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THE ALGORITHMIC REVOLUTION IN THE SOCIAL SCIENCES: MATHEMATICAL ECONOMICS, GAME THEORY AND STATISTICAL INFERENCE*

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Abstract

The digital and information technology revolutions are based on algorithmic mathematics in many of their alternative forms. Algorithmic mathematics *per se* is not necessarily underpinned by the digital or the discrete only; analogue traditions of algorithmic mathematics have a noble pedigree, even in economics. Constructive mathematics of any variety, computability theory and non-standard analysis are intrinsically algorithmic at their foundations. Economic theory, game theory and mathematical finance theory, at many of their frontiers, appear to have embraced the digital and information technology revolutions via strong adherences to experimental, behavioural and so-called computational aspects of their domains – without, however, adapting the mathematical formalisms of their theoretical structures. Recent advances in mathematical economics, game theory, probability theory and statistics suggest that an algorithmic revolution in the social sciences is in the making. In this paper I try to trace the origins of the emergence of this 'revolution' and suggest, via examples in mathematical economics, game theory and the foundations of statistics, where the common elements are and how they may define new frontiers of research and visions. *Essentially, the conclusion is that the algorithmic social sciences are unified by an underpinning in Diophantine Decision Problems as their paradigmatic framework.*

Keywords: Algorithmic Economics, Algorithmic Game Theory, Algorithmic Statistics, Algorithmic Social Science

1   A Preamble on Origins and Traditions

> In mathematics everything is algorithm and nothing is meaning; even when it doesn't look like that because we seem to be using words to talk about mathematical things. Even these words are used to construct an algorithm.
>
> Wittgenstein (1974)[46][1] , p. 468; italics in original.

From my earliest writings and thoughts on algorithmic issues in economics, the part of Wittgenstein's philosophy of mathematics, summarised in the above quote, has been influential in my attempt at understanding the meaning, and aim, of mathematical economics.

Wittgenstein's chosen successor for the *Knightbridge Professorship of Philosophy*, Georg Henrik von Wright, in his first monograph after succeeding Wittgenstein, [44], paid handsome tribute to the pioneers who inspired him in his own work on *Induction and Probability* (ibid, p.12)[2]:

> "The author from whom I have learnt most is undoubtedly Keynes. It seems to me that next to Francis Bacon, his has been the most fertile mind seriously to occupy itself with the questions which are the main topic of this inquiry. I have also drawn much inspiration from the works of von Mises and Reichenbach on probability."

In his admirable new book on the *Minimum Description Length Principle*, [11], with impeccable pedagogical style and content, Peter Grünwald, in turn, noted perceptively:

> "If we ignore [the problems of **uncomputability** and **large constants**], we may use Kolmogorov complexity as our fundamental concept and build a theory of idealized inductive inference on top of it. This road has been taken by Solomonoff ... starting with the 1964 paper in which he introduced Kolmogorov complexity, and by Kolmogorov, when he introduced the *Kolmogorov minimum sufficient statistic....* . Both Solomonoff's and Kolmogorov's ideas have been substantially refined by several authors. ..... Different authors have used different names for this area of research: 'ideal MDL,' 'idealized MDL,' or '**algorithmic statistics**.' It is closely related

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[1] I recall that I was deeply inspired by this Wittgensteinian vision in one of my early works on the *complexity of the policy design process*, almost a quarter of a century ago (cf. *Constructing Objective Functions for Macroeconomic Decision Models: A Formalization of Ragnar Frisch's Approach* by Berc Rustem & Kumaraswamy Velupillai, paper presented at the **5th World Congress of the Econometric Society, Boston, 1985**). I was not aware, then, of a particular Brouwerian influence on Wittgenstein's mathematical philosophy which made this vision quite natural. My current research is very much focused on forging a mathematical economics that is based on Wittgenstein's philosophy of mathematics, Brouwer's intuitionistic constructivism and Husserl's phenomenology.

[2] The *Preface* to the book from which I quote was dated December, 1948, more than two years after the sad and premature death of Keynes, in April, 1946

to the celebrated theory of *random sequences* due to P. Martin-Löf
and Kolmogorov ... ."
  ibid, p.11. (Italics in original; words in bold, added)

In an '*Introduction Some Years Later*', in fact almost forty years later, to
his remarkable UCLA doctoral dissertation of 1951, [30], written under Hans
Reichenbach, Hilary Putnam noted, with characteristic prescience (p.2; italics
added):

  "Reichenbach, following the lead of C.S. Peirce and von Mises,
  identified probability with the relative frequency of an attribute in a
  finite population (which Reichenbach thinks of as a finite sequence),
  or the limit of the relative frequency of the attribute in an infinite
  sequence. This put him in conflict with his good friend Rudolf Car-
  nap who at that time followed Keynes in thinking of *probability as
  a primitive logical notion. For Reichenbach this talk of a primitive
  logical notion was little better than sheer mysticism.*"

Keynes, in turn, was misled into thinking of *probability as a primitive logical
notion* under the unfortunate influence of Russel and Whitehead's *Principia
Mathematica*, where the disastrous attempt to reduce mathematics to logic led,
eventually, to the *grundlagenkrise* of the 1920s between Brouwer and Hilbert,
from which we had the felicitous emergence of the foundations of algorithmic
mathematics: varieties of constructive mathematics and recursion theory. It is
in this context that one must recall Brouwer's famous *first act of intuitionism*
(cf. [**?**]), with its uncompromising requirement for constructive mathematics –
which is intrinsically *algorithmic* – to be independent of 'theoretical logic' and
to be 'languageless':

  "FIRST ACT OF INTUITIONISM *Completely separating math-
  ematics from mathematical language and hence from* the phenomena
  of language described by *theoretical logic*, recognizing that intuition-
  istic mathematics is an essentially languageless activity of the mind
  having its origin in the perception of a move of time."
    ibid, p.4; italics added.

The path from Bacon, via Hume and Kant, to Wittgenstein and Brouwer,
to von Mises and Church, to Kolmogorv and Solomonoff, and, finally, to the
stage set by constructive mathematics and recursion theory enabled *Algorithmic
Statistics* to emerge in the pioneering work of Jorma Rissanen. To the best of
my knowledge *Algorithmic Statistics* was so termed first by Gács, Tromp and
Vitányi ([7], p. 2443; italics added):

  "While Kolmogorov complexity is the expected absolute measure
  of information content of an individual finite object, a similarly ab-
  solute notion is needed for the relation between an individual data
  sample and an individual model summarizing the information in the

data, for example, a finite set (or probability distribution) where the data sample typically came from. *The statistical theory based on such relations between individual objects can be called algorithmic statistics*, in contrast to classical statistical theory that deals with relations between probabilistic ensembles."

The potted outline of a possible path given above, for algorithmic statistics, via the struggles and the visions of the pioneers, is an attempt, eventually, to tell a coherent story of the emergence of the algorithmic social sciences from the foundational debates in mathematics, philosophy and epistemology. The thrust of such a reconstruction of the emergence of the algorithmic social sciences lies in pointing out that only a radically new vision of mathematical economics, game theory and statistics can lead us towards making these subjects truly applied sciences and free of mysticism and subjectivism.

I have been lecturing on *Algorithmic Economics* since at least the academic year 1991-1992, first at UCLA, and then at various Universities in Europe. The gradual awareness that even simple supply-demand models, based on rational agents maximizing utility in interdependent environments, required a reformulation of the foundations of mathematical economics in terms of *Diophantine Decision Problems*, consolidated my prior visions for algorithmic economics.

