Mechanistic Complexity Economics: A Methodological Framework for Economic Science

A Thesis submitted for the degree of Doctor of Philosophy (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
Master of Research (Philosophy) – Macquarie University

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Department of Philosophy
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Contents

Abstract

The central argument of this thesis is that Economic Science requires a methodological re-orientation in order to re-align with contemporary philosophy of science. This is argued with reference to both the history of general philosophy of science - in particular the literatures on scientific explanation and the structure of scientific theories – and the history of the methodology of economics. It is also argued that the heterodox school of economic thought known as Complexity Economics offers a valid basis for achieving such a reorientation.

Statement

The core ideas and preliminary explication of this book were explored in a Master of Research degree at Macquarie University, culminating in a thesis that was submitted as partial fulfilment of that award. The objectives of that short report have been expanded upon, filled in, and validated in this book. Readers who have not read the prior work will not be at a disadvantage, given that all preliminary material is recreated in the current production.

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Introduction

The primary purpose of this book is to argue for a reorientation of methodological practice within the economics profession. This argument, which occupies the content of Parts **1** and **2**, takes the following form:

P1: Scientific methodology should be consistent with up-to-date philosophy of science

P2: Historically, economic scientists have sought to develop methodologies consistent with up-to-date philosophy of science, but have failed to adjust in recent decades

C: Economic science requires a methodological reorientation

The secondary purpose of this book is to argue that the heterodox school of economic thought known as *Complexity Economics* works within a methodological framework that *does* meet the normative requirements of up-to-date philosophy of science, and so offers a solution for the reorientation of methodological practice within economic science. This argument, which is contained in the content of Parts **1**, **3** and **4**, takes the following form:

P1: The Neo-Mechanistic model of scientific explanation and theory structure is the dominant account within the philosophy of science

P2: The methodological framework of Complexity Economics conforms to the normative strictures of the Neo-Mechanistic model of scientific explanation and theory structure

> **C**: Complexity Economics offers a methodological framework that is appropriate for modern economic science

Why do I believe that establishing the conclusions of these two arguments has warranted the amount of research time and ink spilled to produce this book? The theories that are constructed and propagated by the economics community have deep and profound effects on almost every aspect of our lives. If these theories have been constructed on an ill-informed basis, then it is likely that adherence to them by policy makers will have deleterious effects. This practical dimension of economic theorising has much more potentially destructive implications than the theoretical dimension of simply failing to provide valid explanations of economic phenomena.

Why do I believe that this is an appropriate time for the message of this book to be disseminated? A somewhat stretched historical comparison may provide some context. The onset of the Great Depression in the year 1929 spurred the Keynesian revolution. Classical economic theory was incapable of explaining the phenomena constituting the unfolding protracted decline in economic indicators. Fast forward to the year 2007 and the onset of the Global Financial Crisis, when all key economic indicators fell at a faster rate than they had during the early 1930s (Crafts & Fearon, 2010). Perhaps the environment is right for another revolution. In fact, in a recent article documenting the concerns that the most prominent central bank chiefs currently have regarding the adequacy of their models, the author states:

> "With central bankers credited for keeping the economic show on the road over the past decade, it will come as a shock to many to hear how little confidence they have in their models, their policies and their tools." (Giles, 2017).

This lack of confidence is most likely due to the fact that central bank forecasts have never been as inaccurate in the modern period as they have been since 2007 (Morgan, 2009, p.589). And as mentioned above, the consequences of using faulty models are not limited to failures of understanding and prediction, but also, as one academic economist has noted:

> "the central banks did not only fail to respond adequately and early to the potential for financial crisis—they contributed to it." (Morgan, 2009, p.953)

There are several intended audiences for this book. Firstly, those active in, or interested in, the discipline of the methodology of economics should find the content of more than just passing interest. I suspect that this audience will likely be the most critical of the arguments I put forward herein. Secondly, since my project can be viewed as an extended piece of descriptive analysis that assesses various schools of economic thought through the lens of the *new mechanical philosophy*, those with interests in general philosophy of science, specifically in the areas of scientific explanation and theory structure, should find this book of some interest. Thirdly, although I suspect them likely to be the most dismissive of this work, I hope that both academic and professional economists will find it, in the very least, interesting, and more hopefully, disturbing. Another audience that I believe would show interest in what I have to say herein, is the intelligent layman. Since this book covers a vast amount of separate literatures, it has been written with as little technical detail as possible, making it, hopefully, accessible to the non-academic reader.

This book is structured as follows. **Part 1** is concerned with the philosophy of science. Specifically, the sub-disciplines of scientific explanation and theory structure. In Chapter 1, I present and critique the Deductive Nomological model of scientific explanation that dominated the general philosophy of science literature for most of the twentieth century, and which arguably has remained the dominant account for working scientists within many scientific disciplines down to this day, despite being rejected by the philosophical community decades ago. In this chapter I also present several successor accounts to the Deductive Nomological model that have been developed in response to the deficiencies of that model. In each case, I show that these models do not appear to provide a basis for generating normative suggestions for theory construction and development.

In Chapter 2, I present the Neo-Mechanistic model of scientific explanation that has come to dominate the philosophical literature over the past two decades. I show that, in contrast to the models critiqued in Chapter 1, this model does in fact provide a basis for generating methodological norms for the construction and development of theoretical constructs. Succinctly put, the Neo-Mechanistic account states that a valid scientific explanation is one in which a representative model is provided of the mechanism responsible for the generation of the phenomenon to be explained.

Part 2 is concerned with the philosophy of economics. Specifically, it addresses issues of methodology. Chapter 3 explores the history of the methodology of economic science prior to modern mainstream economics. Beginning with the Classical School of economics, it also covers the Historical, Austrian and Institutionalist schools of thought, highlighting the philosophical influences that cemented the methodological convictions of each school. In all instances, I conclude that neo-mechanistic strictures are not satisfied. In Chapter 4 I repeat the exercise of the previous chapter, this time addressing modern mainstream economic methodological practice. I find that current practice also fails to conform to Neo-Mechanistic standards.

Part 3 explores the heterodox school of economic thought known as Complexity Economics. In Chapter 5, I examine the origins of this school of thought and explore the methodological convictions of its most prominent members. Chapter 6 evaluates the methodological framework of Complexity Economics in terms of its Neo-Mechanistic credentials. The findings are positive.

Part 4 presents a case study. In Chapter 7, I compare and contrast the standard contemporary asset pricing model with the Complexity Economics alternative. I argue there that while the standard model fails the normative criteria of the Neo-Mechanistic explanatory framework, the presented alternative appears to satisfy these same criteria.

Part 1: Philosophy of Science

Chapter 1 – Scientific Explanation & the Structure of Scientific Theories

The purpose of this chapter is to show that the account of scientific explanation that underlies the orthodox paradigm within Economic Science does not provide an adequate standard from which to build, develop and revise scientific theories within the discipline. I will establish this conclusion by presenting and critiquing this model.

1.1 Introduction

In this chapter, I present and critique the Deductive Nomological model of scientific explanation that dominated the general philosophy of science literature for most of the twentieth century, and which arguably has remained the dominant account for working scientists within many scientific disciplines down to this day, despite being rejected by the philosophical community decades ago. Also in this chapter, I present several successor accounts to the Deductive Nomological model that have been developed in response to the deficiencies of that model. In each case, I show that these models do not appear to provide a basis for generating normative suggestions for theory construction and development.

This chapter is structured as follows. In Section 1.2, I introduce the concept of scientific explanation via some preliminary comments. In Section 1.3, I present and critique the Deductive-Nomological model of scientific explanation. In Section 1.4, I present a variety of successor theories to the DN model, covering both top-down and bottom-up approaches across a number of categories, including those of *unificationist*, *causal*, *statistical* and *pragmatic*. I conclude in Section 1.5 by summing up the arguments presented.

1.2 Preliminaries

In this sub-section, I will motivate the research project by arguing that the concept of *explanation* lies at the heart of the scientific enterprise and by presenting a preliminary explication of the concept of scientific explanation.

1.2.1 Is Explanation a Goal of Science?

It is broadly agreed that three of the primary goals of the scientific enterprise are the *explanation*, *prediction* and *control* of the phenomena we encounter in the world. Of these three goals, explanation has proven by far the most controversial.

Michael Strevens opens his book Depth with the following assertion:

> "If science provides anything of intrinsic value, it is explanation. Prediction and control are useful, and success in any endeavour is gratifying, but when science is pursued as an end rather than a means, it is for the sake of understanding – the moment when a small, temporary being reaches out to touch the universe and makes contact." (Strevens, 2008, p.3).

This is a sentiment in which I heartily share, but it by no means represents an uncontested position. Over the centuries, many prominent philosophers of science - as well as philosophically minded scientists - have defended an alternative position; that the scientific enterprise merely provides descriptions of the world we encounter. The attitude underpinning this position is based on the belief that *explanation* is a suspicious metaphysical activity involving extra-empirical elements. This sentiment was forcefully endorsed by Karl Pearson in 1911:

> "Nobody believes that science explains anything; we all look upon it as a shorthand description, as an economy of thought." (Pearson, 1957, p.xi).

In 1923, pointing to a commonly-held view that to explain something is to demonstrate the necessary truth of a proposition, and noting that the experimental methods of science can detect no absolute or logical necessity in the phenomena which are the ultimate subject matter of every empirical enquiry, Ernest Hobson contended that:

> "The very common idea that it is the function of Natural Science to explain physical phenomena cannot be accepted as true unless the word 'explain' is used in a very limited sense…Natural Science describes, so far as it can, *how*, or in accordance with what rules, phenomena happen, but it is wholly incompetent to answer the question *why* they happen." (Hobson, 1923, pp.81-82, emphasis in original).

According to this viewpoint then, at best, the sciences can hope only to provide comprehensive and accurate systems of *description*, not of *explanation*. This methodological claim was also made by Paul Samuelson with reference to Economic

Science, over four decades later. Writing in the mid-1960s, Samuelson claimed that an explanation is simply:

> "…a better kind of description and not something that goes ultimately beyond description" (Samuelson, 1965a, p. 1165).

In 1961, Ernest Nagel disputed the opinion expressed by Karl Pearson, that *to explain* is to *demonstrate the necessity of*. Nagel argued that this commonly-held view was predicated on the false premise that there is a single context in which *why* questions can be raised. And so, breaking from the tradition of earlier positivists such as Auguste Comte[1] and Ernst Mach[2], the logical positivist movement expressly endorsed explanation as a legitimate goal of science. Writing in the heyday of the *received view*, Nagel stated that:

> "…the distinctive aim of the scientific enterprise is to provide systematic and responsibly supported explanations." (Nagel, 1961, p.15).

By 1984, Wesley Salmon could rightfully claim that:

> "It is now fashionable to say that science aims not merely at describing the world; it also provides *understanding*, *comprehension* and *enlightenment*. Science presumably accomplishes such high-sounding goals by supplying scientific explanations." (Salmon, 1984, p.9).

And in more modern times, both economists and philosophers of economics have placed high importance on the value of explanation in economic science, as the following quotes attest:

> "…the impossibility of engineering, and the absence of spontaneously occurring, closed social systems, necessitates a reliance on non-predictive, purely explanatory, criteria of theory development and assessment in the social sciences." (Lawson, 1997, p.35).

> "The main task of the social sciences is to explain social phenomena. It is not the only task, but it is the most important one, to which others are subordinated or on which they depend." (Elster, 2007, p.9).

> "All of the Sciences are known to have advanced from description to explanation" (Bunge, 2004, p.182).

1.2.2 What is Scientific Explanation?

When considering the concept of *Scientific Explanation*, two key contrasts are immediately suggested: scientific vs unscientific explanation; and explanation vs non-explanation.

I'll explore the second of these contrasts first. What are the distinguishing features of an explanation? The various models that have been developed to explicate the concept of scientific explanation disagree on what they consider to be the difference between explanation, and something less than explanation, such as for example, mere description.

This is a quest that dates back to at least the ancient Greeks, who sought to distinguish between knowledge *that* and knowledge *why* (Aristotle, Posterior Analytics, 71b18-25), that is, between *descriptive* knowledge and *explanatory* knowledge. For Aristotle, scientific explanations are deductive arguments.

It has been commonplace to regard an explanation as an answer to a *why* question. This means that in order to assess the validity of an explanation, a set of principles would need to be established for determining the validity of answers to *why* questions. Ernest Nagel argued that there are numerous ways in which *why* questions can be posed, creating different sets of explanatory requirements. He identified four different classes of *why* questions, thus delineating four explanatory structures: deductive; probabilistic; functional/teleological; and genetic (Nagel, 1961).

Sylvan Bromberger and Wesley Salmon (Bromberger, 1966; Salmon, 1984) argue that a request for scientific explanation can always be reframed as a *why* question. This is not however a universally accepted point. Peter Achinstein's Illocutionary Act model for example, is a model of scientific explanation intended to account for all manner of questions, not just *why* questions (Achinstein, 1983). And James Woodward states explicitly that his difference-making account of causal explanation is designed to account for a variety of explanatory claims (Woodward, 2003, p.4).

The neo-mechanist movement that dominates current discussion within the literature, also does not consider a scientific explanation to be an answer to a *why* question. Instead, adherents take the thing to be explained to be the phenomenon itself. Thus, for Carl Craver, the exhibition of a mechanism is an explanation; the mechanism itself is an explanation of the phenomenon it produces. William Bechtel disagrees with this highly ontic conception of explanation, proclaiming instead that it is not the mechanism itself that is an explanation, but rather, a description of the mechanism. What both *agree on* for the purposes here though, is that explanation is not restricted to the practice of answering why questions. The neo-mechanist model of scientific explanation is the subject of Chapter 2 below. It will be argued there that this model provides a suitable standard for assessing the explanatory validity of theoretical structures within the various branches of the sciences.

What about the other contrast then? What demarks a scientific explanation from a non-scientific one? This is a heavily contested, and controversial topic. The philosophical literature reflects this uncertainty by the way it draws its examples from all manner of explanatory practice. Ernest Nagel has recognised that:

> "…no sharp line separates beliefs generally subsumed under the familiar but vague rubric of "common sense" from those cognitive claims recognised as "scientific"." (Nagel, 1961, p.2).

But he does provide a few useful pointers to help navigate the divide (Nagel, 1961).

And Stephen Toulmin has argued that:

> "the search for a permanent and universal demarcation criterion, between 'scientific' and 'non-scientific' considerations, appears in vain." (Toulmin, 1972, p.259).

In Bas Van Fraassen's account of explanation – *Constructive Empiricism* - the only difference between a scientific explanation and an ordinary explanation, is that the former includes scientific information (van Fraassen, 1980). And James Woodward is quite clear that his account of causal explanation is designed to account for explanatory claims in everyday life (Woodward, 2003, p.4). Wesley Salmon is not so relaxed about the scientific vs non-scientific division of explanations. He argues strongly that it is only scientific explanations that the literature seeks to explicate, and that most *why* questions do not represent requests for scientific explanation (Salmon, 1984, pp.10-11). The obvious Neo-Mechanist response here is that scientific explanations aren't answers to why questions, they are instead, models of phenomena of interest to scientists.

A third important contrast runs through the literature on scientific explanation: ontological conceptions versus communicative conceptions. Pragmatic accounts of explanation focus on the communicative act of explanation. These accounts highlight the linguistic performances of explainers. For example, in Peter Achinstein's Illocutionary Act Model, the focus is firmly on the intention of the explainer to make information understandable. On the other hand, are those accounts that emphasise the relevant mind-independent facts. For example, causal models of most stripes emphasise the objective facts leading up to the phenomena to be explained. Salmon, for one, has spilt much ink expounding a requirement for *objective* relevance relations. This contrasting focus between ontological and communicative notions can be traced back to an early twentieth century disagreement between adherents of logical positivism and followers of Wittgensteinian ordinary language philosophy. The logical positivists emphasised the objective features of logical language, focusing on syntax and semantics, whereas Wittgensteinians emphasised the pragmatic features of ordinary language.

The vast majority of the contemporary literature on scientific explanation has arisen, in some way or another, in response to the classic Hempel-Oppenheim paper published in 1948. The model presented in that paper – known as the Deductive-Nomological (DN) model - was championed by the logical positivist movement, for its conformity to strict empiricist stipulations, in particular, its underlying Humean conception of causation. Logical positivist philosophy of science centred on an analysis of scientific theories as empirically interpreted deductive axiomatic systems. The DN model encompasses knowledge of both particular facts and general regularities. This model will be presented in Section 1.3.1 below, and critiqued in Section 1.3.2.

1.2.3 Methodological Monism

Francis Bacon is often cited as the fountainhead of modern scientific rationality (Perez-Ramoz, 1991). Although he primarily concerned himself with the physical sciences, Bacon conceived of his method as applicable to the social sciences as well, stating:

> "It may also be asked ... whether I speak of natural philosophy only, or whether I mean that the other sciences, logic, ethics and politics, should be carried by this method. Now I certainly mean what I have said to be understood of them all; and as the common logic ... extends ... to all science; so does mine also, which proceeds by induction, embrace everything." (Quoted in Rashid, 1985, p.246).

During the seventeenth century, Descartes and Newton, working within the dominant mechanical philosophy, sought a unifying framework for natural philosophy (science). This penchant for unification was also embraced by the logical positivist philosophers of science during the twentieth century, as will be shown below. Rudolph Carnap and Otto Neurath were particularly strong evangelists for monism (Carnap, 1934; Neurath, 1930). In this book I also champion methodological monism. However, as will be shown in subsequent chapters, asserting a Neo-Mechanistic explanatory framework for theoretical construction and development is consistent with a variety of pluralist positions concerning methodology.

1.3 The Deductive-Nomological Model

Modern accounts of scientific explanation have developed in response to perceived deficiencies in the Deductive-Nomological model. This model is presented and critiqued here so that the reader can more readily understand how the features of successor theories represent resolutions of long-debated issues.

1.3.1 The 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 covering-law model, and the subsumption theory). 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 Rudolf Carnap in the first quarter of the twentieth century (Carnap, 1923), with an early, classic, re-statement published in 1948 (Hempel & Oppenheim, 1948). Further comprehensive expositions by high profile philosophers were published by Carl Hempel, Richard Braithwaite, and Ernest Nagel (Hempel, 1965; Brathwaite, 1953; Nagel, 1961). Collectively, the elements propounded in this body of work became known as “the received view”.

According to Carl Hempel, a scientific explanation is an answer to a why-question, and if an explanation-seeking request is initially presented in some other form, it can always be restated in terms of a why-question. (Hempel, 1965, p.334). Succinctly put, under the DN account, a theory *explains* a phenomenon by showing how it was expected to result from a set of particular circumstances in accordance with the laws of nature. Hempel puts it like this:

> “…the argument shows that, given the particular circumstances and the laws in question, the occurrence of the phenomenon *was to be expected*; and it is in this sense that the explanation enables us to *understand why* the phenomenon occurred.” (Hempel, 1965, p.337, italics in original).

The essence of the DN model, is that a scientific explanation takes the form of a sound logical deduction from *explanans* to *explanandum*, where the *explanandum* 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. An explanation is thus a linguistic entity. This structure is represented by Hempel as:

C1, C2,…,Ck (facts)

L1, L2,…,Lr (laws)

------------------------ (logical implication)

E (that which is to be explained)

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. The structure can be illustrated by a simple example of event explanation:

Why did the price of oil rise?

L1: The Law of Demand (for all commodities, if the demand for a commodity increases, while the supply remains unchanged, the price increases)

C1: Oil is a commodity; **C2**: The demand for oil increased; **C3**: The supply of oil remained unchanged

E: Therefore, the price of oil rose

Underlying the DN model is a Humean conception of causation[3]. 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 1.4, 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 of these. These two implications provide significant points of contrast with the mechanistic model of scientific explanation that will be introduced in Chapter 2 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 an explanatory "theory of everything".

1.3.2 Criticisms of the DN Model

The DN model has faced criticism on a vast number of fronts. An early seminal piece of work cataloguing a broad range of substantial and technical issues, was published by Frederick Suppe (Suppe, 1974). 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.

I will briefly outline below, several of the most prominent objections that have been recurrently raised in the literature: the *symmetry* objection, the *irrelevance* objection; the appeal to *laws* objection; and rejection of the logical empiricist program.

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. Several inter-related problems relating to the *symmetrical* logical structure of the DN model have been especially prominent targets of criticism. The first of these regards *temporality*. It was a deliberate decision on the part of Hempel & Oppenheim to omit a temporality constraint from their model (Hempel, 1965, pp.317-318). A simple illustration will serve to show why such an omission creates problems for the model: It may seem reasonable to explain a consumer's choices on the basis of his/her preferences and beliefs, along with laws of decision-making, however, a deductive explanation of these choices on the basis of subsequent preferences and beliefs does not strike one as being satisfactory.

To illustrate this further, I'll revisit the oil price example in Section 1.3.1 above. The following patently false argument, according to the DN model, is a valid scientific explanation:

Why did the demand for oil increase?

C: The price of oil increased

L: The law of demand

E: Therefore, the demand for oil increased

A second problem relating to the symmetrical structure of the DN model regards *causality*. A primary reason why an explanation of a consumer's choices in terms of prior preferences and beliefs seems to have some merit whilst a symmetrical explanation in terms of subsequent preferences and beliefs appears intuitively unappealing, is because we can understand how prior states of affairs can *cause* subsequent states of affairs, but not vice versa. It therefore would appear that a satisfactory model of explanation must incorporate a notion of causal relations that is not symmetric in the way that Humean constant conjunction, simply constructed, is.

A third problem relating to the symmetrical structure of the DN model regards what Hempel refers to as *the thesis of structural identity*. The idea behind this thesis is that every adequate singular explanation is a potential prediction, and vice versa. This is a straightforward implication of Hempel's view that an adequate explanation is one for which the explanans provides grounds upon which the explanandum is to be *expected*, i.e., predicted. This thesis has two subcomponents, which, in Hempel's words, are:

> " (i) that *every adequate explanation is potentially a prediction*…;
>
> (ii) that conversely *every adequate prediction is potentially an explanation*," (Hempel, 1965, p.367)

Hempel maintained that sub-thesis (i) is correct, but due to concerns at the time with statistical explanations concerning probabilistic inference, he declared that sub-thesis (ii) was an open question. It has struck many as obvious however, that sub-thesis (ii) is clearly false, irrespective of probabilistic issues. The difficulty can be traced back to the failure of the causal relations as outlined above, since it is possible for adequate predictions to be constructed by conditioning on perfectly correlated instruments, without this information providing a reasonable basis for explanation.

The criticism of the structural identity thesis I have just identified also points to a broader problem for the sufficiency of the DN account of explanation. This broader problem may be referred to as the *irrelevance* objection. This objection relates to the situation where the law cited in the explanans is irrelevant to the explanation. As has been the case for the problems relating to symmetry, this objection has generated a number of counterexamples to illustrate the point. For example, the following (widely discussed) patently absurd explanation meets the DN criteria for validity (Kyburg, 1965):

P1 (L): 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

An equivalent example from economic science would be:

P1 (**L**): All highly leveraged companies that have bald chairpersons will be interest rate sensitive

P2: Company X is a highly leveraged company with a bald chairperson

C: Company X is interest rate sensitive

It is easily seen that these arguments are 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. But of course, both the hexing of the salt and the baldness of the chairperson are facts completely irrelevant for the respective conclusions.

Another category of irrelevance that poses problems for the DN model is that of common cause. When the occurrence of two different phenomena are effects of a common cause, we do not consider it appropriate to declare that either of the effects explains the other, however the DN model does not constrain the concept of explanation in such a common-sense way, so that the following would be considered valid (Salmon, 1989, p.47):

Why did the storm occur?

L: Whenever the readings of a barometer drop, a storm will occur

P: The barometer readings dropped

C: Therefore, a storm occurred

But of course, barometers do not cause storms. Storms and barometer readings are both common causes of drops in atmospheric pressure. The explanation does not therefore strike us as credible.

A third common objection to the DN model relates to the insistence for the citation of laws 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 (Woodward, 2017). And this is just the situation we find ourselves in. Hempel and Oppenheim characterised laws as be true *law-like* sentences conforming to the following stipulations (Hempel & Oppenheim, 1948):

1. They are *universal*;
2. They have *unlimited scope*;
3. They contain *no designation of particular objects*; &
4. They contain *only purely qualitative predicates*.

Despite this characterisation, Hempel lamented:

> "The characterization of laws as true lawlike sentences raises the important and intriguing problem of giving a clear characterization of lawlike sentences without, in turn, using the concept of law. This problem has proved to be highly recalcitrant…" (Hempel, 1965, p.338).

The DN model, with its Humean conception of causation, which views laws simply as universal regularities, thus has trouble distinguishing between genuine laws and accidental regularities. One reason why this strategy fails, is that it does not provide support for counterfactual inferences. For example, in the barometer-storm example above, we can see that the following counterfactual statement is false: if the barometer readings had not dropped, the storm would not have occurred. The storm would still have occurred because it was caused by a drop in atmospheric pressure, not by the drop in barometer readings. Had the barometer been broken for example, the storm would have occurred regardless.

More fundamentally, Nancy Cartwright takes issue with the concept of universal generalisation underlying so-called *laws*, claiming that wherever such laws hold, they only do so under extreme *ceteris paribus* conditions. She argues extensively that:

> "…the laws of physics apply only where its models fit, and that, apparently, includes only a very limited range of circumstances. Economics too…is confined to those very special situations that its models can represent." (Cartwright, 1999, p.4).

There is another problem with the DN model relating to laws that Hempel also never found an adequate resolution to. The DN model is intended to account for both individual events and general regularities. The model says that to explain a law (general regularity) is to derive it from other, more general, true laws. Translated into *expectability* language, this says that a law is explained by showing that its truth was to be expected, given the truth of other laws. However, in the early Hempel & Oppenheim paper of 1948 this idea was not explicated, due to an acknowledged inability of the authors to provide a solution to a self-presented counter-example. Almost twenty years later in an extended treatment of the DN model, Hempel also offers no explication[4]. The problem is that the DN structure allows for the derivation of a law statement from the conjunction of this very same law statement along with other statements, effectively permitting a law to explain itself. This can be illustrated as follows:

L1: The law of Demand & The Law of Gravity

Therefore,

E: The Law of Demand

Another objection targets the DN characterisation of explanation as an *argument*. It will be shown in Section 1.4 below that many successor models break with this idea. For example, Wesley Salmon's Statistical Relevance model reveals that an explanation is an assembly of information that is statistically relevant to an explanandum, and under the mechanistic model of scientific explanation (See: Chapter 2), an explanation of a phenomenon is the presentation of a particular type of representative model of the phenomenon.

The final category of criticism levelled against the DN model that I will discuss here, is the rejection of the logical positivist program upon which the DN model of explanatory structure is built upon. The program has long been considered as self-refuting. The primary assertion is that any statement that cannot be empirically tested is meaningless. However, the assertion itself is a statement that is not empirically testable. Further, cognitive meaning was initially tied to verification, so that unless some finite procedure could conclusively determine the truth of a proposition, it was considered meaningless. But of course, it is impossible to verify every instance of a universal statement to evaluate its truth, rendering such statements meaningless, and so Rudolph Carnap replaced *verification* with the weaker principle of *confirmation* (Carnap, 1936; 1937), and Alfred Ayer replaced verification with *weak verification* (Ayer, 1946). Carnap attempted to build a model of inductive logic in which probability is construed in terms of *degrees of confirmation*, while the idea behind Ayer's account is also that experience can render propositions probable.

Willard Van Orman Quine published a highly influential paper, which has been described as the most important in all of twentieth century philosophy (Godfrey-Smith, 2003, pp. 30-33), which challenged the analytic-synthetic distinction central to the logical positivist program (Quine, 1951). He did so by arguing that any term in any proposition gains its meaning contingently upon the speaker's conception of the entire world; meaning is holistic not atomistic. The logical positivist program has also been criticised along a number of alternative fronts, but since the associated arguments have less to do with the subject of this thesis, I will set them aside here.

Although the DN model has been almost universally rejected by philosophers of science for some decades now, the general principles behind the model remain highly influential within the social sciences, including economic science. As will be shown in Chapter 3, from the birth of economics as a separate scientific discipline in the nineteenth century, right through to current times, deductivism has held a core methodological position in the methodology of economics. Wade Hands remarked in 2001 that:

> "As it currently stands, the D-N model has been harshly criticized, but no other alternative model has gained enough support among philosophers of science to be seriously regarded as a viable replacement. The D-N model remains the standard, if highly criticized, characterisation of scientific explanation." (Hands, 2001, p.85)

Speaking on a similar note with regards to other sciences, and referring to the vast literature of fatal criticism of the DN model, James Woodward also noted in 2003 that:

> "…all this discussion has had surprisingly little impact on philosophers (e.g., those working in philosophy of psychology and biology) who are not themselves specialists in causation/explanation but who draw on ideas about these subjects in their own work. To the extent that there is any single dominant view among this group, it probably remains some hazy version of the DN model." (Woodward, 2003, p.4)

Coming back to economic science, the disregard of more recent developments in the philosophy of science by economic practitioners has been poignantly recognised by Lawrence Boland as recently as 2014, when he stated:

> "There was a time many decades ago when practicing academic economists were openly well versed in the latest view of the philosophy of science, but, needless to say, few if any economic model builders today see themselves in engaging in such an explicit philosophical program such as that which philosophers and would-be philosophers of economics today spend so much time discussing." (Boland, 2014, p.230)

1.4 Responses to the Deductive-Nomological Model

In the wake of the obvious shortcomings of the Deductive-Nomological model of scientific explanation, a number of alternative models were developed and promoted. These models have taken a wide variety of approaches. One dimension upon which these models can be differentiated is top-down versus bottom-up. Both of these categories include models that would appear to have application for economic science. I'll discuss the most prominent of each of these below.

1.4.1 Unificationist Models

Hempel, it was noted in Section 1.2.1 above, was wedded to the idea of scientific explanation as *nomic expectability*. But as Salmon points out, what elevates explanation above mere description in the DN model seems to be *deductive systematisation* (Salmon, 1989, p.131). Strevens also makes this same general point, using the term *pattern subsumption* (Strevens, 2008, p.10). What these two authors are suggesting, is that the force of understanding deriving from the descriptive knowledge required by the DN model, comes from the particular organisation of this body of descriptive knowledge. Both Salmon and Strevens make these claims with reference to passages from Hempel such as the following:

> "The understanding it conveys [scientific explanation] lies…in the insight that the explanandum fits into, or can be subsumed under, a system of uniformities represented by empirical laws or theoretical principles." (Hempel, 1965, p.488).

Unificationist models of scientific explanation elevate this principle to the position of key explanatory relation. The recognised founder of the unificationist approach is Michael Friedman. Friedman claims that although the DN model provides a "clear, precise, and simple condition" for the explanatory relation – logical entailment, and that it makes explanation relatively objective:

> "DN theorists have not succeeded in saying what it is about the explanation relation that provides understanding of the world." (Friedman, 1974, p.9).

Friedman goes on to state explicitly what he thinks provides scientific explanations with the power to induce understanding:

> "…this is the essence of scientific explanation – science increases our understanding of the world by reducing the total number of independent phenomena that we have to accept as ultimate or given." (Friedman, 1974, pp.14-15).

And he goes on to elaborate that:

> "…the kind of understanding provided by science is global rather than local. Scientific explanations do not confer intelligibility on individual phenomena by showing them to be somehow natural, necessary, familiar, or inevitable. However, our over-all understanding of the world is increased; our total picture of nature is simplified via a reduction in the number of independent phenomena that we have to accept as ultimate." (Friedman, 1974, p.18).

Unificationist accounts of scientific explanation thus view such endeavours as attempts to gather various different phenomena into unified accounts. Although Friedman is recognised as the founder of the unificationist approach, it is Philip Kitcher who has developed the model most extensively. As was the case with the DN model, unificationist models aim to remain faithful to a Humean conception of causation. Kitcher claims that:

> "…the 'because' of causation is always derivative from the 'because' of explanation." (Kitcher, 1989, p.477)

This attitude exemplifies the top-down approach to scientific explanation, in which explanatory relations are primary, and causal relations derivative. In making causal judgements, the story goes, we are simply pointing to relationships that derive from our attempts at creating unified accounts of phenomena; causal relations have no independent existence outside of our explanatory endeavours. Also in common with the DN account, Kitcher claims that:

> "…in a certain sense, all explanation is deductive." (Kitcher, 1989, p.448)

According to Kitcher's unificationist 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 (Strevens, 2004). This set of argument patterns is called the *explanatory store*. To show how the explanatory store is constructed, I'll very 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 together. 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.

This model has been subjected to many criticisms. One major criticism is the contention that the model fails to provide an account that is not merely descriptive, since the guiding principle seems to be simply one of descriptive economy. Another major criticism is that the 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 to some degree. Given these characteristics of the model, it does not seem to provide an adequate descriptive account of scientific explanation, let alone as a basis on which to build a normative standard for the generation and development of scientific theories.

Julian Reiss, however, claims that Phillip Kitcher's unificationist model (Kitcher, 1981) provides resources for launching an argument in defence of the claim that economic models accepted by the economics community as explanatory, are in fact so (Reiss, 2012). He argues that the model helps to make sense of the fact that theories are demanded to be mathematised, and to make use of the principles of rational choice theory and equilibrium. He states that:

> "…all these form part of argument patterns from which descriptions of a large range of empirical phenomena can be derived. A credible model is one that is explanatory *because* it is unifying" (Reiss, 2012, p.57).

Similar claims have been made by Uskali Maki, and Aki Lehtinen and Jaakko Kuorikoski (Maki, 2001; Lehtinen & Kuorikoski, 2007). (See also: (Reiss, 2002)). Maki states that:

> "I want to put forward three interrelated claims that I find uncontroversially true: first, much of the most respected parts of economics is motivated by the ideal of unification; second, many developments in economics are celebrated because they are regarded as advancing explanatory unification; and third, the claim that a given theory is not unified and that it does not unify is

> recognised by large portions of the economics profession as one of the most powerful arguments that can be used against a theory." (Maki, 2001, p.490).

Maki rightfully points out that vast swathes of contemporary theoretical achievements in economics are based upon market co-ordination and rational choice principles. Market co-ordination is a macro level principle that proceeds by way of finding equilibrium solutions using the laws of demand and supply. Rational choice is a micro level principle that relies on solving problems conceived as optimisation under constraint; individuals aim to optimise utility and firms aim to optimise profits. There have been prominent debates within the economics profession over the need for micro-foundations for macroeconomic theory. One way of interpreting the motivations of the proponents of this proposition, could be in terms of greater unification.

Some potential evidence for the unificationist viewpoint can be found in the imperialistic tendencies of economic theorising. One need only consider that for the majority of the most prominent economists of the classical period, the scope of economic science was confined to the material wealth accumulating activities of human beings. J.B. Say for instance defined political economy as:

> "Political economy, from facts always carefully observed, makes known to us the nature of wealth; from the knowledge of its nature deduces the means of its creation, unfolds the order of its distribution, and the phenomena at tending its destruction." (Say, 1880, p.11).

The neo-classical school led by Alfred Marshal accepted a broadened scope for economic science by changing the subject matter from material wealth, to human welfare, with the former being a means to the end of the latter. Marshal thus defined economic science as:

> "Economics is a study of mankind in the ordinary business of life; it examines that part of individual and social action which is most closely connected with the attainment and with the use of the material requisites of well-being. Thus it is on the one side a study of wealth; and on the other, and more important side, a part of the study of man." (Marshal, 1890, p.1).

In 1932, Lionel Robbins, attempting to eradicate logical inconsistencies in previous definitions of economic science, devised his own, which has dominated conventional understanding through to current times:

> "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses." (Robbins, 1932)

For the purposes here, the main characteristic of this definition is that it is universal, in that its purported laws are independent of all legal and political frameworks. By the mid-20th century, the Austrian School, expounding the theoretical framework of Praxeology developed by Ludwig von Mises, viewed economic science as only the most worked out branch of a larger, unified, science of Human Action. The categories of ends, means and alternative uses are, for this school of thought, logically implied by the notion of "human action" (see: Section 3.4.3).

Practical outcomes of this broadening scope are easily come by. Nobel laureate Gary Becker, for example, produced much work in traditional sociological fields, including racial discrimination, family organisation, crime, and drug addiction (Becker, 1964; 1968; 1971). James Buchannan, another Nobel laureate, is best remembered for his contribution to political theory. His *public choice theory* extended the economic concept of utility maximisation to the decision problems of politicians and bureaucrats (Buchanan & Tullock, 1962). As just one more example, Richard Posner is well known for extending economics into legal theory (Posner, 1973; 1981). However, in the aftermath of the global financial crises of 2008, Posner has questioned the rational choice theory basis upon which he has derived his theories of law and economics (Posner, 2009).

One can also look toward works within popular culture to observe an imperialistic unifying impulse within the realm of economic theorising. One prominent example is the best-selling series of books titled Freakonomics - a collaboration between Chicago economist Steven Levitt and journalist Stephen Dubner - whose success has spawned, amongst other things, a regular blog and radio show[5]. The authors propose to solve “the riddles of everyday life” by using cornerstone concepts from economic science.

But are these unifying exercises successful in terms of the unificationist models explicated within the philosophy of science literature? Reiss gets it right when he laments that:

> “It is unfortunate, therefore, that the argument patterns economics tends to produce are at best spuriously unifying…Whatever economists think when they say they provide explanations of this or that phenomenon, the accounts they give are not explanatory qua the unifying power of the argument patterns from which they are derived.” (Reiss, 2012, pp.58-59).

So, the top-down approach to scientific explanation as codified in prominent unificationist models neither provides a suitable normative standard for economic science, nor does it provide a credible descriptive account.

1.4.2 Causal Models

The dominant accounts of scientific explanation that sprang up in the wake of the widely recognised failures of the DN model, and that have remained highly relevant right through to current times, have taken a bottom-up approach. The vast majority of these centred on a re-characterisation of causal relations. Wesley Salmon's response was typical, when he stated that the time had come:

> "…to put "cause" back into "because"". (Salmon, 1977, p.160)."

In this sub-section, I'll present three of these models: Wesley Salmon's *Causal Mechanical* model; Michael Strevens' *Kairetic* model; and James Woodward's *Difference-Making* model.

1.4.2.1 Salmon's Causal Mechanical Model

As was shown above in Section 1.3, the particularly bare conception of causation embedded in the DN model is incapable of distinguishing between genuine causal relations and purely accidental regularities. *Process theories* of causation were developed partially in response to this problem. Wesley Salmon's *Causal Mechanical* model is the most prominent of this type of account (Salmon, 1984). The conception of causality underlying this account is one that construes it as being a feature of continuous processes, rather than 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 *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)

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 Causal Mechanical model 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.

The Causal Mechanical model as presented by Salmon is obviously not a suitable model of causal explanation for Economic Science, since the requirement of citing spatiotemporally continuous causal processes is not appropriate. For example, to require a stock market model to cite such features in an explanation of security price determination is simply ludicrous. This same issue also creates severe problems for the application of the model in other higher-level sciences such as Biology and Psychology. Phil Dowe has modified and extended Salmon's Causal Mechanical model with his Conserved Quantity model (Dowe, 2000), but the primary problems remain.

It has also been widely appreciated that the causal nexuses resulting in phenomena contain both elements that appear essential to the production of the phenomena to be explained, as well as factors that would appear to make little difference. Michael Strevens has argued that this problem of discerning the distinction between relevant and irrelevant causal factors points to the need for a modular two-factor approach to causal explanation (Strevens, 2008). In his *Kairetic* model of scientific explanation, Strevens delineates two separate concerns. The first concern is to establish the relevant metaphysical account of causal dependence. The second concern is to establish a relevance relation that picks out the correct causal factors in a particular explanatory case.

1.4.2.2 The Kairetic Model

The Kairetic (K) model was developed by Michael Strevens (Strevens, 2004; 2008). The K model is an attempt to appropriate the technical apparatus of the unificationist model 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).

And so, for Strevens, 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: Section 1.4.4.2 below). In this approach, as will be 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 (Lewis, 1986a) and James Woodward (see: Section 1.4.2.3 below). 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[6] and argues that attempted solutions should be considered failures[7]. And so, given the perceived deficiencies of the probabilistic and counterfactual approaches, 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 unificationist, causal mechanical and difference-making models. It incorporates: the cohesion criteria of the unificationist model within the abstraction process; the appeal to causal mechanisms of the causal mechanical model; and, although not referred to above due to space limitations, nods in the direction of the counterfactual dependence approach of the difference-making model in the exposition of *entanglement* as an explanatory relevance relation in the explanation of generalisations[8]. It also exhibits a pragmatic dimension through the concept of frameworked explanation[9].

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

1.4.2.3 Woodward's Difference-Making Model

The Difference-Making (DM) model is a causal account of scientific explanation associated with James Woodward (Woodward, 2000; 2003). 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.

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 than 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[10]. Nevertheless, a model of explanation that incorporates a non-reductive account of causation is not *a priori* inferior to one that does. This is especially true where the overarching motivation is to provide a practical account for working scientists.

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. Our initial intuitions might suggest that our notions of explanation are not relativised in such a way. However, some consideration of the pragmatic elements of the explanatory enterprise reveals that in practice they are.

Technical issues aside, 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 Chapter 2.

1.4.3 Pragmatic Models

Pragmatic models of scientific explanation have been developed in recognition of the fact that explanatory requests are not exhausted by their syntactic and semantic expression. I present here the two most prominent and influential of the pragmatic models that have been developed: Bas van Fraassen's *Constructive Empiricism* model and Peter Achinstein's *Illocutionary Act* model. Both models will be shown to be unsuitable for the purposes of my thesis.

1.4.3.1 Constructive Empiricism Model

Bas van Fraassen 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 (van Fraassen, 1980). Instead, he considers explanation to be merely a pragmatic virtue of theories. Van Fraassen 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 Fraassen, 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 (van Fraassen, 1980, p.156). Van Fraassen'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 (Kitcher & Salmon, 1987). 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.

1.4.3.2 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 (Achinstein, 1983; 2010).

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** (Achinstein, 1983, p.13).

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 can also be appropriate without being true. By appealing to the truth conditions of the proposition expressed by **U**, Achinstein avoids the subjectivism inherent in van Fraassen'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.

1.4.4 Statistical Models

Before I move on to a presentation and discussion of the mechanistic model of scientific explanation in the next chapter, one more prominent bottom-up approach needs to be discussed. This is the category of statistical explanation. I'll present here the most prominent of these models: Carl Hempel's *Deductive Statistical* and *Inductive Statistical* models, and Wesley Salmon's response to these in the form of his *Statistical Relevance* model. Although I do not provide an explication of his model, I introduce the ideas of

Patrick Suppes on the topic as well, to provide an account that was developed with economic science as its primary motivation and application. Statistical modelling is highly relevant to the goals and methods of the discipline of econometrics.

1.4.4.1 Hempel's Inductive-Statistical Model

The first statistical model of scientific explanation to receive widespread interest in the literature was developed by Carl Hempel (Hempel, 1965, pp.376-412). The model comes in two variants: Deductive-Statistical (DS) and Inductive-Statistical (IS). The DS model applies to the explanation of statistical laws. It is the statistical version of the DN model. Accordingly, a statistical law is explained by deriving it from an explanans that contains, indispensably, at least one statistical law. Obviously, the same issues faced by the DN explanation of laws outlined in Section 1.3 above also prove fatal for DS. This leaves us with IS explanations.

The IS model is intended to apply to the explanation of individual events, and was also developed as a (supposedly) simple analogue of the DN model. Whereas the conclusion of a DN argument – the explanandum – is to be expected with certainty, given the premises – the explanans – the conclusion of an IS explanation is to be expected with *high probability*. Although a precise specification of the model requires a number of technical details, the general idea can be represented by the following schema:

P (G,F) = r (statistical law)

Fb (fact)

----------------- [r]

Gb (explanandum)

What this schema says, is that a valid explanation of the fact that an individual case **b** is **G**, is that the probability of an individual case being both **F** and **G** is **r** (statistical law), where **r** is close to 1, and that the individual case **b** is **F**.

The idea can be illustrated by a simple economics example. We can represent the law of demand as a statistical law, by incorporating recognition of both *Veblen* effects and *Giffen* effects. The law of demand states that the quantity demanded of a good is inversely related to its price. A Veblen consumer is one who violates this law. This type of consumer is attracted to certain goods *because* they are expensive. Giffen consumers also violate the law of demand. These consumers will switch some consumption out of goods (e.g., bread) as they get cheaper, substituting into higher-quality goods (e.g., meat), because they can now afford to do so. Let's assume that the combination of Veblen and Giffen effects is 0.01. The argument structure would look like this:

L1: The probability that the demand by individual i, for good b increases when the price of good b decreases, all other things equal, is 0.99 (statistical law)

P1: The price of good p decreased (fact)

C: The demand by individual i for good b is expected to increase with probability 0.99

So, if it were observed that the demand for good **b** by individual **i** increased, an explanation of this phenomenon, according to IS, would cite the statistical version of the law of demand along with the fact that the price decreased. An obvious problem presents itself here. If we apply this explanation to an individual **j** for whom good **b** is a Giffen good, it seems obviously inappropriate to cite the same explanans as an explanation for why the demand for good **b** by individual **j** *did not* increase.

1.4.4.2 Salmon's Statistical Relevance Model

Wesley Salmon has been the most vocal critic of the IS model. Besides the objection cited above, Salmon presented a number of criticisms of the model that I need not go into here. In response, he developed the Statistical Relevance (SR) model (Salmon, 1971). SR is a response to both DN and IS. This model incorporates 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: $\mathbf{P(x{=}B|A.C) \neq P(x{=}B|A)}$. In words, 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) ≠ P(x=B|A.Cj)** for all **Ci ≠ 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 (Salmon, 1971, pp. 76-77):

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

This can be illustrated using the same example used for Hempel's IS model above. The conditions need to be modified slightly to avoid determinism. Suppose that for both Veblen and Giffen consumers, each has a different price threshold above which the effects are triggered. At any particular price point, the Veblen effect is triggered by 20% of Veblen consumers and the Giffen effect is triggered by 50% of Giffen consumers. Suppose further that normal consumers have 1% rate of spontaneous law-of-demand-violation, for some unspecified reason (or set of reasons). Now, assume that the proportion of consumers in each category is: Normal – 90%; Veblen – 5%; and Geffen – 5%. According to SR, an explanation of the fact that the demand for product **b** by individual **j** increased when the price of good **b** decreased would look as follows:

1. The probability that the demand for good b by any individual increases, given the price of good b decreases is 0.956[11]
2. (i) The probability that the demand for good b by an individual who is not a Veblen consumer, nor a Giffen consumer, increases, is 0.9900; (ii) the probability that the demand for good b by an individual who is a Veblen consumer increases, is 0.8000; (iii) the probability that the demand for good b by an individual who is a Giffen consumer increases, is 0.5000
3. Consumer i is neither a Veblen consumer nor a Giffen consumer

At first sight, this seems to solve the problem identified above for IS. However, Salmon is adamant that the only way a perfectly homogenous partition can be attained for this type of situation, is to partition on every single individual, since there will be some difference, no matter how small, between everyone. Salmon intends his model to be capable of capturing fundamental indeterminism, and so the spirit of his program requires that there be some uncertainty within each cell; these are statistical relationships, not deterministic relationships. Once we realise this point, it is immediately evident that exactly the same problem encountered by IS also plagues SR: the same explanans are 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.

1.4.4.3 Patrick Suppes' Probabilistic Model

Pattrick Suppes sought to save the empiricist world view from the defects of logical positivism. He set out to achieve this by replacing the notion of logic with that of probability as the central element within epistemology, de-emphasising the linguistic analysis of syntactical structure within the philosophy of science, and focusing instead on the complex procedures of measuring and model building. He referred to his position as *probabilistic empiricism*. Suppes stated that:

> "It is probabilistic rather than merely logical concepts that provide a rich enough framework to justify both our ordinary ways of thinking about the world and our scientific methods of investigation." (Suppes, 1984, p.2)

Suppes' efforts represent a move away from the *received view* toward a pragmatist philosophy in which scientific activity is conceived of as perpetual problem solving and scientific theories are viewed typically as local constructs (Galavotti, 1994, p.248). Suppes was a staunch empiricist with a belief in methodological plurality, built around a hierarchy of models: models of theory, models of experiments, and models of data. He developed a statistical relevance model (Suppes, 1970) with the primary goal of creating a probabilistic theory of causation. Suppes was firmly convinced that no strict linkage between causality and explanation exists. Suppes rejected the approach of maximum specificity and homogeneous reference classes espoused by Wesley Salmon. He avoided the problem of *total evidence* by implementing Bayesian techniques (Suppes, 1980, p.56; Suppes, 1966).

Suppes' probabilistic theory proceeds by way of two steps. First step: a factor **C** is a *prima facie* cause of a factor **E** if **C** raises the probability of **E**. Second step: a *prima facie* cause is a *real* cause if and only if **C** continues to increase the conditional probability of **E** in sub-populations that are homogenous with respect to all other potential confounding factors of **E**.

Suppes applied his model directly to economic science for the development of economic theory, and it has been noted that this model also inspired Clive Granger to develop econometric methods for the detection of causal relationships between time series' that have become a staple within the literature (Maziarz, 2015, p.91). New methods for causal inference in econometrics - known as *Bayes-nets methods* - have been built upon the definitions of *cause* produced by Suppes (Spohn, 1980; Pearl, 2000; Spirtes, et al., 1993).

Suppes' account is plagued by an issue that infects all those relying on stratification: purely probabilistic causes, where causes produce effects in tandem, cannot be adequately dealt with (Cartwright, 2002, p.7). An increase in the conditional probability of one factor on another in such cases will not be a sufficient condition for the establishment of a causal relation. It has been shown that Suppes' model fails to distinguish between genuine and spurious causes and between direct and indirect causes (Otte, 1981).

1.5 Conclusions

In this chapter, I presented and critiqued the Deductive-Nomological model of scientific explanation. This model dominated the literature during the twentieth century and arguably remains the dominant account within a number of scientific disciplines. It was shown how under this account, a scientific explanation is a deductive argument that shows how a phenomenon was to be expected, given the laws of nature and the particular circumstances. It was further shown how a number of defects of the Deductive-Nomological model of scientific explanation have resulted in its rejection by the philosophical community. Specifically, it was explained how objections relating to *symmetry*, *irrelevance* and *laws*, have undermined the model.

Following discussion of the Deductive-Nomological model, I introduced several successor theories, which have been developed across a number of various categories in response to the failures of their predecessor. In that section, I explained the main features of the most prominent *unificationist*, *causal*, *statistical*, and *pragmatic* models.

The primary conclusions drawn from the arguments presented in this chapter are, firstly, that the concept of scientific explanation sits at the centre of the scientific endeavour, and secondly, that the Deductive-Nomological model of scientific explanation, along with all the successor models discussed, fail to provide adequate normative stipulations for working scientists.

The following chapter presents and explains what has come to be the dominant modern account of scientific explanation: the *mechanistic model of scientific explanation*. 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. It is this model that will be adopted as a normative standard to be applied throughout the remaining chapters of this book.

Chapter 2 – The Mechanistic Model of Scientific Explanation & Theory Structure

The purpose of this chapter is to present the Neo-Mechanistic model of scientific explanation, and to argue that, unlike the models discussed in Chapter 1, this model does provide a basis for constructing, developing and revising theories within the discipline of Economic Science.

2.1 Introduction

In recent decades, a new mechanistic philosophy has generated a lot of attention in the philosophy of science 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).

According to another prominent figure within the movement, this mechanistic turn represents a sea change in philosophical thinking in the new century that is here to stay (Glennan, 2017, pp. 239-240).

In this chapter I first present some background to what has become known as the *new mechanical philosophy* in Section 2.2. Next, I outline the mechanistic model of scientific explanation in Section 2.3. In Section 2.4, I explore some metaphysical issues that arise in response to the new mechanistic philosophy. In Section 2.5 I discuss how the mechanistic model relates to several important concepts in the general philosophy of science. Next, in Section 2.6, I present, and respond to, several objections to the mechanistic model. Then, in Section 2.7, I will address a number of positions within the methodology of economics and social sciences literature that lay claim to being *mechanistic*, but in fact by Neo-Mechanistic stipulations cannot be said to be so. Finally, in Section 2.8, I summarise the conclusions of this chapter.

2.2 The New Mechanistic Philosophy

A body of research has emerged over the past two decades within the philosophy of science literature known as, amongst other labels, the *New Mechanical Philosophy*[1]. It asserts that most, if not all, the phenomena found in nature depends on mechanisms. The primary aim of science, according to the proponents of this philosophy, is the construction of models that describe, explain, and predict these mechanism-dependent phenomena (Glennan, 2017, p.1). Whereas previous models of scientific explanation (see: Chapter 1) appeal to general laws of nature, neo-mechanists view laws as heuristic devices, and instead believe that scientific methods are aimed at the discovery and representation of mechanisms that are local, heterogeneous, and particular.

Mechanical philosophies can be traced back to the ancient Greek atomists Democritus and Epicurus (Popa, 2017, pp. 14-16). An explosion of mechanical philosophies occurred in the seventeenth century and is exemplified in the works of Rene Descartes and others, who objected to the dominant Aristotelian approach that gave teleological explanation great importance. Robert Boyle, for example, proposed that all natural phenomena could be explained by matter and motion alone (Roux, 2017, p.26).

Eighteenth and nineteenth century mechanical philosophies shifted from a focus on explaining properties of inanimate objects to a focus on explaining properties of living systems. There was widespread concern during these times that the basic mechanistic schema was inadequate for biological explanation (Bechtel, 2011, p.534). This sparked a vitalist opposition, which rightly criticised the sequential organisation that was a feature of the contemporary mechanistic accounts. But by the end of the nineteenth century, vitalism had been relegated to the fringes of biological science as mechanistic approaches developed and flourished (Bechtel, 2008, p.12).

The goals and methods of mechanical philosophers ran afoul of the extreme empiricism that swept through the philosophical world in the early- and mid-twentieth century. However, the extensive research programs initiated within branches of the general philosophy of science in the 1990s on the back of widespread dissatisfaction with the positions associated with logical empiricism have culminated in the New Mechanical Philosophy. This new form of mechanical philosophy is said to differ from its historical antecedents in two primary ways (Glennan, 2017, pp.6-7). Firstly, the neo-mechanists are not necessarily committed to atomism. Their models emphasise that nature is arranged hierarchically; at different levels, new kinds of entities, activities, and interactions emerge. Secondly, it is stressed that there are important differences between *mechanisms* and *machines*. It is often the limited conception of mechanism, which assimilates it to human-made machines, that unjustifiably drives opponents of the mechanistic program (Bechtel, 2008, p.2). Whereas machines are *responsive*, in that they passively accept inputs and generate outputs, mechanisms – especially those within biological and social systems – are often characteristically *active*; they are, or are parts of, organised systems that control the flow of matter and energy in ways that serve to maintain themselves (Bechtel, 2008, Chapter 6).

2.3 The Mechanistic Model of Scientific Explanation

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 Chapter 1. It has been noted that the science of chemistry is the "original home" of mechanistic explanation (Weininger, 2014; Weisberg, Needham & Hendry, 2019). 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. According to Craver, the DN model, along with other models of scientific explanation, is pitched too abstractly to capture recurrent non-formal patterns (Craver, 2002, p.55). Mechanism schemata on the other hand, are claimed to be capable of successfully capturing such diverse phenomena.

The mechanistic model draws heavily on the concept of a causal mechanism from Salmon's CM account (see: Section 1.4.2.1 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 1.4.2.3 above). It combines all of these elements, and more, 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 (Craver, 2002).

2.3.1 Definitions

Although several definitions of *mechanism* have been proposed in the literature (Hedstrom & Ylikoski, 2010), the central features of the mechanistic approach are broadly consistent across the major works of the most prolific authors in this space. The following five 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).

> "A mechanism for a phenomenon consists of entities and activities organised in such a way that they are responsible for the phenomenon." (Illari & Williamson, 2012, p.120)

> "A mechanism underlying a behaviour is a complex system which produces that behaviour by the interaction of a number of parts according to direct causal laws." (Glennan, 1996, p.52)

> "A mechanism for a phenomenon consists of entities (or parts) whose activities and interactions are organised so as to be responsible for the phenomenon." (Glennan, 2017, p.2).

