Why Macroeconomic Orthodoxy Changes So Quickly: The Sociology of Scientific Knowledge and the Phillips Curve

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Summary

Macroeconomics moves fast. This thesis adopts a Sociology of Scientific Knowledge (SSK) perspective to explain why. In only twenty five years three different orthodox positions on the relationship between unemployment and inflation, known as the Phillips Curve, came to dominate the profession, only to decline subsequently. This research explores the role of politics in this rapid cycle of contest and closure.

The research illustrates how empirical and theoretical work in the Phillips Curve debate were configured to conform to the expectations of the analyst. Examination of several clusters of papers within the debate make explicit the dynamics by which regressions and theories were shaped to provide the results required of them.

Macroeconomics is shown to respond to the needs of economic policy making circles. A nuance of the relationship between macroeconomists and policy making, rooted in the role of objectivity in lending legitimacy to Liberal Democracy, means macroeconomists lack the autonomy to define and contest the problems their discipline addresses. This holds heavy implications when economic policy decision-makers experience heightened political pressure. In these instances the faster temporality of the political sphere is imported into macroeconomics, and, in the three cases examined here, the prevailing orthodoxy subsequently fell.

Drawing upon a literature survey and interviews with macroeconomists, including four Nobel Laureates, this research provides valuable insight into the social construction of macroeconomic knowledge.

1. Why Macroeconomic Orthodoxy Changes so Quickly: Introduction

1.1.1 Introduction

Macroeconomics moves fast. Between the late nineteen fifties and the late nineteen seventies three separate formulations of the relationship between unemployment and inflation became widely accepted amongst macroeconomists, only to fall away after ten to fifteen years. This cycle of contest and closure, known as the Phillips Curve debate, is far faster than that seen in many other sciences. This research conducted a literature survey and semi-structured interviews with nineteen macroeconomists in order to locate the Phillips Curve debate in its wider political context and explain why macroeconomic orthodoxy changes so quickly.

1.1.2 How Quickly does Macroeconomic Orthodoxy Change?

We need only look through the well known Science Studies text *The Golem* for examples of how typically lengthy the process of debate is in many sciences (Collins and Pinch 1998). This draws on original research on seven classic scientific controversies, analysed by leading sociologists of science. Of the seven case studies Pasteur's arguments on the origins of life were contested for fifty years, and the dissent only ceased when the critics pass away. The scientific acceptance of relativity took some seventy years; the dispute about the sex life of the whiptail lizard started in the 1970s and is still ongoing; the chemical transfer of memory debate lasted twenty five years; the explanation of the solar neutrinos some forty. The only two disputes that are shorter or of similar length to those witnessed in macroeconomics were those concerning gravitational radiation and cold fusion. These differ from the Phillips

Curve arguments because, as with the chemical transfer of memory, the new idea was defeated and a new orthodoxy was not established. Furthermore, all these examples only exhibit one controversy, surrounded by longer periods of consensus. These examples are not untypical of many sciences.

In macroeconomics, however, the original formulation of the Phillips Curve argument, the Phillips/Lipsey Phillips Curve, went from inception in 1958 to acceptance by around ninety percent of the profession in only four years. Yet five years later, in 1967, a radically different specification, the Friedman/Phelps Phillips Curve, would first appear and, within six years, completely dominate the profession. This dominance would also only be short lived, as by the early Nineteen Seventies the third incarnation of the Phillips Curve, the Rational Expectations Phillips Curve, would first appear. By the late Seventies and early Eighties it would be this specification that dominated, again only to fall shortly after. So in this period three different versions of the Phillips Curve would conquer the profession in twenty five years. This is why we consider macroeconomics to be fast moving, and it is the explanation of the processes underling the swiftness in this period that is adopted as the aim of this thesis.

1.1.3 Why Does the Speed of the Macroeconomic Cycle of Contest and Closure Matter?

Exploring the mechanisms underpinning the rapid cycle of contest and closure in macroeconomics has two central benefits. These are the implications for understanding governments' economic policy making decisions and the contribution to the wider Social Studies of Science literature.

Macroeconomics informs the major economic and often social policy decisions made by governments. These decisions influence the lives of all their citizens and many others beyond. Yet the science that these decisions are based upon is in continual flux. The rapid cycle of contest and closure means governments from decade to decade face radically different constructions of macroeconomic understanding. By exploring the cycle of contest and closure, this thesis makes explicit the mechanisms that underlie the shifts in macroeconomic orthodoxy, and illuminates the role of the policy making circle in shaping the macroeconomic knowledge it relies upon.

Many of the concepts used in this thesis are classic ideas from the Social Studies of Science. By applying them in the case of macroeconomics we can see how they operate in a specific and extreme context of a rapid cycle of contest and closure. This allows further insights into the functioning of the concepts themselves, and continues to add to the already rich Social Studies of Science literature. This is particularly the case as, as will be demonstrated in the following literature chapter, the Social Studies of Science has not addressed macroeconomics as passionately as it has other scientific disciplines, leaving the subject relatively under-researched.

1.2.1 Overview of the Thesis

Following this introduction there are ten chapters and two appendices in this thesis. This is the first chapter, which acts as a introduction. Chapter Two is a Literature Review addressing three sets of texts: those informing the theoretical position adopted in the thesis; comparable empirical studies from the Social Studies of Science; and contributions from the History and Methodology of Economic Thought literature. Chapter Three describes and justifies the methodological procedures used in conducting this research. Chapter Four outlines a simple story of the Phillips Curve debate, providing historical context and technical insight. This is followed by six data-led analytical chapters that are introduced below. After this, the final analytical chapter draws together the insights gained to create the overarching argument of the thesis. The first appendix contains two tables referenced in the methodology chapter. The second appendix contains brief biographic details of all the macroeconomists contacted and interviewed as part of the research.

The six data led analytical chapters can be divided into two sections in two different ways. The first way groups the first three chapters together as explorations of the day to day activities of macroeconomists, and the remaining three as considerations of the role of politics in macroeconomics. The second way the thesis can be divided is to group the first four chapters together as illustrations of Interpretative Flexibility in macroeconomics, and the remaining two as discussions of the forces that instigate changes in macroeconomic orthodoxy. The reasons for this will become obvious as we explore each chapter in turn.

1.2.2 Chapter Five: How Macroeconomists Use Data

The first data-led chapter explores how Interpretative Flexibility is manifest in the day to day empirical work of macroeconomists. This chapter draws mostly upon the literature search to illustrate how data are used in three clusters of papers. The chapter develops six categories of data handling and demonstrates how they are employed by macroeconomists to reduce the massive number of potential interpretations of a data-set into the one interpretation that best fits the macroeconomist's expectations. Comparative studies from the History of Economic Thought literature are discussed. It is concluded that, firstly, data alone cannot settle macroeconomic disputes, and secondly that the Interpretative Flexibility of data provides lubrication for the cycle of contest and closure.

1.2.3 Chapter Six: How Macroeconomists Use Theory

This chapter is closely related to its predecessor on data, and shares a similar format and conclusions. Here two clusters of academic papers are explored to demonstrate how Interpretative Flexibility also exists in the way macroeconomists construct their own theoretical models. The metaphor of building a conceptual machine is developed to shows how the available theoretical tools are configured in a form that produces the patterns of behaviour the analyst expects. The differences between Interpretative Flexibility in data and theory are made explicit, before the conclusion is reached that, like data, theory alone cannot settle macroeconomic disputes, and that the Interpretative Flexibility of theory also allays rigidity in the cycle of contest and closure. Examples from the History of Economic Thought literature are provided as supporting evidence.

1.2.4 Chapter Seven: How Macroeconomists Evaluate Theory and Data

We have already noted that this thesis can be divided in half in two ways. In the first of these divisions this chapter is the final consideration of Interpretative Flexibility. Indeed it is the final chapter in which Interpretative Flexibility is considered in its usual context. Here we explore how Interpretative Flexibility can be used to explain how macroeconomists judge the work of others. This chapter draws heavily upon interview data to demonstrate that by configuring their priorities in a favourable manner macroeconomists can undermine or even ignore the work of their colleagues should they choose. Again these decisions are shown to be made in the form that best supports their own expectations. This chapter also draws together the conclusions of the previous two chapters firmly to substantiate the importance of Interpretative Flexibility in macroeconomics. Finally the familiar conclusion is drawn that both data and theory either alone or combined cannot settle macroeconomic disputes. Furthermore the Interpretative Flexibility endemic in these processes adds suppleness to the cycle of contest and closure.

1.2.5 Chapter Eight: Political Interpretative Flexibility

This chapter is both the first chapter about the role of politics in the cycle of contest and closure and, in the second way of dividing the thesis in half, the last chapter exposing locations of Interpretative Flexibility in macroeconomics. Drawing upon interview data, the argument is made that the political connotations of the three incarnations of the Phillips Curve are flexible and available for negotiation. The conclusion that makes this chapter relevant to the discussion of the role of politics in macroeconomics is that Political Interpretative Flexibility problematises any analysis that attempts to link shifts in influence between political ideologies and the cycle of contest and closure. The second conclusion, that ties the chapter to the first three, is that Political Interpretative Flexibility lubricates the cycle of contest and closure by removing the rigidity to change of political allegiance.

1.2.6 Chapter Nine: Macroeconomists' Construction of Self as Value Neutral and the Legitimacy of Liberal Democracy

Here we explore the role played by macroeconomics in lending authority to liberal democratic politics. Interview data are used to demonstrate how macroeconomists negotiate the commitment to value neutrality necessary for this role and the inherent normative aspects of doing research of the kind illustrated in the previous four chapters. The argument is made that the value neutral construction of self has had two major impacts upon how macroeconomics operates. Firstly it shaped the widespread adoption of statistical techniques after the Great Depression of the 1930s. Literature from both the Social Studies of Science and the History of Economic Thought are discussed at length to substantiate this argument. Secondly it has led macroeconomists to dislocate themselves from the policy setting arena. The full implications of this last claim are the content of the following chapter.

1.2.7 Chapter Ten: Macroeconomics as a Problem Solving Discipline

This is the final data-led analytical chapter in the thesis. Here we explore the arguments of Thomas Kuhn (1970) that sciences are problem solving activities. The novelty of this analysis is that in macroeconomics these processes are performed against the backdrop of a dislocation between macroeconomists and the economic policy making circles in which the problems of their discipline are set. The argument is made that it is the nature of this relationship that drives the rapid cycle of contest and closure observed in the Phillips Curve debate.

1.2.8 Chapter Eleven: Why Macroeconomic Orthodoxy Changes so Quickly: Summary and Conclusion

All the data-led analytical chapters provide Sociologically important insights into the workings of macroeconomics when considered alone. However the thesis is best read as a continuing narrative commencing with the small scale day to day activities of macroeconomists and later relating them to the wider political culture that shapes them. This concise chapter summarises this narrative. Reversing the format followed by the thesis, the analysis starts with the widest political framework and progressively narrows down to locate the day to day actions of macroeconomists within it. This conclusion is not a repetition of the previous six chapters. Instead it reiterates the central arguments of each to demonstrate how they interrelate to provide a full explanation of the rapid macroeconomic cycle of contest and closure.

2. Literature Review

2.1.1 Introduction

This chapter locates this thesis within four areas of literature. The first area concerns the theoretical position adopted in this thesis. It begins by outlining this position, known as the Sociology of Scientific Knowledge (SSK), and then developing some of its history. This is followed by a commentary on the position adopted in relation to criticisms of SSK by other sub-fields within the Social Studies of Science literature; Actor-Network Theory and Discourse Analysis.

The remaining three areas of literature are chosen because to varying extents they cover the same empirical field as this thesis. The first of these are accounts from the Social Studies of Science that consider scientific debate in economics. It is demonstrated that these publications provide sociologically interesting conclusions on the social construction of economic knowledge, while providing ample room for further investigation. The second set of accounts are also from within the Social Studies of Science discipline, in this instance focusing on finance. It is argued that these publications are interesting in our context because they are good Social Studies of Science research, but not because there are any similarities between the fields of economics and finance. The third set of accounts are from the History of Economic Thought literature. It is argued that these publications cover a broad set of agendas and generally have goals sufficiently different from those presented here to limit their usefulness in this context. However there are also examples that are pertinent in this context and are discussed at length both in this literature review and later during the empirical chapters of the thesis.

2.2.1 Discussion of the Theoretical Position Adopted in this Research: The Sociology of Scientific Knowledge

The theoretical position adopted in this thesis is the Sociology of Scientific Knowledge (SSK), best represented by Collins (1981a, 1983a, 1992). In this section we discuss the theoretical canons that underpin this position, some of the historical antecedents it follows, and the position taken in relation to two critiques of SSK. We begin by outlining the methodological implications of Collins' work and their relation to this thesis. Historical precedents are introduced when they are needed to demonstrate a theoretical point. This section is intended to provide the reader with an understanding of the theoretical reasoning behind the goals and methods adopted in this thesis. It is to provide the theoretical argument that justifies the position adopted and subsequently the analytical decisions made.

2.2.2 The Empirical Programme of Relativism

The central methodological prescription of SSK is the Empirical Programme Of Relativity (EPOR) (Collins, 1981a, 1983a, 1992). Collins argues that studies of controversial science are the most fruitful studies of science. This is because it is during periods of disruption that the socio-cultural practices of scientific communities are most visible. This is so because the argument and negotiation amongst disagreeing scientists lays bare the value judgments and loyalties of researchers that in consensual science form part of the taken-for-granted world views. It is when these accepted norms and values of science are challenged and contested, and possibly re-conceptualised and renegotiated, that they are most available to sociological analysis. The Phillips Curve debate exhibited three periods of controversy and closure in a twenty five year period, and is subsequently well suited to the methods espoused by EPOR.

The programme has three stages. The first is the empirical documentation of the Interpretative Flexibility of data. This demonstrates the breadth of legitimate stances that can be taken by scientists in a debate. Analysis of this type appeared in Chapters Five, Six and Seven, and is an essential part of EPOR and writings in the SSK mould. As we shall see, without first demonstrating, in this instance, how macroeconomists negotiate their empirical and theoretical resources to open up Interpretative Flexibility the following two stages of EPOR have nothing to explain. Such a demonstration is an essential part of SSK research.