I began to think of Game Theory in algorithmic modes – i.e., *Algorithmic Game Theory* – after realizing the futility of algorithmising the uncompromisingly subjective von Neumann-Nash approach to game theory and beginning to understand the importance of *Harrop's theorem, [14]*,[3] in showing the indeterminacy of even finite games. This realization came after an understanding of effective playability in *arithmetical games*, developed elegantly by Michael Rabin more than fifty years ago, [31]. This latter work, in turn, stands on the tradition of *alternative games* pioneered by Zermelo, and misunderstood, misinterpreted and misconstrued by generations of orthodox game theorists.

However, algorithmic game theory, at least so far as such a name for a field is concerned, seems to have been first 'defined' by Christos Papadimitriou, [27], pp. xiii-xiv (italics added):

"[T]he Internet was the first computational artefact that was not created by a single entity (engineer, design team, or company), but emerged from the strategic interaction of many. Computer scientists were for the first time faced with an object that they had to feel the same bewildered awe with which economists have always approached the market. And, quite predictably, they turned to game theory for inspiration – in the words of Scott Shenker, a pioneer of this way of thinking ...... '*the Internet is an equilibrium*, we just have to identify the game.' A fascinating fusion of ideas from both fields – game theory and algorithms – came into being and was used productively

[3]I am greatly indebted to my friend, Francisco Doria, for introducing me to this important paper by Harrop.

in the effort to illuminate the mysteries of the Internet. *It has come to be called algorithmic game theory.*"

Christos Papadimitriou is one of the great contemporary scholars of computational complexity theory. However, his scholarship on the origins of algorithmic game theory leaves much to be desired - especially since *alternative games* were there, at the beginning of the emergence of recursion theory, even in the classic work of Gödel, later merging with the work that led to Matiyasevich's decisive resolution of *Hilbert's Tenth Problem*. Hence, the origins of algorithmic game theory, like those of algorithmic statistics, lie in the *grundlagenkrise* of the 1920s.

The three cardinal principles of what I have come to call the *algorithmic social sciences* are, thus: *Diophantine decision problems*[4] in algorithmic economics, *effective playability and (un)decidability* in algorithmic and alternative games[5] and *inductive inference from finite sequences* for algorithmic statistics.

However, I am only standing on the shoulders of Herbert Simon and his visions. This means there is the further unifying vision of algorithmic *problem solving*[6], by algorithmically rational agents situated in algorithmic institutions – i.e., an algorithmic formalization of the environment in which decisions are made, games are played and inductive inferences are made.

This vision is, therefore, not simply an adaptation of orthodox mathematical formalizations in the age of the digital computer. Indeed, the algorithmic revolution I am talking about is not restricted to the digital revolution; the analogue and the hybrid have their role in the emergence of the algorithmic social sciences. It is about a wholesale revamping of the three disciplines – 'an algorithmic revolution in the social sciences'.

Economists, Game Theorists and Probabilists have abundant experiences in announcing bright hopes of 'revolutions': *The Marginal Revolution*, [5] *The Keynesian Revolution*, [17], *A Revolution in Economic Theory, [16]* [7],(i.e., Game Theory), *The Rational Expectations Revolution*, [3] and [23], *The Probabilistic Revolution*, [20], [21], and numerous other similar enthusiasms have all come and gone, leaving their marks in one form or another, in the subject. There are those who claim, among them some of the more recent Nobel Laureates in Economics, that the so-called search for the microfoundations of macroeconomics brought with it an '*information revolution* in economics'. These claims have never been based on any kind of understanding of information theory – classical or modern – and its own combinatorial and algorithmic revolution. Neither

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[4]Naturally, by '*decision problem*' I mean that which is normally meant in mathematical logic, particularly in proof theory and recursion theory.

[5]In particular even in *finite* games.

[6]**Human Problem Solving** by Allen Newell and Herbert Simon, Prentice-Hall, Inc., Englewood Cliffs, New Jersey, 1972.

[7]It was refreshing that Carl Keysen's case, op.cit., was posed as a question - unlike all of the others, most of whom claimed a revolution *ex post*, sometimes to be proved false with a further passage of time. I hope I am making my case *ex ante*, with at least a small case question mark appropriately placed. I hope this essay acts as *a chronicle of a revolution foretold* – not as a 'death foretold'(*pace* Gabriel García Márquez)!

the work of Claude Shannon, nor the monumental developments in *Kolmogorov Complexity Theories*, have had the slightest impact on the work of economic theorists, whether macro, micro or microfounded macro, or even game theory. Some of these revolutions have altered and re-shaped their intended subject matter almost irreversibly; others have spawned counter-revolutions. But none – except, perhaps, the 'Probabilistic Revolution' – have encapsulated what may be called the *zeitgeist* under which the tempo of the science and technology of a whole age seems to be moving. It is in this sense that I am referring to the 'algorithmic revolution' in the social sciences, joining in the great movements in the sciences – pure and applied – and in the humanities. It is not about a narrow adaptation towards a particular kind of mathematics. It is about being a part of the scientific spirit of the times. Either we join the adventure as pioneers or we will be dragged along, screaming and sticking to outmoded systems of thought and modes of practice.

Against the backdrop provided in this preamble, the next section outlines some of the infelicities in mathematical economics, orthodox game theory and the Popperian foundations of statistical inference.. The aim is to dissect and show that the claims of computability and algorithmic feasibilities, routinely made in the core of economic theory and game theory are untenable; and to cast doubts on the equally 'routine' induction-bashing in Popperian visions of inductive inference. Following this, in section 3 it is shown, by example, what has to be done to be consistently and rigorously algorithmic in some central areas of economic theory, game theory and 'Meta–Popperian' inductive inference. The concluding section suggests that beyond even the algorithmic frontiers lies a world where a symbiosis between the indeterminacy of phenomenology and the epistemology of the algorithmic social sciences could lead to a humbling of the social sciences. It is my personal hope that this humbling may lead to the return of the social sciences to their humanistic roots.

2   Algorithmic Infelicities in Economics, Game Theory and Statistical Inference – A Glimpse

Hilbert, [15], (p. 191): 'No one shall drive us out of the paradise which Cantor has created for us.'

Wittgenstein, [45], (p.103): 'I would say, "I wouldn't dream of trying to drive anyone out of this paradise." I would try to do something quite different: I would try to show you that it is not a paradise – so that you'll leave of your own accord. I would say, You're welcome to this; just look about you." '

I shall give a few representative examples of algorithmic infelicities in mathematical economics, game theory and statistics, just to make my point that the practitioners of the orthodox visions are deluding themselves when they think they can have one foot in *Cantor's Paradise* and another in the world of *Algorithmic Mathematics*.

2.1   Game Theory

In one of the standard advanced textbooks in orthodox game theory, [25], almost at the very beginning of the text we are informed that (p. 6; italics added):

> "At the beginning of the twentieth century Zermelo ([47]) suggested that chess is a trivial game for 'rational players': he *described an algorithm that can be used to 'solve' the game.* The technique defines a pair of strategies, one for each player, that leads to an 'equilibrium' outcome .... However, despite this remarkable result .... [its] equilibrium outcome is yet to be calculated; currently it is impossible to do so using *Zermelo's algorithm.* Even if one day it is shown that White has a winning strategy, it may not be possible for a human being to implement that strategy."

There are several infelicities in this extraordinary set of claims:

1. Zermelo did not 'describe an algorithm that can be used to 'solve' the game.';

2. The 'equilibrium outcome is yet to be calculated' precisely because Zermelo did not 'describe an algorithm that can be used to 'solve' it.'

3. Zermelo, in fact, did show that 'White has a winning strategy', but did not do so algorithmically;

In the next section I demonstrate, formally, the exact content of these infelicities. However, the authors of this much lauded and extensively used advanced textbook add further incorrect algorithmic claims in their demonstration of the existence of a subgame perfect equilibrium in finite extensive games with perfect information (ibid, pp. 99-100; italics in the original; bold emphasis, added):

> "The procedure used in [the] proof [of the existence of a subgame perfect equilibrium in finite extensive games with perfect information] is often referred to as *backwards induction.* In addition to being a means by which to prove the proposition, **this procedure is an algorithm** for calculating the set of subgame perfect equilibria of a finite game. Part of the appeal of the notion of subgame perfect equilibrium derives from the fact that **the algorithm** describes what appears to be a natural way for players to analyze such a game so long as the horizon is relatively short."