Stuart Glennan's second definition above is intended to define a *minimal mechanism*, by which he means that it provides necessary, yet not sufficient conditions for mechanism-hood. His purpose in defining mechanism in this way is to provide an ontological characterisation of what mechanisms are as things in the world, as a set of commitments that most new mechanists are, or should be, committed to (Glennan, 2017, p.18). The Machamer, Darden and Craver definition is designed to satisfy descriptive, epistemic and metaphysical needs. Glennan's minimal mechanism definition is quite permissive. Since he argues that mechanisms constitute the causal structure of the world, he ensures that his definition is capable of identifying all causal processes as mechanisms (Glennan, 2017, p.13).

The key terms in the definitions of mechanism provided are: *entities* (parts); *activities* (operations); *interactions*; *organisation*; and *phenomenon*. These concepts will now be explored and elaborated upon. Throughout this book I will predominately use the terminology favoured by Craver and Glennan (entities and activities).

2.3.2 Phenomenon

Mechanistic explanations are not directed at the explanation of *data*. Instead, *phenomena* are the targets of mechanistic explanation (Bogen & Woodward, 1988). Data provide evidence for the existence of phenomena (Craver & Darden, 2013, p.54). Craver notes that the central normative requirement of a mechanistic explanation is that it account completely for the explanandum phenomenon (Craver, 2006, pp.368-383). It is crucial therefore that an adequate characterisation of the phenomenon of interest be constructed. Phenomena to be explained mechanistically includes both individual events, and regularities. The capacities of entities, whether manifested or not, are also targets for mechanistic explanation (Glennan, 2017, p.25). Often the phenomenon being studied will need to be revised as investigations proceed. Such investigations will typically involve experimental interventions that control the values of variables hypothesised to affect the mechanism (Bechtel, 2008, p.38). This process has been referred to as *reconstituting the phenomenon* (Bechtel & Richardson, 1993).

Craver provides five criteria for establishing that a mechanism can fully account for the target phenomenon. 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.

2.3.3 Entities

The entities featuring in mechanistic explanations are not abstract; they are concrete particulars located in space and time. Stuart Glennan provides a set of necessary conditions for entity-hood (Glennan, 2017, p.34):

> **E1**: Entities are what engage in activities and interactions
>
> **E2**: Entities have locations in space and are stable bearers of causal powers (or capacities) over time
>
> **E3**: The causal powers or capacities of entities are what allow them to engage in activities and thereby produce change
>
> **E4**: Most or all entities are systems composed of parts and most or all of the powers of entities will be mechanism-dependent

Condition **E1** simply tells us that the productive activities of mechanisms require actors (entities). Concerning condition **E2**, Glennan tells us that entities must be concrete objects with spatial locations, but that this does not preclude them from being diversely spread out and even overlapping with other entities. In fact, this will often be the case for social mechanisms. Take for instance the example of the management team of a company. One could expect that for the explanation of many economic phenomena this will be considered a valid entity within a mechanistic decomposition. While the boundaries of such an entity are unlikely to be profitably defined in spatial terms, there is no doubt that such entities do have definite locations in space. What truly defines the boundaries of such entities, mechanistically speaking, are the interfaces where they interact with other entities – causal boundaries – within the mechanisms of which they are part. And these boundaries will be different depending upon the phenomena to be explained. Condition **E2** also refers to entities as "stable bearers of causal powers". Glennan states that this stability involves maintaining a cluster of properties over time in the face of perturbations, and relates this to manipulability criteria: that mechanisms are to be decomposed into entities in a way such that it is possible to intervene upon the entities to alter the behaviour of the associated mechanisms. Condition **E3** asserts that it is the capacities of entities that enable them to engage in productive relations. Condition **E4** states that most, if not all, entities are themselves decomposable into further entities, activities and interactions, that is, that typically, entities are themselves mechanistically constituted. The qualifying phrase "most or all" is inserted in condition **E4** to leave open the possibility that there exists a base level of physical entities whose dispositions are brute facts grounding reality.

Carl Craver stresses that valid mechanistic explanations feature real components, as opposed to fictional posits. He provides five criteria for making this distinction (Craver, 2006). 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.

2.3.4 Activities

Stuart Glennan also provides a list of necessary conditions for activities, including interactions (Glennan, 2017, p.31):

> **A1**: Activities require entities (parts, components) to act and be acted upon
>
> **A2**: Activities produce change in entities (parts, components) that act or are acted upon
>
> **A3**: Activities manifest the powers (capacities) of the entities involved in the activity
>
> **A4**: Activities are temporally extended processes
>
> **A5**: Most or all activities are mechanism-dependent

Condition **A1** tells us that activities are activities *of* entities. Bechtel refers to activities as *operations*. He prefers this terminology because it emphasises both acting and being acted upon (Bechtel, 2008, p.14). Condition **A2** covers both monadic activities and polyadic interactions. With reference to condition **A3**, Glennan states that activities represent "actual doings", whereas powers express "capacities or dispositions not yet manifested", and that activities are not merely things that happen passively to entities; they are the active doings of entities. Condition **A4** stresses that activities, including interactions, are temporally extended transmitters of causal influence; they are not merely intersections of extensionless points. Activities may be broken up into sub-activities. Since singular activities may be embedded within numerous larger processes, the specification of starting and ending stages will be dependent upon which larger processes explanation is sought for. The boundaries of these activities also need to be considered spatially, that is, how many entities are involved in the activity and how spread out they are through space.

According to Glennan, condition **A5** highlights the most important aspect of activities: that "the productive character of activities derives from the productive relations between intermediaries in the process". He emphasises that he is not presenting a reductive account of causal productivity; his account of mechanism-dependent production explains productions in terms of other productions. Condition **A5** includes the qualifying phrase "most or all" as was the case for condition **E4** above. And here, it reflects the same concern that it does there: it leaves open the possibility that there is a base level of activities or interactions that are dependent upon brute dispositions.

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 Carl Craver to endorse a manipulationist criteria as a means of restricting the type of input-output relationships that can count as explanatory (Craver, 2006)[2].

2.3.5 Organisation

Entities and activities need to be organised appropriately to constitute a working mechanism. This organisational aspect is critical: it is this organisation that results in the mechanism engaging in behaviour that is different from those of its parts and requires a different descriptive vocabulary. Mechanisms are not mere aggregates.

Organisation can be defined in both horizontal – spatio-temporal and causal - and vertical – the relationship between a mechanism as a whole and the collective organised activities and interactions of its parts - dimensions. The latter is referred to as *mechanistic constitution*. It is a multi-level relation.

Mechanisms are always contextually situated. They function within environments, and their behaviours will often be altered by the conditions in their environments (Bechtel, 2008, p.17).

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 (Craver, 2006, pp. 368-383). This checklist is useful for exercises such as that conducted in Chapters 3 through 7, wherein the works of 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 mechanistic categories of: the explanandum phenomenon, and the parts, activities, and organisation of the mechanism.

2.4 Metaphysical Issues

As the mechanistic theories of scientific explanation that began developing at the turn of the century have blossomed into a fully-fledged movement within the philosophy of science and attendant literatures, a number of metaphysical implications of rejecting the *received view* developed by the logical positivists in favour of the new mechanistic perspective have gained greater attention. Initially, in the move away from laws and theories toward mechanisms and models, commitments were limited to an acceptance that mechanisms are real things existing in the world and that the concept of causation incorporated need be metaphysically fuller than that of the Humean regularity thesis. I will briefly address below some issues that have been highlighted more prominently in more recent times.

2.4.1 Mechanistic Ontology

Ever since the publication of the pioneering articles on the mechanistic model of scientific explanation, it has been recognised that some level of realist commitment is required of adherents. But as more nuanced positions have been staked out, recognition has set in that more fundamental differences exist between the perspectives of the Neo-Mechanists and those of standard metaphysics. Stuart Glennan has provided some commentary around how acceptance of the core ideas of the new mechanistic philosophy need cause one to rethink positions concerning core metaphysical concepts such as *substance*, *property*, *relation*, and *law*. This is a natural outcome of the way that he describes the scope of the New Mechanical Philosophy, which according to Glennan, is both a philosophy of nature and a philosophy of science.

Substances are commonly described as being particulars located in space and time, *properties* are commonly held to be instantiated in particular substances, and *relations* of various kinds are said to obtain between substances.

The mechanistic category of *entity* is the least problematic to map onto traditional metaphysical categories. In so far as composite entities are considered genuine substances, entities simply map on to substances.

Mechanisms are - by definition - composites. Glennan tells us that the three most prominent ways of relating the properties of simple substances to composite substances, via the relations of supervenience, realisation, and grounding, are problematic, and should be rethought along mechanistic lines. He states that:

> "Mechanism-dependence gives an account of how realisation works, or why supervenience holds" (Glennan, 2017, p.44, ch2)

The most problematic mechanistic category appears to be that of activities. This category cannot be simply reduced to the categories of properties and/or relations. The metaphysical commitments implicit in an acceptance of the new mechanistic approach have also been explored by a number of other authors (see, for example: Illari & Williamson, 2011; Beate, 2018).

2.4.2 Models

Models are the central vehicles for representing the world and its causal structure. The Neo-Mechanistic explanatory project is part of a broader movement away from the concepts of *theory* and *laws* to those of *models* and *mechanisms*. What is known as the *semantic view of theories*, postulates that *theories* are families of models (Suppe, 1977; Winther, 2016). The mechanistic viewpoint agrees with this assertion, that scientific theories are simply collections of models, but it provides a distinctive account of what these models are and how they are combined. Stuart Glennan follows a schema devised by Ronald Giere to highlight the modelling enterprise (Glennan, 2017; Giere, 2004):

> **S** uses **X** to represent **W** for purposes **P**

The idea here is that **S** – an individual scientist, group of scientists, or the scientific community as a whole – forms a *theoretical hypothesis* that **X** – the model – resembles **W** – the targeted piece of the world – in some way or ways that accord with **P** – **S**'s purposes. One important implication of this schema is that it identifies models as our source of generality: a single model is often capable of representing a whole class of targets via the fundamental hypothesis. And, we can see that in determining whether "**X** resembles **W**" to some degree or in some respects - which depends upon the purposes of the modeller - processes of *abstraction* and *idealisation* will necessarily be involved (Glennan, 2017, pp. 73-83). Abstraction is a process of omitting details from a representation of a target. Within mechanistic models this will result primarily from omitting irrelevant (for purpose) details of the entities, activities and interactions responsible for the mechanism's phenomenon. Such omissions do not affect the viridity of the model. Idealisation on the other hand, is a process that introduces distortions into the representation of a target. Models produced in this way will be false in some respects. Much of the value in such models will be of a heuristic nature: they suggest further lines of research that may uncover actual entities, operations and interactions.

Models are representations. Mechanistic models are representations of mechanisms. They have two parts: a model of the phenomenal description and a model of a mechanism description (Glennan, 2005; 2017). The phenomenal description describes the overall behaviour of the mechanism. It describes what the mechanism does. The mechanism description describes the mechanism's parts and their functional arrangement. It describes how the mechanism is doing what it is doing. The two components of mechanistic models are not syntactically defined entities; they are semantic entities. There will exist many different syntactical formulations of the same description. The division into phenomenal and mechanistic descriptions is analogous to that between *explanandum* and *explanans* where the latter brings about the former and thus *explains* it (Glennan, 2005, p.448). Note that the phenomenal description may be a law statement, in which case it is explained by the mechanistic description.

Stuart Glennan highlights two further points that are important to keep in mind concerning the construction of mechanistic models. Firstly, the concept of the behaviour of a mechanism presupposes the idea of *normal* functioning. This implies a kind of *ceterus paribus* clause for the phenomenal and mechanism descriptions. Secondly, there is a one-to-many relationship between the phenomenal and mechanical descriptions, since the same behaviour can be generated by different mechanisms.

Carl Craver and Lindley Darden refer to mechanistic models as *mechanism schemas* (Craver & Darden, 2013, p.30). Mechanistic models are not just vehicles for mechanistic explanation (Bogen, 2005; Craver, 2006). They are also enlisted for the tasks of description, exploration, organisation, prediction and control, and etc. These different purposes will require different representational forms.

Craver and Darden specify four independent dimensions upon which mechanistic models may vary from one another. These four dimensions are: completeness; detail; support; & scope (Craver & Darden, 2013, p.30). *Completeness* is defined as the spectrum from *mechanism sketch* to a complete enough for purpose *mechanism schema*. Whereas a mechanism sketch will contain placeholders for incompletely known details (black boxes, filler terms, etc.), a complete for purpose mechanism schema provides a description of a mechanism, its entities, activities and organisational features in sufficient detail for the pragmatic purpose that the description is being used for (see also: Craver, 2006).

Phenomenal models constitute the sketchiest of mechanism sketches; they are complete black boxes that reveal nothing about the underlying mechanism, and merely *save the phenomena* to be explained.

The second dimension of mechanistic models specified by Craver and Darden is *detail*. Detail is defined along a continuum from *abstract* to *specific*. Abstraction is a process of dropping details, whereas specification is a process of adding details. As mentioned above, models provide a source of generality via the process of abstraction.

The third dimension of mechanistic models identified is that of evidential *support*. At one end of the spectrum are *how-possibly* schemas, which are loosely constrained conjectures about how mechanisms work. At the other end of the spectrum are *how-actually* schemas, which describe real components, activities and organisational features of mechanisms. In between these spectrum ends lies a range of *how-plausibly* schemas. A schema will enjoy a larger amount of evidential support the more known constraints on entities, activities and organisational characteristics are satisfied. As investigations continue and more constraints upon the mechanism schema are uncovered, the range of how-plausibly schemas will diminish as more and more plausible schemas become impossible.

The fourth and final dimension of mechanistic models addressed by Craver and Darden is *scope*. This refers to the size of the domain to which the schema applies. As more and more details are filled out in a mechanism schema, it will be applicable in more restricted ranges. In the limit, a complete mechanism schema will be applicable to a single, specific case. Schemas for complex modular subcomponents of mechanisms may have wider scope than for the mechanisms themselves, since evolutionary processes – both natural and social – tend to reuse old modules for new purposes.

2.4.3 Laws of Nature

In contrast to the Deductive-Nomological model of scientific explanation, in which a law of nature *must* feature as an essential premise in the logical argument, the mechanistic model has always objected that such laws are a form of *description* themselves in need of *explanation*. Holly Anderson has put it this way:

> "regularities are what laws describe and what mechanisms explain" (Anderson, 2011, p.325).

Under the traditional empiricist account of laws, these are a type of universal generalisation answering to a form such as: **All Fs are Gs**; or **for all x, if x is F, then x is G**. Under this approach the challenge is to distinguish between those instances that are truly "lawful" from those that merely express accidental generalisations.

According to neo-mechanists, the lawful regularities that mechanisms explain are of restricted scope. This is because they are dependent on the particulars upon which the mechanism is comprised. Others have argued that law statements will be wildly inaccurate unless it is possible to incorporate every potential confounding factor (Cartwright, 1983; Giere, 1999).

2.4.4 Causation

Most theories of mechanistic explanation either supply, or presuppose, a model of causation that underlies the productive operations of mechanisms (Polger, 2018). Stuart Glennan, on the other hand, has used the neo-mechanistic perspective to develop a theory of causation based on mechanisms (Glennan, 2017). This is a position that other Neo-Mechanists, along with non-aligned philosophers are most likely to object to. Carl Craver for example states that:

> "I do not think that causation can be explicated in terms of mechanisms." (Craver, 2007, p.86).

Jon Williamson also criticises Glennan's ontological approach to causation, arguing instead for an epistemic approach that marries both causal and mechanistic insights (Williamson, 2011a; 2011b; 2013). That is not to say, however, that Glennan's approach is inconsistent with current trends in thinking on causality. Specifically, I refer to a growing body of work espousing a pluralistic approach to causal relations. (Hall, 2004; Cartwright, 2004; Godfrey-Smith, 2010; Illari & Russo, 2014; Strevens, 2008; 2013). Glennan's approach fits squarely into this growing literature on causal pluralism. Under his new mechanist account, it is the totality of mechanisms, inclusive of their parts, activities and interactions, which constitutes the causal structure of the world.

Since I do not consider a mechanistic theory of causation to be a necessary requirement for mechanistic explanation, I will provide only a brief sketch of Glennan's proposal here. The new mechanist ontological account of causation is explicitly *singularist* and *intrinsic*. It is singularist because the truth makers of causal generalisations describe patterns of singular instances of causally related events (Glennan, 2017, pp. 151-152). And it is intrinsic because laws do not govern the relations; laws simply describe the ways that mechanisms behave, and it is these behaviours that explain the laws. Glennan claims that both the concepts of causal *production* and causal *relevance* are required to make sense of causal claims. They are considered complimentary concepts referring to different features of the causal structure of the world. Causal production involves transmission from cause to effect via a causal process. Causal relevance on the other hand, describes a relationship where a cause makes a difference to an effect. However, Glennan notes that:

> "While I grant that production and relevance are two different concepts of cause, I will argue that production is fundamental." (Glennan, 2017, p.155)

And he aims to show through the following canonical form of causal statements what the relationship is between these two causal concepts:

> Event c **produced** event e in virtue of **relevant feature** p

Events are particulars described as:

> "happenings with definite locations and durations in space and time. They involve specific individuals engaging in particular activities and interactions" (Glennan, 2017, p.148 (see also pp. 177-179)).

Events are in occurrence wherever one or more entities are engaging in an activity or interaction. Events involving no direct activity of, or change to, an entity's intrinsic properties, but only a change of state determined by relational properties – so-called *Cambridge events* (Kim, 1974) – are excluded as genuine events, since events must be causally productive. Further, although entities and activities may be characterised in more or less determinate ways, the various descriptions are considered to refer to the same event, since they reference the same entities and activities. Different descriptions merely identify different features of the event.

Features refer to:

> "any abstract characterisation or property of the entities, activities, and interaction and their organisation that characterise the productively related events, intervening mechanisms, or their environment (Glennan, 2017, p.175).

Relevant features are described as those that make a difference to the occurrence of an event. Relevant features can include absences and omissions.

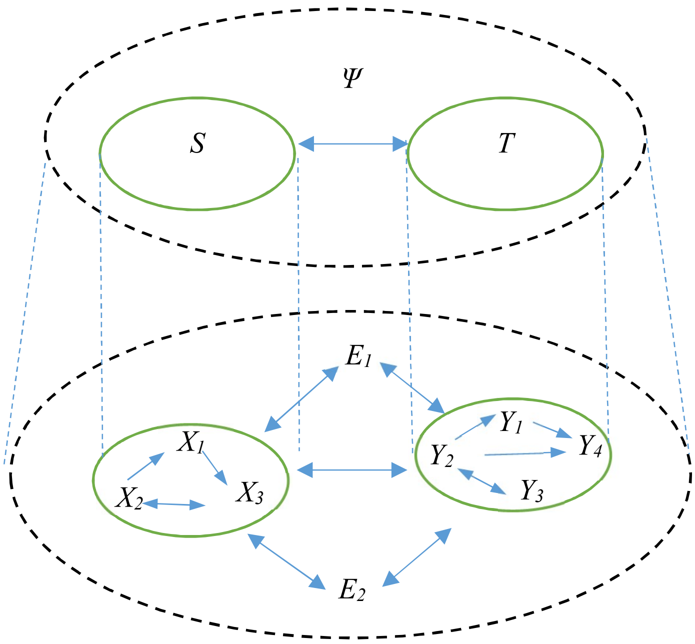
According to the mechanistic theory of causation, causal claims are really existential claims about mechanisms. The (very weak) truth conditions for singular event claims under mechanistic causation are given as (Glennan, 2017, p.156):

> "(MC): A statement of the form "Event c causes event e" will be true just in case there exists a mechanism by which c contributes to the production of e."

Glennan identifies three varieties of mechanistic production (see: **Diagrams 1** through **3** below, recreated from Glennan, 2017. Roman letters represent entities; Greek letters represent activities and interactions; Solid lines represent entity boundaries; Dashed lines represent event boundaries; Dotted lines represent constitution relations; E_1, and E_2 represent entities within the environment).

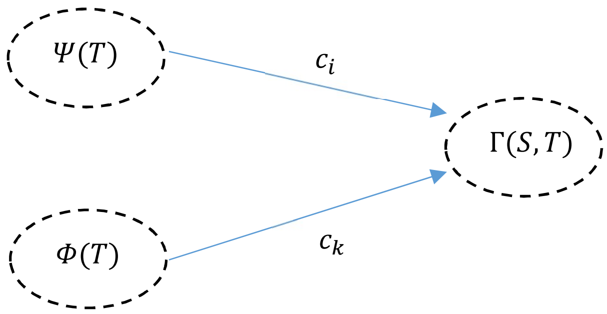
Firstly, there is *constitutive production*. This occurs when an event produces changes in the entities that are engaging in the activities and interactions that constitute the event. The term constitutive here is apt since the changes in the properties of the entities are resultant from the changes to the properties of their parts.

Diagram 1: Constitutive Production



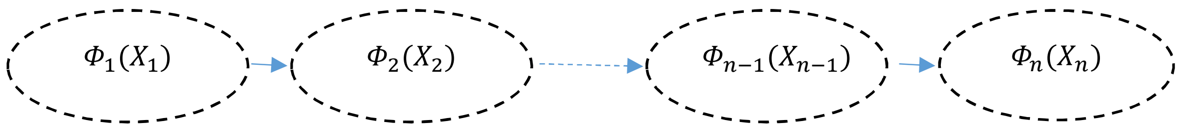
Secondly, Glennan identifies *precipitating production*. This occurs when an event contributes to the production of a further event by bringing about changes in its entities that precipitate a new event. This type of production is referred to as precipitating because one or more events trigger another, by generating the appropriate set of start-up conditions for a different mechanism.

Diagram 2: Precipitating Production



The third type of causal production is called *chained production*. This refers to the situation where an event contributes to the production of another event by way of a chain of precipitative productive events. We are told that the distinction between these three types of causal production is not absolute.

Diagram 3: Chained Production



Two primary concerns can be raised in objection to the mechanistic account of causal production. Firstly, it can be contended that it is possible to have production that is causally irrelevant, calling into question the sufficiency of the mechanistic account of causation, and secondly, that it is possible to have non-productive causation, calling into question the necessity of the account. Glennan's responses to these objections are straightforward. Firstly, concerning irrelevant production, Glennan argues that counterexamples provided presuppose that production is characterised in terms of the transfer of energy or momentum in a style reminiscent of Salmon-Dowe process theories. But instead, the neo-mechanistic account of production is an account of high-level production, in which there is no one characteristic of productive interactions; there are specific types of production deriving from specific types of activities. Obviously, these high-level types of production depend upon lower-level activities, but the lower-level relations are only productive insofar as they are parts of the mechanisms responsible for the higher-level production. Another way of putting this is to state that although numerous *irrelevant* interactions may contribute to the production of a fully determinate event, these productive interactions fail to make a difference to the event described abstractly. And it is a particular abstraction of the fully concrete event that is fashioned in order to define the phenomenon to be explained, that is, which determines relevancy.

Glennan also provides a response to the charge that the existence of non-productive causation challenges the *necessity* of the neo-mechanistic account of causation. Counterexamples constructed for this purpose will typically involve omissions and preventions. Glennan claims that it is possible for something to make a difference to an event without being causally productive of it. In this way he preserves a distinction between the concepts of causal *production* and causal *relevance*. Relevance is defined as a relation between an event and the features of the event – the entities, activities, and organisation that are productive of it. For a maximally determinately defined event, all of such features can be considered relevant. However, event descriptions come in varying degrees of abstraction, and the features that will be considered relevant will thus vary in likewise fashion. So, while causal *production* is concerned with concrete entities and activities, causal relevance is related to abstract difference-making features of those entities and activities.

A further concern relates to a potential charge of circularity, since causation is recursively defined (Psillos, 2004; Craver, 2007; Williamson, 2011a; Casini, 2016). Glennan argues that this is not a case of vicious circularity (Glennan, 1996, p.317). He also argues that it is not a case of infinite regress, since, according to how he understands quantum mechanics, there is no fundamental level of causation. Subsequently, Glennan admitted that his account was in fact circular (Glennan, 2009, p.318). However, in his latest work, he leaves the door open for fundamental mechanisms, while contending that it is possible that there are “mechanisms all the way down” (Glennan, 2017). He claims that there is no ontological problem with his position, since:

> “Higher-level and distal causal connections depend upon lower-level and proximate mechanisms; but this means that the activities and interactions that constitute the mechanism connecting c to e refer to different causal relationships than the one between c and e” (Glennan, 2017, p.185).

2.5 Relation to Other Concepts

In this 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*.

2.5.1 Inference

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 (Bechtel & Abrahamsen, 2005; 2010). In contrast, the DN model provides for only linguistic representations and deductive inference. Since logic only operates on propositional representations, reasoning by scientists via methods such as diagrams and visual images cannot be captured and understood logically (Bechtel, 2008, p.20).

2.5.2 Discovery

Secondly, the mechanistic model provides an account of scientific discovery and development, unlike the DN model. William Bechtel and Adele 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 (Bechtel & Abrahamsen, 2005, p.432). 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[3]. 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[4].

Carl 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 (Craver, 2002, p.360). 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. Craver and Darden provide a framework for putting all these elements together. In this way, they characterise the mechanism discovery process. They describe this as a four-stage process with the following components (Craver & Darden, 2013, p.7):

1. Characterising the phenomenon;
2. Constructing a schema;
3. Evaluating a schema; &
4. Revising the schema.

2.5.3 Testing

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 (Bechtel & Abrahamsen, 2005, p.436).

2.5.4 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, they 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.

According to the traditional account of theory reduction, centred upon the DN model of explanation, theory reduction occurs when a law of nature is subsumed under a more general law, since laws are the engines of explanation. This brings into question the genuine autonomy of the various branches in the hierarchy of scientific disciplines. A major problem for this traditional account of reduction is that the vocabularies between different levels of the sciences differ so that one cannot logically deduce conclusions that use terms that are not in the premises (Bechtel, 2008, p.131). The strategies adopted to overcome this problem appeal to *bridge principles*, or *rules of correspondence* (Stigum, 2003, Ch.12), that equate the vocabularies of the reduced and reducing laws[5]. However, both Paul Feyerabend and Thomas Kuhn (Feyerabend, 1962, 1970; Kuhn, 1962) have argued persuasively that since the meaning of terms used are generally different for the two theories in the reduction, the terms are incommensurable with one another and thus the reduction will fail. A second major problem is that the regularities described by the law statements are only operative under a restricted set of conditions, so that the theory-reduction model requires statements specifying boundary conditions.

But perhaps a more fundamental issue with the traditional account of theory reduction relates to the concept of *levels* of scientific enquiry. A prominent feature of the account is that the different sciences address phenomena at different levels. For example, physics studies phenomena at the most fundamental level, chemistry studies those at the level above, followed by biology, etc. But it is not obvious that in practice these scientific disciplines represent discrete *levels*, since each discipline would appear to incorporate the study of phenomena across a broad range of *levels.* Phenomena within the domain of physics for example, range from subatomic particles through galaxies, to the universe (and beyond). Consequently, it is not entirely obvious that a particular discipline, for example, physics, studies phenomena that are at a lower *level* than say, chemistry or biology.

A variety of approaches have been proposed as alternatives to the disciplinary matrix account of levels. One approach uses the relative sizes of entities to demarcate levels of phenomena (Churchland & Sejnowski, 1988; Wimsatt, 1976, 1994). Another approach, which has been proposed by Adele Abrahamsen, firstly groups phenomena into broadly defined academic disciplines based on type of phenomena: physical sciences; biological sciences; behavioural sciences; & social sciences and then proceeds mereologically (Abrahamsen, 1987). These alternatives to the traditional account of scientific *levels* face serious challenges (Craver, 2007, Chapter 5; Bechtel, 2008, Chapter 4). But we can look to the mechanistic framework itself for the resources required for the formulation of an adequate account of *levels* for the purpose of understanding the nature of mechanistic reduction. William Bechtel explains that:

> "Within a mechanism, the relevant parts are…working parts—the parts that perform the operations that enable the mechanism to realize the phenomenon of interest. These may be of different sizes, but they are distinguished by the fact that they figure in the functioning of the mechanism. It is the set of working parts that are organized and whose operations are coordinated to realize the phenomenon of interest that constitute a level." (Bechtel, 2008, p.146).

So, a working mechanism defines a phenomenal *level* and the working parts of the mechanism represent a lower level. The working parts themselves can be decomposed into lower levels of working parts. This mechanistic account of levels is a local one: it has no way of evaluating whether the parts of a mechanism are at the same *level* as entities outside the mechanism. This means that as we decompose different working parts of a mechanism, the lack of relation between the sub-parts will not allow us to say anything about their relative levels: the question is not well-defined. Another feature of the mechanistic account is that entities of the same physical type may not be at the same *level* if they perform different functions within a mechanism.

2.6 Objections to the Mechanistic Model

I will now discuss three issues that may be considered problematic for the adoption of the mechanistic model of scientific explanation as a normative standard for economic science. The first relates to an objection that has been raised against the mechanistic model on its own terms, in the domains of its intended application. The second, is a broader and more fundamental issue for this thesis – methodological monism. Thirdly, I will address the question as to whether naturalism can provide norms.

2.6.1 Challenges to the Mechanistic Model

In the recent literature, there has been an objection raised to the Mechanistic model (Batterman & Rice, 2014; Ross, 2015). 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 Robert Batterman (Batterman, 2001, p.23), 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 mechanistic 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.

I will briefly note two directions in which I believe a rebuttal to this challenge 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 Carl Craver, between *ideally complete* models and *pragmatically complete* models. Craver describes the difference as follows:

> "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).

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.

2.6.2 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 4, a number of its adherents enthusiastically embraced the model as a standard for economic theory development. David Kaplan and Carl Craver leave it as an open question whether the mechanistic 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 proponents of the mechanistic 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, Carl Craver and Anna 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)

And Stuart Glennan also argues that mechanistic standards are applicable across the whole spectrum of the sciences (Glennan, 2017, pp. 1-2).

2.6.3 Naturalism Cannot Provide Norms

Theories of mechanistic explanation and theory structure have been birthed from a program of *naturalised epistemology*. This is an approach that avails itself of the resources of science by examining how scientific enquiry is conducted by actual scientists (Quine, 1969). Rival approaches have sought to identify the normative canons of science via independent criteria. Logical positivists and logical empiricists for example, derived their philosophical accounts of scientific practice by drawing upon the logical works of Gottleib Frege, Bertrand Russell and Ludwig Wittgenstein. Their program intended to justify scientific claims by showing how they could be logically derived from sentences capable of being confirmed or refuted by observation (Carnap, 1936, 1937; Reichenbach, 1938; Hempel, 1962). Due to a host of issues concerning confirmation and falsification that plague the ultra-empiricist approach, many philosophers abandoned the project of developing a logic of science (Bechtel, 2008, p.5).

It might be argued that as a naturalised pursuit, mechanistic theories of explanation cannot rise above their naturalism to provide normative guidance. While it is true that such endeavours cannot independently specify norms for the practice of science, by drawing upon scientists' own identification of cases exhibiting good and bad scientific practice, we are able to evaluate theories about how science works and to reflect these observations back upon the works within specific scientific pursuits (Bechtel, 2008, p.7; Craver, 2014).

2.7 What Mechanistic Explanation is Not

While Stuart Glennan's *minimal mechanism* account provides a most helpful metaphysical analysis for explaining the causal structure of the world, my purposes in this book are of a more epistemological nature. My project concerns the methodology of science, in particular, the methodology of economic science. And although questions of metaphysical, epistemological and pragmatic natures cannot be entirely untangled, a more constrained definition of mechanism will suit my purposes more precisely. I will therefore adopt the commonality of the accounts propagated by Carl Craver, William Bechtel, and their extensive networks of co-authors.

Petri Ylikoski informs his readers that theorising about mechanisms within the social sciences has multiple origins (Ylikoski, 2017, p.401). And Stuart Glennan and Phyllis Illari tell us that the discourse on mechanisms in the social sciences began at the same time as that in the general philosophy of science, and that both were forged out of a shared dissatisfaction with the logical empiricist view of scientific theories and explanation (Glennan & Illari, 2017, p.1).

Peter Hedstrom and Petri Ylikoski claim that:

> "John Elster has probably been the most influential advocate of mechanisms in the social sciences, and his many books are full of excellent examples of mechanism-based thinking in action." (Hedstrom & Ylikoski, 2010, p.56).

But it must be emphasised here that Elster's definitions of *mechanism* have been ever-changing, insufficiently specified, and increasingly divergent with the conception of mechanism advocated in this paper. To show this, notice the following definitions provided by Elster:

> "A mechanism explains by opening up the black box and showing the cogs and wheels of the internal machinery. A mechanism provides a continuous and contiguous chain of causal or intentional links between the *explanans* and the *explanandum*." (Elster, 1989)

This sounds about right, but then he redefines the term as follows:

> "Mechanisms are frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences." (Elster, 2015, p.26)

And Elster seemingly displays his flimsy devotion to mechanistic explanation when he states:

> "Often, explaining by mechanisms is the best we can do, but sometimes we can do better. Once we have identified a mechanism that is "triggered under generally unknown conditions", we may be able to identify the triggering conditions. In that case, the mechanism will be replaced by a law." (Elster, 2015, p.35).

Petri Ylikoski observes of Elster that:

> "he sees mechanism-based theorizing as clearheaded causal thinking about social processes and for a large group of social scientists this is the core of the mechanistic perspective." (Ylikoski, 2017, p.402).

Mario Bunge has also been a stout proponent of mechanism-based explanation in the social sciences. He advanced several interrelated themes relating to mechanistic explanation in his book *Scientific Research* (Bunge, 1967), and in more recent times has noted that:

> "Recently, Machamer, Darden, and Craver (2000) rediscovered that science explains in terms of mechanisms. They have also asserted that "there is no adequate analysis of what mechanisms are and how they work in science." Though belated, these admissions are true." (Bunge, 2004, p.183).

But Bunge goes on to argue that their account is incorrect, and also criticises the one developed by Stuart Glennan (Glennan, 2002). For Bunge, mechanisms are *processes* in concrete systems, where the systems can be either physical, social, technological, etc. But, unlike the neo-mechanist accounts, Bunge's account requires laws. He states that scientific explanations:

> "resort to law statements. So, mechanismic hypotheses do not constitute an alternative to scientific laws but are components of deep scientific laws. In other words, 'mechanism' (or 'translucent-box')

> opposes ‘phenomenological’ (or ‘black box’), not ‘lawfulness’ (see Bunge 1964, 1967, 1968).” (Bunge, 2004, p.200)

So, whereas neo-mechanists emphasise invariance over lawfulness, Bunge is committed to law statements. Further, according to Bunge, scientific explanations in the social sciences require more than just mechanism identification and elaboration, for he asserts that:

> “What is true is that, in the social sciences, law and mechanism are necessary but insufficient to explain, because almost everything social is made rather than found. Indeed, social facts are not only law-abiding but also norm-abiding; and social norms, though consistent with the laws of nature, are not reducible to these, if only because norms are invented in the light of valuations—besides which every norm is tempered by a counternorm.” (Bunge, 2004, p.197).

Narrowing down further to the single social science of economics, one finds abundant discourse on “mechanisms”. Julian Reiss identifies four different notions of the term causal mechanism operative within the discipline of economics (Reiss, 2013, pp. 104-105). The first of these four notions is one referred to by econometricians and other practitioners who model causal systems as systems of equations. The term *mechanism* here refers to a single equation within the system and is nothing more than a term contrasting mere association; it is simply another term for a causal relation. The second notion of mechanism identified by Reiss refers to a set of variables intervening between a cause and an effect. Reiss refers to this as *mechanism as mediating variable*. The third notion presented by Reiss is referred to as *mechanism as underlying structure or process*. By this he means that aggregate variables are constituted by entities and processes at a lower level. He directly references Hedstrom and Ylikoski (Hedstrom & Ylikoski, 2010) as the source of such mechanism talk in the social sciences and notes that this notion is closely related to the account proposed by Machamer, Darden and Craver (Machamer, Darden & Craver, 2000). A fourth notion of mechanism addressed by Reis is that of *mechanism as a piece of theory*. He tells us that these are, mostly, strongly idealised representations of the mechanisms as conceived in the *mechanism as underlying structure or process* notion above.

Julian Reiss, arguing from a position of methodological pluralism in economics, has argued that:

> "…knowledge about mechanisms…contributes very little at best and that investigating mechanisms is therefore a methodological strategy with fairly limited applicability." (Reiss, 2007, p.163)

Reiss' specific targets are the critical realism movement spearheaded by Tony Lawson (Lawson, 1997; 2003), as well as the mechanistic positions espoused in the works of social science philosophers such as John Elster (Elster, 1983; 1985; 1989) and Daniel Little (Little, 1991; 1998). His opinion therefore, can be construed as even more strongly opposed to the narrower conception of mechanistic explanation championed in this paper.

2.8 Conclusions

In this chapter, I presented the Neo-Mechanistic model of scientific explanation. I showed how under this model, a valid scientific explanation of a phenomenon is a veridical representative model of the phenomenon, which makes recourse to the mechanistic categories of *entities*, *activities*, and *organisation*; to explain a phenomenon is to show how entities engaging in activities are organised in such a way as to be productive of the phenomenon.

I also showed how this model of scientific explanation is capable of generating methodological norms for the construction and development of theoretical constructs, via progressive research programs focused on completing mechanism schemas. Further, I showed how the Neo-Mechanistic model relates to other key concepts within the general philosophy of science literature. In particular, I showed how the model provides substantial resources for inference and discovery, and how it provides a compelling account of theoretical reduction.

In the following chapter, I will explore the history of the methodology of economic science, to show how an attendance to philosophical issues has traditionally driven methodological commitments.

Part 2: Philosophy of Economics

Chapter 3 - Methodology of Economics

The purpose of this chapter is two-fold. Firstly, I will establish that historically, economists have paid attention to contemporary philosophy of science and have developed their methodological approaches to theoretical construction and development with explicit reference to these philosophical influences. Secondly, a simple heuristic will be applied to show that the methodological approaches under discussion fail to meet neo-mechanistic criteria outlined in Chapter 2.

3.1 Introduction

The contemplation of *economic* matters has a written history dating back to ancient times. This can be evidenced with reference to prominent treatments of the history of economic thought. For example, Eric Roll in his classic volume, commences with a brief discussion of the musings of the mythical biblical Hebrew prophets – thought to be reflections of actual historical concerns - before moving on to the works of Plato and Aristotle, as representatives of ancient Greek economic thought (Roll, 1992, pp.9-23). Also, Murray Rothbard, considering all economic deliberations prior to those of the ancient Greeks as irrational, crowns the poet Hesiod, who lived in the middle of the eighth century B.C., as the first true economic thinker (Rothbard, 1995a, pp.3-27), while Roger Backhouse proclaims that Xenophon deserves this honour (Backhouse, 2002, pp. 13-17). Joseph Schumpeter made a distinction between *economic thought* and *economic analysis* (scientific economics), attributing the beginning of the former to the period of the ancient

Egyptians, Assyrians and Babylonians at around the twenty first century B.C. The latter, he dated to the works of Plato and Aristotle in the fourth and fifth centuries B.C. (Schumpeter, 1954, pp.49-62). Elsewhere, the first author to have established economics as a separate scientific discipline has been located within ancient India, and granted to Kautilya (Sihag, 2016).

Conscious reflection upon the methodology employed for systematic contemplations of an economic nature, did not however commence until Nassau Senior published his *Introductory Lecture on Political Economy* in 1827[1], just over fifty years after economics is commonly held to have been born as a distinct scientific discipline, with the publishing of Adam Smith's *An Enquiry into the Nature and Causes of the Wealth of Nations* in 1776[2]. The methodology espoused by Senior and those who followed in his wake, was an a priori one in which conclusions were established on the basis of deductive reasoning from supposedly self-evident axioms. During the following hundred years, various schools of economic thought were established in opposition to the methodology employed by the classical economists. These schools questioned the reliance on deductive methods and championed inductive alternatives based on vastly different philosophical foundations. More than one hundred years had passed since Senior's seminal paper on methodology, when the philosophical movement of logical positivism profoundly impacted the thinking of economic methodologists. At this stage, the a priori method and its rivals suffered widespread rejection in favour of a thoroughgoing empiricism based on the tenets of logical positivism.

Despite the spectacular failures of the logical positivist and logical empiricist movements, economic methodology to this day continues to be dominated by the model of scientific explanation that formed a key plank of the positivist program. While something of a consensus has emerged within the philosophy of science within recent decades regarding scientific explanation and how this relates to the construction and development of scientific theories (see: Chapter 2 above), this has yet to be embraced by the economics community. The objective of this chapter is to explore the literature on economic methodology, to argue that the convictions of economic methodologists have historically been shaped by developments within the philosophy of science. The implication being, that it is time for economists to once more take modern developments in philosophy seriously, and re-orient methodological practices accordingly.

This chapter is structured as follows. In Section 3.3, I discuss the relevance of the philosophy of science for the methodology of economics. Then, in Sections 3.4 through 3.6 I discuss, in turn, the methodological frameworks of the *Classical*, *Austrian*, *German Historical*, and *Institutionalist* schools of economic thought, as well as assessing them on the basis of mechanistic criteria. I conclude in Section 3.7 by summarising the findings of this chapter.

Before moving on, for the sake of clarity, I'll highlight the obvious distinction between the concepts of *method* and *methodology*. On several occasions throughout the following chapters, I will discuss various *methods* employed by economists in order to realise their *methodologies*. The distinction, which should be kept clear at all times, has been adequately addressed by Fritz Machlup:

> "Although methodology is *about* methods, it is not *a* method, nor a set of methods, nor a description of methods. Instead, it provides arguments, perhaps rationalizations, which support various

> preferences entertained by the scientific community for certain rules of intellectual procedure, including those for forming concepts, building models, formulating hypothesis, and testing theories. Thus, investigators employing the *same method* – that is, taking the same steps in their research and analysis – may nevertheless *hold very different methodological positions*. Obversely, supporters of the *same methodological principles* may decide to *use very different methods* in their research and analysis if they differ in their judgements of the problem to be investigated, of the existing or assumed conditions, of the relevance of different factors, or of the availability or quality of recorded data. Thus, while we use a method, we never "use" a methodology…The confusion of methodology with method is, for a literate person, inexcusable." (Machlup, 1978, pp. 54-55).

3.2 Is Philosophy of Science Relevant for Economic Methodologists?

But before we move on to explicit methodological works on economic science, it's worth pausing to ponder the question of the value of philosophical reflection for the theoretical activities of economists. Concluding his review of two books on the methodology of economics by philosophers of science[3], 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 (McCloskey, 1985). This position is further supported by Wade Hands (Hands, 2001), and Bruce Caldwell (Caldwell, 1982). 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 I put forward in this book, if correct, ought to be of interest and benefit to both philosophers and economists alike; philosophers will be presented with a practical application of theoretical philosophical principles that helps to bolster the case for those principles, and both methodological and practicing economists will be presented with a workable solution for methodological reorientation. The primary purpose of this chapter is to show that, historically, economists *have* looked toward philosophy of science as a basis for deriving methodological prescriptions. Alexandre Koyre once noted, of science in general, that:

"in history, the influence of philosophy upon science has been as important as the influence – which everyone admits – of science upon philosophy." (Koyre, 1961, p.177)

Daniel Hausman provides a useful grouping of concerns into five broad categories within traditional philosophy of science that are relevant to economic science (Hausman, 2008). 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?

By taking the account of mechanistic explanation outlined in Chapter 2 above, and filling out the five categories listed above, a set of implied commitments can be arrived at for economic methodologists. The results of such an exercise would look something like:

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. Such a model must make recourse to the categories of: *entities*, *activities* and *organisation*.

Theories - Theories are constructed out of models of mechanisms; theories *are* collections of representative 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 theoretical constructs. Entities, activities and organisational features underlying mechanisms must be shown to exist in reality. The inclusion of fictional posits and empirically falsified constructs renders such models as invalid explanations.

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.

Throughout this, and the following chapters, these commitments will serve as a contrast set when assessing the mechanistic methodological credentials of the various schools of economic thought under discussion. I will now introduce the various schools of economic thought that have dominated the field of economic science since its inception, and explore the philosophical foundations upon which their methodological approaches were constructed. Firstly, I examine the Classical School. Then, in order, I address the Austrian School, Historical School, and Institutionalist School.

3.3 The Classical Methodology – Apriorism[4]

It will be shown in this sub-section that, firstly, philosophical influences were front of mind for the classical economists in developing methodologies and theoretical constructs, and secondly, that the methodologies developed do not conform to the strictures of Neo-Mechanistic explanation.

When Adam Smith penned *The Wealth of Nations*, he was greatly influenced, as were so many other Scottish scientists of the time, by the methods of Isaac Newton. Inspired by Newton's discovery of the natural laws of motion, Smith set out to discover the general laws of economy. It is not surprising then that in reading Smith we can discern:

> "…a series of connections between laws, axioms, and conclusions as elaborated or taken up by Newton and the analyses of economic and social phenomena put forth by Smith…and most importantly, we see that the general schema of Newton's natural philosophy is in line with Smith's general schema of moral, social, and political philosophy." (Diemer & Guillemin, 2011, p.5).

David Hume, Smith's close friend, likewise sought to establish a science of human nature in the image of Newton's great achievements. He explicitly subtitled his *Treatise* "an attempt to introduce the experimental method into moral subjects". (Hume, 1739). By the time the first explicit pronouncements on the methodology of economics had been produced in the following century, the vision was not quite so clear.

Francis Hutcheson - Adam Smith's teacher - also asserted a strong philosophical influence on both Smith and Hume, imparting the basic classical liberal worldview of natural rights, utilitarianism and the beneficence of nature (Pesciarelli, 1999; Rothbard, 1995, p.420).

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[5]. These 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. 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, given its most extreme exposition by Ludwig von Mises in the 1930s and 1940s, reiterated by Frank Knight, and lives on to this day within the modern *Austrian School*, having been championed by Murray Rothbard, and Hans Herman Hoppe.

3.3.1 Nassau Senior

Nassau Senior was the first to explicitly outline the methodological principles of the early classical school (Senior, 1827). Edward Coppleston and Richard Whately were two of the most significant influences on Senior. These philosophers were enthusiastic proponents of the ideas of Dugald Stewart (Rashid, 1985, p.257). Of particular note, is Stewart's interpretation and propagation of Baconian Philosophy of science.

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. Senior attempted to construct the first axiomatic basis for political economy. 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.35)

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.

3.3.2 John Stuart Mill

John Stuart 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 (Mill, 1843). He later stated that he had generalised Dugald Stewart's position on axiomatic reasoning (Mill, 1873, p.109). Mill 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 priori propositions. In fact, Mill espoused a radical empiricism that denied the existence of any a priori knowledge (Mill, 1843). These, at least seemingly, contradictory views leave one somewhat unsure as to what his ultimate methodological position was. On this note, Mark 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, Murray 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, regarding 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.

3.3.3 John Elliot Cairnes

Whereas John Stuart Mill had attempted to inject some inductive, empirical ideas into classical methodology, John Elliot Cairnes returned to the more purely deductive approach of Nassau 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. In this, he was influenced, as was Nassau Senior and John Stuart Mill, by the works of Dugald Stewart, and the *common-sense* philosophy of Thomas Reid. From these influences, he took away the idea that reasoning from common-sense is a form of observation. So, unlike the twentieth century positivists (see: Chapter 4 below), the axioms of economics were not viewed as empty logical principles devoid of empirical content, but instead, were considered as embedding knowledge derived from common-sense imagination.

3.3.4 John Neville Keynes

John Neville Keynes published his methodological treatise during the period of the *Methodenstreit* (method dispute) that raged between Carl Menger of the Austrian School and Gustav Schmoller of the German Historical School (See: Section 3.4 and Section: 3.5 below). 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, 1890, 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.

3.3.5 Lionel Robbins

Lionel Robbins published his treatise on economic methodology in 1932, after a period in which the inductivist methodology of the Institutionalist School had gained significant influence (see: Section 3.6 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 positivism blew through the economics community (see: Chapter 4 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.15)

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, **P1**: individuals arrange their preferences in order. The fundamental postulate of the theory of production is, **P2**: there is more than one factor of production. The fundamental postulate of the theory of dynamics is, **P3**: future scarcities are uncertain.

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 one 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[6].

Robbins makes it clear that economic science:

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

Subsequent generations of economists inspired by the positivist program would completely reject this assertion. In fact, their methodologies would prove wholly incapable of supporting such a proposition.

3.3.6 Conclusions

The classical economists were clear about their philosophical influences and sought to derive their methodological principles based on what they viewed as current philosophy of science. Specifically, Smith and Hume sought to emulate the achievements of Newton, while the likes of Senior, Mill, Cairnes and Keynes developed their methodological principles based on their convictions derived from Baconian philosophy of science, as interpreted and propagated by the common-sense philosophers Thomas Reid and Dugald Stewart.

How do the methodological convictions of the classical school compare with the requirements of a Neo-Mechanistic perspective? As has been shown above, there are, at least in what they say, major differences between the most prominent methodologists of the classical period. Despite this fact however, it is possible to construct an encompassing set of characteristics covering them all. Concerning the *goals* of their science, the classicists viewed explanation as their primary purpose and were committed to realism in the sense that in the construction of their theories, they aimed to faithfully represent truths about the world. 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. Although delivered in verbal mode, *theories* were conceived of as formal structures comprised of fundamental postulates and auxiliary assumptions, along with chains of deductive inference. Theory selection was a matter of choosing those structures that were most complete. *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. The positions of the classicists on the issue of *methodological monism* varied, according to how broadly they conceived of the applicable methodological principles.

Classical methodology clearly was not explicitly *mechanistic*. And while classical theories made reference to various entities and activities, they did so in rather vague fashion, and interaction was mostly absent from the accounts. More importantly, the lack of any enthusiasm for, and often outright rejection of, empirical enquiry rendered the approach incommensurate with Neo-Mechanistic principles.

3.4 The Austrian School

The modern Austrian school personifies radical a priorism. But it wasn't always this way. The key features that bind the Austrian School together, through time, are: methodological individualism and subjectivism. Methodological Individualism declares that valid explanations in the theoretical social sciences, must explain facts about social processes and institutions as unintended repercussions of the interaction between the intended actions of individuals. The principle of subjectivism is an empirical theory, which states that in evaluating objects and actions as goods and services, individuals make recourse only to their subjective preferences.

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 - with the publishing of his book *Grundsätze der Volkswirthschaftslehre* (Principles of Economics), within which he set out to explain exchange and relative prices, by producing a unified price theory based on the principles of methodological individualism and subjectivist evaluation (Menger, 1871). The (not quite) simultaneous discovery of the marginal utility concept was a momentous event in the history of economic theory. It instigated what is known as the *Marginalist Revolution*. This is what conventionally separates *neo-classical* economics from *classical* economics. In 1848, John Stuart Mill had over-optimistically claimed that:

> "Happily, there is nothing in the laws of value which remains for present or any future writer to clear up; the theory of the subject is complete." (Mill, 1848, p.)

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* (Investigations into the Methods of Social Science and Political Economy in Particular) - which is an extended analysis of epistemological and methodological problems of economics (Menger, 1883)[7]. This book included a sustained attack on the principle of methodological collectivism espoused by the German Historical School, and its claim that economic science can only properly be pursued by the methods of history and statistics (see: Section 3.5 below). It was this criticism that sparked the famous *Methodenstreit* (method dispute) between these two schools, and which raged on for many decades. Ludwig von Mises and Friedrich Hayek were to carry on this debate into the second half of the twentieth century.

In this sub-section I will outline the key philosophers who impacted the Austrians and show how their ideas drove the development of the Austrian School methodology. Specifically, it will be shown how the ideas of Aristotle, Karl Popper and Immanuel Kant were embraced and applied. It will also be shown that the modern Austrian methodology is inconsistent with neo-mechanistic principles.

3.4.1 Carl Menger

Concerning the purpose of economic research, Menger claimed:

> "The goal of scholarly research is not only the cognition, but also the understanding of phenomena. We have gained cognition of a phenomenon when we have attained a mental image of it. We understand it when we have recognized the reason for its existence and for its characteristic quality (the reason for its being and for its being as it is)." Menger, 1883, p.43)

And he tells us that we can gain such an understanding of economic phenomena in two different ways. Firstly, through its history: by investigating its individual process of development; by discerning the concrete relationships under which it has developed, which have determined its special quality. Secondly, economic phenomena can be understood in a purely theoretical way.

Menger eschewed the Walrasian general equilibrium approach, and the Marshallian partial equilibrium approach, for a causal explanation of the determination of real, disequilibrium prices. A general equilibrium model of an economy posits markets for each of N commodities, in which consumers - having endowments and demand functions - are assumed to maximise utility subject to budget constraints, and producers - facing production sets - are assumed to maximise profits. In equilibrium, prices adjust so that demand equals supply in all markets and there are zero profits at the industry level. Partial equilibrium approaches focus on prices within a restricted range of market, assuming constant prices in all other markets.

Concerning his alternative causal approach, Menger 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).

To achieve his ambitions, Menger adopted an *essentialist* position grounded in Aristotelian metaphysics (Kauder, 1957; White, 1977). In this sense, Menger was devoted to philosophical realism - the universals of economic reality are discovered through theoretical efforts, they are not the arbitrary creations of economists. Menger applied the Aristotelian distinction between *form* and *matter* to economic phenomena. The former, he referred to as *general* economic phenomena, the activities in pursuit of the explanation of which he called *theory*. The latter, he referred to as *concrete* economic phenomena, whose explanatory methods were called *history* and *statistics*. Menger thus promoted different, but complementary, methods for pursuing economic science; one predominately rationalist, the other predominately empiricist.

Menger lamented that:

> "The progress of our science at present is hindered by the sway of erroneous methodological principles". (Menger, 1838, p.31)

His primary criticism of the German Historical school was that they followed only the methods of *history* and *statistics*, and as such, rejected any possibility of a theoretical economics capable of describing and explaining the general economic phenomena; the *essences* of economic phenomena. Their methods could only hope to explain individual, concrete, economic phenomena.

In Menger's Aristotelianism, the economic laws established by theoretical means, represent timeless, necessary ontological facts. They follow necessarily from the essential natures of the factors involved, and are the most important targets of economic research. Menger distinguished between *exact* and *empirical* laws. Exact economic laws were conceived of as necessary eternal configurations of economic life, beyond the influence of time and place. Empirical laws represent the regularities in the succession and coexistence of real, concrete economic phenomena. For the classical economists, the economic laws arrived at via abstraction from fundamental postulates did not have the ontological status that Menger attributed to them. For Menger, and subsequent Austrian economists, especially Mises, individuals acting in a free market materialise this universal economic structure. And it is against this master-plan that all social phenomena are to be conceived. The master-plan serves as a logical criterion for determining explanatory validity. A valid economic explanation expresses phenomena as manifestations of economic essences. The master-plan also provides the basis for moral evaluation. Emil Kauder explains the implication for moral action:

> "The ontological structure does not only indicate what is, but also what ought to be. Man will understand the essence of economizing and then must organize his actions so that the frictionless functioning of the eternal organon will be materialized in real life. The social ontology straddles the border between pure contemplation and moral action." (Kauder, 1957, p.417).

Under the general equilibrium view of economic phenomena due to Leon Walras, which has dominated economic theory down to the present day, economic forces are conceived of as interdependent, and under a free market are expected to align themselves. In contrast, in the Mengerian view, the entire structure of economic forces is brought into being, by the final cause of marginal utility.

Menger rejected the possibility that a purely a priori method without some empirical content could produce knowledge, stating:

> "Theoretical economics has the task of investigating the general nature and the general connection of economic phenomena, not of analyzing economic concepts and of drawing the logical conclusions resulting from this analysis. (Menger, 1838, p.37).

The Austrian school was at its peak during the first two decades of the twentieth century. By this time, Menger had retired from teaching, and two devotees of his, Eugene Bohm-Bawerk and Friedrich von Wieser, had developed and disseminated his ideas. They were also propagating his methods to a new generation of economists, which included Ludwig von Mises and Joseph Schumpeter[8]. By the 1920s, several of Ludwig von Mises' students, including Friedrich Hayek and Oskar Morgenstern were making significant contributions to the literature in theoretical economics. During this period, the Austrian school effectively ceased to exist as a separate school of thought, as its leading ideas had been absorbed into the dominant teaching of the day.

