Complexity and Mechanism:

Toward a Unified Framework for Economic Science

A Thesis submitted for the degree of Master of Research (Philosophy)

By

Matthew Tuxford

Bachelor of Economics (Econometrics) – University of Newcastle

Graduate Diploma of Applied Finance and Investment – Financial Services Institute of Australasia

Master of Applied Finance (Investment Management) – Kaplan Professional

Graduate Diploma of Philosophy – Macquarie University

Macquarie University

Department of Philosophy

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Abstract

This research study undertakes a rational reconstruction of the methodological writings of key figures within the emerging school of thought known as complexity economics. The reconstruction is conducted within the framework of mechanistic explanation that has dominated the recent literature in the philosophy of science. My central thesis is that complexity economics offers a mechanistic explanatory framework appropriate for the creation, development, and revision of economic theories.

The study is comprised of three sections. The first section surveys the development of models of scientific explanation in the philosophy of science literature, culminating in an endorsement of a mechanistic account. The second section shows how the literature on economic methodology has historically responded to developments within the philosophy of science, and justifies the need for the analysis conducted in the following section. In the final section, I interpret the methodological writings of complexity economists within the mechanistic framework adopted, to establish the claims of the central thesis.

Declaration

I certify that the work in this thesis has not been submitted for a degree nor has it been submitted as part of requirement for a degree except as fully acknowledged within the text. I also certify that the thesis has been written by me. Any help that I have received in my research work and the presentation of the thesis itself has been acknowledged. I certify that all information sources and literature used are indicated in the thesis.

Matthew Tuxford

Introduction

Economics has long been referred to as the dismal science. In the aftermath of the recent global financial crisis, questions are once again being asked about the poor predictive track-record of economic forecasters, and various heterodox movements are reigniting well-trodden debates concerning mainstream methodological practices. It seems an appropriate time to reassess the philosophical basis of conventional practice.

This project is undertaken with the view that philosophers have useful insights to provide to scientists. In particular, it assumes that philosophers of science can have meaningful input into the methodological deliberations of economic scientists. It is in this spirit that I proposes to answer the question: what is an appropriate methodological framework for economic science? The primary thesis of this project is that, in contrast to mainstream practice, the methodological framework of *Complexity Economics* fits this bill.

In arriving at this ultimate conclusion, I proceed in three steps. Firstly, in chapter 1, I explore the literature on scientific explanation, to adopt a framework for use as a normative standard in the assessment of economic methodologies. I conclude that the currently dominant *mechanistic* model fulfils this requirement.

In chapter 2, I establish that economic methodology has traditionally responded to developments within the philosophy of science. I show however, that current mainstream practice has failed to adapt to contemporary advances. This conclusion is reached with the aid of the normative standard adopted in chapter 1.

Having demonstrated that an evolution in methodological practice is required in order for economic science to realign with up-to-date philosophical achievements, in chapter 3, I show that the emerging heterodox school of thought known as Complexity Economics espouses a methodological framework that fulfils just such a purpose. Herein lies the motivation for the title of this thesis. By adopting a *complexity* framework that is *mechanistic*, economic science has the potential to unify its disparate activities under a single overarching scientific framework.

Chapter 1: Philosophy of Science - Scientific Explanation & the Structure of Scientific Theories

Three of the central aims of scientific enterprise are the *explanation*, *prediction*, and *control* of phenomena. These aims are achieved through the practice of constructing theories and models. While it is an underlying assumption of this project that the three concepts are inextricably linked in many ways, such an attitude remains subject to dispute, as will be seen in both this chapter and the one to follow. Nevertheless, the focus of this project is on the concept of explanation, and how this relates to the structure of scientific theories.

Philosophers have long debated what structural features a theory should have in order to constitute a valid explanation, and so in this first chapter, I will draw upon this literature. The purpose of this chapter is to adopt an explanatory framework that can be used as both a descriptive perspective from which to reconstruct economic methodologies, and as a normative standard that can be applied to the methodologies of economic science. In this way, I aim to go some way to answering the question: what is an appropriate methodological framework for economic science? This chapter is designed to answer this question with the answer: a methodological framework that meets *mechanistic* standards.

This chapter is structured as follows. In Section 1.1, I present the historically dominant view of scientific explanation known as the Deductive-Nomological (DN) model, followed in Section 1.2 by a presentation of some of the key literature criticising this model, to show that it is not suitable for the purposes of this study. Next, in Section 1.3, I discuss some key features of several of the most prominent accounts of scientific explanation that have sprung up on the back of the failures of the DN model, and show why these are also not suitable for the purposes of this study. Then, in Section 1.4, I present the *mechanistic* model that will be adopted as a framework for the interpretation and appraisement of theories within the domain of economic science, in chapters 2 and 3, explaining why it represents both a compelling intellectual account, and a practical model for working scientists. In Section 1.5, I discuss some potential objections to the adoption of the mechanistic model of explanation for the purposes expressed. Finally, in Section 1.6, I summarise and reiterate the key points of Chapter 1.

1.1 The Deductive-Nomological Model

The dominant philosophical account of scientific explanation throughout the majority of the twentieth century, was the deductive-nomological (DN) model (also known - amongst other labels - as the hypothetico-deductive model, and the covering-law model). This account was championed by the logical positivist movement and took physics as its model science. The earliest published version of the DN model was by Carnap in 1923 [1], with an early, classic, re-statement published in 1948 [2]. Further comprehensive expositions by high profile philosophers were published by Carl Hempel [3], Richard Braithwaite [4] and Ernst Nagel [5].

Succinctly put, under the DN account, a theory *explains* a phenomenon by showing how it is expected to result from a set of particular circumstances in accordance with the laws of nature. The essence of the DN model, is that a scientific explanation takes the form of a logical deduction from *explanans* to *explanandum*, where the *explanadum* is a sentence describing the phenomenon to be explained, and the *explanans* contains a group of true sentences at least one of which states a law of nature acting as an essential premise. For a scientific theory to be considered a valid explanation, it was deemed necessary to conform to this structure. The DN model is designed to apply to both explanation of particular events and explanation of laws of nature, by more general laws.

Underlying the DN model is a Humean conception of causation [6]. David Hume's regularity theory of causation was designed to avoid problematic metaphysical notions. This strict empiricist account states that all we can really mean when we say that A causes B, is that our experience has shown A and B to be constantly conjoined. It will be shown in Section 1.3, how successive attempts at explicating the concept of explanation, have mostly centred on efforts to re-characterise the notion of causation, while attempting to remain broadly consistent with empiricist concerns.

While there are a number of important implications of the DN model, I'll mention here just two. These implications provide significant points of contrast with the mechanistic model of scientific explanation that will be introduced in Section 1.4 below.

Firstly, given the structure of the DN model, *explanation* and *prediction* constitute symmetrical concepts. They have exactly the same logical structure. The only difference between them is that explanations come after events, whereas predictions come before events.

A second important consequence is that a strictly reductive concept of explanation is implied, in which laws of nature are explained by reference to more general laws, with the consequence that ultimately, the most general law of nature discovered would constitute a “theory of everything”.

1.2 Criticisms of the DN Model

The DN model has faced criticism on a number of fronts. An early seminal piece of work cataloguing a broad range of substantial and technical issues, was published by Frederick Suppe [7]. The work grew out of a symposium held in 1969 that brought together the main proponents and critics of the traditional account at the time. Although it is beyond the scope of this paper to argue the case against the DN model, I will briefly outline three of the most prominent objections that have been recurrently raised in the literature: the symmetry objection, the irrelevance objection, and the appeal to laws objection.

The symmetry thesis has been an especially prominent target of criticism. Numerous counterexamples have been constructed to show that the DN model judges as valid, many instances of explanations that do not intuitively appear to be so, thus calling into question the sufficiency of the account. For example, there seems to be no problem with citing the height of a building as an element in an explanation for the length of its shadow. However, a symmetrical explanation that uses the length of a shadow as an element in an explanation of its height, although identical in structure, strikes one as absurd [8].

Another criticism targeting the sufficiency of the DN account of explanation, is the irrelevance objection. This objection relates to the situation where the law cited in the explanans is irrelevant to the explanation. As with the symmetry thesis, this objection has generated a number of counterexamples to illustrate the point. For example, the following patently absurd explanation meets the DN criteria for validity [9]:

P1: All batches of salt that have been hexed by a witch, dissolve when placed in water

P2: X is a batch of salt that has been hexed by a witch

C: X will dissolve when placed in water

This argument is valid under DN since, the explanandum C is logically entailed by the explanans P1 and P2, and the explanans contains a premise – P1 – that contains a universal generalisation acting as an essential premise.

A third common objection relates to the insistence for an appeal to the laws of nature in the model. Philosophers such as James Woodward, have pointed out that without a clear explication of the concept of *laws*, it is hard to accept that they are required for legitimate explanations [10]. The DN model, with its Humean conception of causation, which views laws simply as universal regularities, has trouble distinguishing between genuine laws and accidental regularities.

1.3 Alternative Accounts

A number of alternative proposals have been generated in the wake of the failures of the DN model. In this sub-section, I will briefly introduce seven of these models. My purpose here is to explore how different notions of causation, and different ways of dealing with explanatory relevance, are central to the development of these models. It will also become evident how the various authors tend to vacillate between descriptive and prescriptive modes in their delivery.

1.3.1 Statistical Relevance Model

The first account I'll discuss is Wesley Salmon's Statistical Relevance (SR) model [11]. This model is motivated by the idea that the DN model provides necessary but not sufficient conditions for valid explanation. Sufficiency is sought by incorporating a notion of causation that appeals to statistical relevance relationships. The intended result is the exclusion of irrelevant information from valid explanations. This form of causal account is in keeping with the metaphysically sparse Humean notion underlying the DN model. However as opposed to the DN model, where valid explanations possess an argument form, the structure of the SR model contains a body of information that is statistically relevant to the explanandum.

The notion of statistical relevance is captured by means of conditional probabilities. Specifically, in a population A, an attribute C is considered statistically relevant to another attribute B, if: $P(x=B|A.C) \neq P(x=B|A)$. This states that the probability that x, a member of the population A, has the attribute B, depends on whether x also has attribute C, so that C is statistically relevant to B.

The SR model incorporates the relevant explanatory factors by means of a *homogenous partition* – a mutually exclusive and exhaustive division of all the explanatory factors into subsets Ci, where $P(x=B|A.Ci) \neq P(x=B|A.Cj)$ for all $Ci \neq Cj$.

An explanation according to the SR model is a linguistic entity – a set of statements, as is the case under the DN model – that constitutes an answer to the question: *Why does this x, which is a member of A, have the property B?* Such answers are said to have the following form [12]:

1. A statement of the unconditional probability of an event for some class of factors A: $P(x=B|A)=p$
2. A set of conditional probability statements $P(x=B|A.Ci) = pi$, for a homogenous partition of A with respect to B: (A.C1,…,A.Cn)
3. A statement of which cell of the partition contains x

Given the structure of the SR model, it can be seen that it has the unintuitive consequence that the same explanans can be capable of explaining both X and not X. The SR account then, is incapable of distinguishing between the causal relationships that are actually operative in the generation of the phenomena to be explained. I take this fact to indicate that the SR model does not provide an adequate basis on which to develop normative standards for the generation and development of scientific theories.

1.3.2 Causal Mechanical Model

After failing to adequately respond to a series of severe criticisms, Wesley Salmon eventually abandoned the SR model. In its place, he went on to construct the

Causal Mechanical (CM) model [13], as an alternative way of accounting for relevant causal relationships. Although it was explicitly intended not to, this approach arguably constitutes a rejection of the strict Humean, constant conjunction account of causation, since it appears to replace the epistemic notion of causation underlying the SR account, with one based on causal realism. The notion of causality underlying the CM model is one that construes the concept as being a feature of continuous processes, rather than as a relation between events. The two central notions deployed in the model are those of *causal process* and *causal interaction*. Together, these notions provide the concept of a *causal mechanism*. A causal mechanism is characterised as a sequence of events or conditions, governed by law-like regularities. Salmon explains the centrality of causal mechanism to his account of explanation when he states:

"Causal processes, causal interactions, and causal laws provide the mechanisms by which the world works; to understand why certain things happen, we need to see how they are produced by these mechanisms." (Salmon, 1984, p.132)

This idea is also a key motivating feature of the mechanistic model presented, and adopted, in Section 1.4 below.

So, how does Salmon cash out the notions of causal process and causal interaction? A causal process is said to be a continuous physical process, characterised by consistency of structure over time. The process must be capable of transmitting a *mark* that is introduced at a spatiotemporal location. That is, once a mark is introduced, it persists to other spatiotemporal locations even in the absence of any further interaction. A causal interaction involves a spatiotemporal intersection between two causal processes, whereby the structures of both are modified.

An explanation of an event under the CM is a case of showing how the event fits into a *causal nexus*. This is achieved by citing *etiological* and *constitutive* features of the event. The etiological condition is achieved by citing the causal processes and interactions preceding the event, and the constitutive aspect is satisfied by citing the processes and interactions that comprise the event.

A major problem with this model, is that it is not capable of discriminating between aspects of the causal processes and interactions cited that are explanatorily relevant and those that are not. It must therefore be considered along with SR as a failed attempt to address explanatory relevance. Conceding that this is the case, Salmon has subsequently suggested that a full account of explanation would need to incorporate the underlying ideas of both the SR and CM accounts [14].

The CM model has inspired other accounts, collectively known as process theories of causation [15]. These models are beset with the same types of issues that riddled Salmon's original CM model, and so as is the case with the SR and CM models, these accounts do not provide an adequate basis on which to derive normative standards for the generation and development of scientific theories. However, as mentioned above, embedded in the idea of a causal process, is a mechanical notion that will be important for the mechanistic model adopted in Section 1.4 below.

1.3.3 Unification Model

Unificationist accounts of scientific explanation view such explanations as attempts at gathering various different phenomena into unified accounts. Like the models outlined above, these aim to remain faithful to a Humean conception of causation. Michael Friedman [16] provided an early exposition of the unificationist idea, but the Unification (U) model has been most influentially developed by Philip Kitcher [17].

In Kitcher's U model, a valid explanation is one that can be derived from the set of *argument patterns* that maximally unifies the set of beliefs accepted at a particular time by the scientific community. The maximal unification is the optimal combination of the attributes: generality, simplicity, and cohesion. This set of argument patterns is called the *explanatory store*. To show how the explanatory store is constructed, I'll briefly introduce some of Kitcher's technical machinery.

A *schematic sentence*, is a sentence which has had some non-logical vocabulary replaced with dummy letters. *Filling instructions* provide direction for filling in the dummy letters in schematic sentences. *Schematic arguments* are chains of schematic sentences. *Classifications* provide rules of inference and designate schematic sentences as premises and/or conclusions.

An *argument pattern* is constructed by combining all the elements above. They are constituted by a schematic argument, a set of filling instructions for each term of the schematic argument, and a classification. An argument pattern is said to be more *stringent* to the degree that it imposes restrictions on its instantiating arguments. The unification process that provides valid explanations can be characterised as one in which different phenomena are collected under as few and as stringent argument patterns as possible.

The U model has been subjected to many criticisms. One major criticism is the contention that it fails to provide an account that is not merely descriptive, since the guiding principle seems to be one of descriptive economy. Another major criticism, is that the U model classifies explanations as either completely valid, or completely invalid; there is no facilitation of the idea that an explanation can be less explanatory than a competing explanation, but nevertheless still be considered explanatory. Given these characteristics, the U model does not appear to provide an adequate descriptive account of scientific explanation, let alone a basis on which to build a normative standard for the generation and development of scientific theories.