These 'algorithmic' claims are also false.

In a relatively recent contribution to an analysis of the mathematical philosophy underpinning proofs in the von Neumann-Morgenstern classic, [43], Nicola Giocoli, [8], makes equally invalid claims about the constructive nature of various demonstrations in **The Theory of Games and Economic Behaviour**. Osborne and Rubinstein's infelicities are due to their incomprehension of the formal meaning of an algorithm, whether from a recursion theoretic – i.e., subject

to the Church-Turing Thesis – or from a constructive point of view. Giocoli's incorrect and misleading claims (particularly in §4.2 and §5, pp.17-24, ibid) are almost entirely due to a lack of any understanding of the meaning of constructive mathematics – and even of Hilbert's formalism. I shall leave a complete analysis of these infelicities for a different exercise, but suffice it to point out that these kinds of infelicities permeate the subject of orthodox game theory – whether in its behavioural, experimental of computational variant.

2.2   Mathematical Economics

It will not be an exaggeration to state that the 'crown jewels' of mathematical economics are the proof of the existence of (a Walrasian) equilibrium, [1], and the two fundamental theorems of welfare economics. In relation to the former, two of the practitioners of so-called *computable* general equilibrium theory had this to say, [33]:

> "The major result of postwar mathematical general equilibrium theory has been to demonstrate the existence of such an equilibrium by showing the applicability of mathematical fixed point theorems to economic models. ... Since applying general equilibrium models to policy issues involves *computing equilibria*, these fixed point theorems are important: It is essential to know that an equilibrium exists for a given model before attempting to compute that equilibrium. .....
>
>   ...
>
> The weakness of such applications is twofold. First, 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. Second, existence per se has no policy significance. .... Thus, fixed point theorems are only relevant in testing the logical consistency of models prior to the models' use in comparative static policy analysis; such theorems do not provide insights as to how economic behavior will actually change when policies change. *They can only be employed in this way if they can be made constructive* (i.e., be used to find actual equilibria). *The extension of the Brouwer and Kakutani fixed point theorems in this direction is what underlies the work of Scarf .... on fixed point algorithms ...."*
>   ibid, pp12, 20-1; italics added

However, in Scarf's classic book of 1973, [32], there is the following characteristically careful caveat to any unqualified claims to *constructivity* of the algorithm he had devised:

> "In applying *the algorithm* it is, in general, *impossible* to select an ever finer sequence of grids and a convergent sequence of sub-simplices. An algorithm for a digital computer must be basically

finite and cannot involve an infinite sequence of successive refinements. .... *The passage to the limit is the nonconstructive aspect of Brouwer's theorem*, and we have no assurance that the subsimplex determined by a fine grid of vectors on S contains or is even close to a true fixed point of the mapping."

ibid, p.52; italics added

Unfortunately, however, all of these claims are false[8]:

1. Scarf did not 'extend the Brouwer and Kakutani fixed point theorems in [the] direction' of constructivity.

2. It is not *necessary* for the fixed point theorems to be made *constructive* for them to 'be employed in this way'; it is *sufficient* for the economic propositions to be formulated recursion theoretically and, then, to appeal to one or another of the celebrated fix-point theorems of computability theory.

3. It is not correct that 'the passage to the limit is the nonconstructive aspect of Brouwer's theorem'.

Obviously, these are the kinds of infelicities that permeate the subject of mathematical economics and the only reasonable explanation with which I can make sense of them – when even outstanding theorists of the calibre of Scarf make such surprisingly false assertions – is: ignorance! But quite apart from ignorance, which can hopefully be dispelled by an investment in knowledge (!!), there is what I call the inexplicable *commitment to real analysis based on its most anti-algorithmic mode*, i.e., the mathematics of *ZFC – set theory with the axiom of choice*. Why this strange commitment? Why not model the economics of general equilibrium theory, *ab initio*, with constructive mathematics or recursion theory? A possible justification, but not an explanation, also replete with analytic infelicities and monumental ignorance, even of the basics of algorithmic mathematics – whether it is of constructive mathematics, recursion theory or even combinatorial mathematics – seems to be something like the following ([24]):

"Computing with real numbers offers some important advantages in the context of scientific computing[9]. It is also relevant to applications in economic theory. *Economic models typically use real variables and functions of them.* A model of computing in which the elementary operations are functions of real variables allows that

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[8]That these claims are false are formally and rigorously demonstrated in [38] and [40].

[9]At this point the authors refer to the book, **Complexity and Real Computation** by Blum, Cucker, Shub and Smale, [2]. However, there is no evidence whatsoever that the authors have either read or, if they have, understood the structure and contents of this fine book. I have had my say on the nature of the claims in **Complexity and Real Computation** in [39]; especially, §4.4).

model to be directly applied to standard economic models, without requiring an analysis of approximations in each application. In cases in which the analysis in the economic model is itself numerical, then, as is the case with numerical analysis generally, computation is necessarily finite and typically carried out by persons who use a finite-state machine."

ibid, pp. 1-2; italics added.

However, the authors do not tell us why 'economic models typically use real variables and functions of them'. The final sentence in the above quote is not only a tissue of nonsense, but also displays the authors' ignorance of the fundamental message and methodology of the conents of the book to which they refer as an anchor (i.e., [2]). This is the kind of reason why a complete overhaul of mathematical economics in an algorithmic mode is imperative.

2.3   Statistical Inference

The path towards an algorithmic vision of inductive inference from finite sequences is most easily subverted by varieties of Popperian infelicities that permeate the methodology of orthodox statistical inference, particularly in econometrics. I shall only outline a small fragment of the many infelicities in the Popperian approach to inductive inference[10]. As was customary with all his claims, he boldly declared that ([28]):

[T]he method of falsification presupposes *no inductive inference*, but only the tautological transformation of deductive logic *whose validity is not in dispute.*

ibid, p.42; italics added.

Paradoxically, today, neither of these assertions are considered true. No one with even a semblance of knowledge of the development of 'deductive logic' as a branch of mathematical logic would dream of being so categorical about its 'validity not being in dispute', particularly if employed by algorithmically rational agents, as in algorithmic economics.

Economic Methodology, as distinct from the methodology of mathematical economics, explicitly and implicitly, has been deeply influenced by three of Popper's seminal ideas: *falsifiability*, *the logic of scientific discovery* and the twin issues of *induction* and *inductive inference*. Underpinning them, in almost all their ramifications, is the ubiquitous spectre of *rationality* and its concomitants: rational behaviour, the rational scientist, the rational scientific enterprise and the rationality of the autonomous processes of nature. All these seem to have fallen on receptive ears, at various levels and practice, in the economic community. None of them have been given any kind of algorithmic content.

Paradoxically, however, these three seminal Popperian conceptual contributions come in the form of negative precepts. Foremost of these negative precepts

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[10]For more details I refer to chapter 14 in my forthdomcing book, **Computable Foundation for Economics**, [41].

is, of course, that there is *no* such thing as a *logic* of scientific discovery to discover; that theories can only be refuted and held, at most, provisionally, waiting for them to be refuted; and, then, there was that insistence about the *impossibility of inductive probability*.

