without the slightest clue as to how one can interpret actions, events and institutional pathologies during processes and their frequent paralysis.

Brock and Colander, as 'complexity theorists', aim to take away the intellectual props that these economic reporters carry as their fall-back position *for policy recommendations*: *general equilibrium theory* and the baggage that comes with it (although, curiously, they do not mention taking away that other prop: game theory, which, in my opinion, is far more sinister). With this aim in mind the six changes that a 'complexity vision' may bring about in the worldview of the *economic reporter*, according to BC, are as follows:

- With a complexity vision the most important policy input is in *institutional design*.
- The complexity vision brings with it an attitude of *theoretical neutrality* to 'abstract debates about *policy*'.
- The '*complexity-trained policy economist*' *will try to seek out the boundaries of the equivalent of the basins of attractions of dynamical systems* - i.e., equipped with notions of criticality and their crucial role in providing adaptive flexibilities in the face of external disturbances, the complexity trained economist will not be complacent that any observable dynamics is that of an elementary, characterizable, attractor.
- There will be more focus on inductive process models than on abstract deductive models.
- Due to the paramount roles played by positive feedback underpinning path dependence and increasing returns in the complexity visioned economist, the attitude to policy will be honed towards a temporal dimension to it being given crucial roles to play.
- *The complexity worldview makes policy recommendations less certain* simply because pattern detection is a hazardous activity and patterns are ephemeral, eternally transient phenomena.

On the whole, then, I base my interpretation of a formal dynamical system representing a 'complex economy' to be underpinned by high dimensionality, nonlinearity and configured on the boundaries of basins of attractions. However, I wish to add three caveats to these highly suggestive and entirely plausible elements for a characterization of a dynamical system encapsulating the essential features of a 'complex economy'.

The first is the joint suggestion in Foley and in Fontana-Buss and in much of the standard literature of 'the sciences of complexity' that *high dimensionality* and *nonlinearity* are *necessary* ingredients for the manifestation of 'complex behaviour' by a formal dynamical system. I think it is quite easy to show 'complex behaviour' in a low dimensional, *asynchronously coupled*, dynamical system, in any sense defined as desirable by the 'sciences of complexity'.

Secondly, it is easy to show, formally, that a dynamical system must possess the computability based property of *self-reproduction* for it to be capable of *self-organization*.

Thirdly, there is a difficulty in reconciling the three desiderata - bordering on formal impossibility - of: *institutional design*, *self-organization* and the idea of a dynamical system delicately poised on the boundaries of its basins of attraction.

Finally, implicit - and occasionally also explicitly - in all of the suggested criteria for defining a dynamical system underpinning a 'complex economy' by the authors cited above, there is the forceful call for a shift away from the formal methods of mathematics and philosophy of science customarily used in economic theory and a move is advocated, by necessity if you like, towards the study of the high dimensional, nonlinear, dynamical systems using the methods of simulation and numerical studies based on the (digital) computer. I am in complete agreement with this 'call', although not just 'high dimensionality and nonlinearity' call forth this shift towards reliance on the (digital) computer, induction/abduction and so on. Now, a serious acceptance of these admonitions requires that the formalisms of dynamical systems must also be underpinned by the mathematics of the computer - recursion theory. I am not aware of any systematic study by the theorists, advocates and practitioners of a 'complexity vision' of basing their formalisms on recursion theory. Thus, there is an uneasy and easily demonstrable dissonance between the various above mentioned desiderata and the actual mathematical formalisms used. For example, it is one thing to state that a dynamical system representing a 'complex economy' should be configured at the boundary of the basin of attraction of attractors; it is quite another thing to make such a criterion numerically meaningful so that it can be investigated on the (digital) computer. Of course, if one is working with analogue computers, the 'complexity vision' can be kept consistent with classical mathematical analytical methods, up to a point.