1.3.4 Constructive Empiricism Model

Bas van Frassen has argued that explanation is not an aim of pure science; the only aim is the construction of theories that provide accurate descriptions of observables [18]. Instead, he considers explanation to be merely a pragmatic virtue of theories. Van Frassen rejects the logical structure of the DN model, in which explanations are captured in the relation of premises to conclusions. In his Constructive Empiricism (CE) model, the logical structure is construed as having a pragmatic relation of questions to answers, and has been developed to specifically address the structure of *why* questions and answers. The only difference between scientific explanations and ordinary everyday explanations under CE, is that the former include scientific information. The CE model is an anti-realist account that draws on Bayesian interpretations of probability.

Under the CE model, *why* questions are construed as having two features. Firstly, the question is explicated as having the form: why the explanandum E obtained *rather than* any other of the possible alternatives. These other possibilities are collectively referred to as the *contrast set* X. Secondly, some *relevance relation* R is assumed to be implicitly contained within the question. The relevance relation is defined by the interests of the questioner in posing the question. In this way, the CE model aims to constrain the space of possible explanations to exclude those that are explanatorily irrelevant.

Answers to *why* questions (explanations) take the form: E in contrast to X because A, where A bears the relevance relation R to [E,X]. According to van Frassen, the main problem with prior accounts of explanation is that they had been conceived as two-term relations between theories and facts, whereas an adequate account in his view would have to view explanation as a three-term relation between theories, facts and contexts [19]. Van Frassen's pragmatic account of explanation, under CE, is deeply subjectivist, since what constitutes a valid explanation for one person need not do so for another.

One devastating objection that has been raised against the CE model, is that the relevance relation R is completely unconstrained [20]. The consequence of this is that for any case where an event E and an answer A are true propositions, there exists a relevance relation R such that A explains E. The CE model thus appears to provide a rather trivial account of scientific explanation, and certainly not one that could be adopted as a normative standard.

1.3.5 Illocutionary Act Model

Like the CE model, the Illocutionary Act (IA) model is a pragmatic account of scientific explanation designed as a general model of explanation, focusing on the intention of the explainer to make information understandable. The IA model however is broader than the CE model, in that it is intended to account for all manner of explanatory cases, not just *why* questions. Also, the IA model represents a rejection of the causal approach aimed at explicating the logical structure of explanations. Instead, it provides an account of the *process* of explanation as a communicative act. This model was developed by Peter Achinstein [21].

Under IA, explanation is conceived as an ordered pair containing:

1. An act type; and
2. A proposition providing an answer to a question, Q

According to Achinstein, an individual S, explains Q, by uttering U, if and only if, S utters U with the intention that the utterance of U render Q understandable, by producing the knowledge, of the proposition expressed by U, that is a correct answer to Q [22].

In place of the notion of a *valid* explanation, Achinstein distinguishes between *correct* and *appropriate* explanations. A correct explanation is one that is true, whereas to be considered appropriate, it must conform to certain *instructions*, which are intended to capture the background knowledge, beliefs and expectations of the intended audience. The criteria of correctness and appropriateness are independent, in that an explanation can be true without being appropriate, and also appropriate without being true. By appealing to the truth conditions of the proposition expressed by U, Achinstein avoids the subjectivism inherent in van Frassen's CE model.

The traditional approaches to explicating the concept of scientific explanation are intended to provide ideal standards that scientists should aspire to satisfy. The IA model denies that there are any universal criteria for the construction of explanations for all contexts and audiences, or indeed even for narrower individual domains such as scientific contexts. It is not surprising then that it does not appear possible to redeem the IA model in order to provide for such a usage.

1.3.6 Difference-Making Model

The Difference-Making (DM) model is a causal account of scientific explanation associated with James Woodward [23]. The DM model is built upon a manipulationist account of causation. Under the manipulationist account, what distinguishes causation from mere correlation, is information concerning manipulability. Facts about manipulability are treated as metaphysically prior to facts about causation. Under the DM model, explanations appeal to a notion of causation characterised as: systematic patterns of counterfactual dependencies related to interventions. Explanations are explanatory because they contain information that can be used to answer a range of *what if things had been different* questions. In this way, the space of valid explanations is constrained so as to screen out explanatorily irrelevant information.

Woodward tells us that:

"…explanatory relationships are relationships that *in principle* can be used for manipulation and control in the sense that they tell us how certain (explanandum) variables would change if other (explanans) variables were to be changed or manipulated." (Woodward, 2000, p.198)

Woodward's manipulationist account rejects the notion of *lawfulness* in favour of that of *invariance*. Invariant generalisations, unlike laws, may have exceptions outside of limited domains, and can come in degrees. The account of invariance is built upon the notion of intervention, which Woodward characterises as an idealised experimental manipulation. The idea is that there must exist some interventions for variables figuring in the relationship, under which the generalisation would continue to hold.

The DM model contains three core elements: a theory of type causation; a theory of singular causation; and a theory of event explanation. Type level causal relations provide the metaphysical basis for causal explanation by determining the facts about singular explanation. They determine the possible causal pathways. This is a theory that construes causation as a relation between types. It is only a theory about the relation between particulars in a derivative sense. Woodward uses the term *variable* to refer to a type. A variable X is a direct cause of another variable Y, relative to a variable set V, just in case there is an intervention on X that will change the value of Y when all variables in V except X and Y are held fixed [24].

The theory of singular causation provides an algorithm to test for counterfactual dependence. The test is not simply one of a single event, but also involves information about the causal path determined by the higher level type relations. It is also couched in manipulationist terms. An event is rendered a cause of the explanandum via the designated path, in the case where the explanandum occurs when the event is activated, but does not occur when the event is deactivated. The events in all other causal paths are held fixed at their actual values.

According to the DM theory of event explanation, some explanations can be better that others, because they convey more manipulatory information. The best explanation for an event E, will not only contain information about the actual causal path of E, but also information pertaining to how E *might* have been caused.

Two serious issues have been raised to question the adequacy of the DM model. Firstly, it is not clear that Woodward manages to escape vicious circularity in his explication of the concept of causation. For the definition of causation requires the concept of intervention, which itself seems to presuppose the notion of causation. One way of arguing this point is to see that in order to distinguish a genuine intervention on X relative to V from a mere manipulation, one needs to have knowledge of the causal pathways connecting the elements of V. But this is to presuppose the information sought for [25].

A second issue has to do with the way that causation is relativised to a variable set V. The causal pathways determined by the type level causal relations are dependent on the set V chosen. But surely our notions of explanation are not relativised in such a way?

Besides these technical issues, the DM model does not seem to provide guidance for the development of explanatory theories, nor is it obvious how it could be implemented – at least without supplementation - as a normative test for explanatory validity. However, the idea of manipulability will be seen to be important for the mechanistic model that will be adopted in Section 1.4 below.

1.3.7 Kairetic Model

The Kairetic (K) model was developed by Michael Strevens [26]. The K model is an attempt to appropriate the technical apparatus of the U account to derive a realist causal model. Strevens strives to analyse explanation in an ontological sense. He contends that explanation is: "something out in the world, a set of facts to be discovered" (Strevens, 2008, p.6), so that explanatory facts are prior to causal claims. In taking such a stance, Strevens can be seen as making a rather minimal metaphysical commitment to causal relations. His *two-factor* theory emphasises the difference between causation and causal explanation. Explanation is viewed as a process of selecting from the totality of causal influences, those that are explanatorily relevant to understanding a phenomenon. To this end, Strevens takes a difference-making approach to screen out the explanatorily irrelevant causal influences. He rejects the two most prominent accounts of difference-making in favour of one derived by himself.

The first traditional approach to difference-making he rejects is the probabilistic account most famously associated with Wesley Salmon (see 1.3.1 above). In this approach, as was shown, C is said to have made a difference to an event E, if it is shown to have changed the probability of E. The problem Strevens identifies with this approach, is that while it is good at identifying the types of factors that typically act as difference-makers, it is incapable of attributing these factors in individual cases.

The second account of difference-making Strevens take issue with, is the counterfactual approach he identifies with David Lewis [27] and James Woodward (see 1.3.6 above). The counterfactual criterion states that:

"a causal influence C on an event E counts as having made a difference to whether or not E occurred just in the case, had C not occurred, E would not have occurred" (Strevens 2004, p.161).

Strevens cities the *pre-emption problem* [28] and argues that attempted solutions should be considered failures [29].

Strevens presents an alternative perspective on difference-making. He devises a process to extract a set of difference-makers from any veridical causal model for an explanandum event E, where such a model is comprised of a set of true statements that causally entails E. The process starts with a deterministic model for E, which represents the causal processes by which E was produced. As many abstractions as possible are made to the features of the model, with the condition that the model remain deterministic. The abstracted veridical model that optimises for generality, cohesion and accuracy is called an *explanatory kernel* for E. This deterministic model is claimed to contain only difference-makers. An explanatory kernel for an event E constitutes a full explanation of it. Individual statements are considered partial explanations of E, if they are members of some explanatory kernel for E.

The K model is an innovative approach that combines elements of the U, CM and DM models. It incorporates: the cohesion criteria of the U model within the abstraction process; the appeal to causal mechanisms of the CM model; and, although not referred to above due to space limitations, nods in the direction of the counterfactual dependence approach of the DM model in the exposition of *entanglement* as an explanatory relevance relation in the explanation of generalisations [30]. It also exhibits a pragmatic dimension through the concept of *frameworked* explanation [31].

But, although Strevens provides an intellectually compelling case that incorporates many of the best elements of prior accounts, his K model provides a rather abstract account of theoretical development, which arguably provides little in the way of practical benefit for working scientists.

1.4 The Mechanistic Model

In recent decades, a new mechanistic philosophy has generated a lot of attention in the literature, to the point that it has been described as:

"the dominant view of explanation in the philosophy of science at present" (Kaplan & Craver, 2011, p.606).

Derived primarily from actual practice within the life sciences - where practitioners rarely appeal to laws in their explanations - this model challenges the *received* *view* represented by the DN model and offers a compelling alternative to the major successors to the DN account outlined in Section 1.3 above. Simply put, a mechanistic explanation of a phenomenon, is one that describes a model of a mechanism thought responsible for the generation of the phenomenon.

The mechanistic model draws heavily on the concept of a causal mechanism from Salmon's CM account (see Section 1.3.2 above). It also takes inspiration from the ideas underpinning Woodard's DM model, including the notion of manipulation and the rejection of lawfulness in favour of invariance (see Section 1.3.6 above). It combines all of these elements in such a way that not only constitutes a compelling intellectual solution to the problem of explicating the concept of scientific explanation within the philosophy of science, but also has the further advantage of being capable of providing pragmatic guidance for practicing scientists in the construction, evaluation and revision of scientific models.

Proponents of the mechanistic model accuse the DN model and its traditional successors of failing to provide an account that moves beyond mere phenomenal description, and therefore failing to meet the cognitive requirements for explanation. Mechanistic explanations, on the other hand, are said to be constitutive, in that they go beyond mere descriptions of phenomena; they explain why the relationships featuring in descriptions of phenomena are as they are [32].

Although several definitions of *mechanism* have been proposed in the literature [33], the central features of the mechanistic approach are broadly consistent across the major works of the most prolific authors in this space. The following two prominent definitions are typical:

> "A mechanism is a structure performing a function in virtue of its component parts, component operations, and their organisation. The orchestrated functioning of the mechanism is responsible for one or more phenomena." (Bechtel & Abrahamsen, 2005, p.423).

> "Mechanisms are entities and activities organised such that they are productive of regular changes from start or set-up to finish or termination conditions." (Machamer, Darden & Craver, 2000, p.2).

According to Craver, the DN and other models of explanation are pitched too abstractly to capture recurrent non-formal patterns. Mechanism schemata on the other hand, are claimed to be capable of successfully capturing such diverse phenomena.

The mechanistic model requires support for a realist worldview, since the models of mechanisms featuring in mechanistic explanations are considered to represent real entities and activities in the world [34, 35]. Also, mechanistic accounts presuppose a fuller account of causation than the metaphysically sparse Humean regularity conception that the DN model is based upon [36].

David Kaplan and Carl Craver, in the context of cognitive and systems neuroscience, provide a model-to-mechanism-mapping (3M) requirement that provides an initial strong constraint on what can constitute a valid mechanistic explanation:

"(a) the variables in the model correspond to components, activities, properties, and organisational features of the target mechanism that produces, maintains, or underlies the phenomenon, and (b) the (perhaps mathematical) dependencies posited among these variables in the model correspond to the (perhaps quantifiable) causal relations among the components of the target mechanism." (Kaplan & Craver, 2011, p.611).

Craver further provides a non-exhaustive checklist of items that can be used to assess mechanistic explanations [37]. This checklist is useful for exercises such as that conducted in Section 3, wherein the writings of complexity economists are assessed for their adherence to mechanistic standards. However the value of such devices is much greater in the evaluation of specific model propositions. The checklist is organised around the idea of manipulability stemming from the work of James Woodward, and is arranged into the categories of the explanandum phenomenon, and the parts, activities, and organisation of the mechanism. I'll outline the key requirements now.

Craver notes that the central normative requirement of a mechanistic explanation is that it account completely for the explanandum phenomenon. He provides five criteria for establishing that this criteria is met. Firstly, the range of *precipitating conditions* should be noted, secondly, *inhibiting conditions* should be noted along with an account of why the phenomena are not produced under these conditions. Thirdly, *modulating conditions* that note how changes in background conditions alter the phenomenon should also be included. Fourthly, a complete characterisation would incorporate an account of how the mechanism behaves under *non-standard conditions*. Fifthly, any *by-products* – features that are of no functional significance for the phenomenon - of the mechanism should be noted.

Craver contends that valid mechanistic explanations feature real components, as opposed to fictional posits. He provides five criteria for making this distinction.

Firstly, they are expected to exhibit a *stable cluster of properties*. Secondly, they should be *robust*, that is, they should be detectable by a number of independent causal and theoretical devices. Thirdly, we should be able to use them to *intervene* into other components and activities. Fourthly, they should be *plausible-in-the-circumstances*, that is, they should be demonstrable under the conditions relevant to the context of explanation. Fifthly, they must be *relevant* to the phenomenon to be explained.

Activities – the things that entities do – are the causal components of mechanisms. A mechanistic explanation that treats activities in mechanisms merely as input-output pairs is considered unsatisfactory. And adding the stipulation that the input-output pairs must support counterfactuals will not be sufficient, since not all counterfactual supporting generalisations are explanatory. This leads Craver to endorse a manipulationist criteria as a means of restricting the type of input-output relationships that can count as explanatory [38].

Mechanistic explanations are not aggregative, and so a valid mechanistic explanation must make recourse the organisational characteristics of the proposed mechanism. The relevant organisational features may be spatial, temporal or hierarchical.

In the remainder of this subsection, I will briefly state some of the ways in which the mechanistic model of explanation relates to other key concepts in the philosophy of science, and how these relations contrast with the standard views embedded in the DN model. The four concepts addressed are: *inference*; *discovery*; *testing*; and *reduction*.

Firstly, under the mechanistic model, *inference* making often involves processes of simulation - including mental animation and building scale models (physical, mathematical, computer, etc) - that utilise a variety of representational devices. In contrast, the DN model provides for only linguistic representations and deductive inference [39].