Behind these vehement negative precepts there was, implicitly, the insistence that the epistemologist was confronted by an environment that was lawful, about which theories could be conjectured, albeit provisionally. As pointed out by Harré in his surprisingly pungent 'Obituary' of Popper, [13]:

> ... Popper's methodology of conjecture and refutation, based upon the idea of the rationality of rejecting hypotheses which have been shown at a particular time and place to be false, also depends upon an assumption of a form of the uniformity of nature. In his case, it is the negative assumption that the universe will not change in such a way as to make what was disconfirmed today true tomorrow. Popper's methodology of conjecture and refutation makes no headway in the testing of that proposition. His claim to have solved the problem of induction must now be rejected.

I shall not address specific issues of economic methodology from any particular Popperian point of view here. Instead, I aim, hopefully, to provide less negative visions of two of these great Popperian themes and help disseminate a more positive attitude towards the rich possibilities of pursuing an inductive methodology in the search for laws of scientific discovery, buttressed by a dynamic, algorithmic, reinterpretation of the meaning of falsifiability. (Classical) recursion theory and applied recursion theory, in the form of algorithmic complexity theory, will be my conceptual and methodological tools in this adventure.

In his 1972 *Addendum* to the 1972 edition of **The Logic of Scientific Discovery**, Popper was quite explicit about the *logical* basis of falsifiability[11]:

> "[T]he content or the testability (or the **simplicity** ...) of a theory may have *degrees*, which may thus be said to relativize the idea of falsifiability (whose logical basis remains the *modus tollens*."
> ibid, p.135; italics in original, bold emphasis added.

It is immediate that two dubious mathematical logical principles are implicitly invoked in any falsifiability exercise based on *Modus (Tollendo) Tollens*: *principium tertium non datur* and *proof by contradiction*. This means an adherence to *non-constructive* methods in all cases involving infinite alternatives. How experiments can be arranged and methods devised to test for falsifiability, even abstracting away from inductive inferential problems, in a non-constructive

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[11] I have often wondered why the German original 'Forschung' was translated as 'Scientific Discovery'! I am sure there must be a perfectly 'rational' Popperian explanation for the particular choice of words in English. Something like The *Logic of Scientific Research* or *The Logic of Scientific Investigation* would have been a more faithful translation of the title (and its contents). I shall, whenever I refer to this book, refer to it as *LdF*, even though it will be to [29].

environment, escapes me. Indeed, how any method to test for falsifiability can be anything other than algorithmic, *in some sense*, is beyond my understanding.

It is this kind of reliance on traditional logic and a limited knowledge of the vast developments in mathematical logic in the 20th century that I find mysterious in a philosopher who seemed to be encyclopedic in his awareness of so much else. I find no evidence, in my perusal and attempted reading of as much as possible of Popper's voluminous writings, of any awareness, either, of the fact that mathematical logic had itself branched off, in the 20th century, into four or five sub-disciplines and, in any case, into: set theory, proof theory, recursion theory and model theory. This is the kind of reason why Glymour, for example, was scathing in his criticism of a class of philosophers in general, but of Popper, in particular, [9]:

> "With only a little logical knowledge, philosophers in this period understood the verifiable and the refutable to have special logical forms, namely as existential and universal sentences respectively. There was, implicitly a positivist hierarchy . . . . Positivists such as Schlick confined science to and meaning to singular data and verifiable sentences; 'anti-positivists', notably Popper, confined science to the singular data and falsifiable sentences. In both cases, what could be known or discovered consisted of the singular data and verifiable sentences, although there is a hint of something else in Popper's view".
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> ibid, p.268.

On the other hand, if one feels it is necessary to retain fidelity to Popper's reliance on *Modus (Tollendo) Tollens* as an underpinning for falsifiability exercises, then it seems to me that the best way to do so would be via formalizations using recursion theory. Classical logical principles retain their validity but methods are given *algorithmic content* which makes them implementable devices in experimental design.

3   Towards Algorithmising Mathematical Economics, Game Theory and Statistical Inference

> "Does the axiom of choice *create* the choice set? Can one supply a missing set just by a declaration of existence? Of course no axiom, no declaration of existence, can create a real object, but it is not the purpose of an axiom to create a real object. An axiom has a part to play only in a formal system. The axiom of choice is a *limitation on the use of the word set* in formalised set theory. The acceptance or rejection of the axiom of choice is a decision about the use of a word. Some prefer one use of the word and others a different sue. To ask

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if the axiom of choice is true is to confuse the world of mathematics with the real world."

R. L. Goodstein, [10], p.82; italics in original.

3.1 Mathematical Economics

In his classic and thoughtful **Three Essays on the State of Economic Science**, [19], Tjalling Koopmans observed (p. 60; italics added):

"Before turning to [the] discussion [of the model of competitive equilibrium] it is worth pointing out that in this particular study our authors [Arrow and Debreu] have *abandoned demand and supply functions as tools of analysis*, even as applied to individuals. The emphasis is entirely on the *existence of some set of compatible optimising choices ... .* The problem is *no longer* conceived as that of *proving that a certain set of equations has a solution.* It has been reformulated as one of proving that a certain number of maximizations of individual goals under independent restraints can be simultaneously carried out"

The new emphasis brought with it a new formalism and a mathematics to encapsulate it that was entirely divorced from numerical meaning and algorithmic significance. The continuous in its real number versions came to be the vehicle of analysis and algorithmic implementations required approximations which were, correspondingly, divorced from theory. It is not as if it was necessary to recast the fundamental economic problem of finding equilibrium solutions between supply and demand, 'even as applied to individuals', as one of finding a proof of the existence a solution to 'maximizations of individual goals under independent restraints'. As Steven Smale perceptively remarked, but over two decades later – yet more than thirty years ago, [34]:

"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.* Furthermore, proofs of fixed point theorems traditionally use difficult ideas of algebraic topology, and this has obscured the economic phenomena underlying the existence of equilibria. 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 equilibrium, 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*."
ibid, p.290; italics added.

The 'mathematics of a different kind' that Smale refers to, surely, is any variant of algorithmic mathematics – constructive analysis, computability theory (including computable analysis) and even combinatorial mathematics. Given the algorithmic foundations of both constructive analysis and computability theory and the intrinsic dynamic form and content of algorithms, it is clear that this will be a 'mathematics with dynamic and algorithmic overtones'. This means, thus, that algorithmic economics is a case of a new kind of mathematics in old economic bottles. The 'new kind of mathematics' implies new questions, new frameworks, new proof techniques - all of them with algorithmic and dynamic content for digital domains and ranges.

Taking a cue from a perceptive mathematician, Steve Smale, to remind us that 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',* and remembering that economic quantities are constrained to be, rational or integer valued[12], the natural formalization is clearly in terms of a Diophantine Decision Problem. Such a formalization, in one fell swoop, encapsulates the idea of 'mathematics with dynamic and algorithmic overtones' to 'attach the equations directly to give existence of solutions'. But there is a price to pay: *algorithmic undecidability.* I shall only give an idea of the formalization, keeping in mind just two important constraints in thinking about the classic problem of equating supply and demand: that they are formulated in the form of equations; that the constants and variables that enter the individual supply and demand equations are constrained to be integer or rational valued (apart from, of course, being non-negative).

Then, the general supply=demand problem can be formulated, abstractly as follows:

Definition 1 *A relation of the form*

$$D\left(a_1, a_2, ..., a_n, x_1, x_2, ..., x_m\right) = 0$$

where D is a polynomial with integer coefficients with respect to all the variables $a_1, a_2, ..., a_n, x_1, x_2, ..., x_m$ (also integer or rational valued), separated into **parameters** *$a_1, a_2, ..., a_n$ and* **unknowns** *$x_1, x_2, ..., x_m$, is called a* **parametric Diophantine equation***.*

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[12] Much fuss is made of the importance of non-negative constraints in the formalization of the economic optimization problem. Even more plaudits are granted the so-called perceptive observation that 'counting equations and variables' is not an adequate way to study the problem of the existence of equilibrum prices and quantities. In all this fuss and brouhaha, no one seems to have wondered why these non-negative quantities and prices were also not constriend to be integer or rational valued, as they must be in the real world of economics.