The dissonance is, however, only apparent and not real. This is because the fact is that the *new* mathematical formalisms for the *new* sciences of complexity came, in fact, from two very special directions, almost simultaneously: developments in the theory and *numerical* study of non-linear dynamical systems and new paradigms for representing, in a *computational* format, ideas about *self-reproduction*, *self-reconstruction* and *self-organization* in (not necessarily) high-dimensional systems. The interpretations of the latter in terms of the theory of the former and the representations of the former in terms of the paradigms of the latter was the serendipitous outcome that gave the sciences of complexity its most vigorous and sustained mathematical framework. The classic contributions to this story are the following: [19], [21], [10], [11], [15] and [17], from which emerged a vast, exciting and interesting work that has, in my opinion, led to the 'complexity vision' and the 'sciences of complexity'. These contributions are classics and, like all classics, are still eminently readable - they have neither aged nor have the questions they posed become obsolete by the development of new theoretical technologies; if anything, the new theoretical technologies have reinforced the visions they foresaw and the scientific traditions and trends they initiated.

Let me end these preliminaries with some remarks on 'policy' in a 'complex economy'. Brock and Colander make the entirely reasonable observation that (op.cit, p.79; italics added):

> "Much of deductive standard economic theory has been directed at providing a general theory based on first principles. Complexity theory suggests that question may have no answer and, at this point, the focus of abstract deductive theory should probably be on the smaller questions - *accepting that the economy is in a particular position, and talking about how policy might influence the movement from that position. That makes a difference for policy in what one takes as given* - it suggests that existing institutions should be incorporated in the models, and that *policy suggestions should be made in reference to models incorporating such institutions ...* ."

Was this not, after all, the message in the *General Theory*? 'Accepting that the economy is in a particular position' of unemployment equilibrium and *devising a theory for policy* that would 'influence movement from that position' to a more desirable position. Such a vision implied, in the *General Theory*, an economy with multiple equilibria and, at the hands of a distinguished array of nonlinear Keynesians, also that other hobby horse of the 'complexity visionaries': 'positive feedback' - in more conventional dynamic terms, locally unstable equilibria in a dynamical system subject to relaxation oscillations. In addition, in the early nonlinear Keynesian literature, when disaggregated macrodynamics was investigated, there were coupled markets, but the mathematics required to analyze coupled nonlinear differential equations was only in its infancy and these nonlinear Keynesians resorted to *ad hoc* numerical and geometric methods.

So, we are in familiar territory, but not the terrain that is usually covered in the education of the economic reporters. To this extent Brock and Colander are right on the mark about the policy-complexity nexus.

Were these precepts also not the credo of the pioneers of classical behavioural economics: Herbert Simon, Richard Day, James March and Co? Studying 'adjustment processes' phenomenologically, eschewing the search for first principles, underpinning economic theoretic closures with institutional assumptions, enriching rationality postulates by setting agents in explicit institutional and problem-solving contexts, seeking algorithmic foundations for behaviour[5], and so on. Was there ever an economic agent abstracted away from an *institutional setting* in any of Simon's writings? Were not all of Day's agents, in his dynamic economics, behaving *adaptively*?

[5] Which, automatically, brings with it undecidabilities, uncomputabilities and other indeterminate problems that can only, always, be 'solved' pro tempore, aiming to determine the boundaries of basins of attraction numerically, and so on. A list that not only encompasses the Brock-Colander set of six-fold precepts but also one that is far richer in inductive content and retroductive - i.e., abductive - realization.

So, these precepts have a noble pedigree in classical behavioural economics and in the 'economics of Keynes' and it is not as if those of us who were trained in these two traditions were not aware of the complexity-policy nexus

For over a quarter of a century I have taught macroeconomics emphasizing the 'paradox of saving', referring to the 'Banana Paradox' in the *Treatise*, the story of the origins of the classical theory of economic policy[6] and the emergence of the Lucasian critique as an outcome of an awareness of the paradoxes of self-referentiality in a dynamical system capable of computation universality. Coming to terms with the wedge that has to be designed between 'parts' and 'wholes' that emerges in a macroeconomy subject to the 'paradox of saving', and related paradoxes, and respecting the conundrums of self-referentiality in underpinning it - the macroeconomy - in a system of rational individual behaviour, are the twin horns on which the 'complexity-policy' nexus has often floundered. As a result the unfortunate trajectory of the theory of policy has oscillated between adherence to a mechanical view of the feasibility of policy and a nihilistic attitude advocating the irrelevancy of policy.