Secondly, the mechanistic model provides an account of scientific discovery and development, unlike the DN model. Bechtel and Abrahamsen show that the very definition of *mechanism* suggests that scientific discovery is a process of unearthing the components, operations, and organisation of the phenomenon to be explained [40]. This has been described as a process of *decomposition*, at both the structural – finding component *working parts* - and functional – finding lower-level operations – levels. The *working parts* of the structural decomposition are those that perform the operations of the functional decomposition. These two decompositions can be conducted independently, followed by a process of *localisation*, in which the parts and operations are linked and their organisation uncovered [41]. In contrast, under the DN model, where the goal of discovery is simply the articulation of laws, scientists are left without guidance. In fact, in emphasising the separation of the contexts of *discovery* and *justification*, early logical positivists considered the process of discovery to be an issue to be pursued by the science of psychology [42].

Craver also fleshes out an account of how the concept of a mechanistic explanation provides guidance for the development of scientific research programs. He suggests that models of mechanisms can be thought of as lying on a continuum between a *mechanism sketch* and an *ideally complete* model [43]. Scientific research programs can then be considered as platforms for moving along this continuum. Explanations, in so far as they provide answers to "why?" questions, presuppose conversational contexts, and it is these contexts that determine the level of abstraction required of the answers, and thus where upon the continuum the appropriate mechanism description for a particular application lies. Craver also provides another way to think about the development of scientific explanations. In the same paper, he defines a continuum between *how-possibly* models and *how-actually* models, within which *how-plausibly* models lie. Once again, scientific research programs can be considered as platforms for moving along this continuum.

Thirdly, Bechtel and Abrahamsen point out that the mechanistic model also has advantages over the DN model in relation to the *testing* of theories. They show that while both models suffer from issues with under-determination and credit-assignment, tests of proposed mechanism sketches can provide diagnostic information useful for revision and further testing [44].

Fourthly, with regard to reduction, mechanisms are said to exist in nested hierarchies. The entities featuring in a given mechanistic explanation may themselves be mechanisms. But as noted above, phenomena cannot simply be explained by appealing to the phenomena generated by their constituent mechanisms, but must appeal to the organisational characteristics of the entities and activities constituting the mechanism under consideration. In this way, although in one sense it can be said that mechanisms can be reduced to their sub-mechanisms, the autonomy of separate disciplines are maintained in the face of such reductionism. In contrast, under the DN model, theory reduction occurs when a law is subsumed under a more general law, bringing into question the genuine autonomy of the various branches in the hierarchy of scientific disciplines.

1.5 Issues with the Mechanistic Model

I will now discuss two issues that may be considered problematic for the adoption of the mechanistic model of scientific explanation as a normative standard for economics. The first relates to an objection that has been raised against the M model on its own terms, in the domains of its intended application. The second, is a broader and more fundamental issue for this thesis.

1.5.1 Challenges to the Mechanistic Model

In the recent literature, there has been an objection raised to the Mechanistic model [45]. The objection applies to all accounts of scientific explanation that deny the validity of non-veridical models. The claim is that there exists a class of models that are designed to account for common features exhibited by systems whose underlying details are vastly different, and are thus deemed explanatorily irrelevant for the purpose.

This idea stems from an earlier work by Batterman [46], in which he distinguishes between two different types of *why* questions that feature in scientific explanations. The first type, which he classifies as type i why-questions, relate to the explanation of singular phenomena. The second type, classified as type ii why-questions, relate to explanations of why certain phenomena occur more generally, that is, why the phenomena is manifested in a number of different circumstances. It is claimed that for type i why-questions, veridical accounts such as the M model provide appropriate conditions for successful explanation. On the other hand, type ii why-questions it is argued, require abstraction and a deliberate distortion of the underlying details for the rendering of a successful explanation.

It is beyond the scope of this paper to provide a rebuttal on behalf of the M account. However, I will briefly note two directions in which I believe such a rebuttal could be formulated. Firstly, it could be argued that the purported explanations for type ii why-questions championed by Batterman and his followers, do not actually provide explanations, but are merely descriptions, themselves in need of explanation. A valid explanation for such questions would make recourse to the underlying mechanisms, pointing toward general patterns in the organisation of mechanisms.

A second possible response could make use of the distinction made by Craver, between *ideally complete* models and *pragmatically complete* models [47]. In this way, it can be argued that a mechanism sketch could be produced at an appropriate level of abstraction for the task at hand, without introducing non-veridical representational devices

1.5.2 Methodological Monism and Mechanistic Explanation

A key assumption underlying this research thesis is the doctrine of methodological monism. This doctrine expresses the belief that there is a single methodological framework at some level of abstraction that provides a normative standard for all disciplines that aspire to the label of *scientific*. As developed, the DN model was explicitly intended to provide a universal normative standard, and as will be shown in Chapter 2, a number of its adherents enthusiastically embraced the model as a standard for economic theory development.

Kaplan and Craver leave it as an open question whether the M model is capable of providing a normative standard for all of science, when they state that:

"There might be domains of science in which mechanistic explanation is inappropriate." (Kaplan & Craver, 2011).

I interpret this statement as an optimistic challenge to adherents of the M model to help establish that this hypothesised possibility is not actually the case. It is in the spirit of this challenge that this thesis gains its motivation. But in fact, Craver & Alexandrova explicitly accommodate such a goal for economic science:

"Suppose that the goals of economics are prediction, explanation, and control. These goals are achieved better when economics aims at the discovery of mechanisms that underlie economic phenomena than when it aims merely at instrumental "as if" models." (Craver & Alexandrova, 2008, p.386)

1.6 Concluding Remarks

In summary, the literature on the philosophy of science has a long tradition of attempts at explicating the concepts associated with scientific theories. In particular, the concept of *explanation* has been deeply investigated, with the development of various models aimed at providing both descriptive and normative accounts. While the DN model represented the *received view* for much of the twentieth century, its most glaring shortcomings drove the development of a succession of alternatives. The mechanistic model is one of these alternatives, and constitutes the dominant contemporary position. This model has many advantages over its rivals. Of particular importance is its ability to provide practical guidance to working scientists in the discovery and development of theoretical constructs.

Given its status as an up-to-date model of scientific explanation, and its demonstrated advantages over its rivals, I deem it appropriate to adopt the mechanistic model as a normative standard to be applied in the appraisement of economic methodological frameworks. In Chapter 2, it will be utilised as a contrast against both historical and contemporary mainstream practice. And In Chapter 3, it will be put to its ultimate use: the validation of the methodological framework of Complexity Economics.

Chapter 2: Methodology of Economics – History & Connections to Philosophy of Science

This research project aims to answer the question: what is an appropriate methodological framework for economic science? In Chapter 1, I suggested that the mechanistic model of scientific explanation and theory structure developed by Craver and others, represents an up-to-date account of scientific explanation that could be appropriately applied to the science of economics. The purpose of this chapter is twofold. Firstly, I aim to show how historically, methodological positions within economic science have responded to developments within the philosophy of science. My second aim is one of justification. By showing how and where mainstream economics has failed to meet a mechanistic normative standard, I hope to motivate the analysis of Chapter 3, wherein I seek to validate the framework of Complexity Economics, by showing that it does meet the adopted standard.

This chapter is structured as follows. I commence in Section 2.1 with a discussion of the relevance of the philosophy of science for economic methodology, in which I introduce a heuristic device to be used throughout the chapter to summarise the philosophical commitments of various schools of thought, and to assess them for adherence with mechanistic standards. Then, in Section 2.2, I present the methodological commitments of the nineteenth century economists - and their a priorist descendants - who worked to establish economics as a distinctive scientific discipline. It will be shown that their formal a priori commitments fail to adhere to mechanistic standards. Next, in Sections 2.3 and 2.4, I show, respectively, that both the modern Austrian School's commitment to a priorism, and the Institutionalist School's explanatory framework of pattern modelling, violate mechanistic standards. In Section 2.6, I show how the logical positivist movement, with their development of the DN model introduced in Section 1.1, impacted the development of methodological views within the economics profession, and reiterate the message of Chapter 1, that the associated commitment to formal deduction and universal laws, falls short of mechanistic standards. In Section 2.7, I describe how the developments discussed in previous sections have evolved into the current methodological landscape. I conclude in Section 2.8, with a summary of this chapter's key argument: that although economic methodology has historically adapted to the evolving standards developed within the philosophy of science literature, the mechanistic standards that dominate this literature in the present day have not been adopted into current practice.

2.1 Is Philosophy of Science Relevant for Economic Methodologists?

Concluding his review of two books on the methodology of economics by philosophers of science [1], Scott Gordon states:

> "The answer to the title question of this essay, "Should economists pay attention to philosophers?" is, I think, Not much…That mythical creature, the economist qua economist, need not pay much attention to philosophy, good or bad, but the philosopher of science had better pay attention to economics, good and bad." (Gordon, 1978, p.728)

Echoing the sentiments expressed by Gordon, Deirdre McCloskey claims that there are no methodological standards that economic science must meet; the normative prognostications of philosophers can safely be ignored [2]. This position is further supported by Wade Hands [3], and Bruce Caldwell [4].

Daniel Hausman, continues this tradition, when he claims:

"If one goes to contemporary philosophy of science in search of hard and fast rules for assessing theories in the light of data, one will be disappointed." (Hausman, 2008, p.18)

However he does go on to make the concession that:

"Philosophy of science has many insights to offer, and those who do not take it seriously are doomed to repeat its past mistakes." (Hausman, 2008, p.22)

Despite the general tone of pessimism here, the normative suggestions made in this study, if correct, ought to be of benefit and interest to both philosophers and economists alike.

Daniel Hausman claims there are five broad groupings of concerns within traditional philosophy of science that are relevant to economic science [5]. These he identifies as:

1. *Goals*: what are the goals of scientific theorising?
2. *Explanation*: what is a scientific explanation?

3. *Theories*: How are theories constructed? How does one choose between competing theories?
4. *Testing*: How are theories tested?
5. *Methodological monism*

Throughout this chapter, I'll make use of these five concerns as a heuristic tool for clarifying the commitments of the various schools of thought discussed. To demonstrate this, I will now apply the tool to the mechanistic model endorsed in Chapter 1. These commitments will serve as a contrast set for analysing the mechanistic credentials of the various methodological views under consideration.

Goals

The goals central to scientific enquiry are explanation, prediction and control. The goals of prediction and control are best served with reference to a realistic explanatory theory, so that the three goals are inextricably linked.

Explanation

To explain a phenomenon is to describe a representational model of the mechanisms thought to produce it.

Theories

Theories are constructed out of models of mechanisms. Theory selection is based upon the extent to which the details of the competing mechanism schemas have been filled in.

Testing

Rigorous empirical testing is required for the validation of theories.

Methodological Monism

As far as explanation is a primary goal of all scientific disciplines, a mechanistic approach provides an appropriate methodological framework for them all.

2.2 The a priorists [6]

Explanation of economic phenomena was considered the primary task of nineteenth century economic science. While predictive capability was also sometimes acknowledged as an implication of successful explanation - and indeed must be considered a presupposition of policy advocacy - predictive power was not a major consideration. For these early theorists, what marked the young discipline of economics as a science, was the certainty of its conclusions, not the certainty of its predictions [7].

In this sub-section, I briefly trace the evolution of a priori economic theorising, from its roots in nineteenth century classical thinkers. Despite the close similarities in methodological approach, a divergent range of perspectives on some key issues - including methodological monism – is revealed. It will be shown that the a priori approach violates mechanistic standards

The Classical Approach

The Nineteenth century economic theorists focused their attention on the premises of economic theories, which were derived from introspection and taken to be either *a priori* truths, or simplifying assumptions approximating truths. Their theorising commenced with these premises, and through chains of inference, implications were established [8]. These implications however, were expected to be borne out only in the absence of disturbing causes, and because of this, it was not considered appropriate, or indeed possible, to subject them to empirical test.

The *a priori* position was prominently restated by Lionel Robbins in the early 1930s [9], given its most extreme exposition by Ludwig von Mises in the 1930s and 1940s [10], reiterated by Frank Knight [11] and lives on to this day, in its most extreme form, with the modern Austrian School, having been championed by Murray Rothbard [12], and Hans Herman Hoppe [13] (see Section 2.3 below).

2.2.1 Nassau Senior

Nassau Senior was the first to explicitly outline the methodological principles of the early classical school [14]. The theoretical branch of political economy, according to Senior, aims to explain the nature, production and distribution of wealth. It proceeds to conclusions by way of deduction from fundamental propositions. These fundamental propositions are said to represent incontrovertible facts. Theories are created by means of logical argumentation from the fundamental propositions combined with assumptions, which act to specify the domain of the theory.

The chief fundamental proposition, which Senior claimed, is as fundamental to Political Economy as gravitation is to Physics, was stated as:

P1: “That every person is desirous to obtain, with as little sacrifice as possible, as much as possible of the articles of wealth” (Senior, 1827, p.30)

The other three axioms of the system are:

P2: The Malthusian population principle: that global population is limited only by fear of a deficiency of the articles of wealth that class habits condition individuals to require;

P3: The productivity of capital; and

P4: Diminishing returns to agriculture.

2.2.2 John Stuart Mill

Mill considered the fundamental proposition of political economy to be a psychological law, and reframed it as:

P1: a greater gain is preferred to a smaller one.

But, according to Mill, this is only one psychological motive among many. The goal of political economy he tells us, is to abstract away from all other motives, to determine outcomes that would be applicable in the absence of all other motives. As such one could not expect the conclusions of economic theorising to ever be borne out precisely in the real world; they are only true in the abstract. These conclusions are simply more or less applicable, depending on the extent to which P1 is mixed with *disturbing causes*. Whereas Senior argued that the fundamental postulates of economics are true, Mill argued that they are partially true.

Although comparisons of theoretical conclusions with empirical reality were considered unable to falsify a theory, there was a place in the Millian system for such a posteriori investigations. It was considered that such tests could possibly detect the presence of intervening factors, which it may be possible to subsequently bring within the scope of the theory.

Mill's methodology was grounded in his philosophy of science [15]. He is well known for his conformational rules of induction: agreement, difference, residues, and concomitant variations. Although Mill promoted methodological monism, he also argued that these four methods for the discovery and confirmation of universal causal laws are not appropriate for the social sciences. Since phenomena in the social sphere are experienced as vast complexes of effects, and controlled experimentation is impossible, Mill endorsed the *abstract a priori* method. Mill however, arguing against Kant, flatly rejected all notions of synthetic a prior propositions [16]. These, at least seemingly, contradictory views leave one somewhat unsure as to Mill's ultimate methodological position. On this note, Blaug claims that Mill's writing:

"…is well calculated to leave the reader utterly confused about Mill's final views in the philosophy of the social sciences." (Blaug, 1980, p.64).

And more extremely, Rothbard declares:

"Mill's ever-expanding intellectual 'synthesis' was rather a vast kitchen midden of diverse and contradictory positions." (Rothbard, 1995, p.277).

And specifically with regard to his economic methodology, Rothbard goes on further to claim that:

"Mill engaged in a strategy of duplicity to confuse the enemy and to win their support" (Rothbard, 1995, p.279).

But whereas Blaug goes on to accuse Mill of being an a priorist hiding behind positivist rhetoric, Rothbard reaches the opposite conclusion: Mill promoted positivist economics while masquerading as an a priorist.

2.2.3 John Elliott Cairnes

Whereas Mill had attempted to inject some inductive, empirical ideas into classical methodology, Cairnes returned to the more purely deductive approach of Senior. Cairnes went so far as to claim that the economic propositions arrived at by introspection accorded them a more certain veracity than their equivalents in the natural sciences. He claimed:

"The economist may thus be considered at the outset of his researches as already in possession of those ultimate principles governing the phenomena which form the subject of his study, the discovery of which, in the case of physical investigation, constitutes for the inquirer his most arduous task." (Cairnes, 1875, p.77)

Cairnes contended that whereas the physical scientists make use of laboratory experiments, economic scientists use mental experiments.