Definition 2 *D in Definition 1 defines a set* $\mathcal{F}$ *of the parameters for which there are values of the unknowns such that:*

$$\langle a_1, a_2, \ldots\ldots, a_n \rangle \in \mathcal{F} \iff \exists x_1, x_2, \ldots, x_m \left[D\left(a_1, a_2, \ldots\ldots, a_n, x_1, x_2, \ldots, x_m\right) = 0 \right]$$

Loosely speaking, the relations denoted in the above two definitions can be called *Diophantine representations.* Then sets, such as $\mathcal{F}$, having a Diophantine representation, are called simply *Diophantine.* With this much terminology at hand, it is possible to state the fundamental problem of Diophantine equations as follows:

Problem 3 *A set, say* $\langle a_1, a_2, \ldots\ldots, a_n \rangle \in \mathcal{F}$*, is given. Determine if this set is Diophantine. If it is, find a Diophantine representation for it.*

Of course, the set $\mathcal{F}$ may be so structured as to possess equivalence classes of properties, P and relations, R. Then it is possible also to talk, analogously, about a *Diophantine representation of a Property* P or a *Diophantine representation of a Relation* R. For example, in the latter case we have:

$$R\left(a_1, a_2, \ldots\ldots, a_n\right) \iff \exists x_1, x_2, \ldots, x_m \left[D\left(a_1, a_2, \ldots\ldots, a_n, x_1, x_2, \ldots, x_m\right) = 0 \right]$$

Hence, given, say partially ordered preference relations, it is possible to ask whether it is Diophantine and, if so, search for a Diophantine representation for it. Next, how can we talk about the solvability of a Diophantine representation? This is where undecidability (and uncomputability) will enter this family of 'inviting flora of rare equations' - through a remarkable connection with recursion theory, summarized in the next Proposition:

Proposition 4 *Given any parametric Diophantine equation, D, it is possible to construct a Turing Machine, M, such that M will eventually* **Halt***, beginning with a representation of the parametric n-tuple,* $\langle a_1, a_2, \ldots\ldots, a_n \rangle$*,* **iff** *$D$ in Definition 1 is solvable for the unknowns,* $x_1, x_2, \ldots, x_m$.

But, then, given the famous result on the *Unsolvability of the Halting problem for Turing Machines*, we are forced to come to terms with the unsolvability of Diophantine equations[13]. Hence, the best we can do, as mathematical economists, and even as algorithmically rational behavioural agents, so long as the constraints are Diophantine, is to act according to the gentle and humble precepts of *Linnean classifications*: 'collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions', as George Temple wisely noted, when referring to the study and use of differential equations, [37][14]:

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[13] It must, of course, be remembered that all this is predicated upon an acceptance of the *Church-Turing Thesis.*

[14] I have substituted the word 'Diophantine' for 'differential' in this perceptive observation by George Temple.

"The group of problems which I propose to describe belong to that Cinderella of pure mathematics- the study of *Diophantine equations.* The closely guarded secret of this subject is that it has not yet attained the status and dignity of a science, but still enjoys the freedom and freshness of such pre-scientific study as natural history compared with botany. The student of *Diophantine equations* ... is still living at the stage where his main tasks are to collect specimens, to describe them with loving care, and to cultivate them for study under laboratory conditions. The work of classification and systematization has hardly begun.   ....

... An inviting flora of rare equations and exotic problems lies before a botanical excursion into the *Diophantine* field."

ibid, p.233.; italics in original

The mathematical economists have rushed into '*Cantor's Paradise*', arming themselves with *unreal axioms*, to use non-algorithmic mathematics to formalize a subject that is *intrinsically algorithmic* and providing false hopes, time after time.

3.2   Game Theory

I shall confine myself to the formalization of Zermelo's game, simply to demonstrate – once again – the intrinsic algorithmic nature of the original game theoretic model, but also to show a continuity and a consistency with the example in the precious subsection.

In the case of game theory the subversion into its non-algorithmic, subjective vision of economic behaviour in adversarial situations was a direct consequence of the fixed point approach pioneered by von Neumann and Nash. I think I can make a strong case to substantiate this assertion[15]. My starting point would be Zermelo's celebrated lecture of 1912 (op.cit, Zermelo, 1913) and his pioneering formulation of an adversarial situation into an *alternating game* and its subsequent formulation and solution as a mini-max problem by Jan Mycielski in terms of *alternating the existential and universal quantifiers.*

The Zermelo game has *no subjective component of any sort.* It is an entirely objective game of perfect information, although it is often considered, incorrectly in my opinion, part of the orthodox game theoretic tradition. Let me describe the gist of the kind of game considered by Zermelo, first. In a 2-player game of perfect information, alternative moves are made by the two players, say A and B. The game, say as in Chess, is played by each of the players 'moving' one of a finite number of counters available to him or her, according to specified rules, along a 'tree' - in the case of Chess, of course, on a board of fixed dimension, etc. Player A, say, makes the first move (perhaps determined by a 'chance' mechanism) and places one of the counters, say $a_0 \in A_0$, on the designated

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[15] In direct analogy with the kind of observation made by Steve Smale about transforming an intrinsic equation approach to the problem of supply-demand equilibrium, to one of inequalities formulated as fixed point problems.

'tree' at some allowable position; player B observes the move made by A, with perfect recall, the placement of the counter a_0 - and makes the second move by placing, say $b_1 \in B_1$, on an allowable position on the 'board'; and so on. Let us suppose these alternating choices terminate after Player B's n^{th} move; i.e., when $b_n \in B_n$ has been placed in an appropriate place on the 'board'.

Definition 5 *A **play** of such a game consists of a sequence of such alternative moves by the two players*

Suppose we label the alternating individual moves by the two players with the natural numbers in such a way that:

1. The even numbers, say, $a(0), a(2), ..., a(n-1)$ enumerate player A's moves;

2. The odd numbers, say, $b(1), b(3), ..., b(n)$ enumerate player B's moves;

   - Then, each (finite) play can be expressed as a sequence, say γ, of natural numbers.

Suppose we define the set α as the set of plays which are wins for player A; and, similarly, the set β as the set of plays which are wins for player B.

Definition 6 *A strategy is a function from any (finite) string of natural numbers as input generates a single natural number, say σ, as an output.*

Definition 7 *A game is **determined** if one of the players has a winning strategy; i.e., if either $\sigma \in \alpha$ or $\sigma \in \beta$.*

Theorem 8 *Zermelo's Theorem: $\exists$ a winning strategy for player A, whatever is the play chosen by B; and vice versa for B*

Remark 9 *This is Zermelo's version of a minimax theorem in a perfect recall, perfect information, game.*

It is in connection with this result, and the minimax form of it, that Steinhaus observed, with considerable perplexity, [36]:

> "[My] inability [to prove the minimax theorem] was a consequence of the ignorance of Zermelo's paper in spite of its having been published in 1913. .... J von Neumann was aware of the importance of the minimax principle [in [42]]; it is, however, *difficult to understand the absence of a quotation of Zermelo's lecture in his publications.*"
>
> ibid, p. 460; italics added

Why didn't von Neumann refer, in 1928, to the Zermelo-tradition of *alternating games*? The point I wish to make is something else and has to do with the *axiom of choice* and its place in *game theory*. So, let me return to this theme.