3.4.2 Ludwig von Mises

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. And this is to be achieved by gaining a logical understanding of the concept of human action. Mises states that:

> "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." (von Mises, 1949, p.6).

And more concretely, this leads to:

> "…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 (von Mises, 1933; 1949; 1978)[9]. 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." (von 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 praxeology:

> "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." (von Mises, 1949, pp.11-12)

Praxeology is a rival epistemology to that of empiricism. It rejects the analytic/synthetic distinction, and asserts that the conclusions of theoretical economic science are necessary, a priori synthetic truths. Economic science under praxeology is conceived of as a chain of deductive inferences from necessarily true axioms, to necessarily true conclusions, which 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 (Mises, 1962, p.12). He claims to have demonstrated 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.

But there is much more that Mises was committed to methodologically. Mises first came to renown after publishing research on monetary theory and policy in 1912. He combined methods of subjectivism and marginal utility theory inherited from Carl Menger and Eugene Bohm-Bawerk - his Austrian forebears - and applied them to the field of monetary economics. This project had two primary purposes. Firstly, Mises set out to explain the nature and significance of money. And secondly, he sought to explain the consequences for the economic system of the manipulation of money and credit by government authorities. While carrying out this program, Mises developed a microeconomic theory of industrial fluctuations. The central concept underpinning this theory was that relative prices play a crucial role in guiding human decision making. Mises did not assume optimising behaviour on the part of the individual decision makers. He merely assumed that individuals act purposefully, by engaging in risk assessment and evaluation of alternative projects, via the information provided by relative market prices. Mises' conclusion was that manipulation of the value of money distorts exchanges, resulting in the mis-coordination of production plans and consumption demands. The source of business cycles therefore has been identified as the political manipulation of money and credit.

Mises expanded on this and other associated pieces of work in a most forthright manner in 1920, in what was to become a most famous and controversial paper: *The Problem of Economic Calculation in the Socialist Commonwealth*. Mises argued that since socialism requires the confiscation of private property in the means of production, it implies the cessation of all market exchange. And with no rivalrous competition for the factors of production, there could be no indications of the value of various resources in a common unit of account. Without recourse to the judgements about relative scarcities embedded in relative market prices, socialist economic planners have no means by which to determine whether a given plan will be socially productive or socially wasteful. Mises essentially argued that the institutions of private property, freedom of contract, and profit and loss accounting are essential for the coordination of plans across ranges of diverse actors. Mises did not deny that socialism, studied under static conditions with planners in possession of complete knowledge, implies that an economy can be easily managed. But he rejected this type of analysis on the grounds of the patently false assumptions. Economic data are always changing and so a static approach is patently unacceptable. Mises thus rejected static analysis in favour of a dynamical one. In this, he is clearly following in the path of Menger – the founder of the Austrian school - who sought to explain economic phenomena in a causal, non-equilibrium setting.

3.4.3 Friedrich Hayek

Friedrich Hayek followed Carl 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 (Hayek, 1937, 1945). For Hayek, the purpose of a social science such as economics is:

> “to explain the unintended or undesigned results of the actions of many men.” (Hayek, 1952, p.41).

> “to grasp how the independent action of many men can produce coherent wholes, persistent structures of relationships which serve important human purposes without having been designed for that end.” (Hayek, 1952, p.141)

Hayek was highly influenced by Ludwig von Mises, who was his teacher in Vienna. Indeed, he described Mises as the person:

> “from whom I have probably learnt more than from any other man.” (Hayek, 1994, p.72).

This influence extended to both his earlier methodological convictions and to his own work in developing the Austrian business cycle theory. Hayek’s work followed Mises in arguing that the primary problem of economics is the explanation of how individual production and consumption decisions are efficiently coordinated. His answer, like that of Mises, was that entrepreneurial activity guided by the price system was the solution (Hayek, 1931). By the 1930s, Mises’ arguments against the possibility of rational socialist economic planning had begun to elicit some vocal responses from colleagues of Hayek’s at the London School of Economics (see for example: Dickinson, 1933; Lerner, 1934). And so Hayek became embroiled in the Socialist Calculation Debate. He argued staunchly against the unrealistic institutional shortcomings evident in his opponents' approach: the assumptions of omniscient, omnipotent and omnibenevolent central planners, and reinforced Mises' insights concerning the centrality to economic science of the knowledge generating properties of market processes. He was profoundly vexed by the use on the part of his opponents of a static equilibrium framework when, as Mises had pointed out, the problem was fundamentally one of economic dynamics.

Hayek had modified his methodological views substantially by 1937. His primary concern was with the assumption of perfect knowledge required for the equilibrium concept as a basis for deductive economic theorising. In a paper, titled *Economics and Knowledge*, Hayek stated:

> "My main contention will be that the tautologies, of which formal equilibrium analysis in economics essentially consists, can be turned into propositions which tell us anything about causation in the real world only in so far as we are able to fill those formal propositions with definite statements about how knowledge is acquired and communicated." (Hayek, 1937, p.33).

This became a central concern for Hayek, since his primary preoccupation involved understanding how high level macroeconomic outcomes emerge as the result of the coordinated activities of individuals with dispersed knowledge. He sought to demonstrate how individuals following basic rules of property and individual learning in the context of changing local conditions and relative prices can give rise to design-like order via a mechanism lacking any top-down control. The reality of the knowledge assumptions thus became of the utmost importance to him. Lionel Robbins had earlier stated that the commonly used assumptions of rationality and perfect foresight weren't meant as reflections of reality, but instead, to:

> "enable us to study, in isolation, tendencies which, in the world of reality operate only in conjunction with many others, and then, by contrast as much as comparison, to turn back to apply the knowledge thus gained to the explanations of more complicated situations." (Robbins, 1935, p.94)

These assumptions then had no empirical content, and were not suited to the purposes of Hayek. It appears that Hayek's change in methodological views relates to his burgeoning friendship with Karl Popper. In fact, Hayek claimed in print that:

> "…ever since his *Logik der Forschung* first came out in 1934, I have been a complete adherent to his general theory of methodology" (Hayek, 1982, p.323)

The shared intellectual corpus between these two individuals is also evident in the comment by Popper, reminiscent of the quote above from Hayek, referring to Mises:

> "I think I have learnt more from you than from any other living thinker, except perhaps Alfred Tarski." (Hacohen, 2000, p.486).

Whether Popper influenced Hayek most, or vice versa, or whether in fact neither changed opinion based on the other, has been subject to much debate (Caldwell, 2006). But what is important to note here is that, firstly, the Austrian school during this period was not universally devoted to a priorism, and secondly, the methodological underpinnings of the leading theorists were developed with serious consideration of the philosophy of science, with reference to key figures within that field. Hayek pursued a protracted program under the title of "Abuse of Reason" in which he immersed himself in the study of economic methodology and the history and philosophy of science. The two central pillars of this program were: the epistemological status of the social sciences and the appropriate methodology for the study of complex phenomena; and an institutional analysis of law, social mores and politics (Boettke, Stein & Storr, 2018, p.67). This project led Hayek to a number of important conclusions. One conclusion was that there is an important epistemological distinction between the natural and social sciences. Hayek argued that even if attempts to reduce mental phenomena to physical phenomena were successful, the mental categories would still remain the appropriate explanatory categories. For while the physical sciences seek to determine the simple underlying causes of complex natural phenomena, the social sciences begin with an understanding of the underlying simple causative unit - the individual agent – and seek to reconstruct social complexity on this basis (Hayek, 1980). Another conclusion drawn by Hayek is the distinction between the sciences of *simple* and *complex* phenomena. He argued that the success of the natural sciences, as exemplified by physics, is due to the fact that the phenomena is considered simple enough to explain with a model containing only a few variables; not as the result of the application of a superior methodology (Hayek, 1967). Hayek believed that economists could only believe in the validity of the programs of market socialism and Keynesian demand management by disregarding the complexity of the phenomena under study. He argued that the degree of predictive specificity and comprehensiveness required to carry out these programs is utterly unattainable in the sciences of complex phenomena.

Hayek's philosophical influences went much further than Popper. Ludwig Wittgenstein was his cousin, and Hayek claims to have been one of the first readers of the Tractatus when it was released in 1922. He claimed that as he was, along with Wittgenstein, also influenced by Ernst Mach, the book had a significant impact on his subsequent thinking (Hayek, 1977).

It has been well recognised, even by Hayek himself, that around the time of publishing *The Pure Theory of Capital* in 1941, he left the field of economic theory for philosophical pursuits (Hayek, 1964, p.91). Consequently, it is difficult to discern how and when Hayek's methodological convictions evolved. In fact, it has been noted that:

> "the questions about whether and which of Hayek's scientific, methodological, and philosophical attitudes were more or less continuous across the arc of his career remains perhaps the central issue in Hayek scholarship" (Scheall, 2015, p.32).

But what is clear, is that Hayek came to realise that the analytical tools he had been employing in his early business cycle work were inadequate for the explanation of the complex phenomena under investigation (Hayek, 1941, p.v).

While Hayek eschewed the extreme a priorism advocated by Mises, he rejected outright methodological monism. He referred to this position as scientism, which he described as the illegitimate intrusion of the methods of the natural sciences into the realm of the social sciences (Hayek, 1952). But what he seems to mean here, is that the deductive-nomological model pushed by the logical positivists was inappropriate for economic science. This has already been argued in Chapter 1. It will be shown in Chapter 5 that many of Hayek's concerns and approaches are compatible with the methodology of complexity economics, which given the conclusion that complexity economics conforms to neo-mechanistic requirements, would suggest that Hayek's intended program could qualify as *mechanistic*.

3.4.4 Conclusions

The Austrian school of economics, at its commencement, followed the classical a priori methodology in their theoretical practices. And, like some of the methodologists of the classical era, they at least claimed that certain empirical approaches had a valid place within the overall enterprise of economics, whether, in practice, they pursued such avenues or not.

Carl Menger was inspired by the scholastic Aristotelianism that dominated intellectual circles in Vienna at the time. Friedrich Hayek, although spurning the methods of the natural sciences, was inspired by Karl Popper to take empirical content seriously. The modern Austrian school however, follows a methodological prescription of extreme a priorism based on the epistemological works of Ludwig von Mises, which were inspired by the concept of synthetic a priori propositions developed by Immanuel Kant. In this way, the modern school sharply diverged, methodologically, from the mainstream that the earlier Austrian School had merged into.

While the *goal* of the Austrians is squarely on the *explanation* of economic phenomena, with a focus on dynamic processes involving realistic assumptions, the purely deductive, a priori method used to construct explanatory *theories* is not compatible with the mechanistic explanatory requirements set out in Chapter 2. Further, the outright hostility to *methodological monism* and empirical *testing* of theoretical constructs - that are cornerstones of the modern Austrian School approach – strongly violates Neo-Mechanistic prescriptions.

3.5 The German Historical School[10]

The Historical School of economics developed in nineteenth century Germany in opposition to the Classical School. For almost 40 years, it was the dominant school of economic thought in German-speaking countries (Roll, 1992, p.276). The philosophical thought of Immanuel Kant and George Wilhelm Friedrich Hegel were the key starting points for the Historical School. The founders of the school believed that Kant had fatally undermined the rationalist project of the classical economists, wherein they had attempted to establish an entire system of *natural* economics and law based on reason alone. Hegel's work, heavily influenced by Kant, interpreted history as revealing over time, an essential underlying principle. It was this Hegelian concept of history that drove the methodology of the German Historical School.

Wilhelm Roscher, Bruno Hildebrand and Karl Knies comprise what is known as the *Older* Historical School. This group was followed by the *Younger* Historical School (Gustav von Schmoller, Lujo Brentano, Karl Bücher, Friedrich Knapp, and Adolph Wagner) and the *Youngest* Historical School (Arthur Spiethoff, Werner Sombart, and Max Weber) (Shionoya, 2005, p.1). I'll briefly touch upon the philosophical influences of each of these groups, and how these influences inspired their methodological convictions, in turn below. It will be shown that the methodological pronouncements of this school of economic thought also fail to conform to neo-mechanistic strictures.

3.5.1 The Older Historical School

Wilhelm Roscher was the first recognised economist of the Historical School. He claimed that historical empiricism should be an important element in the methodology of economic science, because economic laws are contingent upon their historical and social context (Roscher, 1843). Roscher felt than the main goal of economic science should not be directed toward generating a greater understanding of national wealth and its increase, but instead:

> "…representation of the economic aspect of what peoples have thought, wanted and felt, what they have striven for and attained, why they have striven for it and why they have attained it." (Roscher, 1843, p.IV).

In the pursuit of this directive, Roscher promoted a comparative study of all peoples, with an emphasis on the evolution of cultural stages. The goal was to elicit *law-like* features from amongst the body of information produced, summarised as a *developmental law* (Roscher, 1843, p.2). This body of work was to be conducted upon the lines of that produced by the Historical School of Law. The overriding principle of this legal tradition is that law is viewed as a custom and tradition of particular groups of people. Thus, law is to be *found* by jurists, not *made* by the organs of the state based on universal principles. Based on the Hegelian conception of spirit, law was considered a historical necessity, unable to be transplanted from one cultural context to another. The Historical School of Law was developed in opposition to the Natural Law approach, which assumes that law can be discovered only through a process of rational deduction from the nature of man.

Roscher failed to carry out the comparative studies he promoted, instead, he produced works on the history of economic thought (Tribe, 2002, p.7).

Contemporary to Roscher, was Bruno Hildebrand. Hildebrand rejected the idea of timeless economic laws for all countries, arguing for the contingency of economic phenomena, and promoted collaborations with branches of history (history of law, history of culture, history of civilisation, etc.) and statistics, to study the changing economic experience of mankind (Hildebrand, 1848). In emphasising the rejection of economic laws abstracted from time and place, Hildebrand proved a much more stringent critic of the classical economists than did Roscher. Although, he forcefully promoted the search for developmental laws, as had been laid out in Roscher's program. Hildebrand also made the distinction between theoretical economic analysis and practical policy implications, which Roscher hadn't, and concentrated his efforts upon theoretical analysis.

The third founder of the Historical school was Karl Knies. More extreme than both Roscher and Hildebrand, Knies argued that historical study was the *only* legitimate methodology for economics (Knies, 1853). He rejected entirely the deductive methods of the classical school. Knies also argued against the positions taken by both Roscher and Hildebrand. He criticised Roscher for failing to reject deductivism outright - for having effectively promoted historicism as an adjunct to classical methodology - and he criticised both Roscher and Hildebrand for promoting the search for developmental laws, because he viewed this as being suspiciously close to the goals the classicists were pursuing with their pure theory.

The key criticism made of the Older Historical School by defenders of the tradition, was that they did not engage in the systematic comparative histories of economic systems for which they so strongly called (Tribe, 2002, p.9).

3.5.2 The Younger Historical School

Gustav von Schmoller was the leader of the Younger Historical School. His vigorous attacks on the methodology of the classical and Austrian schools - in response to Menger's criticisms of historicism in his 1883 book - comprised what has become known as the *Methodenstreit* (method dispute) (See: Section 3.4 above). The *Methodenstreit* was to span decades. From the perspective of the Historical School, three key issues defined these methodological disputes: deductivism versus inductivism; the nature of the premises in the classical system; and the unity of social life (Roll, 1992, pp. 280-281). I will briefly address each of these in turn now.

First, the deduction versus induction debate. The classicists mostly saw themselves as carrying out a combined inductive-deductive methodology. The premises their theories were built from were considered to be based on empirical observation, and the conclusions of their theories were intended to be checked against reality so as to both determine the scope of these theories and to identify disturbing causes. Whereas Eric Roll argues that these considerations indicate that the historicist charge was unsubstantiated (Roll, 1992, p.281), I maintain, along with Blaug (Blaug, 1992, p.51), that the inductive proclamations of the classicists were hollow.

The second attack on classical methodology relates to the axioms from which deductive inference begins. The historicists argued that the assumption that man acts solely out of self-interest, is false. They wished to incorporate ethical elements into economic theorising to both increase realism and broaden the scope of economic analysis. But as was described in Section 3.2 above, this criticism mischaracterises the classical position. John Stuart Mill and John Neville Keynes, for instance, were quite clear that they considered the assumption of economic man as an abstraction from which to isolate purely economic factors, and that eventually economic science may well be able to isolate further factors, and create more comprehensive theories for the explanation of social phenomena. And, in a somewhat opposite direction, as was shown in Section 3.3.2 above, Ludwig von Mises was later to recognise that in the marginal utility approach lies a mode of analysis that is applicable to *all* human actions operating in a means-ends framework, no matter what the underlying motivation.

The third point of attack stressed the unity of social life and the organic nature of society. The historicists argued that society as a totality had an existence beyond the sum of its members. Inspired by developments in biological science, the historicists regarded society as an organic unity, composed of parts vitally related to one another, and undergoing a continuous process of development. Explanation of social phenomena was *not* to be found in the actions of individuals, as the Classical and Austrian Schools demanded.

The arguments Schmoller and his associates were putting forward assisted greatly in the realisation of their political goals. By rejecting classical economic theory, they could attack both laissez-faire liberalism and Marxist socialism, since both these movements relied on the results of classical economic analysis. They were then in a position to push for implementation of economic reforms on the basis of the results of their comparative economic studies.

The main criticism levelled against the Younger School was that, while they managed to generate a substantial quantity of economic-historical studies, it is far from clear how these studies related to the historicist programme that was originally set out by Roscher in 1843 (Tribe, 2002, p.9).

3.5.3 The Youngest Historical School

With the Youngest School of historical economics, the hostility toward analytic economic theory came to an end. These economists adopted a methodological approach that incorporated both theoretical and empirical elements. Wilhelm Dilthey was a particularly strong influence on this generation of Historical economists[11]. He viewed himself as the philosophical spokesman of the Historical School, and took it upon himself to provide the philosophical justification for the methodology they employed (Dilthey, 1883). He agreed with Auguste Comte and John Stuart Mill that social studies have scientific status, but rejected the validity of the methods of the natural sciences in these disciplines. Dilthey claimed that while the natural sciences had become independent of metaphysics through a prolonged clarification of their epistemological basis, the social sciences needed to go through such a process for themselves so they could also become independent of metaphysics. It was Dilthey's goal to expand upon Kant's *Critique of Pure Reason*, which he considered primarily nature-oriented, to create a *Critique of Historical Reason* oriented toward the social and cultural dimensions of human experience (Makkreel, 2016). He hoped that the human sciences would then be in a position to arrive at lawful explanations just like the natural sciences.

Dilthey's efforts resulted in the construction of a theoretical framework for studies in the humanistic sciences. This framework has four salient features (Krabbe, 1985, p.103). Firstly, research must begin with description and analysis of the most complex phenomena. Second, collective entities have no independent existence, but it is impossible to reduce social and cultural phenomena to the activities of individuals. The whole can only be understood in terms of the parts, while the parts can only be understood in terms of the whole. Third, understanding of the actions of individuals can be had by other individuals, since humans reflect upon and judge their actions, which represent outward expressions of their inner motivation. This form of knowledge differs from perceptual knowledge of external objects, which forms the basis of the empirical methods of the natural sciences. Fourth, Hermeneutics is an essential ingredient of social science methodology.

Hermeneutics is the theory and methodology of interpretation. It is a process of understanding that takes the outer manifestations of human action and explores their *meaning*. Dilthey claimed that it is only through the method of hermeneutics that one can move from an understanding of what is singular in history, to the level of universal validity. In his *Drafts for a Critique of Historical Reason*, Dilthey analyses the categories of life that are relevant to historical knowledge, distinguishing between *formal* and *real* categories (Dilthey, 1910). The *formal* categories of unity, plurality, identity, difference, degree and relation are said to be common to both the natural and human sciences. The *real* categories of meaning, value and purpose are considered central for the human sciences. The *understanding* (verstehen) approach to the social sciences amounts to:

> "…isolating formal categories into which historical individuals can be subsumed and uncovering how their behaviour is influenced by an absorption into a progressively more complex and heterogeneous whole." (Krabbe, 1985, p.104)

The objective common to the leaders of the Youngest Historical School was to shed light on the modern capitalist society, which they viewed as a special phase in historical development.

Werner Sombart is recognised as a leader of the Youngest School. He was a pupil of both Gustav von Schmoller and Wilhelm Dilthey. He attempted to fulfil Schmoller's vision of transforming economic science into an all-encompassing science of society. And he pursued this task by developing his own method of *understanding* based on Dilthey's body of philosophical work. This method involved the creation of *ideal type* economic systems to be used for analysing concrete reality, with a focus on three key aspects: the form and plan of organisation; the body of technology; and the unique spirit (state of mind) (Sombart, 1929).

Max Weber was another leader of the Youngest School. He was a pupil of Karl Knies -one of the founders of the Old Historical School. Weber dedicated a substantial part of his research effort to the examination of the methodological problems of the social sciences. Weber rejected much of the historicist work on the evolution of economic phases on the basis that it oversimplified the characteristics of reality (Weber, 1922). However, Weber promoted the idea that to gain understanding of social phenomena, one requires the aid of several ideal types. On ideal types Weber stated:

> "They are obtained by a one-sided emphasis on one or more different historical characteristics, and by bringing together a quantity of different and discreet phenomena which agree in possessing the particular historical characteristic which has been one-sidedly extracted. These are unified into a single mental picture. In its full conceptual purity this mental picture is never to be found empirically in the real world. It is a 'Utopia' and the task of the historian is to ascertain in each single case how near or how far the real world approximated to this ideal picture, that is, for example, how far the

> economic relations in a particular city correspond to the concept of the 'City Economy'". (Weber, 1973, p.191).[12]

Weber's theoretical ambition was a conceived work on the entire sociological system. He intended to incorporate the disciplines of economics, law, politics and religious sociology. The project never came to fruition. Although the majority of Weber's efforts were directed toward methodological issues, he is probably most known - at least in the English-speaking world - for his book *Die protestantische Ethik und der Geist des Kapitalismus* (The Protestant ethic and the Spirit of Capitalism) (Weber, 1905), in which he explains the emergence of capitalism, as a direct result of the ethical teachings of Protestantism.

Arthur Spiethoff is also recognised as a leader of the *Youngest* Historical School. He endorsed the legitimacy of analytical theory, and sought in his own works to balance this form of methodology with historico-statistical investigation. Speithoff's output on methodology and his original applied research (mostly on business cycle analysis) were both equally well received. In his works on methodology, Speithoff created the concept of economic *styles*. Arguing that since most economic phenomena change over time, theorists need to differentiate between a large number of patterns of economic life that have existed in history. He stated that:

> "as many patterns must be delimited as there are essential and typical differences in the basic institutions. Patterns of that kind are called economic styles." (Speithoff, 1933, p.132)

Speithoff asserted that each of these economic *styles* requires its own explanatory economic theory. He claimed further, that economic theories could be either timeless, and thus universally valid, or historical. Historical economic theories, built on ideal types, are not universally valid. Speithoff believed that the pure theoretical constructs of the classical school had approached perfection – particularly in the works of Ricardo, Menger, Jevons and Pareto. But to be of any use in producing understanding of concrete economic phenomena, these theories needed to be fused with appropriate historical ones, these being the theories produced by Schmoller, Sombart, Weber and himself.

3.5.4 Other Historical Schools

A movement related to the German Historical School sprang up in England, with its own variant of the *Methodenstreit*. The movement developed three main lines of argument in its opposition to classical methodology (Coats, 1954, p.143). Firstly, the purpose and scientific status of political economy was questioned. Secondly, the narrow scope of the discipline was attacked. Thirdly, they rejected the methodology of deduction from theoretical proposition, instead championing a historical approach based on empirical observation and inductive reasoning.

3.5.5 Conclusions

Each successive generation of the German Historical School had a clear understanding of the philosophies and philosophers that underpinned their approach to economic research.

Further, they devoted considerable portions of their energies to addressing methodological concerns, and developing appropriate methodological practices on the basis of these considerations.

The *goal* of each generation of Historical School economists was the explanation of economic phenomena. While it was broadly agreed that an inductive process focusing on historical techniques and statistical analysis should be the cornerstone of economic methodology, *explanatory requirements* varied by "generation" and by individual, with particularly high variation in the conceptual constructs that were considered necessary for building valid theoretical structures. William Roscher argued for the centrality of developmental laws, Karl Knies rejected deductive practices outright, favouring a methodology based exclusively on historical studies, Werner Sombart argued for the necessity of incorporating "ideal types", and Arthur Speithoff advocated a methodology that wedded classical deductive theoretical structures with historico-statistical analysis requiring consideration of "economic styles". The *testing* of theoretical constructs by the Historical School can be divided into two separate categories. Firstly, rigorous empirical analysis was considered essential. And secondly, given the prominence of hermeneutics within the methodological convictions of the school, interpretation of the content of theoretical constructs should provide *understanding* of the phenomena to be explained, by means of the categories of meaning, value, and purpose, else they fail to explain. *Methodological monism* was thus rejected by members of the Historical School, since the methodology of the social sciences, in their opinions, requires incorporation of the categories required for *understanding*.

It is my contention that with the adoption of the mechanistic framework of explanation presented in Chapter 2, it becomes possible to see how many of the concerns and insights of the German Historical School could be incorporated into a set of progressive research programs. In Chapter 5, it will be argued that the framework of Complexity Economics has the potential to realise such a goal.

3.6 Institutionalist Economics

The influence of the German Historical School was extremely strong in American economics in the 1880s and 1890s. This influence fed into a new economic movement called *Institutionalism*. The movement flourished on the back of works by Thorstein Veblen (Veblen, 1898; 1899; 1906a), Wesley Mitchell (Mitchell, 1913; 1914; 1915), and John Commons (1924). After an initial surge in popularity, Institutionalism drifted to the fringes of the discipline, until experiencing something of a revival later in the latter half of the twentieth century. The movement that gained widespread support during the decades of the 1920s and 1930s however, bears little resemblance, methodologically speaking, with what came after. In particular, the early Institutionalists thoroughly endorsed the application of the methods of the natural sciences to the problems of economic science, whereas later institutionalists flatly rejected such a stance of methodological monism. In this section, I'll trace the development of the methodological convictions of the institutionalist school of thought, with reference to the scientists and philosophers who inspired them. I will also show that Institutionalism in all its methodological forms fails to satisfy normative criteria set out by the neo-mechanistic explanatory framework.

3.6.1 The Beginning & Interwar Period

Thorstein Veblen, the recognised founder of the Institutionalist school, sought to build upon the methodological platform of the Historical School by redressing their failures. He stated of the Historical School:

> "The whole broad range of erudition and research that engaged the energies of that school commonly falls short of being science, in that, when consistent, they have contented themselves with an enumeration of data and a narrative account of industrial development, and have not presumed to offer a theory of anything or to elaborate their results into a consistent body of knowledge." (Veblen, 1998, p.375).

As a self-identified movement, Institutionalism emerged at the Thirty-First Annual Meeting of the American Economic Association in 1918, where Walton Hamilton delivered the manifesto of the group, wherein he boldly proclaimed that:

> "The "institutional approach" doubtless has some importance because it is a happy way to acceptable truth, but its significance lies in its being the only way to the right sort of theory… it is a denial of the claims of other systems of thought to be "economic theory."" (Hamilton, 1919, p.309).

The early institutionalists rejected the deductive nature of economic explanation espoused by the classical theorists, for an inductivist approach based on quantitative and historical studies. They did not promote the discarding of deductive methods altogether, for they believed them to be a natural part of any branch of science. But, they considered human sciences to be so much more complex and fluid than the natural science so that the former needed to be less deductive and more inductive than the later. John Clark wrote that:

> “Economics must come into closer touch with facts and embrace broader ranges of data than "orthodox" economics has hitherto done. It must establish touch with these data, either by becoming more inductive, or by much verification of results, or by taking over the accredited results of specialists in other fields, notably psychology, anthropology, jurisprudence and history. Thus the whole modern movement may be interpreted as a demand for procedure which appears more adequately scientific” (Clark 1927, p. 221).

In recognition of the need for a more thorough empirical approach to economic research, Rexford Tugwell argued for an *experimental economics*. He claimed that no theoretical economic results should be accepted as true unless they have been experimentally verified. In this, he claimed to be taking his inspiration from Newton and Galileo (Tugwell, 1924, pp. 386, 387).

The institutionalists forcefully rejected the ontology of classical economics. 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…" (Hamilton, 1919, p.318).

And concerning human behaviour it was claimed that:

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

Although it has been recognised that "there is no unanimity" in the definition of the concept of *institutions* (Hodgson, 2006, p.1), they have been defined in the following manner:

> "All human societies are characterised by more or less complex and overlapping networks of regular social interactions and practices. Whether economic, political or cultural, such repeated interactions require agreed and predictable rules – ways of doing things; such sets of rules constitute institutions." (Leftwich, 2006, p.1)

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. The resulting non-formal approach rejects the idea of universal economic generalisations, and instead, emphasises the uniqueness and individuality of particular systems.

Early American institutionalists saw themselves as engaged in the task of developing economics into a genuine science. To do this, they attempted to mimic the empirical aspects of natural science, arguing against what they viewed as the speculative metaphysical practices of the neoclassical program. They took major philosophical inspiration from Charles Sanders Pierce, William James and John Dewey, seeking to found their discipline upon a psychological approach to economic problems. They sought an interdisciplinary approach, establishing connections with other branches of social science to broaden the sources of available data for theoretical validation. Quantitative programs were embarked upon to assist in the provision of explanations of social phenomena.

Thorstein Veblen argued that the neoclassical program was scientifically backward, claiming:

> "…economics is helplessly behind the times, and unable to handle its subject-matter in a way to entitle it to standing as a modern science." (Veblen, 1898, p.373).

And what he thought was holding the profession back was:

> "…it is this facile recourse to inscrutable figures of speech as the ultimate terms of theory that has saved the economists from being dragooned into the ranks of modern science…By their use the theorist is enabled serenely to enjoin himself from following out an elusive train of causal sequence." (Veblen, 1898, p.383)

With the result that:

> "features of the process that do not lend themselves to interpretation in the terms of the formula are abnormal cases and are due to disturbing causes. In all this the agencies or forces causally at work in the economic life process are neatly avoided." (Veblen, 1898, p.384).

Two interconnected key criticisms that permeate throughout Veblen's writings on the scientific method and economic theorising are on display here in these quoted passages. Firstly, Veblen subscribed to a 'post-Darwinian' evolutionary concept of the scientific enterprise (Veblen, 1898). He claimed that previously, science had been infused with metaphysical notions seeking to explain phenomena in terms of teleological destinations. Metaphors centred around the purposive natural laws of a creator god gave way to metaphors of the designs of master craftsmen as the industrial age thrived, but these explanatory practices all centred around explaining phenomena in terms of end-state destinations (Veblen, 1906a). For example, Veblen highlights Adam Smith's invisible hand metaphor as an attempt to explain market activity in terms of an equilibrium end state (Veblen, 1898, p.381). For Veblen, pursuing such teleological explanations places a highly speculative methodology, where one based on cumulative cause and effect ought to be. He praised the Austrian School for focusing their attention on dynamics, but lamented that they limited their investigations to an extremely narrow scope, using the methods of 'old' science (Veblen, 1898, p.386-389). Veblen criticised contemporary economic science for:

> "Living over again in its turn the experiences which the natural sciences passed through some time back." (Veblen, 1898, p.384).

In his own words, this is how Veblen distinguished between the 'old' pre-evolutionary science and the 'new' post-evolutionary science:

> "For the earlier natural scientists, as for the classical economists, this ground of cause and effect is not definitive. Their sense of truth and substantiality is not satisfied with a formulation of mechanical sequence. The ultimate term in their systematization of knowledge is a " natural law." This natural law is felt to exercise some sort of a coercive surveillance over the sequence of events, and to give a spiritual stability and consistence to the causal relation at any given juncture." (Veblen, 1898, p.878).

Veblen contends that under this methodological approach any causal sequence that seems to contradict the working out of a natural law is disregarded as merely a "disturbing factor", when they should constitute the real focus of analysis.

The second major criticism that Veblen launched against the methodology of the classical, and Austrian, economists involved the assumed psychological nature of the theoretical human agents. He and his followers referenced advancements in psychology, sociology, social psychology, biology and cultural anthropology, to argue against such psychological assumptions. Of particular influence in this regard were William McDougall (Social Psychology), Wilfred Trotter (Social Psychology), and John Watson (Behaviorism). Veblen objected to what he called the hedonistic conception of man. This conception is founded upon the hedonistic psychology promoted by the British utilitarian philosophers during the eighteenth and nineteenth centuries, and which Veblen described as:

> "…that of a lightning calculator of pleasures and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated, definitive human datum, in stable equilibrium except for the buffets of the impinging forces that displace him in one direction or another." (Veblen, 1898, p.389).

Veblen rejected this conception as based on outdated psychological science. Instead, he, and his followers, allied themselves strongly to modern behaviourist psychology, informed as it was by modern anthropological research. Given this alternative conception, the evolution of individual economic agents is an important aspect of explaining economic processes and their outcomes; inherited traits and past experiences interact amongst a body of traditions, conventionalities and material circumstances, cumulatively causing the successive frames of mind that instigate economic actions. What is true for the individual under this psychological conception is also true for the groups within which the individual lives.

With these two key criticisms in place, Veblen establishes what he believes constitutes an appropriate basis for the methodology of economic science:

> "an evolutionary economics must be the theory of a process of cultural growth as determined by the economic interest, a theory of a cumulative sequence of economic institutions stated in terms of the process itself." (Veblen, 1898, p.393).

At the end of the nineteenth century, Pragmatist philosophy had surpassed natural-law philosophy in American intellectual life (Yonay, 1994, p.50). Charles Pierce and John Dewey wielded significant influence over the institutionalists. The ideas espoused by Dewey, concerning science as a product of problem solving activity and acting as an instrument of social reform, were especially embraced by the institutionalists following the first world war. These economists focused their attention on ethical concerns for welfare and well-being (Edie, 1926, p.viii; Wolfe, 1924, p.478). And Frederick Mills took pains to show that his views on the philosophy of science taken from Lord Kelvin, Clerk Maxwell and Karl Pearson were in accordance with the instrumentalist principles espoused by John Dewey (Mills, 1924, pp.43-46). Tugwell, showing his instrumentalist leanings, argued that:

> "the truth must be useful; and if science does not help to solve a problem it cannot reach out toward truth" (Tugwell 1924, p. 387).

Whereas Veblen conceived of science as a disinterested enterprise, emphasising explanation, the institutionalists of the inter-war period proved much more pragmatically focused.

Although Veblen was the primary inspiration for generations of institutionalists, his vision of economic methodology was not wholeheartedly embraced by his followers; some divisions emerged. John Commons, for instance, was critical of the wholesale importation of the methods of the natural sciences into the human sciences, going so far as to reject outright Veblen's appeals to cumulative efficient causation (Commons, 1934, pp.96, 651-655). Wesley Mitchel, although not criticising Veblen's vision, did however, eventually contend that due to the impossibility of acquiring historical data of suitable quality, it was impossible to pursue studies of cumulative change and life histories as proposed by Veblen. Because of this, Mitchell claimed that Veblen's *actual* practice resembled those of most orthodox economists (Mitchell, 1936, p.xxxi). In terms of how practice *should* proceed however, Mitchell held wholeheartedly that imitation of the experimental methods of the natural sciences was essential. He states:

> "There seemed to be one way of making real progress, slow, very slow, but tolerably sure. That was the way of natural science (Mitchell, 1928, p.413).

This was the driving idea behind the establishment of the National Bureau of Economic Research (NBER) as a *statistical laboratory*. The goal was to improve economic measurement, with the purpose of producing quality data for empirical analysis. NBER subsequently played a vital role in the development of a vast number of measurement areas, including national income accounting, monetary and financial data, economic indicators for business cycle analysis, and general statistical improvements of government agency activities – including the Federal Reserve and Department of Labor.

In the mid-1950s, Kenneth Boulding, taking a fresh look at old institutionalism after its apparent demise, discerned three primary areas of dissension against orthodox economics. Firstly, there was discontent with the static nature of economics and an associated push for dynamical models. Secondly, the institutionalists took issue with the highly abstract nature of orthodox theorising, arguing instead for an integration with other social sciences by incorporating more realistic accounts of psychological and sociological variables. And thirdly, they were highly dissatisfied with the lack of empirical feedback into economic theory, arguing for detailed and accurate empirical research (Boulding, 1957, pp. 8-11).

But how do all these platitudes combine to form an actual explanatory methodological practice? The resulting methodology has been described as a form of storytelling, called *pattern modelling* by Abraham Kaplan (Kaplan, 1964). 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" (Wilber & Harrison, 1978, p.73).

Despite apparently sharing a common methodology, it has been widely noted that the institutionalists have not proven capable of generating a body of shared theory (Wisman & Rozansky, 1991). This isn't 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 (Wilber & Harrison, 1978). 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 hypotheses – interpretations of the themes – for validity testing. Sources of empirical evidence for validity testing include quantitative and statistical methods, case studies, documentary evidence, judicial opinions and court proceedings. 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.

3.6.2 Middle Institutionalism

The institutional approach all but died out within mainstream economics circles for a period between the 1940s and 1970s. Kenneth Boulding noted that:

> “there are a few economists today who would call themselves Institutionalists, but these tend to be isolated individuals, and there is not today anything which would be called either an institutionalist “movement” in economics nor even an institutionalist group.” (Boulding, 1957, p.1)

Whatever research that was still carried out under the institutionalist banner during this period, took place not within economics departments, but instead, was banished to the departments of sociology and related social sciences (Rutherford, 2001). The decline of Institutional economics can be explained as being the result of the rise of mathematical economics and the formalist revolution (see: Chapter 4 below). Gunnar Myrdal explains that after World War II conventional economists “narrowed and hardened their isolation from the other social sciences" (Myrdal, 1978, p. 773). This move from *Political Economy* to *Economics* was considered to be in exactly the wrong direction.

During the 1970s, the Institutionalist approach resurfaced, as several prominent Institutionalists produced a body of work organised around the idea that power and conflict constitute central aspects of the economic process. (Galbraith, 1973; Samuels, 1971, 1974; Mueller, 1979; Craypo, 1975).

3.6.3 European Institutionalism

Institutionalism in Europe did not enjoy the virtual dominance that the movement in the United States achieved during the 1920s. One prominent institutionalist operating within Europe during the twentieth century was John Hobson. Hobson, who was excluded from the academic community in London due to his ferocious attacks on the classical orthodoxy, has been described as Thorstein Veblen's English counterpart, but without his picturesqueness and wit (Boulding, 1957, p.6). But by far the most prominent European institutionalist was Gunnar Myrdal.

Myrdal, who helped to establish the Stockholm School of Economics, earned his doctorate in 1927 at Stockholm University with an analysis of the role of expectations in price formation. His dissertation supervisor was Gustav Cassel, a mathematician turned economist responsible for the "Walras-Cassel" general equilibrium model that via the Vienna Colloquium in the 1930s through to the Cowles Commission in the United States initiated modern mathematical economics (see: Section 4.3 in the following chapter).

Myrdal has divided his own life into three distinct periods of methodological convictions: theoretical, political, and institutional economics (Myrdal, 1969, p.10).

Myrdal's theoretical work in monetary economics, exemplified by his publication *Monetary Equilibrium* (Myrdal, 1939), was conducted in a standard neoclassical form. And initially, Myrdal was not amenable to the Institutionalist program. For example, while on a one-year trip around the United States to study American social science methodology and the methods of American social psychology, funded by a Rockefeller grant, Myrdal wrote back home to Gustav Cassel of Wesley Mitchel's "banalities" and "senseless generalisations" (Cherrier, 2009, p.39). And Myrdal claimed that the formation of the Econometric Society, which he helped establish, was a defence strategy against the advancing institutionalists (Myrdal, 1978, p.772). However, his publication *An American Dilemma: The Negro Problem and Modern Democracy* (Myrdal, 1944) marked a move to Institutional Economics (Cherrier, 2009, p.34). Myrdal stated that he learnt from his increasing involvement in field work research that theorising was not adequate for the development of economic models (Myrdal, 1973, p.196). He argued that real-world institutions should play a critical role in economic analysis. And he also became increasingly convinced that there are no economic, sociological, or psychological problems; there are only problems that are mixed and composite (Myrdal, 1978, p.772).

In giving an account of what he considered Institutional Economics to comprise of, Myrdal states:

> "The most fundamental thought that holds institutional economists together is our recognition that even if we focus attention on specific problems, our study must take into account the entire social

> system, including everything else of importance for what comes to happen in the economic field. Foremost, among other things, is the distribution of power in society and, more generally, economic, social, and political stratification; indeed, all institutions and attitudes. To this must be added, as an exogenous set of factors, induced policy measures, applied with the purpose of changing one or several of these endogenous factors...I believe the common denominator among institutional economists is their tact acceptance of a master model which encompasses the movement of the whole social system, within which there is causal interdependence." (Myrdal, 1978, pp. 773-774/775)

The recognition that there is no basic single causal factor implies the necessity of an approach that deals explicitly with interdependence. And it also implies that there is unlikely to be any role for equilibrium concepts to play.

Myrdal became highly sceptical of the intimate connection between economic theory and mathematics, arguing that given such an approach deals exclusively with economic factors, it has no future. Myrdal was convinced that the profession would move wholesale to embrace the methodology of Institutional Economics since:

> "much that is now hailed as most sophisticated theory will in hindsight be seen to have been a temporary aberration into superficiality and irrelevance." (Myrdal, 1972, p.11)

Myrdal also became increasingly critical of economists hiding their values under the guise of objectivity and demanded that hidden pre-analytic value premises be made explicit. In his own case, he promoted the values of equity, and concern for the poor and underprivileged, in addition to the value of economic efficiency. These were values that he claimed were held by the majority of individuals within society. He contended that these explicit values need to be tested for: relevance; significance; compatibility; and feasibility (Myrdal, 1972, p.55). As was common across the institutionalist spectrum, Myrdal spoke vociferously against *welfare theory*, arguing that it is based on the superseded philosophical theories of hedonism and the moral psychology of utilitarianism. He argued that normative content derived from utilitarianism and natural law philosophy had infused economic concepts such as *productivity*, *equilibrium*, *value*, *utility*, *welfare*, and etc., from the works of John Stuart Mill, John Elliot Cairnes and John Neville Keynes through to the modern welfare economics propounded by Arthur Pigou (Myrdal, 1930, p.xlvii).

A key methodological principle promoted by Myrdal goes by the name of *Circular Cumulative Causation* (CCC). It was first formulated in Appendix 3 of his 1944 book on race relations. The Circular "C" refers to positive feedback mechanisms. Myrdal states that:

> "…circular causation will give rise to a cumulative movement only when…a change in one of the conditions will ultimately be followed by a feed-back of secondary impulses…big enough not only to sustain the primary change, but to push it further. Mere mutual causation is not enough to create this process." (Myrdal, 1968, p.1875).

And, by limiting one's analysis to purely economic factors, one will be incapable of capturing positive feedback cycles between economic and non-economic factors, which may be the primary causal factor in an explanatory situation. The relevant factors, can only be determined empirically. CCC is the antithesis of the stable equilibrium approach to economic analysis. It accommodates both continual evolution of the system as well as the idea that interaction between factors can affect the pathways upon which the system may travel.

Karl William Kapp, considered to be an important intermediary between American and European institutionalism (Heidenreich, 1998, p.966), considered CCC to be the key concept of institutional economics. He both developed and applied it in his work. Kapp acknowledged that taking CCC seriously, demands interdisciplinarity, and questions the genuine autonomy of individual social sciences. Kapp listed the main characteristics of CCC as:

(1) It frames problems;
(2) It brings problems closer to solution;
(3) It necessitates an identification of relevant causal factors;
(4) It necessitates a causal analysis of real interaction relationships;
(5) It necessitates a systems view;
(6) It necessitates analysis of temporal processes; and
(7) It avoids teleology, the projection of ready-made meanings, relationships, results and processes.

(Berger, 2008a, p.360).

Kapp insisted on an integrated social enquiry based on an historical and empirical approach (Kapp, 1957). He argued that by isolating purely economic factors, the neo-classical approach violated the epistemological demands determined in the philosophy of science set out by philosophers such as John Dewey (Kapp, 1968, p.1). Instead, he advocated a theoretical approach that was heavily influenced by cultural anthropology, social psychology and sociology. And in fact, alongside his more "economic" work, Kapp worked on areas of modern behavioural theory, cultural anthropology, biology, and systems theory (Berger, 2008b, p.385). Kapp admitted that the "rationality assumption" of neoclassical economic analysis is a good approximation of the behaviour of entrepreneurs, and thus a legitimate starting point for the theory of production, but that it provides a particularly poor approximation of individual consumer behaviour, noting that:

> "There seems to be fundamental agreement among psychologists of different schools that the behaviour of human beings is influenced and determined by a complex mixture of instincts, emotions, passions, impulses, habits and prejudices and a complicated interaction of customs, conventions, fashions, mass-suggestions and other modes of persuasion. Hardly any of these factors is conducive to rational choice." (Kapp, 1943, p.142).

And he concludes that:

> "it becomes evident that the traditional acceptance of consumers' preferences as the sole and only measure of what contributes best to their well-being fails to serve any scientific purpose…the economist's neutrality towards consumers' "ends" tends to defeat his search for philosophical truth…any non-committal attitude which takes as premises only what consumers are actually seen to do, without inquiring into the forces which mould their preferences and desires, can only produce non-committal, if not misleading, conclusions of little practical significance." (Kapp, 1943, p.147).

Kapp advocates for the devotion of great attention to the study of the forces that influence consumer behaviour. And he expects that such an analysis will likely show that the forces discovered will be so great in number so as to render individual behaviour irregular. In such case, the use of any simplifying assumption would render analysis unrealistic and misleading. Kapp therefore, rejecting the idea of individual rationality, dismisses the notion of consumer sovereignty – that the individual is the best judge of his general wellbeing. The alternative is to search for objective standards for the appraisement of individual "ends", and to proactively employ programs of education and persuasion to encourage and develop the rational faculties of mankind.

3.6.4 New Institutionalism

By the late 1970s, some elements of the old institutionalist program were being pursued within the neoclassical paradigm. Malcolm Rutherford points to several motivating factors that were responsible for this revival (Rutherford, 2001, pp.186-190). One factor was a concern with the overregulation of markets. Another factor was renewed concern over a more plausible theory of psychology. This concern led to a number of works in the areas of decision-making, bounded rationality, expectations, and game theory. The growing body of research in these areas resulted in a movement called the *new institutional economics*. The important thing to note about this movement is that unlike the earlier institutionalist movements, this one is wedded to the methodology of the orthodox neoclassical program; it merely seeks to extend the application of this methodology to some of the issues that earlier institutionalists were interested in. This school of thought then belongs, methodologically speaking, within the positivist inspired framework described in Chapter 4 below.

3.6.5 Conclusions

The early Institutionalists were highly concerned for the scientific status of their discipline. In an attempt to secure scientific status, these economists argued strongly that a new methodological approach was required based on modern developments in philosophy and the other sciences. They sought to apply the methods of the natural sciences to the explanation of economic phenomena, in order to transform the discipline from a deductively based one to an inductive one centred on quantitative studies. To do this, they championed an *evolutionary* causal approach in opposition to the dominant equilibrium based one, and a complete reorientation away from the traditional hedonistic and utilitarian grounded psychological assumptions about theoretical human agents in favour of ones based on modern behaviourist psychology. In all their methodological pronouncements, the early institutionalists were heavily influenced by the works of the pragmatist philosophers of science including Charles Sanders Pierce and John Dewey.

What can be said about the neo-mechanistic credentials of the institutionalist methodological commitments? Although, in the works of Thorstein Veblen, explanation looms large as the primary *goal* of the scientific enterprise, the heavy influence of the American pragmatist philosophers of science dictated that the goals of prediction and control were the predominant concerns of the early institutionalists. It is not entirely clear as to what constitutes a valid scientific *explanation* under the institutionalist framework. Since they left no shared body of theoretical work, we can only refer to their explicit methodological pronouncements, and these can only get us so far. *Theories* are supposed to be constructed through a participant-observer process known as pattern modelling.

Rigorous empirical work is required as a means of *testing* and validating theoretical constructs. The institutionalists were proponents of *methodological monism*.

I conclude, based on the preceding observations – derived using the heuristic introduced in Section 3.1 above – that the institutionalist methodology is not particularly mechanistic. This is not to say, however, that the concerns of the institutionalists cannot be satisfied under a neo-mechanistic methodological framework. In chapter 6, I will argue that the Complexity Economics framework provides a basis for doing just that.

3.7 Conclusions

In this chapter, I explored the methodological convictions of the most prominent schools of economic thought from the inception of economics as a distinct branch of scientific investigation in the pre-modern paradigm period. I showed how, in all cases, serious consideration was given to contemporary philosophy of science in deriving methodological approaches. I also showed that these methodological frameworks fail to conform to the stipulations of the Neo-Mechanistic model of scientific explanation that dominates current discussion.

There is no doubt that each of the schools of thought discussed have methodological convictions that are of great value to the practice of economic science. In particular, the *Institutionalists* promote an inductive approach based on evolutionary principles (no hypothetical steady state analysis), and the *Austrians* promote a dynamical approach.

What I claim is missing, is an appropriate overarching methodological framework capable of unleashing the productivity of these various approaches.

In the following chapter, I continue my analysis of the relationships between accounts of scientific explanation within general philosophy of science and the methodological convictions of economic scientists. I do this by exploring the methodological approach of the current orthodox paradigm.

Chapter 4 - Positivist Economics

The purpose of this chapter is to show how the methodological practices of the mainstream of the economics profession during the twentieth century, and beyond, was, and continues to be, influenced by the logical positivist movement, and the Deductive-Nomological model of scientific explanation in particular. And as such, given the analysis in Chapter 1 and Chapter 2, the associated methodological commitments clearly do not meet the normative strictures of the Neo-Mechanist model of scientific explanation and theory structure.

4.1 Introduction

During the period from the 1930s to the 1960s, mainstream economics embraced the flourishing philosophical school of positivism. The rise of a mathematical approach to economic theorising consigned the methods of the Classical, Austrian and Institutionalist schools to the fringes of the discipline. In this Chapter, I will show how this process unfolded, with reference to key works of economic methodology and the underlying philosophical convictions. 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 Milton Friedman and Paul Samuelson. It will also be shown in this Chapter that the methodology employed by the positivist-inspired neoclassical synthesis does not conform to neo-mechanistic standards. This chapter is structures as follows. Firstly, in Section 4.2 I show how Terrence Hutchison, Oskar Morgenstern and Fritz Machlup introduced the language of logical positivism into debates on the methodology of economic science. Then, in Section 4.3, I explain how the result of this introduction of positivist language was the complete mathematisation of economic theory through the mathematical economics and econometrics movements. Next, in Section 4.4, I present the methodological pronouncements of Milton Friedman and Paul Samuelson which have thoroughly shaped the methodological commitments of the economics profession through to current times. In Section 4.5, I document the emergence of a distinct sub-field of economic science: economic methodology, and provide several remarks regarding contemporary practice within modern economic science, before re-iterating the central arguments of this chapter, with which I conclude in Section 4.6.

4.2 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 Chapter 1 (see: Section 1.3), 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 economic methodologists attest to the influence that positivist philosophers had on economic methodology. Three significant examples can be found in the works of Terrence Hutchison, Oskar Morgenstern and Fritz Machlup. I'll address each of these methodologists in turn below.

4.2.1 Terrence Hutchison

Terrence Hutchison, who has been described as one of the three most influential (pre-1980s) twentieth century economic methodologists (after Milton Friedman and Paul Samuelson) (Hands, 2001, p.48), devoutly introduced logical positivist philosophy, and its attendant language to bear on economic methodology. In his *Significance and Basic Postulates of Economic Theory*, 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, 1938). Throughout his writings, Hutchison amalgamated (at least) three different ways that positivist philosophers had demarked the cognitively meaningful propositions of science from the cognitively meaningless propositions of non-science: the logical positivist criterion of cognitive meaningfulness; the logical empiricist criterion of empirical testability; and the Popperian criterion of falsifiability (Hands, 2001, p.50).

Hutchison aimed a fervent attack on the methodology of the a priorists, with Ludwig von Mises his primary target (later adding the Marxists to his a priorist target list). 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 Moritz Schlick as a criterion for meaningfulness for a proposition (Schlick, 1918). At the time of writing, the impossibility of verificationism had been recognised, and so Hutchinson argued for a falsification criterion for intersubjective empirical tests.

Hutchison believed that the primary goal of economic science was to provide relevant advice for improving economic policy. Since this advice would generally require predictive capacities, he argued that economic theories need to be constructed in such a way as to facilitate prediction. Although he enthusiastically endorsed and promoted the positivist program, there were some meaningful departures evident in his works. For one, he was circumspect about the nature of the *laws* of economic science, recognising that they do not conform to the required status of universal generalisations; they are not valid for all times and places. Hutchison interpreted this observation as meaning that *more* testing of theoretical propositions is required in the social sciences than in their physical counterparts.

4.2.2 Oskar Morgenstern

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, as well as Karl Menger's Mathematical Colloquium, 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 Gottlieb Frege, Bertrand Russell and Ludwig Wittgenstein, 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 axiomatisation 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 (von Neumann & Morgenstern, 1944). 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).

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. He argued that economists had failed to verify theoretical constructs through experimentation and offered means by which this situation could be rectified so that economic science could eventually take its place amongst the "advanced empirical sciences" (Morgenstern, 1950; 1954). To this end, he advocated both small scale controlled experiments by business firms, and large scale direct experiments on the economy as a whole. Morgenstern also advocated "laboratory" experiments where possible. He stated that:

> "The possibilities of controlled direct experiments in the economy as a whole are very numerous—contrary to a widespread belief of the opposite. Indeed, they are only limited by the amounts of money one wishes to devote to them and by restrictions of ethics, common decency, political prejudices and the like—all of them very sound restrictions. However, even within these restrictions a larger monetary effort could provide significant quantities of new information not available so far" (Morgenstern, 1954, p.515).

The publication of *Theory of Games and Economic Behaviour* in 1944 is considered to have profoundly influenced the development of the established sub-field of economics known as Experimental Economics (Roth, 1993, p.186). Morgenstern also promoted computational experiments. He stated that:

> "We distinguish two types of experiments: (1) Experiments of the first kind are those where new properties of a system are to be discovered by its manipulation on the basis of a theory of the system; (2) Experiments of the second kind do not primarily rely on a theory but aim at the discovery of new, individual facts. The distinction is not sharp, since the results of the experiments of the second type are eventually incorporated into a theory whereby they receive their standing.

> We can now state a general thesis: Every computation is equivalent to an experiment of the first kind and vice versa. The equivalence rests on the fact that each experiment (certainly each of the first kind) can be conceived of as being—or using—an analogue computing machine" (Morgenstern, 1954, pp. 499–500).

The specific concern that led Morgenstern to promote widespread conducting of experiments was the problematic reliance on analysis of existing data. He believed that verification of theoretical propositions required new experimental sets of microdata (Ortmann, 2016, p.206).

4.2.3 Fritz Machlup

Fritz Machlup, drawing heavily on Richard Braithwaite's exposition of the Deductive-Nomological model (Braithwaite, 1953), 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 1955 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" (Machlup, 1955, 1956; Hutchison, 1956).

4.2.4 Conclusions

Terence Hutchison, Oskar Morgenstern, and Fritz Machlup provide three prominent examples of how logical positivist positions in the philosophy of science became transplanted and propagated throughout the economics profession from the 1930s. While Hutchison was prompted to action in response to the a priorist methodological programs promoted by Lionel Robins and Joan Robinson at his home university at Cambridge, Morgenstern and Machlup were responding to the developments within the various intellectual circles in Vienna of which they were both associated with to various degrees. The primary methodological principles espoused by this group included: the use of mathematical language in place of natural language; deductive derivation of theoretical propositions from fundamental postulates; and the requirement of submitting theoretical structures to empirical test. Essentially, the deductive-nomological model of scientific explanation was imported into economic science from the philosophy of science.

4.3 Mathematical Economics & Econometrics

The result of all this and resultant activity was the complete mathematisation of economic theory. While the deployment of mathematical expression in economic discourse has a long history, it has been argued that the earlier associated works had little to no impact upon the profession until the work of Antoine-Augustin Cournot in 1838. This observation has been attributed to a predominant belief in the failure of the analogy between the price system and the rational-mechanics underpinning the science of physics, to which the authors of works of mathematical economics sought to emulate (Mirowski, 1991). And while it was common to regard both the mathematical elaboration of economic theory and methods involving statistical estimation as part of a single research program (Mirowski, 1989a), by the mid-1950s two separate distinctive approaches had clearly emerged: mathematical economics and econometrics. Each of these are discussed in turn below.