2.2.4 John Neville Keynes

John Neville Keynes published his methodological treatise during the period of the *Methodenstreit* that raged between Carl Menger of the Austrian School and Gustav Schmoller of the German Historical School [17]. Keynes attempted to provide an elaboration of the classical a priori position that emphasised empirical elements, with the intention of providing something of a reconciliation of the two opposing positions. He did this by claiming that economics:

"…must begin with observation and end with observation." (Keynes, 1891, p.227).

But all he seems to have meant by this, is that the fundamental propositions of economics are derived from observation, and that conclusions of economic theorising be checked against observed facts to detect the existence of disturbing causes. He presumably considered the process of introspection justifying the validity of the central postulates of economics to be a fundamentally empirical one.

In attempting a reconciliation between the positions of the Austrian school and the German Historical School, Keynes reinforced the Millian conception of economic man; that it is an abstraction from a complete real man. In this way, Keynes was able to sympathise with the idea that institutional factors and non-economic motives can play powerful roles in the generation of actual economic outcomes. Presumably, these factors are to be detected as disturbing causes when the conclusions of economic theories are tested against actual outcomes, thus bolstering the role of empirical elements within economic methodology.

2.2.5 Lionel Robbins

Lionel Robbins published his treatise on economic methodology in 1932 [18], after a period in which the inductivist methodology of the institutionalist school had gained significant influence (see Section 2.5 below). In this work, pointing to the writings of Senior and Cairnes, Robbins reasserted the thesis that the proper methodology for economic science follows an a priori deductive process from self-evidently true fundamental postulates. The position expounded by Robbins in this work represented the core mainstream position that was attacked by the positivists as the winds of logical positivists blew through the economics community (see Section 2.6 Below).

Robbins identified the scope of economic science as:

"Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." (Robbins, 1935, p.75)

Robbins argued that historical induction is the worst possible approach to generating explanations of economic phenomena, and that controlled experimentation is not much better. Instead, he tells us that:

"The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and undisputable facts of experience…they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious." (Robbins, 1935, p.79).

On the fundamental postulates of economics, Robbins declares that the fundamental postulate of the theory of value is: individuals arrange their preferences in order. The fundamental postulate of the theory of production is: there is more than one factor of production. The fundamental postulate of the theory of dynamics is: future scarcities are uncertain. Robbins makes it clear that economic science:

"relies upon no assumption that individuals will always act rationally." (Robbins, 1935, p.95).

As the theoretical structure grows more complicated, subsidiary postulates enter the framework, and these limit the applicable scope of the various theoretical statements to the situations in which the assumed conditions obtain. But wherever there is a correspondence between the assumptions and the facts of the matter, the conclusions of the theories are inescapable. However, since the values of the variables represented by the postulates are dynamic, it is impossible to make quantitative predictions, even in the absence of impeding influences. Instead, the best we can do is to make conjectures about the potential directions of change. The significance of economic theory for policy makers is that it makes it possible to determine which sets of objectives are compatible with each other and which are not, and the conditions upon which such compatibility is dependent.

This, to Robbins, is all so simply obvious, that those who have seriously attempted to question it have done so because they have had political agendas. He levels this charge at both the Historical School and the Institutionalists [19].

2.2.6 Summing up the Classicists

I am now in a position to show, using the Hausman-based heuristic device, how the classical approach fails to meet the methodological commitments of the mechanistic model.

Goals

The primary goal of economic scientists within the classical school was the explanation of economic phenomena. The classicists were realists in the sense that in the construction of their theories, they aimed at faithfully representing truths about the world.

Explanation

Successful explanation was considered to be achieved when it was shown that the conclusions of economic science follow logically, via a process of deduction, from self-evidently true fundamental postulates.

Theories

Theories were formal structures comprised of fundamental postulates and auxiliary assumptions, along with chains of deductive inference.

Testing

Testing of the fundamental axioms of economic science was considered unnecessary, since these were considered self-evidently true. Testing of the conclusions of theories was considered problematic, due to the existence of *disturbing causes*, as well as due to uncontrollability that is the result of the dynamic nature of the hypothesised variables.

Methodological Monism

The positions of the classicists on the issue of methodological monism varied, according to how broadly they conceived of the applicable methodological principles.

Clearly, the purely formal structure of theoretical constructs, and the purely deductive nature of theoretical development, do not conform to the non-formal and empirical strictures of the mechanistic model.

2.3 The Austrian School

While I have lumped the Austrian school in with other forms of a priorism in section 2.2 above, their methodological commitments are sufficiently distinct so as to warrant a section of their own.

The Austrian school was founded by Carl Menger, one of the three simultaneous discoverers of the principle of marginal utility – the others being William Stanley

Jevons and Leon Walras - in 1871, with the publishing of his book Grundsätze der Volkswirtschaftslehre [20]. The term *Austrian School of Economics* was first used after the publication of Menger's second book - Untersuchungen über die Methode der Sozialwissenschaften und der Politischen Oekonomie insbesondere - in 1883 [21]. Menger eschewed the Walrasian general equilibrium approach, and the Marshallian partial equilibrium approach, for a causal explanation of the determination of real, disequilibrium prices. He states:

"I have devoted special attention to the investigation of the causal connections between economic phenomena involving products and the corresponding agents of production, not only for the purpose of establishing a price theory based upon reality and placing all price phenomena (including interest, wages, ground rent, etc.) together under one unified point of view, but also because of the important insights we thereby gain into many other economic processes heretofore completely misunderstood. This is the very branch of our science, moreover, in which the events of economic life most distinctly appear to obey regular laws." (Menger, 1871, p.49).

By the mid-1930s the school had all but disappeared, with the exception of two highly prominent members: Friedrich Hayek and Ludwig von Mises. Hayek followed Menger's focus on the dynamic processes of economic systems, in an attempt to understand how dispersed knowledge becomes coordinated via the decentralised price system. Mises also followed the Mengerian approach, producing prominent works on monetary economics. He dedicated most of his energies however, to analysing the logical status of economic propositions, with the goal of establishing a solid epistemological foundation for the methodology of economic science.

After Ludwig von Mises published *Human Action* in English in 1949, a resurgence of the Austrian School began. For modern Austrians, the primary goal of economic science is the explanation of the regularities in economic phenomena.

"The main question that economics is bound to answer is what the relation of its statements is to the reality of human action whose mental grasp is the objective of economic studies." (Mises, 1949, p.6).

Specifically:

"…explaining how monetary exchange gives rise to the processes of economic calculation that are essential to rational resource allocation in a dynamic world." (Salerno, 1999, p.56).

This modern Austrian school of thought is built upon the epistemological framework of *praxeology* developed by Ludwig von Mises [22]. In the works of Ludwig von Mises, economics is viewed as part of a unified theory of human action. Mises states:

"Until the late nineteenth century political economy remained a science of the "economic" aspects of human action, a theory of wealth and selfishness…The transformation of thought which the classical economists had initiated was brought to its consummation only by modern subjectivist economics, which converted the theory of market prices into a general theory of human choice…No treatment of economic problems proper can avoid starting from acts of choice; economics becomes a part, although the hitherto best elaborated part, of a more universal science, praxeology." (Mises, 1949, pp. 2-3)

Mises saw that much was at stake in the vigorous methodological debates of the times. The Historical School looked to replace economics with history, and the positivists sought to replace it with the logical structure of the natural sciences. Mises therefore sought to provide an epistemological foundation for economic science that established logical legitimacy and validated the achievements of classical economic theory.

Mises is clear about what he believes demarks the subject matter of economics:

"The field of our science is human action, not the psychological events which result in an action. It is precisely this which distinguishes the general theory of human action, praxeology, from psychology. The theme of psychology is the internal events that result or can result in a definite action. The theme of praxeology is action as such." (Mises, 1949, pp.11-12)

Praxeology is a rival epistemology to that of empiricism. It rejects the analytic/synthetic distinction, and asserts that the fundamental axioms of economic science are necessary, a priori synthetic truths. Economic science under praxeology is conceived as a chain of deductive inferences from necessarily true axioms, to necessarily true conclusions, that are capable of providing knowledge of the real world. Empirical testing of assumptions or conclusions is thus viewed as mistaken.

The task of economists from the praxeological viewpoint becomes one of explanation, and attempts at empirical prediction are considered fundamentally misguided. All economic theories can do is to explain stylised facts, and to show policy makers why their market interventions are incapable of achieving their stated aims.

The modern Austrians claim that Ludwig von Mises solved the problem of how to account for a priori synthetic truths without recourse to idealism. In doing so, he is said to have:

"contributed path-breaking insights regarding the justification of the entire enterprise of rationalist philosophy." (Hoppe, 2007, p.50)

Mises saw himself as the latest in a line through Leibniz and Kant, in opposition to one through Locke and Hume [23]. He sought to demonstrate that the propositions of economic science are of the synthetic a priori type. He did this by arguing that denial of the central axiom of praxeology – that humans act - cannot be achieved without self-contradiction, and that the categories of values, ends, means, choice, preference, cost, profit, and loss, are logically implied in this action axiom, and are presupposed in any attempt to deny it. And so Mises declares that all true economic propositions can be deduced by means of formal logic from knowledge of the meaning of *action* and its categories. Economic explanations then, must make recourse to individuals and the categories of action, to count as valid.

Using our heuristic device, the following commitments can be derived for the modern Austrian school:

Goals

The goal of economic science is explanation. While the conclusions of economic science are said to be necessarily true, this does not imply that theories can be used for the prediction of actual events. The prediction of stylised facts is the best that can be hoped for.

Explanation

To be considered a successful explanation, a theory must begin with the fundamental axiom of human action, which is considered an a priori necessary truth. It must then be shown how, via a chain of deductive inference, the event or phenomenon explanandum is a logical implication.

Theories

Theories are discovered by a process of introspection. Theories incorporate laws, whose scope is limited by the obtainment of certain specified conditions, subject to ceterus paribus clauses.

Testing

Efforts to empirically assess the validity of the theories of economic science are considered misguided. The only way that a theoretical conclusion can be falsified is if it can be shown that the chain of logical inference from the fundamental axiom is somehow faulty.

Methodological Monism

The methods of the natural sciences are flatly rejected as inappropriate for the social sciences, in favour of the a priori deductive approach of praxeology.

The rejection of methodological monism, and the formal a priori deductive methodology that denies any role for empirical investigation, clearly does not meet the standards of the mechanistic model.

2.5 Institutionalist Economics

Institutionalist economics is a school of thought that flourished in the 1920s on the back of works by Thorstein Veblen (1899) [24], Wesley Mitchell (1913) [25], and John Commins (1924) [26]. After an initial surge in popularity, Institutionalism drifted to the fringes of the discipline, until experiencing something of a revival later in the twentieth century. The institutionalists rejected the deductive nature of economic explanation espoused by the classical theorists, for an inductivist approach that flatly rejected the idea of methodological monism.

The motivating force behind this methodological dissent would appear to be due to a rejection of classical ontology. Walton Hamilton claimed that only the institutional approach could explain how *parts* of the economic system relate to the *whole* of the social system. And this is because neoclassical economics does not recognise that:

"The proper subject-matter of economic theory is institutions…Economic theory is concerned with matters of process…Economic theory must be based upon an acceptable theory of human behaviour…" (Hamiltom, 1919, p.318) [27].

And concerning human behaviour it has been claimed that:

"…the single most important characteristic of institutionalism is the idea that the individual is socially and institutionally constituted." (Hodgson, 2000, p.327).

Since its founding days, Institutionalism has rallied against a priorist and positivist methodology, seeking to develop an alternative based solely on explanation, which emphasises holism, systematicity and evolution, and gives central roles to the notions of power, conflict and non-rational, non-general behaviour. This non-formal approach rejects the idea of universal economic generalisations, and instead, emphasises the uniqueness and individuality of particular systems. It has been described as a form of storytelling, called *pattern modelling* by Abraham Kaplan [28].

Under the pattern modelling approach, an event is explained by:

"…identifying its place in a pattern that characterizes the ongoing processes of change in the whole system" (Wilbur & Harrison, 1978, p.73).

Despite sharing a common methodology, it has been widely noted that the institutionalists have not proven capable of generating a body of shared theory [29]. This isn't really surprising when one recognises how loosely defined the pattern model approach to explanation is.

The process has been described as a three-step participant-observer method [30]. In the first stage, the theorist is socialised into a single self-maintaining social system in order to experience a number of current *themes* under a variety of contexts, which are supposed to illuminate the unity of the system. In the second step, the theorist explicitly organises the information gained, into hypothesis – interpretations of the themes – for validity testing. Finally, after several themes have been validated, a model is constructed by linking validated hypothesis into a network. The resultant model is referred to as a *pattern model*.

Making use of our heuristic device, the following set of philosophical commitments can be identified:

Goals

The goal of economic science is *understanding*, as a special form of explanation. Theories are historico-relative, so that predictive accuracy is unattainable.

Explanation

Explanation is successfully achieved with the production of pattern models. An explanandum is explained when it is shown how it is a part within a unified whole.

Both the explanans and the explanandum are particularised to the system being described.

Theories

Theories are discovered through an observer-participant process. They are explicated by means of models. The models are comprised of a network of connected hypothesis that may contain statements of laws. These laws need not be generalised beyond the system under study.

Testing

The various hypothesis that pattern models are comprised of, are subjected to contextual validation. This process of validation requires testing on a variety of data sets, which includes previous case studies, survey data, and personal observations. The model is accepted as a true representation until it is superseded by a model capable of incorporating a greater variety of data [31].

Methodological Monism

Institutionalists argue that since certain types of economic data are unstable, and that the quality of experimental data is poor due to variations in all factors at once, the subject matter of economics is so different to that of the physical sciences that the empirical methodologies appropriate for the latter are not appropriate for the former.

The rejection of methodological monism, along with insufficient levels of rigour and clarity around the empirical elements of theoretical construction, render the pattern modelling approach inferior to the mechanistic model as a normative standard for theoretical development in economic science.

2.6 Positivist Economics

During the period from the 1930s to the 1960s, mainstream economics embraced the flourishing philosophical school of positivism. In this subsection, I will show how this was achieved, with reference to key works of economic methodology. This survey moves from the early incarnations of *logical positivism* through to the more mature positions of *logical empiricism*, and concludes with the impact of the instrumentalism and descriptivism of Friedman and Samuelson.

2.6.1 Logical Positivism & Logical Empiricism

For the logical positivists, breaking from the tradition of earlier positivists such as Auguste Comte and Ernst Mach, explanation was considered the primary goal of the sciences, including economic science. As was discussed in Section 1.1 above, scientific theories were viewed by the logical positivists as vehicles for showing why the occurrence of particular phenomena were to be expected. Such explanations became the basis for prediction, with the two concepts being tightly, symmetrically, defined. Several developments relating to prominent methodologists attest to the influence that positivist philosophers had on economic methodology.

Firstly, Terrence Hutchison devoutly introduced logical positivist philosophy, and its attendant language to bear on economic methodology. In his *Significance and Basic Postulates of Economics* [32], he quotes repeatedly from a number of the leading figures of the logical positivist movement, including Ayer, Carnap, Hahn, Hempel and Oppenheim, Neurath, Popper, and Schlick, as well as from their forerunners, Russell, and Wittgenstein.