In the second stage of EPOR attention turns to how closure of the debate is achieved in spite of this Interpretative Flexibility. This is why the detailed exploration of the existence and form taken by Interpretative Flexibility in stage one is essential. It opens the subject matter to social analysis by showing that the empirical and theoretical resources available to scientists by itself cannot solve scientific disputes. Instead, sets of normative judgements are mobilised to find closure in a scientific debate. Inherently these judgements are informed by cultural values permeating the scientists social world. It is the task of the second stage of EPOR to show how these cultural values lead to the normative judgements used to resolve technical disputes. In the context of this thesis these sets of judgements have been mobilised each time the cycle of contest and closure completes. Mechanisms exploring these dynamics appear throughout the thesis.

Finally the third stage of EPOR relates the closure process to the wider social and political structures. In essence the cultural values operating in the localised setting of the scientific debate are located amongst, and related to, wider socio-cultural norms and values. Inevitably wider normative context shapes social action and perceptions in any social setting. It is the task of the third stage of EPOR to elucidate how this operates in scientific practice, in this case the Phillips Curve debate. Chapters Nine and Ten perform this role here.

The three stages of EPOR are not intended to be a chronological sequence to be followed rigidly. Instead they represent the progressive abstraction away from the localised traditional arena of science, that of the journal, the conference, and in many cases the laboratory, through the culture of the scientist and finally to wider societal influence.

2.2.3 Methodological Relativism

Central to the implementation of EPOR is methodological relativism. Relativism of this sort was first introduced into the sociological study of science by David Bloor (1973, 1976) in what is termed the Strong Programme of Sociology. To understand SSK it is first necessary to understand the strong programme.

Bloor's 1976 book, in conjunction with Barry Barnes' 1974 contribution, are the foundational texts in the position known as the Edinburgh school. Here historical studies are undertaken and frequently 'social interest' theories as described in Barnes (1977) are used to explain scientific disputes. One leading proponent of such theories is Steve Shapin, whose work is discussed in Chapter Eight (Shapin 1975, 1979). However, more important for us than the use of social interest theories is the Strong Programme within which they are operationalised. This is because interest theories have not influenced SSK in the same way as Bloor's contribution. Furthermore, as Bloor explains (Bloor 1984), although social interest theories are compatible with the tenets of the Strong Programme they are not the only form of analysis that is compatible. An acceptance of the Strong Programme does certainly not necessitate their employment.

Bloor's (1976) Strong Programme provides a set of theoretical, philosophical and methodological prescriptions that underpin his view of the sociology of science. These are formalised in four tenets for the social studies of science. Firstly the sociology must be causal. This implies emphasis be placed upon on the conditions that bring about states of scientific knowledge. Secondly it must be impartial, meaning explanation is required for both truth and falsity, both success and failure. Thirdly the programme requires symmetry of explanation. Subsequently the same types of cause must explain both truth and falsity. Finally the sociological analysis must be reflexive. The modes of explanation used to describe the science must be applicable to sociology itself.

The significance of these tenets becomes most apparent when compared to the dominant sociology of science that preceded it. Bloor (1976) argues that the various sociologists and historians of science would adopt some form of teleological model. He describes how practitioners using the teleological model consider the scientific method an epistemologically privileged set of techniques that operate as a continual process towards the discovery of truth. Given this, it is meaningful to argue a scientific debate was closed with a particular group becoming dominant because this group's arguments were correct, or at least the best available.

Under the teleological model in the majority of cases the correctness of the winning group is a suitable explanation of why they won the debate. The remaining cases are those where the debate was deemed to have been won by the 'wrong' argument, i.e. with the hindsight of modern science the analyst believes the debate was not settled in the correct way. Here a plethora of social explanations are offered. These are used to account for the 'incorrect' application of the scientific method. Social explanations are used to explain why the truth was not discovered. In this way social analysis is relegated to a sociology of error.

A central figure of the pre-Bloor orthodoxy was Robert Merton. Beginning with his doctoral thesis (Merton 1970, originally written 1938) that explored the rise of seventeenth century English natural science Merton established himself at the forefront of the Sociology of Science. He worked within a functionalist framework that shaped his research questions. To Merton the role of the Sociology of Science was to explain the characteristics of scientific practices that allowed them maintain a privileged social position. To this end he would explore social influence that impacted upon the trajectory or formation of scientific enterprises, but would not comment on the actually content of the scientific arguments. It is the neglect of social techniques in explaining this content that separates Merton's work from the contemporary equivalent in the Social Studies of Science and Technology.

The crucial distinction between the two positions is the autonomy of knowledge. The strong programme allows no room for truth to guide the process of its own discovery. The teleological/Mertonian model allows room for no other explanation. Bloor argues that neither construction can offer any definitive argument to topple the other. Both are logically coherent and defendable. Bloor simply argues that the strong programme is more useful as a tool to understand how science works in practice. To argue one group in a scientific controversy succeeded because they were correct does not allow room for any understanding of the culture in which the debate was placed, or how it was established that they were correct. The research presented in this thesis demonstrates that illuminating these cultural influences can and does provide interesting and authoritative conclusions, conclusions that are beyond the potential of the teleological model.

Collins' (1981a, 1983a, 1992) SSK represents the first three of Bloor's four tenets of the Strong Programme through his principle of methodological relativism. Causality, impartiality and symmetry lead the analytical procedure. They are manifest in the research process by the sociologists' alternation between competing constructions of reality, or Wittgensteinian 'forms-of-life' (Wittgenstein 1953, Winch 1958). Wittgenstein's ideas inform the central argument in SSK analysis that cultural context is integral to the meanings associated with acts and ideas (Collins 1984). The taken for granted form-of-life provides the framework of all understanding, and understandings can only be shared if aspects of these forms-of-life are also shared. Methodological relativism allows sociologists to immerse themselves firstly in one position in a scientific debate, i.e. one form-of-life, to understand the norms and values that underpin it. Then secondly they can repeat the procedure in the form-of-life of a competing position and form-of-life. By alternating between the competing positions the analyst can explore the differences and similarities between the accounts that furnish the analysis. This process of immersion is characterised by Collins as the attainment of participant comprehension, a notion discussed shortly.

Collins (1984) notes that the Wittgensteinian position is compatible with the work of Thomas Kuhn (1962). Kuhn uses the term paradigm to describe a dominant school of thought in any science. He argues that at any time there are potentially a number competing paradigms seeking the dominant position. During times of scientific revolution one paradigm is replaced by another in a process Kuhn terms a paradigm shift. The insight that made Kuhn's work stand-out for many was that these paradigm shifts do not represent progress in any absolute manner as indicated by the more traditional philosophies of science prevalent at the time (for example Popper 1963). Any particular paradigm cannot be judged as an improvement upon that which it replaced as there is no overarching criteria by they can both be judged. This is because each paradigm entails its own research questions, and deriving from them, their own set of criteria for judging success. Kuhn terms this the incommensurability of rival paradigms, and has clear parallels with the differing ascriptions of meaning between competing Wittgensteinian forms-of-life. It is through this incommensurability that Kuhn establishes his relativism.

The rather untidy presentation of the incommensurability thesis in Kuhn has been addressed by Gerald Doppelt (1978). He concurs that Kuhn's incommensurability suggests that paradigms are in some sense sufficiently different, disparate, and incongruous relative to one another so as to block the possibility of comparative evaluation with a single criterion. The novelty in Doppelt's work is his identification of four forms in which Kuhn empirically demonstrates incommensurability in his work. These are incommensurability of firstly, the scientific concepts or theoretical language, secondly, the observational data or mode of scientific perception, thirdly, the agenda of problems to be solved, and fourthly the criterion of adequacy for scientific explanation. Doppelt argues that Kuhn fails satisfactorily to delineate these concepts from one another. Kuhn's notions of paradigms and incommensurability are used in Chapter Ten, and emphasis is placed upon Doppelt's third and fourth locations of incommensurability. Kuhn's work is still influential today, as demonstrated by his obituaries (Edge *et al.* 1997).

By contrast Collins' relativism is entirely methodological (Collins 1981a, 1983a, 1992). It is not premised upon philosophical argumentation. Instead it is employed as a useful methodological tool for the study of competing scientific forms-of-life. By locating methodological relativism in EPOR, and combining it with the aforementioned participant comprehension, to be discussed shortly, Collins provides a practical methodology for SSK. The sociologist should alternate between the competing forms-of-life held by scientists, or in our case macroeconomists, who adopt competing knowledge claims (Collins 2004a). Doing so provides an understanding of the norms and values underlying each competing knowledge claim. The sociologist can then observe the contrasts and similarities between how researchers from each side of the debate construct the other.

As a final note we should make clear the distinction between methodological relativism and epistemological and ontological relativism. Epistemological relativism suggests that no external judgement can be made between two competing knowledge claims held by different social groups. Ontological relativism argues these groups are experiencing genuinely different realities. Methodological relativism does not venture into such philosophically profound territory, and does not require the acceptance of either as an implicit assumption in its operation. Instead it is a simple methodological tool that has demonstrated its own usefulness. It is on these grounds that it is accepted here.

2.2.4 Participant Comprehension

A final element bound to EPOR, and employed centrally in this thesis, is participant comprehension (Collins, 1984). Here the sociologist attempts full immersion into participants' form-of-life. With complete participant comprehension the actions and the related norms and values of the participants do not appear strange. Instead they are considered as normal as any other member of the researched population would consider them. The research is the socialisation process the sociologist undergoes to achieve native competency. Social fluency becomes a possible indicator that the research is complete. We should also make explicit that under participant comprehension the data collected are not notes or interview transcripts, but the socialisation process itself. Such notes are still useful, however, to aid the socialisation process, to aid memory after exiting the research field, and to explain the nature of the field to others. We will now locate this thesis in the reflexivity debate of contemporary social studies of science.

2.2.5 Debates over the Social/Natural Divide

Contemporary social studies of science is engaged in an internal debate over the legitimacy of maintaining a social/natural divide in its research methodology. This section locates this thesis within the debate. The discussion here is located within an exchange on the issue between Harry Collins and Steven Yearley on one side, and Michel Callon and Bruno Latour on the other (Collins & Yearley 1992a, 1992b, Callon & Latour 1992). This position adopted here is that of Collins and Yearley.

Put simply, Collins and Yearley maintain a distinction between the natural and the social and Callon and Latour do not. Central here are, firstly, the interpretation of Bloor's tenet of reflexivity, and secondly, the goals to which the social studies of science literature is orientated. Bloor argues that reflexivity means the modes of explanation used by sociology to explore scientific practice must be applicable to sociology itself. To Collins and Yearley this is an 'in principle' argument, and does not mean the methods of sociology should be changed in response to the work of the social studies of science literature. The sociologists should remain naïve social realists regarding their own data. Callon and Latour argue that, since the social studies of science has demonstrated that the social/natural divide should itself be deconstructed, the distinction should not have a place at the heart of the social studies of science methodology. Here reflexivity is interpreted as learning from the findings of the social studies of science literature and using these discoveries to improve the research effort. Let us explore how each group operationalise their position.

Collins and Yearley argue the sociologist can meta-alternate between the methodologically relativist rejection of the social/natural divide when exploring the conclusions of their respondents and the naïve realist acceptance of the distinction when producing their own conclusions. Although the social and the natural are firmly distinguished there remains a significant interplay between them. The natural is represented by a Hesse net. By this we mean the relationship between concepts held in a taken for granted reality. Hesse nets were developed by Mary Hesse, who assigns probability weightings of the strength of the relationship held between each concept (Hesse 1974). Collins has argued that numerical weightings are too formal a mechanism for this, and instead uses the sets of rules embodied by the form-of-life of the actor (Collins 1992). The social both reflects and expresses the Hesse net, and informs and shapes it. Collins and Yearley argue that naïve social realism should be employed to explore how the Hesse net and socialness interact and reinforce each other.

Callon and Latour argue the social/natural divide should not be taken as a given; the social construction of the dichotomy is as much a subject for sociological research as any other is a fake distinction. The social studies of science analyst should not be seeking to explain the form taken by scientific knowledge through the social context in which it is maintained. Instead there is another mode of exploration at ninety degrees to the social/natural divide, the study of how network building combines natural and social influence. To this end Callon and Latour advocate Actor Network Theory (ANT) (Law & Hassard 1999). This position grants agency to both human actors and non-human actants, in producing networks of consensus. Callon and Latour criticise Collins and Yearley's use of two separate networks, one for the social and one for the natural. Instead the two exist in a single network where actors and actants interact in battles of strength to best translate, meaning represent the position, of each other. By charting the dynamics of the translations an analyst can explore scientific debates without either relegating the social to a sociology of error or maintaining a social/natural divide.

Understanding the differences between the two positions is simpler once the goals and vision of each group is understood. To Collins and Yearley the role of the social studies of science is to demonstrate that science is a cultural practice and that any claims it lays to discovering knowledge must be justified not through epistemological privilege but by reference to the real cultural resources it can offer. This is achieved by illuminating the processes of the social network permeating the Hesse net. However, to Callon and Latour, it is the maintenance of these two arenas that provide the natural scientist with their power. The concept that there is a separate domain of the natural to which scientists have access is the grounding of their claim to privilege. Instead Callon and Latour wish to demonstrate the fallacy of unique scientific epistemological superiority by drawing the social, to which we all have access, together with the natural, to which science alone claims to have access.

To Collins and Yearley, the philosophical radicalism of ANT is politically more conservative. By denying the social/natural distinction and granting agency to actants

Callon and Latour reinstate nature's autonomy as it is held in the teleological model. Unlike the teleological model, of course, ANT also bestows autonomy on what Collins and Yearley term social influence in the construction of knowledge.