4.3.1 Mathematical Economics

The foundations of mathematical economics were established during the nineteenth century, with particular momentum being gained in the final quarter of that century, when a large number of scientists and engineers trained in physics produced prominent works in economic theory. The most notable names within this cohort includes: William Stanley

Jevons, Leon Walras, Francis Ysidro Edgeworth, Irving Fisher, and Vilfredo Pareto. All of these theorists imported the concept of *equilibrium in a field of force* from physics and sought to construct economic theories on this basis. (Mirowski, 1991). The key pieces of literature include: Cournot, 1838; Jevons, 1871; and Walras, 1874. These works, and those developed on the basis of them, have been referred to as "classical mathematical economics" (Derakshan, 2017).

A second substantially-sized cohort of physical scientists and engineers entered the economics profession during the ten year period of roughly 1925 to 1935. Whereas the former cohort was concerned with transforming economics into a truly scientific explanatory endeavour, this latter cohort was more interested in applying the scientific method for the purposes of prediction and control, as a means of social engineering. Some of the most notable individuals of this cohort included: Ragnar Frisch, Tjalling Koopmans, Jan Tinbergen, Maurice Allais and Kenneth Arrow. One mathematician turned economist claimed that:

> "There is not only an opportunity for mathematics and economics, but even a duty; and on mathematics in an unusual degree lies the responsibility for the economic welfare of the world." (Evans, 1925, p.110)

Upon entering the economics profession, these scientists found the field populated with formal neoclassical models, the structures of which they were familiar with, and so took easily to the development of these models as well as evolving them by means of the application of newer methods more recently developed in the physical sciences (Mirowski, 1991).

The debates within the science of mathematics on the foundations of its discipline during the late nineteenth and early twentieth centuries impacted the development of mathematical economics appreciatively. The earlier mathematical economists adhered to a view of the nature of mathematics as physical truth. Under this interpretation, reasoning was constrained by the natural phenomena and physical reality toward which the mathematics was used to explain; mathematics was closely allied to physics. Calculus was the primary instrument of practice. But early in the twentieth century, David Hilbert promoted a different interpretation of mathematics, one as rigorous reasoning from axioms by the deduction of theorems, whose validity is established on the basis of consistency. Hilbert inspired the Bourbaki collective that transformed mathematics in 1930s France. Roy Weintraub has summarised the situation thus:

> "The crisis, or rather the interlocked crisis, of mathematics and physics was resolved by the formalist position on explanation whereby mathematical analogy replaced mechanical analogy, and mathematical models were cut loose from their physical underpinnings in mechanics. The result was that in the first decades of the twentieth century a rigorous argument was reconceptualised as a logically consistent argument instead of as an argument that connected the problematic phenomenon to a physical phenomenon by use of empirical data: propositions were henceforth to be "true" within the system considered (because they were consistent with the assumptions) and not "true" because they could be grounded in "real phenomena."" (Weintraub, 2002, p.51).

And Weintraub, citing arguments provided by Giorgio Israel (Israel, 1988; 1991a; 1991b; Israel & Nurzia, 1989), shows how this new reconceptualization of the notion of "rigorous argument" within mathematical science was highly suited to the development of economic theory at the time due to the non-empirical nature of the discipline (Weintraub, 2002, pp. 50-51). One prominent heterodox economist once quipped that "the prestige accorded to mathematics in economics has given it rigor, but alas, also mortis", since it is not possible to pursue the grand style of projects carried out by the classical economists under such a restrictive methodology (Heilbroner, 1979, p.198). Weintraub provides an enlightening discussion of the methodological convictions of Griffith Evans and how his convictions concerning the proper approach to mathematical economics was rejected as the formalist revolution swept through the discipline. He summarises the outcome as:

> "The point is that Evans's views on mathematical modelling are the views of an econometrician or applied economist today, or one who insists that the assumptions and conclusions of an economic model, a model constructed and developed mathematically, must be measureable or quantifiable. This is the distinction between "modelers" (or "applied economists") and "theorists" that divide modern departments of economics even as both groups consider themselves to be neoclassical economists…In a real sense, the distinction between rigor as materialist-reductionist quantification and rigor as formal derivation, a distinction contested at the end of the nineteenth century but which disappeared as formalism took hold in mathematics, re-established itself in the distinction between *econometrics* and *mathematical economics*" (Weintraub, 2002, pp. 70-71 (italics mine)).

The members of Karl Menger's colloquium, or the Vienna Mathematical Colloquium as it is also known, examined the general equilibrium model developed by Leon Walras and

Gustav Cassel, redesigning it from its very foundations. This group included the economists Oskar Morgenstern and Karl Schlesinger, the mathematicians Karl Menger, Abraham Wald and John von Neumann, and the logician Kurt Godel. The mathematical philosophy of David Hilbert provided two methodological pillars for this group: “the axiomatic method, and the principle of hierarchical interdependence between a plurality of theories and the unifying uniqueness of the metatheory behind them” (Punzo, 1991, p.5). The axiomatic method promoted by Hilbert generates the notion of economic science as the analysis of formal systems. It is no longer considered a system of synthetic representations of actual economies. And, it requires a metatheory to explicitly state and justify the “rules of the game”. The metatheoretical principles are essentially the logical positivist model of theory construction. These ideas sourced from the formalist school of mathematics were introduced into economics directly, without any time lag, since the mathematicians concerned were extremely interested in finding applications for their approach within various fields of the applied sciences (Punzo, 1991, p.6). I will now outline in brief some of the early landmarks in mathematical economics that this group generated.

John von Neumann axiomatised utility theory and constructed a model of an expanding multi-sectoral economy. Von Neumann opted for an approach based on inequalities, instead of equations, and proved the *minimax theorem*, which establishes the existence of an equilibrium for a class of two-person zero-sum games. This new formulation of the problem resulted in a move away from arithmetic and geometric methods, in favour of combinatorial and topological methods. Von Neumann also introduced the notion of several alternative model realisations, breaking the one-to-one relationship between an economy and its model. And thus, the canonical general equilibrium model was born, with all its essential formal rules and equilibrium properties. The model is a formal system conceived as a closed logical universe[1].

Abraham Wald constructed the first proof of equilibrium existence (Wald, 1936) and introduced the Weak Axiom of Revealed Preference (WARP), which would later be developed by Paul Samuelson. He also introduced the *Wald test* (Wald, 1943), which would become a staple of econometric practice.

The central analytical issue that was common to all this work is the proof of logical consistency. The application of the formalist methodology with its different mathematical techniques resulted in there being very little of Walras in the neo-Walrasian program. In particular, the dynamical elements of the approach taken by Walras and Cassel were excised. The Walrasian *tatonnement* was a dynamic argument for the existence of an equilibrium (Goodwin, 1951), and Cassel's existence argument outlines a heuristic algorithm mirroring a market equilibrating mechanism (Punzo, 1991), but the purely logical procedure for the statement of a dynamic property, imported from Hilbert, necessarily rendered neo-Walrasian general equilibrium theory static.

Gerard Debreu took Bourbakism to the Cowles Commission in 1949, and for a variety of reasons, within a year it had "become the house doctrine" (Weintraub, 2002, p.119). Under the current research director at the commission – Tjalling Koopmans – the research program had been reoriented from a focus on empirical work toward mathematical theory. Debreu's *Theory of Value* (Debreu, 1959) was intended to serve as a direct analogue of Bourbaki's *Theory of Sets* (Bourbaki, 1939-), and was to:

> "establish the definitive analytic mother-structure from which all further work in economics would depart, primarily either by "weakening" its assumptions or else by superimposing new "interpretations" on the existing formalism" (Weintraub & Mirowski, 1994, p.265).

Bourbaki promoted the primacy of *pure* science over *applied* science. Murray Gell-Mann lambasted this mode of pure, isolated analysis:

> "The apparent divergence of pure mathematics from science was partly an illusion produced by the obscurantist ultra-rigorous language used by mathematicians, especially those of a Bourbakist persuasion, and by their reluctance to write up non-trivial examples in explicit detail…Pure mathematics and science are finally being reunited and, mercifully, the Bourbaki plague is dying out" (Gell-Mann, 1992, p.7)

The core of modern mathematical economics was developed by a group of graduate students in the United States in the 1930s, which included Paul Samuelson (see: Section 4.4.2 below), Kenneth Arrow, Milton Freedman (see: Section 4.4.1 below) and George Stigler. Another group, centred on John Hicks, emerged at the London School of Economics. Hicks produced a mathematical interpretation of John Maynard Keynes' General Theory, introducing the IS-LM model. Early success was not forthcoming: The entire 1933 volume of the American Economic Review, for example:

> "contained exactly four pages where any mathematical symbol appeared, and two of them were in the Book Review Section" (Debreu, 1991).

But by 1950 the line of work carried out by these groups had become the mainstream of economics. The transformation of economic science during the late 1940s and 1950s has become known as the "Formalist Revolution" (Ward, 1972, pp. 40-41). It marks the time when the profession formed an absolute preference for the *form* of an argument over its *content* (Blaug, 2003, p.145). Debreu points to the publication of von Neumann and Morgenstern's Theory of Games and Economic Behaviour in 1944 as the sharp turning point in the history of mathematical economics. Continuing his commentary above concerning the quantity of mathematical reasoning in the American Economic Review, he states:

> "In 1940, less than 3 percent of the refereed pages of its 30th volume ventured to include rudimentary mathematical expressions. Fifty years later, nearly 40 percent of the refereed pages of the 80th volume display mathematics of a more elaborate type." (Debreu, 1991)

The keystone paper that epitomises the acceptable form of economic research is the 1954 article by Kenneth Arrow and Gerard Debreu titled: *Existence of Equilibrium for a Competitive Economy*. The research program of mathematical economics consisted of two main elements (Yonay, 1991, p.382). Firstly, it involved the transformation of Marshallian economics into a developable mathematical framework. Calculus was the primary tool used within a setting of maximisation and minimisation under constraints. The second major element was equilibrium theory based on the work of Leon Walras. There was a certain tension evident between these two elements, which was particularly evident at

Chicago University, where the Walrasian program was carried out by the Cowles Commission while the economics department dedicated themselves to the Marshallian program. George Stigler's textbook, *The Theory of Price*, first published in 1946, represented the "Chicago view" of the economics department. The text focuses on partial equilibrium analysis. In the introductory chapter Stigler proclaims:

> "general equilibrium is a misnomer: no economic analysis has ever been general in the sense that it considered *all* relevant data…The most that can be said is that general equilibrium studies are *more* inclusive than partial equilibrium studies, never that they are complete." (Stigler, 1946, p.28 (italics in original)).

And by the fourth edition, which was published in 1987, all references to general equilibrium had been removed (Weintraub, 2002, pp. 281-282). Milton Freedman is also on record criticising the Walrasian program. In a review of Oscar Lange's Price Flexibility and Employment (Lange, 1945), Friedman states:

> "…the analysis seems unreal and artificial. Here is a brilliant display of formal logic, abstract thinking, complicated chains of deduction; yet the analysis seems more nearly a rationalization of policy conclusions previously reached than a basis for them. What is there about the type of theorizing employed that makes it sterile even in the hands of so competent a practitioner as Lange?" (Friedman, 1946, p.613)

Friedman argues that the categories Lange has selected for analysis could only have been selected for the sake of logical analysis; not for empirical applications or testing. "The theory provides formal models of imaginary worlds, not generalizations about the real world." (Friedman, 1946, p.618). Friedman's criticism progressed on a number of fronts. Firstly, he argued that Lange's analysis is overly simple:

> "The theorist who seeks to devise a generalisation from observed facts will also have to simplify and abstract from reality. But it is clear that he need not limit himself to anything like so simple a system as Lange uses." (Friedman, 1946, p.620)

Secondly, Friedman criticises Lange for using classifications that have no direct empirical counterpart.

> "Lange's classification is designed to classify theoretical possibilities; it has no direct counterpart in the real world." Friedman, 1946, p.622)

Friedman concludes his review by noting that he has not, and will not, read the final chapter of Lange's book dedicated to policy problems, on the grounds that this chapter:

> "represents the combination of unsupported empirical statements and theoretical conclusions that, as we have seen, neither deserve any particular confidence nor bear very directly on the real world." (Friedman, 1946, pp. 630-631)

The increasing mathematisation of economic theory has raised practical as well as theoretical issues, with one prominent member of the movement to mathematise economic theory stating that:

> "The spread of mathematized economic theory was helped by its esoteric character. Since its messages cannot be deciphered by economists who do not have the proper key, their evaluation is entrusted to those who have access to the code. But acceptance of their technical expertise also implies acceptance of their values. Our profession may take pride in its exceptional intellectual diversity…Yet that diversity is strained by the increasing impenetrability to the overwhelming majority of our Association of the work done by its most mathematical members." (Debreu, 1991)

It was this situation that led to a famous quote by Paul Samuelsson:

> "By 1935 economics entered a mathematical epoch. It became easier for a camel to pass through the eye of a needle than for a nonmathematical genius to enter into the pantheon of original theorists. A Kind of Gresham's law operated, as those of us who benefited from it know only too well." (Samuelson, 1976, p.25)

Paul Romer has noted, in more recent times, that this situation has resulted in a style of theorising that is really academic politics masquerading as science. Romer refers to this mathematical style, which uses a mixture of words and symbols leaving room for slippage between statements formed in natural and formal languages, as “mathiness”, and characterises modern mathematical economic theory thus:

> “Presenting a model is like doing a card trick. Everybody knows that there will be some sleight of hand. There is no intent to deceive because no one takes it seriously” (Romer, 2015, p.93).

The implications of this are profound. In another paper, Romer explains how, when faced with overwhelming evidence that a model contradicts known facts, the general response is “all models are false”. Romer refers to such models as “post-real models”, whose methodological evasions reveal a “noncommittal relationship with the truth” that goes “far beyond post-modern irony”. He argues that:

> “For more than three decades, macroeconomics has gone backwards. The treatment of identification now is no more credible than in the early 1970s but escapes challenge because it is so much more opaque. Macroeconomic theorists dismiss mere facts by feigning an obtuse ignorance about such simple assertions as “tight monetary policy can cause a recession”. Their models attribute fluctuations in aggregate variables to imaginary causal factors that are not influenced by the action that any person takes. A Parallel with string theory from physics hints at a general failure mode of science that is triggered when respect for highly regarded leaders evolves into a deference to authority that displaces objective fact from its position as the ultimate determinant of scientific truth.” (Romer, 2016, p.1)

Modern mathematical economics does not even appear to lay claim to being an explanatory science.

4.3.2 Econometrics

The second approach to the mathematisation of the economics discipline was econometrics. The Cowles Commission for Research in Economics – a private fund dedicated to economic research – supported the development of The Econometric Society, which was founded in 1930 (by Irving Fisher of Yale University, Ragnar Frisch of the University of Oslo, and Charles Roos of Cornell University), and funded its associated journal *Econometrica* in 1932. The Cowles Commission was established by economist and investment consultant Alfred Cowles, who sought assistance in researching the forecasting accuracy of professional stock market forecasters. Initially, the Commission was housed in Colorado Springs, with Charles Roos appointed the first research director in 1934. But in 1939, after the departure of Roos and the death of Cowles' father, the Cowles Commission moved to the University of Chicago, where Schultz had built a strong tradition of mathematical economics and econometrics before his untimely death.

It moved to Yale in 1955 after James Tobin, of that institute, declined to move to Chicago to take up directorship. In the original articles of incorporation in 1932 the purpose of the Cowles Commission was stated as:

> "The particular purpose and business for which said corporation is formed is to educate and benefit its members and mankind, and to advance the scientific study and development…of economic theory in its relation to mathematics and statistics." (Christ, 1952, p.11).

During the 1930s and 1940s a vast number of intellectuals emigrated from Europe to the United States. As mentioned in the previous sub-section, several passionate proponents of positivist philosophies brought their research programs from Austria - and other European countries - during this period and quickly transformed the dominant Institutionalist inspired landscape into what is now modern economics.

Econometrics was developed into a means for adjudicating between rival neo-classical theories. This approach was in opposition to that pursued by the Institutionalists. Instead, the Institutionalists sought to develop new theories based on statistical research. Their approach was criticised by Tjalling Koopmans as being "measurement without theory" (Koopmans, 1947). Shortly after this and other criticisms, the institutional approach died out.

The Cowles Commission program aimed to construct and estimate a simultaneous equations system to describe the operation of the economy (Christ, 1994, p. 31). They desired to learn from this system how economic policy could be used to improve the performance of the system. But despite the initially stated aims of the positivist econometricians, McCloskey noted in the 1980s that:

> "no proposition about economic behaviour has yet been overturned by econometrics." (McCloskey, 1985, p.182)

And Hahn, in the 1990s claimed:

> "I know of no economic theory which all reasonable people would agree to have been falsified." (Hahn, 1993)[2]

The first usage of the term econometrics was in a paper by Ragnar Frisch (Frisch, 1926). This paper has also been described as the first important example of axiomatisation in economic science (Boumans & Davis, 2016, p.25). Frisch describes econometrics as:

> "Intermediate between mathematics, statistics, and economics, we find a new discipline which for lack of a better name, may be called econometrics. Econometrics has as its aim to subject abstract laws of theoretical political economy or 'pure' economics to experimental and numerical verification, and thus to turn pure economics, as far as possible, into a science in the strict sense of the word" (Frisch, 1926)[3]

The constitution of the Econometric society was drafted by Frisch. In it, the aim of the society is stated as:

> "The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics. ... Its main object shall be to promote studies that aim at a unification between the theoretical-quantitative and the empirical-quantitative approach to

> economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences." (Roos, 1933, p.106).

Frisch claimed that economics had so far failed to become a true science because the profession had vacillated between periods of rationalism and empiricism, as successive waves of practitioners failed to make headway. He therefore advocated the unification of theory and statistics as the means to reach his vision; all theorists were also to become statisticians (Frisch, 1930). For Frisch, the primary goal of economic science is control. The science aims to develop predictive methods for the purpose of social engineering. Econometricians right through to modern times have tended to emphasise prediction as their primary goal (Keuzenkamp, 2000, p.241). Frisch rejected causality as a metaphysical concept, and instead, focused on developing simultaneous equation models. Leon Walras had explicitly introduced systems of simultaneous equations into economics in 1874. Frisch innovated in the area of interviews as a source for the acquisition of data. In his early years, interviews were primarily conducted as a means for estimating the flexibility of marginal utility of income. Later, in the 1950s and 1960s, he utilised this method for the construction of macroeconomic preference functions (Frisch, 1961).

In its early days, Econometric methods were far from universally accepted. John Maynard Keynes for example, referred to Tinbergen's work for the League of Nations, in which he tested various business cycle theories, as "statistical alchemy and black magic" (Keynes, 1940, p.156). Frisch himself was aware of the hazards of econometric work, and stated that:

> "It should be stated explicitly that such an increase in the number of men devoted to econometrics is desirable only on the condition of quality. ... There are so many chances of abusing it, of doing more harm than good with it, that it should only be put in the hands of really firstrate men. Others should be absolutely discouraged from taking up econometrics' (Frisch, 1946, p.4)

Jacob Marschak moved to Chicago in 1943 to take up the positions of Research Director for the Cowles Commission and Professor of Economics with Chicago University. The econometrics revolution was initiated by the research staff that he assembled (Christ, 1994, p. 31). Tjalling Koopmans – who joined the Cowles Commission in 1944 and headed it from 1948 until 1967 - produced a landmark paper, which was published in a Cowles Commission monograph in 1950 offering some solutions to the *Identification Problem*[3] by adapting statistical methods for econometric analysis. One prominent reviewer, while praising the technical skill of the author, expressed reservations, stating:

> "These misgivings do not stem from any discovery of error in the deductive logical processes carried out, but rather in a failure to accept the premises as being realistic and the large sample characteristics of the estimators as applying to small samples." (Orcutt, 1952)

During his time at the commission, Koopmans oversaw the transformation of Walrasian economics from its roots in the Lausanne School to its modern axiomatised form. He personally introduced linear programing and activity analysis for application in both theoretical and practical applications. In 1957 he published *Three Essays on the State of*

Economic Science, in which he provided a classical exposition of the methodology and theory of Neo-Walrasian general equilibrium theory. Besides contributing to these important milestones in mathematical economics, Koopmans also developed and disseminated the Cowles Commission approach to econometrics (Koopmans, 1937; 1947; 1950).

Modern-day econometrics has become much more narrowly focused. It is better described as the application of statistical methods to economic models. This is a result of the work of Trygve Haavelmo who studied under Frisch, and became Director of Research at the Cowles Commission in 1948, as well as Professor of Economics in the University of Chicago. Haavelmo also followed Frisch's penchant for social engineering. He described econometric models as theoretical experiments implementable by policy control:

> "What makes a piece of mathematical economics not only mathematics but also economics is, I believe, this: When we set up a system of theoretical relationships and use economic names for the otherwise purely theoretical variables involved, we have in mind some actual experiment, or some design of an experiment, which we could at least imagine arranging, in order to measure those quantities in real economic life that we think might obey the laws imposed on their theoretical namesakes." (Haavelmo, 1944, p. 5)

Haavelmo rejected the dominant view that equation parameters acted as carriers of statistical information, instead, pioneering a causal definition in direct contradiction to the conception promoted by Frisch (Pearl, 2015, p.153). His nonparametric analysis of *structural causal models* has been developed in modern times into a framework for deriving causal and counterfactual conclusions. This has been achieved by unifying:

> "structural equation modelling with the potential outcome paradigm of Neyman (1923) and Rubin (1974) and the possible-world semantics of Lewis (1973)." (Pearl, 2015, p.157).

This body of work represents significant progress in the identification and elucidation of causal relations. In fact, James Woodward draws on it in the development of his manipulationist approach to causal scientific explanation (Woodward, 2003, pp. 258-260, 321-322) (See: Chapter 1: Section 1.4.2.3). Without an overarching framework of mechanistic explanation, it is not clear how these methods can be unified with other branches of economics. However, with the Neo-Mechanistic program in mind, it becomes ascertainable how they can contribute to the development of theories, by assisting in the elaboration of the *activities* carried out by mechanism *entities*.

In an address marking the twenty-fifth anniversary of *Econometrica*, Haavelmo lamented that:

> "The concrete results of our efforts at quantitative measurements often seem to get worse the more refinement of tools and logical stringency we call into play" (Haavelmo, 1958 p. 354-355).

He had two suggestions for why this had been the case. Either:

> "the "laws" of economics are not very accurate…we have been living in a dream-world of large but somewhat superficial or spurious correlations" (Haavelmo, 1958, p. 355).

Or alternatively, prevailing economic theories were unsatisfactory and the econometricians had failed to make this fact accepted by theorists so that they could improve upon them. I suspect the latter to be true, but without an overarching shared vision, the division of labour between *theorists* and *experimentalists* is unlikely to achieve the type of theoretical advancement one would expect of a mature science. In his Nobel acceptance speech, more than thirty years later, Haavelmo continued to lament that:

> "the possibility of extracting information from observations of the world we live in depends on good economic theory. Econometrics has to be founded on theories that describe in a reasonably accurate way the fashion in which the observed world has operated in the past. [...] I think existing economic theories are not good enough for this purpose." (Haavelmo, 1989)

Whereas the early progenitors of econometrics were directly inspired by a variety of positivist thinkers, it has been acknowledged that Karl Popper is basically the only philosopher of science to be occasionally quoted in Econometrica in modern times (Keuzenkamp, 2000, p.248).

4.3.3 Conclusions

Mathematical Economics and Econometrics were initially part of a single research program, inspired by positivist philosophy of science. In modern times, the disciplines have diverged, with mathematical economics concerned solely with explicating and developing economic theory in mathematical terms, and econometrics concentrating on the estimation of systems of equations. The relevance and utility of mathematical tools depends upon the mode of application. Mathematical explication has a crucial role to play in representing various aspects of the mechanisms underlying economic phenomena. However, this is not how these powerful tools are used in practice. Tjalling Koopmans, while commenting on the difficulties he had experienced while working on interdisciplinary projects with scientists from other fields, quoted one engineer as complaining:

> "Economics is the Thermodynamics of the Social Sciences. Everything is deduced from a few simple postulates without the necessity for knowing detailed mechanisms." (Koopmans, 1979, p.12).

This is a criticism in which I thoroughly share, and demonstrates as much as anything else that modern economic analysis in both mathematical economic and econometric forms fails to be adequately *mechanistic*.

4.4 Milton Friedman & Paul Samuelson

Two theorists whose methodological writings arguably remain the most influential within the economics profession down to this day, whilst maintaining that they operated within the tenets of the positivist philosophy, broke with it in terms of the symmetry thesis[5], and completely rejected the idea of explanation as a goal for economic science. These theorists are Milton Friedman and Paul Samuelson. I'll now outline their respective methodological commitments in turn below.

4.4.1 Milton Friedman

Milton Friedman's paper *The Methodology of Positive Economics*, published in 1953, was the most cited work on economic methodology in the twentieth century (Hands, 2009, p.143). And, for several decades, almost everything written on economic methodology seemed to start with Freedman's essay (Hands, 2001, p.57). 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 (Reichenbach, 1938), 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)

Mark Blaug once described Friedman's classic 1953 essay as: "a sort of vulgar, Mickey Mouse Popperianism" (Blaug, 2000, p.215). Nevertheless, Wade Hands was able to remark in the early 1990s that: "many, perhaps most practicing economists think of their work exclusively in instrumentalist terms" (Hands, 1991, p.71). And Uskali Maki, in the same volume, noted that: "even though many economists have declared themselves advocates of Popperian methodology, they nevertheless simultaneously feel comfortable with instrumentalist beliefs and practices, with no appreciable interest in matters of truth" (Maki, 1991, p.86). Recall from above (see: Section: 4.3.1) that this is the same Friedman who in 1946 criticised Lange's Walrasian equilibrium analysis for providing "formal analysis of imaginary worlds".

4.4.2 Paul Samuelson

The second of the positivist theorists to have dramatically shaped modern economic methodology was Paul Samuelson. During a period that encompassed at least the 1950s and 1960s, Samuelson's thought dominated university-level economics education in the United States. His book *Economics* (Samuelson, 1948) became the key undergraduate text, while his book *Foundations of Economic Analysis* (Samuelson, 1947) proved the key post-graduate text. These two books defined, and continue to define, the teaching of modern economics in both form and content (Hands, 2001, p.60). 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* (Samuelson, 1938; 1948). His goal was to provide a basis for the empirical testing of theories, and to disencumber metaphysics from economic theory. Theoretical terms were to be replaced by terms which refer to the immediate objects of phenomenal reality, otherwise they were to be considered meaningless. Henry Schultz, referencing Percy Bridgman, was the first economist to explicitly advocate *operationalism,* in his book *The Theory and Measurement of Demand* (Schultz, 1938).

In the realm of *consumer behaviour,* Samuelson redefined *utility* as *revealed preference* in order to render economic theory neutral on the psychological mechanisms leading to individual choices (Rosenberg, 1992, p.61). Revealed preference theory is built upon a set of choice axioms and based on the idea that observation is free from subjective elements, so that it provides a solid basis for grounding theoretical structures that does not rely on introspection. But, although Samuelson's writings are peppered with references to the work of Percy Bridgman, it turns out that his version of *operationalism* has little in common with the philosophy developed under that name by Bridgman (Bridgman, 1927), and that attacked by Karl Popper (Popper, 1934) and Carl Hempel (Hempel, 1954). Samuelson's *operationalism* has been interpreted as a form of falsificationism, reminiscent of the views espoused by Hutchison (Blaug, 1980, p.89; Caldwell, 1982, p.190). Whatever the case, Samuelson claims that his operational definitions were inspired by the Vienna circle (Samuelson, 1998, p.1380).

During the 1950s, Samuelson was criticised by Donald Gordon for not sufficiently operationalising economics (Gordon, 1955). During the 1960s, a series of debates played across the pages of the *American Economic Review*. Participants included Ernst Nagel (Nagel, 1963), Fritz Machlup (Machlup, 1964) and Samuelson (Samuelson, 1963; 1964; 1965a). Machlup and Nagel launched an attack on Samuelson's *logical equivalency* thesis. Simply put, they argued that a theory cannot be logically equivalent to a set of observation statements because the former includes unrestricted universal statements while the latter is comprised only of a finite conjunction of observational statements. Samuelson, using operationalist language, had claimed that a theory could be defined as:

> "a set of axioms, postulates or hypotheses that stipulate something about observable reality." (Samuelson, 1963, p.233).

On this basis, he stated that a theory (**B**) has a set of consequences (**C**) which are logically implied by the theory and a set of assumptions (**A**) which logically implies the theory. He then goes on to argue that **A**, **B** and **C** are identical. That is, that they are logically equivalent, so that the degree of empirical realism had by any one of **A**, **B**, or **C** is also shared by the others. It has been noted however that:

> "Samuelson's operational redefinitions of non-observable concepts failed to conform to operationalist constraints. Furthermore, his derivations of operationally meaningful theorems from fundamental hypotheses was deductivist, generally in line with a deductive-nomological model of explanation." (Cohen, 1995, p.73).

Later, Samuelson embraced a form of descriptivism. With this move, he declared that the only valid form of scientific explanation is phenomenal description. It can be argued that he was pushed into this direction by the obvious contradictions in his *operationalism* that had been pointed out by philosophers and economic methodologists alike. He held steadfast to professed methodological views while at the same time failing spectacularly to practice what he taught (Machlup, 1964, p.735; Hausman, 1992, p.158; Cohen, 1995, p.73). Samuelson claims to have held steadfastly to operationalism throughout his career (Samuelson, 1972, p.256), despite a preponderance of evidence to the contrary. For example, Samuelson originally presented his *revealed preference theory* of consumer choice as a replacement for the standard utility maximisation framework. He attacked the standard theory on the basis that several of its concepts including *utility* and *preference* were fundamentally mentalistic and subjective. His goal was to purge economic theory of these unobservable intentional concepts and replace them with more scientifically acceptable ones (Samuelson, 1938, p.71). But, as Wade Hands describes:

> "By the 1948 and 1950 papers, his position seems to be that subjective intentional concepts are perfectly acceptable in economic science. WARP [Weak Axiom of Revealed Preference] no longer seems to be a replacement for the concept of preference but instead is just a convenient tool for empirically discovering this elusive, but evidently explanatory, intentional phenomenon." (Hands, 2001, p.68).

Interestingly, this process mirrors what happened at the same time within psychological science, wherein authors such as Stanley Stevens introduced operationalism in an attempt to eliminate mentalistic concepts (Stevens, 1939), but eventually the newly introduced operational concepts were used to defend the traditional concepts; operational concepts introduced as replacements for traditional concepts eventually became used to justify them (Green, 1992).

By the late 1970s, Terrence 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.

4.4.3 Conclusions

Following on the heels of Hutchison, Morgenstern and Machlup, Milton Friedman and Paul Samuelson introduced positivistic ideas into economic science that acted to cement the deductive-nomological model of scientific explanation into standard practice. Whereas the former group were personally informed of the breaking developments within general philosophy of science, the latter, although inspired by the same philosophical movement, relayed their own take on the relevant issues. The primary ideas they propagated were: prediction is the main purpose of economic science; and description is the only valid form of scientific explanation.

4.5 The Modern Landscape

It was shown in Chapter 1 (see: Section 1.3.2) that serious deficiencies with the deductive-nomological model of scientific explanation were identified and debated many decades ago by philosophers of science. The result was a rejection of the model, and in more recent times, a new consensus has formed around the neo-mechanistic model as an appropriate replacement. But this new consensus has yet to reach the various communities within the economics profession. In this sub-section I will show that this is the case, despite the birth of economic methodology as a legitimate sub-field of the profession. In this sub-section, I firstly document the birth of this sub-field during the 1980s and describe the primary focus of the associated body of research in recent times. Then, I provide a few remarks on the current state of economic science, showing that the deductive-nomological model and associated pieces of positivist philosophy remains the core methodological principle underpinning practice. I conclude this sub-section with a discussion of a current debate within the literature on economic methodology concerning the proposition that the positivistic grip on economic science has been loosened, and we have entered a new era.

4.5.1 A New Discipline

In the final decades of the twentieth century, two prominent economic methodologists, Mark Blaug and Bruce Caldwell, published notable works (Blaug, 1980; Caldwell, 1982). 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. Mark Blaug's landmark book on methodology has been described as "probably the best, and best-known, book on the subject since John Neville Keynes." (O'Brien, 2013, p.36). And he has been credited with creating the new sub-discipline of economic science known as *Economic Methodology* (Mireles-Flores, 2013, p.iii). 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 economic practice, which he ultimately found deficient, because as Roy Weintraub has commented:

> "For a historian wedded to the Popperian view, twentieth-century economic thought is a melange of prescientific musings about social problems wrapped in the language of science, without any real science in evidence" (Weintraub, 2002, p.262)

In 1976, Blaug had endorsed the Popper-inspired Lakatosian approach of progressive and degenerative research programmes as an appropriate framework for analysing economic science, in an article that generated a substantial literature (Blaug, 1976). Yet he met with widespread overt hostility concerning the application of Lakatosian ideas to economics by the early 1990s (O'Brien, 2013, p.37). Blaug, describing the attitude of respondents at a conference he had organised on the topic with Neil De Marchi states:

> "I was personally taken aback by what can only be described as a generally dismissive, if not hostile, reaction to Lakatos's MSRP." (Blaug & De Marchi, 1991, p.500)

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. Essentially, he argued that the conventional practices of mainstream economists could not justify casting out unpopular alternatives as "unscientific". Caldwell characterises the period marked by his and Blaug's books as the re-emerging of economic methodology (Caldwell, 2018, p.82).

The journal *Economics and Philosophy* was launched in 1985 to facilitate mutual enrichment between the disciplines of economics and philosophy. Its most prominent topics include the methodology and epistemology of economics. *The Journal of Economic Methodology* was launched in 1994 as a dedicated outlet for research on the methodology of economics. As pointed out by Bruce Caldwell, before these two publications, the only journal where someone could publish on methodology were the Journal of Economic Issues, edited by Warren Samuels of the Institutionalist School, and the South African Journal of Economics (Caldwell, 2018, pp. 83-84). Economic methodology, unlike most fields within economic science, does not have a standardised framework for analysis, instead, a wide range of approaches, styles, tools and goals are utilised. Speaking about the state of the literature in those early days of the new discipline, Caldwell states that his first book was motivated by:

> "the disarray I found in the literature on methodology in economics, where no one seemed interested in explaining why we did what we did, but everyone was sure that what nonmainstream groups were doing was not scientific." (Caldwell, 2018, p.85)

In 2015, Wade Hands published a paper discussing developments within the field of economic methodology. His purpose was to explore the three-way relationship between orthodox economics, heterodox economics, and economic methodology during the last few decades. He made the observation that during most of the second half of the twentieth century, the economic mainstream orthodoxy consisted of neoclassical microeconomics combined with some version of macroeconomics, and that those working outside the orthodoxy were self-identified members of a small number of heterodox schools of thought. Hands refers to works by Mark Blaug and Terrence Hutchison, two of the most prominent economists to be working prolifically within the field of economic methodology, showing how these authors consistently attacked both orthodox and heterodox economics movements for their failure to comply with the normative strictures of Popperian Falsificationism. Hands argues that this mode of analysis was the dominant type within the methodology of economics literature during the period 1975-2000. But he argues that more recent work:

> "has changed its general philosophical focus from universal rules borrowed from the shelf of scientific philosophy to local practical advice grounded in the interests and concerns of particular subfields; and it has changed its domain of inquiry from neoclassical and heterodox economics in general to the more pluralistic microeconomic approaches at the edge of the current research frontier." (Hands, 2015, p.76).

However, although the literature on the methodology of economics has expanded considerably, it has done so in such a way that:

> "prevented it from engaging in much constructive criticism, or in playing any significant role in the actual practice of economic theorising, or allowing orthodox theory to respond to the criticisms of heterodox economists (or vice versa) in any meaningful way." (Hands, 2015, p.70).

Despite the burgeoning literature on the methodology of economics, Hands laments that the economics profession, particularly in the United States, still has little to no interest in elevating the sub-discipline of economic methodology to a legitimate position within the discipline of economics (Hands, 2015, p.62).

In a recent paper, Luis Mireles-Flores has produced a review of the recent literature on the methodology of economics (Mireles-Flores, 2018). According to Mireles-Flores, three broad trends have defined the core of this research during the last two decades. She identifies these broad trends as:

(1) The philosophical analysis of economic modelling and economic explanation;
(2) The epistemology of causal inference, evidence diversity and evidence-based policy; &
(3) The investigation of the methodological underpinnings and public policy implications of behavioural economics.

The first major trend in the recent literature on economic methodology is concerned with an analysis of both modelling in general and how it relates to economics in particular (Mireles-Flores, 2018, pp. 95-100). This body of research is inspired by works within the philosophy of science such as: Giere, 2006; Suppes, 2002; van Fraassen, 2008; Weisberg, 2013 and Wimsatt, 2007. The motivating idea is that *agents* use *models* to *represent* some aspects of economic phenomena to some relevant extent, for particular epistemic *purposes*. The most prominent responses to this literature have been: Hausman, 1992; Maki, 1992, 1994, 2004, 2009a, 2011; Morgan, 2012, 2015. Two approaches dominate this literature: the *isolationist view* – models are means by which to isolate causal relations –

and the *credible constructions* view – models are hypothetical worlds from which credible inferences can be drawn (Boon & Knuuttila, 2009; Mireles-Flores, 2015). Mireles-Flores notes that the literature includes a proliferation of research concerning truthfulness and realisticness in economics, focusing on how false models can lead to reliable scientific knowledge. In discussing how these debates relate to similar issues in other sciences he notes:

> "The only special aspect about modelling in economics is, perhaps, that in contrast to all other sciences, theoretical economic results are often shamelessly presented and celebrated as if there was nothing more to the practice of economics apart from cultivating and elevating the craft of formal modelling." (Mireles-Flores, 2018, p.95)

Julian Reiss has maintained that although economic models do not satisfy the criteria set out by contemporary accounts of scientific explanation, some nonetheless manage to explain. He considers this paradox to be genuine and likely to stay (Reiss, 2012). He sets up his trilemma as:

(1) Economic models are false;
(2) Economic models are nevertheless explanatory;
(3) Only true accounts can explain.

Anna Alexandrova and Robert Northcott argue however, that there is no paradox because economic models[6] *do not explain* (Alexandrova & Northcott, 2013). They fail causal criteria for explanation due to the inclusion of unconstrained abstractions and idealisations that are patently false. Economists are simply misled by their intuitions that their models do manage to explain. On the Neo-Mechanistic account of scientific explanation the paradox is also resolved by the rejection of the premise that economic models explain.

An alternative response has been to claim that although economic models definitely do not provide *how-actually* explanations, they may in fact provide *how-possibly* explanations, which these authors claim do not need to be true of the explanandum (Grune-Yanoff, 2013; Hands, 2016; Rohwer & Rice, 2013). But under strict Neo-Mechanistic conditions, a patently false *how-possibly* explanation is no explanation at all. False models need to be discarded from the *how-possibly* set of explanations in the process of moving toward *how-actually* explanations. If, as Mireles-Flores suggests, this form of argument is designed to justify: “the existence, and high appreciation, of some theoretical models which are extremely formal and *obviously not meant to provide actual explanations of anything*” (Mireles-Flores, 2018, p.99; italics mine), then such arguments simply operate as apologetics.

The second trend in the recent literature on economic methodology identified by Mireles-Flores relates to causal inference and evidence in economics (Mireles-Flores, 2018, pp. 100-109). An epistemological focus on developing and improving methods for determining causal relations has been at the heart of this research lately, displacing ontologically oriented discussions (Heckman, 2000; Hoover, 2001; Morgan, 2013; Reiss, 2015). These notions and methods have predominately been debated within the fields of econometrics and economic policy analysis. The Cowles Commission approach to econometrics is a theoretically based one premised on the ideas of Trygve Haavelmo. It is a structural modelling approach that seeks to determine parameter values for theoretical constructs (see: Section 4.3.2 above). The alternative non-structural approach of vector autoregression (VAR) presupposes no theory and leaves the system unspecified since it merely reflects correlations. It was considered an advantage that this type of modelling is free from the serious difficulties that arise in attempting to test the a priori assumptions of the modelled theory. It is not, however, possible to make counterfactual inferences in such a framework (Reiss & Cartwright, 2004). In order to facilitate an analysis that seeks to distinguish between correlation and causation, some structural restrictions were introduced to VAR to create SVAR (structural vector autoregression) (Hoover, 2005).

The key issue that has engaged philosophers and methodologists has been in exploring the trade-off between the inclusion of preconceived theoretical assumptions and the validity of the causal inferences derived. In response to these debates, Kevin Hoover developed an interventionist account of causality, similar, but not identical to, those produced by Judea Pearl and James Woodward (Hoover, 2001; Pearl, 2000; Woodward, 2003). The benefit of Hoover's approach is that it makes theoretical assumptions explicit and assesses them relative to their purpose. Aris Spanos has also produced a body of research focused on methodological problems in econometrics. He argues that a large portion of econometric results produced and published are not justified due to invalid methodological practices (Spanos, 2008, 2010, 2012, 2015; Mayo & Spanos, 2010).

Within fields such as development economics and growth economics, *design-based econometric analysis* has received a lot of attention (see for example: Banerjee, 2007; Soderbom, et. al., 2015). This approach to policy analysis derives from the *potential outcomes framework* developed within the discipline of statistics (Morgan & Winship, 2007). The approach aims to limit theoretical presuppositions, to test causal relations using techniques that are similar to those utilised in controlled experimental designs. The goal, in contrast to theory-based approaches that use structural models to make counterfactual policy predictions, is to evaluate prior policy implementations via the causal interpretation of data. The approach searches for databases that can be interpreted and analysed as if the data had been randomly generated (Angrist & Pischke, 2015). The subset of this research most relevant to this thesis is that which is concerned with asking whether it is enough to know the strength of causal effects without knowing about the mechanisms responsible for the causal relationships (Marchionni, 2017; Reiss, 2007; Russo & Williamson, 2007, Ruzzene, 2014; Steel, 2013; Weber, 2007). The *Russo-Williamson thesis* holds that to establish a causal connection, one requires both probabilistic evidence and evidence for the existence of a mechanism connecting the cause with the effect. Claveau has endorsed a pluralistic stance with regard to valid sources of evidence for causal induction (Claveau, 2011), as have Kuorikoski and Marchionni (Kuorikoski & Marchionni, 2016). And Moneta and Russo have argued that, in the context of econometric models, for researchers to make causal interpretations of statistical models, they require both probabilistic and mechanistic types of evidence (Moneta & Russo, 2014).

A body of research under the banner of *Evidence-based Economics* has emerged in response to research produced on the basis of pure theory. Philosophical attention has mainly been focused on the difficulties in evaluating and combining evidence from diverse sources (Stegenga, 2011; Howick, 2011). In relation to economics, Julian Reiss has argued for a supplementation to the theory-based orthodoxy (Reiss, 2004), and others have promoted the design-based approach to empirical research (Banerjee & Duffo, 2011; Cohen & Easterly, 2009), under an evidence-based approach.

The third substantial research trend in the literature on the methodology of economics that has been identified and discussed by Mireles-Flores is *Behavioural Economics* (Mireles-Flores, 2018, pp. 109-115). The movement grew out of a rejection of the "unacceptably unrealistic" core assumptions of neoclassical rational choice theory and as a response to the cognitive revolution in psychology, where behaviourist approaches were abandoned in favour of ones focused on the development of computational models of mental representations and learning processes (Nagatsu, 2015a). Three stages of development in behavioural economics have been identified: acceptance of the growing body of empirical anomalies accumulating in standard rationality theory; development of new economic theory based on empirically grounded assumptions; and an ongoing phase focused on attempts to apply insights gleamed from the body of research to public policy (Angner & Lowenstein, 2012). A number of studies have involved discussions of the validity of the alternative methods employed within the research programs of behavioural economics, including laboratory and field experiments as well as the use of computer simulations (Angner & Lowenstein, 2012; Boumans, 2016; Guala & Mittone, 2005). Mireles-Flores divides the recent methodological literature focused on behavioural economics into four broad concerns; (1) the validity and epistemic merits of *neuroeconomics*; (2) whether behavioural economics is truly an alternative to neoclassical economics; (3) appraisal of the *normative implications* of the behavioural economics approach to welfare economics; and (4) the *libertarian paternalism* approach to public policy.

Roberto Fumagalli provides a useful discussion of the current methodological debates concerning neuroeconomics (Fumagalli, 2016). The consensus position on neuroeconomics within the literature on economic methodology appears to be that it is "more hype than substance" (Marchionni & Vromen, 2012). Specifically, it has been argued that: the program has not achieved as much as it has claimed for itself (Harrison, 2008; Maki, 2010; Ross, 2012); the apparently "surprising" results are not all that surprising at all (Vromen, 2010); and there are both empirical and conceptual reasons to reject decision theoretic analysis based on neuro-psychological notions of utility (Fumagalli, 2013). Christopher Clarke, on the other hand, has argued in favour of neuroeconomics on the basis that currently available cognitive and neurobiological data constitute economic evidence for answering economic questions, but only under specific proposals for what the *aims* of economic science are (Clarke, 2014). Carsten Herrmann-Pillath has noted that in the current methodological debates about neuroeconomics, most contributions concentrate on the relationship between economics and neuroscience using methodological stances from the methodology of economics based on axiomatic deduction. Herrmann-Pillath argues that *mechanistic analysis*, which is so prevalent in the neurosciences, should form the basis for integration of the two fields (Herrmann-Pillath, 2016). Obviously, I wholeheartedly agree with this proposal.

A second substantial strain in the recent literature on behavioural economics is intended to address the question: is behavioural economics different from neoclassical economics? Ariel Rubenstein has questioned the innovativeness of behavioural economics, criticising the approach for simply modifying and developing more sophisticated mathematical formalisms in line with the neoclassical methodological style (Rubinstein, 2003). Others have also argued that behavioural economics is just neoclassical economics in disguise (Berg & Gigerenzer, 2010), while it has also been argued that behavioural economics does not engage with proper economic phenomena, and so could potentially end up being just a branch of psychology (Ross, 2014a; 2014b).

The third major issue concerning behavioural economics in the literature on economic methodology concerns *Behavioural Welfare Economics*. Since behavioural economics questions the traditional rationality axioms, it casts serious doubt about the validity of using the fundamental theorems of welfare economics for normative analysis (McQuillin & Sugden, 2012). Most methodological discussion in this area has focused on either: the ontology of preferences and other mental states (Angner, 2018; Guala, 2017); or the normative justifications of what “welfare” is assumed to involve (Kahneman & Sugden, 2005; Sugden, 2007, 2008; Hausman & Welch, 2010; Mills, 2015; Nagatsu, 2015b).

Public policy debates in relation to libertarian paternalism represent a fourth major strand in the recent methodological literature relating to behavioural economics. The core idea behind libertarian paternalism is that policy interventions that can subtly influence individual decisions should be implemented where they are expected to make the individuals better off, according to their own preferences. The goal is not to directly intervene on individual choices, but to intervene on the environmental frame, called the “choice architecture” (Osman, 2016). Two books that have been greeted with widespread popular enthusiasm, have helped generate significant policy discourse in both academic and non-academic settings in relation to these ideas (Thaler & Sunstein, 2008; Akerlof & Shiller, 2009). A substantial amount of research has been centred on questions such as how policy makers can know, and account for, what is actually best for individuals (Anderson, 2010; Bovens, 2009; Guala & Mittone, 2015). Conrad Heilmann, in proposing a set of necessary and sufficient conceptual conditions, as well as further practical conditions, for the useful implementation of *nudges*, clarifies issues around a number of debates relating to whether libertarian paternalism is respectful to individual liberty (Heilmann, 2014).

Despite the burgeoning literature on Economic Methodology, and economic modelling in particular, it has been recently noted that there is a paucity of research on the context of modelling within economic science and the relation of a model to other models and *explanations* (Aydinonat, 2018, p.213).

4.5.2 The Modern Landscape

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).

Colander et. al., more recently, put it this way:

> "In our view, the interesting story in economics over the past decades is the increasing variance of acceptable views, even though the centre of economics has not changed much." (Colander, Holt & Rosser, 2004, p.487)

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 Deductive-Nomological model of scientific explanation; practitioners are engaged in engineering pursuits without the guidance of an overarching scientific framework. Paul Feyerabend would be delighted. Citi Chief Economist Willem Buiter, speaking in the aftermath of the global financial crisis noted that:

> "The Bank of England in 2007 faced the onset of the credit crunch with too much Robert Lucas, Michael Woodford and Robert Merton in its intellectual cupboard. A drastic but chaotic re-education took place and is continuing. I believe that the Bank has by now shed the conventional wisdom of the typical macroeconomics training of the past few decades. In its place is an intellectual potpourri of factoids, partial theories, empirical regularities without firm theoretical foundations, hunches, intuitions and half-developed insights. It is not much, but knowing that you know nothing is the beginning of wisdom." (Buiter, 2009).

And he doles out the blame for this crisis of methodology in the following manner:

> "Most mainstream macroeconomic theoretical innovations since the 1970s (the New Classical rational expectations revolution associated with such names as Robert E. Lucas Jr., Edward Prescott, Thomas Sargent, Robert Barro etc, and the New Keynesian theorizing of Michael Woodford and

> many others) have turned out to be self-referential, inward-looking distractions at best. Research tended to be motivated by the internal logic, intellectual sunk capital and aesthetic puzzles of established research programmes rather than by a powerful desire to understand how the economy works" (Buiter, 2009)

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 (Johnson, 1996, p.289). 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 classic book stated:

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

And this shared belief has translated through to current times. As Tony Lawson has argued, if positivism has been "killed off" from academic faculties of philosophy, it lives on in modern economics in the form of deductive-nomological explanation (Lawson, 2018, p.22)[7].

Richard Lipsey noted in 2001 that for more than a generation of US economists, Milton Freedman' s essay *The Methodology of Positive Economics* published in 1953 has been the only work on methodology they have read (Lipsey, 2001, p.174). This is in stark contrast to what Robert Solow once proclaimed in a review of Koopmans's book *Three Essays on the State of Economic Science*, wherein he states that it:

> "reverts to pure methodology, a common subject for the off-duty reflections of economists" (Solow, 1958, p.179)

Peter Boettke has recently noted that:

> "Whereas earlier generations of economists from John Stuart Mill to Paul Samuelson sought to justify their scientific status by reference to philosophers, the post-1990 economist just does what others do" (Boettke et al, 2018, p.59).

Deborah Redman, speaking of the relationship between the disciple of the philosophy of science and that of the science of economics, has pointed out that:

> "The modern relationship between the two disciplines has grown so confused, confusing, and involved" (Redman, 1991, p.95).

And so how has this situation translated into methodological work? Daniel Hausman once noted quite critically that:

> "Most methodological writing on economics is by economists. Although the bulk is produced by lesser members of the profession, almost all leading economists have at one time or other tried their hand at methodological reflection. The results are usually poor. If one read only their methodology, one would have a hard time understanding how Milton Friedman or Paul Samuelson could possibly win Nobel Prizes. It thus is less surprising that the economics profession professes such scorn for philosophizing than that its members spend so much of their time doing it." (Hausman, 1984, p.231)

And Bruce Caldwell has recently remarked that:

> "The philosophy of science that had been dominant in the twentieth century had been some form of positivism. It had been developed with the physical sciences in mind, but was in eclipse. When they wrote about methodology, those economists who made any reference to the philosophy of science usually invoked some variant of positivism, but they often were not careful in their borrowing. In short, economists writing about how to practice economics properly tended to borrow, badly, from a defunct philosophical position." (Caldwell, 2018, p.83).

Despite the apparent 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 (Farmer, 2012, p.7). 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. Haavelmo's lament noted above in Section 4.3.2, that the theoretical and empirical components within the economics profession are not adequately connected so as to effect feedback between them, still holds. And if the following observation by Paul Romer, concerning an overwhelming reluctance to criticise leading figures within the profession is correct, one cannot help but be pessimistic about near-term rectification of this debilitating situation. Reflecting on the attitudes within the profession, Romer remarks of economists that they:

> "seem to have assimilated a norm that the post-real macroeconomists actively promote – that it is an extremely serious violation of some honor code for anyone to criticize openly a revered authority figure – and that neither facts that are false, nor predictions that are wrong, nor models that make no sense matter enough to worry about." (Romer, 2016, p.21)

4.5.3 A New Paradigm?

Julian Reiss claims that positivistic trends in economics have now been abandoned and that with this, explanation has once again become a priority for working economists (Reiss, 2008, p.180). 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.

A significant literature has grown out of the conviction that the neoclassical stranglehold is being progressively loosened (Coyle, 2007; Davis, 2006, 2007, 2008; Colander, 2000a, 2000b, 2003, 2004, 2005a, 2005b, 2008, 2009; Santos, 2011). These authors suggest that the once dominant neoclassical framework has been replaced by a new, pluralistic mainstream, which is more open to psychology and allows for a much broader class of modelling strategies. The most important piece of evidence marshalled in support of this proposition is a change in the type of research published in the leading economics journals: the *American Economics Review*, *Quarterly Journal of Economics*, *Economic Journal* and the *Journal of Political Economy* (Hands, 2015, p.70). A second piece of evidence presented is the fact that historically, the specialty areas of research and teaching – labour economics, environmental economics, public finance, managerial economics, international economics, etc. – have simply been applications of the standard neoclassical utility and profit maximising framework, whereas more recently they have developed their own tools and conceptual frameworks. The progenitors of the "neoclassical economics is dead" thesis recognise that teaching of economics is still utterly dominated by the neoclassical framework. It has been argued that the new pluralist economics is not only *not* neoclassical, it is also *not* heterodox either. Wade Hands claims that although many of the issues identified in the recent literature had previously been raised by economists working within heterodox traditions, the contemporary authors working on these issues do not self-identify as members of heterodox movements and do not cite the works of authors from traditional heterodox literatures.

However, one author has noted that the identification of mainstream economics as pluralist is highly contestable:

> "As Dequech (2007) points out, while there is openness to new ideas within the mainstream (and there have been some methodological changes with respect to admissible types of data for example), the mainstream has not shown itself to be open to more fundamental methodological challenge posed by heterodox economics. If mainstream economics defines economics in terms of a particular (logical positivist) set of methodological principles, then challenges from a different methodological perspective are simply not recognised and communication is ruled out. Meador (2009) goes so far as to argue that orthodox and heterodox economics reflect different epistemes, in the Foucauldian sense, which suggests that communication is impossible, with important implications for strategy…I would argue that it is the refusal by most mainstream economists to address methodological issues that has been a very significant stumbling block in such attempts at communication." (Dow, 2011, p.1159/1161)

Sheila Dow argues that dialogue is difficult to establish because while heterodox economists consider mainstream economics as a particular school of economic thought, the mainstream views heterodoxy as something other than economics; as some form of sociology, history, politics, or philosophy (Dow, 2011, p.1162). And so Dow's conclusion as to the state of the landscape of modern economic methodology is that:

> "There have been some changes to methodology, for example with respect to theory testing, so that now questionnaire evidence is admissible, for example, and indeed there has been an increasing emphasis on gathering evidence. But the core deductivist principles remain as the exclusive methodological approach, such that, while behavioural economics, for example, has introduced experimental evidence and new ideas about behaviour that challenge the core rationality axioms, the agenda is to improve the deductivist system rather than to replace it." (Dow, 2011, p.1163)

Elsewhere, Dow notes that even though the empirical results flowing from *experimental economics* and *new behavioural economics* seem to falsify key elements of pure theory in mainstream economics, any attempt to incorporate these results, particularly with input from psychology, "has run up against the strictures of mathematical formalism", due to a penchant for a type of theory that eludes definitive direct testing (Dow, 2013, p.27). Herbert Simon once stated that: "My economist friends have long since given up on me, consigning me to psychology or some other distant wasteland" (Simon, 1991, p.385). Esther-Mirjam Sent explores the differences between the programs of the "old behavioural economics" – including Simon's - and "new behavioural economics", explaining that the latter has not suffered the same fate of dismissal as the former since it has "situated itself squarely within the mainstream" and "suggested ways in which their insights may help build the mainstream stronghold" (Sent, 2004, p.742/754); whereas Herbert Simon, and fellow travellers, had sought to develop a psychological basis for economics alternative to the mainstream, based on concepts such as *satisficing* and *bounded rationality*, the "new" program uses the standard rationality assumptions as a benchmark for assessing deviations from this, based on insights from the work of Kahneman and Tversky, and other researchers. Sheila Dow concludes from this that:

> "The appraisal of new behavioural economics is thus conditioned by acceptance of the formalist mainstream methodological framework…While the development of partial theories (feedback theories, prospect theory, and so forth) could be said to be empirically progressive, this is incompatible with trying to fit such theories into a general equilibrium framework deduced from the rationality axioms. As long as new behavioural economics accepts the mainstream framework, therefore, it is likely to become degenerative (Dow, 2013, p.39).