The *via media* that is being wisely suggested in Brock and Colander - except for their advocacy of institution design for a complex economy - is entirely justifiable for a complex economy.

2 Undecdidability of Policy in a 'Complex Economy'

I am not sure what Brock-Colander mean by 'an elementary, characterizable, attractor' simply because the formalism for '*characterizability*' is not precisely defined. I take it, however, that they mean by the phrase 'elementary characterizable attractor' the standard limit points, limit cycles and 'strange' (i.e.,

[6] By the 'classical theory of economic policy' I am referring, now, to the so-called 'target-instrument' approach that is usually attributed to Ragnar Frisch, Jan Tinbergen and Bent Hansen and not the theory of policy of the classical economists. It may be useful to record the origins of this approach (as narrated to me by Mrs Gertrud Lindahl, during personal conversations at her home in Lund, in 1983). In the early 1930s, the Social Democratic Minister of Finance of a Sweden grappling, like most other economies, with the ravages wrought in the labour market by the 'great depression', was Ernst Wigforss. He approached the two leading Swedish economists, Gunnar Myrdal and Erik Lindahl, both sympathtic to the political philosophy of the Social Democrats, and requested them to provide him with a 'theory for the underbalancing of the budget' so that he can justify the policy measures he was planning to implement to combat unemployment due to insufficent effective demand. He needed a 'theory', he told them, because the leader of the oppositon in that Parliament was the Professor of Economics at Stockholm University, Gösta Bagge, who was versed only in a theory that would justify a balanced budget. Thus was born, via the framework devised in Myrdal's famous memorandum to Wigforss ([12]), the classical theory of economic policy, made mathematically formal, first, by Frisch, Tinbergen and Hnsen and famously known as the 'target-instrument' approach. It was built on the essential back of the 'paradox of saving', the main macroeconomic repository of the wedge between 'wholes' and 'parts' that makes a mockery of reductionism. The pioneers of macroeconomics, Keynes, Myrdal, Lindahl, Lundberg and others, were theorists of the complexity-policy nexus before their time - rather like Molière's famous unconscious purveyor of prose, Monsieur Jourdain.

'chaotic') attractors. As for 'characterizable' I shall assume 'effective characterization of defining basins of attraction'. Then, given the observable trajectories of a dynamical system, say computed using simple Poincarè maps or the like, an 'elementary characterizable attractor' is one that can be associated with a Finite Automaton[7]. Thus, limit points, limit cycles and strange attractors are effectively characterizable in a computably trivial sense; however, dynamical systems capable of computation universality have to be associated with Turing Machines. Hence, trajectories that are generated by dynamical systems poised on the boundaries of the basins of attractions of simple attractors may possess undecidable properties due to the ubiquity of the *Halting problem for Turing Machines*, the emergence of *Busy Beavers* (i.e., uncomputabilities), etc. Any theory of policy, i.e., any rule - fixed or discretionary - that is a function of the values of the dynamics of an economy formalized as a dynamical system capable of computation universality, will share these exotic properties.

I shall assume an abstract model of a 'complex economy', or of an 'economy capable of complex behaviour', to be a dynamical system capable of *computation universality*. No other dynamical system would satisfy the imaginative characterizations suggested explicitly, and by implication, by Foley and Brock-Colander, above. I shall assume, moreover, familiarity with the formal definition of a dynamical system (cf. for example, the obvious and accessible classic, [8] or the more modern, [2]), the necessary associated concepts from dynamical systems theory and all the necessary notions from classical computability theory (for which the reader can, with profit and enjoyment, go to a classic like [14] or, at the frontiers, to [4]). Just for ease of reference the bare bones of relevant definitions for dynamical systems are given below in the usual telegraphic form[8]. An intuitive understanding of the definition of a 'basin of attraction' is probably sufficient for a complete comprehension of the main result - provided there is reasonable familiarity with the definition and properties of Turing Machines (or partial recursive functions or equivalent formalisms encapsulated by Church's Thesis).