Both theories are logically coherent. They are orientated towards different aims, and it is the choice of which aim to follow that leads the decision which of them to adopt. It is the argument here that the two sets of aims are not greatly different. Both assert that science should justify its claims to knowledge through cultural resources as would be the expectation in any other cultural arena. Neither argues science is unable to do this, simply that instead it opts to claim legitimacy through misplaced faith in epistemology. The differences are in tactics, Collins and Yearley privileging social explanations and Callon and Latour not. This thesis is concerned with exploring how the socio-cultural context shaped macroeconomic debate, and from the outset was cast in the terminology of SSK associated with Collins and Yearley. Given the logical coherence of each, the relative similarity between the endpoints embraced by both, and the prior location within SSK both intellectually and as a pool of expertise, the Collins and Yearley position has been adopted. Bloor's tenet of reflexivity is accepted as something that must be potentially available and applicable on a discipline-wide level, but not a necessary commitment of each research project as an individual entity. Furthermore naïve realism will be adopted in analysing the data collected. No notion of the reality of the inflation/unemployment relationship will be granted autonomy in its own discovery. Finally, since the analytical conclusions of ANT theorists are premised on such fundamentally different views of how science should be studied they are not used in the development of the conceptual framework presented in the thesis.

We will now outline the rise, and decline of discourse analysis in the Social Studies of Science in the 1980-1990s and locate the practical implications of the debate for the data collection and analysis deployed in this thesis.

2.2.6 Debates over the use of Discourse Analysis

A second debate internal to the Social Studies of Science discipline worthy of discussion concerns the use of Discourse Analysis. Proponents of Discourse Analysis present a critique of the methods associated with the Sociology of Scientific Knowledge position adopted in this thesis. This section presents arguments from both sides of the debate, and finally accepts the counter arguments against the use of Discourse Analysis. A central text promoting Discourse Analysis in the Social Studies of Science is Gilbert and Mulkay's *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, and it is with this that the discussion will commence (Gilbert & Mulkay 1984).

The majority of the empirical work in the book reports a dispute among biochemists. However, it is the methodological and theoretical position that is of interest to us here. Gilbert & Mulkay point to analytical weaknesses in the sociological research into scientific practice being conducted at the time. They typify Marlan Blissett's work as a prime example (Blissett 1972). Blissett argues the scientists studied in his work were actively political in terms of promoting and advertising their research. Furthermore he claims that this was a common part of conducting scientific research. Gilbert & Mulkay note that Blissett's claims are about scientists' actions and the consequences of their actions. However they insist that his data cannot justify such claims since his data are interviews and publications in the field which are only accounts of actions and cannot be relied upon as accurate. The distinction is central to Gilbert & Mulkay's critique of the wider Social Studies of Science literature, as it is the unquestioning use of accounts of action as accurate representations of these actions that they problematise.

Gilbert & Mulkay present a simple formula for the type of sociological analysis of science found in the literature of the time, as typified by Blissett. Firstly statements are collected in interview or observational settings. These statements are then studied for broad similarities between them. Once similarities are found the statements are taken as accurate accounts of reality and generalised as an analytical conclusion. Gilbert & Mulkay are deeply critical of this pattern of research because it ignores the context-dependence of participants' discourse. In essence they argue there is an endemic interdependence between participants' discourse and the context or situation in which it is produced. Because of this any statement is never accurately accounting the social action to which it refers. Similarities between many quotes may represent only similarities in the contexts in which they are spoken, be it an interview setting or an informal chat over coffee, but cannot represent the actions they describe. Thus conclusions such as Blissett's based upon the unquestioning adoption of interview quotations as depictions of reality must be treated with suspicion.

Indeed, Gilbert & Mulkay make the case more strongly than simply this. They do not question the specific form taken by Blissett's conclusions on his data, implying that a more considered analysis would provide conclusions that have a greater claim to represent reality. Instead they question the possibility of answering any questions about the reality of actions at all. They deny the possibility that sociologists have the ability to identify 'good' from 'bad' discourse through tacitly acquired skills. This is incompatible with their view of the world where discourses have infinite linguistic potentialities that, firstly, can be realised in many ways, and secondly, are continually reformulated through time. Since any statement could imply any meaning, and this meaning will vary with the context of production and consumption, sociological commentary on the actions referred to in the statement is impossible. Instead Gilbert & Mulkay propose a new set of questions for the Social Studies of Science, a set of questions that they argue can reasonably by addressed through the tools of Discourse Analysis.

From this position the analyst uses scientists' discourse to explore the interpretative methods used by scientists, and analysts, to depict scientific actions. Instead of studying what science is really like, the Discourse Analyst's interests lie in how scientists' accounts of action are socially generated. In doing so no part of the scientists' discourse is privileged, none is selected as the utterance most cleanly representing reality. Instead the analyst looks to explain how all statements are linked to the context in which they are produced. One example of this practice from Gilbert & Mulkay (1984) itself is the use of the empiricist and contingent repertoires, two selectively employed forms of discourse, the first found in formal and informal settings, the second only in informal settings.

These types of arguments surfaced in the 1980s, and a number of publications followed their prescriptions, for example Potter & Wetherell (1987) and Bazerman

(1988). However the counter arguments surfaced in the same period, and publications employing Discourse Analysis in the social studies of science diminished in the 1990s. Next the discussion turns to a more targeted attack by Mulkay, Potter and Yearley (1983) on a particular piece of Sociology of Scientific Knowledge research by Collins and Pinch (1979). Collins and Pinch's research explored the social construction of knowledge in the scientific study of the paranormal. They developed the concepts of the constitutive and contingent forums. The constitutive forum refers to the practices such as scientific theorising, the experiment and publication. In contrast the contingent forum refers to social aspects including the role of raising finances or recruiting students and informal conversations. Collins and Pinch argue that practitioners of paranormal research utilise both forums in order to improve their scientific credibility among researchers from other fields. Parapsychology is not generally accepted by mainstream science, and Collins and Pinch demonstrate how mainstream scientists used both forums to present their criticisms. Parapsychologists realised that doing 'proper' science, implying the practices located in the constitutive forum, was failing to improve their credibility in other disciplines. Subsequently they further started to develop the role of the contingent forum in their work to equate with the mainstream sciences. In doing this they experienced the improved social standing they sought, and as they started to be accepted by mainstream science, so the possibility of the existence of the paranormal was marginally increased.

Mulkay, Potter and Yearley take issue with several aspects of this account. The central critical points can be summarised for the purpose of this discussion in the following way. Firstly Collins & Pinch are accused of employing some of the terminology and interpretative practices of some parapsychologists unreflexively and in a way that introduces a pro-parapsychology bias. The term parapsychologist is itself an example, as they list actions undertaken by parapsychologists without recognising the word has many interpretations and could refer to disparate subgroups of people. Furthermore a similar point is made even more disparagingly regarding Collins & Pinch's use of the category of the orthodox scientist. This term is not used by the actors involved and carries a negative undertone for those it is used to describe as it implies closed-mindedness.

Secondly Mulkay, Potter and Yearley raise concerns with Collins and Pinch's allocation of acts into the contingent and constitutive forum categories. They claim the judgement is based upon whether the act appears to be linked to the knowledge claim. However they argue that the accounts vary with the context in which they are uttered, and thus are not consistent or suitable for analysis. The differences may simply reflect differing sources of information, be they documentary sources or interview data. Subsequently Collins & Pinch are criticised for writing of two distinctly separate types of action and allocating them to separate categories of action, although in practice the differences may represent only alternative sources of information.

As with Gilbert and Mulkay's argument above, the resolution to these problems is to look not at what the practice of science actually involves, but instead to explore how scientists' accounts of actions and judgements vary and are produced to conform to the expectations of the context of utterance. As in Gilbert & Mulkay, the concepts of empiricist and contingent repertoire are used as examples of this working in practice.

Collins and Pinch (1983) were provided with the opportunity to reply to these criticisms. On the first point of the unreflexive and bias use of terminology they note that 'orthodox' can have a positive interpretation, and deny that their use of the term was derogatory. It is interesting, given the thrust of their argument, that Mulkay, Potter and Yearley were unable to recognise the multiple available interpretations of the term. Collins and Pinch further contend that using actors' terminology does not introduce bias or privilege into the account unless the actors' epistemological evaluations are also adopted in their usage.

To repel the critique regarding the reflection of differing sources of information in the identification of contingent and constitutive forums Collins and Pinch restate that the forums refer to locations, either physical such as a laboratory or abstract, where the actions typically take place. They are not used to describe the actions themselves. More centrally, they argue that analysts are in a position to use their own native competence to identify and legitimate these categories. In addition they note that when discussing historical case studies that rely heavily upon documentary sources, a method biasing them towards linkages with the constitutive forum in Mulkay, Potter and Yearley's account, cues were readily available that lead the analysts to locate some cases in the contingent forum. This, Collins and Pinch argue, undermines the notion that the context of production leads the analytical outcome.

Collins (1983b) strengthens these arguments by locating them in a wider critique of the Discourse Analysis literature. He starts by noting that a criticism, such as that provided by Discourse Analysis, that can be applied to anything, is less a criticism and more a philosophical puzzle. This is extended by demonstrating that the discourse analysts' work does not in any real way counter the perceived problem of the multi-vocality of utterances and the inherent interpretation in their analysis. This is because the procedures advocated by Discourse Analysis proponents still inevitably involves judgements of the relevance and form of the statements they deal with. In following the logic of their reservations over established Sociology of Scientific Knowledge methodology they have managed to move only one stage back in the definition of what is to count as relevant data, but are still faced with all the recurrent issues of researcher interpretation. Furthermore, writers such as Mulkay & Gilbert only offer one interpretation of the statements gathered in their book, again ignoring the multiple interpretations available. Collins acknowledges that the Discourse Analysts have limited the ambitiousness of their studies to cater to their methodological concerns. However, since they are still yet to escape the concerns such self imposed limitations seem mis-placed.

Another exchange between a Discourse Analysis advocate and a writer whose position is closer to that adopted here is that of Woolgar (1981) and MacKenzie (1981). Woolgar commences the exchange by taking issue with MacKenzie's exploration of social interests in statistical theory (MacKenzie 1978). This is of particular interest as firstly it deals with historical research, as does this thesis, and secondly Chapter Eight discusses the role of social interests in the Phillips Curve debate. Woolgar's first concern is that authors, including MacKenzie, traditionally write using terms that suggest the author is discussing reliable accounts of reality. However instead they should use language that suggests they have reconstructed an account from evidence and their own interpretation. It is their duty to make it entirely clear to the reader that this is the case. This reflects the concern over the unreflexive use of statements as reflections of reality discussed by Gilbert and Mulkay.

MacKenzie takes the point loosely, but suggests it is unlikely that the readers are unaware of this and doubts it is necessary constantly to remind them.

Woolgar's second criticism reflects the view of Discourse Analysts that it is dangerous to employ the terminology of the participants as an analytical category in an unreflexive manner. In MacKenzie's case it the notion of 'interests' that is used by both the analyst and the group studied. Woolgar suggests that researchers should wait for the scientists' interest issues to be settled before using the term. MacKenzie has little time for this argument, responding that it is unnecessary and unfeasible. He uses the example of class struggle as a term that is used by both analysts and participants but is unlikely to reach settlement in the foreseeable future. This should not prevent analysts from using the term class struggle, or any other.

Woolgar also raises concerns over the visibility of explanatory forces, arguing that such forces, interests being an example, must be observable in their own right and not only through means of their effects. MacKenzie notes that Woolgar is inconsistent on this issue, sometimes insisting explanations have this character, and at other junctures claiming it is impossible for them to do so. At times he goes so far as to argue explanation itself should be avoided. He argues that the assertion of 'social' and 'cognitive' interests by MacKenzie is a misplaced reading based on assumption not evidence. MacKenzie disagrees claiming it is not assumption but the product of the considered work of the analyst. This demonstrates a sharp difference between the Discourse Analysts and those opposed to the view. Like Gilbert and Mulkay, Woolgar is denying the ability of the sociologist to make reasoned judgements about their evidence beyond the linguistic form taken by the participants' discourse. MacKenzie, like Collins above, disregards this claim.

Discourse Analysis quickly evolved and by the mid-1980s had developed a new element to its position. In an acknowledgement of part of the spirit of both Collins' and Woolgar's arguments above authors in the Social Studies of Science engaged in presenting the multiple available interpretations of their work in their own texts. This was actualised in a variety of creative ways, most notably by Michael Mulkay's *The Word and the World* (Mulkay 1985). Mulkay used a variety of different textual forms, including a chapter giving one of his respondents the opportunity to comment on his work, a parody, and a play. These textual devices are used to break down the privileged and unreflexive form of the monograph in sociology, the textual form used throughout this thesis. Mulkay argues that allowing the multi-vocality of both his respondents and his own text be visible the sociological analysis is improved. Arguments using this style became known as New Literary Forms.

A sturdy rebuttal of this position is provided by Trevor Pinch, who adopts a tool of the New Literary Forms movement known as the second voice device in his article co-authored with himself (Pinch & Pinch 1988). The second voice device is when an imagined second voice interrupts and disagrees with the main author's statements, in this case a pro-New Literary Forms voice interrupting the critique.

Pinch does not argue that New Literary Forms should be abandoned. Indeed, he points to several texts that pre-date the New Literary Forms movement that use unconventional writing techniques to make the paper easier to understand, for example Lakatos (1963-4) and Collins (1983c). However, Pinch objects when such textual forms are used explicitly to display how the analyst's text itself is socially constructed as advocated in Woolgar (1983). Pinch's criticism is that the voices used in the New Literary Forms are given an authority that may not be dependable. In these multi-voiced texts each voice is intended as a genuine representation of a set of values. However, as Pinch notes, there is no avenue available to the reader to judge the accuracy of these representations. Direct quotations may be used, but may still be used out of context. Furthermore it is often not clear whether the voice is meant genuinely or as a caricature.