Hutchison aimed a fervent attack on the methodology of the *a priorists*. Leaning on the analytic-synthetic distinction, he claimed that most of what passed for economic propositions was tautological; it only dealt with conceptual connections and could say nothing about the empirical world. Hutchison exhorted economic scientists to limit their enquiries to intersubjective empirically testable statements. This, he claimed is the criterion that demarks science from pseudoscience. But he emphasised that it need only be logically possible to test economic propositions, not practically possible. This distinction traces back to the verification principle expounded by Schlick as a criterion for meaningfulness for a proposition [33]. At the time of writing, the impossibility of verificationism had been recognised, and so Hutchinson argued for a falsification criterion for intersubjective empirical tests.

Secondly, Oskar Morgenstern, a stout methodological monist, sought to incorporate the mathematical and experimental methods of the natural sciences into economic practice. Morgenstern was a frequent attendee at the meetings of the Vienna Circle [34], as well as Karl Menger's Mathematical Colloquium [35], so it is no wonder that he stridently introduced the ideas of the logical positivists into methodological debates within economics.

Inspired by the works of philosophers such as Frege, Russell and Wittgenstein [36], he embraced mathematical logic as a means of formalising economic theory. He lamented that:

> "one of the most powerful and impressive steps forward that the human spirit has made in the last two generations has up to now apparently been totally overlooked by the social sciences" (Morgenstern, 1936, p.389).

Morgenstern followed up on his commitment. Specifically, he worked on axiomatization methods to formalise various branches of economic theory. Most prominently, along with John von Neumann - who had worked on formalising quantum mechanics - he provided an axiomatisation of utility theory [37]. Morgenstern argued that many of the confusions besetting economic controversies were due to a lack of rigour in the use of language. He promoted formal mathematical methods as a means of establishing a scientific language for economics, superior to verbal exposition.

Further, Morgenstern saw no limit to the application of mathematical methods to economic science. Responding to suggestions that such limits existed, he remarked:

"If we were to ask today what the limitations of mathematics are in physics, both mathematicians and physicists would be baffled by the question, brush it off as meaningless, and go on with their work." (Morgenstern, 1963, p.444).

Since Morgenstern viewed economics as ultimately an empirical science, besides making use of data generated naturally by economic phenomena, he also sought to incorporate experimental methods into economic methodology [38]. To this end, he advocated both small scale controlled experiments by business firms, and large scale direct experiments on the economy as a whole. Mortenstern also advocated "laboratory" experiments where possible.

Thirdly, Fritz Machlup [39], drawing heavily on Richard Braithwaite's exposition of the DN model [40], also applied this model directly to economic theory. Machlup argued that economics comprises a hypothetico-deductive system in which only the lower level assumptions and deduced changes require testing. He distinguished between *fundamental assumptions*, *specific assumptions* and *deduced low-level assumptions*. Fundamental assumptions are those such as the fundamental postulates that the a priorists considered self-evident truths. He also refers to these as "heuristic principles", "useful fictions", procedural rules", and "definitional assumptions". Machlup argues that it is impossible to subject these fundamental assumptions to independent verification. He states:

"there is no need for direct test of the fundamental postulates in physics, such as the laws of conservation of energy, or of motion; there is no need for direct test of the fundamental postulates in economics, such as the laws of maximising utility and profit." (Machlup, 1955, p.17).

Instead, the whole system of hypothesis can be tested by taking together a set of fundamental postulates and a set of specific assumptions, deducing logical consequences from these, and subjecting those to empirical test. In a series of articles between 1954 and 1956, Machlup debated with Hutchison over this point. In this debate, Hutchison claimed that the fundamental postulates should be subjected to empirical tests, and for this, Machlup labelled him an "ultra-empiricist" [41].

2.6.2 Friedman & Samuelson

By this stage, positivist philosophy was firmly established as the basis for economic methodology. Then, two theorists whose writings arguably remain the most influential to this day, whilst maintaining that they operated within the tenets of the positivist philosophy, broke with it in terms of the symmetry thesis, and completely rejected the idea of explanation as a goal for economic science [42].

The first of these two theorists was Milton Friedman. His paper *The Methodology of Positive Economics*, published in 1953, was the most cited work on economic methodology in the twentieth century [43]. Friedman, following Machlup, claimed that economists should not bother about the realism of the assumptions their models are constructed upon.

Friedman claimed that the goal of economics is:

"to provide a system of generalisations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields." (Friedman, 1953, p.146)

Friedman thus espoused a strict instrumentalism. He rejected all forms of introspection and causal empiricism, for a single principle of theoretical validity, in which the only relevant criteria for determining the validity of economic theories, is that their predictions match experience. Friedman declared that:

"the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience." (Friedman, 1953, p.149)

By validity, Friedman means that the hypothesis has yet to be falsified. But note that hypotheses are not to be read literally. Models, in his view, are not meant to be representational in the sense of mirroring some part of the actual world. Friedman maintains that models are simply abstract conceptual worlds. Theories are merely vehicles for *analysing* phenomena in the real world. They contain a set of abstract conceptual statements, and a set of rules that allow the conceptual apparatus to be applied to the real world. Given these perspectives, Friedman declared realism to be a methodological vice that constrained theoretical development. He claimed:

“Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions.” (Friedman, 1953, p.152)

Friedman considers validity to be a necessary, but not sufficient, criteria for selecting among competing theories. The other relevant considerations, which he states cannot be objectively specified, include *simplicity* and *fruitfulness*. Friedman admits that selection amongst valid theories must be considered somewhat arbitrary.

And what, in Friedman’s view, guides the development of hypotheses? Following Reichenbach’s distinction between the context of discovery and the context of justification [44], he tells us that:

“The construction of hypotheses is a creative act of inspiration…The process must be discussed in psychological…studies…not treatises on scientific method” (Friedman, 1953, p.173)

Friedman advocated methodological monism, and claimed that:

“The inability to conduct so-called “controlled experiments” does not, in my view, reflect a basic difference between the social and physical sciences…The denial to economics of the “crucial experiment” does not hinder the adequate testing of hypothesis” (Friedman, 1953, p.150-151)

The second of these positivist theorists was Paul Samuelson. Samuelson promoted two central principles in his methodological writings. First, he argued that economists should limit themselves to *operationally meaningful* theories. Second, he declared that science does not seek to explain, only to describe.

Samuelson initially couched his methodological views in terms of *operationalism* [45]. His goal was to provide a basis for the empirical testing of theories. But it turns out that his version of operationalism has little in common with the philosophy developed under that name by Percy Bridgman. Samuelson's operationalism has been interpreted as a form of *falsificationism*, reminiscent of the views espoused by Hutchison [46]. Later, Samuelson embraced a form of *descriptivism* [47]. With this move, he declared that the only valid form of scientific explanation is phenomenal description.

By the late 1970s, Hutchison was still able to remark of mainstream economics that:

> "Perhaps a majority of economists…would agree that improved predictions of economic behaviour or events is the main or primary task of the economist." (Hutchison, 1977, p.8)

The weak conception of explanation endorsed by the logical positivists had been firmly established as appropriate for economic science.

2.6.3 Summing up the Positivists

Despite some fundamental differences in methodological positions, our heuristic device reveals the following set of commitments:

Goals:

For the early positivists, the goal of economic science was explanation. However, according to the DN model they championed, this goal was intimately tied up with the goal of prediction. As the views of the positivists evolved from logical positivism through to logical empiricism, prediction became the primary, and eventually only, goal of economic science.

Explanation:

The DN model defined the concept of explanation for the early positivists, and arguably continues to do so for practitioners working in this tradition right down to this day. Explanation is mere description.

Theories:

For the positivists, theories are comprised of a tautological conceptual body of statements, combined with a set of rules for applying this body to situations in the real world. Theories are *as-if* constructs. They incorporate *laws* such as: all consumers aim to maximise utility, and all firms aim to maximise profit. Theory choice is dictated by empirical success.

Testing

Testing of the conclusions of economic theories is encouraged, though seldom seriously undertaken. Testing of assumptions is considered misguided.

Methodological monism

The positivists are committed to methodological monism.

Commitments to methodological monism and empirical testing of theories align with the mechanistic model, however, acceptance of the DN model as a mode of explanation clearly fails mechanistic standards.

2.7 The Modern Landscape

In the final decades of the twentieth century, two prominent economic methodologists, Mark Blaug and Bruce Caldwell, published notable works [48]. Both volumes make explicit reference to the developments in the philosophy of science, from which they draw conclusions for the active practice of economic science. Both of these volumes show how the economics profession at the time had been responding to the *growth of knowledge theorists,* Karl Popper, Thomas Kuhn, Paul Feyerabend and Imre Lakatos in the development of their methodological convictions.

Blaug argued that falsificationism was established in the philosophy of science as an appropriate normative standard, and went on to apply this standard to contemporary practice, which he ultimately found deficient. Caldwell's assessment of the contemporary literature in the philosophy of science was that there were no agreed normative rules for the assessment of scientific theories, and so he rejected positivism, along with alternative positions on which to base economic methodology, arguing instead for a position of *pluralism*.

The following comment by Mark Blaug in 1980, in summing up the state of contemporary methodological practice, arguably remains true to this day:

> "It is possible to discern something like a mainstream view…economics is held to be only a "box of tools"…it is also ultrapermissive within the "rules of the game": almost any model will do…" (Blaug, 1980, p.110).

Arguably, what we are witnessing here, is a wholesale drift into methodological anarchy, due to a failure to reorient philosophical underpinnings in the wake of the degeneration of the DN model; practitioners are engaged in engineering pursuits without the guidance of an overarching scientific framework. Paul Feyerabend would be delighted.

Against this backdrop of methodological permissiveness however, the DN model introduced by the logical positivists retains its sway. Colleen Johnson recognised in 1996 that the DN model continued to be the model of explanation the mainstream paradigm clings to as descriptive of the discipline of economic science [49]. Various covering laws - such as profit maximising firms, utility maximising consumers and the law of demand - are combined with specific boundary conditions to predict observable outcomes.

And, Blaug, in his book states:

> “I myself remain persuaded that the covering-law model of scientific explanation survives all the criticisms it has received.” (Blaug, 1980, p.10)

Further, despite the permissive character of the current methodological landscape, it is possible to crudely individuate it. As Doyne Farmer points out, economics, as currently practiced, is polarised between two extreme approaches [50]. On the one hand there is a theoretical approach that is focused on building elegant analytic models with no concern for empirical adequacy. And at the other extreme, is econometrics, which is a relatively arbitrary data-driven approach that pays little regard to fundamental theoretical concerns.

Julian Reiss claims that positivistic trends in economics have now been abandoned and that with this, explanation has once again become a priority of working economists [51]. If he is correct, then the time may be ripe for the adoption of mechanistic standards. And insofar as the Complexity Economics movement, which is the subject of discussion in the following chapter, can be seen to incorporate these standards, we may be coming to a new epoch in which the economics profession once again aligns with developments within the philosophy of science literature.

2.8 Concluding Remarks

In this chapter, I have shown how the philosophy of logical positivism led to a revolution in economic methodology, as practitioners abandoned the *a priori* method and embraced the DN account of scientific explanation and theory structure. It was further suggested that, although the DN model remains influential in current times, its failures have engendered somewhat of a methodological anarchism as practitioners have failed to reorient their philosophical foundations. Through discussion of current practice, I showed that the standards of the currently dominant model of scientific explanation within the contemporary philosophy of science literature – the mechanistic model - are clearly not being adhered to. Consequently, the answer to the question as to what is an appropriate methodological framework for economic science, has been answered in this chapter with: not those adopted by the current mainstream.

In the next chapter, I outline some broad methodological claims made by proponents of the emerging heterodox school of economic thought known as *Complexity Economics*, and argue that in contrast to current mainstream practice, these prescriptions do display adherence to mechanistic standards.

Chapter 3: Complexity Economics – Central Themes & Objections to Mainstream Economic Methodology

I suggested in Chapter 2, that orthodox economic practice fails spectacularly to meet the mechanistic standards outlined in Chapter 1. But changes in scientific practices can only be initiated on a large scale where there is an alternative paradigm available. I believe that just such a paradigm has emerged – the school of thought known as Complexity Economics. The goal of this research project is to establish that the methodological approach of this school of economic thought, meets normative standards established in the philosophy of science literature on mechanistic explanations. In this way, I will propose an answer to the question: what is an appropriate methodological framework for economic science? I aim to answer with: the methodological framework of complexity economics.

This final chapter is structured as follows. In Section 3.1, I introduce the complexity economics movement, by providing some brief comments on its history and motivations. In Section 3.2, I outline some major objections the movement has against mainstream methodological practice. Specifically, it will be shown that an ontological commitment to disequilibrium combined with an epistemological insistence on a generative standard of explanation based on realistic agent-based modelling, creates an unbridgeable gap between the competing methodological frameworks. In Section 3.3, I outline the philosophical commitments of the complexity economics school of thought, by using the heuristic device introduced in Chapter 2. In Section 3.4, I investigate the methodological writings of some key figures within complexity economics movement, and establish that the methodological framework that emerges can be considered to meet the standards of the mechanistic model introduced in Chapter 1. Section 3.5 presents some potential objections to the analysis in the preceding section, along with some tentative responses. In Section 3.6, I outline some avenues for further research. I conclude in section 3.7 with a brief summary of the argument presented in this chapter, which establishes that the methodological framework of complexity economics conforms to the normative standards demanded by up-to-date philosophy of science, as represented by the mechanistic model.

3.1 The Complexity Economics Movement

Complexity economics has emerged out of the broader movement of complexity science. This multi-disciplinary movement aims to bring a set of complex systems tools to a wide variety of disciplines, and to bring the rigour of analysis associated with the "hard sciences", to bear in the "soft sciences". Complex systems analysis is built upon non-linear mathematics and studies how emergent phenomenon arise out of the interactions of lower-level building blocks. Doyne Farmer [1] has remarked that, given the early metaphor of Adam Smith's *invisible hand* in economics [2], it is strange that this is the scientific discipline in which the complex systems revolution has had the least impact.

Magda Fontana has published a comprehensive paper chronicling the motivation behind, and the history of, complexity economics, from its conception at the Santa Fe Institute for the Study of Complex Systems (SFI) in the late 1980s [3]. In this sub-section, I will briefly state some of her findings.

The genesis of the complexity economics approach can be traced to a ten day workshop in September 1987, co-chaired by Kenneth Arrow – a Nobel Laureate in economics – and Philip Anderson – a Nobel Laureate in Physics. Ten physicists and ten economists were invited to participate in the workshop. Through a series of lectures and discussions focused on theories and methods, a dialogue was to be opened up, with the intention of productive interaction. Fontanna shows that the founding motivation was to discover methods that could complement the neoclassical approach so as to stave off some of the criticisms that had been levelled against it at the time; the founders were not intending the interdisciplinary workshop to result in an alternative approach to that of the neoclassical orthodoxy. The papers from the workshop proceedings were published in a volume titled *The Economy as an Evolving Complex System* [4]. Fontana shows that while the published workshop papers (arguably) reveal a consensus on methodological issues, subsequent published material by some of the participants paint a different picture [5].