Definition 1 *The Initial Value Problem (**IVP**) for an Ordinary Differential Equation (**ODE**) and **Flows**. Consider a differential equation:*

$$\dot{x} = f(x) \tag{1}$$

where x is an unknown function of $t \in I$ (say, t : time and I an open interval of the real line) and f is a given function of x. Then, a function x is a solution of (1) on the open interval I if:

$$\dot{x}(t) = f(x(t)), \forall t \in I \tag{2}$$

[7] The analogy here is like that between the Chomskey hierarchy of formal languages and abstract computing machines. Foley's discussion of this link in [5], particularly §1.4.3, pp. 44-6, is particularly illuminating. Wolfram, in [22], developed these ideas in the direction that I am trying to exploit here.

[8] In the definition of a dynamical system given below I am not striving to present the most general version. The basic aim is to lead to an intuitive understanding of the definition of a basin of attraction so that the main theorem is made reasonably transparent. Moreover, the definiton given below is for scalar ODEs, easily generalizable to the vector case.

The initial value problem (ivp) for (1) is, then, stated as:

$$\dot{x} = f(x), \qquad x(t_0) = x_0 \tag{3}$$

and a solution $x(t)$ for (3) is referred to as a solution through x_0 at t_0. Denote $x(t)$ and x_0, respectively, as:

$$\varphi(t, x_0) \equiv x(t), \text{ and } \varphi(0, x_0) \equiv x_0 \tag{4}$$

*where $\varphi(t, x_0)$ is called the **flow** of $\dot{x} = f(x)$.*

Definition 2 ***Dynamical System***

*If f is a C^1 function (i.e., the set of all differentiable functions with continuous first derivatives), then the **flow** $\varphi(t, x_0), \forall t$, induces a **map** of $U \sqsubset \mathbb{R}$ into itself, called a C^1 **dynamical system on** $\mathbb{R}$:*

$$x_0 \longmapsto \varphi(t, x_0) \tag{5}$$

if it satisfies the following (one-parameter group) properties:

1. $\varphi(0, x_0) = x_0$
2. $\varphi(t + s, x_0) = \varphi(t, \varphi(s, x_0)), \forall t$ & s, *whenever both the l.h and r.h side maps are defined;*
3. $\forall t, \varphi(t, x_0)$ *is a C^1 map with a C^1 inverse given by:* $\varphi(-t, x_0)$;

Remark 3 *A geometric way to think of the connection between a **flow** and the induced **dynamical system** is to say that the flow of an **ODE** gives rise to a dynamical system on $\mathbb{R}$.*

Remark 4 *It is important to remember that the **map** of $U \sqsubset \mathbb{R}$ into itself may **not** be defined on all of $\mathbb{R}$. In this context, it might be useful to recall the distinction between partial recursive functions and total functions in classical recursion theory.*

Definition 5 ***Invariant set***

*A set (usually compact) $S \sqsubset U$ is **invariant** under the flow $\varphi(.,.)$ whenever $\forall t \in \mathbb{R}$, $\varphi(.,.) \sqsubset S$.*

Definition 6 ***Attracting set***

*A closed invariant set $A \sqsubset U$ is referred to as the **attracting set** of the **flow** $\varphi(t, x)$ if $\exists$ some neighbourhood V of A, s.t $\forall x \in V$ & $\forall t \geq 0$, $\varphi(t, x) \in V$ and:*

$$\varphi(t, x) \rightarrow A \text{ as } t \rightarrow \infty \tag{6}$$

Remark 7 *It is important to remember that in dynamical systems theory contexts the attracting sets are considered the **observable** states of the dynamical system and its flow.*

Definition 8 *The basin of attraction of the attracting set A of a flow, denoted, say, by* Θ_A*, is defined to be the following set:*

$$\Theta_A = \cup_{t \leq 0} \varphi_t(V) \tag{7}$$

where: $\varphi_t(.)$ *denotes the flow* $\varphi(.,.), \forall t$.