Mycielski (cf., Steinhaus, op.cit, pp. 460-1) formulated the Zermelo minimax theorem in terms of *alternating logical quantifiers* as follows[16]:

$$\sim \left\{ \bigcup_{a_0 \in A_0} \bigcap_{b_1 \in B_1} \ldots\ldots\ldots \bigcup_{a_n \in A_{n-1}} \bigcap_{b_n \in B_n} (a_0 b_1 a_2 b_3 \ldots a_{n-1} b_n) \right\} \in \alpha$$

$$\implies \left\{ \bigcap_{a_0 \in A_0} \bigcup_{b_1 \in B_1} \ldots\ldots\ldots \bigcap_{a_n \in A_{n-1}} \bigcup_{b_n \in B_n} (a_0 b_1 a_2 b_3 \ldots a_{n-1} b_n) \right\} \notin \beta$$

Now, summarizing the structure of the game and taking into account Mycielski's formulation in terms of alternating we can state as follows:

1. The sequential moves by the players can be modelled by alternating existential and universal quantifiers.

2. The existential quantifier moves first; if the total number of moves is odd, then an existential quantifier determines the last chosen integer; if not, the universal quantifier determines the final integer to be chosen.

3. One of the players tries to make a logical expression, preceded by these alternating quantifiers *true*; the other tries to make it *false*.

4. Thus, inside the braces the win condition in any play is stated as a proposition to be satisfied by generating a number belonging to a given set.

5. If, therefore, we can extract *an arithmetical form* - since we are dealing with sequences of natural numbers - for the win condition it will be possible to discuss recursive solvability, decidability and computability of winning strategies.

The above definitions, descriptions and structures define, therefore, an *Arithmetical Game* of length n. Stating the Zermelo theorem in a more formal and general form, we have:

Theorem 10 *Arithmetical Games of finite length are determined.*

The more general theorem, for games of arbitrary (non-finite) length, can be proved by standard diagonalization arguments and is:

Theorem 11 *Arithmetical Games on any countable set or on any set which has a countable complement is determined.*

Now, enter the *axiom of choice*! Suppose we allow, at first, any unrestricted sets α and β. Then, for example if they are assumed to be *imperfect sets*[17], the game is not determined. If we work within *ZFC*, then such sets are routinely acceptable and lead to games that cannot be determined - even if we assume perfect information and perfect recall. Surely, this is counter-intuitive? For this reason, *this tradition* in game theory chose to renounce the axiom of choice and work with an alternative axiom that restricts the class of sets within which arithmetical games are played. The alternative axiom is the *axiom of determinacy*, introduced by Steinhaus:

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[16] This is the formal way Gödel derived *undecidable sentences.*

[17] A set $\mathcal{F}$ is a *perfect set* if it is a *closed set in which every point is a limit point.*

Axiom 12 *The **Axiom of Determinacy**: Arithmetical Games on every subset of the Baire line[18] is determined.*

The motivation given by Steinhaus (op.cit, pp. 464-5) is a salutary lesson for mathematically minded economists or economists who choose to accept the axiom of choice on 'democratic' principles or economists who are too lazy to study carefully the economic meaning of accepting a mathematical axiom:

> "It is known that [the Axiom of Choice] produces such consequences as the decomposition of a ball into five parts which can be put together to build up a new ball of twice the volume of the old one [the Banach-Tarski paradox], a result considered as paradoxical by many scientists. There is another objection: how are we to speak of perfect information for [players] A and B if it is impossible to verify whether both of them think of the same set when they speak of ["α"]? *This impossibility is inherent in every set having only [the Axiom of Choice] as its certificate of birth.* In such circumstances it is doubtful whether human beings will ever play really [an infinite game].
> *All these considerations impelled me to place the blame on the Axiom of Choice.* Sixty years of the theory of sets have elapsed since this Axiom was proclaimed, and some ideas have .... convinced me that a purely negative attitude against [the Axiom of Choice] would be dangerous to propose. Thus I have chosen the idea of replacing [the Axiom of Choice] by the [above Axiom of Determinacy].
> italics added.

There is a whole tradition of game theory, beginning at the beginning, so to speak, with Zermelo, linking up, via Rabin's modification of the Gale-Stewart infinite game, to *recursion theoretic formulations of arithmetical games* underpinned by the *axiom of determinacy* and completely independent of the *axiom of choice* and **eschewing all subjective considerations**. In this tradition notions of *effective playability, solvability* and *decidability* questions take on fully meaningful computational and computable form where one can investigate whether it is feasible to instruct a player, who is known to have a winning strategy, to actually select a sequence to achieve the win. None of this is possible in the orthodox tradition, which *suggests* there are no alternative mathematics for investigating, mathematically, adversarial situations in the social sciences.

Finally, once formulated as an alternating arithmetical game, it is easy to consider the problems of effective playability, solvability and decidability in terms of the framework of Hilbert's tenth Problem, i.e., as a Diophantine Decision Problem - just as in the case of the problem of supply-demand equilibrium, as discussed in the previous subsection.

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[18] A *Baire line* is an *irrational line* which, in turn, is a line obtainable from a continuum by *removing a countable dense subset.*

3.3 Statistical Inference

Popper does not seem to have paid much attention to the great achievements in recursion theory, proof theory or model theory to substantiate his case for empirical methodology or for falsification. As to why he did not seek recourse to recursion theory, in the case of *inductive inference* or the logic of scientific discovery, could it, perhaps, be because such a framework may have cast doubts on his negative critique against these thorny concepts? One can only speculate and I do speculate simply because these three branches of modern mathematical logic provide literally the proverbial 'tailor-made' formalisms for empirically implementable mathematical structures for *falsifiability*, the *logic of scientific discovery* and for *induction in all its manifestations*.

There are two characteristically prescient Popperian observations very early on in *Ldf*:

> [I] am going to propose ... that the *empirical method* shall be characterized as a method that excludes precisely those ways of evading falsification which ... are logically possible. According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival.

> ... The root of [the problem of the validity of natural laws] is the apparent contradiction between what may be called 'the fundamental thesis of empiricism' - the thesis that experience alone can decide upon the truth or falsity of scientific statements - and Hume's realization of the inadmissibility of inductive arguments. This contradiction arises only if it is assumed that all empirical scientific statements must be 'conclusively decidable', i.e., that verification and their falsification must both in principle be possible. If we renounce this requirement and admit as empirical also statements which are decidable in one sense only - unilaterally decidable and, more especially, falsifiable - and which may be tested by systematic attempts to falsify them, the contradiction disappears: the method of falsification presupposes no inductive inference, but only the tautological transformations of deductive logic whose validity is not in dispute.

Firstly, in what other way, if not by means of an *algorithm*, can we understand the processes implied by implementing an *empirical method*?

Secondly, Popper endeavours to drive a wedge between verifiability and falsifiability in terms of *decidability* - but, we know, based on *Modus (Tollendo) Tollens*. There is, however, a much simpler way to drive this wedge *and* preserve the *algorithmic character* of implementable *empirical methods* Moreover, it will not be necessary to make the incorrect claim that 'the method of falsification presupposes no inductive inference'.

Thirdly, there is the need to be precise about what is meant by a *natural law* and a *scientific statement*, before even discussing the meaning of their *truth* or *falsity*.

I shall take it that Popper means by a *natural law* something as paradigmatic as, for example, *Newton's Law of Motion* or, at a slightly more sophisticated level, say, the *General Theory of Relativity*. As an economist, I have never felt that we have the equivalent of a *natural law*, in the above senses, in economic theory. Perhaps, at a much lower level sophistication, we may, as economists, invoke one of the popular theories of growth, say *Solow's Growth Theory*.

Such *natural laws*, for example *Newton's Laws of Motion* are framed, when mathematized, as formal *dynamical systems*. Of such systems we ask, or test, whether, when they are appropriately initialized, they enter the definable *basin of attraction* of, say, a limit point, a limit cycle, a strange attractor or, perhaps, get trapped in the boundaries that separate a limit cycle and a strange attractor. In the case of the *Solow Growth Model*, theory predicts that the dynamical system, for all economically meaningful initial conditions, enters the basin of attraction of a limit point. The theory and its law can, in principle be '*verified*'.