Subsequent to the workshop, an Economics Programs was established at SFI in 1988. Brian Arthur – the only heterodox economist to have been invited to the workshop - was appointed as director. Fontana shows how under the influence of Brian Arthur and John Holland, the direction of the research conducted within the Economics Program diverged sharply from that of the economics mainstream. After quoting at length what Holland describes as the distinguishing features of the economy, Fontana concludes that he:

"…provides a framework in which economies and economic actors operate under hypotheses that are very different from the neoclassical economics ones, and he refuses a purely mathematical approach to economics in favour of a computational analysis." (Fontana, 2009, p.8)

By the late 1990s the economics of the Economics Program had become strongly heterodox; it represented an alternative to the neoclassical approach. A workshop held in 1996, designed to overview the contribution of complexity research to economics, resulted in the publication of *The Economy as an Evolving Complex System II*. The proceedings evaluated this contribution by contrasting the conclusions of complexity research with two central elements of mainstream practice: the equilibrium approach and the manner in which dynamical systems are represented. The conclusions were quite condemning, with the editors of the proceedings papers exclaiming:

"…the equilibrium approach does not describe the mechanism whereby the state of the economy changes over time – nor indeed how an equilibrium comes into being. And the dynamic system approach generally fails to accommodate the distinction between agent – and aggregate – levels except by obscuring it through the device of representative agents. Neither accounts for the emergence of new kinds of relevant state variables, much less new entities, new patterns, new structures." (Arthur, Durlauf & Lane, 1997, p.3)

Fontana goes on to show how subsequent researchers within the Economics Program, pursuing different objectives to those followed under the leadership of Arthur, moved to a position of reconciliation, at least seemingly, with mainstream economics. The economists associated with the *strongly heterodox* period at the

Santa Fe Institute, however, have continued to promote their research at other institutions.

In another paper [6], Fontana argues, that what has come to be known as complexity economics, constitutes a new paradigm in the Kuhnian sense [7, 8].

The basis of Fontana's argument, is that the difference in ontology between complexity economics and the mainstream, is inconsistent with the possibility of a shared methodology, so that it is impossible for the insights of the complexity school to be simply absorbed into the mainstream framework. It is my contention, that what these theorists are arguing for, is the rejection of mainstream methodological practice in favour of one that is essentially based on mechanisms; it is a rejection of descripitivist and instrumentalist practice focused on *prediction*, in favour of a realist alternative targeted at successful *explanation*.

Complexity economics has been developed along a number of lines. I use the term here, as coined by Brian Arthur [9], to refer to the body of central tenets I deem common to the main variants. I consider these main variants, besides the body of work produced by Arthur, to include *generative economics* [10], *interactive-agent economics* [11], *agent-based computational economics* [12] and *complex economics* [13].

3.2 Objections to Mainstream Methodology

The attacks of complexity economists on mainstream methodology are numerous and diverse. However, in my view, it is possible to discern two related primary differences in approach that suffice to show that the divide between the respective methodologies is unbridgeable. The first of these is the reliance of mainstream practice on market equilibrium as the central organising concept. The second difference is the insistence of the complexity school on a constitutive approach based on agent-based modelling. I will briefly outline each of these differences below.

The first major difference is primarily of an ontological character. The mainstream view of economic phenomena treats the systems under study as existing at equilibrium. These systems are admitted to be subject periodically to exogenous perturbations, but are assumed to experience only temporary effects, since strong dampening forces are assumed to work at the speedy restoration of equilibrium. Based on this perspective, the Walrasian equilibrium model, first developed in the nineteenth century [14], remains the central working concept in theory construction. The alternative game theoretic approach is also equilibrium based. Successful explanation of an observed phenomenon is achieved when it is demonstrated to be a Nash equilibrium of some game.

In contrast, the complexity approach views the economy as a complex system that is perpetually creating novel structures and possibilities for exploitation. It is a system in which economic agents constantly alter their actions and strategies in response to mutually created outcomes. Embedded in this viewpoint is a commitment to endogenously generated disequilibrium. Arthur argues that the existence of endogenous disequilibrium in economic phenomena can be primarily attributed to two sources [15].

Firstly, he argues that it is a result of the inductive procedural rationality of individual human agents. Mainstream models assume a notion of perfect deductive rationality on the part of individual decision-makers. This notion is rejected by complexity economists, as not only implausible, but more importantly, as being demonstrably impossible. Citing Frank Knight's acknowledgement of fundamental uncertainty [16], and George Soros' reflexivity principle [17], Arthur argues that, because all situations involving choice in the economy involve the outcomes of future events, which are by definition unknowable, the optimisation problems that traditional models assume individuals conduct, are not well-defined, and so the notion of deductive rationality is logically impossible.

Given the impossibility of deductive rationality, complexity economists look to the findings of behavioural economics and cognitive science, to more faithfully represent in their models, the processes that are hypothesised to lead to the generation of aggregate economic phenomena. The primary way of doing this, is by modelling individual agents as forming subjective beliefs, which are updated in the face of evidence of the efficacy of these beliefs [18]. Complexity economics therefore, replaces the impossible assumption of deductive rationality, with the empirically plausible assumption of inductive procedural rationality.

A second source of endogenous disequilibrium is identified in *technological change* [19]. Under the equilibrium view, novel technologies are modelled as one-off exogenous shocks that impact on the production functions of firms. The result is an endogenous growth shift to a new equilibrium point. In contrast, the complexity approach sees technological advancement as permanently ongoing self-reinforcing waves of disruption, acting in parallel and at all scales. New technologies are created out of existing ones, alter production and consumption patterns, and propagate the further evolution of technological innovation.

Acceptance of the ongoing adaptation identified in these two sources, requires a change in methodology, to properly characterise and analyse economic phenomena. This has led complexity economists to embrace the algorithmic way of thinking that underlies the concept of computation [20, 21]. Arthur thus states that:

"formally, we can say that the economy is an ongoing computation" (Arthur, 2015, p.8)

And Epstein states that agent based modelling renders:

"society as a distributed computational device, and in turn the interpretation of social dynamics as a type of computation." (Epstein, 2006, p.4)

"trade networks (markets), are essentially computational architectures. They are distributed, asynchronous, and decentralised and have endogenous dynamic connection typologies." (Epstein, 2006, p.16)

The second major difference is primarily of epistemological character. In their explicit methodological writings on explanation, complexity economists have extolled a generative normative standard. Joshua Epstein, rejecting the *as-if* models of standard practice, summarises it succinctly as:

"If you didn't grow it, you didn't explain it." (Epstein, 2006, xii).

Further, he states that:

"To explain a macroscopic regularity x is to furnish a suitable microspecification that suffices to generate it." (Epstein, 2006, p.51)

In the standard equilibrium approach, an abstract auction pricing mechanism acts as a coordination device. This approach involves no interdependence of agent decisions. This eliminates the possibility of strategic behaviour. The generative stance in contrast, leads to a realist, agent-based computational modelling approach to theory construction, underpinned by interdependent, reactive, goal-directed agents. Epstein and Axtel, referring to social science, thus see:

"the artificial society as its principal scientific instrument" (Epstein & Axtel, 1996, p.20)

Agents are broadly defined as:

"…bundled data and behavioural methods representing an entity constituting part of a computationally constructed world." (Tefatsion, 2005, p.6).

Under this definition, possible agent entities include individuals, social groups, institutions, biological entities and physical entities. They:

> "…range from active data-gathering decision-makers with sophisticated learning capabilities to passive world features with no cognitive functioning." (Tesfatsion, 2005, p.6).

Combining the two major methodological differences outlined, it is evident that an ontological commitment to disequilibrium combined with an epistemological commitment to a generative explanatory standard, results in a methodological commitment to a realistic agent-based approach to theory construction. These methodological commitments cannot be accommodated within an abstract equilibrium framework. There exists therefore, an unbridgeable gap between the methodological frameworks.

3.3 Philosophical Commitments

In Chapter 2, I introduced a heuristic tool for categorising and contrasting the philosophical commitments of various approaches to economic methodology. I'll now use this device to express the commitments of the complexity economics school.

Goals

Complexity economics embraces both the epistemic and practical aims of scientific theorising, but the main emphasis is on explanation. Epstein states that the core of his program concerns the notion of a scientific explanation, and emphasises that his works constitute an argument in response to the question: What is to be the accepted standard of explanation in the social sciences [22]? He goes on to argue that:

"The scientific enterprise is, first and foremost, *explanatory*." (Italics in original) (Epstein, 2006, p. 50)

Practitioners take a realistic approach to their subject matter.

Explanation

I argue in section 3.4 below that complexity economics is committed to a mechanistic mode of scientific explanation. Arthur claims that under complexity economics:

"a solution is no longer necessarily a set of mathematical conditions but a pattern, a set of emergent phenomena, a set of changes that may induce further changes, a set of existing entities creating novel entities. Theory in turn becomes not the discovery of theorems of underlying generality, but the deep understanding of mechanisms that create these patterns and propagations of change." (Arthur, 2015, p.25)

And as we have seen, the movement is characterised by Epstein's admonishment that for a theory to be explanatory, it must be generative.

Theories

heoretical development has an essential empirical flavour, as the following statements testify:

"We can often do much useful pre-analysis of the qualitative properties of nonequilibrium systems, and understand the mechanisms behind these; still, in general the only precise way to study their outcomes is by computation…We can use carefully-designed computer experiments…to isolate phenomena and the mechanisms that cause these." (Arthur, 2015, p.9)

"The computer is an exploratory lab for economics, and used skilfully, a powerful generator for theory" (Arthur, 2015, p.11)

Testing

Rigorous empirical procedures are undertaken in order to demonstrate the explanatory and predictive adequacy of theoretical constructs.

"The computer is a powerful laboratory in which to conduct experiments concerning the generative sufficiency of agent specifications." (Epstein, 2006, p.xiii)

"it is precisely…empirical falsifiability – that qualifies the agent-based computational model as a *scientific* instrument." (Epstein, 2006, p.16)

Methodological Monism

Complexity economics was spawned from the broader complexity science movement which aims to bring common toolsets to the various branches of science. This attitude toward scientific enquiry reveals a commitment to methodological monism.

This short list reveals that complexity economics shares many of the philosophical commitments of the mechanistic model: that non-formal explanation forms the basis of scientific theorising, that theoretical development and testing require sustained, rigorous, empirical investigation, and that there is a methodological model appropriate to all scientific disciplines.

3.4 Is Complexity Economics Mechanistic?

In Section 3.3 above, I claimed that the philosophical commitments of the complexity economics school match the broad requirements for the mechanistic model of scientific explanation introduced in Chapter 1. I will now dig deeper into the methodological writings of complexity economists to explore adherence to some of the more specific requirements. After making a few general remarks, I will address the categories of: *phenomena*; *entities*; *activities*; *organisation*, and *bottoming-out*.

3.4.1 General Observations

There are several general observations on the relation between mechanistic explanations and the methodology of complexity economics that are worth noting, before digging into specifics.

Firstly, it is worthwhile noting the history behind the mechanistic model. According to Bechtel and Richardson, the development of this model was derived from actual scientific practice, with the express purpose of:

“understanding the behaviour of *complex systems* in biology and psychology.” (Bechtel & Richardson, 2010, p.17 (italics mine))

It is perhaps not too surprising then that the mechanistic approach also appears to be a prominent methodological component of those taking a complex systems approach to economic science.

Secondly, the generative standard espoused by the complexity economists conforms to the constitutive requirement of the mechanistic model. They are both requirements that legitimate explanations go beyond mere descriptions of their target phenomenon.

Thirdly, complexity economists reject the production of mathematical models as explanatory devices. Arthur for example states that a detailed economic theory:

> “…would seek to understand deeply the mechanisms that drive formation in the economy and not necessarily seek to reduce these to equations.” (Arthur, 2015, p.21-22)

And Epstein declares that:

> "…the mere formula…is *devoid of explanatory power despite its descriptive accuracy*." (Epstein, 1999, p.51)

And perhaps more fundamentally, given the view of the economy as an endogenously evolving system, Packard rejects the reduction of explanations to equations, stating:

"once a dynamics is embedded in the form of equation(s), there is no way for the system to endogenously change its own path" (Packard, 1988, p.170)

The mathematical approach is therefore viewed as incapable of capturing the appropriate explanandum phenomena.

Arthur justifies his rejection of mathematical reductionism by pointing to explanations within the biological sciences. He specifically references theories of embryological development, biochemical pathways, molecular genetics and cell biology, as exemplars of the type of explanatory structure he considers to be appropriate for economic science [23]. And, as mentioned above, these are precisely the type of theories that motivated the mechanistic model of explanation in the first place.

3.4.2 Phenomena

A correct specification of the phenomena to be explained is an essential criteria for successful explanation under the mechanistic account. The complexity school's rejection of the equilibrium approach reveals a commitment to faithfully specify economic explanandum. Arthur, for example, objects that the equilibrium approach posits:

"…an idealised, rationalised world that distorts reality" (Arthur, 2015, p.4)

And from this, he argues that by approaching economic analysis in such a way, we filter out the phenomena that should form the targets of our explanations.

Kirman also affirms this stance, when he states:

"the vision of the world reflected in modern macroeconomic models leaves out aspects of the economy which seem to be central to understanding how it functions and evolves." (Kirman, 2011, p.3)

Two other considerations point to the importance of a faithful rendering of explanandum phenomena. Firstly, is the incorporation of time. Mainstream models are either static, or dynamic only in the sense that time is included as a reversible parameter. In contrast, the algorithmic approach used by complexity economists, incorporates a notion of time that faithfully represents the path dependency of historically situated phenomena.

A second factor is the appeal to meso-level phenomenon. The meso-level is a level between the micro-level and the macro-level. It is a realm of temporal phenomena. To illuminate the idea, I'll introduce a traffic jam example provided by Arthur [24]. In this example, the micro level equates to the individual car level, in which relevant features include its speed and distance to other cars. The macro level is the aggregate level characterised by statistical variables such as average speed. Traffic jams Arthur tells us, are phenomena that exist at a level in between these two. Phenomena that become targets of explanation at the meso-level include self-reinforcing behaviours, clustered volatility and sudden percolations. And these phenomena are explained with reference to strategic behaviour. With the standard equilibrium assumptions of mainstream models, there is no room for strategic behaviour on the part of individual agents. Within the representative agent approach, all agents are assumed to react identically to the equilibrium conditions, with the consequence of there being no scope for further action. This is because the relevance of the equilibrium assumptions is that they constitute an answer to the question: what low level conditions are consistent with equilibrium aggregate behaviour? These economists therefore take idealised abstractions as their explanandum phenomena. In contrast, complexity economists are interested in explaining real world phenomena. In their approach, perfect rationality is replaced with procedural rationality, where agent behaviour can be characterised as being directed at the exploitation of niches in their environment. As a consequence, the incorporation of this more realistic behaviour, reveals patterns of exploitation indicative of what occurs in actual economic systems.

Summing up the impact of the standard equilibrium approach on the phenomena offered up for explanatory analysis in economic science, and how this differs under the complexity approach, Arthur states:

"Complexity economics…is a different way of thinking about the economy. It sees the economy not as a system in equilibrium but as one in motion, perpetually "computing" itself – perpetually constructing itself anew. Where equilibrium economics emphasises order, determinacy, deduction, and stasis, this new framework emphasises contingency, indeterminacy, sense-making and openness to change." (Arthur, 2015, pp.24-25)

We are now in a position to see how some of the concerns of the Institutionalist and Austrian Schools of economic thought, which were discussed in Chapter 2, can be accommodated within the complexity economics framework. Within this framework, the economy is viewed as being organic, evolutionary and historically-contingent. Technological change and institutional arrangements constitute both key explanatory targets in their own rights, as well as important explanatory elements for other economic phenomena. These factors are considered central to the task of successful explanation, for members of the institutionalist school. And for the Austrian school, it is the dynamic economic processes that are the key targets of economic explanation. The complexity approach accommodates both of these concerns through its focus on the non-equilibrium dynamics of a complete, realistic ontology incorporating downward causation.