Remark 9 *Intuitively, the basin of attraction of a flow is the* ***set of initial conditions*** *that eventually leads to its attracting set - i.e., to its limit set (limit points, limit cycles, strange attractors, etc). Anyone familiar with the definition of a Turing Machine and the famous Halting problem for such machines would immediately recognise the connection with the definition of basin of attraction and suspect that my main result is obvious*[9]*.*

On the policy side, my formal assumption is that by 'policy' is meant 'rules' and my obvious working hypothesis - almost a thesis, if not an axiom - is the following:

Claim 10 *Every rule is reducible to a recursive rule*[10]

Remark 11 *This claim and the results below are valid whether by 'rule' is meant an element from a set of preassigned rules (i.e., the notion of policy as a fixed 'rule' in the 'rules vs. discretion' dichotomy) or a rule as a (partial recursive or total) function of the current state of the dynamics of a complex economy (discretionay policy)*[11]*.*

Remark 12 *If anyone can suggest a rule which cannot be reduced to a recursive rule, it can only be due to an appeal to a non-algorithmic principle like an undecidable disjunction, magic, ESP or something similar.*

Definition 13 ***Dynamical Systems capable of Computation Universality:***

A dynamical system capable of computation universality is one whose defining initial conditions can be used to program and simulate the actions of any arbitrary Turing Machine, in particular that of a Universal Turing Machine.

Proposition 14 *Dynamical systems characterizable in terms of limit points, limit cycles or 'chaotic' attractors, called 'elementary attractors', are not capable of universal computation.*

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[9] In the same sense in which the Walrasian Equilibrium Existence theorem is obvious for anyone familiar with the Brouwer (or similar) fixed point theorem(s). The finesse, however, was to formalise the Walrasian economy topologically, in the first place. A similar finesse is required here, but space does not permit me to go into details

[10] Firstly, 'recursive' is meanto to be interpreted in its 'recursion theoretic' sense; secondly, this claim is, in fact, a restatement of Church's Thesis (cf. [1], p.34).

[11] It may be useful to keep in mind the follwing caveat introduced in one of the famous papers on these matters by Kydland and Prescott ([9], p.169):

"[W]e emphasize that the choice is from a [fixed] set of fiscal policy rules."

Proposition 15 *Only dynamical systems whose basins of attraction are poised on the boundaries of elementary attractors are capable of universal computation.*

Theorem 16 *There is no effective procedure to decide whether a given observable trajectory is in the basin of attraction of a dynamical system capable of computation universality*

Proof. The first step in the proof is to show that the basin of attraction of a dynamical system capable of universal computation is recursively enumerable but not recursive. The second step, then, is to apply Rice's theorem to the problem of membership decidability in such a set.

First of all, note that the basin of attraction of a dynamical system capable of universal computation is recursively enumerable. This is so since trajectories belonging to such a dynamical system can be effectively listed simply by trying out, systematically, sets of appropriate initial conditions.

On the other hand, such a basin of attraction is not recursive. For, suppose a basin of attraction of a dynamical system capable of universal computation is recursive. Then, given arbitrary initial conditions, the Turing Machine corresponding to the dynamical system capable of universal computation would be able to answer whether (or not) it will halt at the particular configuration characterising the relevant observed trajectory. This contradicts the unsolvability of the Halting problem for Turing Machines.