And Tony Lawson has recently claimed that the positivist explanatory program of deductivism in economic science is:

> "now more influential than ever." (Lawson, 2018, p.21)

There are two competing visions running threaded throughout the "neo-classical economics is dead" literature. One view states that the future of the economics discipline will become increasingly pluralist, while the other interprets increasing pluralism as a transitionary stage to a new mainstream paradigm. Those authors supporting the latter vision are split between those who believe a revolution is required, and those who do not. I will have more to say about this debate in the following chapter. But it is worth noting where the position advocated for in this thesis sits in the revolution vs pluralism debate. My thesis agues for methodological monism and advocates a mechanistic explanatory methodology that economic science ought to be committed to. But this standard for theoretical construction is quite pluralist; the resources available for assembling mechanistic explanations are broad. Craver and Darden clearly state that:

> "In our view the search for mechanisms is inherently a pluralistic endeavour". (Craver & Darden, 2013, p.198)

So perhaps the debate is dissolved if one takes a Neo-Mechanistic stance. But is it possible to reach such a pluralism in economic methodology by peaceful means? I address this question further in the following chapter.

Despite the ever-spawning literature suggesting that economic science has entered a new era, there is much cause for pause. The editors of *The Routledge Handbook of Heterodox Economics* have recently claimed that:

> "The pushback against heterodox economics that started in the 1980s has not slowed since the 2007–8 'great crisis' or the ensuing stagnation/depression. Rather, across the world and perhaps most noticeably in Europe, several consecutive years of austerity policies have placed significant pressures on publicly funded universities, and heterodox economists appear to suffer disproportionately from the shrinking of funding, resources, and academic positions" (Tae-Hee, J., Chester, L., & D'Ippoliti, 2017, p.13).

And Peter Bottke, reflecting upon Bruce Caldwell's *Beyond Positivism*, 35 years after its initial publication, has noted the fact that:

> "Unfortunately, Caldwell's humble and reasonable suggestion that our profession cultivate a constructive conversation between diverse perspectives, and that we train new generations of economists in the art of constructive criticism and appraisal, was widely ignored. Confidence in conventional practice has actually grown bolder since that time. The self-contradictory methodological stance of the modal economist - the insistence on both the futility of methodological inquiry and the adequacy of mainstream method - remains firmly entrenched. In fact, with isolated

> exceptions, in the 35 years or so since that fleeting moment of methodological self-reflection that Beyond Positivism epitomized, practitioners of elite economics have continued to debate methods of modeling and testing, while cautiously avoiding methodological inquiry into the significance of modelling and testing for economic science." (Boettke, 2018, p.73)

The primary aim of the Handbook of Heterodox Economics, quoted from above, is:

> "The Handbook aims, first, to provide realistic and coherent theoretical frameworks – as an alternative to that provided by the mainstream (orthodox) perspective that dominates the teaching of economics and has informed many contemporary policies – to understand the capitalist economy in a constructive and forward-looking manner." (Tae-Hee, J., Chester, L., & D'Ippoliti, 2017, p.3).

And the authors state that one of their major conclusions is that the volume:

> "…demonstrates the engagement of many heterodox economists with methodological pluralism compared to the monist methodology of mainstream economics" " (Tae-Hee, J., Chester, L., & D'Ippoliti, 2017, p.3).

Whether founded upon Paul Samuelson's operationalism or Milton Friedman's instrumentalism, formalism and empiricism have served to dismiss alternatives not committed to positivist methodology. In response, a lot of the work carried out by the members of heterodox economics societies – the International Confederation of Associations for Pluralism in Economics (ICAPE), the Association for Heterodox Economics

(AHE), the Society of Heterodox Economists (SHE), the Heterodox Economics Newsletter, and others – have often explicitly promoted the development of a synthesis of heterodox approaches as a means to rival the mainstream. It is one of the primary objectives of this book to show that there is an appropriate monistic *methodology* for economic science that also accommodates a plurality of *methods*: the Neo-Mechanistic model of scientific explanation.

4.6 Conclusions

In his 2006 presidential address to the History of Economic Thought Society, Wade Hands sought to answer the question: Why did mainstream economics, circa 1945-1965, look so much like mainstream philosophy of science during the same period? (Hands, 2007). His claim was that such a "stabilisation" did not occur between philosophy of science and other sciences, including physics, biology and psychology and so this particular coupling requires explanation. Hands provides seven points to highlight the "very curious" similarity between the two fields. He then goes on to provide three partial answers to the question he is addressing. Firstly, as has been outlined in numerous places within this chapter, the roots of both the "received view" and the "neo-classical synthesis" reach firmly back to the 1930s Vienna Circle and associated groups, which represents one of the most important time-place combinations in the history of western intellectual life. This intellectual environment was not constrained by the sharp disciplinary boundaries that have since come to dominate in modern times. Hands' second partial answer is that the Harvard community during the period of 1937 to 1941 provides a common origins story, with the

Science of Science Discussion Group, which included Rudolf Carnap, Herbert Feigl and Joseph Schumpeter, being prominent at the same time that Paul Samuelson was putting the finishing touches on his *Foundations*. But since almost everyone involved here had positivist philosophical convictions, and a number of these had been members of the Vienna Circle, this would seem to merely be an extension of the prior partial answer. The third partial answer provided by Hands is borrowed from Philip Mirowski (Mirowski, 2004; 2005). Mirowski's argument is that during, and in the years following, World War II, when scientists, economists, political scientists, and philosophers were working together in Operations Research at places like RAND in the United States, particular forces led to the development of a shared intellectual vision that shaped both economic science and philosophy of science.

The main argument running through both this and the previous chapter is that until modern times economists have *always* looked to the philosophy of science to find inspiration for their methodological commitments. A conclusion of this argument is that a close affinity between mainstream philosophy of science and mainstream economic methodology is *to be expected*. The purpose of this book is to argue that an uncoupling of the two disciplines has occurred as modern economic methodology has failed to adequately respond to developments within contemporary general philosophy of science.

I argued my case in this chapter by first showing, in Section 4.2, how the programs of the positivist philosophers were introduced into economic science by Terence Hutchison, Oskar Morgenstern, and Fritz Machlup. I then showed, in Section 4.3, how these methodological convictions resulted in the wholesale mathematisation of economic science through the programs of mathematical economics and econometrics. Next, I explained in Section 4.4

how prominent works by Paul Samuelson and Milton Friedman ensured that the Deductive-Nomological model of scientific explanation remained the key methodological principle for the profession throughout the twentieth century, and into current times. In Section 4.5, I documented the emergence of the specialist economic sub-field of *economic methodology*, and showed how, despite this increase activity in scrutinising the philosophical principles underlying modern economic methodology, new dominant ideas within the philosophy of science have not made any headway into the serious considerations of working economists.

Part 3: Complexity Economics

Chapter 5 - Central Themes of Complexity Economics

The purpose of this chapter is to introduce the *Complexity Economics* school of thought and its underpinning philosophical motivations, and to show that the methodological convictions of this school of thought are inconsistent with those of the orthodox paradigm.

5.1 Introduction

I suggested in Chapter 4, that orthodox economic practice fails spectacularly to meet the mechanistic standards outlined in Chapter 2. 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 chapter is structured as follows. After a preliminary discussion on the terms *mainstream*, *orthodox* and *neoclassical economics* in Section 5.2, I introduce the complexity economics movement, by providing some brief comments on its history and motivations in Section 5.3. In Section 5.4, 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 5.5, I outline the philosophical commitments of the complexity economics school of thought, by using the heuristic device introduced in Chapter 3 (see: Section 3.2). In Section 5.6, I show how the complexity economics methodological approach is capable of addressing some of the key concerns of the heterodox schools of economic thought that were discussed in Chapter 3. I conclude in section 5.7 with a brief summary of the argument presented in this chapter.

5.2 Preliminaries

Some readers may have felt some unease at the manner in which I have deployed the terms *mainstream*, *orthodox*, and *heterodox* in the last two sections of the preceding chapter. For good reason. In using such wide labels one runs the risk of both accentuating aspects that support one's thesis and obscuring aspects that run against it. Nonetheless, I justify the practice in this instance in two ways. Firstly, I appeal to the fact that the terms are in current usage throughout the various literatures relevant to the arguments substantiated in this book. Secondly, the broad level at which these arguments are pitched facilitates the deployment of broadly defined terms. Specifically then, in applying the terms *mainstream* and *orthodox* to bodies of economic thought throughout this book, I am using them interchangeably to refer to *neoclassical economics*.

Some authors, however, distinguish between these terms. David Colander for example, argues that while *mainstream economics* is a largely sociologically defined term, referring to the body of ideas jointly held by individuals in the leading academic institutions, organisations and journals at any given point in time, *orthodox economics* is an intellectual category defined by what historians of economic thought have identified as the most recently dominant economic school of thought. Colander recognises that this school of thought is *neoclassical economics*, which he defines as:

> "an analysis that focuses on the optimizing behaviour of fully rational and well-informed individuals in a static context and the equilibria that result from that optimization…When a dynamic context is assumed, individuals understand the probability distributions of possible outcomes over infinite time horizons at the moment of decision…Perhaps the most important characteristic of the neoclassical orthodoxy is that axiomatic deduction is the preferred methodological approach." (Colander, Holt & Rosser, 2004, p.490).

Colander claims that, for reasons to be discussed in the following chapter (see: Section 6.3.1), and touched upon briefly in the previous chapter (see Section 4.5.3 above), the neoclassical orthodoxy has lost its stranglehold on the economics profession, so that the terms *mainstream economics* and *orthodox economics* do not currently coincide. I'm happy to concede to Colander's definitions of the terms, however, since I disagree on his assessment of the current orthodoxy, the terms become interchangeable for me. The reader will readily discern that a number of the authors I quote throughout this book also use the terms in like fashion.

5.3 The Complexity Economics Movement

In this sub-section, I introduce the Complexity Economics movement by examining its origins and motivations.

5.3.1 Origins

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. Properties such as self-organisation and adaptation are also central concerns. Although the answer to the central foundational question of: *what is complexity?* continues to resist agreement (Gell-Mann, 1995; Mitchell, 2009; Ladyman, Lambert & Weisner, 2012; Holland, 2014), the field has been described as evolving out of five distinct intellectual traditions: dynamical systems theory; systems science; complex systems theory; cybernetics; and artificial intelligence and cognitive sciences (Castellani, 2013).

5.3.2 Other Scientific Disciplines

During the second half of the twentieth century, scientists working in the fields of physics, chemistry and biology became increasingly interested in far-from-equilibrium systems that were dynamic and complex. These were systems that never settled into a state of rest.

During the early 1970s, research was initially focused on complex systems in which the elements were simple. However, by later in this decade interest was directed toward complex systems whose elements were more complex. Such systems came to be known as *Complex Adaptive Systems* (Beinhocker, 2007, p.18). It is this latter type of representational system, I argue, that can meet the requirements for mechanistic explanation, since the entities involved are capable of causal production.

The *Santa Fe Institute for Science* was established in 1984 as a research centre for cross-disciplinary science with the mission of bridging the ever-widening rift between scientists and humanists and tackling the big problems in science that cut across many fields. It was the brainchild of George Cowan - a research head at the Los Alamos National Laboratory. The founding members included: George Cowan – President; David Pines – Vice President; and Murray Gell-Mann – Chairman. John German explains that:

> "The new, privately funded institute was to bring the tools of physics, computation, and biology to bear on the social sciences, reject departmental and disciplinary stovepipes, attract top intellects from many fields, and seek insights that were useful for both science and society." (German, 2014).

Within the biological sciences, complex systems analysis has generated revolutions in the disciplines of ecology, population biology, and evolutionary studies and is slowly making inroads into those of biochemistry, development, genetics, and whole-plant biology. More recently, molecular biology has also adopted such an approach (Trewavas, 2006, p.2420).

Although momentum is building for a complexity science approach in chemistry, and despite the early interest in complex systems in the 1970s, the movement is still in its infancy in this discipline (Ludlow & Otto, 2008). Research programs based on complexity science have been devised for the legal profession (Ruhl et. al., 2017). The medical sciences are also said to be at a major transition point to a complexity science approach (Berlin et. al., 2017).

A key feature of the complexity science movement is the rejection of the traditional reductive method in scientific practice. In 2000, Robert Laughlin and David Pines lamented that:

> "The fact that the essential role played by higher organizing principles in determining emergent behavior continues to be disavowed by so many physical scientists is a poignant comment on the nature of modern science. To solid-state physicists and chemists, who are schooled in quantum mechanics and deal with it every day in the context of unpredictable electronic phenomena such as organogels (47), Kondo insulators (48), or cuprate superconductivity, the existence of these principles is so obvious that it is a cliché not discussed in polite company… For the biologist, evolution and emergence are part of daily life." (Laughlin & Pines, 2000)

I urge the economics profession to also transition away from the traditional reductionist approach.

5.3.3 Complexity Economics

Doyne Farmer has remarked that, given the early metaphor of Adam Smith's invisible hand in economics (Smith, 1776), it is strange that this is the scientific discipline in which the complex systems revolution has had the least impact (Farmer, 2012, p.2). And it has been recognised that this failure to address economic phenomena in complex systems terms has resulted in a lack of understanding of the mechanisms that give rise to distributed control within markets (Holland, 2012, p. 8).

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 (Fontana, 2009). 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. The workshop was funded by John Reed – the then-soon-to-be CEO of Citicorp – who lamented the lack of economic theory relevant to the management of a global financial organisation. Ten natural scientists 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 ongoing productive interaction. Fontana 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 (Anderson, Arrow & Pines, 1988). 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[1].

Subsequent to the workshop, an Economics Program was established at SFI in 1988. Brian Arthur – the only heterodox economist to have been invited to the workshop - was appointed as director[2]. 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 (Arthur, Durlauf & Lane, 1997). 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 (Fontana, 2008), Fontana argues, that what has come to be known as complexity economics, constitutes a new paradigm in the full Kuhnian sense. John Davis also forcefully asserts this position (Davis, 2017) as does Wolfram Elsner (Elsner, 2017, p.943), amongst others.

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 descriptivist 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 (Arthur, 1999), 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 (Epstein, 1999; 2006), interactive-agent economics (Miller & Page, 2007), agent-based computational economics (Tesfatsion, 2002; 2006) and complex economics (Kirman, 2011).

5.4 Objections to Mainstream Methodology

Eric Beinhocker argues that there are five "Big Ideas" that distinguish Complexity Economics from orthodox economics, which he calls *Traditional Economics* (Beinhocker, 2007, p.96). These are: dynamics; agents; networks; emergence; and evolution. Simply put, the contrasts, as provided by Beinhocker are as follows:

Dynamics: Orthodox economics studies closed systems that are linear and in equilibrium. Where dynamic models are used, in the sense that they represent variables moving though time, they incorporate static relationships between variables. In contrast, Complexity economics studies systems that have significant interactions with their environments, that is, open systems. These systems are far from equilibrium, are nonlinear, and incorporate dynamic relationships between model variables.

Agents: In orthodox economics agents are modelled as either a single collective individual or as a group of identical individuals. Where heterogeneity is introduced, it is done so in a manner that renders divergences from the standard, perfectly rational individual, as simply noise that cancels out. The agents are modelled as knowing all information, both past and present, and can use this information to solve complex deductive optimisation problems. In solving these optimisation problems, it is assumed that they do so perfectly and without bias. These agents have no need for learning or adaptation since they are *perfect*. In contrast to the orthodox approach to modelling agents, complexity economics models them individually and as being strongly heterogeneous. Decision-making is inductive, based on rule-of-thumb heuristics. Agents have incomplete information and are subject to numerous, significant biases. These agents are modelled as capable of learning from the results of their past behaviours and as adapting to changing circumstances.

Networks: Under the orthodox approach to economics, there are two methods for modelling agent interaction. The first approach is general equilibrium modelling, in which there is no direct interaction between agents. Instead, activities are indirectly mediated through an abstract auctioneer device. This god-like apparatus, which has been dubbed the *Walrasian Demon* (Leijonhufud, 1967), incorporates all available information to co-ordinate the exogenously determined preferences of agents, by determining sets of price-quantity pairs for all markets simultaneously. The second approach is game-theoretic. Under this approach, all agents are connected to all other agents. In contrast to the orthodox approaches to modelling agent interactions, complexity economics explicitly models the network structure of interactions. These network relationships dynamically change over time and are endogenously determined.

Emergence: In orthodox economics, microeconomics and macroeconomics are distinct disciplines. Macroeconomics uses microeconomics in a simple aggregative manner. The analysis of the aggregate is reduced to the analysis of a single representative agent, ignoring by construction, heterogeneity and interaction. In short, these models assume that the economy in aggregate behaves the same as an individual does. These methods are used even though Kenneth Arrow has proven – more than sixty years ago - that it is impossible for a group of individuals to collectively make a decision that displays the same rationality that an individual can (Arrow, 1951). Besides playing a fundamental role in theoretical work, econometric analysis also depends on the assumptions of representative agent and linearity (Forni & Lippi, 1997). In contrast, complexity economics focuses on how macro-level patterns emerge from micro-level behaviours and interactions.

Evolution: Novelty, and growth in order and complexity have no endogenous explanations within orthodox economic models. In contrast, complexity economics explicitly models the evolutionary processes of differentiation, selection and amplification as explanatory factors of the production of novelty, and growth in order and complexity of economic systems.

While the attacks of complexity economists on mainstream methodology are numerous and diverse, 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 (Walras, 1874), remains the central working concept in theory construction[3]. 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.

It is obvious that a theory which posits equilibrium is incapable of explaining the process of economic growth that is a central feature of economic systems. Joseph Schumpeter pointed this out almost a century ago (Schumpeter, 1934, p.xix). Modern growth theory (*endogenous growth theory*) is based on the work of Paul Romer (Romer, 1990). Romer rejected the standard growth models of the time, which were based on the work of Robert Solow (Solow, 1956), and in which the exogenous forces of population growth and technology were the driving factors. In his model, Romer endogenised growth by incorporating investment in technology as a positive feedback factor. But the problem remains that the equilibrium framework in which it is captured is ontologically false, and the factor is uncoupled from the rest of the model. Eric Beinhocker describes the move as simply moving the black box from the outside to the inside; it's still a black box (Beinhocker, 2007, p.464).

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. John Holland, rejecting the equilibrium concept notes that:

> "if the system ever does reach equilibrium, it isn't just stable. It's dead." (Waldrop, 1992, p.147)

Several authors have pointed out that economists' dedication to the equilibrium concept represents a blind adherence to an old physics and an attendant failure to recognise progress in that science (Beinhocker, 2007, pp. 64-75; Gallegati & Kirman, 2012, pp. 5-6)[4]. The equilibrium framework developed by Walras and Jevons is based on the first law of thermodynamics – that energy is conserved. Subsequent developments ignored the discovery of the second law of thermodynamics – that entropy in a closed system increases. The implication is that an economic system will develop to a completely disordered state at equilibrium. This is the crux of the Holland quote above. Beinhocker shows that a detailed understanding of open systems was not forthcoming until the 1960s and 1970s. For a system to be increasing in complexity, as our global economic system has been doing ever since the emergence of homo sapiens, at an exponential rate, it must be an open system. And such systems are characteristically far-from-equilibrium systems. The economic system is clearly an open one. It is firmly rooted in the physical world. Its continued complexification requires a constant input of energy. That modern economic theory is based on outdated physical science was immediately obvious to the high-profile physicists present at the first SFI workshop described in Section 5.3.3 above. Relaying the remarks of one participant, Beinhocker states:

> "what really shocked the physical scientists was how to their eyes, economics was a throwback to another era…it looked to them as if economics had been locked in its own intellectual embargo, out of touch with several decades of scientific progress, but meanwhile ingeniously bending, stretching, and updating its theories to keep them running." (Beinhocker, 2007, p.47).

Brian Arthur argues that the existence of endogenous disequilibrium in economic phenomena can be primarily attributed to two sources (Arthur, 2015, pp. 4-7). 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 (Knight, 1921), and George Soros' reflexivity principle (Soros, 1987; 2008), Arthur argues that, because all situations involving choice in the economy involve the outcomes of future events, which are by definition unknowable, and further, they involve an infinite regress of if-then decisions based on other agents' behaviour, the optimisation problems that traditional models assume individuals conduct are not well-defined as is required for the determination of a solution, and so the notion of deductive rationality is logically impossible. In a recent paper exploring the possibility of convergence to approximate Nash equilibriums in two-player NxN games and n-player binary action games, the authors remarked that:

> "if specialized algorithms cannot compute an (approximate) equilibrium, it is unreasonable to expect selfish agents to "naturally" converge to one." (Babichenko & Rubinstein, 2016, p.1)

Thus reinforcing a famous quote by Kamal Jain, that:

"If your laptop can't find it, then neither can the market." (Papadimitriou, 2015, p.800)

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[5]. Complexity economics therefore, replaces the impossible assumption of deductive rationality, with the empirically plausible assumption of inductive procedural rationality.

Herbert Simon argued long ago that human agents *satisfice* – they have limited information available to them and do the best they can with that information; they do not rationally optimise (Simon, 1976; 1987). For this empirically derived observation Simon was awarded a Nobel Prize. Building on Simon's work, decades of research by economists and psychologists initiated the *Behavioural Economics* movement. Daniel Kahneman and Vernon Smith were awarded a Nobel Prize in 2002 for pioneering work in this area, and more recently, both Robert Schiller (2013) and Richard Thaler (2017) have also been selected for the award. Despite such high profile accolades for these individuals, their work has barely made a dent in mainstream modelling due to the ideas being mostly mathematically intractable.

Complexity economics however, has incorporated these ideas into the very heart of their research programs. For example, John Holland (computer scientist), Keith Holyoak (psychologist), Richard Nesbett (Psychologist) and Paul Thagard (cognitive scientist) have developed an empirically informed framework of inductive decision-making that can be used to model human decision-making in a realistic way (Holland et. al., 1986). The framework is structured around the categories of: Agents; Goals; Rules of Thumb; and Feedback and Learning.

A second source of endogenous disequilibrium is identified in technological change (Arthur, 2009; 2015). 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 series' of 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 of endogenously generated disequilibrium 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 (Arthur, 2015; Farmer, 2012; Epstein, 2006; Tesfatsion, 2005)[6]. 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 I identify in methodological commitments between the complexity and mainstream schools 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).

Norton Wise has remarked on a similar situation within physics, where the traditional model of explanation, deduction from partial differential equations, has been giving way to explanation via simulations which seek to:

> “explain complex phenomena by *growing* them rather than by referring them to general laws.” (Wise, 2011, p.349. Italics in original. See also, Wise, 2017))

Epstein states further, 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).

The orthodox approach to furnishing microspecifications for macroeconomic phenomena, to which complexity economists vehemently object, is drastically different. During the mid-1950s the economics profession had two separate approaches to explaining aggregate economic phenomena. These approaches were known as *general equilibrium theory* and *Keynesian macroeconomics*. General equilibrium theory was based on the assumptions of fully flexible prices and market clearing, whereas Keynesian macroeconomics emphasised market rigidities and imperfect information. The *neoclassical synthesis* reconciled these two approaches by claiming that the former describes long-run trends while the latter describes short-run fluctuations. A Phillips curve augmented by mechanically generated expectations describes the transition between the two runs. However, these two frameworks are fundamentally at odds. The market clearing assumptions of the general equilibrium approach implies the impossibility of phenomena such as involuntary unemployment, while this concept forms a core idea within Keynesian macroeconomics.

From the beginning of the 1970s, attempts to understand the relationship between these competing approaches were pursued in a search for a microeconomic foundation for macroeconomic theory. Two separate research programs were formed: *New Classical Macroeconomics* and *New Keynesian Macroeconomics*. New classical models incorporated the device of *representative agent*. With this device, any differences between individual and aggregate behaviour are assumed away, by modelling aggregate behaviour as the outcome of either a single representative agent, or a group of identical agents. New Keynesian researchers sought to incorporate price rigidities and the non-neutrality of money in an equilibrium (or partial-equilibrium) framework by means of imperfect competition. The competing classical and Keynesian visions eventually merged into one another, resulting in a shared methodology: traditional macroeconomic issues are now studied using the same tools and techniques applied in microeconomics (Janssen, 2006, p.6). That is, all macroeconomic propositions are derived from fundamental hypotheses on the behaviour of individual agents. All these models incorporate equilibrium and *rational* behaviour. Where agents' expectations are incorporated, these are also *rational*. Expectations for model variables are considered *rational*, when they match the values of variables predicted by the model.

It is quite clear that in representing aggregate economic phenomena as the result of a single individual decision problem, the possibility of emergent phenomena is excluded from the outset: properties that do not exist at the individual level cannot exist at the macro level. And the essential problem of how prices act to coordinate the activities of individual agents to create wealth is assumed away by the device of assuming perfect coordination (Van Ees & Garretson, 1990, pp. 139-142).

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.

5.5 Philosophical Commitments

In Chapter 3, I introduced a heuristic tool for categorising and contrasting the philosophical commitments of various approaches to economic methodology (see: Section 3.2). 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? (Epstein, 2006, p.xii). He goes on to argue that:

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

In their construction of explanatory theories, practitioners take a realistic approach to their subject matter.

Explanation

I argue in Chapter 6 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

Theoretical 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.

Note however, that this perspective is sometimes explicitly denied by complexity economists. Alan Kirman and Mauro Gallegati, for example, argue that the epistemological status of the *hard* sciences differs radically from that of the *soft* sciences due to the different roles that laws play (Gallegati & Kirman, 2012, p.6). However, as discussed in Chapter 2, the mechanistic account of explanation focuses on the notion of *invariance* in place of that of *laws*, and it is with reference to this framework that I'm interpreting 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.

5.6 Relations to Other Schools of Thought

In Chapter 3, I showed, through the lens of philosophical convictions, how various schools of economic thought are committed to methodological practices that are at odds with the mainstream paradigm inspired by the logical positivists and their Deductive-Nomological model of scientific explanation and theory construction. We are now in a position to see how some of the concerns of the Austrian, Historical and Institutionalist Schools of economic thought 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, including by means of downward causation. These factors are considered central to the task of successful explanation for members of both the institutionalist and historical schools. And for the Austrian school, it is the dynamic economic processes that are the key targets of economic explanation. The complexity approach accommodates this concern through its focus on non-equilibrium dynamics with an agent-based approach that satisfies the requirement of methodological individualism and subjectivism.

And all these schools of thought clamour for a realistic rendering of human psychology, which the complexity approach also requires. As pointed out in Chapter 4 (see: Section 4.5.3), the Neo-Mechanist model of scientific explanation is inherently pluralistic. As is the Complexity Economics framework; it provides a wide range of deductive, inductive, experimental and other resources for the task of theory construction.

5.6.1 Austrian Economics

Brian Arthur once stated that:

> "Right after we published our first findings, we started getting letters from all over the country saying, "You know, all you guys have done is rediscover Austrian economics"… I admit I wasn't familiar with Hayek and von Mises at the time. But now that I've read them, I can see that this is essentially true." (Arthur, 1996).

And another practitioner reports that:

> "The science of complexity pays attention to exactly the same features of complex systems which are stressed by Hayek." (Slanina, 2014, p.1).

5.6.2 Institutionalist Economics

Claudius Grabner has hypothesised that a complementary relationship exists between "the admittedly very heterogeneous" Complexity Economics research program and Old Institutional Economics. His threefold argument is that: (i) eminent institutional economists have expressed ontological views about the economy being a complex system; (ii) complexity economics lacks meta-theoretical foundations, which institutionalist theory is capable of providing; and (iii) the methods of complexity economics could greatly benefit institutional economics (Grabner, 2017; Grabner & Kapeller, 2015). Grabner argues, as I do, that *mechanistic explanation* provides the meta-theory required to unify complexity economics and institutional economics: mechanistic explanation provides the overarching methodological framework for both schools of thought, and complexity economics provides methods for institutional economics in the context of this framework. The reader will be well aware that this argument mirrors the basic overall thesis of this book: that mechanistic explanation provides an overarching framework within which all the diverse activities carried out by economists can be unified, and that complexity economics offers a suitable platform from which to launch such a methodological reorientation.

The primary point of departure between my argument and that of Grabner is the conception of *mechanism* I appeal to. Grabner, while recognising that the term is often used ambiguously, utilises a definition provided by Mario Bunge, that a mechanism is a:

> "process (or sequence of states, or pathway) in a concrete system" (Bunge, 2004, p.186)

And I argued in Chapter 2 (see: Section 2.7 above) that Bunge's conception of mechanism does not provide an adequate basis on which to form mechanistic evaluations. Grabner, however, uses this definition to contend that Institutionalist Economics satisfies the normative criteria stemming from a commitment to mechanistic explanation. I have argued in Chapter 3 (see Section 3.6.5 above) that this is not the case.

Wolfram Elsner has also recently remarked that complexity economics is capable of providing the tools required to carry out research programs on issues dear to the old institutionalists (Elsner, 2017). He suggests further that complexity economics is consistent with *most* issues of importance to the different heterodoxies and is capable of providing them with the tools required, and that the evolution of such a process could lead to convergence (Elsner, 2017, p.943). Two important concepts from old institutionalism are clearly addressed by complexity economics: Circular Cumulative Causation (CCC) and Open

Systems Approach (OSA) (Berger & Elsner, 2007). CCC, as discussed in Chapter 3 (see: Section 3.6.3) was a key methodological commitment developed by Gunnar Myrdal on the basis of Thorstein Veblen's concept of *cumulative causation*. Recall that Veblen contended that the primary characteristic of an evolutionary economist is that:

> "he insists on an answer in terms of cause and effect…the notion of cumulative causation" (Veblen, 1898, p.377)

And he defines "cumulative change" as:

> "For the purpose of economic science the process of cumulative change that is to be accounted for is the sequence of change in the methods of doing things – the methods of dealing with the material means of life…The economic life history of the individual is a cumulative process of adaptation of means to ends that cumulatively change as the process goes on…" (Veblen, 1898, pp. 387/391

Myrdal added the "circular" C to CC to produce CCC:

> "…circular causation will give rise to a cumulative movement only when…a change in one of the conditions will ultimately be followed by a feed-back of secondary impulses…big enough not only to sustain the primary change, but to push it further. Mere mutual causation is not enough to create this process…" (Myrdal, 1968, p.1875).

And thus Myrdal added the concept of self-reinforcing positive feedback, which is also a key tenet of complexity economics, as has been shown in this chapter.

Another key institutionalist concept is the Open Systems Approach (OSA). The OSA was built upon the biological theory of open systems developed by Ludwig Bertalanffy and Erwin Schrodinger in the 1930s (Berger & Elsner, 2007, p.531). The complex formation of the life process, which is viewed as an open system, implies that the economic process is an entropic transformation (Georgescu-Roegen, 1966, p.97). There are two primary implications of this conclusion drawn by the institutionalists. Firstly, from the physical perspective, an economic system can be expected to convert low entropy into higher entropy via the production of irrevocable waste. The second implication is that in order to research the workings of the socioeconomic system, economists need to analyse the institutional setting within which the open economic system is embedded and the institutional changes the system elicits in the wider cultural frameworks. These thoughts are clearly in line with those expressed by complexity economists.

One unsympathetic reviewer of the old institutionalist movement, who described an essay by Rexwell Tugworth as: “the worst piece of writing on economic subjects I have ever encountered” (Boulding, 1957, p.11), had the charity to admit that: “the sources of dissent were all valid and still are.” (Boulding, 1957, p.12). With the methods of Complexity Economics, perhaps these “valid” concerns can be addressed.

5.6.3 Historical Economics

As mentioned in Section 3.6, the Old Institutionalist movement was influenced by the German Historical school of thought. In as far as many of the key concerns of interest to the latter school have been reflected in the endeavours of the former, these may be expected to find research realisation via the tools of Complexity Economics under a mechanistic explanatory methodological framework. The driving ambition of the historical economists was to analyse individual economic activity in relation to the environment. Individual economic actions are expected to differ in both nature and effects according to the physical, social, religious and political conditions in which the individuals are embedded. Theoretically, the laws of economics are not considered to be universal truths; they are provisional and conditional - they are historically determined and undergo continual evolution. This *organic* approach was contrasted with the *mechanical* approach of classical economics. And as has been stressed at various times throughout this book, the *neo-mechanistic* approach to theoretical construction and development is a radical reworking of old mechanical philosophies that is specifically designed to address issues of complex systems such as those of primary interest to organicists.

5.7 Conclusions

In this chapter, I introduced the school of economic thought known as *Complexity Economics*. I explained how 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 with the methodological framework of the orthodox paradigm. I also showed that the complexity economics framework provides resources for addressing the concerns of other schools of economic thought such as the *Institutionalists* and the *Austrians*.

In the following chapter, I seek to establish that the methodological framework of complexity economics conforms to the normative standards demanded by up-to-date philosophy of science, as represented by the neo-mechanistic model.

Chapter 6 - Does Complexity Economics Incorporate a Mechanistic Methodology?

The purpose of this chapter is to show that the methodological framework of Complexity Economics satisfies the normative criteria of the Neo-Mechanistic model of scientific explanation, and as such provides a suitable basis for the methodological reorientation of Economic Science.

6.1 Introduction

To establish the conclusion of this chapter, firstly, in Section 6.2, I examine the conformation of the Complexity Economics framework with several mechanistic categories. Then, in Section 6.3, I address a number of potential objections to my argument, before concluding in Section 6.4.

6.2 Is Complexity Economics Mechanistic?

In the previous chapter (see: Section 5.5), 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 2. 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. I will then address methodological commitments with reference to the key concepts of realism, reductionism and experimentation, as well as discussing the issue of modelling environmental factors.

6.2.1 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 purely mathematical approach is therefore viewed as incapable of capturing the appropriate explanandum phenomena. This is by no means a new insight. Alfred Marshal, for example, noted as far back as 1890 that:

> "But while a mathematical illustration of the mode of action of a definite set of causes may be complete in itself, and strictly accurate within its clearly defined limits, it is otherwise with any attempt to grasp the whole of a complex problem of real life, or even any considerable part of it, in a series of equations. For many important considerations, especially those connected with manifold influences of the element of time, do not lend themselves easily to mathematical expression; they must either be omitted altogether, or clipped and pruned till they resemble the conventional birds and animals of decorative art. And hence arises a tendency towards assigning wrong proportions to economic forces; those elements being most emphasised which lend themselves most easily to analytical methods." (Marshal, 1890, p.850).

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 (Arthur, 2015, p.16). And, as mentioned above, these are precisely the type of theories that motivated the development of the mechanistic model of explanation in the first place.

6.2.2 Phenomena

A correct specification of the phenomena to be explained is an essential criterion for successful explanation under the Neo-Mechanistic account. As Craver and Darden point out, a mechanism is always a mechanism of a given phenomenon, and as such a mechanism description must be capable of producing the phenomena under investigation; not some stylised version of it (Craver & Darden, 2013, p.52). 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 explicit 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[1]. It is a realm of temporal phenomena. To illuminate the idea, I'll introduce a traffic jam example provided by Arthur (Arthur, 2015, p.12). 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 in economic contexts 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)

But are 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,)

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.

Complexity economists also often speak in terms of mechanisms. Although orthodox economists also speak metaphorically in terms of mechanisms, the following quote from John Holland appears to indicate a more serious attitude in this regard:

> "Few network studies concentrate on the formation of borders within a network. And there is even less study of mechanisms for the formation of hierarchies – mechanisms that would explain the pervasiveness of hierarchies in natural systems. That is due in part to the extreme difficulty of the mathematics of such processes; however, it is also due in part to the current focus of network studies, which are not mechanism-oriented." (Holland, 2012, pp. 17-18)

6.2.3 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.

Neo-mechanists tell us that:

> "Learning what the putative components of a mechanism can do, especially under the circumstances considered to be the normal operating conditions for the mechanism, constrains the space of plausible mechanisms for a phenomenon: the space of plausible mechanisms includes only mechanisms consistent with the abilities of the mechanism's components." (Craver & Darden, 2013, p. 106)

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 the previous chapter (see: Section 5.4) 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 with real capabilities. Agents are represented as encapsulated pieces of software incorporating data and behavioural methods of acting on that data. Behavioural methods can include socially instituted behavioural methods and private behavioural methods. Methods for changing behavioural methods are also included (Tesfatsion, 2005, p.8).

6.2.4 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. 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).

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. Neo-Mechanists also emphasise the importance of including time in models of mechanisms, since the temporal order of stages is a crucial part of indicating the flow of productivity in a mechanism. Facts about productive order are considered crucial constraints on the space of possible mechanisms and as "essential guides to the overall working of a mechanism." (Craver & Darden, 2013, p.111-114). Complexity modellers are clearly committed to this requirement, unlike their mainstream counterparts.

An important thing to notice about the mechanistic account adopted here, is that activities aren't merely characterised as interactions[2]. 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)

A purely 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[3].

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 criterion for mechanistic explanation that was identified in Chapter 2. A further criterion is that representations of activities be veridical. Testimony for complexity economists' adherence to this criterion 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 (see, for example: Arthur, 2015, p.4). This approach is contrasted to mainstream practice, in which assumptions are made on the basis of analytic convenience.

6.2.5 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[4]. 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.

6.2.6 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)

6.2.7 Environment

I pointed out in the previous chapter (see: Section 5.4) that economic systems are paradigmatically *open* systems. This means that environmental conditions can be expected to play important explanatory roles. Note that the definition of mechanism provided by Machamer, Darden, and Craver (see: Section 2.3.1) includes mention of set-up conditions. And that further, in the process of properly characterising the phenomenon to be explained, precipitating, inhibiting, modulating, and non-standard, conditions need to be considered and understood (See: Section 2.3.2). Environmental factors are therefore an important consideration for Neo-Mechanistic explanation.

John Holland explains that local environmental conditions play an essential role in the adaptation and evolution of complex adaptive systems. It is by moving about in an inhomogeneous environment that agents encounter the differential conditions that serve to drive evolutionary mechanisms. He concludes that in attempting to understand such phenomena, an underlying *geometry* should form an explicit aspect of explanatory models (Holland, 2012, p. 53). Joshua Epstein and Robert Axtel have produced a model aimed at explaining the emergence of economic systems. Their model incorporates only a few simple elements; agents with a few simple abilities and an environment with some natural resources (Epstein & Axtel, 1996). Agents are heterogeneous and are endowed with vision and metabolism. The model is capable of generating a number of emergent behaviours reminiscent of those associated with real world economic systems, but the point I'm highlighting here is that models like this one take seriously the idea that economic systems are open and that environmental factors are key explanatory factors.

6.2.8 Realism

It has been touched on repeatedly above, but it's worth reiterating here that Complexity Economics is committed to representative realism. As Eric Beinhocker rightfully points out, when Leon Walras borrowed wholesale the concepts and equations from contemporary introductory physics textbooks in order to create a "science of economic forces", he initiated a habit that was to be followed by economists throughout the following century: the trading off of realism for the sake of mathematical predictability (Beinhocker, 2007, p.33). Brian Arthur, describing the informal exchanges between the economists and physicists at the first SFI workshop discussed in the previous chapter (see: Section 5.1.3), is quoted as saying:

> "The physicists were shocked at the assumptions the economists were making…I can just see Phil Anderson, laid back with a smile on his face, saying, "You guys really *believe* that?" The economists backed into a corner would reply, "Yeah, but this allows us to solve these problems." …And the physicists would come right back, "Yeah, but where does that get you – you're solving the wrong problem if that's not reality." (Waldrop, 1992, p.142)

This objection to unrealistic assumptions in economic models by physicists in not new. In fact, Henri Poincare voiced the same concern in response to the work of Walras almost a century earlier (Ingrao & Israel, 1990, p.159). The economists' position on assumptions expressed above is the legacy of Milton Friedman's methodological convictions, which were presented in Chapter 4 (see: Section 4.4.1). But as was explained in Chapter 2, a valid mechanistic explanation requires a veridical representative model. One set of authors put it this way:

> "Commitment to the goal of correctness for mechanism schemas places a variety of empirical constraints on any acceptable mechanism schema" (Craver & Darden, 2013, p. 97).

The *Econometrica* associate editor's report in response to the submission of the classic Arrow and Debreu paper included the following note:

> "The paper leaves the reader with the definite impression that the existence of equilibrium for an economic system requires rather strong assumptions. If one would like to derive some realistic conclusion from this, this conclusion would be that very likely the real system would be deprived of such assumptions and of an equilibrium, also." (Weintraub, 2002, p.199)

The objections to unrealistic assumptions in economic models have become to hold a core position within the Complexity Economics community. Herbert Simon - a highly prominent intellectual ancestor of complexity economists – vehemently objected to Friedman's pronouncements on the goals of science and the realism of assumptions. He argued that the purpose of developing scientific theories is to *explain* things, and the purpose of making predictions is to test whether the explanations are correct (Archibald, Simon & Samuelson, 1963, pp. 229-231).

One may well ask: how do orthodox economic theories do when it comes to prediction, despite the falsity of their model assumptions? Alan Kirman notes that:

> "almost no one contests the poor predictive performance of economic theory." (Kirman & Gerard-Varet, 1999, p.8).

One piece of evidence for such poor predictive performance was presented by Prakash Loungani in 2001. Loungani analysed consensus predictions for real GDP growth for sixty-three countries throughout the 1990s. He found that at a period of one-year in advance, only two of sixty recessions were predicted (Loungani, 2001, p.430). The research was updated in 2018, with much the same results (An, Jalles & Loungani, 2018).

Another piece of evidence relates to the standard Dynamic Stochastic General Equilibrium (DSGE) models, which failed central banks so spectacularly in forecasting the global financial crisis that commenced in 2007. In June 2010, a workshop on Agent-Based modelling (ABM) funded by America's National Science Foundation, was attended by, amongst others, economists from the US Federal Reserve bank and the Bank of England (The Economist, 2010). It isn't too surprising that the DGSE models failed to predict the financial crisis, since they do not incorporate a financial sector. The DSGE model used by the ECB only incorporates households, firms and monetary policy rules (Smets & Wouters, 2003). Dissatisfaction with the models used by central banks was voiced by US government officials at a House of Representatives subcommittee hearing in July 2010 titled Building a Science of Economics for The Real World. The hearing was just the latest in a series that investigated:

> "how the global financial meltdown of 2008 may have been caused or abetted by financial risk models, many of which are rooted in the same assumptions upon which today's macroeconomic models are based." (US Government Printing Office, 2010)

Note the claim of potential causation here. Standard models did not only fail to explain financial market phenomena, but may in fact constitute causes of highly detrimental events, the results of which included:

> "hundreds of billions of dollars in losses to financial firms, and to a global recession with trillions of dollars in direct and indirect costs imposed on U.S. taxpayers and working families...People around the world are losing their homes, their jobs, their dignity and their hope." (US Government Printing Office, 2009)

Some models used by central banks do however incorporate the banking sector, although in a homogeneous manner. A recent study using agent-based modelling shows that the standard binary classification of banking institutions into big/central and small/peripheral is far too simple to capture risk within interbank markets. A more realistic heterogeneous network model was shown to provide early warning signals of financial crises (Squartini, et.

al., 2013). Since the 2008 financial crisis there has been increasing interest in using ideas such as these to explain financial market events (Battiston, et. al., 2016).

6.2.9 Reduction

As noted in Chapter 2, there is a tension between the pre-twentieth-century sense of the term *mechanistic* and the way that it is used in the Neo-Mechanist literature. One can often find statements by complexity economists decrying mechanistic approaches to theory construction. What they mean by this though, is old-fashioned logical positivist reduction.

Stuart Kauffman, a theoretical biologist, and a participant at the first SFI Complexity Economics workshop is credited by Carl Craver and Lindley Darden as being an early pioneer of the Neo-Mechanistic approach to the philosophy of Biology (Craver & Darden, 2013, p.26). Kauffman receives this credit for his work analysing reduction in terms of decomposition into parts (Kauffman, 1971). The authors also credit one of Kauffman's colleague at the University of Chicago at the time – William Wimsatt - with this accolade, quoting from him:

> "At least in Biology, most scientists see their work as explaining types of phenomena by discovering mechanisms, rather than explaining theories by deriving them from or reducing them to other theories, and this is seen by them as reduction, or as integrally tied to it." (Wimsatt, 1972, p.67).

Craver and Darden stress that reduction in the sense of decomposition into parts and processes is incompatible with the traditional view of *levels* of a scientific discipline. They provide the example of Biological Science in which a hierarchy exists that includes: ions; small molecules; macro molecules; cells; organs; organisms; populations; and ecosystems. They argue that a neo-mechanistic explanatory approach does not imply the division of labour between these strict levels. Instead, they argue that since any explanatory target within the hierarchy will require an understanding of feedback processes between components at various levels, interfiled research programs are required. Such a view is compatible with the complexity economics approach, in which models designed to explain higher level aggregate outcomes are calibrated with empirically derived components at the individual agent property level.

Further, steadfast adherents of complexity science follow a truly interdisciplinary approach. This is because a number of key complexity themes are viewed as being substrate neutral, while at the same time, the approach is thoroughly empirical. For example, evolution is a key principle featuring in the models of complex systems researchers; it is conceived of as a mechanism (or group of mechanisms) that is instantiated in a variety of substrates, including biological systems and economic systems. This means that an explanatory research program in, for example, economic science, may require an evolutionary theory expert, a computational expert and economists specialising in the relevant sub-disciples, at a minimum. The case study in the following chapter, in which I analyse the Santa Fe Artificial Stock Market Model – which attempts to explain real world asset pricing dynamics - bears this conviction out; the researchers involved are: Brian Arthur – Economist; Blake LeBaron – Economist; John Holland – Computer Scientist & Psychologist; Paul Taylor – Computer Scientist; and Richard Palmer – Physicist.

6.2.10 Experimentation

Experiments are indispensable in the search for mechanisms. Craver and Darden delineate three different types of experiment that constrain mechanism schemas (Craver & Darden, 2013, pp.119-142). Firstly, there are experiments to determine whether one entity, property, activity, or organisational feature is causally relevant to another. Secondly, there are experiments designed to test whether the behaviour of the mechanism as a whole is altered by intervening on an entity, property, activity, or organisational feature. The bulk of experimental work is expected to be of a third type of experiment. This category is a heterogeneous grouping comprised of all experimental designs conducted for the purpose of answering specific questions about a mechanism. These experiments involve complex, multiple patterns of intervention and detection.

Complexity economists are committed to undertaking rigorous study of economic systems through controlled computational experiments. To meet this commitment, they:

> “model the salient structural, institutional, and behavioural characteristics of economic systems…formulate interesting theoretical propositions about these models, evaluate the logical validity of these propositions by means of carefully crafted experimental designs, and condense and report information from their experiments in a clear and compelling manner. Finally, they need to test their experimentally-generated theories against real world data.” (Tesfatsion, 2005, p.10).

Experimentation within the orthodox paradigm is generally conducted by practitioners who are separate from those developing theory, as was discussed in Chapter 2. Although these data generated models cannot, to my mind, in all honesty be accurately described as experiments.

Ne-Mechanists promote a wide range of experimental approaches. Experiments designed to determine causal relevance investigate whether entities and activities at one stage of the hypothesised mechanism act as stimulants, inhibitors or maintainers of those at another stage. Experiments designed to determine which parts are relevant to the behaviour of a hypothesised mechanism as a whole, are referred to as *interlevel experiments*. These types of experiments can either be bottom-up: intervening on a component to detect changes in the behaviour of a mechanism as a whole, or top-down: intervening on the explanandum phenomenon to detect changes in the activities or properties of the components of a hypothesised mechanism. The three most common kinds of interlevel experiments are: *interference* experiments; *stimulation* experiments; and *activation* experiments.

Interference experiments are bottom-up, inhibitory experiments. The goal of this class of experiments is to diminish or disable a component in a lower-level mechanism and determine the effect on the explanandum phenomenon. Stimulation experiments are bottom-up excitatory experiments. The goal of this class of experiment is to excite or intensify a component in a lower-level mechanism and determine the effect on the explanandum phenomenon. Activation experiments are top-down excitatory experiments.

The goal of this class of experiment is to trigger the explanandum phenomenon and determine whether one or more components of its mechanism changes.

6.2.11 Conclusions

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.

6.3 Objections

In this sub-section, I respond to six potential objections to the main argument of this chapter - that Complexity Economics is Mechanistic. First, I address the objection complexity economics is not fundamentally different to the orthodox approach. Next, I respond to the objection that simulation studies are not experiments. Then, I rebut the claim that orthodox practice also utilises simulation methods, before addressing the issue of dynamics in orthodox models. Following this, I respond to the objection that complexity economists simply wish to reduce economic science to a different type of mathematics than that practiced by the mainstream. Finally, I consider the contention that due to the extreme interdisciplinary nature of the Complexity Economics program, is impossible for it to be institutionalised.

6.3.1 Is Complexity Economic Really So Different from Orthodox Economics?

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 Deductive-Nomological account, and so he claims that the standard can be considered to fall within the hypothetico-deductive framework[5]. But, admitting that his requirements for explanatory candidacy are weak, he goes on to seek philosophical legitimacy by claiming common elements with van Fraassen's constructive empiricism model (see: Section 1.4.3.1)[6].

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.

John Holland expresses a similar lamentation when he states, regarding complex adaptive systems (cas) that:

> "Owing to the current lack of a full-blown formal theory of *cas* mechanisms, the *cas* framework cannot serve as a ready-made candidate for an overarching framework of signal/boundary systems." (Holland, 2012, p.22)

He does however recognise that:

> "both as humans and as scientists we generally understand much more than we can establish through logical argument." (Holland, 2012, p.22)

And to back this up, he provides the example of artificial flight. Noting that an understanding gained from logical argumentation via Newton's laws of motion is impossible due to an overwhelming amount of required detail, he acknowledges that an understanding can only be attained by:

> "concentrating on the mechanisms that generate the dynamics." (Holland, 2012, p.23)

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. For example, responding to a question as to whether complexity economics is controversial, Arthur states:

> “No, not any more. Complexity economics is an extension of equilibrium economics to the nonequilibrium case. And since nonequilibrium contains equilibrium it's a widening of economics — a generalization. So that's not controversial, that's inevitable.” (Arthur, 2017).

He goes on to clarify however, that he expects it to take another 20 to 30 years before such changes have taken place within standard economics departments. In Chapter 4 (see: Section 4.5.3), I introduced the literature arguing that the neoclassical stranglehold in economic science is being progressively loosened. I noted there, that two prominent competing visions run through this literature. One view states that the future of the economics discipline will become increasingly pluralist, while the other interprets increasing pluralism as a transitionary stage to a new mainstream paradigm. Those authors supporting the latter vision are split between those who believe a revolution is required, and those who do not. One prominent view within this literature mirrors Arthur’s sentiments in the above quote. David Colander, for example, heavily promotes the idea that the neoclassical era in economics has ended and has been replaced by "the complexity era". In a co-authored paper, he claims that:

> "The complexity era has not arrived through a revolution. Instead, it has evolved out of the many strains of neoclassical work, along with work done by less orthodox mainstream and heterodox economists. It is only in its beginning stages, but it is, in our view, the wave of the future." (Holt, et. al., 2011, p.357).

Colander's views must, however, even on face-value, be taken somewhat sceptically due to glaring inconsistencies between a number of his propositions. For instance, he claims that the *neoclassical era* is supposedly dead, having been peacefully replaced by the *complexity era*, because there is heterodox work being done on the research frontiers by graduate students at elite schools (Colander, 2003). It is fully admitted that it will take at least another generation before these works filter into economics textbooks, since Colander observes:

> "I do not want to overstate the degree of change that is currently taking place in the profession; one sees only slight change in the work of most existing economists." (Colander, 2003, p.3)

But he is quite sure is will happen, even though:

> "the current elite are…quite closed minded when it comes to alternative methodologies." (Colander, et. al., 2004, p.493).

Has the complexity era arrived? Or is it sure to arrive at some distant point in the future? Either way, Colander also argues that a commitment to formal mathematical methods aligns the complexity movement with orthodox economics, which I have strongly argued against above.

There are several reasons why an argument of the hypothesised form is insufficient to establish the objection that: I have overplayed the methodological divide between current mainstream economics and Complexity Economics. Firstly, with relation to the Epstein and Holland statements provided, 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, such as the one by Arthur provided above, 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. Besides, one can oftentimes unearth quotes that contradict such statements, such as this one from Arthur:

> "I'm often asked how this new approach [Complexity Economics] fits with standard economics. Isn't it simply a variation of standard economics? And wont it be absorbed seamlessly into…the neoclassical framework? My answer to both counts is no…It is economics done differently, economics based on different concerns…an economics where the problems are different and the very idea of a solution is also different." (Arthur, 2015, p.xx).

6.3.2 Are Simulation Studies Experiments?

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 (Parker, 2009) and Eric Winsberg (Winsberg, 2010).

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. 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" (Winsberg, 2010, p.60). While Winsberg mostly agrees with Parker's position, including the epistemic 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 (Axtell et. al., 1996; Epstein, 2006, p.29). Agent-based simulation models of the types promoted by complexity economists do however have longer histories in other scientific disciplines. Such models have been developed heavily within Ecology for over 40 years (Grimm, et. al., 2005).

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.

6.3.3 Mainstream Models Use Simulations Too

Some who do not deny experimental status to simulation studies, may nonetheless object that mainstream modellers do in fact incorporate these tools in their research work. And it certainly is true that there is a role for computer simulation methods within orthodox practice. One might point to, for example, macroeconomics in which simulations are often used to solve DSGE models. These models assume infinite time horizons and so even simple models are extremely difficult to solve analytically. But this usage of simulation models is purely for the purpose of *calculation*.

Aki Lehtinen and Jaakko Kuorikoski have published a paper summarising their ideas on why economists shun simulation models. The authors believe that this fact, particularly in relation to the discipline of Physics – which economists have traditionally viewed as a paradigm for sound theoretical methodology, requires an explanation. Their primary finding is that for economists:

> "Analytical solutions are considered necessary for a model to be accepted as a genuine theoretical contribution…Economists' image of understanding emphasizes analytical rather than numerical exactness, and adeptness in logical argumentation rather than empirical knowledge of causal mechanisms" (Lehtinen & Kuorikoski, 2007, p.305/306).

Economists do not therefore grant simulation models an independent epistemic status. After examining a number of issues that could potentially cause epistemic problems for simulation models, including processes of robustness analysis – for which the authors claim is higher for simulation models than analytic models, Lehtinen and Kuorikoski conclude that the resistance to simulation amongst economists cannot be explained by epistemic reasons alone.

So, simulations are utilised by mainstream economists, but for the purposes of calculation, *not for theory generation*. Due to a penchant for *exactness* - as a means of distinguishing economic science from the other social sciences - simulation models are shunned in favour of perfect analytical tractability.

6.3.4 Don't Orthodox Economic Theories Incorporate Dynamics?

I will address this objection with reference to the Dynamic Stochastic General Equilibrium (DSGE) model. DSGE is the current state-of-the-art in explaining macroeconomic behaviour (Guilmi, Landini & Gallegati, 2017; Christiano, Eichenbaum & Trabandt, 2018). The model comes in two forms: representative agent versions and heterogeneous[7] agent versions. Both models assume no interaction between agents; all interaction is indirect through a hypothetical auctioneer. If any imperfections in the market clearing device are to be incorporated, then the framework needs to be abandoned for a game theoretic approach. It was pointed out by Alfred Marshal as long ago as the nineteenth century that if agents interact directly, there will exist non-equilibrium prices that undermine the efficiency of the market and the whole equilibrium framework collapses; the hypothesised unique ergodic steady state is not possible (Guilmi, Landini & Gallegati, 2017, p.5). In short, the DSGE model explains fluctuations in aggregate economic activity as the efficient response of the economy to uncertainty in agents' environment. Interest rate changes induced by monetary policy cause the representative agent to reallocate consumption over time. I will briefly, describe the three components of DSGE models in turn, to show that the model *does not* incorporate *dynamics* in the required sense.