In addition to these practical pitfalls of the New Literary Forms approach Pinch provides further philosophical criticism. He notes that Discourse Analysts argue that in order for any claim to be made some areas of the discourse must be privileged, and subsequently by privileging its own discourse the Social Studies of Science literature is ignoring its own multi-vocality and neglecting the tenant of reflexivity. Pinch argues that Bloor privileges *his* discourse in his work on the Strong Programme (Bloor 1973, 1976, 1984,), Collins privileges *his* discourse in his work on the Empirical Programme of Relativism (Collins 1981a, 1984, 1992), yet Mulkay and Woolgar claim to privilege nothing at all, and thereby as far as Pinch can see claim nothing at all. However, by mounting the criticism of the Social Studies of Science literature set out above they must privilege their own discourse and thus be susceptible to their own criticism. Similar to Collins' argument above, Pinch is demonstrating that while the Discourse Analysis community have developed a critique of the established methods of the Social Studies of Science literature, they have only managed to achieve one step away from these methods at considerable cost in explanatory potential and without finding a space where they themselves are immune to their own critique. Like the wider Discourse Analysis literature, publications employing New Literary Forms diminished in numbers by the 1990s and are very rare in the Social Studies of Science literature. However there are still some rare exceptions, for example Doing (2004).

The arguments against the use of Discourse Analysis presented above have several commonalities. Firstly it is claimed that the critique of SSK presented by the Discourse Analysts is also applicable to themselves. Collins (1983b) demonstrates the use of researcher interpretation present in Discourse Analysts conclusions and Pinch argues that for them to criticise the SSK approach they must prioritise their own discourse (Pinch & Pinch 1988). This argument is often associated with another claiming that Discourse Analysts make large sacrifices in the breadth and depth of the research questions made available to the sociologist. Collins argued this directly. Pinch insists that if Discourse Analysts stay true to their tenet of not privileging any discourse then they cannot make any analytical claims. MacKenzie (1981) highlights ambiguities in Woolgar's (1981) position on the role for explanation, suggesting he claims explanation may be best avoided altogether. Indeed there is little resistance against this claim from the Discourse Analysts themselves, with Gilbert and Mulkay (1984) and Mulkay, Potter and Yearley (1983) explaining the necessity of reducing the ambitiousness of their research questions. Both Collins and Pinch (1983) and MacKenzie counter this claim, arguing that the skilled sociologist is capable of making reasoned judgments about the role of actions in their analysis. Indeed, one can point to the twenty year research effort since the Discourse Analysts' critique in the Sociology of Scientific Knowledge that have successfully done just that.

As stated earlier, in this thesis the SSK position has been adopted. The research questions answered by the thesis are beyond the limits proposed by the Discourse

Analysts. Since adopting their techniques would limit the research goals, while not escaping the concerns of their critique, there seems to be little motivation do adopt their methods. Subsequently the tools of Discourse Analysis will not be used in this thesis. However, as Collins (1983b) contends, the arguments against adopting the methods of Discourse Analysis do not also imply that their analytical conclusions are inherently fallacious. In keeping with this position, the work of Gilbert and Mulkay is discussed in Chapter Nine when considering the construction of value neutrality among macroeconomists.

2.3.1 Empirical Studies of Economics and Finance

Attention now turns to empirical studies that have focused upon Economics and Finance. The first two sections consider contributions from the Social Studies of Science literature, on Economics and then Finance. The third section discusses publications from a different discipline, the History of Economic Thought, and considers the relevance of these varied texts in the context of this thesis.

2.3.2 Empirical Studies of Economics from the Social Studies of Science

The literature from the Social Studies of Science on economics is not large. One of the motivations for pursuing this research is to address this situation. However a small number of useful contributions are available. The most complete example of a social studies of science exploration of macroeconomics is by Robert Evans (1997, 1999). This is an account of the social processes underlying macroeconomic forecasting in mid nineteen nineties Britain. There are both similarities and differences between Evans' work and that presented here. We start by exploring the similarities.

Evans (1999), as with this thesis, establishes Interpretative Flexibility in three locations. Firstly it is demonstrated in the process of testing and building macroeconomic models by Evans' own replication of an established model. He argues that knowing how to interpret significance tests depends on prior commitments to theoretical positions. Attaining the appropriate skills is to be achieved through a socialisation process into the macroeconomic form-of-life. This is because to recognise a correct prediction from a macroeconomic model the researcher must draw upon existing shared ideas of how the economy works and what the answer should be.

Evans' second element of macroeconomic Interpretative Flexibility is in the practice of macroeconomic forecasting. Evans shows that an economic forecast can be configured to predict any result. The actual expertise that finalises the prediction is not a property of the model itself but of the modeller who makes constant adjustments to the model to produce the result they consider satisfactory. Evans argues that the ability to know when the results are correct is as much a product of mobilising knowledge gained beyond the modelling process as it is of the process itself.

The third element of Interpretative Flexibility reviews the processes involved in the evaluation of existing forecasts and models. Here Evans reiterates how sensitive macroeconomic models are to small changes in the data-set. He demonstrates that any claim supporting a model will have a contesting counter claim, and that all of the many models are capable of providing support for a range of alternative hypotheses.

All three of Evans' elements of Interpretative Flexibility are exemplified in Chapters Five to Seven in this thesis. The presentation of Evans' demonstration of Interpretative Flexibility is different, however.

Another major difference is the research topic. Evans takes the specific setting of the Panel of Independent Forecasters charged with advising the U.K. government after the UK's exit from the Exchange Rate Mechanism in 1992. This research field is interesting because of its uniqueness in British political culture. Seven macroeconomists from a range of intellectual positions were brought together to discuss their predictions for the following period and assess economic policy choices. Thus the debate studied by Evans is limited in personnel and time scale. The participants were only, and could only be, the members of the panel, and the research was conducted at the time the debates were occurring. In contrast the research reported here addresses a historical period of twenty five years and potentially could include hundreds if not more macroeconomists.

The second difference between this research and Evans' is that respondents used their empirical and theoretical resources to try to make successful predictions. In contrast, in the Phillips Curve debate empirical and theoretical work was used to account for what had already occurred, providing explanations for the current and previous economic climate.

These differences lead to analytical differences in the texts. The chapters of Evans' book on the role of macroeconomics in policy are very different from that presented here. Evans' focus is on an evaluation of the unique Panel of Independent Forecasters. He demonstrates that despite massive differences amongst the panel members they achieved a short-lived consensus in the localised setting of their 1993 pre-budget consultation. This was achieved because of a determined effort by most of the panel to fulfil their perceived role and maintain credibility. In doing so, Evans argues, they undermined their potential benefit of providing a politically legitimate expression of macroeconomic uncertainty. In an implicit prelude to a further elaboration on this position, Evans argues political legitimacy for uncertain science is best achieved through engaging wider locations of expertise beyond certified experts working as academic macroeconomists (Collins & Evans, 2002). As we shall see, the difference in scale of the research settings is echoed in the conclusions drawn on the role of macroeconomics in policy debate in this thesis.

The final difference returns us to the Interpretative Flexibility of macroeconomics. Although both texts agree it exists, the distinction between forward and backward looking research impacts upon how it is embodied.

A social studies of science exploration of debates in economics preceding the Phillips Curve debate is offered by Yuval Yonay (1994). Here an Actor-Network Theory approach is used to illuminate the inter war debates between Institutionalist and Neo-classical economists. Yonay notes that this debate was not over the content of the economics; the theories and policy recommendations of the two groups were not greatly dissimilar. Rather, the debate was over how economics should be done. Institutionalists were more inclined to favour empirical research compared to the more theoretically minded Neo-classicalists.

Yonay notes that for a forty year period these paradigms co-existed and vied for prominence. The debate involved advocates of each side claiming to represent and subsequently embody the weight of culturally influential phenomena. In Actor-Network Theory terms this amounts to claiming the legitimate translation of powerful actants in the debate. Yonay identifies five allies both sides attempted to enrol into their position. These allies are philosophical views, the methods of prestigious disciplines, theories from neighbouring disciplines, relevance to practical problems, and the discipline's past. Although these were the five studied in detail, Yonay also notes many others existed that could, and should, be studied. By claiming to represent the true interests of each ally, economists of both sides hoped to draw the cultural support lent to them on to themselves.

In Yonay's account the theoretical basis of each paradigm were black boxed and unchallenged, instead it was the methodology that was contested. He argues this debate differed from many social studies of scientific debates because the black boxing was the other way round, the methods were assumed stable and the contents of the theories were contested. An interesting conclusion drawn from this, that has bearing on this thesis, is that in economics the two paradigms were able to co-exist, and the success of one did not imply the death of the other. Furthermore, unlike many other social studies of science accounts of sciences, Yonay argues both sides of the debate shared common goals and standards of evaluation.

These two insights are ratified in this thesis, even though the periods of study are up to fifty years apart. Both insights, that the existence of one paradigm does not impede the existence of another, and that they can share goals, are inherent to the argument presented here. As will become apparent, the insight added by this thesis relates to how these goals are derived and what happens when they change.

Yonay concludes by noting that due to their similarities and shared historical context both sides claimed to translate the positions of the same actants. He notes the flexibility in how translations occur that allow this. Equally, as power shifts occur between the allies, the economists would reshape their translations in reflection.

Daniel Breslau (1997) provides an account of the production of economic knowledge in a policy driven setting for the period following that studied in this thesis. He takes the example of research evaluating the Comprehensive Employment and Training Act instigated in the U.S. in the early 1980s. Central to his analysis are the different approaches and their justifications to the evaluation adopted by economists working in academic settings and those working in independent research institutions, or, in his terminology, contract shops. The evaluations were tasked with judging if people who partook in training earn more money afterwards than they would without the training. The problem is how to estimate the figure for how much the individual would earn if they had not undertaken the training. In response the academic economists would construct post hoc data from available data-sets and attempt to correct for non-random selection. The contract shops would intervene in the administration of the programme and randomly assign applicants to an untrained control group.

Breslau argues that the academic economists remained faithful to an ethos of detachment and rationalism. In effect, their value emphasised attaining a generalised understanding of the processes in operation above what could be learned from any individual research setting. The data would inform an overarching model of the wider processes that would then inform their conclusions. This stood in contrast to the contract shop approach that criticised the academic economists for lacking localised knowledge of the research setting and not basing their conclusions on the empirical findings without mediation by a wider model. These differences are justified by each group by reference to their perceived audience. The academic economists prioritise their peers as their audience, and processes of peer review and the wide ranging theoretical models, the product of a community effort, are stressed to legitimate their work. The contract shops, however, identify the government as the audience, and mobilise notions of the suitability of their output for policy making purposes to lend credibility to their preferred methodology. Due to this, Breslau argues, the debate actually concerns which social networks, and their associated claims to credibility, are most suited to evaluating government policies.

With its focus on the relationship between the work of academic economists and the policy setting arena it would seem at first glance that Breslau's work should provide an excellent opportunity for contrast and elaboration on the processes discussed in the later sections of this thesis. However the context is quite different. Breslau's example is specific to a set of interactions over the evaluation of a particular policy. Here we explore the construction of the macroeconomic knowledge through which policy choices are made. Furthermore Breslau's account is primarily one of normal science, Thomas Kuhn's (1970) term for periods of science when the orthodox position is not being seriously challenged, whereas the investigation of the cycle of contest and closure pursued in this thesis emphasises the processes that underlie shifts between normal sciences. This contrast in focus, of course, does not make Breslau's conclusions any less acceptable, only less pertinent in the context of this thesis.

One account that does share many similarities with the arguments presented in this thesis is Donald MacKenzie's (2003) study of the development of the Black-Scholes, or Black-Scholes-Merton, option pricing equation. He argues the equation has become an essential part of modern financial economics. This has, in part, been a product of the increasing importance of options trading. Writers on the topic in the 1950s and 1960s received very little attention for their efforts compared to Black, Scholes and Merton. However, MacKenzie shows, the increased importance of option trading is also a reflection of the success of the equation itself. MacKenzie begins by charting the history of the equation.

MacKenzie explains how Fischer Black started working for the consultancy firm Arthur D. Little Inc in 1965 where he met Jack Treynor. Treynor had developed what would become to be known as the Capital Asset Pricing Model (Treynor 1962). The model provided a systematic count of the 'risk premium' of investments in capital assets. It argued that, through diversification and other protective strategies, investors could protect themselves against many of the risks involved in asset trading. However they could not safeguard themselves against general fluctuations in the market. Treynor stipulated that the less an asset was linked to market fluctuations the less risky it was, and subsequently the lower the return the investor should receive. Treynor developed his model for trading in stocks. However Black started to use it for other areas and assets, including warrants and options. In 1968 Black met Myron Scholes and, with Michael Jensen, took on early empirical tests of the Capital Asset Pricing Model. Independently Black and Scholes had both been exploring how to apply the model to warrants and options, but both struggled to complete their models.

Upon comparing notes, and introducing the calculus technique of Taylor expansion discovered by Black when working on companies valuations of cash flows with Treynor, they managed to make progress. However the model did still not produce results completely analogous to the Capital Asset Pricing Model. In the spirit of experimentation they introduced an equation used by a graduate student at Yale in the 1950s called Case Sprenkle (1961), and made numerous modifications to his equation. After a moment of intuition they found the configuration they required to make their model directly analogous to the Capital Asset Pricing Model.

Working independently, Robert C. Merton and Paul Samuelson had also been modelling warrant trading. Upon discovering the Black and Scholes work, Merton reconfigured the relationship again, this time in his preferred continuous-time model, meaning investors could alter their investment portfolios instantly at any time. This reduced all of the risk in the warrant trading, and was adopted by Black and Scholes (1973) when they finally published their work.