However, it is for very few dynamical systems that we can answer the above type of question unambiguously, i.e., '*verifiably*'. This is the key point made by Popper in his almost lifelong quest for a kind of scepticism about theories and the natural laws inherent in them. It is just that such a scepticism comes naturally to those accustomed to formalizing in terms of proof theory, model theory and recursion theory - i.e., for those working in the domain of the constructive, non-standard or computable numbers.

Moreover, a *natural law* in any of the above senses is, at least from Popper's point of view, which I think is the commonsense vision, is a *scientific statement*, as indeed referred to as such by Popper in the above characterization. What, next, does it mean to formalize the notion of a *scientific statement*? Clearly, in the form of something like a *well formed formula* in some formal, mathematical, logic. Obviously, what is, then, meant by 'deciding upon the truth or falsity of scientific statements', must also be a commonsense interpretation; i.e., the 'truth' or 'falsity' of the implications of the scientific statement which encapsulates the natural law. I shall assume, therefore, that the set of meaningful scientific statements form an *enumerable infinity*.

Fourthly, Popper claims that the distinction between verifiability and falsifiability depends on allowing for a certain kind of *one-way decidability*. More precisely, verifiability is characterized by a 'strong' sense of decidability and falsifiability by a somewhat 'weaker' concept of decidability. In Popper's case, of course, the underpinning to formalize the distinction between a 'strong' and a 'weak' sense is *Modus (Tollendo) Tollens*. I seek a more dynamic version of the possibility of such a distinction, simply because many, if not most, meaningful *natural laws* are framed dynamically or as dynamical systems. By 'dynamically', I mean, the implication of the theory, when formulated as a *natural law*, and subject to experimental procedures, generates a sequence of outcomes, usually

numerical[19], which has to be sequentially monitored and tested.

Fifth, there is a need to be absolutely precise about what Popper means, formally, by 'exposing to falsification, *in every conceivable way*, the system to be tested'. How many conceivable ways would there be, given an 'experimental method', to 'expose to falsification the system to be tested'? Suppose, as in conventional economic theory, the domain of definitions is the real number system. Then, in principle, an uncountable infinity of 'conceivable ways' would have to be devised for 'the system to be tested'. This is meaningless in any empirical system.

The best that can be attempted, in principle, is to enumerate a countable infinity of *empirical methods* and for the case, for example, of *natural laws* formalized as dynamical systems, to quantify the notion of *every conceivable way* by varying the initial conditions in a precisely formalized countably infinite, enumerable, mode - i.e., *algorithmically* - but not necessarily subject to the *Church-Turing Thesis*. In other words, *algorithmically* could also be encapsulated within the broader canvas of *constructive mathematics* (or also more narrowly than even recursion theory)[20].

Finally, there is the need to be precise (and sensible) about what Popper could have meant by 'select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival'. It is here, contrary to enlightened Popperian critics, that I find that *inductive inference* enters the Popperian world with almost a vengeance. How does one formalize the selection criterion that is suggested by Popper? What could be meant by 'fittest'? Surely not some facile neo-Darwinian formalism via, say, genetic algorithms in the conventional sense.

This is where Glymour and Harré, for example, presumably locate Popper's adherence to the Platonic assumption of the 'unalterability of nature'. For, if not, we cannot, of course, 'expose them *all*' to any kind of test, let alone the more specific test of 'the fiercest struggle for survival'. By the time we come, say, to *scientific statement*, say, #10948732765923, and the *natural law* implied by it, and say *empirical method* #371952867 for testing it, there is no guarantee that our theoretical world picture would not have changed - from the Ptolemaic world vision to the Copernican vision. This would mean some of the *scientific statements* had become meaningless and others, not in the original enumerated list, become feasible candidates for testing.

I shall circumvent these issues by suggesting that we interpret Popper's criterion of the 'fittest' by the analogous criterion, in some precise sense formalizable notion, of 'most likely' or 'most plausible' by invoking yet another historical nemesis of Popper: *Ockham*.

From here, via Algorithmic Statistics, to justify inductive inference even in a Popperian framework is quite simple. I shall, instead, discuss the possibility and meaning of algorithmic falsification.

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[19] If not explicitly numerical then, in principle, codifable number theoretically using one of the well-known procedures emanating from 'Gödel Numbering'.

[20] I shall, however, work within the framework of classical recursion theory here and, hence, subject to the *Church-Turing Thesis*.

My suggestion for the algorithmic formalism of falsifiability proceeds as follows. First, three definitions.

Definition 13 *Recursive Set*
$S \subseteq \aleph$ *is* recursive iff $\exists$ *a Turing Machine for* deciding *wether any given member of $\aleph$ belongs to S.*

Definition 14 *Decidable Set*
A set S is decidable *if, for any given property $P(s)$, $\forall s \in S$, $\exists$ a Turing Machine such that it halts* iff *$P(s)$ is valid.*

Definition 15 *Recursively Enumerable Sets*
$S \subseteq \aleph$ *is* recursively enumerable *(R.E)* iff *it is either empty or the range of a Turing Machine (i.e., the range of a partial recursive function).*

Thus, for any decidable set, we know there will be *effective* experimental methods - i.e., algorithms - to characterize any member of the set. It is clear from the above definitions that a *recursive set is decidable*. This is the universe of the *verifiable*.

Falsifiability and verifiability are methods, i.e., *procedures* to decide the truth value of propositions. Popper claims, in view of his allegiance to classical logic and *Modus)Tollendo) Tollens* that the only viable procedure in a scientific enterprise is one which is capable of falsifying a law. This translates into the following: a set has to exhibit *undecidabilities*. This means it is not sufficient to work with an outcome space that is confined to recursive sets. A subtle modification of the definition of a recursive set to allow for an open-endedness, suggested as a requirement by Popper, will achieve it.

The intuitive idea is the following. Suppose the inferred *scientific statement* and its implied *natural law* are formalized as the hypothesis that is to be experimentally tested. The idea is that some implication of the hypothesis is to be verified or falsified. If the set of outcomes of the implication forms a recursive set, then we know that it is decidable and, hence, verifiable. Suppose, however, the set of outcomes of the implications form a *recursively enumerable set*. Then, whether or not any particular $P(s)$ is valid is undecidable in the following precise sense. Given an arbitrary predicted outcome of the experimental procedure of the law, say $n \in \aleph$, we test whether it is the range of a Turing Machine. If it is, it can, eventually, be decided. If it is not, we will never know. The next output of the experimental setup, after say output # 32786591 may well be the confirming instance. But there will be an open-endedness which means such laws can, at best, be accepted provisionally if they meet other criteria of adequacy.

There is a precise sense in which the above scheme generalizes and meets objections to Popper's more classical definition of falsifiability. Even although recursion theory is based on classical logic, the exclusive reliance on *Modus (Tollendo) Tollens* and singular data and falsifiable sentences are removed to be special cases. To put it in a different way, the verifiable relied on the existential

form for a testable sentence (i.e., $\exists x$ s.t $S(x)$); and the falsifiable relied on the universal quantifier (i.e., $\forall x$, s.t $S(x)$).

In terms of Gödel's results, my suggestions can be stated in yet another, equivalent, form. The Gödel scheme shows how to transform any given proposition into one about polynomials. Then, there exist arithmetical equations, linking two polynomials representing propositions, *preceded by some finite sequence of existential and universal quantifiers* that are effectively undecidable. This is the sense in which there is no longer any reliance on singular data or singular sentences.

This last observation links the suggested framework for mathematical economics and game theory with the one for statistics to provide a unified algorithmic approach to the social sciences.