The first component of the DSGE model refers to its so-called *dynamic* nature. These models assume infinite time horizons and ergodic steady states. Consumption is determined by infinitely lived consumers with infinite foresight. Sbordone, et. al. of The Federal Reserve Bank of New York explain that these models are dynamic because:

> "expectations about the future are a crucial determinant of today's outcomes." (Sbordone et. al., 2010, p.25).

But these are simply the expectations of:

> "one single combination worker-owner-consumer-everything-else who plans ahead carefully and lives forever." (Solow, 2010, p.2).

These changing expectations create a *dynamic* connection between the three basic "blocks" of a DSGE model: demand, supply, and monetary policy. The first block is the demand block. This block determines real activity as a function of the ex-ante real interest rate, as well as expectations for future real activity. The second, supply block, takes the level of activity and future expected inflation as determined by the demand block, as the key determinants of inflation. The determined output and inflation levels from the demand and supply blocks feed into the third block – monetary policy. This block represents the enaction of monetary policy by central banks as a function of real activity and inflation. The result is a model of the relationship between the three key variables of output, inflation, and nominal interest rates. In the DSGE model, expectations are the key channel through which policy impacts economic activity.

The second component of the DGSE model refers to its *stochastic* character. The stochastic component refers to exogenous shocks, the most common of which are productivity shocks to the supply block, despite scant empirical support for them (Korinek, 2015). Every period, random exogenous shocks perturb the equilibrium conditions in each of the three blocks of the DSGE model. These shocks are the only source of change in the model. Without them, the economy evolves along a perfectly predictable path determined by unchanging equilibrium relationship conditions.

The third component of the DSGE model is *General Equilibrium*. The connections between model variables are considered true equilibrium relationships; they are stable, long-run relationships defining the path of the economy. Economic fluctuations – movements around the long-term equilibrium trend - as explained in the previous paragraph, are only uncertain because they are driven by exogenous shocks. DSGE models then simply provide a static picture, with periodic random shocks that do not have any lasting impact on the equilibrium conditions.

DSGE models are built from the bottom-up. Prior to DSGE models, the dominant approach to modelling macroeconomic phenomena used structural equation models based on measured statistical relationships between macroeconomic variables. There is a clear contradiction in the current approach between the required micro-foundations and empirical reality. It is common to incorporate assumptions and parameter values that are clearly falsified by actual measured economic behaviour (e.g. labour supply elasticities to fit employment changes, utility functions to fit inflation rates, etc.).

As pointed out by Anton Korinek, the typical modern approach to writing a paper in DSGE macroeconomics has the following structure (Korinek, 2015, p.6):

1) Establish "stylized facts" about the quantitative interrelationships of certain macroeconomic variables (e.g. moments of the data such as variances, autocorrelations, covariances, and etc.) that have hitherto not been jointly explained;
2) Write down a DSGE model of an economy that is formally derived from micro-foundations describing the behaviour of consumers and firms, and that is subject to a defined set of shocks that aims to capture the described interrelationships; and
3) Show that the model can "replicate" or "match" the chosen moments when it is fed with stochastic shocks generated by the assumed shock process.

Even proponents of DSGE models who believe they are central to the future of macroeconomics recognise that they are seriously flawed. Oliver Blanchard for example, admits that:

> "They are based on unappealing assumptions. Not just simplifying assumptions, as any model must, but assumptions profoundly at odds with what we know about consumers and firms…Their standard method of estimation, which is a mix of calibration and Bayesian estimation, is unconvincing…While the models can formally be used for normative purposes, normative implications are not convincing…DSGE models are bad communication devices…All these objections are serious." (Blanchard, 2016, pp. 1-3).

And Robert Solow, who describes himself as a "quite traditional mainstream economist" suggests that:

> "I do not think that the currently popular DSGE models pass the smell test…The advocates no doubt believe what they say, but they seem to have stopped sniffing or to have lost their sense of smell altogether…A thoughtful person, faced with the thought that economic policy was being pursued on this basis, might reasonably wonder what planet he or she is on." (Solow, 2010, p.2)

The problem is that the methodological restrictions required for using the DSGE model force formalisations that are of no help in understanding the corresponding real world phenomena, since they bear little or no resemblance to these phenomena (Vilmunen, 2017, p.60). I conclude that the "dynamics" built into mainstream macroeconomic models are severely inadequate for explanatory purposes. Many of those who use these models, however, remain unconvinced that alternatives are available, with the following being somewhat typical:

> "People who don't like dynamic stochastic general equilibrium (DSGE) models are dilettantes. By this we mean they aren't serious about policy analysis...As Lucas (1980) pointed out roughly forty years ago, the *only* place that we can do experiments is in our models" (Christiano, Eichenbaum & Trabandt, 2017, p.2 (italics in original))[8].

This is simply false, since on the contrary, Complexity Economics provides several avenues for experimental investigations that incorporate *dynamics* in the fullest sense.

6.3.5 Don't Complexity Economists Simply Wish to Reduce Economics to a Different Type of Mathematics?

A potential objection to my thesis is the charge that complexity economists are no more mechanistic than their mainstream counterparts; they are just as committed to mathematics. A prominent debate within the field of *Cognitive Science* played out through the pages of a special volume of *Behavioral and Brain Sciences* in 1998, which I will refer to in order to present a first pass rebuttal of this objection. In this debate, conclusions about the complexity economics methodology can be generated with reference to the Santa Fe approach to cognitive science. This approach is contrasted with the Dynamical Systems Theory (DST) that is committed to a mathematical formalism, which in its strong form is argued to be incompatible with a mechanistic approach to explanation (Bechtel, 1998).

The Santa Fe complexity approach seeks to understand how functional information-processing structures emerge out of spatially extended dynamical systems. Melanie Mitchel, arguing that explanations in Cognitive Science do not reduce to dynamical systems theory, explains that a valid framework would need to incorporate the notions of computation and adaptation alongside insights drawn from dynamics (Mitchel, 1998). Mitchel's arguments reflect the shared multi-disciplinary methodology of the SFI community.

Within economic science there is a branch of agent-based modelling that does indeed attempt to simply replace the traditional formally derived macroeconomic models with models formally derived using different mathematics. This field is a branch of *Econophysics* and is referred to by its practitioners as the *Analytically Solvable Heterogeneous Interacting Agents* (ASHIA) program. The goal of the ASHIA program is to provide an alternative microfoundation for macroeconomics by using the analytic tools of statistical physics (Guilmi, et. al., 2017). If ASHIA was what one was referring to when making the objection, one would be correct. A program such as this is no more *mechanistic* than the formal mathematics practiced by the economics mainstream. But ASHIA is not what I would call Complexity Economics. Although I do not deny that the program is capable of producing work that could be of use to complexity economists in building mechanistic models[9].

6.3.6 Is it Possible to Institutionalise Complexity Economics?

Before moving on, I'll address one more objection: a wholesale movement to a complexity economics approach is easier said than done. Current institutional impediments provide a substantial inertial force. A report by the Gulbenkian Commission on the Restructuring of the Social Sciences in 1996 found that several changes are required to open the social sciences to emerging methodological advancements. They recommended: re-organise social science faculties according to a post-disciplinary structure; embrace a mixed-methods toolkit, based on the latest advances in computational and complexity science method; and work in interdisciplinary teams (Castellani, 2014). Implementation of such recommendations, while assuredly not easy, is not impossible. However, one must be aware that such concerns and proposals are not new. For example, Tjalling Koopmans, explaining the difficulties he had encountered in his own cross-disciplinary work noted almost 40 years ago that:

> "while our universities are the principal training ground for future scientists of all kinds, they do not seem to be the best place for gaining experience in interdisciplinary interaction. I believe that the root of the difficulty lies in the procedures for academic appointment and promotion." (Koopmans, 1979, p.13).

It remains the case that academic economists interested in truly interdisciplinary work tend to be banished from the economics department.

6.4 Conclusions

In this chapter, I established that the methodological framework of Complexity Economics conforms to the strictures of the mechanistic model of scientific explanation. I did this by showing both how it is committed to explanatory practices relating to the fundamental mechanistic categories of entities, activities, and organisation, as well as aligning with the mechanistic perspective in relation to several important concepts within the general philosophy of science. I also responded to several potential criticisms regarding the primary argument of this chapter – that unlike orthodox practice, Complexity Economics is Mechanistic. I conclude that none of these objections are valid, and stand by my thesis. In particular, I showed that the orthodox paradigm fails to adequately incorporate dynamics, and to avail itself of the experimental tools of simulation, both of which are essential elements within a mechanistic approach.

In the following chapter, I present a case study of asset pricing models to provide a specific example of the conclusions drawn in this chapter.

Part 4: Case Study

Chapter 7 – Asset Pricing Models

The purpose of this chapter is to show, with reference to a specific set of models, that the Complexity Economics framework for developing explanations of asset pricing phenomena conforms to the normative requirements of the mechanistic model of scientific explanation, whereas the framework of the orthodox paradigm contravenes many of these norms.

7.1 Introduction

In Chapter 2, I argued for the adoption of a mechanistic normative standard for economic methodology, on the basis that it provides an up-to-date model of scientific explanation that both avoids the failures of its predecessors, and delivers guidance for theoretical development. In Chapters 3 and 4, I argued that although economic science has historically embraced advances within the philosophy of science literature, it has yet to embrace the new mechanistic movement. In Chapters 5 and 6, I argued that the complexity economics movement within economic science offers a platform suitable for mechanistic theory development. In this chapter, I move beyond explicit methodological reflections, to provide a comparative evaluation in mechanistic terms of the orthodox and complexity frameworks for asset pricing models, to argue that the mechanistic *bona fides* of complexity economics are genuine.

In Section 7.2 I introduce the standard stock market model in modern finance, tracing its development by highlighting the key methodological innovations in its evolution. Then, in

Section 7.3 I present the Santa Fe stock market model platform as a complexity economics alternative to the standard model. Finally, in Section 7.4, I provide a comparative evaluation of both models in terms of the mechanistic model of scientific explanation presented in Chapter 2.

It will be shown that the complexity economics model meets the normative standards to a much higher degree than its rival. It will also be shown that where the complexity model is lacking in some regards, progressive research programmes appear to be moving in the right directions.

7.2 Orthodox Asset Pricing Models

To contrast the mechanistic credentials of complexity economics with standard practice, in this subsection, I first outline the development of the standard rational expectations model of stock markets, and showing how, as it currently stands, it conforms to the structure dictated by the Deductive Nomological model of scientific explanation examined in Chapter 1. I will pay close attention to the development of the orthodox framework, in order to highlight the shortcomings of the underlying methodology. I omit technical details wherever possible for fluidity of exposition.

First, I'll introduce the Capital Asset Pricing Model. Next, I explore the Efficient Markets Hypothesis and the Rational Expectations Hypothesis, both of which play prominent roles in modern portfolio theory. I then introduce a number of anomalous results for the mainstream paradigm and explain how these have been addressed. Finally, I present the

Arbitrage Pricing Theory model, which has replaced the Capital Asset Pricing model in many applications.

7.2.1 The Capital Asset Pricing Model

The Capital Asset Pricing Model (CAPM) is an attempt to explain the pricing phenomena of financial market securities. The first key piece of literature in the development of CAPM was the introduction by Harry Markowitz, of the mean-variance model of portfolio choice (Markowitz, 1952; 1959). Markowitz used mathematical properties of random variables to show that the diversification of a portfolio of shares could reduce the variability of returns. The insight is simply that while the expected value of a portfolio of shares is equal to the weighted sum of the expected values of the individual shares, the variance of the weighted sum of the portfolio is not equal to, but less than, the weighted sum of the individual variances. He thus provided a financial interpretation of some mathematical results.

Markowitz states that the process of selecting a portfolio consists of two stages. The first stage involves observation and experience, and results in beliefs about the future performance of available securities. The second stage involves using these beliefs to choose a portfolio. His paper deals exclusively with the second stage. Markowitz showed that if one considers all possible portfolios of risky assets, these portfolios determine all possible combinations of mean and variance, and a parabola forms the boundary of these combinations. A higher mean return is considered a good outcome while a higher variance is considered a bad outcome. The upper left boundary is the *efficient frontier*; no other portfolio has either the same mean with lower variance or the same variance with higher mean. All investors are assumed to transact simultaneously, determining asset prices for all available securities. Markowitz's 1952 paper is recognised as marking the birth of modern financial economics (Rubenstein, 2002). It is the first mathematical formalisation of the idea of investment diversification. And for this contribution Markowitz co-shared the Nobel Prize in 1990[1].

In his 1959 book Markowitz attempted to show that his mean-variance criterion considered over multiple reinvestment periods was consistent with the utility maximisation formalisation that had been recently developed (von Neumann & Morgenstern, 1947). These works were inspired by the Deductive Nomological model of scientific explanation and represent early milestones in the development of formal economics (see: Section 4.2). Markowitz's mean-variance framework remains a cornerstone of investment management practice to this day[2].

The next significant piece of research in the development of CAPM is due to James Tobin. He presented a *separation theorem* as a means to obtain mean-variance-efficient portfolios that are available under the assumption of risk-free borrowing and lending (Tobin, 1958). By introducing the assumption of unlimited borrowing and lending at a risk-free rate, Tobin could show that there is a single portfolio of risky assets that is *efficient*. He thus reduced the portfolio selection problem to a two-asset model; the efficient portfolio and the risk-free asset. The problem simply has two separate stages: find the efficient portfolio of risky assets, then find the optimal fractions to invest in the risky and risk free assets. These fractions are determined by an individual's level of risk aversion and other attributes, represented in the form of an indifference curve. Essentially, the model shows that investors are rewarded by taking on the price of time (the risk-free interest rate asset) and the price of undiversifiable risk (the portfolio of risky assets) in the proportions that maximise their individual utilities. The result is a set of prices for all available securities.

David Cass and Joseph Stiglitz discovered a set of necessary and sufficient conditions on investor utility functions required for investors to have separating optimal portfolio decisions (Cass & Stiglitz, 1970). Tobin was awarded the Nobel Prize in 1981 for his work on financial markets.

William Sharpe added a further assumption to the Markowitz model, to guarantee mean-variance-efficiency: that investors correctly agree on the distribution of future returns (Sharpe, 1964; Lintner, 1965[3]). He did this for the purpose of constructing a market equilibrium theory of asset prices. In his 1964 paper, Sharpe first defines an individual's utility function in terms of the first two moments of the probability distribution of their expectations for future security prices. He then expresses the investor's utility in terms of their terminal wealth, which is a function of the rate of return. Next, he assumed that, as per Tobin, all investors are able to both borrow and lend funds, on equal terms, at the same *pure rate of interest*. And further, that all investors share identical expectations for the future prices, variances and correlation coefficients of all available securities. On this assumption about shared expectations, Sharpe notes, echoing Milton Friedman, that:

> "Needless to say, these are highly restrictive and undoubtedly unrealistic assumptions. However, since the proper test of a theory is not the realism of its assumptions but the acceptability of its implications, and since these assumptions imply equilibrium conditions which form a major part of

> classical financial doctrine, it is far from clear that this formulation should be rejected – especially in view of the dearth of alternative models leading to similar results." (Sharpe, 1964, p.434).

Under Sharpe's model, as investors transact in order to arrive at their optimal portfolios, the resulting price changes induce a revision of expectations, which induce further transactions. This process continues until all prices are in equilibrium and every asset is included in at least one optimal portfolio. In contrast to Tobin's model, where there exists only a single efficient portfolio of risky assets, Sharpe's model allows for many such portfolios. However, all these portfolios must be perfectly, positively correlated. Sharpe was co-awarded the Nobel Prize, along with Harry Markowitz, in 1990 for his contribution to modern portfolio theory. While the credit (usually) goes to William Sharpe for deriving CAPM, in reality it was simultaneously derived by Sharpe and three other individuals: Jack Treynor (Treynor, 1962); John Lintner (Lintner, 1965); and Jan Mossin (Mossin, 1966). The CAPM model is represented as:

Equation 1: CAPM

$$Expected\ Return = r_f + \beta(r_m - r_f)$$

Where, $r_f = risk - free\ rate;\ \beta = Beta;\ r_m = market\ return$

Benoit Mandelbrot argued that the random walk model built on the assumption of a Gaussian distribution of price changes is invalidated by empirical data, and so he generalised the approach by modelling with stable Paretian distributions, which have infinite variance and where covariance is not a well-defined statistical concept (Mandelbrot, 1962; 1963), with the profound implication that Markowitz's definition of an efficient portfolio loses its meaning. It is therefore necessary to use some parameter other than variance as a measure of dispersion. Eugene Fama developed a formal model that had been introduced by William Sharpe to simplify portfolio analysis (Sharpe, 1963), and showed theoretically that:

> "diversification leads to a reduction in dispersion of the distribution of the return on a portfolio, even though the variance of this distribution is infinite" (Fama, 1965a, p. 418).

Fama admitted, however, that whilst sufficient to accomplish the theoretical goal of validating the benefits of diversification, there remain serious difficulties in practically implementing the idea.

Two subsequent developments of the model are worth mentioning. Firstly, Fischer Black showed that the same results obtain if the assumption of risk-free borrowing and lending is replaced with an allowance for unrestricted short sales of risky assets. In this way, he created a one asset version of CAPM (Black, 1972). And secondly, Robert Merton introduced an intertemporal version of CAPM, by adapting the assumption that investors only care about end-of-current-period wealth, to accommodate concerns for variations in future wealth due to economic variables (Merton, 1973a). Stephen Ross has noted however that extensions of CAPM such as this have "proven to be somewhat less robust than might have been hoped for" (Ross, 1978, p.898).

In response to empirical tests of CAPM (Jensen, 1972; Black, Jensen & Scholes, 1972; Blume & Friend, 1973; Fama & MacBeth, 1973; & etc.), Richard Roll argued that CAPM is not a testable scientific theory (Roll, 1977). He noted that under CAPM, expected returns are linear in beta if and only if the market portfolio is mean variance efficient. Consequently, CAPM can only be refuted by examining the implications of the statement that the market portfolio is mean variance efficient. Tests of CAPM however, rely explicitly on proxies for the market portfolio, which represent only tiny fractions of the totality of traded securities. And further, Roll showed that there is a whole family of inefficient portfolios for which the usual statistical tests of the linear relation will falsely accept the efficiency of the proxy. Eugene Fama concluded from this that:

> "In truth, all we can really say at this time is that the literature has not yet produced a meaningful test of the Sharpe-Lintner hypothesis." (Fama, 1976, p.370)

Later, Fama would claim that CAPM is useless for precisely what it was developed to do, and that:

> "our tests do not support the most basic prediction of the SLB model, that average stock returns are positively related to market betas," (Fama & French, 1992, p.428)

CAPM has been faced with even more severe criticism in recent times. On the theoretical front, for example, the logical status of the model has been scrutinised. Tom Smith and

Kathleen Walsh, for example, have argued that CAPM is a tautology (Smith & Walsh, 2013), and Tsong-Yue Lai and Mark Stohs provide a proof that the model faces either a serious problem of endogeneity or of circularity, or both, with the implication that “CAPM appears to be almost useless for predicting the rate of return for an asset in the real world” (Lai & Stohs, 2015, p.156), which is a conclusion also arrived at by others (for example, Levi & Welch, 2014). And on the empirical front, it has been reported that:

> “Echoing a recent disturbing conclusion in the medical literature, we argue that most claimed research findings in financial economics are likely false.” (Harver, Liu & Zhu, 2015, p.1)

Yet, despite recognising these serious flaws, many remain staunch defenders of CAPM, claiming that it is “the only game in town” (Smith & Walsh, 2013).

To summarise, CAPM was developed as an explanation for why the prices of financial securities take on the values they do. The explanation is that individuals attempting to maximise their utility, by intertemporally smoothing consumption over their lifetimes, select securities based on the mean-variance criterion. The result is that assets earn premia over the riskless rate, which increases with the level of their risk, and that this risk is not the intrinsic risk of the asset, but the covariance between the asset and the market portfolio; risk is priced through substitutability with other assets.

To construct this explanation, the process begins with statements about individuals and mathematically derives conclusions, in accordance with the requirements of the deductive-nomological model of scientific explanation.

7.2.2 The Efficient Markets Hypothesis

Michael Jensen points out that:

> "The Efficient Markets Hypothesis is in essence an extension of the zero profit competitive equilibrium condition from the certainty world of classical price theory to the dynamic behavior of prices in speculative markets under conditions of uncertainty" (Jensen, 1978, p.96).

The Efficient Markets Hypothesis (EMH) was developed during the 1960s as a theoretical explanation of the random character of stock market prices. A market is said to be "efficient" with respect to an information set if the price "fully reflects" that information set. The EMH asserts that financial markets are efficient. The idea was not new. In 1863, Jules Regnault – a French broker's assistant – proposed the hypothesis, validated empirically that price deviations are directly proportionate to the square root of time, and gave a theoretical interpretation (Regnault, 1863). This book is the first known attempt to construct a science of financial economics. In 1900, Louis Bachelier – a French mathematician – developed the first model of Brownian motion based on Regnault's hypothesis (Bachelier, 1900). He thus worked out the distribution function for the Wiener stochastic process, linking it mathematically with the diffusion equation, five years before

Albert Einstein's famous derivation (Einstein, 1905), and concluded that the mathematical expectation of the speculator is zero. This model became the basis for empirically testing EMH. It has been noted that what physicists call Brownian motion, statisticians call a random walk (Cootner, 1962, p.25). I therefore use the terms interchangeably throughout this chapter.

In 1925, Frederick Macaulay showed that stock price fluctuations behave like the chance curve derive from throwing a dice (Macaulay, 1925). Then, during the 1930s and 1940s, a number of studies were carried out at the Cowles Commission that sought to compare stock price fluctuations with random simulations. This represented the commencement of a period of intensive empirical investigation aimed at properly characterising the phenomenon of asset price fluctuations. Alfred Cowles was a professional investment consultant who suffered a crisis of confidence in his ability to successfully forecast stock prices. In 1933 he conducted a study which concluded that stock market forecasters cannot forecast (Cowles, 1933). Then, in the following year, Holbrook Working published a paper which concluded that stock returns behave like numbers drawn from a lottery (Working, 1934). In 1936, John Maynard Keynes famously claimed that the decisions of most investors can only be the result of "animal spirits" (Keynes, 1936). Later, in 1944, Cowles published a continuation of his 1933 paper, reaching the same, negative conclusion (Cowles, 1944). Next, Working showed that in an ideal futures market, it is impossible for professional forecasters to successfully predict price changes (Working, 1949).

Over the following decades numerous studies were published purporting to validate the characterisation of asset price fluctuations as being genuinely randomly generated: Maurice Kendall concluded that "the random changes from one term to the next are so large as to swamp any systematic effect which may be present" and "there is no hope of being able to predict movements on the exchange for a week ahead" (Kendall, 1953); Harry Roberts concluded from his study of weekly price changes of the Dow Jones Index that "these changes behave very much as if they had been generated by an extremely simple chance model" (Roberts, 1959)[4]; Matthew Osborne, a physicist, showed that "common-stock prices, and the value of money can be regarded as an ensemble of decisions in statistical equilibrium, with properties quite analogous to an ensemble of particles in statistical mechanics" and found that log prices conform to Brownian motion. He also finds evidence for the square root of time rule, first proposed by Regnault (Osborne, 1959); Arnold Larson derived the stochastic process implied in Harold Working's "anticipatory market model" (Working, 1958) - the first theoretical argument for EMH - and applied a new time series analysis tool – the index of continuity – to subject the theory to empirical test by estimating the implied process. Working proposed that price changes are closely tied to market news, which tends to truthfully reflect changing demand and supply conditions. He demonstrated "the existence of a high-order, low-weight moving average stochastic process generating price changes" as per Working's theory (Larson, 1960); and Sidney Alexander also concluded that a random walk model fits the data best (Alexander, 1961).

But there was also a steady stream or research questioning the characterisation of asset price fluctuations as conforming to the random walk hypothesis during this period, so that by the time Eugene Fama was expounding extensively on the EMH, beginning with his 1965 papers (Fama, 1965b; 1965c), significant doubts had been raised. As early as 1915, Wesley

Mitchel had suggested that the distribution of price changes is too peaked to be Gaussian (Mitchel, 1915); Maurice Olivier had shown that the distribution of returns is leptokurtic (Olivier, 1926); Frederick Mills also proved the leptokurtosis of returns (Mills, 1927); Alfred Cowles and Herbert Jones had found significant evidence of serial correlation in averaged time series indices of stock prices (Cowles & Jones, 1937), however twenty three years later the results were largely retracted on the basis that they were then considered to be mostly artefacts of the averaging process used, and that after readjustment although serial correlation effects were still present, they were not considered by the author to be large enough to allow for substantial profits after brokerage costs (Cowles, 1960); Hendrik Houthakker conducted an analysis using stop/loss orders, finding that: "the existence of patterns of price behaviour would not be present if price changes were random" (Houthakker, 1961, p.168), the distribution of price changes is highly leptokurtic, the variance of price changes is not constant over time at either short or long intervals, the series' are non-stationary, and evidence of nonlinearity; Although he found that a random walk model fits the data best, Sidney Alexander had found leptokurtosis in the distribution of returns, and more importantly, using filter rules he discovered that there *are* trends in stock market prices, and concluded that "in speculative markets price changes appear to follow a random walk over time, but a move, once initiated, tends to persist" (Alexander, 1961, p. 26; emphasis in original); Paul Cootner, proposed what he expected would be considered heresy, that the stock market is not a random walk, since all his statistical tests detected autocorrelation (Cootner, 1962); Matthew Osborne discovered, amongst other things, evidence of clustered activity, and concluded that "In general, the picture of price motion as simple random walks is supported qualitatively; quantitatively there are some substantial departures from this simple picture" (Osborne, 1962, p.345); Arnold Moore detected serial correlation in index returns (Moore, 1962); Clive Granger and Oskar Morgenstern conducted a spectral analysis on New York stock prices, finding that long-run movements do not conform to the random walk hypothesis (Granger & Morgenstern, 1963); Benoit Mandelbrot, commenting on prior research, stated concerning the Brownian motion thesis that "it is now obvious that it does not account for the abundant data accumulated since 1900 by empirical economists, simply because *the empirical distributions of price changes are usually too "peaked" to be relative to samples from Gaussian populations*" (Mandelbrot, 1963, p.394; italics in original), and so Mandelbrot presented and tested an alternative model that replaced Gaussian distributions with "stable Paretian"[5] ones and found that the extreme tail areas of the distribution of price changes follow a power law (Mandelbrot, 1962; 1963); Eugene Fama followed up on this work and concluded that the tested market data conforms to Mandelbrot's model (Fama, 1965a); Sidney Alexander revisited his 1961 study in light of criticism of his methods, finding that the data strongly falsifies the random walk hypothesis (Alexander, 1964); and William Steiger also concluded that stock prices do not follow a random walk (Steiger, 1964).

In 1965, Paul Samuelson provided the first formal proof of EMH. He proposed a general Martingale stochastic model of price changes and derived a theorem stating that price movements are uncorrelated (Samuelson, 1965b). Unlike random walk models, the martingale process exhibits dependence in successive price changes, however, the dependence is such that it does not form a basis for increasing expected profits. The logic of Samuelson's argument is that market participants utilise all information available to them in order to maximise their welfare, and in doing so, drive prices to their equilibrium values. The result is that in more efficient markets, prices will appear more random. In 1966, Benoit Mandelbrot also formally derived theorems from an economic model, showing that in competitive markets with rational risk-neutral investors, security prices follow a martingale; they are unpredictable (Mandelbrot, 1966).

The term *Efficient Markets Hypothesis* was first coined by Harry Roberts, who also devised the taxonomy of *weak* and *strong* form efficiency, which Eugene Fama would build upon (Roberts, 1967). Credit goes to Eugene Fama for establishing the EMH as a cornerstone of modern portfolio theory. He defined, and has expounded extensively on, the concept that prices at any time "fully reflect" available information (Fama, 1965a; 1965b; 1970; 1976; 1991; 1998). For this body of work, he was awarded the Nobel Prize in 2013. In a reproduction of his doctoral thesis in the Journal of Business Fama concludes that:

> "it seems safe to say that this paper has presented strong and voluminous evidence in favour of the random walk hypothesis" (Fama, 1965c).

The EMH asserts that it is impossible to achieve higher risk-adjusted returns than the maximally diversified market portfolio. EMH is defined in three forms, according to which set of information is assumed to be "fully reflected" in current asset prices (Fama, 1970). *Weak form* efficiency assumes that information about historical prices is fully discounted. This implies that superior returns cannot be gained from studying patterns in historical prices; technical trading is not profitable. *Semi-strong form* efficiency assumes that prices fully reflect all publicly available information. This implies that superior returns cannot be gained by analysing information such as company earnings and balance sheets; fundamental investing is not profitable. *Strong form* efficiency assumes that all insider information is fully reflected in asset prices, so that monopolistic access to information cannot provide superior returns. Fama concluded on the basis of *semi-strong* and *strong* forms of tests for EMH that:

> "In short, the evidence in support of the efficient markets model is extensive, and (somewhat uniquely in economics) contradictory evidence is sparse" (Fama, 1970).

These categories of efficiency were subsequently revised by Fama (Fama, 1991). Weak-form tests, which were concerned with the forecasting power of past returns, were re-categorised as *tests for return predictability*, which also incorporates historical variables other than price. Semi-strong tests were renamed as *event studies*, and strong-form tests were renamed to *tests for private information*, although the scope of these two categories remained unchanged. The body of Fama's empirical research aims to validate these hypotheses.

Tests for *semi-strong* efficiency were conducted in two early event studies, which sought to determine the speed of adjustment of prices to new information (Ball & Brown, 1968; Fama, Fisher, Jensen & Roll, 1969). The former study analysed stock price movements around earnings announcements, while the latter measure reactions to stock splits. Both studies concluded that the market anticipated the information, with most of the price adjustment complete before the event was released to the market, and the remainder of the adjustment occurring rapidly after the announcement. Ray Ball, however, showed that there is a consistent anomaly in the behaviour of security prices after public announcement of earnings: on average systematic excess returns can be earned from buying the securities of firms whose announced earnings are above expectations and selling those of firms who failed to meet expectations (Ball, 1978). Ball concludes that the anomaly is not explainable as either market inefficiency or systematic experimental error. Instead, he puts the anomaly down to "omitted variables or other specification errors" in implementing the two-parameter Sharpe CAPM model, with the specific hypothesis being: (i) the two-parameter model, when applied to a portfolio of common stocks, mis-specifies the process generating securities' yields in equilibrium, and (ii) earnings and dividend variables proxy for the underlying determinants of equilibrium yields. Essentially, Ball argues that the market portfolio used in the experiments is not a mean-variance efficient portfolio.

Fama provides a set of sufficient conditions for market efficiency:

(i) There are no transactions costs in trading securities;

(ii) All available information is costlessly available to all market participants; and

(iii) All market participants agree on the implications of current information for the current price and distributions of future prices of each security

He admits that these conditions are not descriptive of real markets in practice, but since they are merely sufficient and not necessary, as long as markets approximate them to some degree, this is sufficient to guarantee efficiency. However, since deviations from these assumptions represent potential sources of market inefficiency, empirical measurement of the real world effects becomes a major goal of empirical research in the area.

Some of the first tests for *strong form* efficiency found negative evidence. One study conducted by Donald Rogoff showed profitable trading by company insiders (Rogoff, 1964). Another study by Gary Glass found profitable trading by corporate insiders (Glass, 1966). Similar results were provided by James Lorie and Victor Niederhoffer (Lorie & Niederhoffer, 1968). Yet another study conducted by Jeffrey Jaffe found that “For all the samples in the study, it was concluded that insiders do possess special information”, thus rejecting strong-form market efficiency (Jaffe, 1974, p.427). He finds further that the market does not respond very quickly to public information about insider trading. However, other early studies found evidence contrary to these studies just mentioned (Driscoll, 1956; Wu, 1963; Scholes, 1972). In his second major survey on tests for EMH, Eugene Fama recognised the evidence provided by the early studies and discussed further results by others (Fama, 1991; Stickel, 1985; Seyhun, 1986). However, Fama claims that due to issues with joint-tests of EMH with particular asset pricing models, EMH is not rejected.

The “fair game” properties of the EMH model are implications of the two assumptions that: the conditions of market equilibrium can be stated in terms of expected returns; and the information set is fully utilised by the market in forming equilibrium expected returns and thus current prices (Fama, 1970, p.385). In general terms, such theories posit that the equilibrium expected return on a security is a function of its “risk”, conditional on some relevant information set. The process of price formation must be specified in more detail in order to render the model testable. Fama thus recognised that any test of the EMH is necessarily a joint test of market efficiency and a specific asset pricing model. And so anomalous empirical results may indicate either market inefficiency or an inaccurate asset pricing model – or both.

Robert Lucas derived a simple model to provide a context for examining the conditions under which a price series not conforming to the Martingale property could be considered to represent evidence of market inefficiency. He concluded that:

> "A relatively crude use of hindsight, applied in a reasonably stationary physical environment, will lead to behaviour well-approximated by rational expectations" and that "the outcomes of tests as whether *actual* price series have the Martingale property do not in themselves shed light on the generally posed issue of market efficiency" (Lucas, 1978, p.1444; emphasis in original).

Given the problems with jointly testing EMH and CAPM, EMH was taken up as a working assumption, and empirical studies came to be considered tests of the asset pricing model. Early studies clearly rejected CAPM in this context, since the prediction of the model that portfolios uncorrelated with the market portfolio have expected returns equal to the risk-free rate of interest was clearly not borne out (Black, Jensen, & Scholes, 1972; Blume & Friend, 1973; Fama & MacBeth, 1973).

The EMH does not rule out small abnormal profits. Sandford Grossman and Joseph Stiglitz provided a formal proof that a sensible model of equilibrium price formation must incorporate some incentives for security analysis; perfectly informationally efficient markets are impossible (Grossman & Stiglitz, 1980). This means that any proposition to the effect that markets are always in equilibrium is false. The Grossman-Stiglitz model suggests that when information is very inexpensive, or when informed traders get very precise information, equilibrium exists and market prices will reveal most of the information available. However, under these conditions, trading volumes will be thin due to the high level of homogeneity of beliefs. It has come to be accepted that a more reasonable version of EMH claims that prices reflect information to the point where the marginal benefits of acting on information – the profits to be made – do not exceed the marginal costs of information gathering (Fama, 1991, p.1575).

Willem Buiter, commenting on how the toolbox of modern portfolio theory has failed central bankers in their pursuit of macroeconomic and financial stability, has stated that:

> "The EMH is surely the most notable empirical fatality of the financial crisis. By implication, the complete markets macroeconomics of Lucas, Woodford et. al. is the most prominent theoretical fatality. The future surely belongs to behavioural approaches relying on empirical studies on how market participants learn, form views about the future and change these views in response to changes in their environment, peer group effects etc. Confusing the equilibrium of a decentralised market economy, competitive or otherwise, with the outcome of a mathematical programming exercise should no longer be acceptable." (Buiter, 2009).

7.2.3 The Rational Expectations Hypothesis

All of this work since the late 1970s incorporates the methodological tool incorporated within the Rational Expectations Hypothesis (REH). Prior to this, John Hicks' adaptive-expectations model was the dominant expectations model in economics (Hicks, 1939). The essence of the concept underlying REH is that actual outcomes do not systematically diverge from peoples' predictions of them; individuals learn the relationship between the distribution of returns and the price, and use this in deriving their demand for risky assets. REH was first formulated by John Muth (Muth, 1961), and became a central tool of theoretical development within economics after a series of papers by Robert Lucas in the 1970s and 1980s. REH builds on rational choice theory (von Neumann & Morgenstern, 1944). Lucas published an article on econometric policy evaluation that embedded an argument which has become known as the *Lucas-critique.* Lucas' arguments was that:

> "…given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of the series relevant to the decision maker, it follows than any change in policy will systematically alter the structure of econometric models" (Lucas, 1976, p.41)

Prior to this paper, the dominant Keynesian models had incorporated a fixed parameter set. Lucas argued, however, that a number of these parameters are likely to change in response to policy actions, so that aggregate prices and quantities are likely to react differently than previously thought, since agents may change their behaviour in accordance with such policy moves. He therefore advocated a split between variable, regime-dependent parameters and a set of fixed, taste and technology parameters. In response, *representative agent* models proliferated in order to avoid the Lucas-critique. In these models, the representative agent simply recalculates their optimisation problem, given their objective function and budget constraints. Thus was born the New Classical School.

(see: Section 5.4 for more details). The purpose was to show that Keynesian policies could not have any effect on output and employment, since rational expectations are unbiased estimates of the true underlying stochastic process. In effect, Lucas argued that model forecasts and rational choice were at odds with one another so that such models were internally inconsistent. Charles Goodhart had independently asserted that the implementation of a certain policy that is based on some statistical regularity will unavoidably change this regularity. This has become known as *Godhart's Law* (Goodhart, 1975). A number of statistical studies were subsequently marshalled against Lucas' critique, which aimed to show that his proposition was unfounded (Klein, 1985; Eckstein, 1983; Favero & Hendry, 1992). However, the wholesale adoption by the economics profession of Lucas' solution to the econometric model inconsistency problem led him to later over-optimistically claim, concerning the new macroeconomic program, that it:

> "…has succeeded: its central problem of depression prevention has been solved, for all practical purposes, and has in fact been solved for many decades to come" (Lucas, 2003, p.1)

But of course, as Hashem Pesaran has pointed out:

> "The "representative" agent whose behaviour is under consideration knows, or already learned perfectly, all the structural and auxiliary parameters of the model, while it is only the observing econometrician who is supposed to be left in the dark!" (Pesaran, 1987, p.204).

In more recent times, the Lucas critique has been applied to itself. Specifically, Christian Muller-Kademann has argued that Lucas' solution fails to solve the problem he identified. While the representative model agent optimises over infinite time horizons, the current "best model" only looks forward two years, so that inconsistency remains evident. And further, Muller-Kademann points out that:

> "Deep rational expectations require that models must not only feature model-consistent expectations, but also take into account the transitory nature of the model itself. Rationality, therefore, does not only require rational expectations to be applied to the model, but also to the model choice." (Muller-Kademann, 2018, p.58).

He concludes that the only way out of the dilemma is to introduce fundamental uncertainty into economic analysis in a quest:

> "for understanding human behaviour in an environment in which humans constantly create, amend, destroy and re-create social relationships without ever arriving at invariant social laws that govern human life" (Muller-Kademann, 2018, p.59).

But representative agent models are also motivated by the desire to build Walrasian General Equilibrium models based on the modern Arrow-Debreu framework. Since this framework is far too complex to solve for heterogeneous agents, using a representative agent competitive equilibrium helps simplify the solution of a competitive equilibrium allocation. The concept of a representative agent has been traced back to the nineteenth century, to the works of Francis Edgeworth, who used the term *representative particular* (Edgeworth, 1881, p.109), and Alfred Marshal, who introduced a *representative firm* with the intention of simplifying his argument (Marshal, 1890). The merits of the device of representative agent were vigorously debated between the two world wars of the twentieth century, with the idea of the representative firm eventually abandoned from competition theory (Hartley, 1997). The technique was, however, resurrected in an attempt to give micro foundations for macroeconomic models. But it has been pointed out that there is a fundamental conflict between the equilibrium concept and the device of representative agent. For when prices are out of equilibrium, individual traders are required to re-establish equilibrium pricing by taking advantage of arbitrage opportunities (Kirman, 1992, pp. 119-121). Further, Paul Kupiec and Steven Sharpe, in an attempt to discern what impact the introduction of margin requirements has on the volatility of stock prices, have shown:

> "…the dangers implicit in using representative agent models. Uncovering the "deep parameters" of a representative agent model may be insufficient or even useless for macrofinancial policy analysis." (Kupiec & Sharpe, 1991, p.728)

Comparative statics therefore become useless.

Since General Equilibrium Theory (GET) is meant to be an explanation of social economic behaviour, it is absurd to think such a theory could be based on a single representative agent. In his original formulation in Elements of Pure Economics, Leon Walras – the recognised founder of GET - introduced a number of different economic actors: capitalists;

landlords; workers; and entrepreneurs (Walras, 1874). And Joseph Schumpeter introduced banker agents to support entrepreneurial agents (Schumpeter, 1911). But in attempting to meet expected methodological strictures, a regression in realism has taken place as axiomatization schemes have been developed.

REH has become embedded in many neoclassical theories: General Equilibrium Theory; Computable General Equilibrium; Dynamic Stochastic General Equilibrium; and EMH. In a seminal paper, Lucas set the standard technique for asset pricing models: representative, risk averse investors, with rational expectations, in an equilibrium model (Lucas, 1978). Lucas' seminal paper provided a theoretical construct to study issues that could not be addressed within CAPM.

One thing to keep in mind is that the REH was not intended as a behavioural description. As John Muth made clear, it is a property expected to approximate the *outcome* of an unspecified process of learning and adaptation (Muth, 1961; Lucas, 1978).

7.2.4 Anomalies

Subsequent empirical work by other researchers identified several categories of anomalies to CAPM and EMH. The most important of these can be organised under the labels of beta, value, size, momentum, volatility, seasonality, rationality, and the equity premium puzzle. I'll discuss some of the relevant research pertaining to each of these in turn below.

7.2.4.1 Beta

As was intimated in Section 7.2.1 above, empirical studies have repeatedly questioned the validity of the CAPM beta, suggesting that the model is seriously flawed. This was the conclusion arrived at by Eugene Fama and Kenneth French (Fama & French, 1992). Others had reached the same conclusion much earlier. Marc Reinganum, for example, had prominently showed that estimated betas are not systematically related to average returns across securities, by demonstrating that the average returns of high beta stocks are not reliably different from the average returns of low beta stocks (Reinganum, 1981). Josef Lakonishok and Alan Shapiro had also arrived at the conclusion that the CAPM beta is an inappropriate measure of risk to use in estimating risk-adjusted returns (Lakonishok & Shapiro, 1986).

The invalidation of CAPM beta has been very robust throughout time. More recent research, which confirms that low beta stocks provide higher returns – the inverse of what CAPM predicts, has concluded that the anomaly is evidence of mispricing due to behavioural and institutional frictions, rather than misspecification of risk (Baker, Bradley & Taliafero, 2013; Baker & Wurgler, 2014).

7.2.4.2 Value

The idea behind value strategies is that a diversified portfolio of stocks with lower price to fundamental value ratios provide higher risk-adjusted returns than those with higher ratios. The fundamental ratios that feature most prominently in value strategies are: price/book value; price/earnings; price/dividend; and price/cashflow. I'll briefly introduce a few of the most prominent early academic studies that highlighted this fact. Note that where ratios are expressed as yields, it is the inverse ratio that is under consideration.

Sanjoy Basu found that the securities of NYSE firms with high earnings yields return more than those with lower earnings yields on a risk-adjusted basis, and that this effect is not independent of the size effect. He concluded that the EMH-CAPM joint-hypothesis is violated (Basu, 1983). Barr Rosenberg, Kenneth Reid and Ronald Lanstein found that stocks with high book-to-price ratios earn abnormal returns. They also tested another strategy based on reversal of common factor effects and concluded that their study provided evidence of market inefficiency (Rosenberg, Reid & Lanstein, 1985). Josef Lakonishok, Andrei Schleifer and Robert Vishny provide evidence that value strategies - based on price-to-earnings ratios, dividends, historical prices, book assets, etc. - outperform the market, and they conclude that this is because these strategies exploit the suboptimal behaviour of the typical investor, *not* because they are fundamentally riskier, as per CAPM. The authors conclude that:

> "While one can never reject the "metaphysical" version of the risk story, in which securities that earn higher returns must by definition be fundamentally riskier, the weight of evidence suggests a more straightforward model. In this model, out-of-favor (or value) stocks have been underpriced relative to their risk and return characteristics, and investing in them has indeed earned abnormal returns." (Lakonishok, Schleifer & Vishny, 1994, p.1574).

Further studies confirmed that book-to-market ratios have strong explanatory power, when controlling for beta (Chan, Hamao, & Lakonishok, 1991; Fama & French, 1992).

7.2.4.3 Size

Rolf Banz demonstrated the existence of the “size effect”: that the securities of small firms exhibit higher risk-adjusted returns than those of large firms (Banz, 1981). He concludes that this represents evidence that CAPM is mis-specified. Marc Reinganum was also an early discoverer of this fact (Reinganum, 1981). One hypothesis, in keeping with the risk-return relation of CAPM, that has been proposed as an explanation for this empirical observation, is that the existence of various transactions costs means that shares of small companies are held in portfolios that are on average relatively undiversified. Investors in these portfolios are then expected to require higher rates of return as compensation for taking on total risk rather than systematic risk (Levy, 1978; Mayshar, 1979; 1981; 1983). This thesis was tested by Josef Lakonishok and Alan Shapiro. After discussing the techniques used for their battery of tests, the authors state their conclusion that:

> “The results can be more easily summarized than the techniques used to derive them: they reject the implication of the Levy-Mayshar hypothesis that total risk, as opposed to systematic risk, is more important for small firms. Unfortunately for modern capital market theory, these results also reject – at standard levels of statistical significance – the fundamental tenet of the CAPM, that beta matters.” (Lakonishok & Shapiro, 1986, p.131).

7.2.4.4 Momentum

Momentum refers to the phenomenon where prices move in trends. Momentum can be positive or negative and defined over various time horizons. Examining contrarian strategies, Werner DeBont and Richard Thaler found reversal in long-term returns; extreme losers over three to five years outperform the market over the subsequent three to five years (DeBont & Thaler, 1985; 1987). Narasimhan Jegadeesh and Bruce Lehman both followed this research up and concluded that the contrarian reversal strategy also outperforms over shorter periods – over one week and one month periods (Jegadeesh, 1990; Lehman, 1990). Narasimhan Jegadeesh and Sheridan Titman documented short-term momentum in security returns. They showed that a strategy of buying stocks that have performed well in the past and selling stocks that have performed poorly generates significant positive returns over three, six and twelve month holding periods. They report that these abnormal returns are not a result of systemic risk or delayed reactions to common factors. They suggest that such price behaviour is evidence for positive feedback trading (Jegadeesh, 1990; Jegadeesh & Titman, 1993). James Poterba and Lawrence Summers, in discussing the linkage between short-term positive serial correlation in stock returns and negative correlation over longer-term intervals, reject market efficiency, suggesting that:

> "Noise trading, trading by investors whose demand for shares is determined by factors other than their expected return, provides a plausible explanation for transitory components in stock prices" (Poterba & Summers, 1988, p.54).

7.2.4.5 Volatility

Robert Shiller, following up on his earlier work on the bond market, argued that the variation in stock market prices over the prior century was too large (between five and thirteen times too high) to be justified by the subsequent variation in dividend payments. Shiller concluded that:

> "The failure of the efficient markets model is thus so dramatic that it would seem impossible to attribute the failure to such things as data errors, price index problems, or changes in tax laws" (Shiller, 1981, p.434).

Shiller's study, along with others, came under criticism of the model-free methods used to establish their conclusions. This provoked a second generation of studies on the topic. These papers provided models for dividends and derived the econometric properties of the variance bounds statistics under the joint assumption that the present-value model is correct and the dividend equation is well specified. In concluding their survey of the literature on the new generation of tests for volatility bounds, Christian Gilles and Stephen LeRoy state:

> "This finding of excess volatility is robust and is difficult to explain within the representative-consumer, frictionless-market model…there is no longer any room for reasonable doubt about the

statistical significance of the excess volatility…when the explanation of excess volatility is found, the critical ideas will lie farther from the neoclassical paradigm" (Gilles & LeRoy, 1991, p.753/789).

7.2.4.6 Seasonality

A literature also developed during the 1980s addressing seasonal anomalies in stock market returns (Keim, 1988). Michael Rozeff and William Kinney found seasonal patterns in monthly returns in the NYSE aggregate (Rozeff & Kinney, 1976). Donald Keim also found seasonal patterns in monthly return series' and relates this to the small cap effect (Keim, 1983). Robert Ariel found that stocks appear to earn positive average returns around the beginning, and during the first half, of the calendar month, and zero average returns during the second half (Ariel, 1987). Kenneth French found that over a twenty five year period, returns for Monday were negative over every five-year sub-period, while the average return for each of the other four days of the trading week were positive (French, 1980). Lawrence Harris examined weekly and intraday pricing patterns and found, amongst other things, that: there are cross-sectional differences in weekday patterns in both trading and non-trading period returns; there are significant weekday differences in intraday trading returns in the first forty-five minutes of trading; there are systematic intraday return patterns which are common to all of the weekdays; these patterns are pervasive over time and over market value groups (Harris, 1986).

7.2.4.7 Rationality

Recall that *Rational Expectations* are those which correctly anticipate the results of the appropriate economic theory. Studies have also been conducted on investor expectations data to assess these for "rationality". Robin Greenwood and Andrei Shleifer found that: measures of expectations of stock market returns are highly positively correlated with each other; these measures tend to be extrapolative – they are positively correlated with past stock market returns as well as with the level of the stock market; and the measures are also highly correlated with investor flows into mutual funds. But more importantly, they found that measures of investor expectations are *negatively* correlated with the predictions from expected returns models. They conclude that:

> "At a minimum, our evidence rules out rational expectations models in which changes in market valuations are driven by the required returns of a representative investor" (Greenwood & Shleifer, 2014).

7.2.4.8 The Equity Risk Premium

One further empirical anomaly is worth mentioning here. In 1985, Rajnish Mehra and Edward Prescott noted that the US equity risk premium – the difference between average equity returns on US stocks and the returns on a risk-free asset - was historically far too high to be in accordance with the restrictions of general equilibrium models. They concluded from this that the problem lies with the Arrow-Debreu economy as a model of equilibrium asset returns; the results cannot be rationalised in the context of the standard neoclassical paradigm of financial economics. (Mehra & Prescott, 1985). The result has become known as "the equity premium puzzle" and has spawned a substantial literature in response, which seeks to explain it away (Cochrane, 2008; Akdeniz & Dechert, 2007; 2012; Mehra, 2006; 2008)[6]. More than twenty years later, Mehra, co-wrote a paper evaluating many of the proposed strategies seeking to provide aggregate risk explanations for the empirically observed equity risk premium. In it, the authors stated that:

> "the significance of the equity premium and related puzzles cannot be overstated. The consistency of neoclassical inter temporal economics would seem to rest, in large measure, on its eventual resolution" (Donaldson & Mehra, 2007, p.89).

An implicit assumption is that agents use both equity and the riskless asset to intertemporally smooth consumption. This is a direct consequence of the first order condition for the representative household in the model, which saves by optimally allocating resources between equity and riskless debt. In what is a typical standpoint in economic science, Levent Akdeniz and Davis Dechert state, concerning their research into the risk premium puzzle, that their model:

> "is not meant to be our view of the "correct" model that explains the equity premium. It is used solely because it is analytically tractable" (Akdeniz & Dechert, 2012, p.145)

7.2.4.9 Responses to Anomalous Results

Eugene Fama and James MacBeth, in testing CAPM, concluded that additional risk variables were empirically warranted for the model (Fama & MacBeth, 1973). Their battery of statistical tests was aimed at discerning whether *market efficiency* is a workable representation of reality (p.633) and they were unable to reject the hypothesis that:

> "average returns on New York Stock Exchange common stocks reflect the attempts of risk-averse investors to hold efficient portfolios." (Fama & MacBeth, 1973, p.633).

Eugene Fama, commenting on the literature on the empirical anomalies of CAPM, stated that the premier anomaly was not the size or value premiums but the weak role of market beta in the cross-section of average returns (Fama, 1991, p.1592). To account for the finding of the anomalies literature, further variables were added to CAPM by Eugene Fama and Kenneth French, culminating in their "three factor model" (Fama & French, 1992; 1993; 1996). The model explains the expected return on a portfolio in excess of the risk-free asset by its sensitivity to:

1) The excess return on a broad market portfolio;
2) The difference between the return on a portfolio of small stocks and the return on a portfolio of large stocks; and
3) The difference between the return on a portfolio of high book-to-market stocks and the return on a portfolio of low book-to-market stocks.

The model is represented as:

Equation 2: Fama and French Three-Factor Model

$$r_i = r_f + \beta_1(r_m - r_f) + \beta_2(SMB) + \beta_3(HML) + \varepsilon$$

Where: $(r_m - r_f) =$ market risk premium; SMB = size premium; HML = value premium; ε is a white noise error term; and $\beta_1, \beta_2\ and\ \beta_3$ are factor sensitivities calculated from the time series regression:

$$r_i - r_f = \alpha_i + b_i(r_m - r_f) + b_2(SMB) + b_3(HML)$$

It has been argued that the additional variables brought in to describe the distribution of asset returns generally resist interpretation as contributing to a risk-return relation (Dempsey, 2013, p.18). Fama and French reject this assertion and hold strongly to the risk-return thesis. To do so, they contend that the decades' worth of anomalous results do not invalidate the core idea behind CAPM – that expected return is directly related to expected risk – but that they instead show that stock risks are multidimensional (Fama & French, 1992, p.428). Specifically, they argue that smaller stocks are fundamentally more risky than large ones, and that price-to-book ratios proxy for financial distress, and that neither of these risks are captured in the market return.

Subsequently, Fama and French tested an empirical asset pricing model incorporating a momentum factor (Fama & French, 2012). Such a model had been developed and tested by Mark Carhart, and found to be superior to the French and Fama three-factor model (Carhart, 1997). In their paper, Fama and French raised an important issue regarding empirical tests of CAPM: If the model generating asset prices is the CAPM and pricing is globally integrated, then betas to the global market explain expected returns on all assets, but local CAPM models will fail. Conversely, if pricing is not globally integrated, then the global CAPM will fail, even if the local CAPM holds in all cases. Global models fared poorly in their tests, so they moved on to local models. They found value premiums in all regions, strong momentum returns in all regions except Japan, but no size premium in any region. The impacts of the value and momentum factors were found to increase with respect to stratification on the size factor, with the exception of Japan.

In 2014, Eugene Fama and Kenneth French introduced a five factor asset pricing model that they claim performs better than the three-factor model (Fama & French, 2014; 2015a; 2015b). They were motivated by evidence suggesting that the latter model fails to capture a significant amount of the variation in average returns related to profitability and investment. By manipulating the *Dividend Discount Model* (Gordon & Shapiro, 1956; Gordon, 1959) and incorporating a classic result from finance theory (Modigliani & Miller, 1961), they demonstrated that Book-to-Market:

> "is a noisy proxy for expected return because the market cap M_t, also responds to forecasts of earnings and investment" (Fama & French, 2015a, p. 2).

So, informed by the work of others (e.g., Novy-Marx, 2013; Titman, Wei & Xie, 2004), they tested the incorporation of two new elements to their model. The first of the two new factors is a profitability factor, classified from *weak* to *robust*. The second, investment factor is defined between *conservative* and *aggressive*. The five-factor model is represented as:

Equation 2: Fama and French Five-Factor Model

$$R_{i,t} - R_{F,t} = a_i + b_i\left(R_{M,t} - R_{F,t}\right) + s_i SMB_t + h_i HML_t + r_i RMW_t + c_i CMA_t + e_{i,t}$$

They conclude that there are patterns in average returns related to size, book-to-market, profitability, and investment.