But is economic phenomena characterised in mechanistic terms by the complexity economists? Let's explore Arthur's description of *the economy* for some clues. He tells us that:

"The economy is a vast and complicated set of *arrangements* and *actions* wherein *agents* – consumers, firms, banks, investors, government agencies – buy and sell, speculate, trade, oversee, bring products into being, offer services, invest in companies, strategize, explore, compete, learn, innovate, and adapt. In modern parlance we would say it is a massively parallel system of concurrent behaviour. And from all this concurrent behaviour markets form, prices form, trading arrangements form, institutions and industries form. *Aggregate patterns* form." (Arthur, 2015, pp.2-3, italics mine)

This is not a formal definition, but a mere characterisation. Yet in this characterisation we can see all the basic elements of the mechanistic approach. Firstly, we see that the basic units are *agents*. We can view these as the *entities* that are required for a successful mechanistic explanation. Secondly, these entities can be considered to have *properties*, on the basis of which they can be grouped into a variety of categories. Thirdly, these agents are said to be engaged in *actions*. These actions can be viewed as corresponding to the *activities* carried out by entities in mechanistic explanations. Fourthly, the agents are said to be subject to a set of *arrangements* structuring the interactions between them. These arrangements can be viewed as the organisational features of mechanisms. The orchestrated organisation of agents and their activities is said to be responsible for the generation of *aggregate patterns*. These aggregate patterns are the explanatory targets of the theories of economic science.

3.4.3 Entities

So, complexity economics appears to delineate its explanatory targets in a manner consistent with the mechanistic model. And further, it claims to proceed in its explanatory pursuits by making recourse to entities and activities organised in such a way so as to be productive of these explanatory targets, just as the mechanistic model requires. But do complexity economists really treat their entities in a way that the model requires? I believe that this can be demonstrated.

Complexity economics is clearly committed to a representational modelling approach that delineates entities *realistically*. The agent-based methodology seeks to properly describe the parts of the mechanisms underlying economic phenomena, as opposed to merely positing relationships among fictional components. The agents are heterogeneous, and are defined at different levels, and so elaboration of the *properties* of these entities is also a feature of the explanatory endeavour. It was shown in Section 3.2 above, that possible agent types include physical entities, biological entities, social groups and institutions. Economic theories then are expected to make recourse to a set of *real* entities.

3.4.4 Activities

According to the mechanistic model, activities are the producers of change. Possible activities are determined by entities and their properties. Entities and activities are thus said to be interdependent [25].

Complexity economics is clearly committed to developing models that incorporate faithful representations of the causal activities carried out by the entities. The agent-based methodology explicitly represents these processes in the algorithms executed by individual agents. They are an important component of the micro specifications in generative models.

An important thing to notice about the mechanistic account adopted here, is that *activities* aren't merely characterised as *interactions* [26]. Within this mechanistic account, interactions are like activities, in that they emphasise spatio-temporal intersections and changes in properties. However, unlike activities, interactions do so "without characterising the productivity by which those changes are effected at those intersections" (Machamer, Darden & Craver, 2000, p.5). It is the productive activities engaged in by entities that render entities *causes* of phenomena.

Going back to a quote from Arthur above:

"The economy is a vast and complicated set of *arrangements* and *actions* wherein *agents* – consumers, firms, banks, investors, government agencies – buy and sell, speculate, trade, oversee, bring products into being, offer services, invest in companies, strategize, explore, compete, learn, innovate, and adapt." (Arthur, 2015, pp.2-3 (italics mine))

An interactionist account would de-emphasis individual actions and highlight common interactions. In contrast, in this quote, which is typical of the way complexity economists speak, we see that the actions of individual actors are emphasised [27].

And revisiting another quote from above, we see that agents:

> "…range from active data-gathering decision-makers with sophisticated learning capabilities to passive world features with no cognitive functioning." (Tesfatsion, 2005, p.6).

Note that decision-makers are *active*, that is, they engage in activities.

During model development, hypothesised activities are heavily simulated and investigated. These efforts may be viewed as attempts to ensure satisfaction of manipulability criteria. They are tested for support of non-backtracking counterfactuals. This is a criteria for mechanistic explanation that was identified in Chapter 1. A further criteria is that representations of activities be veridical. Testimony for complexity economists' adherence to this criteria can be obtained by citing Arthur's appeals to the findings of behavioural economics and cognitive science as sources of information for modelling the strategies of individuals [28]. This approach is contrasted to mainstream practice, in which assumptions are made on the basis of analytic convenience.

3.4.5 Organisation

The heavy emphasis on network theory within complexity economics attests to the importance of *organisational structure* in the explanatory models of its practitioners. In fact, the motivating idea behind complexity science is the question as to how novel phenomena arise from the organisation of lower-level building blocks.

Kirman, for instance, states that:

"we need to know about the network of links between the individuals, whether these are consumers, firms or other collective entities…Almost any serious consideration of economic organisation leads to the conclusion that network structures both within and between organisations are important." (Kirman, 2011, p.35)

And he goes on further to claim:

"we have to acknowledge that the direct interaction between agents and the way in which that interaction is organised has fundamental consequences for aggregate economic outcomes." (Kirman, 2011, p.37)

Complexity economists recognise that aggregate behaviour will be fundamentally different in the situation where agents are directly linked to one another and influence each other, than in an anonymous market system where agents are linked only by the price system. They thus argue at length that we cannot infer the behaviour of the aggregate from that of the (representative) individuals. This acknowledgement requires that greater emphasis is placed on the rationality of agents (attributes). And this greater emphasis on rationality necessitates the incorporation of explicit representations of the two-way interactions between the attributes of individuals and the organisational structures that they both collectively create, and are conditioned by.

Kirman affirms that:

"The passage to the aggregate level is mediated by the network structure in which individuals find themselves." (Kirman, 2011, p.37)

Kirman argues that by incorporating this extra detail, our analytical tasks are actually simplified. This is because, although the analysis appears more complex, the reasoning and calculating capacities we need to attribute to agents are far less than what needs to be assumed by standard models, in order to generate the relevant aggregate behaviour.

Not only is it a requirement to incorporate realistic interaction structures in our models, but:

"the next step is to understand how these networks form and if, and why, they persist" (Kirman, 2011, p.38)

Sociologists have long acknowledged the importance of networks for aggregate social outcomes [29]. They have recognised that if preferences are influenced by identity, and identity is influenced by position in social networks, then these networks need to be taken into consideration.

Two extreme approaches are pursued within the neoclassical paradigm. On the one hand there is the approach in which individuals are treated as independent, acting in isolation from one another, with their activities coordinated by market signals. On the other hand, there is the full game-theoretic model, in which individuals are treated as being completely interdependent; they are connected to all others and assigned extra-human powers of knowledge and reasoning.

Both of these extreme approaches are unrealistic. But if we are to allow network structures to feature in our models, which networks do we consider endogenous and which exogenous? This is where, under the complexity approach, experimental work becomes important. This experimental effort seeks to delineate the networks that are operative in the mechanisms that are productive of the explanandum phenomena of interest.

3.4.6 Bottoming-Out

With the appeal to behavioural economics and cognitive science as a basis for realistic agent-based modelling, the complexity economists display a belief in the hierarchical nature of mechanistic explanation and *bottoming-out* that serves both to demarcate the boundaries of scientific disciplines and provide constraints on the models constructed within those disciplines.

Besides the appeals by Arthur cited above, Kirman tells that:

"behaviour is very much determined by the network of neurons that is activated in a certain situation" (Kirman, 2011, p.37)

These appeals to cognitive science mirror the claim of Craver and Alexandrova that:

"neuroscience and economics should integrate results through efforts to construct and constrain descriptions of multilevel mechanisms." (Craver & Alexandrova, 2008, p.381).

The bridging discipline is neuroeconomics, whose goal is said to be:

"to explain economic behaviour by revealing how brain mechanisms work, how the components in the brain (body, and world) work together in such a way that organisms exhibit the patterns of decision-making they do." (Craver & Alexandrova, 2008, p.382)

3.4.7 Conclusion

In this sub-section, I have demonstrated that the methodological framework of complexity economics, as described in the explicit methodological writings of key figures within the movement, conform to the standards outlined by the developers of the mechanistic model of scientific explanation. Firstly, it was shown that explanandum phenomena are required to be realistically represented, with recourse made to the mechanistic categories of entities, activities, and organisation. Then, it was shown how the objects within each of these categories are delimited experimentally by way of agent-based simulation experiments, in a manner consistent with the requirements of the mechanistic model.

3.5 Objections

One possible objection to my thesis that complexity economics and mainstream practice are methodologically incompatible, is that I have simply overplayed the differences in their explanatory standards. One way of arguing this, would be to cite instances where complexity economists seem to directly contradict my position.

For example, one could quote from Joshua Epstein. In a footnote, Epstein attempts to claim a legitimate place within the philosophy of science literature for his proposed normative standard for scientific explanation. Lamenting his informal usage of the term *explanation*, he reluctantly admits that, since no covering laws are involved in the generative standard espoused, the model fails one of Hempel & Oppenheim's DN requirements. However, he then goes on to argue that, since by the Church-Turing thesis, there is a corresponding logical deduction for every computation, the generative standard does in fact meet the deductive requirement of the DN account, and so he claims that the standard can be considered to fall within the hypothetico-deductive framework [30]. But, admitting that his requirements for explanatory candidacy are week, he goes on to seek philosophical legitimacy by claiming common elements with van Frassen's constructive empiricism model (see Section 1.3.4).

What we appear to have here, is a case of Epstein conceding a certain legitimacy to mainstream explanatory practices on the grounds that they conform to certain philosophical standards. He is attempting to reject these practices, while at the same time maintaining adherence to these very same standards. [31]

Another avenue for fleshing out an argument to the effect that I have overplayed the differences in the explanatory standards of complexity, and mainstream, economists, is to unearth quotes from complexity economists denying the revolutionary nature of their approach.

But there are a number of reasons why an argument of the hypothesised form is insufficient to establish the objection. Firstly, with relation to the Epstein statements described, since the complexity economists surveyed are not explicitly working within a mechanistic framework, one should not be surprised that they appeal to alternative criteria for validity. It is the aim of this thesis to provide an *independent* validation of the Complexity Economics framework. Secondly, with relation to the unearthing of quotes contradicting my thesis, one must be careful when interpreting statements of specific aims, given the sociological factors at play. There are certainly strategic reasons why authors may wish to avoid issuing confrontational platitudes that have the potential to further marginalise their already heterodox positions.

Another objection I will consider here, is the claim that simulation experiments are not equivalent to standard laboratory experimentation in the physical sciences, so that the empirical criteria of the mechanistic model are not capable of being fulfilled by means of the simulation procedures conducted by complexity economists. I respond to this objection by appealing to the work of Wendy Parker and Eric Winsberg.

Parker's work is focused on dissolving the distinctions between experiments and simulations, by emphasising their commonality. She claims that what they both have in common, is that an object is carefully set up, intervened on, and then observed for the purpose of learning about some target [32.] From this, she makes a case that simulations shouldn't be considered epistemically inferior to experiments.

Winsberg refers to Parker's position as "the simulation account of experiment" [33]. While Winsberg mostly agrees with Parker's position, including the epstemic status of simulation in relation to experiment, he nevertheless seeks to locate the conceptual distinction between the two terms. He proposes that the difference is one concerning justification:

"When an investigation fundamentally requires, by way of relevant background knowledge, possession of principles deemed reliable for building models of the target systems; and the purported reliability of those principles, such as it is, is used to justify using the object to stand in for the target; and when a belief in the adequacy of those principles is used to sanction the external validity of the study, then the activity in question is a simulation. Otherwise, it is an experiment." (Winsberg, 2010, p.66-67).

The agent-based simulation models of the complexity economists have one distinct advantage in the provision of such justification. These economists make the ontological claim that their target systems are computational. In arguing that their object systems are of the same algorithmic character as their targets, these economists go some way to establishing legitimacy for their modelling procedures.

There is also (at least) one countervailing concern however. Since agent-based modelling platforms have only developed in recent decades, it would be sensible to be somewhat critical as to whether any of these platforms has sufficiently proven themselves to warrant the requisite level of confidence. Some authors, for instance, have noted that the field lacks standards for model comparison and replication [34].

However, even if one were to concede the point over the balance of pros and cons, it is far from established that the simulation methods that economists can afford themselves of, are incapable of providing a basis for determining the non-backtracking counterfactuals that the manipulation criteria of the mechanistic model requires.

3.6 Further Research

The interpretations provided in this chapter, have been limited to high level methodological writings. If the primary conclusion of this chapter – that complexity economics meets mechanistic standards - is warranted on this basis, an obvious direction for further research presents itself. This further research would take the form of detailed case studies of paradigmatic models developed within the complexity economics tradition. In this way, it could be investigated whether mechanistic standards obtain in actual practice and not merely in methodological rhetoric. Besides the descriptive benefits such studies would provide, they could also possibly reap substantial prescriptive dividends. For if it is found that certain tenets of the mechanistic model have been violated, the mechanistic framework may provide some guidance for the construction of restorative research programs.

There are a number of potential paradigmatic models warranting such investigation. One possible candidate is the Santa Fe stock market model [35]. This model explores asset pricing dynamics under endogenous expectations formation. It is an attempt to explain real world asset pricing dynamics. Another candidate is the fish market model developed by Alan Kirman [36]. In this model, Kirman attempts to show how the relationship between the behaviour of individual market participants and aggregate market behaviour, is mediated by structural market features. It is an attempt to explain real world market features. A further candidate is Joshua Epstein's adaptive organisations model [37]. This model investigates how individual agents endogenously generate internal organisational structures. It is an attempt to explain how real world organisational hierarchies form and dissolve in response to their dynamic environments. The last possibility that I will mention here is Brian Arthur and Wolfgang Polak's model of combinatorial evolution [38]. This model is designed as a first step in representing the key mechanism proposed by Arthur, to be responsible for the process of technological change in real-world economies. Many other possibilities for detailed case studies exist.

3.7 Concluding Remarks

In this chapter, I introduced the emerging school of economic thought known as complexity economics, and described some major ontological and epistemological differences the movement has with the mainstream economics profession, namely, the rejection of the equilibrium view and a commitment to a generative mode of explanation based on agent-based modelling. These differences were shown to represent an unbridgeable methodological divide.

Next, I examined the methodological writings of complexity economists for conformity to a normative explanatory standard based on mechanisms, which was the ultimate purpose of this study. It was found that complexity economics shares the philosophical commitments of the mechanistic model: that explanation forms the basis of scientific theorising, that successful explanation involves the description of a veridical representative model of the mechanism responsible for the production of the explanandum phenomena, that theoretical development and testing requires sustained, rigorous, empirical investigation, and that these commitments constitute a methodological model that is appropriate for all scientific disciplines. Furthermore, it was shown that these economists seek to describe mechanisms in terms of the categories of entities, activities, and organisation, in accordance with the standards that the mechanistic model requires.

Conclusion

Economic science has a long history of responding to developments within the philosophical community. In particular, economic methodologists have traditionally paid attention to up-to-date literature in the philosophy of science. In recent decades, a new mechanistic philosophy has come to dominate the literature on scientific explanation, yet working economists and economic methodologists alike, have so far failed to embrace this new philosophy.

In this research project, I have explored the ramifications for economic science of taking seriously the normative implications of the new mechanistic philosophy. I have used the question of what constitutes an appropriate methodological framework for economic science, as a lens through which to focus this exploration. I have applied this approach through a three-pronged process.