4   Beyond the Algorithmic Frontiers

"Quite probably, with the development of the modern computing technique it will be clear that in very many cases it is reasonable to conduct the study of real phenomena avoiding the intermediary stage of stylizing them in the spirit of the ideas of mathematics of the infinite and the continuous, and passing directly to discrete models. This applies particularly to the study of systems with a complicated organization capable of processing information. In the most developed such systems the tendency to discrete work was due to reasons that are by now sufficiently clarified. It is a paradox requiring an explanation that while the human brain of a mathematician works essentially according to a discrete principle, nevertheless to the mathematician the intuitive grasp, say, of the properties of geodesics on smooth surfaces is much more accessible than that of properties of combinatorial schemes capable of approximating them.

Using the brain, as given by the Lord, a mathematician may not be interested in the combinatorial basis of his work. But *the artificial intellect of machines must be created by man, and man has to plunge into the indispensable combinatorial mathematics.* For the time being it would still be premature to draw final conclusions about the implications for the general architecture of the mathematics of the future."

Kolmogorov, [18], pp. 30-1; italics added.

The key to the algorithmic revolution in the social sciences if the prescient observation by Kolomogorov that '*the artificial intellect of machines must be created by man, and man has to plunge into the indispensable combinatorial mathematics.*' Every formalization of economic theory indulges in one or another form of mathematical assumption about the 'artificial intellect of machines', and almost without exception 'created by man'. The assumption of the rational agent used in mathematical economics and game theory endows such an agent

with an 'artificial intellect of a machine', albeit that which is idealized beyond all physical laws and empirical constraints. Then to imagine that this kind of idealization is similar to idealized billiard balls moving on frictionless surfaces is simply bad social science.

Thus, in the algorithmic social sciences, the rational agent is modelled as an algorithmically rational agent – but this was a path suggested first by Herbert Simon more than half a century ago. The economy is assumed, in the algorithmic social sciences, to be generating – and to be capable of processing – data that makes algorithmic sense: recursive sequences, recursively enumerable sequences, and so on. Maury Osborne's wise remarks on the need to be careful about such matters is worth recalling, [26]:

> "When you read descriptions of an auction market, think carefully of what sense, and for what variables the words continuous or continuity is being used. ...
>
> ....
>
> The distinction between the 'market concept' of continuity ... and the mathematical concept of continuity .. are of great importance to the profit and safety of the market maker.
>
> ...
>
> As for the question of replacing rows of closely spaced dots by solid lines, you can do that too if you want to, and the governors of the exchange and the community of brokers and dealers who make markets will bless you. If you think in terms of solid lines while the practice is in terms of dots and little steps up and down, this misbelief on your part is worth, I would say conservatively, to the governors of the exchange, at least eighty million dollars per year.
>
> ibid, pp. 33-4.

This estimate of eighty million dollars was made, one must recall, in 1977!

Even more pertinently, especially for the audience at this conference, was a characteristically perceptive rhetorical question, posed by Richard Hamming, [12], about the kind of numbers that are appropriate for a theory of probability, when that theory is being devised with particular applications in mind – and not just as a theory in its pure mathematical vein:

> "Thus without further examination it is not completely evident that the classical real number system will prove to be appropriate to the needs of probability. Perhaps the real number system is: (1) not rich enough - see non-standard analysis; (2) just what we want - see standard mathematics; or (3) more than is needed - see constructive mathematics, and computable numbers. ...
>
> What are all these uncountably many non-computable numbers that the conventional real number system includes?....
>
> The intuitionists, of whom you seldom hear about in the process of getting a classical mathematical education, have long been ar-

ticulate about the troubles that arise in the standard mathematics
....

What are we to think of this situation? What is the role in *probability theory* for these numbers which can never occur in practice?"
ibid, p.190.

The kind of considerations articulated by these pioneers of different aspects of algorithmic visions, Kolmogorov, Osborne and Hamming, are those that motivate the algorithmic social scientist: to build rigorous theoretical models of aspects of the real world one is trying to probe, taking account of the nature of the 'prober', the 'probed' and the methods used in the 'probing'. In contrast to the non-reflective, non-algorithmic, social scientist, the algorithmic social scientists does not subscribe to the 'one size fits all' philosophy of mathematical modelling – i.e., reliance on the orthodox mathematics of real analysis, buttressed by a set theory based on *ZFC*.

Alas, the price the algorithmic social scientist has to pay, for this enlightened approach to the mathematical modelling of the subject matter of the social sciences, comes in the form of algorithmically defined *indeterminacies*, *undecidabilties*, *uncomputabilities* and *unsovabilities*. This is, after all, the stuff of which the real world is made. For too long, the orthodox social scientist, with an ostensible command of an imperial mathematics has been ruling the roost by assertions of uniqueness, stability, determinacy and computability of equilibria in markets peopled by super-rational agents – in Herbert Simon's apt description: the Olympian Rational Agent – in an environment characterized by varieties of mathematically manageable environments.

Pedagogically, then, one would begin by teaching first year students of the social sciences the elementary principles of solving Diophantine equations, immediately applying the concepts of solvability, by a digital computer, to an elementary supply-demand equilibrium in some well defined market. From there to the construction of 'the artificial intellect of the machine' is only a small step in this age and time.

But where does one's visions, as an Algorithmic Social Scientist, proceed – to which new frontiers? My immediate, perhaps somewhat unreflective, answer is unambiguous: towards an enrichment of the algorithmic mathematics of Brouwer, Weyl, Bishop, Church, Turing and Post with the phenomenology of Husserl. There, I believe, lies the fertile future symbiosis between *epistemology* and *methodology*, for the enrichment of the algorithmic social sciences.

It is a kind of enrichment recently articulated by Spiro Latsis, in his elegant acceptance lecture on the occasion of the award of an Honorary Doctorate at Witten-Herdecke University, [22]. The 'enrichment' I am seeking seems to lie in the interface between Brouwerian Intuitionistic Constructivism and Husserlian Phenomenology, where indeterminism must flourish. I believe a central core of the argument in *Encounters with Freedom*, [22], is the advocacy of the '*opaque landscapes*' vision for the social sciences:

"The 'opaque landscapes' in which we find ourselves are composed of intertwined physical, social, moral and psychological com-

ponents woven together into evolving artefacts. They are continually interacting with innumerable others. The difficulty for an agent to stand outside and prise out of them objective situational schemas is increasingly recognized and understood. I am using the expression 'opaque landscapes' simply to distinguish them from individualistic situational schemas and to suggest that they are not reducible to such schemas."

    ibid, p. 54

I believe the algorithmic approach in the social sciences gives numerical – mathematical – content to the 'difficulty for an agent to stand outside and prise out of [the 'opaque landscapes'] objective situational schemas. Husserlian phenomenology, I think, might provide the epistemological underpinnings for this algorithmic agent – 'an evolving artefact' – to recognise the situational dilemmas.

I cannot resist the temptation, therefore, of ending this paper with the pungent suggestion Harold Edwards made, at his June 18, 2009 lecture at the *Computability in Europe* conference, [6]:

> "I often hear mention of what must be 'thrown out' if one insists that **mathematics needs to be algorithmic**. What if one is throwing out error? Wouldn't that be a good thing rather than the bad thing the verb 'to throw out' insinuates? I personally am not prepared to argue that what is being thrown out is *error*, but I think one can make a very good case that a good deal of confusion and lack of clarity are being thrown out. .....
>
> How can anyone who is experienced in serious computation consider it important to conceive of the set of all real numbers as a mathematical 'object' that can in some way be 'constructed' using pure logic? .... Let us agree with Kronecker that it is best to express our mathematics in a way that is as free as possible from philosophical concepts. We might in the end find ourselves agreeing with him about **set theory**. It **is unnecessary**."
>
>   ibid, p. 14; bold emphases added.

What must be thrown out, if one insists that the social sciences need to be algorithmic, is partially achieved when it is realized the set theory is unnecessary. But there is a great deal of rubbish left, which needs to be thrown out, before the social sciences become truly algorithmic.

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