7.2.5 Arbitrage Pricing Theory

In the face of repeated rejection of CAPM by empirical analysis, in 1971 Stephen Ross introduced an alternative to CAPM that retained the intuitive results of the original theory (Ross, 1971; 1976; 1977). He called this alternative the *Arbitrage Pricing Theory* (APT). APT implies that the market portfolio, central to CAPM, plays no special role. Whereas CAPM is based on the assumption that portfolio return is determined by exposure to a single factor – the market portfolio, which is mean-variance efficient – APT postulates that portfolio returns depend on the level of exposure to a number of systematic factors. It is assumed that K common factors are the dominant sources of covariance among security prices and that other sources of risk can be ignored in large diversified portfolios. Empirical work has suggested that these factors are (Roll & Ross, 1995, p.123):

1) Unanticipated *inflation*;

2) Changes in the expected level of *industrial production*;

3) Unanticipated shifts in *risk premiums*; and

4) Unanticipated movements in the shape of the *term structure of interest rates*

Stephen Ross and Richard Roll remark that it is unsurprising that these are the factors that have been discovered, since they relate directly to the traditional Discounted Cash Flow (DCF) formula for the valuation of security prices: changes in the expected levels of inflation and industrial production relate to the numerator of the DCF formula, while unanticipated changes in risk premiums and interest rate structure relate to the denominator – the risk adjusted discount rate. The APT model is a linear model, represented as:

Equation 3: Arbitrage Pricing Model

$$R = E + b_1 f_1 + b_2 f_2 + b_3 f_3 + b_4 f_4 + e$$

Where: $\boldsymbol{R}$ is the return on any asset; $\boldsymbol{E}$ is the expected return on the asset; $\boldsymbol{b_1}$, $\boldsymbol{b_2}$, $\boldsymbol{b_3}$ and $\boldsymbol{b_4}$ are the sensitivities of the four factors listed above; $\boldsymbol{f_1}$, $\boldsymbol{f_2}$, $\boldsymbol{f_3}$ and $\boldsymbol{f_4}$ are the movements in the factors listed above; and e is the return due to unsystematic, idiosyncratic factors.

Stephen Ross noted that it was the tractability of CAPM that ensured its popularity, despite the restrictiveness of its assumptions. On theoretical grounds it is difficult to justify the assumption of normality of returns or of quadratic preferences to guarantee market efficiency. In his 1976 paper, Ross derives theorems and explores corollaries for the model he earlier introduced, based on some supporting conditions, to bolster the theory. He showed that the model does not require identical expectations, but notes that arbitrage theory still requires *essentially* identical expectations and agreement on the beta coefficients. Ross stated that for this assumption to be fundamentally weakened, closer examination of the dynamics by which ex ante beliefs are transformed into ex post observations is required. Ross also showed that unlike the mean-variance framework of CAPM, APT holds not only in equilibrium, but in “all but the most profound sort of disequilibria” (Ross, 1976, p.343).

APT does not specify the sources of systematic risk a priori. Consequently, empirical studies concerning APT have typically involved the construction of reference portfolios designed to mimic these factors. These constructed portfolios have provided the basis for exploratory studies designed to determine the macroeconomic variables underlying asset pricing relations. It has been noted that there is “an embarrassingly large number of ways to construct such basis portfolios” (Lehman, 1985, p.2). The logic behind APT is simple. Because the systematic factors are the primary sources of risk, it follows that they are the primary determinants of expected and actual returns on portfolios. But, unlike CAPM, this is not because of a direct relationship between risk and return. Instead, it is because two portfolios with the same sensitivity to each systematic factor are considered very close substitutes, so that they must offer the investor the same expected return, else arbitrage would occur to make this the case. The idea harks back to the *law of one price*, which states that if there are two ways to purchase the same cashflow, they must have the same price (Modigliani & Miller, 1958).

Robert Jarrow and Andrew Rudd established a set of conditions for which CAPM and APT are equivalent (Jarrow & Rudd, 1982). First, they lay out the assumptions used to derive each model. These are, for CAPM and APT respectively:

A1: Frictionless markets;

A2: Investors have a von Neumann-Morgenstern preference function for portfolio returns which is continuous and monotone increasing;

A3: There exists a riskless asset with rate of return r;

A4: Investors are risk averse, and satisfy the mean-variance criterion; and

A5: Investors have homogenous expectations.

B1: There exists at least one asset with limited liability;

B2: There exists a riskless asset with rate of return r;

B3: All investors are risk averse; and

B4: There exists at least one investor who (i) has a coefficient of relative risk aversion which is uniformly bounded, (ii) is not asymptotically negligible, and (iii) believes that security returns are generated by the single factor model:

$$X_i = E[X_i] + b_{if} + u_i, \text{ where}$$

$$E[u_i] = 0;\ E[u_i, u_j] = 0\ if\ i \neq j; E[u_i, u_j] = \sigma_i^2 \leq \sigma_{max}^2 < \infty\ if\ i = j$$

The authors show that the assumptions of both models are consistent, and both can hold simultaneously. Three more assumptions are added in order to make the limiting form of CAPM meaningful: **C1** ensures that the market portfolio requires a risk premium; **C2** uniformly bounds the size of an asset's beta; and **C3** states that the factor return is uncorrelated with the specific returns of every asset. The authors conclude that:

> "There are two conditions for the single factor APT and 'simple' CAPM to be asymptotically equivalent; namely, the factor must be uncorrelated with the residuals and the market must be well diversified" (Jarrow & Rudd, 1982, p.303).

Gur Huberman defined the notion of arbitrage introduced by Stephen Ross and formalised the concept to prove that "no arbitrage" implies Ross' linear-like relation among mean returns and covariances. He then justifies the no arbitrage assumption in an equilibrated economy of von Neumann-Morgenstern expected utility maximisers. He concludes that "one needs to make assumptions on agents' preferences in order to relate existence of equilibria to absence of arbitrage" (Huberman, 1982, p.190).

The *no arbitrage* condition that the APT has been derived from has become known as the Fundamental Theorem of Finance[7]. It is one of the pillars supporting the modern theory of Mathematical Finance. The economic interpretation is that by betting on the process **S** without bearing any risk, it should not be possible to make something out of nothing. The canonical example of an arbitrage opportunity is the ability to borrow at one rate of interest and lend at a higher rate. If this were possible, individuals would look to implement the strategy at unlimited scale. In doing so, simple logic tells us that arbitrageurs would drive the rates together. Further, if there is an arbitrage opportunity, demand and supply for the assets involved will be infinite, which is inconsistent with equilibrium. The Fundamental Theorem of Finance states that:

The following two statements are *essentially* equivalent for a model S of a financial market:

(i) S does not allow for arbitrage (NA)

(ii) There exists a probability measure Q on the underlying probability space (Ω, F, P), which is equivalent to P and under which the process is a martingale (EMM).

The central tenet of the Fundamental Theorem is that the absence of arbitrage is equivalent to the existence of a positive linear pricing operator and the existence of an optimum for some agent who prefers more to less. A related important result is known as the Pricing Rule Representation Theory, which asserts that a positive linear pricing rule can be represented as using state prices, risk-neutral expectations, or a state-price density, with different equivalent representations being useful in different contexts (Dybvig & Ross, 2003, p.2). The Fundamental Theorem has its roots in the work of Fisher Black, Myron Scholes and Robert Merton (Black & Scholes, 1973; Merton, 1973b) who considered a model of geometrical Brownian motion proposed by Paul Samuelson (Samuelson, 1965b), and built on the idea of *spanning* a market by forming linear combinations of primitive assets introduced by Kenneth Arrow (Arrow, 1964)[8]. This model is widely known today as the Black-Scholes model. The technique is simply to change the underlying measure P to an equivalent measure Q under which the discounted stock price process is a Martingale.

This makes it possible to use the extensive machinery of martingale theory. Derivatives are then priced by taking expectations with respect to the risk-neutral Martingale measure Q. This technique had been known for a long time (Bachelier, 1900), however what is important about the contributions of Black, Scholes, and Merton is that they linked this pricing technique with the concept of arbitrage. They showed that the pay-off function of an option can be precisely replicated by trading dynamically in the underlying stock. Unfortunately, the result rests on the assumption of “complete markets”, which is contravened by reality. The “complete markets paradigm” is a staple of both New Keynesian and New Classical theorists. The central premise of this paradigm is the extremely unrealistic proposition that:

> “there are markets for contingent claims trading that span all possible states of nature (all possible contingencies and outcomes), and in which intertemporal budget constraints are always satisfied by assumption, default, bankruptcy and insolvency are impossible” (Buiter, 2009, p.1).

To formalise the notion of arbitrage, in words, is to state that it is not possible to find a security which bears no risk, yields some gain with strictly positive probability, and is such that its price is less than or equal to zero. The first precise version of the Fundamental Theorem was established by Stephen Ross. His proof relied on the Hahn-Banach theorem (Ross, 1978a). There were several limitations which effectively reduced the scope of the derived results, which were subsequently addressed by other authors (Harrison & Kreps, 1979; Harrison & Pliska, 1981; Kreps, 1981). It was shown that under proper assumptions on the convexity and continuity of the preferences of agents, model extensions are valid, and it becomes a viable model of economic equilibrium. Harrison and Pliska proved, for finite probability spaces, the theorem that: the market model contains no arbitrage possibilities if and only if there is an equivalent martingale measure for S, thus formalising without the use of the word "essentially". Freddy Delbaen and Walter Schachermeyer have also derived a battery of theorems to address this issue (Delbaen & Schachermeyer, 1997).

Under the assumption of *complete markets*, the implicit state-price density is uniquely determined by investment opportunities and must be the same as viewed by all agents. This significantly simplifies the portfolio choice problem. The *optimal portfolio* is arrived at by solving first-order conditions for quantities. By solving these first-order conditions for prices, we have *asset pricing models*. Further, solving them for utilities provides *preferences*, and solving them for probabilities provides *beliefs*.

For the portfolio choice problem to have a solution, the price of each payoff pattern needs to be unique – *the Law of one Price* – and positive, to rule out arbitrage opportunities ("free lunches").

Stephen Ross has claimed that:

> "Supply, demand, and equilibrium are the catchwords of economics, but finance, or, if one is being fancy, financial economics, has its own distinct vocabulary. Unlike labor economics, for example, which specializes the methodology and econometrics of supply, demand, and economic theory to problems in labor markets, neoclassical finance is qualitatively different and methodologically distinct" (Ross, 2004, pp. 1-2).

However, Ross goes on to state that:

> "as a formal matter, the methodology of neoclassical finance can be fitted into the framework of supply and demand, depending on the issue" (Ross, 2004, p.2).

It is quite clear that neoclassical finance sits firmly within the methodological paradigm of neoclassical economics: explanations are propositions formally derived from statements on the behaviour of individual decision-makers.

7.3 Complexity Asset Pricing Models

In 1994, an article published jointly by Richard Palmer, Brian Arthur, John Holland, Blake LeBaron and Paul Taylor, titled: Artificial Economic Life: A Simple Model of a Stockmarket, appeared in the journal *Physica D*. This model has become known as The Santa Fe Artificial Stock Market (SFI-ASM). In this section, I argue that this model captures the spirit of mechanistic explanation. I undertake an examination of this model and its extensions. The key pieces of literature that I draw on to do so are: the original model (Palmer, et. al., 1994); the first extension of the model, which alters the pricing mechanism (Arthur, et al., 1997, LeBaron et al., 1999, Arthur, 2015); and various pieces by Blake LeBaron that include surveys of the literature on agent-based financial modelling, actual modelling exercises, and commentaries on the main issues encountered in building such models (LeBaron, 2000, 2001a, 2001b, 2001c; 2002a, 2002b). I also draw upon some key reference pieces, including sections from the Handbook of Computational Economics (Hommes, 2006;

LeBaron, 2006). Before examining the SFI-ASM, I will introduce several other complexity approaches to the explanation of financial market phenomena, which also seek to address similar concerns, based on similar criticisms of the mainstream paradigm.

7.3.1 Alternative Frameworks

Complexity Economics is a broad church. A number of recognisable approaches to explaining asset pricing phenomena can be grouped under this banner. While the primary focus of this sub-section is on the Santa Fe Artificial Stockmarket Model, I will first introduce a number of other prominent models that have been developed in response to dissatisfaction with the methodology practiced within the mainstream paradigm. In order, I will discuss the Adaptive Markets Hypothesis, the Fractal Markets Hypothesis, the Multifractal Model of Asset Returns, the Coherent Markets Hypothesis, and the New Kind of Science approach.

7.3.1.1 The Adaptive Markets Hypothesis

The Adaptive Markets Hypothesis (AMH) is a behavioural market hypothesis developed by Andy Lo (Lo, 2004; 2017). Lo argues that since economic systems are so much more complex than their physical counterparts, deduction based on a few fundamental postulates is likely to be far less successful. To this end, Lo recommends an alternative to the traditional deductive approach of neoclassical economics, based on the application of evolutionary principles coming out of the emerging discipline of *evolutionary psychology*.

This discipline builds on the principles of competition, reproduction, and natural selection to social interactions that was initiated by Edward Wilson (Wilson, 1975). In doing so, Lo claims to have fully reconciled the EMH with all of its behavioural alternatives. He claims that AMH is a "new version of EMH, derived from evolutionary principles".

Contrary to EMH, in which individuals maximise expected utility, AMH – taking its cue from Richard Dawkins (Dawkins, 1976) - views individuals as "organisms that have been honed, through natural selection, to maximize the survival of their genetic material", and subscribes to Herbert Simon's concept of bounded rationality and satisficing as an alternative to the hyper-rational agents of EMH. Lo claims that the problem with Simon's theory of bounded rationality is that it provides no indication as to how agents are to determine the point at which the operation of optimising behaviour has resulted in a satisfactory decision. In AMH, individuals make decisions based on past experience and their best guess of the optimal choice, and they learn by receiving positive or negative reinforcement from the outcomes. Under AMH, prices will reflect as much information as dictated by the combination of environmental conditions and the number of *species* within the *ecology* of the economy. Species' are distinct groups of market participants behaving in common fashion. Examples of different species include pension funds, retail investors, hedge funds and market makers. By interpreting economic profits as the ultimate food source upon which market participants depend for their survival, the dynamics of market interactions and financial innovation can be derived. Under the AMH, investment strategies experience cycles of profitability and loss in response to changing environmental (business) conditions, entry and exit of participants, and the type and magnitude of profit opportunities. The implications of AMH are to be derived through a combination of deductive and inductive inference, including theoretical analysis of evolutionary dynamics, empirical analysis of evolutionary forces in financial markets, and experimental analysis of decision making at the individual and group levels.

Lo documents what he believes to be four concrete implications of AMH (Lo, 2004, pp. 21-24). Firstly, to the extent that a relationship between risk and return exists, it will not be stable through time. An important corollary of this, is that the equity risk premium is time-varying and path-dependent. Secondly, contrary to EMH, arbitrage opportunities do exist within AMH. This implies more complex market dynamics including trends, cycles, bubbles, crashes and other phenomena characteristic of real life markets. Thirdly, the performance of different investment strategies will fluctuate through time in response to the environmental conditions. Fourthly, in AMH, survival is the only objective that matters; utility maximisation, profit maximisation and general equilibrium are subordinate to the organising principle of survival that determines the evolution of markets and financial technology.

David Nawrocki and Fred Viole note that:

> “the traditional approach uses mathematics to build financial theories. Unfortunately, mathematical models require boundary conditions (assumptions) in order to generate a closed form solution. The devil is in the assumptions – primarily the rational investors, symmetric information and no market costs assumptions. With those assumptions, we are able to generate beautiful closed form market models. Without those assumptions, we lose some of the simple beauty of mathematics but hopefully are able to derive a better understanding of markets. We still can use mathematics and statistics on closed form micro models while making fewer assumptions. But in the end, we have to give up the vision of a mathematical theory of everything promised by the traditional approach” (Nawrocki & Viole, 2014, p.3).

And as Andy Lo has pointed out:

> "Despite the fact that new theories of economic behaviour have been proposed from time to time, most graduate programs in economics and finance teach only one such theory: expected utility theory and rational expectations, and its corresponding extensions, e.g., portfolio optimization, the Capital Asset Pricing Model, and dynamic general equilibrium asset-pricing models. And it is only recently that departures from this theory are not rejected out of hand: less than a decade ago, manuscripts containing models of financial markets with arbitrage opportunities were routinely rejected from the top economics and finance journals, in some cases without even being sent out to referees for review." (Lo, 2004, p.10)

And in speaking upon how monolithic the mainstream paradigm is, he expresses hope that:

> "Perhaps over the next 30 years, the *Journal of Portfolio Management* will also bear witness to the relevance of the Adaptive Markets Hypothesis for financial markets and economics." (Lo, 2004, p.24)

Lo recognises other biologically inspired approaches to the explanation of financial market phenomena, including the application of complex adaptive systems, noting that they are "not yet part of the economic mainstream" (Lo, 2017, p.217).

Lo co-authored a paper with Doyne Farmer applying evolutionary arguments to the concept of market rationality (Farmer & Lo, 1999). Farmer, individually, published a paper documenting an extended analogy between financial markets and biological ecologies (Farmer, 2002), which builds upon the argument of Sandy Grossman and Josef Stiglitz that markets must exhibit some level of inefficiencies, to provide motivation for financial transactions. Farmer argues that these inefficiencies support a rich ecology of trading approaches.

While Lo criticises the economics profession for "physics envy" and considers a biologically inspired approach more suitable than a physics inspired one, Farmer, who commenced his professional career as a physicist, notes that economics bears no resemblance to the physics he knows. The problem according to Farmer is not an underlying desire to mimic the methods of physics, but the fact that economists have a conception of physics that is completely wrong. Farmer asserts that the root cause of problems in economics is:

> "An epistemology that does not employ the scientific method as it is used in other successful fields of science, in particular for accumulating empirical facts" (Farmer, 2013b,p.2).

And he extends on this in the following manner:

> "Although it is often said that economics is like too much like physics, to a physicist economics is not all all like physics. The difference is in the scientific methods of the two fields: theoretical economics uses a top down approach in which hypothesis and mathematical rigor come first and empirical confirmation comes second. Physics, in contrast, embraces the bottom up 'experimental philosophy' of Newton, in which 'hypothesis are inferred from phenomena, and afterward rendered general by induction'. Progress would accelerate if economics were to truly make empirical

> verification the ultimate arbiter of theories, which would force it to open up to alternative approaches." (Farmer, 2013a, p.377).

As Mauro Gallegati has pointed out, the union of axiomatization and non-falsifiability has led to the Lakatosian degeneration of the paradigm of mainstream economic theory (Gallegati & Kirman, 2012, p.6).

7.3.1.2 The Fractal Markets Hypothesis

Edgar Peters introduced the *Fractal Market Hypothesis* (FMH), in which markets are assumed to be comprised of heterogeneous traders with varying investment horizons, who react differently to inflowing information with respect to these horizons (Peters, 1991; 1994). A crucial notion of FMH is that if investment horizons are uniformly represented throughout the market, then supply and demand for assets will be met and markets will exhibit efficiency and stability. Where a particular investment horizon becomes dominant however, markets are not efficiently cleared and extreme events will happen. The model predicts that short investment horizons will dominate during periods of financial turbulence since: 1) long-term investors panic, start selling, and consequently short horizons dominate over long ones; and 2) long-term investors stay out of the market until the situation calms, marking short horizons dominant. The two key ideas that differentiate FMH from the more established market theories are: the role of liquidity; and the differential impact of information. Nicola Anderson and Joseph Noss have developed a quantitative model formalising the qualitative conjectures of the FMH (Anderson & Noss, 2013), and the theory has been tested on data for the global financial crisis of the late 2000s, and appears to characterise the period very well (Kristoufek, 2012; 2013). The implications of the model for policy guidance have also been discussed (Haldane, 2011).

7.3.1.3 The Multifractal Model of Asset Returns

Recall from above (see: Section 7.2.1), that Benoit Mandelbrot rejected the idea that asset returns follow a Brownian motion process. In response, he developed the *Multifractal Model of Asset Returns* (MMAR) (Mandelbrot, 1972; 1974; Mandelbrot, Fisher & Calvet, 1997; Calvet, Fisher & Mandelbrot, 1997). This model, as the name suggests, represents an application of fractal mathematics to financial economics. A *fractal* is an iteratively produced structure that exhibits the properties of self-similarity and scale invariance. In general terms, fractals display identical geometric patterns regardless of the scale at which they are being viewed. Self-affinity, is a weaker property. A self-affine return series has the same distributional properties – after rescaling – when returns are measured at any frequency. Self-similarity is therefore a special form of self-affinity. The distinguishing feature of the multifractal model is multiscaling of the return distribution's moments under time-rescalings. The idea is that the distribution of price changes is the same under multifractal deformations of time. Trading time of the stock market expands and contracts in relation to clock time as action increases and decreases.

Mandelbrot lists ten "facts" that are either embedded into his theory or derivable from it, which he refers to as "heresies". These are (Mandelbrot, 2004, pp. 227-252):

1. *Markets are turbulent* - they exhibit the tell-tale empirical signatures: scaling – the parts scale up to resemble the whole; long-term dependence – events at a particular place and time impact all other events elsewhere and into the distant future; variations are well outside normal distribution expectations; changes are concentrated in clusters; and there are discontinuities;
2. *Markets are more risky than standard theory suggests* – turbulence implies higher rates of ruin. Wild swings are hard to predict, even harder to protect against, and harder still to profit from. The financial system is not a linear, continuous, rational machine as the standard paradigm assumes[9];
3. *Large gains and losses are concentrated in small packages of time* – meaning that market timing matters greatly. News events are not a long series of random events spread out over time as the orthodox model assumes;
4. *Prices often leap, not glide* – prices are discrete, they are not continuous as standard portfolio theory suggests. The works of Bachelier, Markowitz, Sharpe, Black-Scholes, etc., only work under the assumption of continuous price changes. Discontinuous price change movements imply higher risk than otherwise. Mandelbrot contends that this is the principal conceptual difference between economics and classical physics;
5. *Market time is flexible* – time is different for every investor. Each time-scale has its own kinds of risks. In terms of the statistical distribution of risks, they are the same across time periods under the multifractal model, *but*, price variation scales with time;

6. *All markets work alike* – the parameters in the MMAR representing market properties are consistent across markets and times, since all markets are subject to a spontaneous internal life based on the way humans organise themselves;
7. *Markets are inherently uncertain and bubbles are inevitable* – these are implications of scaling. Massive movements become more likely after large movements under Paretian distributions, so price changes are much wilder than expected under the standard Gaussian assumptions. Analysis of the implied conditional probabilities suggests that bubbles are inevitable;
8. *Markets are deceptive* – price patterns are mostly spurious;
9. *Forecasting prices may be futile, but forecasting volatility isn't* – data overwhelmingly shows that the magnitude of price changes depends on past changes. The market exhibits *dependence* without *correlation*; the *size* of price movements displays dependence, but the *direction* of movements does not;
10. *In financial markets, the idea of "value" has little value* – *value* is not a single number that can be rationally derived as a function of an information set. Even if it were, the highly dynamic nature of such a number would render the usage of it unpractical; the turbulence inherent in markets suggests that its usefulness is vastly overrated.

Fractal properties have been reported for individual stock returns and those of market indices, for commodity prices, inflation rates, and currency exchange rates (Barkoulas & Baum, 1996; Di Matteo, Aste & Dacorogna, 2005; Alvarez-Ramirez et al., 2002; Lee, 2005; Fisher, Calvet & Mandelbrot, 1997; Batten & Ellis, 2001; Calvet & Fisher, 2002; Fillol, 2003; Goddard & Onali, 2016). While most work in the field has focused on discovering evidence of fractality in return distributions by reporting estimates of the Hurst exponent, or by graphical analysis of returns data, John Goddard and Enrico Onali have developed hypothesis tests for evaluating the statistical significance of departures from normally distributed, independent, identically distributed returns assumed by mainstream asset pricing models (Goddard & Onali, 2012).

The MMAR has been tested on emerging markets stock exchanges and found to be "mostly superior" to other models (from the GARCH family). The superior performance of the MMAR is attributable to its incorporation of three important stylised facts that have been established as features of financial time series: firstly, fat tail return distributions are accommodated; secondly, long memory is incorporated via fractal Brownian motion; and thirdly, it includes the trading time property (it models the relationship between observed clock time and unobserved natural time measurements of the return process) (Gunay, 2016). It has been argued that the source of the multifractality observed in financial market time series' are the characteristic fat tailed distributions (Barunik, et al., 2012).

7.3.1.4 The Coherent Markets Hypothesis

The *Coherent Market Hypothesis* (CMH) is a model of stock price fluctuations based on a *Theory of Social Imitation* (Callen & Shapero, 1974), which is a nonlinear statistical model. CMH was constructed for the purpose of assisting in investment decision making through better market timing. This theory is essentially the Ising model of ferromagnetism applied to the social sciences[10]. The positive and negative polarisation of the iron molecules are translated into positive and negative sentiment. CMH was developed by Tonis Vaga as a statistical version of chaos theory (Vaga, 1990). CMH, like FMH, is based on the premise that markets shift between stable and unstable regimes. The random walk is a special case and represents the stable regime, while more ordered states emerge as "normal" behaviour transitions to "crowd behaviour". The Ising model includes two parameters: one for internal clustering and one for external forces. The coupling of the level of internal correlations and the strength of outside influences determines the state of the system. The external force is the economic environment, and the risk/return trade-off of the market is a combination of market sentiment and market fundamentals. The model thus represents the views of both technical and fundamental analysts. The market can be in one of four states depending on the particular combination of the two parameters. When the crowding parameter is low and aggregate fundamental views are neither bullish nor bearish, the market is in the *random walk* – EMH – state. For higher values of the crowding parameter, the market becomes *unstable* and the probability distribution becomes bimodal. When fundamental bias is low, the market is in the *chaotic* state, where large swings accompany small news events. Finally, when crowd behaviour and fundamental bias are strong, the market is in either *coherent bear* or *coherent bull* phase.

CMH has been implemented in a portfolio optimisation model. Testing of the model has suggested that it produces consistently higher returns than the market at a substantially lower level of risk, violating the tenets of the EMH (Steiner & Wittkemper, 1997).

7.3.1.5 The New Kind of Science Approach

Stephen Wolfram showed that simple systems can generate complexity. He castigates scientists for explicitly limiting their scope as a strategy to avoid contact with complexity, where the activity of description via mathematical equations inevitably fails (Wolfram, 2002, p.3). Wolfram claims that:

> "thinking in terms of simple programs will make it possible to construct a single, truly fundamental theory of physics, from which space, time, quantum mechanics and all the other known features of our universe will emerge." (Wolfram, 2002, p.4).

And specifically with regard to the social sciences, Wolfram rejects the assumption that theories must be formulated in terms of "numbers, equations and traditional mathematics", since such a method is not capable of capturing "fundamental mechanisms for phenomena". Wolfram's unified framework is built on a single crucial idea: that the rules for any system can be viewed as corresponding to a program, and its behaviour can be viewed as corresponding to a computation. That is, it is possible to think of any process that follows definite rules as a computation, regardless of the kinds of elements it involves, even though, in a sense, all the computation does it generate the behaviour of the system. Wolfram's innovative concepts of *computational equivalence* and *computational irreducibility* are underpinned by the works of Alan Turing and Stanislaw Ulam. The concept of cellular automata goes back originally to John von Neumann (von Neumann & Burks, 1966)[11]. Cellular automata models are a subset of agent-based modelling, and were the original means of implementing that approach (Macal & North, 2010, p.154). The Principle of Computational Equivalence (PCE) asserts that when viewed in computational terms, there is a fundamental equivalence between many different kinds of processes. In general terms, it states that almost all processes that are not obviously simple, can be viewed as computations of equivalent sophistication, and that there is essentially just one highest level of computational sophistication, which is achieved by almost all processes that do not seem obviously simple. And a fundamental implication of PCE is that among all possible systems with behaviour that is not simple, an overwhelming portion are *universal*. And to say that a particular system is universal, is to say that it is possible by choosing appropriate initial conditions to make the system perform computations of essentially any sophistication.

Wolfram identified the least complex type of cellular automata. Cellular automata have been developed as possible models of the basic structure of our world. Two properties thought to be universal rules of physics have been incorporated into the concept of cellular automata from the very beginning. Firstly, cellular automata behaviours are local; there are no nonlocal effects, and each effect on one cell is mediated only by its immediate neighbours. By way of contrast, traditional mathematical models almost always involve continuous quantities, and even the most basic arithmetic operations on continuous numbers typically involve significant non-locality. And secondly, effects are time-dependent in the manner of physical causality (Franke, 2013, p.6).

Wolfram tells us that a consequence of the PCE is that observers will tend to be computationally equivalent to the systems they observe, with the inevitable consequence that they will consider the behaviour of such systems complex. In this way, he explains the phenomenon of complexity.

Another principle, the *principle of computational irreducibility* (PCI), follows from PCE (Wolfram, 2002, pp. 737-750). And this principle relates directly to the conflicting methodological principles of deductive-nomological and mechanistic explanation. Wolfram states that the great historical achievements of theoretical science are extremely similar in their basic character. This shared characteristic, he states, is the derivation of a mathematical formula that allows one to determine the outcome of the evolution of a system without explicitly tracing its steps. So that even though the systems themselves generate their behaviour by proceeding through a whole series of steps, the process can be short-cut and the outcome discerned with much less effort. If the behaviour of a system is obviously simple, and can be characterised as either repetitive or nested, then it will always be computationally reducible. However, whenever computational irreducibility exists in a system, there can be no way to predict how the system will behave except by going through almost as many steps of computation as the evolution of the system itself. Wolfram summarises the situation thus:

> "So when computational irreducibility is present it is inevitable that the usual methods of traditional theoretical science will not work. And indeed I suspect the only reason that their failure has not been more obvious in the past is that theoretical science has typically tended to define its domain specifically in order to avoid phenomena that do not happen to be simple enough to be computationally reducible." (Wolfram, 2002, p.742).

Joost Joosten has postulated another principle: the *Generalised Natural Selection Principle* (GNS). This principle states that computational processes of high computational sophistication are more likely to maintain than processes of lower computational sophistication (Joosten, 2013, p.14). Joosten combines GNS with the Church-Turing thesis to show equivalence with PCE. This is meant to explain why universal complexity abounds in nature. A paper co-authored by Stuart Kauffman has argued that the nature of computational irreducibility varies across different types of phenomena. The authors suggest that the computational irreducibility of biological and social systems is distinguished from physical systems by the underlying processes of functional contingency, biological evolution, and individual variation. (Beckage, et. Al. 2013).

The deductive-nomological approach essentially assumes that all phenomena are computationally reducible, so that mathematical representation and derivation are explanatory. Whereas the mechanistic approach, which views mathematical explication as short-hand descriptions, explicitly promotes analysis via simulation of mechanisms (Bechtel, 2011; 2012; Bechtel & Abrahamsen, 2005; 2010). PCI leads to a fundamental problem of prediction. It implies that even in principle, if one has all the required information to discern the evolutionary behaviour of a system, it can take an irreducible amount of computational work to actually do so.

Vela Velupillai laments that these ideas have not had a greater impact in economics. He champions the inductive approach but hastens to add a word of caution about:

> "The mischief indulged in by economists, particularly those advocating blind agent-based modelling in economics and finance, claiming that their practice makes the case against formal mathematics in its deductive underpinnings, enhancing the case for a 'new mathematics' that is inductively based, shunts research toward pointless ends." (Velupillai, 2013, p.102).

Philip Maymin, who has argued that the only general way to determine the full effects of even simple systems is to simulate them, has shown that with just a single trader and a single asset, and only two internal states, complex security price series' can be generated. This model has become known as the *minimal model of financial complexity* (Maymin, 2011b). Maymin laments that even though such a strategy of simulation is perhaps the most useful for the field of finance, this is the field in which it has been least applied (Maymin, 2013). Stephen Wolfram had noted that most academic market-based explanations of the complexity of market systems ignore the vast amount of seeming randomness and focus instead on the small pockets of predictability. Wolfram promoted an alternative strategy aimed at directly modelling the randomness. He proposed a one-dimensional cellular automata model. Wolfram suggests that the randomness apparent in the markets is likely to be the consequence of internal dynamics rather than of external factors. Maymin shows that the simple trading rule can be expressed as a combination of four component rules: profit taking in bull markets; momentum in bear markets; buying on dips; and buying on recoveries. The simple rule replicates the high kurtosis, negative skewness and rich panoply of autocorrelations, characteristic of real markets. And it does it "so much better" than random walk models. Maymin claims that:

> "Mining the computational financial universe requires abandoning all preconceptions of what should or should not work and instead trying hundreds, thousands, millions of possibilities to see what does indeed work…The NKS approach to market-based finance requires overcoming enormous inertia to flip standard academic practice completely on its head" (Maymin, 2013, p.8).

Maymin, following Wolfram, promotes an algorithmic approach to modelling financial markets. Under the algorithmic hypothesis, which states that there is a rule-based component driving the market – as opposed to a purely stochastic one – the full toolset of the theory of algorithmic information can be applied. The resultant search for explanations proceeds inductively and is expected to help ground economics as an inductive science Hector Zenil and Jean-Paul Delahaye claim that:

> "One may well ask whether a theory which assumes that price movements follow an algorithmic trend ought not to be tested in the field to see whether it outperforms the current model. The truth is that the algorithmic hypothesis would easily outperform the current model…In our understanding, the profits attributed to the standard model are not really owed to the model as such, but rather to the mechanisms devised to control the risk-taking inspired by overconfidence that the model generates" (Zenil & Delahaye, 2011, p.460).

Maymin also shows that the cost of searching information sets for profitable strategies is an exponential task, so that as the amount of data gets large, at some point the aggregate ability to discover patterns is overwhelmed. The implication is that there should be positive excess returns for those who do find patterns, until those patterns become largely known (Maymin, 2011a). This accords with what has been found in the literature (Toth & Kertesz, 2006; Schwert, 2003), and with my own experience working in the global financial markets where quantitative fund managers often state that "if it has been published, it has already been arbitraged".

Maymin argues that based on the concept of *computational efficiency* in the field of computer science, markets cannot be *efficient*. The argument relies on the distinction between two classes of algorithms. One group of algorithms, classified as **P**, are those that can *find* a solution to an input of length **N** in a timeframe that is polynomial in **N** (P is short for Polynomial). Another group of algorithms, classified as **NP**, are those that can *verify* a proposed solution for an input of length **N** in a timeframe that is polynomial in **N** (NP stands for Nondeterministic Polynomial). P is a subset of NP, and so the issue becomes whether **P**=**NP**, which is an open question in computer science. Applying this to the issue of market efficiency, the conclusion is that markets cannot be efficient unless **P**=**NP**. Maymin cites research to show that the majority of financial academics believe in weak form efficiency (Doran, Peterson & Wright, 2007), while the majority of computer scientists believe that **P** does not equal **NP** (Gasarch, 2002), and that they cannot both be right: either **P**=**NP** and the markets are weakly efficient, or P does not equal **NP** and the markets are not weakly efficient.

7.3.2 The Santa Fe Artificial Stock Market Model

It has been stated that the SFI-ASM emerged from a discussion at the Santa Fe Institute in 1989 between, on the one side, Ramon Marimon and Thomas Sargent, who claimed that agents in a stock market simulation would quickly learn the neoclassical rational equilibrium solution, and John Holland and Brian Arthur on the other side, who did not agree with this proposition (Waldrop, 1992, p.270). For their part, Marimon and Sargent produced a piece of research that aimed to bolster their position by showing that artificial agents could solve a certain problem (Marimon, McGrattan & Sargent, 1990)[12]. On the other hand, Arthur and Holland decided to create their own artificial stock market model in order to assess the situation. The first version of their model was up and running by the end of 1989 (Ehrentreich, 2008, p.93) and published in 1994. The authors of the published paper concluded that:

> "Our model does not necessarily converge to an equilibrium, and can show bubbles, crashes, and continued high trading volume." (Palmer, et. al, 1994, p.264)

One of the primary goals in building the SFI-ASM was to study the dynamics around a well-studied equilibrium based on fundamental pricing. Three key aspects of this program are (Tesfatsion, 2012):

1. Examine whether the introduction of agent interactions and group learning helps to explain empirical observations;
2. In particular, does it help to explain well-documented "anomalies" (deviations from fundamental stock pricing)?; and
3. Stress on statistical characteristics of price and trading volume outcomes.

The original Santa Fe model replaced the representative agent approach underpinning traditional rational expectations models, with a heterogeneous agent approach. The model is carefully constructed so that for degenerate values of key parameter values, it collapses into the standard model. This provides a measure of validation, and expresses the early objective of exploring the dynamics of relatively traditional models. Subsequent models in this class have become increasingly complex, however the field is yet to grow beyond its infancy. The SFI-ASM was not designed as a single stand-alone explanation of financial market phenomena, but as a framework for the development of sets of explanatory models.

7.3.2.1 Preliminaries

Before introducing the SFI-ASM, I'll briefly address two issues that provide context for the model. Firstly, the rationale for rejection of the representative agent approach will be presented. Secondly, the case for modelling with artificial adaptive agents is outlined.

Alan Kirman provides the rationale for rejection of the representative agent approach (Kirman, 1992). First, he notes that the approach suffers from the fallacy of composition. There are no results on the relationship between the properties of individual and aggregate demand, so that one cannot simply assume that the whole economy behaves in the same way as a single individual. And if one were to impose restrictions on the behaviour of individuals to force analogous behaviour at the aggregate level, these would be so artificial that "few economists would consider them plausible". Therefore, it is essential to directly model the interactions between different individuals that result in aggregate activity. It has long been recognised that macroeconomic phenomena suffer from the fallacy of composition (Gorman, 1953; Green, 1964; Stoker, 1984; Caballero, 1992; Hartley, 1997).

But we also have another problem, as mentioned in Section 7.2.3 above. If we have a single representative agent, when prices become out of equilibrium, by what process could the arbitrage trading required to restore equilibrium take place? It is blatantly contradictory to posit a single representative agent and then assume that different individual actions will return the market back to equilibrium. As Kirman points out, noting a discussion by Joseph Stiglitz (Stiglitz, 1989), in this case the representative agent approach is used to provide the model with the *stability* and *uniqueness* of equilibria that are not guaranteed by the underlying model. In fact, various results that were initiated in the 1970s show that conditions implied by assumptions on individuals to guarantee uniqueness and stability *do not exist* (Sonnenchein, 1972; Debreu, 1974; Mantel, 1976; Kirman & Koch, 1986; Grandmont, 1992). Kirman thus refers to the program of supplying microfoundations for macroeconomics using a representative agent approach as “pseudo-microfoundations”. He points out that tests of representative agent models are joint hypothesis tests: there is the test of a particular behavioural hypothesis and the test that the choices of the aggregate can be described as the choices of a single utility-maximising agent.

Arthur Lewbel, in a paper exploring the assumptions regarding both functional forms of demands and distributions of agents that are required to make the demand equations arising from exact aggregation equal to those arising from utility maximisation by a representative agent, claimed that:

> “It is a fact that the use of a representative consumer assumption in most macro work is an illegitimate method of ignoring valid aggregation concerns. However, the representative consumer framework vastly simplifies a great deal of macro work and thought, and so is not likely to be abandoned.” (Lewbel, 1989, p.631).

Kirman’s conclusion is that:

> "it is clear that the 'representative' agent deserves a decent burial, as an approach to economic analysis that is not only primitive, but fundamentally erroneous" (Kirman, 1992, p.119).

It has often been argued that inefficiencies at the individual agent level will cancel out, creating rationality at the aggregate level consistent with the assumption of a fully rational representative agent. Paul Samuelson has suggested that the reverse is actually true: that rationality and efficiency are higher at the micro level than the macro level, and that:

> "…there is no persuasive evidence, either from economic history or avant garde theorizing, that MACRO MARKET INEFFICIENCY is trending toward extinction" (Samuelson, 1998, p.36; capitalisation in original).

Jung and Shiller performed a test of this hypothesis, analysing individual US stocks and an index of these stocks, finding evidence to support the idea that rationality is higher at the micro level than the macro level (Jung & Shiller, 2005).

Another concern with the representative agent approach as a means of appropriately grounding macro theory is that it wrongly characterises microeconomics as a monolithic enterprise (Hartley, 1997, Ch. 12). The approach is understandable given the explicit methodological commitment of deriving all economic theory from statements of individual behaviour, however, in practice things aren't so clear.

John Holland and John Miller explain the benefits of modelling with Artificial Adaptive Agents (AAA) (Holland & Miller, 1991). Firstly, they point out that purely linguistic descriptions, while infinitely flexible, often fail to be logically consistent, and on the other hand, mathematical descriptions trade off flexibility for consistent structure. The AAA approach is contrasted to these two, and is said to retain much of the flexibility of pure linguistic models while having precision and consistency enforced by a computer language. Kenneth Judd similarly points to a trade-off between deductive and inductive methods in economics. He notes that one can either achieve logical purity with deductive methods in low-dimensional models of the economy at the cost of sacrificing realism, or analyse more realistic high-dimensional models at the cost of numerical imprecision (Judd, 1997). Secondly, Holland and Miller argue that AAA provides an ideal framework within which to assess the "as-if" assumptions of traditional theory. That is, it is possible to test adaptive mechanisms driven by market forces to discern if the resultant behaviours of agents act as if they were optimising. AAA then can be used to analyse the conditions under which optimising behaviour can occur. Thirdly, it provides a useful benchmark for existing human experiments, since aspects such as utility, risk aversion, information, knowledge, expectations, and learning of agents can be carefully controlled and analysed. Fourthly, AAA models can be helpful in studying systems with either an absence or an abundance of analytical solutions. But the primary rationale of AAA models is that it is possible to endogenously produce emergent behaviours. In summary, the authors claim that:

> "Beyond complementing current theoretical and empirical work, AAA offer the potential for unique extensions of current theory. The mechanisms generating the global behavior of a complex system can be directly observed when the computer is an integral part of the theory. For such theories, the

> computer plays a role similar to the role the microscope plays for biology: It opens up new classes of questions and phenomena for investigation…More generally, the potential for the development of a general calculus of "adaptive mechanics" exists. A calculus of these systems would combine the advantages of analytic perspicacity with computer-driven hypothesis testing." (Holland & Miller, 1991, p.367).

Brian Arthur, Steven Durlauf and David Lane provide a list of six features of the economy which cause traditional linear mathematical models to fail when studying complex adaptive systems (Arthur, Durlauf & Lane, 1997, pp. 3-4). These are:

1. There are dispersed parallel interactions between many heterogeneous agents;

2. There is no global entity that controls the agent interactions. Instead of a centralised control mechanism, agents compete with each other and coordinate their actions;

3. The economic system has many hierarchical levels of organizations and interactions. Lower levels serve as building blocks for the next higher level;

4. Behaviours, strategies, and products are continuously adapted as agents learn and accumulate experience;

5. Perpetual novelty leads to new markets, new behaviours, and technologies. Niches emerge and are filled; and

6. The economy works far away from any optimum or global equilibrium with constant possibilities for improvement.

And because of these unsurmountable difficulties for traditional analytical methods, agent-based modelling with artificial agents provides a compelling alternative for studying complex adaptive systems and to develop explanations of their phenomena.

Unlike most traditional analytic models, agent-based simulations do not produce theorems and existence proofs. Instead, the approach usually generates time series' of state variables on both the agent and macro levels. To gain an understanding of the behaviour of the model, the generated time series' need to be analysed with econometric methods, and general conclusions are made by means of inductive reasoning (Ehrentreich, 2008, pp. 16-17). A major advantage of agent-based simulations of markets therefore, is that these models provide for the removal of restrictive assumptions that analytical models require for tractability. The agents in agent-based computational models are:

> "as free to act within their computational worlds as their empirical counterparts are within the real world" (LeBaron & Tesfatsion, 2008).

With the ascent of powerful and affordable microcomputers and the availability of huge economic data sets, the nascent field of computational economics has begun to grow rapidly. LeBaron and Tesfatsion list three requirements that must be met for agent-based models to facilitate understanding of a real-world macroeconomy. Firstly, an appropriate empirically based taxonomy of agents must be included. The taxonomy can be adjusted to the application rather than the application to the taxonomy. The data and methods for each agent type are to be based on available evidence from field studies, econometric studies, human-subject laboratory experiments, surveys, and interviews. It is a relatively easy exercise to successfully refine the taxonomy of a model. Secondly, the scale of the model must be suitable for the particular purpose it is designed for. The model should not be too simple. Between the extremes of *perfect competition*, which represents a large number of market participants, and *monopoly*, which involves a single seller, lies a vast region of *imperfect competition*, which is mostly unexplored, beyond a few cases such as *oligopoly* and *monopolistic competition*. And thirdly:

> "model specifications must be subject to empirical validation in an attempt to provide genuine insight into proximate and ultimate causal mechanisms" (LeBaron & Tesfatsion, 2008, p.246).

ACE models can contain a large number of parameters, and flexibility with regard to functional forms and learning mechanisms opens up the problem of degrees of freedom. Therefore, empirical validation of all aspects of ACE models is important.

Doyne Farmer, commenting on his success in using data mining methods to make money in the investment markets, made the comment that:

> "If I needed an investment strategy now, I would certainly use the empirical approach. But to really *understand* how markets work, we have to use agent-based models." (Farmer, 2001, p.70; italics mine)

And Rama Cont, continuing the theme, contends that:

> "…statistical analysis alone is not likely to provide a definite answer for the presence or absence of long-range dependence phenomenon in stock returns or volatility, unless economic mechanisms are proposed to understand the origin of such phenomena." (Cont, 2007, p.290)

Cont claims that agent-based models of financial markets are capable of providing such insights into the responsible mechanisms.

Others have also promoted the use of agent-based models, in addition to theoretical reasoning, as a means for understanding how markets function, and to evaluate the effectiveness of various stabilisation policies by explicitly incorporating a central authority into these models (Westerhoff, 2008; Westerhoff & Franke, 2012). Also, in the wake of the Global Financial Crisis, the OECD has commissioned research into agent-based models. One such piece of work provides recommendations on how to avoid future episodes of financial crises by improving transparency around creditor-debtor relationships between financial market participants (Thurner, 2011). This paper makes the point that in order to prevent future crises, it is crucial that work be done to *understand the mechanisms* through which various phenomena such as excessive leverage can cause system-wide issues. This is something that traditional general-equilibrium models are incapable achieving. Stefan Thurner states that:

> "The approach of many financial institutions, politicians and investors to risk management in the industry has been built upon such traditional equilibrium concepts, which have been proven to fail spectacularly when most needed." (Thurner, 2011, p. 6)

It is important to remember that the Santa Fe stock market model is not intended to be a single stand-alone model of stock markets. Instead, it was developed as a framework for constructing sets of explanatory models.

7.3.2.2 The Original Santa Fe Model

The Santa Fe model is motivated by the failure of the standard neoclassical model and rejection of the approach taken to develop extensions. All of these models are rejected on the basis they provide patently false representations of reality. The authors quote George Soros approvingly: “It may seem strange that a patently false theory should gain such widespread acceptance” (Soros, 1994), and reject the “behavioural noise trader literature on the basis of the falsity of its two core assumptions”: the existence of unintelligent noise traders who do not learn over time that their forecasts are erroneous; and the existence of rational investors who possess full knowledge of both the noise traders’ expectations and those of the homogeneous “rational” class of investors.

Heterogeneous agent models can be traced back to an early model by Christopher Zeeman. He introduced a stock market model consisting of both traditional REH agents, as well as a class of “noise traders” who follow rules based on information in past prices. Zeeman showed that such a market exhibits unstable behaviours analogous to those exhibited by real markets (Zeeman, 1974). Others, using an artificial stock market model, have attempted to show that aggregate rationality is an emergent property of a complex adaptive market system with irrational agents (Chen & Yeh, 2002). On the other hand, it has been shown that a small minority of irrational agents is sufficient to generate large deviations from aggregate rationality (Fehr & Tyran, 2005).

Many researchers have sought to undermine the evolutionary argument behind the traditional "as-if" modelling approach (Winter, 1964; Farrell, 1966; Blume & Easley, 1982; DeLong, et. al., 1991; Biais & Shadur, 2000), while the "as-if" modelling approach itself has been lambasted as unscientific (Conlisk, 1996; Sunder, 2003). Larry Blume and David Easley in particular, have shown that the conclusions of the evolutionary type of argument for rationality proposed by Ronald Fischer, Armen Alchian and Milton Friedman (Fisher, 1930; Alchian, 1950; Friedman, 1953) are unjustified. Fisher developed the *Fundamental Theorem of Natural Selection*, which formalised the notion of the survival of the fittest in evolutionary biology, with what have come to be known as "replicator equations". His selection equation has since been shown to be a special case of the Lotka-Volterra predator-prey equations (Schuster & Sigmund, 1983). Blume and Easley showed that while market selection favours profit maximising firms, the long-run behaviour of evolutionary market models is not consistent with equilibrium models based on the profit maximisation hypothesis (Blume & Easley, 2002). Others have also shown that the formalised evolutionary argument with replicator equations does not ensure emerging rationality (Lansing, Kremer & Smuts, 1998). Replicator dynamics suffer from the fact that they contain no mechanism for discovery; they represent a simple selection mechanism without mutation. Therefore, mutation equations were developed, resulting in a selection-mutation equation, also known as the Fisher-Eigen equation. These models are used in evolutionary game theory, since they allow for rigorous mathematic analysis.

Those who are not wedded to the strictures of mainstream methodology tend to utilise more flexible representative models incorporating genetic algorithms, classifier systems and neural networks, via simulation approaches. The canonical genetic algorithm was developed by John Holland (Holland, 1975), with many variants having been developed since (Eshelman, 1997; Goldberg & Richardson, 1989; Mitchell, 1996).

Another, experimental approach, has sought to provide an accumulating source of evidence against the rationality hypothesis based on psychological principles. One author of this approach states that:

> "Neoclassical economics and psychology have radically different views of the decision-making process. First, the primary focus of the psychologist is to understand the nature of these decision elements, how they are established and modified by experience, and how they determine values. The primary focus of economists is on the mapping from information inputs to choice. Preferences, or values, can be treated for most economic applications as primitives of the analysis, and the decision process as a black box. The aphorism "Economists know the price of everything and the value of nothing" correctly characterizes the discipline's scientific priorities." (McFadden, 1999, p.75).

And he goes on to explain further, that the psychological view of the decision-making process is "local, adaptive, learned, dependent on context, mutable, and influenced by complex interactions of perceptions, motives, attitudes, and affect". Whereas the standard economic model of rationality states that individuals act as-if information is processed to form perceptions and beliefs using strict Bayesian statistical principles – "perception-rationality", where preferences are primitive, consistent and immutable – "preference-rationality", and the cognitive process is straightforward preference maximisation, given constraints – "process-rationality". The standard rationality model has also suffered attacks upon its axiom base (Allais, 1953; Ellsberg, 1961; Kahneman & Tversky, 1979; Loomes & Taylor, 1992).

So, if agents are not modelled as perfectly rational; as knowing exactly what to do in any situation, then they will need to learn how to respond. Many approaches to modelling learning processes have been proposed and explored in recent times (Slembeck, 1999; Sobel, 2000; Brenner, 2004). Thomas Brenner has categorised these models into three broad categories based on their fields of origin: psychology-based models; rationality-based models; and models inspired by computer science and biology. The first category includes reinforcement learning models; the second category includes Bayesian and least-squares learning models; and the third category includes evolutionary algorithms and neural networks.

The core innovation in the SFI-ASM is the rejection of homogenous, deductively derived, correct expectations. Expectations in the SFI-ASM are endogenised through a process of realistic inductive expectation generation. It is argued that under heterogeneity, deductive logic leads to expectations that are not determinable:

> "…perfect rationality in the market cannot be well defined. Infinitely intelligent agents cannot form expectations in a determinate way" (Arthur, et. al, 2015, p.46).

This insight dates back to John Maynard Keynes, who, in a chapter discussing expectations formation, invoked the metaphor of a beauty contest. He famously remarked that:

> "…each competitor has to pick, not those faces which he himself finds prettiest, but those which he thinks likeliest to catch the fancy of the other competitors, all of whom are looking at the problem from the same point of view. It is not the case of choosing those which, to the best of one's judgement, are really the prettiest, nor even those the average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some I believe, who practice the fourth, fifth and higher degrees." (Keynes, 1936, Ch. 12)

While it has been proven that these belief hierarchies are theoretically "well behaved", common knowledge assumptions are required to force convergence to a well-defined set (Mertens & Zamir, 1985). This was why earlier, John Harsanyi had introduced the *common prior assumption.* This is a restriction on first-order beliefs that requires a joint prior, common to all agents, so that there is a common knowledge link between an agent's private valuation and their first-order belief. Higher-order beliefs are thus tamed, since for each agent they are also a common knowledge function of their own private valuations (Harsanyi, 1967). The problem with this strategy is that it implies that agents will not agree to disagree, so that no trading will take place after reallocation for risk sharing (Aumann, 1976); there will be no speculative trading (Miligrom & Stokey, 1982; Tirole, 1982). This is a clear contravention of reality (Shiller, 1995). It has been shown that tractability can be achieved without assuming common priors, however a common knowledge link is still required between an agent's private valuation and their first-order belief (Biais & Bossaerts, 1998).

Each agent in the SFI-ASM acts as a "market statistician", continually developing multiple explanatory "market hypothesis" of market price and dividend determination, through a process of learning and adaptation. These agents, who act by using inductive reasoning, are referred to as *inductively rational*. The multiple expectations models generated by each agent is said to "compete" and "survive" on the basis of their predictability.

The SFI-ASM takes a neoclassical structure, but departs from the standard model by assuming heterogeneous agents with expectations inductively generated in the manner just described. The "obvious" approach to modelling these inductive expectations – assuming a set of individual-agent expectations models sharing the same functional form, whose parameters are updated differently by each agent, starting from different priors – is rejected in favour of one that uses a genetic algorithm to select the "best hypothesis", because, unconstrained by a priori priors, this allows for individuality of strategies to evolve over time, and it:

> "will better mirror actual cognitive reasoning, in which different agents might well "cognize" different patterns and arrive at different forecasts from the same market data" (Arthur, et al., 2015, p.49).

A neural network model is also rejected on the basis that it would not assist in explaining expectations in terms of different classes of information. This genetic algorithm learning mechanism – a type of general evolutionary learning mechanism - was developed by John Holland, one of the authors of the paper, and has been used in both computer science and economics to study complex optimisation problems.

A second learning mechanism based on classifier systems - also developed by John Holland - is also incorporated into the SFI-ASM. The classifier system possesses the power to endogenously partition a stream of empirical information into states of the world. Agents' subjective expectational models are represented by sets of *predictors*. Each predictor is a *condition/forecast* rule. It contains two components: a market condition that may be fulfilled by the current market state; and a forecasting formula for the price and dividend at the next period. Each agent holds M of these predictors simultaneously and uses the most accurate of these that match the current state of the market. The genetic algorithm creates new predictors. It does this in one of two ways: by altering the values in the predictor array - *mutation*; or by combining a part of one predictor array with the complementary part of another – *crossover*. At each point in time, each agent selects the H most accurate predictors from those that match the current market state and produces a linear forecast for the next-period price and dividend. A standard neoclassical pricing model is used to translate these expectations into desired stock holdings and to generate bids and offers (Bray, 1982; Grossman & Stiglitz, 1980). Upon market clearing, the price and dividend are revealed and predictors are updated.

The authors list four primary advantages of using their selected model architecture. Firstly, different potential market dynamics are expressible within it. Secondly, it avoids the bias inherent in the selection of a particular functional form for expectations. Thirdly, learning is concentrated in the appropriate places. This is because predictors that represent actual market states often will be activated and operated on most often. And fourthly, it is possible to organise the descriptor bits into variously defined information sets to help understand how the information is being used and to see how new strategies can "emerge".

The SFI-ASM market structure is simple. There are only two assets: a risk-free bond in infinite supply paying a constant rate of interest, and a risky stock paying a stochastic dividend that follows an autoregressive process. The price of a share of the risky stock is determined endogenously in the market. The model is a partial-equilibrium one, since there is no market clearing restriction for the risk-free bond. Preferences are modelled as a simple constant absolute relative risk aversion (CARA) format for equity demand. The very first SFI-ASM model used an excess demand price adjustment mechanism. An updated version replaced this pricing mechanism with one predicated on market clearing. It is this version of the model that I refer to in this sub-section.

I'll now briefly document the model structure. I'll do so by explaining 10 key model equations (**Equations 4** through **13**). Firstly, the mean-reverting stochastic dividend process is given as:

Equation 4: Dividend Generation Process

$$d_{t+1} = \bar{d} + \rho\left(d_t - \bar{d}\right) + \epsilon_{t+1}$$

Where, $\bar{d}$ is the dividend mean; ρ is the speed of mean reversion; ϵ is a random shock normally distributed with zero mean and σ_ϵ^2 variance.