Therefore, by Rice's theorem, there is no effective procedure to decided whether any given arbitrary observed trajectory is in the basin of attraction of such recursively enumerable but not recursive basin of attraction. ■

Given this result, it is clear that *an effective theory of policy is impossible in a complex economy.* Obviously, if it is effectively undecidable to determine whether an observable trajectory lies in the basin of attraction of a dynamical system capable of computation universality, it is also impossible to devise a policy - i.e., a recursive rule - as a function of the defining coordinates of such an observed or observable trajectory. Just for the record I shall state it as a formal proposition:

Proposition 17 *An effective theory of policy is impossible for a complex economy*

Remark 18 *The 'impossibility' must be understood in the context of effectivity and that it does not mean specific policies cannot be devised for individual complex economies. This is similar to the fact that non-existence of general purpose algorithms for solving arbitrary Diophantine equations does not mean specific algorithms cannot and have not been found for special, particular, such equations.*

What if the realized trajectory lies outside the basin of attraction of a dynamical system capable of computation universality and the objective of policy is to drive the system to such a basin of attraction? This means the policy maker is trying to design a dynamical system capable of computational universality with initial conditions pertaining to one that does not have that capability. Or, equivalently, an attempt is being made, by the policy maker, to devise a method by which to make a Finite Automaton construct a Turing Machine, an impossibility. In other words, an attempt is being made endogenously to construct a 'complex economy' from a 'non-complex economy'. Much of this effort is, perhaps, what is called 'development economics' or 'transition economics' and I interpret the Brock-Colander remarks on institution design in this context and, therefore, claim that it is recursively impossible. Essentially, my claim is that it is recursively impossible to construct a system capable of computation universality using only the defining characteristics of a Finite Automaton. To put it more picturesquely, a *non-algorithmic step* must be taken to go from systems incapable of self-organisation to ones that are capable of it. This interpretation is entirely consistent with the original definition, explicitly stated, of an 'emergent property' or an 'emergent phenomenon', by George Henry Lewes. This is why 'development' and 'transition' are difficult issues to theorise about, especially for policy purposes.

Thus, the Brock-Colander desideratum of requiring 'the '*complexity-trained policy economist' to 'try to seek out the boundaries of the equivalent of the basins of attractors of dynamical systems*' is a recursively undecidable task. It must, however, be remembered that this does not mean that the task is impossible in any absolute sense. There may well be non-recursive methods to 'seek out the boundaries of the equivalent of the basins of attractors of dynamical systems'. There may also be *ad hoc* means by which recursive methods may be discovered for such a task. The above theorem seeks only to state that there are no *general purpose effective methods* for such a policy task. Hence the admonition to be modest about policy proposals for a complex economy given by Brock and Colander is entirely reasonable Hence, also, when Brock-Colander hold the other horn of the complexity worldview and 'warn' the complexity vision holders that 'the complexity worldview makes policy recommendations less certain ....', they are being eminently realistic and insightful, although it may well be for other than algorithmic reasons.

3 Remarks on Generating a GET Complex Economy

Both Duncan Foley and Fontana and Buss reflect accurately the intuition of complexity theorists that a conjunction of *nonlinearity* and *high dimensionality* (just a 'little of both'), underpinned by *adaptation* (the basis for *dynamics*), might be the defining criteria for the genesis of a complex adaptive dynamical system (CADS). It is almost inevitable that the 'methods of classical analytical mathematical methods' are inadequate for the purposes of studying these systems in traditional ways - i.e., looking for closed form solutions - and the computer and its mathematics have to be harnessed in a serious way in the study of CADS. The additional *ad hoc* criterion of choosing those dynamical systems whose attracting sets are located on the boundaries of elementary attractors is less compelling from a theoretical point of view, at least to this writer.