In the first leg of the argument, I presented the historical context of the mechanistic model, and justified the adoption of it as a normative standard. It was argued that the mechanistic model of explanation has come to prominence on the basis that it provides both an explication of the concept of scientific explanation that overcomes many of the shortcomings of its traditional forebears, and perhaps more importantly, that it provides practical guidance for working scientists engaged in the activities of theoretical discovery and development. Chapter 1 therefore answered the question of what constitutes an appropriate methodological framework for economic science, with the answer: one that meets mechanistic standards.

The argument of Chapter 2, was directed at establishing that whilst economic methodologists have, in the past, responded to developments within the philosophy of science, the current mainstream has failed to do so. To affect this argument, the normative standards that were established in the previous chapter, were applied. The question of what constitutes an appropriate methodological framework for economic science, was thus answered with: not those practiced by the current mainstream of the profession.

The third leg of the argument was directed at validating the methodological framework of the heterodox school of thought known as Complexity Economics. This was achieved by applying the mechanistic normative standards established in chapter 1, which the neoclassical mainstream was shown to have failed in chapter 2. It was shown that complexity economics shares the philosophical commitments of the mechanistic model, and that these economists seek to describe mechanisms in terms of the categories of entities, activities, and organisation, in accordance with the standards that the mechanistic model requires. In this manner, I arrived at a third and final answer to the question of what constitutes an appropriate methodological framework for economic science. This ultimate answer, it was argued, is: the methodological framework of complexity economics.

Endnotes

Chapter 1

1. Frederick Suppe (Suppe, 1974, p.) suggests that this publication is: (Carnap, 1923)
2. See: (Hempel & Oppenheim, 1948)
3. See: (Hempel, 1965)
4. See: (Braithwaite, 1953)
5. See: (Nagel, 1961)
6. See: (Hume, 1748)
7. See: (Suppe, 1974)
8. (Bromberger, 1966, p.92)
9. This argument was formulated by Kyburg (Kyburg, 1965, p.147)
10. See: (Woodward, 2002, p.38) & (Woodward, 2014)
11. See: (Salmon, 1971)
12. See: (Salmon, 1971, pp.76-77)
13. See: (Salmon, 1984), (Salmon, 1994) & (Salmon, 1997)
14. See: (Salmon, 1997)
15. See, for example: (Dowe, 1992) & (Dowe, 2000)
16. See: (Friedman, 1974)
17. See: (Kitcher, 1981) & (Kitcher, 1989)
18. See: (van Frassen, 1980)
19. (van Frassen, 1980, p.156)
20. See, for example: (Salmon & Kitcher, 1987)
21. See: (Achinstein, 1983) & (Achinstein, 2010)
22. (Achistein, 1983, p.13)
23. For expositions of this account, see, for example: (Woodward, 2000) & (Woodward, 2003)
24. (Woodward, 2003, p.55)
25. This objection is due to Strevens. See: (Strevens, 2006, p.15)
26. See: (Strevens 2004) & (Strevens, 2009)
27. For a statement of Lewis' account, see: (Lewis 1986a)
28. For a discussion of the pre-emption problem, see: (Lewis 1973)

29. The solutions he takes issue with are those proposed at: (Lewis 2000) & (Woodward 2003). This is argued at: (Strevens 2003)
30. Strevens, 2008, p.242. The relation of enganglement is succinctly paraphrased by Stephan Hartmann: “F is *entangled* with P if “All Fs have P” is true and has sufficient scope, and if it is *not* true that “if such and such an object with F had not had F, it would still have had P” (Hartmann, ?)
31. Strevens developed *frameworked* explanations – as an inferior type of explanation to deep standalone explanations – to account for multiply realisable properties and functional specifications. These explanations cite difference-making relations *relative to a background state of affairs*. The background state of affairs, or *framework*, is a set of fixed background conditions, against which difference-makers are established.
32. (Craver, 2002, p.70)
33. See (Hedstrom & Ylikoski, 2010, p.51) for an exercise in the comparison and contrast of a number of prominent proposals
34. This commitment is noted at both: (Bechtel & Abrahamsen, 2005, p.424) & (Craver, 2006, p.362)
35. As pointed out by Uskali Maki (Maki, 1998), there are many diverse senses of the term *realism*. The claim here is made primarily to contrast against instrumentalist and associated ontological positions.
36. For Hume’s account of causation see: (Hume, 1748). While mechanistic accounts of causation have been developed, one need not base a mechanistic account of explanation on a mechanistic account of causation. The point here is simply that whatever account of causation one builds a mechanistic account of explanation upon, it must make stronger metaphysical claims than the Humean account.
37. See: (Craver, 2006, pp.368-383)
38. In arguing this point, he points toward the works of: (Pearl, 2000) & (Woodward, 2003)
39. For a discussion of this point, see: (Bechtel & Abrahamsen, 2005)
40. See: (Bechtel & Abrahamsen, 2005, p.432)
41. For an extensive treatment of these issues, supplemented with examples from the biological sciences, see: (Bechtel & Richardson, 2010)

42. Prominent statements of this position can be found at: (Popper, 1934), (Carnap, 1935) & (Reichenbach, 1938)
43. See: (Craver, 2002, p.360)
44. See: (Bechtel & Abrahamsen, 2005, p.436)
45. See: (Batterman and Rice, 2014) & (Ross, 2015)
46. See: (Batterman, 2001, p.23)
47. Craver: “Mechanistic models are *ideally complete* when they include all of the relevant features of the mechanism, its component entities and activities, their properties, and their organisation. They are *pragmatically complete* when they satisfy the pragmatic demands implicit in the context of the request for explanation.” (Craver, 2006, p.367)

Chapter 2

1. These two books are: (Hollis & Nell, 1975) & (Rosenberg, 1976)
2. See: (McCloskey, 1985) & (McCloskey, 1994)
3. See: (Hands, 2001)
4. See: (Caldwell, 1994)
5. I have adapted this schema from the one presented by Hausman at: (Hausman, 2008, p.5)
6. I group these methodologists together under a single label following Blaug (1992), being fully aware of Caldwell's criticism of this grouping (Caldwel, 1982). Caldwell objects that the views of these authors are quite diverse, and that the primary thing they have in common is that they object to the submission of theories to empirical test. I accept this criticism, but retain the grouping nonetheless, on the basis that all the authors in question are committed to a process of theorising that begins with supposedly irrefutable assumptions and proceeds without reference to empirical facts. I recognise however, that if taken to the extreme, by this rationale, the so-called positivist and falsificationist theorists who failed to practice what they preached, would also perversely qualify as a priorists. I therefore accept that the common characterisation of these authors as a priorists is somewhat arbitrary. In fact, in his opposition to contemporary mainstream economic methodological practice, Tony Lawson lambasts its "largely a priori" nature (Lawson, 1999)
7. Ricardo is quoted as saying in front of Parliament that some of the conclusion of economics are: "as certain as the principles of gravitation." Quoted at: (Blaug, 1992, p.53)
8. The key pieces of methodological literature from this period are: (Senior, 1827), (Senior, 1836), (Mill, 1836), (Mill, 1848), (Cairnes, 1875), (Keynes, 1890), & (Marshall, 1890)
9. See: (Robbins, 1932)
10. See: (Mises, 1933) & (Mises, 1949)
11. See: (Knight, 1940, 1941)
12. See: (Rothbard (1976) & (Rothbard, 1957)
13. See: (Hoppe, 1995)
14. See: (Senior, 1827)
15. See: (Mill, 1843)

16. For Kant's elaboration of transcendental idealism, in which he attempts to show how synthetic a priori statements are possible, see: (Kant, 1781)
17. Due to space limitations, I have been unable to cover the German Historical School here.
18. See: (Robbins, 1932).
19. See: (Robbins, 1935, pp.81-82)
20. The roots of the Austrian school stretch back to the late scholastics of the fifteenth century, who sought to delineate causal economic laws
21. In (Mises, 1969), Mises provides a brief history of the Austrian school. The claims made above can be found at p.1
22. Mises devoted a significant portion of his works to establishing an unassailable epistemological basis for economic science. His position is most prominently stated in: (Mises, 1933, 1949, 1962)
23. See: (Mises, The Ultimate Foundation of Economic Science, p.12)
24. The elements of Veblen's approach can be traced back to an article by Walton Hamilton (Hamilton, 1919)
25. Mitchell was primarily interested in explaining business cycles. His theories are constructed from inductive generalisations developed out of his empirical research
26. Commins emphasised that the law plays an important role in the generation of economic outcomes
27. As quoted in Hodgson, 2000, p.317
28. See: (Kaplan, 1964), see also: (Diesing, 1971)
29. For a statement of this contention, see, for example: (Wilbur & Harrison, 1978, p.72)
30. For a detailed discussion, see: (Wilbur & Hollis, 1978, pp.74-83)
31. Benjamin Ward provides a check-list to assist in the verification of models. See: (Ward, 1972, p.189)
32. (Hutchison, 1938)
33. See: (Schlick, 1932)
34. See (Morgenstern, 1976b)
35. Karl Menger is the mathematician son of Carl Menger, the founder of the Austrian school of economics
36. Of particular note were: Frege (1884), Russell (1915) and Wittgenstein (1922)

37. See: (von Neumann & Morgenstern, 1944)

38. See: (Morgenstern, 1950, 1954)

39. See: (Machlup, 1955; 1956; 1978)

40. See: (Braithwaite, 1953)

41. The articles in which this exchange took place, are: (Machlup, 1954, 1956; Hutchison, 1956)

42. It has been claimed that since Friedman is writing in the logical positivist tradition, and given that the symmetry thesis is an integral part of this tradition, Friedman is telling us that theories providing accurate predictions must also be regarded as providing successful explanations, no matter what other attributes they have (Wilbur & Harrison, 1978, p.66). Against this, I maintain that Friedman simply does not endorse the symmetry thesis and has jettisoned the concept of explanation from methodological consideration completely.

43. See: (Hands, p.143)

44. See: (Reichenbach, 1938)

45. See: (Samuelson, 1948)

46. For expressions of this view, see: (Blaug, 1980, p.89) & (Caldwell, 1982, p.190)

47. See: (Samuelson, 1972)

48. These works are: (Blaug, 1980) & (Caldwell, 1982)

49. For her argument on this point, see: (Johnson, 1996, p.289)

50. Farmer makes this point at: (Farmer, 2012, p.7)

51. See: (Reiss, 2008)

Chapter 3

1. See: (Farmer, 2012, p.2)
2. See: (Smith, 1776)
3. See: (Fontana, 2009)
4. (Anderson, Arrow & Pines (eds.), 1988)
5. Fontana appeals to quotes to this effect from published accounts by Colander (Colander, 2003, p.8) and Waldrop (Waldrop, 1992, p.142), as well as from a personal interview conducted with Kenneth Arrow in 2009. I'm not so sure that the published papers reveal a consensus either. Holland's (Holland, 1988) and Kauffman's (Kauffman, 1988) papers in particular, read to me as quite subversive.
6. See: (Fontana, 2008)
7. For the classic statement by Kuhn, see: (Kuhn, 1962)
8. For alternative views, see: (Durlauf, 2012) & (Holt et. al., 2011)
9. Arthur coined the term in: (Arthur, 1999)
10. Generative Economics had been developed by Joshua Epstein. See: (Epstein, 1999) & (Epstein, 2006)
11. For an exposition, see: (Miller & Page, 2007)
12. Agent-Based Computational Economics (ACE) has been developed most prominently by Leigh Tesfatsion. See: (Tesfatsion, 2006) & (Tesfatsion, 2002)
13. This formulation is due to Alan Kirman. See: (Kirman, 2011)
14. This device was introduced by Leon Walras. See: (Walras, 1874)
15. See: (Arthur, 2015, pp.4-7)
16. Frank Knight argued that…See: (Knight, 1921)
17. George Soros has developed a principle of reflexivity…See: (Soros,) & (Soros, 1987)
18. There has been two alternative views within the community regarding the fruitfulness of faithful representation of the cognitive strategies of individual agents. The views of the key thinkers I present here can be contrasted with those of Blume (Blume, 1996) and Padgett (Padgett, 1997). According to these authors, overemphasising the role of cognition is to make the mistake of methodological individualism. To them, how individuals act doesn't matter so much. Instead, they focus on the structures of interaction through which

individuals act. From a mechanistic perspective, both of these considerations are important. Depending on the context of explanation, either one may be more appropriately emphasised, but a full explanation would need to be robust to both set of factors. Another way of approaching this difference is with reference to the distinction made by Bechtel & Richardson between analytic and synthetic approaches to determining system components and their functions. Analytic approaches are said to be bottom-up (they use knowledge of components to reconstruct the behaviour of the system as a whole), whereas synthetic approaches are referred to as top-down (they decompose system behaviour into hypothesised coordinated sub-processes). See Richardson & Bechtel, 2010, p.18.

19. Arthur presents a book length elaboration of this idea at: (Arthur, 2009). He also provides a succinct summary of his position on technological change at: (Arthur, 2015)
20. Prominent statements of this view can be found in: (Arthur, 2015), (Farmer, 2012), (Epstein, 2006) & (Tesfatsion, 2005)
21. Agent-based modelling is enacted through computer simulations. On the epistemological implications of simulation studies as scientific instruments see Winsberg, 2010 and DeLanda, 2011.
22. See: (Epstein, 2006, p.xii)
23. See: (Arthur, 2015, p.16)
24. This example is discussed at: (Arthur, 2015, p.12)
25. Machamer, Darden & Craver (2000), provide an explicitly dualist account of mechanisms in which both entities and activities are included in the ontology. The guiding purpose for developing this account is to capture the intuitions behind both the substantialist (activities reduce to entities) and process (entities reduce to activities) ontologies. See: Machamer, Darden & Craver, 2000, pp.4-8.
26. This is important to note in the current case, since definitions of mechanisms have been given in terms of complex systems, which are interactionist. For example, Glennon defines a mechanism as: "A mechanism for a behaviour is a complex system that produces that behaviour by the interactions of several parts, where the interactions between parts can be characterised by direct, invariant, change-relating generalisations." (Glennon, 2002, pS344)

27. This is not to say however, that they do not also speak in interactionist terms. Examples of this type also abound.
28. For an expression of this sentiment, see: (Arthur, 2015, p.4)
29. Such acknowledgments are evident, for example, in the work of Robert Putnam on social capital
30. In the final section of his book, Epstein calls for work to be done in developing an explicit formalisation for agent-based models. He claims here that "As an epistemological matter, it is important to insist that the activity is therefore deductive in nature." (Epstein, 2006, p.345) And he quotes Einstein's contention that the "grand aim" of all science is "to cover the greatest number of empirical facts by logical deduction from the smallest possible number of hypotheses or axioms" (p.347). All this is of course misplaced, on the mechanistic account
31. Things become somewhat confused when, after appealing to van Frassen's notion of empirical adequacy once again, the reader is directed to another footnote, wherein Epstein clarifies that in the example under discussion: "The question then becomes: where, in the population of simulated histories is the true history?" This appeal to truth conditions is in stark contrast to the anti-realist framework promoted by van Frassen.
32. See: (Parker, 2009)
33. See: (Winberg, 2010, p.60)
34. See for example: (Axtell et al., 1996) and (Epstein, 2006, p.29)
35. See: (Arthur, Holland, LeBaron, Palmer & Taylor, 1994, 1997, 2015)
36. See: (Kirman, 2011)
37. See: (Epstein, 2006)
38. See: (Arthur & Polak, 2006, 2015)

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