MacKenzie draws three conclusions from this section of the paper. First he argues that the development of economic arguments fit with Michael Lynch's (1985) concept of bricolage seen in a neurobiology laboratory. By this MacKenzie means that the mathematical work is best described as creative tinkering and not led by a system of rules. Whatever mathematical tools that happened to be found to hand, for example Sprenkle's equations, were used and reconfigured until the equations worked. This argument has much in common with the analysis presented in Chapters Five and Six of this thesis, where macroeconomists contributing to the Phillips Curve debate follow similar actions. The second of MacKenzie's conclusions at this stage also has a heavy association with the analysis presented in this research. He notes that this bricolage was not randomly done. Instead it was goal orientated. Here MacKenzie references Kuhn's (1962) problem solving mechanism to demonstrate that Black and Scholes only judged their equations to be working when they operated in the same way as Treynor's Capital Asset Pricing Model. Merton then reconfigured them again to fit his preferred continuous-time model. The notion that macroeconomic research is goal orientated is present throughout this thesis, furthermore Kuhn's mode of analysis is developed in detail in Chapter Ten. MacKenzie's final conclusion at this stage is that the label 'orthodox' in economics does not imply a unity of thought. He contrasts

Black and Scholes' work to Merton and Samuelson's, and to a further work by Kassouf and Thorp to show that their views, all labelled orthodox, were different and were derived using different standards and goals.

MacKenzie then develops further evidence for his fourth conclusion, that the Black-Scholes equation is 'performative'. Performativity implies that the existence of a knowledge claim changes the reality of the world that it describes. He substantiates this claim in four ways. Firstly the Black-Scholes equations altered the patterns of option prices. Two early empirical tests of the equation found some supportive evidence, but only a limited amount. However by the late 1970s the evidence was very supportive. MacKenzie argues the reasons for this include the use of the Black-Scholes equations as a guide to arbitrage. By following the patterns of behaviour suggested by the model market participants acted more in accordance with its assumptions. Secondly the model legitimised options trading and thus helped to create the efficient liquid markets assumed in the model. By providing traders with hedging techniques, i.e. guided estimations of safer ways to invest, the equations gave the markets confidence to write options at lower prices, again making them closer to the theory. Thirdly major investment banks today almost fit the zero transaction costs assumption in the equations. This is because the equations allow a measurement of the risks involved in trading and thus provide the capability of off-setting them against one another. This makes the risk almost zero allowing the banks to provide liquidity as if there were no transactions costs. Finally MacKenzie argues the theory has been extended to be applicable in other circumstances, including obvious areas of financial economics such as bond theory, but also areas as distant as pharmaceutical innovation and the production of film sequels. MacKenzie's arguments on performativity do not have an equivalent in this thesis. They clearly are interesting, but it is his first three conclusions that best support, and are supported by, the analysis presented here.

Finally, a rare venture into the social studies of science literature on macroeconomics was made by Collins (1991) in the early nineties. Commenting directly on the Phillips/Lipsey Phillips Curve, Collins introduces ideas he has explored in more depth elsewhere on topics with which he is more familiar (Collins, 1992, 1988). Aided by a conversation with Nancy Wulwick, discussed in her own right later in the section about the History of Economic Thought literature, Collins demonstrates the meaning of the experimenters' regress and replication with versions of the Phillips/Lipsey Phillips Curve as an example. These are firstly a scatter diagram charting the relationship for 1861-1913, secondly Phillips' own curve, thirdly a similar looking curve made in the 1970s with an advanced non-linear regression technique, and finally a even more advanced kernel-regression on the data producing a sharp jagged line with no visible similarities to Phillips' curve.

Collins makes the distinction between replicating work and repeating work. Repetition is always about checking the results, but not contesting their meaning. Replication, in contrast, is about how the relationship ought to be considered. As an example he takes the third smooth curved and fourth jagged line replications of Phillips Curve. For an analyst to adopt either of these as a successful replication of Phillips' curve is a statement on how the relationship ought to be considered because they represent different visions of what macroeconomics should be looking for. One version represents a smooth law like regularity and the other does not. To Collins, since the original scatter diagram does not display a smooth relationship, Phillips' representation of the relationship as a curve was a direct claim that macroeconomics should be the pursuit of law like regularities in economic phenomena. Accepting either of the replications either confirms or rejects that commitment.

Collins notes that this issue cannot simply be side stepped by claiming to accept the version that is most correct. This is because, without knowing in advance what the correct formulation of the relationship is, the macroeconomist has no criteria by which to recognise the correct result. This is the 'experimenter's regress'. The only way one can decide between the smooth or jagged curve is through a judgment on the law-like nature of economic phenomena.

This draws to a close our discussion of writers considering macroeconomics from a social studies of science perspective. Now we turn to parallel work on the social construction of Finance.

2.3.3 Empirical Studies of Finance from the Social Studies of Science

There is a growing literature within the social studies of science on the topic of finance. Discussions during the development of this thesis have shown some readers feel it is relevant to the work presented here, and it is, but not through the mechanism that is often identified. The notion is that since the social studies of finance literature explores social scientific quantifications of the flows of money, and macroeconomics also is a social science that quantifies the flows of money, that there must be a mutual core of ideas. However this is not necessarily the case. The macroeconomic debate explored here is distinct to the form of action observed in these studies. As we will show by discussing three authors in the field, where these arguments have relevance to this thesis it is because they are good social studies of science arguments, but not because of any shared field of enquiry.

The first author we consider is Donald MacKenzie. His work applies Barry Barnes' (1983) concepts of S-Terms and N-Terms to finance theory (MacKenzie 2001). N-type terms are those where the meaning is a predetermined set of relationships between terms. The decision to apply the term to a specific entity is based upon the degree of fit the entity displays with the definitional relationships. In contrast to this, S-type terms are those where the use of the term performs a socially recognisable act. They are not labels for existing actions, but are the actions themselves. MacKenzie argues that finance theory engages with S-type terms.

MacKenzie provides the Black-Scholes-Merton option pricing theory as an example of this. His argument is that as the theory was used and institutionalised the originally questionable assumptions became more realistic because the markets were shaped in the image the theory cast. It was not by chance that this happened. Although not the only processes leading to the reality of the assumptions, the existence of the model did add to their ratification. The model identified perceived weaknesses and opportunities in the market, and acting upon the identification of these, actors in the market changed their behaviour and acted more in line with the assumptions of the model.

MacKenzie's argument is interesting, and S-terms can certainly be applied to macroeconomics. Only one of many examples from the case study used in this thesis is the Friedman/Phelps Phillips Curve. Advocates of this position argue there is a natural rate of unemployment, and there is a negative impact for the economy if it is held at a different level. Thus they advocate removing any entity, such as government intervention or Trade Union influence, that prohibits the attainment of the natural rate. In doing so the policy creates in the real world the assumptions underlying the model and add to its validity.

However, MacKenzie's arguments are interesting because they are good social studies of science arguments. There is nothing intrinsic about the application of them in the financial setting that heightens their relevance to this thesis. The fact that finance theory considers money, and, as MacKenzie himself charts, has progressively encroached upon the mainstream of the economics discipline should not privilege its position in this context. Furthermore, although applicable to the macroeconomics case, the S-term and N-term analysis is not the best available concepts for addressing the issues central to this thesis, and are not discussed again.

Karin Knorr Cetina and Urs Bruegger (2002) have conducted an ethnographic study of financial markets. They note that these markets are both small scale local operations and simultaneously global. The mediation of this relationship forms the core of this work, a relationship they term the global microstructure. Their argument is that, against what may be expected, global microstructures do not entail increased social complexity or patterns of authority. They explore the notion of response presence, meaning real-time communication and shared observations between individuals who are not in the same geographical location, typified by electronic means of communication. Financial markets, they argue, have acquired a particularly developed form of temporal interconnectivity that permits the global microstructure to operate across time zones. The existence of a temporal relationship, however, is not beyond normal patterns exhibited in microstructures.

This is far from a complete encapsulation of Knorr Cetina and Bruegger's work. However it is sufficient to demonstrate the clear difference in focus between their work and that presented here. They are foremost exploring the sociality underscoring localised community networks operating on an instantaneous global scale. We are exploring the process of constructing knowledge in macroeconomics over twenty five years. Again, simply because both fields measure flows of money there is not a necessary relevance beyond the more general contribution to the social studies of science literature.

The final contribution to the sociology of finance literature that we consider here is from Michel Callon (1998). His direction of thought is different again. By exploring the economic concept of externalities, meaning impacts, positive or negative, of an economic act that are not represented in the market price, he hopes to persuade the reader that economic and social constructivist arguments can be brought together to forward the understanding of economic phenomena. His analysis has similarities to MacKenzie's discussed above. Both suggest a role for the social scientific work intended to understand the markets in shaping their real world form. An example from Callon is the work of economists in measuring externalities in the market. By quantifying the hitherto unmeasured effects they alter how the actors involved in the markets consider their own positions, and the horizons of policy makers. Again, as interesting as this indeed is, it does not imply that it has any relevance to the work here simply because it considers the quantification of economic phenomena. Another tenet to all three of these studies is that they call for further research into a wide and under researched field of study. The discussion here has demonstrated this width as, even within works that may be considered similar and interconnected, great distances in subject matter still exist.

2.3.4 Publications from Another Discipline: the History of Economic Thought

The History of Economic Thought literature is wide and varied. Many of its texts discuss similar topics to those presented here, and are informed by the Social Studies of Science literature (see Hands 1997, and discussion by Pinch 1997) and often refer to themselves as using the 'naturalistic turn' (Hands 2001, Weintraub 2002). However another subset operate with entirely different interests and methods. One example is Ancil and Hakes (1991) who seek hitherto unappreciated antecedents to Fred Hirsch's (1976) division of the economy into material and positional, and then proceed to debate his arguments as if they were his contemporary peers. Even those authors with more shared interests with the Social Studies of Science literature frequently fall short of embracing the full analytical potential of wider social forces.

Although always concerned with the production of knowledge in economics, such texts rarely employ explanatory forces beyond the internal intellectual concerns of the discipline. The wider cultural context is often neglected and the analyses certainly do not fit the prescriptions of EPOR and the expectations of the Social Studies of Science community discussed above. There are exceptions of course, and some of these will be discussed in this section while others are left for consideration in the empirical chapters of the thesis. In most cases the objective of the historians' analysis is to highlight some way in which economic methodology can be improved. We will start by discussing some of the contributions that are furthest away from the approach employed in this thesis. The examples chosen do consider debates that have relevance to the arguments made here. Their difference is a product of the goals and methods adopted. As this discussion develops we move towards contributions from the History of Economic Thought literature that share more common-ground with the approach adopted here. This section is not intended as a comprehensive account of the entire literature. It is too vast to attempt such a feat in this context. Instead it is to provide an awareness of the breadth of positions found in the literature and to highlight the differences between the approaches adopted in this thesis and the History of Economic Thought literature. Those contributions that are most relevant to the analysis presented in this thesis are discussed at greater length later in the thesis.

The author from the History of Economic Thought who most closely shares the same empirical ground as this thesis is Robert Leeson. Many of his publications consider issues related to the Phillips Curve debate, and he has edited Phillips' complete works (Leeson 1999a). However his approach and interests are different from those pursued here. For example, Leeson (1999b) considers the opinion Keynes would have had of the Phillips Curve relationship had he lived to see its adoption in his name. The paper explores Keynes' writing before, during, and after the *General Theory* to show that the original Phillips Curve argument, termed the Phillips/Lipsey Phillips Curve in the empirical sections of this thesis, goes against the thrust of his work. Leeson stresses the need for terminological changes necessary to protect Keynes' writings from the embarrassment of the collapse of the early Phillips/Lipsey Phillips Curve relationship. He insists, firstly, that the policy of accepting ongoing inflation in the hope of sustainably reducing unemployment should be called pseudo-Keynesian, and, secondly, that the policy of increasing unemployment in the hope of obtaining benefits in inflation should be called anti-Keynesian. Leeson substantiates this claim through detailed references to Keynes' works highlighting the intolerance towards accepting ongoing non-trivial rates of inflation.

It is difficult to see the relevance of this argument for this thesis. It shares none of the analytical frameworks adopted in this thesis. As with many texts in the History of Economic Thought literature, the boundary between writing about economics and doing economics is not as obvious as one would expect from a Social Studies of Science perspective. Leeson is making claims about how Keynes' work should properly be interpreted. His new terminology makes statements about what Keynes' theories imply and their incongruence with policy descriptions. Of course, interpretations of economists' work will be made in this thesis. However in this context the interpretations will explore the norms and values informing economists' actions. This stands in contrast to Leeson's assertions of the 'real' meaning of Keynes' analysis for policy debates. In effect Leeson is performing the same types of actions as the economists he studies, but with the benefit of hindsight to correct 'mis-informed' beliefs of the time. His work intends to correct the misinterpretations of the past. There is considerable distance between such modes of enquiry and the prescriptions of the Strong Programme (Bloor 1973, 1976) and SSK (Collins 1981a, 1983a, 1992). As discussed above, Bloor provides an account of the teleological model. This is the thesis against which Bloor's Strong Programme is developed. Here truth is granted autonomy in its own discovery. However, even this model has more in common with the position adopted here than that used by Leeson. Although the teleological model assumes the 'truthfulness' of a knowledge claim is an explanatory factor in its success, it does not go so far as to contest the content of this 'real' truth. By challenging these interpretations Leeson is acting not as a distant observer of the Phillips Curve debate, but as a participant. This is not to say that Leeson is wrong for doing so. He is a Historian of Economic Thought, and should be judged by the standards of his field not those adopted here. However it does demonstrate a clear distinction between the two sets of literature and show why such analysis has little utility in this context.