In the citadel of economic theory, *General Equilibrium Theory*, there is more than a 'little of both', *nonlinearity* and *high dimensionality.* The general equilibrium theorist can justifiably claim that the core model has been studied with impeccable analytical rigour, 'using classical mathematical analytical methods'. Therefore, since there is so much more than 'just a little of both', nonlinearity and high dimensionality, in the general equilibrium model, it must encompass enough 'complexity' of some sort for us to be able to endow it with a 'complexity vision'. Why, the puzzled economic theorist may ask, don't we do precisely that, instead of building *ad hoc* models, without the usual micro closures [12]. On the other hand, there are three fundamental criticisms of orthodox theory - by which I shall understand the 'rigorous' version of GET and not some watered down version in an intermediate textbook - implied by the 'complexity vision'. Orthodox theory is weak on *processes*; it is almost silent on *increasing returns technology*; it is insufficiently equipped to handle *disequilibria.* The first obviates the need to consider *adaptation*; the second, in conjunction with the first, rules out *positive feedback processes*; and the third, again in conjunction with the first, circumvents the problem of the *emergence of self-organised*, novel, *orders.* I am in substantial agreement with these fundamental criticisms. However, I do not believe that orthodox theory has to be thrown overboard and an entirely new closure has to be devised for the 'complexity vision' to be encapsulated in a dynamical system capable of computation universality to represent a 'complex economy'. To be fair, I should add that BC do maintain, in varying degrees of emphasis, that the 'complexity vision' should be viewed as complementing orthodox visions and they do not envisage or advocate a throwing away of the proverbial baby with the bath water.

It is my belief that orthodox GET has been much maligned by the complexity theorists and I would like to suggest a pathway to redress some of the 'mischief'. If the pathway is reasonably acceptable, then the complexity-policy nexus can be buttressed by the fundamental theorems of welfare economics as foundations for policy[13]. Lack of space prevents me from making my case formally. I shall, however, suggest the broad line of attack that might make the case for the 'defence', so to speak. There are three elements to my pathway:

1. The Sonnenschein-Debreu-Mantel (S-D-M) ([18]) theorem on excess demand functions;

2. The Uzawa Equivalence theorem ([20]) between the Walrasian Equilibrium

[12] I use this word 'closure' instead of the more loaded word 'microfoundations' deliberately. I believe the idea of the 'closure' of neoclassical economics, i.e., based on preferences, endowments and technology, is more fundamental and can be given many more foundations than the orthodox one.

[13] It must, however, be remembered that the more important of the two theorems as policy underpinnings is the second one. On the other hand, this theorem, in its general form, relies for its proof on the Hahn-Banach theorem, which is recursively dubious.

Existence theorem and the Brouwer fixed point theorem;

3. The Pour-El and Richards theorem ([13]) on the genesis of computable generation of a non-recursive real as the solution to the IVP of an ODE;

As a consequence of the S-D-M theorem, the only structural properties that have to be preserved when, say, introducing *ad hoc* price dynamics for a general equilibrium exchange economy are Walras's Law and continuity. This is entirely compatible with the characterizations given above in defining flows of dynamical systems. Next, the main implication of the Uzawa equivalence theorem is that the economic equilibrium is a non-recursive real whereas the initial conditions of the economy given by the endowments, etc., have to be recursive reals. The question is, then, how to devise an IVP for an ODE such that for recursive reals as inputs, a non-recursive real solution is the result. Such a flow can then be associated with the price dynamics of a general equilibrium exchange economy in view of the arbitrariness sanctioned by the S-D-M theorem. It is at this point that the Pour-El and Richards construction comes into play and allows an unusual *tâtonnement* to be devised such that its actions can simulate the activities of a Turing Machine - i.e., a *tâtonnement dynamics* that is capable of computation universality.

What of policy in such a GET complex economy? The only question of policy is whether the out-of-equilibrium *tâtonnement dynamics* can be 'speeded up' towards the non-recursive real solution, while not violating the strictures of at least the second fundamental theorem of welfare economics. Needless to say, it is relatively easy to show that no effective procedure can be devised to achieve such 'speeding up'. Since the frontiers of theoretical policy discussions are almost entirely about driving out-of-equilibrium configurations towards (stochastic) dynamic equilibrium paths, the bearing of such an ineffective result must be obvious to any sympathetic reader. It is, however, a complete research agenda and not an issue that can be settled with throwaway remarks at the end of a paragraph!