N agents are initially endowed with one unit of the risky stock and X units of the risk free bond. Each period, agents determine an amount to invest in the risky stock, with the residual to be invested in the risk free bond. Agents are homogenous with respect to their utility function. The shared CARA expected utility function is given as:

Equation 5: Expected Utility Function

$$U\left(W_{i,t+1}\right) = -e^{-\lambda W_{i,t+1}}$$

Where, λ is the risk-aversion parameter; and $W_{i,t+1}$ is agent i's expected wealth level next period. In determining their demand for the risky stock, only next period's expected return is considered by agents. Expected utility is maximised subject to a budget constraint. This is represented as:

Equation 6: Agent Optimisation Problem

$$W_{i,t+1} = x_{i,t}(p_{t+1} + d_{t+1}) + \left(1 + r_f\right)\left(W_{i,t} - p_t x_{i,t}\right)$$

Where, $x_{i,t}$ is the amount of the risky stock held by agent i in period t. Assuming normally distributed returns for the risky stock, the optimal demand, which is the desired holding, is determined as:

Equation 7: Equity Demand

$$\widehat{x_{i,t}} = \frac{E_{i,t}[p_{t+1} + d_{t+1} - p_t(1 + r)}{\lambda \sigma^2_{t,p+d}}$$

Where, $E_{i,t}[p_{t+1} + d_{t+1}]$ is agent i's expectation of the next period stock price and dividend, at time t and $\sigma^2_{t,P+d}$ is the observed empirical variance of the price plus dividend time series. The difference between an agent's desired and actual holdings determines their effective demand for the risk stock. Each agent submits both their effective demand and their partial derivative with respect to price to a market specialist. Solving for a fixed number of shares, the specialist sets a temporary market clearing equilibrium price by balancing effective demands. After the price is set, agents update their portfolios and trading volume is recorded.

Agents, while homogenous with respect to utility functions, are heterogeneous with respect to the processing of an identical information set. The following linear equation represents the determination of agent i's price and dividend forecast for the next period:

Equation 8: Returns Forecast

$$E_{t,i}[p_{t+1} + d_{t+1}] = a_{t,i,j}(p_t + d_t) + b_{t,i,j}$$

Where, $a_{t,i,j}$ and $b_{t,i,j}$are real-valued parameters of the predictor part of chosen trading rule j.

Individual agents in the SFI-ASM use condition-forecast classifiers to map market conditions into linear forecast parameters. The classifier rules are given by a bit-string and a parameter vector, an example of which is:

Equation 9: Classifier Rule

$$(0, \#, 1, \#; a_j, b_j, \sigma_j^2)$$

The bit-string in the first part of the classifier rule matches current market conditions: a "1" matches a true condition, a "0" matches a false condition, and a "#" – "don't care" symbol – matches either a true or false condition. The conditions used in the model are:

1. Price x Interest/Dividend > $1/4$

2. Price x Interest/Dividend > $1/2$
3. Price x Interest/Dividend > $3/4$
4. Price x Interest/Dividend > $7/8$
5. Price x Interest/Dividend > 1
6. Price x Interest/Dividend > $9/8$
7. Price > 5-Period MA
8. Price > 10-Period MA
9. Price > 100-Period MA
10. Price > 500-Period MA
11. On: 1
12. Off: 0

The conditions span three categories: fundamental conditions; technical conditions; and zero information conditions. The latter category is designed as a check to assess the relative importance attached by each agent to relevant and useless information. A process of *generalization* is applied by the classifier to condition rules that have not been used for a long time. This process changes a 0 or 1 value to #. It is possible for forecast rules to be created that could never be activated due to contradictions in the conditions. These rules will eventually be modified by the generalisation procedure.

Forecasting accuracy is the basis on which rules are evaluated and selected. They are updated by:

Equation 10: Forecast Accuracy

$$v_{t,i,j}^2 = \left(1 - \frac{1}{\theta}\right) v_{t-1,i,j}^2 + \frac{1}{\theta}[(p_t + d_t) - [a_{t,i,j}(p_{t-1} + d_{t-1}) + b_{t,i,j}]^2$$

The forecast accuracy is a weighted average of prior and current squared forecasting errors. The rule with the greatest forecast accuracy over the recent past is selected from the set of rules activated by the current market condition. θ is an open parameter. It determines the length of the time window over which agents estimate a rule's accuracy.

The fitness of each forecasting rule is determined by:

Equation 11: Rule Fitness

$$\Phi_{t,i,j} = C - \left(v_{t,i,j}^2 + bit\ cost\ x\ specificity\right)$$

C is a constant to ensure positive fitness values; $v_{t,i,j}^2$ – the rule forecast accuracy - is used as the variance estimate for the forecasting rule; specificity is the number of conditions in a rule that are not ignored; and bit cost is a cost associated with the use of each non-ignored condition. It was expected that non-# trading bits would only survive if they convey predictive ability, since the bit costs bias the distribution of bits toward all #s.

Agents use an additional GA learning procedure, which provides for alteration of the forecast rule set through replacement of poorly performing rules with new ones. This assists in the maintenance of a diverse population of strategies. The GA is invoked every K periods asynchronously by the individual agents. This K represents the learning speed of agents, and it has been designated as the most crucial parameter in the SFI-ASM (Ehrentreich, 2008, p.99). New trading rules are created via a process of either *mutation* or *crossover*. For mutation, one parent is chosen using tournament selection [13]. An identical offspring is created and its real-valued prediction parameters are mutated in one of three ways according to a probability schedule: uniform change to a value within a permissible range; uniform distribution within +/- X% of the current value; or it is left unchanged.

The condition bits in the condition part of the forecast rule are also mutated, according to a bit-transition matrix. For example:

Equation 12: Bit Transition Probabilities

$$P = \begin{array}{c c} & \begin{array}{ccc} 0 & 1 & \# \end{array} \\ \begin{array}{c} 0 \\ 1 \\ \# \end{array} & \begin{pmatrix} 0 & 1/3 & 2/3 \\ 1/3 & 0 & 2/3 \\ 1/3 & 1/3 & 1/3 \end{pmatrix} \end{array}$$

Mutation occurs with predictor mutation probability Γ. Crossover occurs with predictor mutation probability $1-\Gamma$. Crossover requires two genetic parents chosen via tournament selection. A uniform crossover procedure is undertaken on the condition parts of the selection rule, within which, with equal probability, a bit is selected from either parent and allocated to the offspring. For example:

Equation 13: Crossover Procedure for Condition Parts

Parent 1	**#**	**0**	**1**	**#**	**#**	**#**	**#**	**1**	**1**
Parent 2	**#**	**0**	**1**	**1**	**1**	**0**	**0**	**0**	**0**
Offspring	**#**	**0**	**1**	**#**	**1**	**#**	**0**	**1**	**0**

The real-valued forecast parameters are also subject to crossover. This is achieved in either one of three ways: both parameters are taken from a randomly selected parent; each parameter is selected from either one of the two parents; or an average of the two parents' values, weighted by $1/\sigma^2_{j,p+d}$ and normalised.

Now that the model has been introduced and documented, I'll briefly present some of the most important results and conclusions that were drawn from the original SFI-ASM. These results can be partitioned into two separate regimes, as per the design of the model: the *rational-expectations* regime and the *complex* regime.

The rational-expectations regime is defined as that in which agents continually explore expectations space, but at a low rate. The published results for the model show that under this regime the market price rapidly converges to the homogeneous rational expectations equilibrium, even though the agents started with "nonrational" expectations. The authors conclude that "homogeneous rational expectations are an attractor for a market with endogenous, inductive expectations" (Arthur, et al, 2015, p.53). This attractor is however weak, and differs from the theoretical rational expectations equilibrium in that it is neither assumed nor arrived at deductively, and the equilibrium is a stochastic one.

The complex regime is one in which more realistic values for the learning speed parameter are set. Under this regime, the market displays characteristics markedly different from those in the rational-expectations regime. It does not settle into any recognisable equilibrium. Firstly, there is systematic evidence of bubbles and crashes, which appears to be related to the use of technical trading rules by agents. Secondly, the statistical properties of the data series conform to real financial market phenomena, particularly with regard to excess kurtosis and heightened volume levels, reflecting continued heterogeneity of agent beliefs as the market evolves. Also, the time series' exhibit persistence of volatility and trading measures as well as cross-correlation between the measures. Perhaps the most important characteristic of the complex regime is the emergence and sustainability of technical trading strategies. Under the rational-expectations regime, those agents using strategies other than HREE are forced to discard them since the vast majority who are using strategies close to HREE drive the price toward the weak attractor, invalidating those non-HREE strategies. In the complex regime, however, exploration is high enough to offset the natural attraction to HREE for long enough that other strategies can gain a toehold and to co-evolve in a mutually sustainable way that is self-reinforcing. The authors suggest that a simple evolutionary argument is sufficient to explain the close correspondence between the results of the SFI-ASM and the empirical evidence accumulated from almost a century of empirical research. They conjecture that:

> "Both in real markets and in our artificial market, agents are constantly exploring and testing new expectations. Once in a while, randomly, more successful expectations will be discovered. Such expectations will change the market, and trigger further changes in expectations, so that small and large "avalanches" of change will cascade through the system…Changes then manifest in the form of increased volatility and increased volume." (Arthur, et al, 2015, pp. 58-59).

The simulation results show that autocorrelations increase as the predictor accuracy-updating parameter is increased, providing evidence for the conjecture. Experimental variation of the model parameters and the expectation-learning mechanism indicate that the qualitative phenomena of the complex regime are robust; they are not an artefact or deficiency of the model.

It's certainly true that the original SFI-ASM was designed to be as simple as possible (LeBaron, 2002): there are only two assets; the interest rate is an exogenously determined constant; the system is not calibrated to actual data; the time period is not specified - the model was designed to provide a qualitative comparison with stylised facts, not for quantitative calibration to actual time series; the only piece of fundamental information in the model is the dividend payment, which is paid each period; and the linear restriction on forecasting may not be appropriate. Further, Blake LeBaron observes that a fundamental criticism of the model is the equilibrium setting; real life markets are never truly in equilibrium. In the SFI-ASM some unspecified market institution somehow balances supply and demand. A number of these issues would subsequently be addressed by other researchers.

7.3.2.3 Evolution of the SFI-ASM

The authors note that in building this first generation artificial stock market model, they were not attempting absolute realism, but to show that given the inevitable inductive nature of expectations formation when heterogeneity is present, complex behaviour will emerge even under neoclassical conditions. They achieved their initial ambitions, and so others have created a progressive research program directed toward developing mechanistic explanations of financial market phenomena.

Blake LeBaron created a new version of the model that he refers to as "a second generation artificial market", which substantially increased its mechanistic credentials. First, the CARA preferences structure was replaced with an intertemporal constant relative risk aversion (CRRA) one, whereby wealth distributions determine relative price impacts for traders and strategies. This was an issue with the original SFI-ASM, wherein the wealth of individual agents does not figure in the calculation of demand for shares of the risky stock, so that individual traders have the same impact on prices, which is clearly at odds with reality. Secondly, fundamental movements were calibrated to actual financial time series, with the dividend sequence following a stochastic growth process calibrated to data from the US stock market. Thirdly, social learning was incorporated. Another major change was replacement of the classifier system with a neural network. In the original model, the classifier system was used to provide a means for easily determining which bits of information were being acted upon by individual agents. The neural network structure implemented by LeBaron was a very simple one, which was also designed to enhance tractability of the learned rules.

Blake LeBaron used this new model to examine the implications of agents with heterogeneous information frames, who select strategies from a common pool in order to form expectations (LeBaron, 2001c). He showed that the interaction of agents with different lengths of past information series' creates a market ecology in which it is difficult for the more stable longer horizon agents to dominate the market. The model replicates well-known features of real markets such as excess kurtosis, volatility persistence and volume/volatility cross-correlations, within a framework of restricted intertemporal preferences. LeBaron recognised, however, that the model shows too much weight in the tails of the returns distribution. He noted that while the model conforms to real data series in a manner far superior to analytical models, more research needed to be conducted to explore other heterogeneities in a model of increasing realism.

In another paper exploring the same issues, LeBaron shows that agent strategies become more homogenous near sharp price declines, suggesting that market crashes and excess volatility events are caused by liquidity issues (LeBaron, 2001d). In this model, agent information set horizons range from one year to twenty years, and strategies are selected from a pool of 250 active decision rules based on information on past returns, dividend yields, and two moving average technical indicators, which can be combined in any way. The investment decision-rules in this model represent investment advisors. As long as a strategy is in use, the investment advisor is considered to have clients, and it will continue to exist with no change to the same strategy. If an advisor has no clients, it will be removed from the market and replaced with another. The new advisor is created from the pool of active advisors with a genetic algorithm. The success of a strategy is determined purely by whether it is being used. Other measures of fitness, such as expected returns, are not appropriate, since they are not viewed by agents from a common perspective, due to them being evaluated over different time horizons. A Walrasian auctioneer is implemented via a numerical procedure which searches for a price that sets demand equal to the fixed supply. LeBaron noted that a consideration of the actual market microstructure would provide a significant improvement in realism. The paper highlights two key results. Firstly, it provides a counter-argument to those who propose evolutionary arguments to the effect that less rational agents will be driven out of the market (e.g., Friedman, 1953). Secondly, it questions whether such agents are actually "less rational", since the model shows that investors who take a long-term fundamental approach will find it difficult to go against the current market, since eventually, performance relative to others will induce changes toward shorter horizons, further adding to market instability and deviations from fundamental valuation. LeBaron remarked that the model easily captures a large number of empirical realities that analytical models are unable to account for, that it therefore provides a realistic alternative, and further, that it provides a platform for the advancement and testing of further hypothesis.

In another paper exploring how well agent-based models are able to account for the empirical facts provided by financial markets, LeBaron argues that even when traditional models fit some subset of the empirical data, it comes with the cost of "moving farther from economic believability" (LeBaron, 2006a, p.221).

LeBaron has also explored the interdependence between the issues of multiple time scales, stationarity and long memory that econometricians face, and which heterogeneous agent-based models are capable of exploring (LeBaron, 2006b). LeBaron highlights issues such as: how much past data should an agent use to make investment decisions? He points out that in order to understand the dynamics of financial markets, an approach that is capable of spanning multiple time frequencies is required. The traditional approach concentrates on a specific time frequency.

The SFI-ASM model has also been used to provide an explanation of why technical trading is so widespread in real world financial markets (Joshi, Parker & Bedau, 1998). It has also been used to show that frequent revision of forecasting rules in search of an optimum learning rate results in high levels of technical trading, and this creates "a negative externality in the market by causing positive-feedback and destabilizing prices, thus decreasing all traders' earnings"; the optimal global state is unstable, and technical trading drives the market into sub-optimal equilibria. The authors of this latter study conclude that what they observe is a typical prisoner's dilemma situation in which rational behaviour by individual agents drives aggregate behaviour that diverges from the optimal social outcome; the rational expectations regime is not reached (Joshi, Parker & Bedau, 2002).

Inspired by the SFI-ASM, other researchers created extensions of the model to both test the robustness of the model and to develop applications. Nicholas Tay and Scott Linn for example, with a minor adjustment to the prediction rule selection procedure introducing fuzzy logic - to simplify the agent reasoning process - show higher levels of kurtosis than the original model (Tay & Linn, 2001). Markus Wilpert tested a number of different modifications to the model, including changing the trading rule fitness criteria from forecast accuracy to generated profits, which had been suggested by others (Wilpert, 2004; Brock & Hommes, 1997). Laszlo Gulyas, Adamcsek Balazs and Arpad Kiss tested the model on an alternative platform, and created an extension in which real human subjects were incorporated [14]. The authors found that the results of the SFI-ASM replication were consistent with those published in the original model paper. They also concluded that:

> "blending the techniques of experimental economics and agent-based modelling can greatly assist in testing hypothesis about human economic behaviour, and about theoretical assumptions embedded in computational models" (Gulyas, Balazs & Kiss, 2003).

Thomas Stumpert, Detlef Seese and Malte Sunderkotter present results of a model based on the SFI-ASM. They show that a parsimonious nonlinear framework with an equilibrium model can replicate real-world stock market dynamics including phases of speculative bubbles and crashes (Stumpert, Seese & Sunderkotter, 2005). Haijun Yang and Shuheng Chen have added increasing realism of agent strategies to the SFI-ASM. They divide agents into four kinds in terms of different learning speeds, strategy sizes, utility functions, and level of intelligence, and show that their model replicates statistical features of real markets (Yang & Chen, 2018). Agent-based models of financial markets have also been used to explore the dynamics of heterogeneous agents with incomplete information (Chiarella, et. al, 2003).

Norman Ehrentreich programmed a Java version of the SFI-ASM and found excess kurtosis, volatility persistence, nonlinearities and heightened trading volume, in accordance with the original published results (Ehrentreich, 2008). Dramatically, however, Ehrentreich initially rejected the interpretation, promoted by the authors of the original SFI-ASM, that the model exhibits the emergence of technical trading. He attributed the result to a design flaw in the mutation operator within the genetic algorithm, which makes a zero-bit solution impossible. Instead, Ehrentreich's reconstructed model displayed behaviours where agents stopped using their classifier systems, indicating that the information contained in them provided no additional explanatory power, and that the only source of wealth accumulation within the model was the aggregate risk premium. The results of the Joshi, Parker and Bedau of 2002, in which they documented lock-in to a socially suboptimal equilibria – as mentioned above – were also observed by Ehrentreich.

However, Ehrentreich conducted a battery of tests on the components of the genetic algorithm and classifier mechanisms, and found that the issue of *genetic drift* had not been addressed. By examining the continuing relevant debates within population genetics (Suzuki, et. al, 1989; Harrison, et. al, 1988; Hedrick, 1999; Kimura, 1955; 1962; 1968; 1983; Ohta, 1973; 2002; Conner & Hartl, 2004), Ehrentreich applied the relevant principles to interpreting the SFI-ASM. A key result is that for selection to be the primary determinant of allele frequency, the condition: $S>1/(2N)$ must be satisfied. Also, he discovered that low levels of fitness above bit cost are not sufficient to guarantee fitness-based selection and propagation throughout the population. He concluded that:

> "In hindsight, the choice of initial parameter values made bit-neutral SFI agents particularly vulnerable to getting locked into the zero-bit solution through genetic drift" (Ehrentreich, 2004, p.158).

Since the trading rules were initialised with a bit probability of only 0.05, 95% of all trading bits were initiated as "don't care, # signs", random drift was much more likely to fix the # bits than propagate the 0 and 1 alleles throughout the population. For cases in which initial bit probabilities are set high, first, the generalisation procedure lowers the bit level, then genetic drift pushes it toward the zero-bit solution. Ehrentreich explained that the analogy between an agent's rule set and the sub-population concept in population genetics is not a contentious one. And in both cases, genetic drift clearly tends to create sub-population differentiation. So, the strategy of trying to detect bit usage by looking across all agents is not an effective one. It was shown that with higher initial allele frequencies, the fixation probability of trading bits increases, and once it has occurred, it is hard to reverse. But the purposes of the SFI-ASM is not to show that certain strategies would become fixated, but instead, to show that the stock market is a constantly co-evolving dynamic environment.

By replacing the original mutation operator with an unbiased operator, drastically different bit equilibrium distributions under no selection pressure were observed. These distributions are the result of the interaction of genetic drift, mutation, and crossover. Ehrentreich showed that while the new mutation operator is unbiased, it is more susceptible to genetic drift than the original operator, so that with low initial bit probabilities, the absorbing state at the zero-bit level is most likely to be achieved. So, Ehrentreich concludes that the problem with the original SFI-ASM is not in the model design after all, but in the interpretation of the simulation results. With prior knowledge of the biased mutation operator, additional tests are required for valid interpretations to be determined. By observing the actual fitness of the forecast rules used by classifier agents, and focusing on the best trading-rule per simulation run, it was shown that "SFI agents are able to produce significantly better trading rules than non-classifier agents" at high levels of statistical significance. And the dramatic final conclusion of all Ehrentreich's analysis is that it:

> "finally proves beyond a doubt that there is indeed technical trading in the model…If the authors of the original SFI-ASM would have performed such a fitness analysis to begin with, no questions would have arisen about whether the continued existence of trading bits reflects technical trading or not." (Ehrentreich, 2008, p. 175).

And as to the question of whether there is an increase in technical trading at higher learning speeds, the conclusion based on a battery of tests is:

> "Since this is significant at even the highest levels of confidence, we can finally establish the emergence of technical trading for faster learning speeds beyond a doubt." (Ehrentreich, 2008, p.177)

Does this suggest that simulation methods are perhaps not robust enough when compared to purely analytical methods? I believe not. For one, purely analytical procedures can also be difficult to definitively establish. Consider for instance, an example from physics. Lee Smolin has described how a key fundamental result in string theory is that it does not produce infinities in its solution space. It has commonly been accepted that this result was proved in 1992 by Stanley Mandelstam. However, Smolin, who himself has worked on string theory during his career and accepted this as fact, and indeed claims that it was this result that led him to work enthusiastically to develop the theory in the first place, tells of how, when preparing for a presentation on the major achievements of the field of quantum gravity, he discovered that there was actually no proof of finiteness (Smolin, 2006, pp. 278-282). While the proof of Mandelstam was recognised by mathematicians to be incomplete, Smolin discovered that out of fifteen review papers he consulted on the subject, the majority nonetheless claimed that the result had been proven. And what's more, he could not find anyone in the field who recognised that this was not the case. The closest to recognition he received was from a few well-known string theorists who claimed:

> "that they had proved the theory's finiteness decades ago and didn't publish only because of some technical issues that remained unresolved" (Smolin, 2006, p.281).

There will always be a trade-off between realism and tractability. As long as the source code is freely available, as it is in the case of the SFI-ASM, replication and robustness analysis can be adequately achieved. While, it has been remarked that in the early days of the approach, this necessary step was often neglected (Axelrod, 1989), professional platforms such as Repast and Storm are now available and widely used.

7.3.3 Summary

Unrestricted by the requirement of formally deducing propositions about inductively rational agents from fundamental economic theories, complexity economists have developed alternative frameworks for the construction of scientific explanations of financial market phenomena. The SFI-ASM in particular, rejects the "as-if" modelling approach characteristic of the economics mainstream. The main conclusion of the SFI-ASM is that markets where agents are learning, do not converge to traditional simple rational expectations equilibria. Instead, they evolve to some other steady state in which a rich set of trading strategies survive and evolve alongside one another. In this steady state, the market demonstrates the empirical signatures evident in most financial market time series.

7.4 Comparative Mechanistic Evaluation

Now that the asset pricing approaches of the mainstream and complexity paradigms have been explained, in this section, I will embark upon a relative mechanistic evaluation of these two approaches to the explanation of asset-pricing phenomena. I will do this with reference to the key mechanistic categories of *phenomena*, *entities*, *activities*, and *organisation*. It will be shown that the complexity economics framework fares much better than the alternative.

7.4.1 General Comments

I'll provide a few general comments pertaining to mechanistic credentials before addressing specific mechanistic categories.

Thomas Brenner has pointed out that orthodox economists aren't interested in studying the learning processes of agents themselves; they are only concerned with the consequences of these learning processes for economic behaviour (Brenner, 1999). It was noted in Chapter 3 (see: Section 3.4.2) that for the modern Austrian economist, following Ludwig von Mises, the scope of economic science also does not include the psychological bases of decision-making. However, in their case, this is not because of a dedication to "as-if" modelling. Instead, for the Austrians, economic science is concerned with the logical implications of *given* choices. From a mechanistic perspective however, this is not acceptable; insights need to be integrated across multi-level hierarchical mechanism schemas and constrained by findings across boundaries.

Doyne Farmer has argued that the failure of complex systems methods to be widely used in economics is a result of the "ironclad requirement that theories must have economic content" (Farmer, 2012, p.5). The requirement of "economic content", Farmer explains, is the requirement that for a set of statements to be considered a valid economic theory they must be derived from a statement which asserts that *selfish individuals maximise their preferences*. Farmer shows that the inevitable trade-off between "economic content" and "economic realism" precludes the application of complexity methods to economic problems, needlessly restricts what can be accomplished, and severely slows down progress in economics. Far too often, the baby is thrown out with the bathwater.

In Chapter 4 (see: Section 4.3.1), I showed how the debates on the foundations of mathematics influenced the methodological development of the logical positivist inspired mathematical economics movement in Vienna in the first few decades of the twentieth century. It is the victorious Bourbaki perspective that underpins the way that modern neoclassical asset-pricing theory is developed. The complexity model on the other hand is not beholden to such a top-down approach. Benoit Mandelbrot once observed that:

> "The study of chaos and fractals ought to provoke a discussion of the profound differences that exist…between the top down approach to knowledge and the various "bottom up" or self-organising approaches. The former tend to be built around one key principle or structure, that is, around a tool. And they rightly feel free to modify, narrow down, and clean up their own scope by excluding everything that fails to fit. The latter tend to organise themselves around a class of problems…The top down approach becomes typical of most parts of mathematics, after they have become mature and fully self-referential, and it finds its over-fulfilment and destructive caricature in Bourbaki. The serious issues were intellectual strategy, in mathematics and beyond, and raw political power. An obvious manifestation of intellectual strategy concerns "taste". For Bourbaki, the fields to encourage were few in number, and the fields to discourage or supress were many. They went so far as to exclude (in fact, though perhaps not in law) most of hard classical analysis. Also unworthy

> was most of sloppy science, including nearly everything of future relevance to chaos and fractals" (Mandelbrot, 1989, pp. 10-11).

This aptly describes the attitude adopted by mainstream economists, in which methods other than those practiced by their membership are actively frowned upon.

The equilibrium approach assumes a close connection between the price of a security and the "value" of the security. Information disseminated throughout the market is assumed to provide the basis on which investors push price values toward "true" values via trading. But as Benoit Mandelbrot has recognised (see: Section 7.3.1.3), given the extremely dynamic nature of the environment within which firms operate, the concept of "fundamental value" is so amorphous as to render it of little practical importance. The vast majority of investment strategies based on fundamental value are built on the view that there is some tendency for the market to move toward fundamental value; markets are weakly efficient, so that arbitrage profits can be generated: buy stocks when they have been inefficiently priced and sell them when they become efficiently priced. In contrast, the value strategy employed by Eugene Fama's investment management company - Dimensional Funds Advisors - views markets as strongly efficient, so that trading profits are gained by arbitraging differences in risk appetite through time: buy those stocks whose prices are low as a function of fundamental value because investors at this time have rationally increased their risk aversion, and selling those stocks whose prices are high as a function of fundamental value, when investors have rationally lowered their level of risk aversion. If Mandelbrot is correct, both of these justifications are misguided, since strong disequilibrium is permanent due to the dynamic nature of fundamental value, which nobody can consistently estimate.

7.4.2 Phenomenon

As shown in Section 7.2 above, much empirical work was conducted for the purpose of adequately characterising the phenomenon of market price fluctuations. However, when evidence became overwhelming that initial characterisations had been incorrect, the theories built on the basis of the characterisation were not discarded. Instead of admitting that EMH is invalid, since it is an explanation built on a faulty characterisation of the target phenomenon, Eugene Fama has stood firm, declaring that the theory has not been falsified. So instead of seeking to faithfully describe and explain the phenomena of the real world, mainstream economics seeks to rebuild the world in its own image (Davis, 2017). This has clearly not been the approach pursued by complexity economists. Instead, they have continually endeavoured to construct models capable of exhibiting the actually observed phenomena.

Rama Cont provides a set of stylised empirical facts that have been discovered through statistical analysis of price variation in several types of financial markets (Cont, 2001). These stylised facts represent a set of properties common across many instruments, markets and time periods, which have been observed in a number of independent studies. Cont argues that these stylised facts are so constraining that it is difficult to find an ad hoc stochastic process which possesses the same set of properties, and that one has to go to great lengths to reproduce them in a model. Cont lists eleven features that characterise the phenomenon of asset returns:

1. *Absence of Autocorrelations*: autocorrelations of asset returns are generally found to be insignificant, except for small intraday intervals that are thought to be related to market microstructure;
2. *Heavy Tails*: unconditional return distributions display a power law or Pareto-like tail. The tail index is finite, greater than two, and less than five, for the majority of datasets studied. This excludes the normal distribution;
3. *Gain/Loss Assymetry*: Drawdowns in stock prices and stock price indices are greater than upside movements;
4. *Aggregational Gaussianity*: The distribution of returns looks more like a normal distribution as the scale parameter over which returns are calculated increases;
5. *Intermittency*: at any time scale, returns display a high degree of variability, as quantified by the presence of irregular bursts in the time series' of volatility estimators;
6. *Volatility Clustering*: High volatility events tend to cluster in time, as quantified by positive autocorrelation of volatility measures;
7. *Conditional Heavy Tails*: After correcting for volatility clustering, the residual time series' still have heavy tails;
8. *Slow Decay of Autocorrelation in Absolute Terms*: As a function of the time lag, the autocorrelation function of absolute returns decays slowly, seemingly as a power law with exponent in the range of [0.2, 0.4];

9. *Leverage Effect*: Most volatility measures of an asset are negatively correlated with the returns of that asset;
10. *Volume/Volatility Correlation*: Trading volume is correlated with all measures of volatility; and
11. *Assymetry in Time Scales*: Course grained measures of volatility predict fine scale volatility better than the other way around.

Although the issue is not addressed by Cont, he notes that one very important question regarding these stylised facts is whether they can be used to "rule out certain modelling approaches used in economic theory". Any methodological approach to the explanation of financial market phenomena must be capable of constructing models that exhibit the observed phenomena. The discussion of the standard approach to modelling financial market phenomena in Section 7.2 above makes it clear that these models are incapable of capturing the stylised facts listed here. On the other hand, the modelling approaches associated with the complexity economics school of thought were shown in Section 7.3 above to be up to the task.

7.4.3 Entities

In order to satisfy the criteria of analytical tractability required for decductive theory generation and development, the dominant paradigm in financial market theory is forced to make highly unrealistic constraining assumptions regarding the fundamental elements of the economic system. Mechanistic explanation, however, requires faithful representation of mechanism entities. Complexity Economics approaches to explanatory modelling of financial market phenomena clearly conform to this stricture much more faithfully than does the mainstream paradigm. Firstly, the approach is dedicated to representing agents in their actual heterogeneity, unlike the traditional approach, in which agents are represented as being homogeneous. Real agents are heterogeneous across a vast number of dimensions, including time-horizons, preferences, information levels, and investment strategy. While the SFI-ASM introduced heterogeneity into asset pricing models in a relatively simple manner though differentiation of forecast-rule selection, in accordance with a desire to assess deviation from the standard model, other complexity-based approaches, as outlined above, have introduced further heterogeneities.

Blake LeBaron has remarked that the market mechanism in the SFI-ASF avoids the entire microstructure issue of the market, and suggests that "realistic modelling of market institutions and trading is an important extension that needs to be considered" (LeBaron, 2002, p.6). Research in this direction has been undertaken (Daniels, et al, 2002; Bouchard, Mezard & Potters, 2002; Zovko & Farmer, 2002; Smith, et. al, 2002; Slanina, 2008; Gould, et. al, 2013; Lehalle & Laruelle, 2018). Daniels et al were the first to show that the most basic properties of a market including the spread, liquidity, and volatility emerge naturally from properties of order flow.

In general, complexity models still only include a limited number of assets, and do not explain the dispersion in prices. Also, most models still assume that there is a single risk-free bond available to all, which pays a constant interest rate, and is in infinite supply. Extensions in these directions should be pursued.

Information is an exogenous entity under the standard paradigm, which is immediately available to all agents in the same form, to be interpreted in a uniform manner. In the ecology of real-world markets, information providers emerge as feeders on particular investment strategies. They survive by efficiently providing information that is required for specific strategies, in particular formats. This purchased information is by nature proprietary. Furthermore, many investment managers distinguish themselves by the type and quality of the information that they themselves are (supposedly) capable of generating.[15]. Early Complexity Economics models treated information *availability* in a similar way to the standard paradigm, but differentiated on the dimension of information *processing*. As mentioned above (see: Section 7.3.2.3), research in this direction has been pursued under the complexity banner, and the platform of agent based modelling under which the research is constructed provides a platform that is capable of much more.

7.4.4 Activities

The methodological commitments underpinning the mainstream paradigm force the modelling approach into unrealistic representations of the activities carried out by individual agents. Specifically, individuals are represented as making decisions on the basis of solving complicated optimisation problems, for which they possess all relevant information, and from which their derived expectations of future events match exactly what the relevant economic model predicts. This is not only unrealistic, but actually impossible. For those who promote the approach, this is generally of no concern, since they do not view this optimising activity as a behavioural assumption. Instead, as originally promoted by John Muth, and popularised by Robert Lucas, it is an "as-if" assumption, whose validity can only be assessed by means of predictive accuracy. This is a result of the methodology promoted and propagated by Milton Friedman (see: Section 4.4.1), and is evident from the comment made by William Sharpe above (see: Section 7.2.1), wherein he states that although the model assumptions are "undoubtedly unrealistic", they are nonetheless appropriate, since "the proper test of a theory is not the realism of its assumptions but the acceptability of its implications".

Not all researchers working within the paradigm have been so blasé however. Harold Working, for example, stated in his "anticipatory market mode" paper that:

> "The major problem in designing our model is to state appropriate specifications concerning the information and the quality of judgment employed by traders, and the manner in which they act. The specifications must be such as to permit deducing what sort of price fluctuations the model would generate; else the model will be of no use in the study of price fluctuations. Second, the specifications must not depart too much from reality; else the usefulness of the model will be impaired" (Working, 1958, p.192).

Working argued that the traditional model of a "perfect market", where all traders have both equal knowledge and equal ability to trade on that knowledge is unsatisfactory since it eliminates differences of opinion, which is the source of much trading in a real market.

The Complexity Economics approach is dedicated to realism; assumptions that are patently false have no place within their theoretical constructs. And in the cases where it is deemed necessary to incorporate false assumptions, the intention is always to progress the research program in a manner that moves in the direction of increasing realism. Agent activities are modelled with serious consideration to the empirical evidence and with reference to relevant theoretical insights stemming from the other sciences. In particular, insights from cognitive science, behavioural finance and experimental economics are combined with theoretical constructs originating in the physical, biological, and computer sciences, in order to faithfully represent the activities carried out by individual agents. In the SFI-ASM, agents follow a realistic process of inductive decision making through a process of learning. Under the neo-mechanistic model of explanation, possible activities are determined by entities and their properties; activities and entities are interdependent. A faithful rendition of entities is required for a proper characterisation of activities.

In real-world asset management, institutional fund managers typically follow a straight-forward process in developing investment strategies. Firstly, there is some factor that is believed to drive asset prices, for which there is some philosophical justification based on economic rationale. Secondly, there is an assertion that markets are sufficiently "rational" so as to drive prices toward those implied by the underlying causal factor, while remaining "irrational" enough to present opportunities for arbitrage in the first place. Thirdly, an investment process is designed as an implementation of the investment philosophy. Fourthly, back-tests are conducted to provide evidence that the strategy makes money; as an empirical validation of the investment philosophy. And the process will be adjusted based on the relative performance of various sub-measures. On the basis of these results, a product is built and taken to market. The key feature that I wish to highlight here is that investment strategies do not get off the ground unless there is some empirical evidence of their efficacy. And this evidence is discerned by evaluating forecasting rules on past data. Institutional asset managers do not act in a vacuum. In order to sell their products, they present them to investment consultants who evaluate these products and recommend them to their clients. And how do these asset consultants make their evaluations? Asset consultants are specialist evaluators of such products and are therefore well versed in the philosophical underpinnings of the various offerings available. Their decisions are made, to a large degree, on the basis of demonstrated track records for the investment strategies under consideration. Further, the fund management companies who prove the most successful in making money for their clients tend to find staff members leave to start their own boutique organisations, creating multiple mutant copies of the original investment strategy. Two key elements of the process I have just described are captured within the evolutionary learning processes incorporated within the SFI-ASM. Firstly, in real stock markets, agents make stock purchase decisions based on forecasting rules that have proved profitable in the past. And secondly, investment strategies are certainly not homogeneous. A vast array of differentiated investment processes exist based on alternative philosophical bases, and which utilise vastly different information sets. It is very common for investment managers to view the strategies implemented by others with scorn. Clearly, the complexity approach to modelling asset market prices is much more realistic in modelling these processes than the traditional paradigm.

7.4.5 Organisation

The way in which entities and activities are organised so that they comprise a functional mechanism is a key component of mechanistic explanation. Organisational issues are ignored in the standard approach to asset market modelling, where individuals are treated as isolated identical entities. On the other hand, as was shown above (see: Section 7.3 and Section 6.2.5), complexity economists are highly concerned with organisational issues.

The first version of the SFI-ASM used excess demand functions to determine price vectors. The first modification to the model replaced this procedure with a market clearing mechanism, enhancing the organisational features of the model. Likewise, the original SFI-ASM did not incorporate direct interaction between agents: agents learn and evolve based on feedback from the environment, but social learning is not possible since there is no rule-sharing between agents. As the research program has progressed, this has been rectified. Early models purposed with providing dynamical frameworks for agent interactions include (Lux, 1997; Kirman; 1991; Chiarella, 1992).

In real world markets, due to diversification benefits, it is standard practice to combine different investment strategies into a single portfolio. It is also well understood how different investment strategies interact with one another throughout market cycles. As a simple example, momentum strategies do well outside of market turning points, relative-value strategies do well as markets turn down from the top, and growth strategies do well as markets turn up from the bottom. Academic research has also explored this issue (Frankel & Froot, 1986; De Grauwe, Dewachter & Embrechts, 1993). Asset consultants understand that the market is an ecology populated by a number of different strategies competing for the money of investors. And this is what the SFI-ASM asserts and explicitly seeks to model.

7.4.6 Further Remarks

While the complexity economics approach to asset-pricing modelling has shown to be a promising, progressive research program, which appears to be successfully developing mechanistic explanations, the mainstream paradigm exhibits all the signs of degeneration. Eugene Fama has made the sensible claim that:

> "the market efficiency literature should be judged on how it improves our ability to describe the time-series and cross-section behavior of security return" (Fama, 1991, p.1576).

However, despite repeated empirical invalidation, and ridicule from those outside the mainstream paradigm, he has consistently stood firm in his defence of EMH, noting that:

> "a ubiquitous problem in time-series tests of market efficiency, with no clear solution, is that irrational bubbles in stock prices are indistinguishable from rational time-varying expected returns" (Fama, 1991, p.1581).

This is the same stance taken by classical economists in the face of the great depression, who argued that, in the face of steep increases in unemployment, there is no way to distinguish between genuine unemployment and time-varying leisure preference. Fama responds in the same way to evidence from volatility tests pioneered by Robert Shiller (Shiller, 1979; 1981) and Stephen LeRoy and Robert Porter (LeRoy & Porter, 1981), and summarised by John Cochrane (Cochrane, 1991). He concludes that the tests are not informative about market efficiency due to joint-hypothesis concerns, and that the results are most likely linked somehow to changing business conditions. He claims that "it now seems clear that volatility tests are another useful way to show that expected returns vary through time", and contends that "The volatility tests however, give no help on the central issue of whether the variation in expected returns is rational" (Fama, 1991, p.1586). In response to overwhelming evidence of the value anomaly, which is prominently featured in his models, Fama claims that:

> "To judge whether the forecast power of dividend yields is the result of rational variation in expected returns or irrational bubbles, other information must be used. As always, even with such information, the issue is ambiguous" (Fama, 1991, p.1583).

And in the aftermath of the global financial crisis, ardent supporters of the mainstream paradigm have clamoured to defend it. Robert Lucas for example, published an article in The Economist, arguing that the crisis did not invalidate orthodox theory (Lucas, 2009), and Burton Malkiel has defended the paradigm, also arguing that it is consistent with the GFC event (Malkiel, 2011). Eugene Fama has been as steadfast as anyone in the wake of the global financial crisis. In his address to the 65th Chartered Financial Analyst's Annual Conference, he expressed his opinion that:

> "I take a particularly contrary view on it. I don't think it was a financial disaster that caused an economic disaster. I think you can't reject the hypothesis that it was an economic disaster that caused the financial disaster." (Harrison, 2012).

Under EMH, news conveying information about an underlying economic reality is perfectly transmitted into asset price movements. So, according to Fama, disorderly asset markets are a symptom of an underlying disorderly economy.

I explained in Chapter 5 how dissatisfaction with mainstream economic models impelled the Chairman of Citigroup in 1987 to search for practical alternatives. Almost two years after the onset of the global financial crisis of 2007, the Chief Economist of the same institution expressed an eerily similar prognosis:

> "The Bank of England in 2007 faced the onset of the credit crunch with too much Robert Lucas, Michael Woodford and Robert Merton in its intellectual cupboard. A drastic but chaotic re-education took place and is continuing. I believe that the Bank has by now shed the conventional wisdom of the typical macroeconomics training of the past few decades. In its place is an intellectual potpourri of factoids, partial theories, empirical regularities without firm theoretical foundations, hunches, intuitions and half-developed insights. It is not much, but knowing that you know nothing is the beginning of wisdom." (Buiter, 2009)

This is a welcome development. A mechanistic framework for theoretical development can both make sense of this situation, as well as guide the process.

In 2016, Janet Yellen, who was head of the US Federal Reserve at the time, noted that new ways of thinking about economic phenomena had emerged in the wake of the Great Depression of the 1930. She went on to question why this has not happened since the financial crisis of 2007-2008 (Yellen, 2016). More positively however, mainstream institutions, including the European Central Bank (ECB), the International Monetary Fund

(IMF), the Organisation for Economic Co-operation and Development (OECD), and the National Bureau of Economic Research (NBER), have expressed increasing interested in finding alternatives (Holland & Black, 2018).

7.5 Conclusions

The primary conclusion of this chapter is that the Complexity Economics framework for developing explanations of asset pricing phenomena conforms to the normative requirements of the mechanistic model of scientific explanation, whereas the framework of the orthodox paradigm contravenes many of these norms. Further, it was shown that by failing to be adequately mechanistic, the orthodox paradigm has become a degenerative research program, while Complexity Economics, availing itself of non-formal tools in assistance of mechanistic explanation, has proven itself to be a progressive research program.

Conclusion

Throughout the annals of the history of economic thought the objective of being "scientific" in approach has required of economists that they take the epistemological basis of their scientific discipline seriously. Accordingly, since the inception of Economic Science as a distinct scientific discipline, economists have sought out philosophical expertise when devising appropriate methodological principles. This was documented in chapters 3 and 4. There has however, been an uncoupling between contemporary philosophy of science and contemporary methodology of economics over recent decades, as the new mechanistic philosophy has not penetrated economic circles. This was shown in chapters 2 and 4. Economic science therefore requires a methodological reorientation.

The Complexity Economics movement does however conform to contemporary standards. This was show in chapters 4 through 7. There is therefore a practical basis upon which to re-orient methodological practice within the economics profession: Mechanistic Complexity Economics. And I highly recommend that this opportunity be taken up.

Endnotes

Chapter 1

[1] – **Auguste Comte** – For discussions of Comte's views on scientific explanation, see: (Bourdeau, 2018; Guillan, 2016; Comte, 1798; 1853).

[2] – **Ernst Mach** – For discussions of Mach's views on scientific explanation, see: (Pojman, 2011; Marr, 2003).

[3] – **Hume on Causation** – See: (Hume, 1748) for Hume's constant conjunction theory of causation.

[4] – **Explanation of Laws** – The referral here is to the "notorious footnote 33", which appears in the version of the Hempel & Oppenheim paper in: (Hempel, 1965).

[5] – **Freakonomics** – See: http://freakonomics.com/. The book series currently extends to: (Levitt & Dubner, 2005; 2009; 2014; 2015)

[6] – **Pre-emption Problem** - For a discussion of the pre-emption problem, see: (Lewis 1973)

[7] – **Pre-emption Solutions** - The solutions he takes issue with are those proposed at: (Lewis, 2000) & (Woodward, 2003). This is argued at: (Strevens, 2003).

[8] – **Entanglement** – See: Strevens, 2008, p.242. The relation of entanglement is succinctly paraphrased by Stephan Hartmann and Jonah Schupbach: "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 & Schupbach, 2010)

[9] – **Frameworked Explanation** - 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.

[10] – **DM Circularity** - This objection is due to Strevens. See: (Strevens, 2006, p.15).

[11] – **Probability Calculation** – This is simply calculated by: (0.99)(0.90) + (0.80)(0.05) + (0.50)(0.05) = 0.956

Chapter 2

[1] – **New Mechanical Philosophy** – For discussions, see for example: (Bechtel, 2008; Glennan, 2017; Glennan & Illari, 2017).

[2] – **Manipulationist Criteria** – Craver argues his point with reference to: (Pearl, 2000; and Woodward, 2003).

[3] – **Decomposition and Localisation** – For an extensive treatment of these issues, supplemented with several examples drawn from the biological sciences, see: (Bechtel & Richardson, 2010)

[4] – **The Logic of discovery -** Prominent statements to this effect can be found in: (Popper, 1934; Carnap, 1935; Reichenbach, 1938). Norwood Hanson, however, lamented that the logic of discovery was neglected, arguing that discovery does not belong to the psychological or sociological sciences, but is more properly logical; explanatory reasoning is the basis for the logic of discovery (Hanson, 1958, p.1074). He states that: "More philosophers must venture into these unexplored regions in which the logical issues are often hidden by the specialist work of historians, psychologists, and the scientists themselves. We must attend as much to how scientific hypothesis are caught, as to how they are cooked." (Hanson, 1958, p.1089)

[5] – **Bridge Principles** – It hs been pointed out that: "Judging from the econometric literature, bridge principles seem to be principles that econometricians avoid at all costs." (Stigum, 2003, p.262)

Chapter 3

[1] - **Nassau Senior** - This is the standard line. It is also debatable. Salim Rashid, for example, finds that: “Already in the 1790s, however, Dugald Stewart can be shown to have taken a large step towards the axiomatization later achieved by Senior”. (Rashid, 1985, p.245).

[2] - **Adam Smith** – Although this has become the standard line, not everyone agrees. Schumpeter, for example, claimed that “the Wealth of Nations contained no really novel ideas”. And Rothbard claims that “Smith, far from being the founder of economics, was virtually the reverse. On the contrary, Smith actually took the sound, and almost fully developed proto-Austrian subjective value tradition, and tragically shunted economics on to a false path, a dead end from which the Austrians had to rescue economics a century later.” (Rothbard, 1995a, p.xi).

[3] Scott Gordon - These two books were: (Hollis & Nell, 1975) & (Rosenberg, 1976)

[4] The a priorists - I group these methodologists together under a single label following Blaug (1992), being fully aware of Caldwell’s criticism of this grouping (Caldwell, 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), a criticism that I also accept.

[5] Certainty of economic conclusions - Ricardo is quoted as saying in front of Parliament that some of the conclusions of economics are: “as certain as the principles of gravitation.” Quoted at: (Blaug, 1992, p.53)

[6] – **Robbins on Political Agendas** – He levelled this charge against both the Historical School and the Institutionalists

[7] - **Menger’s 2nd Book** – This book was eventually published in English as: Problems of Economics and Sociology, in 1963

[8] - **Schumpeter** – Hayek states that although much indebted to Bohm-Bawerk, Schumpeter “absorbed so many other influences (particularly that of the Lausanne School) that he cannot be wholly regarded as a member of this [Austrian] group.” (Hayek, 1992, p.51.) The Lausanne School was the heart of the German Historical School, and so Schumpeter is often claimed as a member of the *youngest* historical school (see: Section 3.5)

[9] – **Praxeology** – Mises initially used the term *Sociology*, but later changed terminology, as a heterogeneous group inspired by Auguste Comte began arguing under this banner for a change to the methodological framework of economics (Hulsmann, 2003, p.xvii).

[10] - **German Historical School** – Heath Pearson (Pearson, 1999) has claimed that there was no such thing as the German Historical School, since the themes pursued weren’t limited to economists located in Germany and that also, not all recognised stages of the school were opposed to the methods of the classical economists. Bruce Caldwell (Caldwell, 2001) has adequately countered Pearson's thesis.

[11] - **Dilthey** – Dilthey's methodological influence stretched far beyond the Historical school of economics. Krabbe tells us that: "Dilthey's epistemology and methodology have a permanent bearing upon theories and methods of social sciences. His influence was very pronounced on Husserl, the founder of phenomenology; Heidegger and Jaspers, the originators of modern existentialism; and Max Weber. Dilthey's influence, however strong during his life, increased after his death through publication of his collective works." (Krabbe, 1985, p.114)

[12] – **Max Weber** – Quoted in: (Krabbe, 1985, p.110)

Chapter 4

[1] – **Von Neumann** – von Neumann did not believe that formal analysis was all there was to economic science. He simply thought that the science needed to pass through all the stages that the more mature physical sciences had. In effect, he considered himself to be creating the foundations upon which the discipline would develop over the subsequent centuries. In the Introduction to the Theory of Games and Economic Behaviour, it is asserted: "Economists frequently point to much larger, more 'burning' questions, and brush everything aside which prevents them from making statements about these. The experience of more advanced sciences, for example physics, indicates that this impatience merely delays progress, including that of the treatment of the 'burning' questions. There is no reason to assume the existence of shortcuts." (von Neumann & Morgenstern, 1944, p.7). But, as has been argued by Salim Rashid, von Neumann's methodological commitments did evolve over time (Rashid, 1994), particularly with regard to the split between theoretical and experimental work. Von Neumann would eventually admit that: "As our first results are already rather paradoxical, early experimental tests became highly desirable. In the subsequent developments the incompleteness of the theory and the extreme mathematical difficulties and ambiguities which beset it gave the experimental approach an even more far-reaching and quite peculiar significance: Experiments became necessary in order to provide guidance for the mathematical development of the theory – which, had it been carried on 'more mathematico' would have presented overwhelming difficulties." (von Neumann, 1963, V, pp. 238-239)

[2] – **Hahn Quote** – Quoted in: (Hutchison, 1994, p.234)

[3] - **Frisch** – Quoted in: (Chipman, 1998, p.58)

[4] – **The Identification Problem** – For a definition and discussion of the problem, along with a recent literature review of proposed strategies, see: (Santeramo, 2015)

[5] - **Friedman: Symmetry thesis** - 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 (Wilber & 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.

[6] – **Alexandrova & Northcott: Economic Models** - Alexandrova & Northcott clarify that what they mean by economic models is: “the idealized rational choice models characteristic of contemporary mainstream economics” (Alexandrova & Northcott, 2013, p.266)

[7] – **Lawson: Positivism** - Lawson also argues that much of heterodox analysis is wedded to the DN methodology as well (Lawson, 2018).

Chapter 5

[1] – **Fontana: Methodological Consensus** – To establish her conclusion, Fontana refers to quotes in published works by Colander (Colander, 2003, p.8) and Waldrop (Waldrop, 1992, p.142), and to a personal interview with Kenneth Arrow in 2009. Personally, I do not find that the published papers reveal a consensus. In particular, I discern the papers by Holland (Holland, 1988) and Kaufman (Kaufman, 1988) to be quite subversive.

[2] – **Arthur: Heterodox Economist** – Arthur's early research included pioneering work on positive feedback effects in economics. See for example: (Arthur, 1983)

[3] – **General Equilibrium Analysis** - Lionello Punzo has remarked that: "what is commonly called the study of general equilibrium is neither very clear nor agreed upon. Is it an analysis, a theory, a sequence of models, a metatheory? Nevertheless, everybody seems to agree that it is the fundamental economic theory" (Punzo, 1991, pp. 2-3)

[4] – **Equilibrium: Old Physics** - Philip Mirowsky argues that since those who co-opted the framework of classical mechanics for the purposes of economic analysis did not understand the physics it was constructed to explain, the equilibrium concept in economic science has never been valid (Mirowsky, 1984).

[5] – **Inductive Rationality** - 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).

[6] – **Computation** – Agent-based modelling is enacted through computer simulations. For a discussion on the epistemological implications of simulation studies as scientific instruments, see: (Winsberg, 2010; DeLanda, 2011).

Chapter 6

[1] – **Meso Level** – Recognition of meso-level analysis has been traced back to Joseph Schumpeter (Dopfer, 2012). Kurt Dopfer has worked on the development of a micro-meso-macro framework or economics based on Schumpeter's approach (same paper). See also: (Barbera, 2012) for a discussion on the search for mechanisms at the meso-level in Analytic Sociology.

[2] – **Activities/Interactions 1** – Stuart Glennan had earlier defined 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 (Glennan, 2002, p.S344). Under this definition, the productive power of mechanisms derives from the interaction between parts, and does not reference the productive, unitary, processes performed by mechanism parts.

[3] – **Activities/Interactions 2** – This is not to say, however, that they do not also speak in interactionist terms. Examples of this type of statement also abound.

[4] – **Sociological Networks** – See, for example, the works of Robert Putnam on Social Capital (Putnam, 2000).

[5] – **Epstein Quote** – 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 that: "As an epistemological matter, it's 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). Under the mechanistic account, all this is of course is misplaced.

[6] – **Epstein: Constructive Empiricism** – Things get somewhat confused when, after appealing to van Fraassen'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 does not seem to fit with an account that is anti-realist and denies that explanation is a goal of the scientific enterprise.

[7] – **Heterogeneous DSGE Models** – This literature is still very young. In this newer version of the DSGE model, consumers face idiosyncratic shocks. Some early studies in this area include: (McKay, Nakamura & Steinsson, 2016); (Farhi & Werning, 2017); (Kaplan, Moll & Violante, 2018).

[8] – **DSGE Support** – This quote is from the unpublished manuscript of the paper eventually published as: (Christiano, Eichenbaum & Trabandt, 2018)

[9] – **Econophysics** – For a discussion on the origins and foundations of Econophysics, and the relation of programs such as ASHIA to the SFI approach, see: (Schinckus, 2018).

Chapter 7

[1] – Andrew Donald Roy – A. D. Roy published a paper in 1952 on mean-variance trade-off for a portfolio of correlated assets, the same year that Markowitz published his landmark paper (Roy, 1952). Markowitz has noted that Roy deserves an equal share of the honour of "father of modern portfolio theory" (Markowitz, 1999, p.5).

[2] - Mean-Variance – There are many ways to estimate ex-ante co-variances, yielding different predictions. And the resultant co-variance matrices are highly unstable. In more recent times, a number of quantitative investment managers have begun to abandon the approach for stock selection models. A substantial portion of fundamentals based investment managers have always eschewed the mean-variance "black-box" approach for a qualitative assessment of co-variance risk.

[3] – Sharpe & Lintner – William Sharpe and John Lintner developed their accounts independently. Sharpe's paper appeared in print after Lintner had sent his paper off to the printers (Lintner, 1965, p.13). The respective models were derived from different perspectives, with Sharpe approaching the problem from the perspective of individual investors choosing securities, and Lintner approaching it from the perspective of a corporation issuing stock.

[4] – Roberts - In the paper, Roberts claims that he has also analysed the series' over both shorter and longer periods than weekly intervals, which "worked fairly well" (Roberts, 1959, pp. 2-3)

[5] – Mandelbrot - Since the Gaussian distribution is a limiting case of the stable Paretian family of distributions, Mandelbrot's model can be considered a generalisation of Bachelier's model.

[6] - Equity Premium Puzzle - It has been said that: "The ink spilled on the equity premium puzzle would sink the titanic" (Cochrane, 2008, p.261).

[7] - No Arbitrage Condition - It is also referred to as the Fundamental Theorem of Asset Pricing.

[8] – Spanning - To say that all payoff patterns are *spanned* is to say that "each potential payoff pattern can be generated at some price by some portfolio of assets" (Dybvig & Ross, 2003, p.7).

[9] – **Markets are Risky** – Mandelbrot believes that this is the real solution to the equity risk premium puzzle: "Real investors know better than economists" (see: Section 7.2.4.8) (Mandelbrot, 2004, pp. 230-231).

[10] – **Ising Model** – The Ising model is the simplest theoretical description of ferromagnetism. It was invented by Wilhelm Lenz (Lenz, 1920), and solved for the one-dimensional case by Ernst Ising (Ising, 1925). For a recently published history of the model and its applications, see: (Ising, et al., 2017). For an earlier history, see: (Brush, 1967).

[11] – **Cellular Automata** – For a brief history of cellular automata, see: (Sarkar, 2000).

[12] – **Wicksell's Triangle** – They showed that their agents always converged upon the good with the lowest storage cost as a medium of exchange, in accordance with the neoclassical solution.

[13] - Tournament Selection - In the SFI-ASM, this refers to a process of randomly selecting two genetic individuals from the gene pool and choosing the fittest one.

[14] - Participatory Simulation - Experiments with a mixture of artificial and human agents are known as *participatory simulation*.

[15] – **Information** – An implication is that academics are limited in the information available to them in conducting their scientific research. Corporate researchers are at a distinct advantage.

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