This is not the only example from Leeson. One 1997 paper demonstrates how the work of Keynes, Phillips, Friedman and Tinbergen are all vindicated by the collapse of the Phillips/Lipsey Phillips Curve discussed above (Leeson 1997a). Another illustrates how the work of Alvin Hansen and Sumner Slichter took Keynesianism away from Keynes' stated position on the goal of zero inflation (Leeson 1997b). However not all of Leeson's work follows this format. Leeson (1997c) details the work of early detractors of the Phillips/Lipsey Phillips Curve. This paper proved a useful bibliographic source early in this research, as discussed in Chapter Three. Furthermore its analysis has similarities with the documentation of Interpretative Flexibility as prescribed in EPOR (Collins 1981a, 1983a, 1992) and pursued in Chapters Five, Six and Seven. The paper demonstrates alternative constructions of the early relationship and attempts by economists to undermine Phillips' conclusions. However the paper unintentionally illustrates a further issue regarding the native position of Leeson within the economics field. The dynamite of Leeson's argument is that for up to four years after Phillips' initial 1958 publication there were numerous critiques still being published. The argument's controversy is premised on the reader believing the relationship was accepted with less hostility than a reasonable volume of contradictory papers for four years. The argument made here is that this demonstrates Leeson's unreflexive position within an academic economists' form-of-life. It seems difficult to believe that any scholar within the Social Studies of Science field would consider a four year trail of dissenting publications represents a 'surprising' level of controversy. More likely the four year time period would be considered particularly fast. This difference is not a result of any particular privileged position held by the Social Studies of Science field over Historians of Science. Indeed we can suppose that historians of other disciplines, and their practitioners, would concur that four years is a remarkably small amount of time for such a reconfiguration of the orthodox position to occur. As argued in Chapter One of this thesis, economic debate moves at a substantial rate far beyond that witnessed in other sciences. Leeson's claim that four years worth of controversy would be a surprising finding suggests he may be too close to the field to recognize this. Indeed, this may suggest why no other attempts at explaining the speed of the macroeconomic cycle of contest and closure were found.

We will consider one final paper by Leeson (1997d). This paper provides an analysis with greater linkages to wider social influences than those discussed so far. It discusses the political economy of the early Phillips/Lipsey Phillips Curve debate in the lead up to the 1960 American Presidential campaign. Leeson's analysis starts as far back as the New Deal, through McCarthyism in the 1950s, and into the 1959 American Economics Association (AEA) conference. He argues that Nixon's campaign team had two main attacks upon Kennedy. The first suggested Kennedy was soft on Communism. However Leeson describes how Kennedy side-stepped this issue by applying the same label to Nixon for not backing anti-Castro Cuban exiles. Kennedy knew that Nixon was in fact doing exactly that, but could not acknowledge so publicly because the action was secret. The second main attack on Kennedy's campaign insisted that his 'big government' economic policies and pursuit of full employment would have catastrophic inflationary results. Leeson argues this position was exemplified by one of Nixon's advisors, Arthur Burns, the then President of the AEA, in his speech to the 1959 conference. Nixon claimed the inflation resulting from Kennedy's proposals could reach 25%, a chilling figure in the inflation-obverse America of the time. However, also at the 1959 AEA conference, Democrats Samuelson and Solow presented their paper highlighting the Phillips/Lipsey Phillips Curve argument, which until this time had only been applied in the UK context. They argued that Phillips' relationship suggested the increase in inflation would be far from the magnitude suggested by Burns and Nixon, and that the level could be controlled. The suggestion is that the Phillips/Lipsey Phillips curve was quickly adopted by other Democratic Fiscalist Keynesians and economic advisors within the Kennedy campaign because it sidestepped Nixon's second campaigning point in the 1960 Presidential election.

The contribution of Leeson's (1997d) paper is ambiguous in this context. Like his other papers discussed above (Lesson 1997a, 1997b, 1997c, 1999b), it is a very detailed account of the period it describes. Indeed such detailed accounts appear greatly valued in the History of Economic Thought literature in their own right beyond any analytical conclusions drawn upon them. However, in this instance, for all the detail the final argument is limited. The paper shares most ground with Chapter Eight in this thesis. Here the notion of Political Interpretative Flexibility is developed to illuminate how the political connotations of an economic theory, or any idea, is subject to social negotiation and has infinite potential forms. In this chapter an account of the Phillips/Lipsey Phillips Curve as expressive of mainstream leftwing thought is developed. Leeson's account has provided additional evidence of this account being substantiated in the late 1950s/early 1960s. However Leeson's account has more in common with the 'social interests' model used by Shapin (1975, 1979) to explore the case of Phrenology in Eighteenth Century Edinburgh, as discussed in Chapter Eight. This is because both identify a clear association between a scientific claim and a political discourse that mutually reinforce each other. However Shapin's account employs the tenets of Bloor's (1973, 1976) Strong Programme to locate and substantiate his claim. Despite the detail of Leeson's analysis his assertion of an association between political and theoretical resources lacks an equivalent theoretical base. This is not to say Leeson is wrong in identifying the association, indeed Chapter Eight here argues it is both a socially available option and one that is instantiated in the data presented here, only that by the standards of the Social Studies of Science literature the association is causally made.

Another Historian of Economic Thought who shares very little common ground with the theoretical position adopted in this thesis is Mark Blaug (1976, 1991, 1992). Blaug also adopts a position fitting with Bloor's (1973, 1976) characterisation of the teleological model, in this case that of Imre Lakatos (1963-4, 1971). Firstly we will discuss his application of Lakatos' scheme to the emergence of Keynesian ideas (Blaug 1991). Here Blaug responds to Wade Hands' (1985) criticism of such a position. Blaug responds by reasserting his preference for a Lakatosian framework over a relativist one, here represented by Thomas Kuhn (1962), and tightened his argument in response to the criticism.

The basis of Hands' critique is that Keynes did not produce any 'novel facts' as stipulated by Lakatos. These are essential for a Scientific Research Program (SRP) to be 'progressive', and thus gain professional approval in the Lakatosian framework. Such facts must be unknown prior to the new SRP and not be used as foundational ideas in its creation. Hands argues typically proposed elements of Keynes thought, such as mass unemployment and the consumption function, do not comply with Lakatos' definition. Subsequently, using Lakatosian language, the adoption of Keynesianism in economics was 'irrational'. Instead Hands proposes that Keynes' success is a product of his convincing explanation of mass unemployment compared to the orthodox theory. Blaug argues Hands takes the Keynesian Revolution as assumed in his criticisms and use it to discredit the use of the Lakatosian framework in the History of Economic Thought. Instead, Blaug suggests, we can take the

Lakatosian framework as assumed and use it to further our understanding of the Keynesian Revolution. Hands' argument is deemed ineffective because it ignores the difference between pre-Keynes U.K. and U.S. economic thinking. In the US in particular there were people prescribing similar policy advice to that provided by Keynes.

Blaug discusses a number of alternative issues that contributed to Keynes' success. He used static equilibrium analysis which made his theories simple and rigorous. Furthermore by linking his variables wherever possible to measurable units he associated his work with the new rising trend for statistical use. Finally the *General Theory* had many questions and space for further research to follow on from its analysis. However these circumstances are not the 'novel facts' the Lakatosian framework demands. For Blaug the principle novel fact found in Keynes is that fiscal policy can raise real income up to the full employment ceiling in a single time period. This is because the instantaneous multiplier is above unity for both public and private investment. Blaug argues that this was not known before the *General Theory* and is an unanticipated result of the combination of Keynes' consumption function and Keynes' co-definition of investment and savings. Given this Keynes' arguments are 'theoretically progressive', in Lakatosian terms, and thus suitable for Lakatosian analysis. Blaug notes that this is not the only novel fact found in Keynes' work, the lowness of investment elasticity and that assertion that the average propensity to consume decreases as national income rises were both identified in a list of other examples, however it is the one that most granted Keynes professional approval among his fellow economists. Blaug concludes by arguing that Keynes' theory became so widely successful so quickly because it contained a number of truthful statements about the economic system. Subsequently Blaug closes, its adoption was 'rational', in the Lakatosian sense, for economists in the 1930s.

There is clearly no compatibility between Blaug's account and the type of analysis prescribed by the Strong Programme and the Sociology of Scientific Knowledge. The success of Keynes' arguments is attributed to their truthfulness. The additional supplementary arguments, that it could be associated with the increasing statistical work in economics and it allowed room for further contributory work to follow, still deal exclusively with technical economics issues and make no reference to the wider social context beyond the economics community. The first of those claims, incidentally, relies on the assumption that there was increasing statistical work in economics in this period, an assumption that is questioned by Biddle (1999). There is a clear distinction between this work and that presented in this thesis.

Blaug (1976) defends his use of the Lakatosian framework over one based upon Kuhnian or Popperian prescriptions. He does so through an empirical demonstration of the relative suitability of a Kuhnian and Lakatosian analysis as applied to the Keynesian Revolution. This account is the one he improves in the light of Hands' (1985) criticism in the article discussed above (Blaug 1991). He hopes to demonstrate that the Keynesian Revolution is not the paradigm shift that Kuhnian analysts would purport, but instead is a collection of linked sub-paradigms that are better characterised as Lakatosian Scientific Research Programs. He identifies each of the sub-paradigms, for example the relatively free market with maximising individuals, and uses Lakatosian terminology to elucidate how Keynes used them. He contrasts this with a short and somewhat patronising account of how Kuhnians would describe the period. Here the "Walt Disney" (Blaug 1976 p163) history of economics details "a whole generation of economists, dumbfounded by the persistence of the Great Depression, unwilling to entertain the obvious remedies of expansionary fiscal and monetary policy, unable to find even a language with which to communicate with the Keynesians, and, finally, in despair, abandoning their old beliefs in an instant conversion to the new paradigm" (Blaug 1976 p194).

As noted earlier in this chapter, elements of Kuhn's paradigm shift analysis are used in this thesis to describe the cycle of contest and closure in macroeconomics in the period following the Keynesian Revolution, subsequently Blaug's criticisms are ones that should be addressed. Firstly we should note, to use Blaug's language, how 'Walt Disney' his characterisation of a Kuhnian explanation is itself. A similar defence of the Kuhnian position is found in Doppelt (1978), who is arguing against the attack of Dudley Shapere (1964, 1966) and Israel Scheffler (1967, 1972). Doppelt argues that Shapere and Scheffler are using overly harsh interpretations of Kuhn in their criticisms. In the Shapere and Scheffler case it is their strict interpretation of Kuhn's notion of 'incommensurability' discussed earlier in this chapter. By prioritizing incommensurability located in, firstly, the scientific concepts or theoretical language and, secondly, the observational data or mode of scientific perception, over, thirdly, the agenda of problems to be solved and, fourthly, the criteria of adequacy for scientific explanation, Shapere and Scheffler orientate their criticisms towards a very brutally portrayed version of Kuhn's position. It can be argued that Blaug has done the very same thing. His caricature of economists unable to communicate with Keynesians and unable to accept obvious policy prescriptions also employs a very austere interpretation of Kuhn. As Doppelt argues, the first two locations of incommensurability discussed above can be found in Kuhn, but are not necessary for a Kuhnian explanation. Doppelt's preference is to focus upon the latter two locations, as followed in Chapter Ten of this thesis, where Blaug's Walt Disney account would not be a fair reflection.

However there is a second dimension to be asserted. Blaug demonstrates differences between his account of the Keynesian Revolution and his interpretation of the standard Kuhnian story of science. He uses these differences as justification to abandon the Kuhnian scheme. However this need not necessarily be the case. Indeed, an alternative path has been followed in this thesis. Chapter Ten also describes in detail a number of differences between the standard Kuhnian analysis and the empirical evidence for the Phillips Curve debate. However this is not used to abandon the Kuhnian framework. Instead it is used to highlight a specific mechanism subsumed within Kuhn's account. The demonstration of difference provides one of the central conclusions of this thesis. It demonstrates that Kuhn's problem solving mechanism is substantiated differently in different scientific disciplines. Kuhn's arguments are not rejected because of this, but contributed to with further nuance. This is because, as discussed at length in the opening sections of this chapter, this thesis is based within the Social Studies of Science literature in general and the Sociology of Scientific Knowledge in particular. From this position the more sophisticated accounts of Kuhn's problem solving mechanism and the role of scientific paradigm changes are not controversial issues. The existence of paradigms in macroeconomics is not an issue available for contest. It is an assumed proposition within the position. The interest lies in exploring how they are maintained and negotiated in practice, exactly the task performed by this thesis. Just as Blaug (1991) in his response to Hands (1985) assumed the Lakatosian position, here we assume a more subtle account of Kuhn's position in relation to macroeconomics. We now turn to a number of authors employing more interesting methodological approaches, starting with Deirdre McCloskey.

McCloskey was one of the first to introduce philosophical and literary criticism to economics (McCloskey 1985, 1990a, 1990b, 1994, 1998). McCloskey focuses on the ways economists use rhetoric and metaphor to persuade their readers. They appeal to wider authorities in an attempt to establish some of that credibility for themselves. For example McCloskey demonstrates the quantity of rhetorical devices employed by Paul Samuelson (1947), Nobel prize winner and populariser of the Phillips/Lipsey Phillips Curve, to persuade his audience in only two pages of a central early mathematical text. McCloskey notes how Samuelson presented the mathematics with an air of incredible confidence, frequently assuming the reader could understand what was complex and probably unfamiliar mathematical formulations, to lend himself a persuasive authority on the subject. Furthermore Samuelson employs 'we' when discussing his cold facts of mathematics, but 'I' regarding his enthusiastic portrayal of the economic outcomes. Other rhetorical devices used by Samuelson are references to significant historical economists, relaxing central assumptions to allow further speculation, accounting dynamics in hypothetical unrealistic economies, and the use of analogy and metaphor for economic notions. This all within just two pages.

Central to McCloskey's argument throughout is how economics can be methodologically improved. McCloskey does not argue that the literary nature of economics is a sin of subjectivity and something to be eradicated. Instead she argues the rhetorical aspects of economic writing should be made explicit, a task she has been busily undertaking for sometime now, and used to its fullest effect.

McCloskey's writing is always a witty, engaging and enjoyable read. Furthermore it does explore the positive/normative constructions central to the Chapters Nine of this thesis. Yet, while the text remains convincing that economics is indeed a rhetorical enterprise, it does not add further analytical insight to our context. As is the case with many of the authors discussed in this section, the subject matter appears similar, and in some cases is similar, but the focus and conclusions are orthogonal to our interest.