4 Policy, Poetry and Political Economy

The 'complex economy' considered in the previous sections did not encapsulate any notion of 'emergence'. Emergence, at least as defined and intended by the pioneers, John Stuart Mill, George Henry Lewes and C Lloyd Morgan was meant to be an intrinsically non-algorithmic concept. So, almost by definition and default, no question of policy as rules to maintain or drive trajectories of emergent dynamical systems to desired locations in phase space is even conceivable, at least not in any formal, effective way.

Perhaps this is the reason for Hayek's lifelong scepticism on the scope for policy in economies that emerge and form spontaneous orders! It is not for nothing that Harrod's growth path was on a knife-edge and Wicksell's cumulative process was a metastable dynamical system, located on the boundary defined by the basins of attractions of two stable elementary dynamical systems (one for the real economy, founded on a modified Austrian capital theory; the other for a monetary macroeconomy underpinned by a pure credit system.)

When policy discussions resort to reliance on special economic models the same unease that causes disquiet when special interests advocate policies should be the outcome. Any number and kind of special dynamic economic models can be devised to justify almost anything - all the way from policy nihilism, the fashion of the day, to dogmatic insistence on rigid policies, justified on the basis of seemingly sophisticated, essentially *ad hoc*, models. Equally, studying patterns by simulating complex dynamical models and inferring structures, without grounding them on the mathematics of the computer is a dangerous pastime. *A fortiori*, suggesting policy measures on the basis of such inferred structures is doubly dangerous. Nothing in the formalism of the mathematics underlying the digital computer, the vehicle in which such investigations are conducted, and simulations by it, justifies formal inferences on implementable effective policies.

Impossibility and undecidability results do not mean paralysis. Arrow's impossibility theorem did not mean that democratic institution design was abandoned forever; Sen's theory of the impossibility of a Paretian liberal did not forbid individual advocacy along lines that could only be interpreted as a philosophy of a Paretian liberal; Rabin's powerful result that even though there are determined classical games, it is not possible to devise effective instructions to guide the theoretical winner to implement a winning strategy has not meant that game theory cannot be a useful guide to policy. Similarly, the impossibility of an effective theory of policy does not mean that the poets in our profession cannot devise enlightened policies that benefit a complex economy. I can do no better to illustrate this attitude than to repeat a 'Keynes story' that was told, almost with uncharacteristic awe, by the redoubtable Bertrand Russell:

> "On Sunday, August 2, 1914, I met [Keynes] hurrying across the Great Court of Trinity. I asked him what the hurry was and he said he wanted to borrow his brother-in-law's motorcycle to go to London. 'Why don't you go by train', I said. 'Because there isn't time', he replied. I did not know what his business might be, but within a few days the bank rate, which panic-mongers had put up to ten per cent, was reduced to five per cent. This was his doing."
>
> [16], pp.68-9

There are similar stories about Lindahl's intuition on policy relating to the bank rate. Keynes and Lindahl did not rely on mechanical deductions from formal mathematical models of the economy. Perhaps the growth of the complexity of economies calls forth more than intuition based on a thorough familiarity of the institutions of an economy and its behavioural underpinnings, the kind of familiarity a Keynes or a Lindahl possessed. There is, however, no question that the relevant knowledge and its manifestation in policy actions could have resulted from recursive procedures. Poetry is not an algorithmic endeavour - either in its creation or in its appreciation; nor is policy, especially in a complex economy.

Essentially the main message of this seemingly negative paper is that the justification for policy cannot be sought in effective formalisms. One must resort to poetry and classical political economy, i.e., rely on imagination and compassion, for the visions of policies that have to be carved out to make institutions locate themselves in those metastable configurations that are defined by the boundaries in which dynamical systems capable of universal computation get characterised. This is, essentially, the enlightened message that I inferred from the Brock-Colander discussion of policy in a complex economy, the paper which guided my thoughts in framing the questions posed in this paper. I am not sure the answers will be welcomed by either Brock-Colander or Foley, the other thoughtful prop which was my inspiration for the framework and contents of this paper.

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