Another author focusing on the role metaphor is Philip Mirowski (Mirowski 1989a, 1994, 1999, 2002). Mirowski is a vocal critic of contemporary economics. His 1989a volume asserts that neo-classical economics adopted the logic of nineteen century physics, by replacing energy with utility flowing through the physicists' metaphor. His criticism is based on his argument that the logic was used incorrectly, as the concepts of the conservation of energy were not properly incorporated. The 1994 edited collection furthers the cause with another twenty authors contributing chapters explaining how qualitative dynamic mathematics (Bausor 1994), evolutionary biology (Hodgson 1994), Newtonian natural philosophy (Cohen 1994), physiology (Christensen 1994) and various other scientific metaphors have informed, and typically weakened, the work of various economists through the ages. These works often demonstrate a knowledge of the social studies of science literature, and do make moves to introduce methods of explanation beyond the internal intellectual concerns of economics itself. However the limit of this extension is too often the internal intellectual concerns of other sciences. In all, as is becoming the trend in this section, they do not reach to wider socio-cultural processes to the extent that we will here.

Mirowski does attempt a wider extension to cultural influences in his 2002 book. Again neo-classical economics is attacked, here with the emphasis almost exclusively on game theory. In this effort the impact of World War Two and cold war funding priorities are explored. Interestingly, neo-classical economics is subdivided into three strains, the Chicago school, here associated with the Friedman/Phelps Phillips Curve, the MIT economics department, here associated with the Phillips/Lipsey Phillips Curve, and the Cowles Commission, each of which, Mirowski argues, can be seen as adopting metaphors of understanding from their military employment in research units, the Statistical Research Group at Columbia, the RAND Corporation in Santa Monica, and the Rad Lab in Cambridge, Mass. respectively. Subsequently Mirowski's wider social exploration, as interesting as it may be, is simply a mechanism demonstrating how the macroeconomists became initially subject to alternative patterns of thought, that could later be employed as metaphors for economic thinking. Again the account offers stimulus for changes in the economics from within its own intellectual curiosities or those of the sciences from which it borrows.

Another consideration of the impact of Physics in macroeconomics worthy of a brief discussion is found in Togati (2001). The paper emphasizes the links between Keynes' 'General Theory' and Einstein's Relativity Theory. The argument is made that there are significant analogies between the two works, both in terms of the general worldview employed and the specific methodological principles. Togati concludes that these insights undermine accounts of Keynes as an isolated theorist and further highlights Einstein's intellectual significance. This type of work is clearly unrelated to the kind of research programme followed here.

Some examples of more interesting discussions are Mary Morgan (1990), Nancy Wulwick (1989, 1996) and E. Roy Weintraub (1991, 2002). Morgan and Wulwick offer accounts of economists' empirical work and are discussed at greater length in Chapters Five and Nine. Wulwick studies the production of two influential early contributions to the Phillips Curve debate. Indeed, as acknowledged in Chapter Five, her work almost pre-empts the analysis presented in the first section of the chapter. She explores how Phillips' (1958) and Lipsey's (1960) papers were constructed, highlighting the distance between the procedures follow and those advocated by economics methodology. However the ends to which she orientates her work differ greatly those pursued here, and with it the conclusions drawn. Morgan studies the development of econometrics in the first half of the Twentieth century. She discusses how econometricians recognised the need to manipulate theoretical and empirical resources, and has parallels with the analysis presented here in Chapters Five and Six. However, again, the purpose to which this analysis is put differs greatly to here. Morgan's work is a historical exploration and provides a richly detailed discussion of the fifty year period studied. However it is clearly not an exploration of the social context of this knowledge construction in the form adopted by the Social Studies of Science field. Both authors will be discussed at greater length later in the thesis.

Weintraub (1991) follows Morgan (1990) in contemplating the first half of the last century. He explores debates around economic stability, equilibrium, and dynamics. His analysis clearly adopts a constructivist agenda, and subsequently looks for modes of explanation outside of the assumed truth and falsity of the winning and losing ideas. Indeed the book is also a deliberate attempt to demonstrate and popularise this type of analysis. As fiery as this may have been in its time and in its context, the analysis itself does not look beyond the intellectual puzzles of the discipline itself for a mode of explanation. The analysis is not guided by economic truth, but it is still guided by economics.

More interesting is Weintraub's (2002) more recent book 'How Economics Became A Mathematical Science'. Despite the title that book in not, in fact, an attempt at a definitive account of the mathematicalisation of economics. Indeed, in the later parts of the book such attempts are deemed inadvisable. Instead the book seeks to add further nuance to the existing understanding by focusing on the role of the changing image of mathematics in economics, and the changing image of mathematics of itself, in the adoption of mathematics in economics. The book uses histories of numerous actors, including writers of varying professional statures, from the disciplines of economics and mathematics. In the closing sections these histories turn more autobiographical as Weintraub discusses his father's, uncle's, and his own, life stories in economics and mathematics. The book is clearly written using the naturalistic turn of the History of Economic Thought texts that fit most favourably with the mode of analysis adopted here. Indeed, as we shall see, several of the modes of analysis employed by Weintraub have parallels in the Social Studies of Science literature.

The early chapters of the book describe how wider shifts in mathematics impacted upon economics at the beginning of the last century. Weintraub suggests that in 1900 there was still an active debate as to the limits and shape of economics. He cites the German Historical school, the Austrian school, and the American Institutionalists as examples. Such a claim is not controversial, and supportive accounts can be found in Morgan and Rutherford (1998b) Backhouse (1998) and Biddle (1999). The novelty in Weintraub's argument is that this reflected similar debates in mathematics and physics. Furthermore, the resolution of the debates in these disciplines influenced the associated resolutions in economics. He identifies a period of crisis in mathematics during the late 19th Century, based upon, firstly, the inability of Euclidean geometry to explain beyond its limits, secondly, set theories failure to reconcile with Cantor's new notions of infinity, and thirdly, paradoxes in arithmetic and logic. By the 1920s and 1930s mathematicians had reformulated the foundations of their discipline through the axiomatisation and formal modelling of set theory, arithmetic and logic. In physics Weintraub argues the failure of rational mechanics to explain issues of thermodynamics, quanta and relativity led a movement towards statistical mechanics, quantum mechanics and relativity theory associated with Einstein and Plank. Such a shift required a new mathematics employing statistical augmentation and algebra, and Weintraub links this to the changes in mathematics.

Weintraub discusses a number of mathematicians, some keen to promote the adoption of mathematics by economics, and economists. For each he describes their view of what mathematics is, and where appropriate what a mathematical economics should look like. Weintraub's point is that there are a number of competing conceptions of economics, and that these evolve over time. He frequently cites other works in the History of Economic Thought that he believes are misguided because they fail to realise this. Also central to Weintraub's argument is that these different conceptions of mathematics carry with them different notions of rigour. This has parallels to the Kuhnian position, discussed above and again in Chapter Ten, that specific scientific goals imply their own set of criteria for judging successful science (Kuhn 1962).

Weintraub starts his account around 1900 by demonstrating the competing visions of mathematics available to economists. He begins by discussing Alfred Marshall as a representation of English, and in particular, Cambridge University, mathematics. The mathematics taught at Cambridge was a mixture of applied physics, thermodynamics, optics, and geometry among others. It had remained this way for some time, ignoring changes elsewhere around the world, most notably the Continental tradition discussed next. The Cambridge tradition, as perpetuated by the Tripos examinations, identified rigorous mathematics with a substrate of physical reasoning.

Weintraub compares this with the Continental tradition of writers such as Vito Votterra, Vilfredo Pareto, Felix Klein, and Francis Ysidro Edgeworth. This mathematics is associated with the period before and leading into the crisis in mathematics discussed above. This was the mathematics of rational mechanical argumentation where models were to represent as realistically as possible the natural phenomena they represented. They should produce quantifiable predictions that should be tested. The empirical reality of the models provided the basis for their rigour in this image of mathematics.

However, as noted above, this form of mathematics was facing a crisis, and came to be replaced by a formalist mathematics of axiomatisation. Weintraub charts the progressive marginalisation of Griffith C. Evans, a keen student and follower of Votterra, as his arguments for a mathematics of rational mechanical argumentation became less and less acceptable within the Post-War economics profession. Weintraub notes the definitional problems inherent in discussing formalist mathematics, as highlighted through his account of David Hilbert. To Weintraub, Hilbert's views involve two related sets of ideas that evolve across his work. First it is a quest for certainty in mathematics based upon the need for a proof of the consistency of set theory. Second it is about the quest to develop axiomatic formulations in mathematics and other sciences. Here the cogent axiomatic base of a model testifies to its rigour. It was the shift from a vision of mathematic rigour found in the empirical reality of a model to the location of rigour in its axiomatic base that led to Evans' marginalisation. Weintraub notes that Evans', and thus Votterra's, image of mathematics re-emerges again with the rise of Econometrics. Indeed, Evans was a founding member of the Econometric Society in 1932.

Weintraub argues that the introduction and spread of this formalist mathematics in economics was a product of many contingencies of different disciplines and personal accidents and encounters. His account details how N. Bourbaki's formalism entered economics through Gerald Debreu. Nicolas Bourbaki was not a real person, but a pseudonym adopted by a number of, mainly French, mathematicians. Under this name they wrote a book entitled *Elements de Mathematique, Livre I (Fascicule de resultants)* in 1939, (published in English as Bourbaki 1968 *Elements of Mathematics: Theory of Sets*, and termed simply Theory of Sets by Weintraub). This set the agenda for a vision of mathematics that required no relation to the physical world and is reduced to axioms and isolated formulas. The text was intended as a handbook for the mathematician to learn and follow this new way of doing their subject. Debreu, Weintraub argues, advocate this method for economics in his 1959 book *Theory of Value*, an exact equivalent handbook for the economist. Debreu started to work for the Cowles Commission in 1950. The Commission moved to Yale in 1955 and became the Cowles Foundation, and, as Weintraub argues, its graduate students would migrate out to other departments and take Debreu's formalist mathematics with them. He suggests that Debreu's book, which was the most prominently featured of all his contributions when he won the Nobel Prize for Economics in 1983, was successful because of its fortuitous timing when American economics was turning away from its earlier empiricist work. The movement towards a vision of economic rigour framed exclusively in theoretical terms provided an opportunity to stand above many of the ongoing disputes in economics. Weintraub notes how economics lags behind mathematics. By the 1970s the Bourbakist position became a historical relic in mathematics, while economists have only started to question it in recent times.

More interesting analysis is found later in Weintraub's book as he discusses a correspondence between economist Don Patinkin and mathematician Cecil Phipps. Weintraub demonstrates the incommensurability of the two men's positions in their discussion over a proof developed in Patinkin's research. Phipps was a mathematician who regularly studied economics papers to highlight what he considered to be mathematical errors. He claimed Patinkin (1948) had used contradictory assumptions in his article on the derivation of prices in the classical system. Weintraub argues that Phipps could not understand why Patinkin would consider one assumption acceptable while another was not. Similarly, Patinkin could not identify with Phipps' idea that the analyst could select their preferred axioms to produce interesting results. Weintraub argues this demonstrates an incommensurability in their visions of mathematics due to their different socialisations in different discursive practices and persuasive techniques. Both men had different ways of accessing evidence and even employed separate languages. Again here Weintraub is using very similar notions to Kuhn (1962) and some of his terminology, as used later in Chapter Ten of this thesis.

The final section of Weintraub's work that we will discuss, and perhaps that which is most interesting in our context, is his discussion of the acceptance of Kenneth Arrow and Gerard Debreu's 1954 article on the 'Existence of an Equilibrium for a Competitive Economy'. Weintraub asserts that this paper is considered a classic that provides proof that competitive equilibrium existed. Weintraub charts how a paper published in a journal with low readership during a period when few economists had the mathematical competencies to understand it managed to become so successful so quickly. Based on studying advanced graduate student text books of the period, Weintraub argues the proof was accepted by 1958, only four years after its initial publication. His account focused on the refereeing procedures that granted it publication.

Weintraub suggests there was an issue for *Econometrica*, the journal that published the article, in terms of choosing referees. They faced a trade-off between finding someone sufficiently qualified to understand the paper and someone sufficiently impartial to not have a prior view on its findings. Furthermore a similar paper written by McKenzie was also being refereed at that time and the same referees could not be used for both publications. The Cowles Foundation and RAND, the two major sources of expertise in the area, were also excluded as Debreu and Arrow respectively worked there. Georgescu-Roegen was charged with finding suitable referees in these difficult circumstances. He chose William Baumol and Cecil Phipps, the mathematician whose views were shown to be incommensurable with Patinkin's economics by Weintraub earlier in his book.

Baumol thought the paper was important, and should be published, even though he admits he did not read it thoroughly. Contrary to this, Phipps criticised the paper claiming the use of axioms was mathematically incorrect. Georgescu-Roegen said he could not understand much of Phipps' criticisms and suggested that maybe the authors would understand them better. Georgescu-Roegen also refereed the paper himself and argued it should be published. Again he admitted to not reading the paper in much detail, and instead based his decision on the reputation of the two authors. The paper was published, and subsequently Phipps' wrote a letter of complaint, which again was rejected. His letter was rubbished by many of Arrow and Debreu's contemporaries, again referencing the authors' reputations relative to his. Weintraub uses the terminology of Steve Shapin (1994) to characterise Phipps as standing outside the moral economy of truth-makers. His analysis was not trusted, and thus ignored. Shapin's analysis of the social history of truth is used in Chapter Nine of this thesis when discussing value neutrality in macroeconomics.

Like his 1991 work, Weintraub closes this book by suggesting more History of Economic Thought publications should be informed by writers from the Social Studies of Science. He suggests more writers should reject narratives led by notions of economic progress, implying the teleological model identified by Bloor (1973, 1976). Weintraub's account is interesting and comparable to the form of analysis pursued here. His observations are commented upon later in the thesis. For now we will turn to one last detailed discussion of a History of Economic Thought text that also employs the naturalistic turn, before quickly noting some other useful contributions used in this thesis and closing with some concluding remarks, the contribution of Adrienne van den Bogaard.

Van den Bogaard's (1998) book is among the closest to a Social studies of Science style text in the History of Economic Thought. She explores how the Central Planning Bureau (CPB) in the Netherlands developed a macroeconomic model of the economy that 'depillarized' the distinct social divides present in the Netherlands. Dutch society was separated clearly into four pillars, the liberals, Protestants, Catholics and social-democrats. Between the pillars potential for conflict over any issue, especially one as mutually effecting as economic policy, was ever present. Van den Bogaard argues that to circumvent such hostility the CPB developed a macroeconomic model of the economy that bridged these pillars with a specific form of Dutch objectivity. This objectivity meant the ability to maintain one's exclusive identity while stepping beyond it to agree between pillars on national policies. A mathematical model of the economy, most closely associated with Jan Tinbergen, was developed and agreed by all sides.

Van den Bogaard notes that economic knowledge was expected to fulfil a specific role in Dutch society. Here the adoption of a value neutrality, meaning objective knowledge used for policy making, was especially evident, more so than in France and Norway. This was essential to objectify culturally the economy and place it beyond the reach of the four confrontational pillars. By doing so it embodied a new type of economic political debate. The outcomes of policies advocated by political commentators were not available for contention. If a politician claimed their policy would have a certain outcome, and the model agreed, then all accepted the authority of the model. Instead political economic discourse contested which outcomes were most beneficial. This is interesting in the context of this thesis as it adds nuance to our understanding of how value neutrality lends legitimacy to liberal democratic politics, the subject of Chapter Nine. The specifics of the Dutch case are compelling, and do not contradict the findings presented here on the Anglo-American context.

Van den Bogaard's analysis is totally acceptable in the context of this thesis, in a manner that Leeson's (1997, 1999a, 1999b, 1999c, 1999d) and Blaug's (1976, 1991, 1992) are not. Other examples of useful and sociologically informed texts from the History of Economic Thought include Cartwright (1999), Furner and Supple (1990), and Morgan and Rutherford (1998a), all of which are discussed at length later in the thesis. Cartwright explores the uses of models in economics and physics to construct order where there is none, Furner and Supple describes the relationship between economists and policy makers in the U.S. and U.K., and Morgan and Rutherford discuss how the construction of objectivity changed during the early part of the 20th century.

This section has not attempted a complete review of all the relevant History of Economic Thought literature. Instead relevant texts are discussed as they are needed throughout the remainder of the thesis. Instead this section has hoped to demonstrate some of the differences between writers in the field, and employed the theoretical debates that started this chapter to discriminate between those with more or less similarities to the research presented here. In effect, the discussion has located this thesis closest to those History of Economic Thought texts that employ the naturalistic turn. However, this thesis is not itself a part of this literature. This is because is has different goals. While frequently History of Economic Thought texts are intended to forward our understanding of economics, or raise methodological issues within economics, this thesis remains within the Social Studies of Science literature, and subsequently the conclusions do not stop with economics. This thesis is as much about exploring how established Social Studies of Science concepts are manifest in various sciences, in this case economics, and thus commenting upon the nature of these concepts themselves, as it is about describing macroeconomic practice. Examples include established Social Studies of Science concepts such as Interpretative Flexibility (Chapters Five to Eight), Social Interest Theories (Chapter Eight), and Problem Solving Paradigms (Chapter Ten). Another difference between the History of Economic Thought literature and work in the Social Studies of Science is that the former typically assumes a detailed knowledge of economics among its readership. Subsequently the texts can deal with technical issues in a debate at a level of complexity that might be found alienating by the typical audience of a text such as this. Rarely does the History of Economic Thought literature adopt the role of providing a basic understanding of the issues of contention for a lay audience as is provided in the following chapter. Furthermore the authors can refer to alternative areas of economic analysis beyond the empirical focus of their work without the need to explain its content and meaning to reader. In this thesis the History of Economic Thought literature is used to illuminate the discussion where it is interesting but not so complex as to inhibit the understanding of the lay reader.

2.4.1 Concluding Remarks

This chapter has been about locating this thesis in relation to a range of different literatures. Firstly located the thesis theoretically within the Sociology of Scientific Knowledge (SSK). It further commented upon some critiques of SSK by Actor Network Theorists and Discourse Analysts. Secondly it demonstrated commonalities with other contributions from within the Social Studies of Science literature that use economics as an empirical base. Thirdly it discussed contributions to the Social Studies of Finance literature. It argued that these publications were interesting in this context because they are good Social Studies of Science research, but not because there is any inherent shared empirical ground between macroeconomics and finance. Fourthly it located this research nearest to those contributions within the History of Economic Thought literature that employ the naturalistic turn, although noted that there were still differences in approach.

3. Methodological Discussion

3.1.1 Research Goals

Any methodology must be orientated towards the research objectives. Since the original ESRC research funding proposal, the intention has always been to explore the Phillips Curve debate, as a case study of macroeconomics, through a Sociology of Scientific Knowledge (SSK) perspective. The swiftness of the cycle of contest and closure and an exploration of Interpretative Flexibility always lay at the heart of the research.

3.1.2 Theoretical Position

The theoretical position adopted in this thesis was discussed at length in the previous chapter. To restate, the research follows the prescriptions of the Sociology of scientific Knowledge (SSK) and the Empirical Programme of Relativism (EPOR) (Collins 1981a, 1984, 1992). Methodological Relativism and Participant Comprehension are used to interpret the data. We will now explore how these were implemented as we discuss the research design.

3.2.1 Overview of the Research Design

Semi-structured interviews and a literature search of the Phillips Curve publications were always intended to be the methods employed in this research. The literature search was conducted throughout the development of the thesis, although it was also the sole activity pursued for the first eight months of the data collection process. Following this nineteen interviews were conducted with macroeconomists who had had varying degrees of participation in the debate. We will discuss the strategies underpinning each of these in turn. First, however, we will address a methodological challenge lying fundamentally at the heart of this research.

3.2.2 The Issue of Historical Distance

The interest and drama of this research lies in its longitudinal nature, following the life course of a theoretical development in macroeconomics. The current work on the sociology of economic knowledge is still in its infancy compared to the study of other sciences. In this context the contribution of a study with historical breadth is great. An understanding of theoretical shifts over several decades provides stimulus and basis for further contemporary studies. However a retrospective longitudinal study incorporates a barrier to participant comprehension. Historical distance rules out direct contact with the research setting. The cultural dislocation between the respondents and researcher is not only reflected in disciplinary difference, but also a temporal distance from the wider social context through which the debates were played out.

Gaye Tuchman provides a useful commentary on historical social science (Tuchman 1998). She discusses various modes of historical investigation. Included among these is the cliometric approach to history. Here records from the time are employed to deduce the facts as they stood. This can be likened to the empirical documentation of Interpretative Flexibility constituting the first stage of EPOR. The likeness, however, only holds superficially. Where the cliometric historian will use the data to narrow accounts down to a conclusive truth, this research demonstrates differences in the accounts to illustrate potentially available constructions of the Phillips Curve debate. Tuchman continues by examining the potential gains of the cliometric approach. At best it can discover patterns in history. It cannot, Tuchman argues, attribute any meanings to these findings. The written accounts frequently do not provide what is sociologically interesting because to the writers such norms and values are so inherent to their everyday lives that they are not even noticed, merely taken for granted. To address this Tuchman stresses ethnographic goals in historical research, characterising history as the story of lived experience grounded in oral history.

To the benefit of this research, ethnographic methods as well as their goals are accessible, as many of the participants in the debate are available for interview. However, interviewing the informants retrospectively does not entirely remove the problems Tuchman identifies. The respondents themselves are still dislocated from the history of the time. Tuchman argues for the corroboration of evidence to counter this. However there are concerns over this in the context of this research and its methodological relativism. To match the various accounts against each other to try and uncover a central position of truth denies the validity of actor experience. That each macroeconomist has a differing set of experiences and socio-cultural norms conforms to our expectations of reality. That this analysis has illustrated contradictions between the formally published accounts of the debate and the informal accounts as presented in interviews, or indeed between these interviews, is a finding in its own right. The analyst must exercise caution in how evidence is corroborated. Consider the example, present in Chapter Five, where two macroeconomists of opposing sides disagree regarding the existence of data supporting the Friedman/Phelps Phillips Curve. These claims cannot be corroborated. This is because they are derived from opposed positions within the debate. To try to corroborate them and draw the conclusion that there is some evidence for the Friedman/Phelps Phillips Curve would miss the sociologically interesting conclusion. That being, as discussed in Chapter Five, that macroeconomists perceive the relevance of empirical work within a framework where the criterion of good research is its conformity with their expectation. The corroboration here is of the dynamics involved in the debate, not of the statements regarding the state of the evidence.

The central point here is that historical distance is a clear barrier to participant comprehension, and posses a question to the research design of this thesis. Discussion of how these issues are addressed laces the remainder of this chapter.

3.3.1 Analysis of Economics Publications: The Challenges Faced

The literature research was intended to achieve three interrelated goals. The first was furnishing the documentation of Interpretative Flexibility identified as the first stage of EPOR. The second was to provide sufficient understanding of the technical details as areas of dispute to permit effective interviewing. The third was movement towards attaining participant comprehension.

Early in the literature search two inherent difficulties became apparent. Firstly the vast quantities of Phillips Curve literature proved debilitating. Twenty five years of rigorous debate had spawned a plethora of publications that could be relevant. With three and a half years of conducting the literature search, it is not unimaginable that there are still many authors, let alone publications, on the Phillips Curve issues that remain untouched by this search. This is not a weakness. The literature search was never intended to cover every paper in existence, and its failure to do so does not undermine any of its conclusions. Instead it is an issue necessary of consideration in developing the literature search strategy.

The second problem encountered is the lack of any clear distinction between those papers that should be included and those that should not. Initial work with the Economics journals demonstrated a breadth of disciplinary background within the debate. There are a number of papers that clearly by any definition should be included in the study. Phillips's original paper, for instance, and those directly challenging his findings. However there are also papers that develop themes and ideas claiming to analyse the same phenomena as Phillips without making it explicit to the novice reader. To expand upon this it is worth reminding ourselves of the number of issues the Phillips debate can be conceived to be about. Phillips drew an empirical link between the rate of change of money wages and the level of aggregate demand as represented by unemployment. This relationship draws together authors from several previously separate arenas of economic research, including labour market analysts, those concerned with inflation and the price level, policy analysts and control theorists, general theorists or general modellers and anybody making a methodological contribution. Each of these categories is also diverse and undefined groupings of macroeconomists, and it would not be unexpected to find macroeconomists who align themselves with several fields of enquiry. This is further complicated by the historical development of the debate. For example, in the early 1970s the microeconomics of the labour market began to exert a greater influence within the debate. Attention was drawn to the microeconomics of search theory and this was incorporated into some interpretations of the Friedman/Phelps Phillips Curve. From here onwards search theorists were able to contribute to discussions concerning the Phillips Curve debate, but not before. The historical unfolding of the debate draws its own boundaries of inclusion and exclusion, and poses the question of what point in history the substantial contributions of the search theorists should be included in the analysis of economic literature.

3.3.2 Analysis of Economics Publications: Approach Adopted

Given the quantity of literature available and the permeability of the boundaries around the Phillips Curve debate a demarcation strategy was clearly necessary. However it was deemed unwise to attempt developing any structured criteria in advance. Instead a reflexive snowballing sample guided by the principles of participant comprehension was favoured. The lose goals of ensuring the representation of all sides of the debate, covering publications with varying levels of influence, and of fully researching all interview participants were adhered to. Within this framework the quantity of publications allowed a level of flexibility concerning the exact publications chosen. Furthermore the publications were considered in differing levels of detail. Some, for example those discussed in Chapters Five to Seven, were studied at great length to understand exactly how the empirical and theoretical work therein operated. Many others were considered in far less detail. Initially papers were relatively randomly read and browsed to try to position them in relationship to each other. However as participant comprehension extended it became possible to identify various categories of papers more easily. Social fluency allowed the papers to be categorised by their intellectual allegiance, background, influence, and status between path breaking and more normal science. As participant comprehension grew, subtler signifiers allowed these demarcations to be made. A paper could be located within the debate, and the decision to study it further could be based upon identifiers such as the terminology employed, the author themselves or those they acknowledge and cite and the presentation of mathematics and variables they use. Employing these in relation to the publication date to locate them historically further added to the categorisation potential. With these categorisations it could easily be decided whether pursuing the paper further would help attain the specified goals. Exploring and reading literature in a field in which the researcher does not have a background is of course common place in the social studies of science. An example engaging with economic texts is Evans' (1997) exploration of macroeconomic reports and Atkinson's (1996) readings in health economics. A further example studying a different discipline is Clarke's (1998) study of the reproductive sciences through the twentieth century.

A number of strategies for locating literature were followed. Initially very formalistic chronological searches through major journals were employed. These were useful early in the research process but soon displayed their weaknesses. Firstly pursuing this dogmatically would miss important articles published in journals that in this context were on the fringe, for example an early influential paper by Dow and Dicks-Mireaux (1958) in the *Oxford Economic Papers*. Secondly, as a threshold level of participant comprehension was met, it became apparent that a detailed analysis of each paper as they occurred chronologically involved significant repetition of papers with similar characteristics, when the time would be better spent exploring other avenues. We should remember that, under participant comprehension, the familiarity of the researched world view is a signifier of the completion of the research effort. Reading endless similar papers with no gains in social fluency proved no way to proceed, a situation termed 'saturation' in Glaser and Strauss (1967), and is paralleled by the ethnographic literature on leaving the field (Barley 1983, Fine 1983, Maines *et al.* 1980). Finally on this, adopting this method imposed the chronology of the debate upon the research process. The use of these strategies had a lasting impact upon the conduction of both the literature search and the interview schedule.

Another early strategy informing the literature search involved using published surveys of the data (Leeson 1997c, Parkin & Laidler 1975, Seater & Santomero

1978). Initially the content of these articles proved effective in aiding understanding the technicalities of the debate and locating pockets of opinion within it. However, there was a lack of intellectual pay off from these 'secondary' sources which quickly became apparent. Instead the original texts were preferred as they provided more detail and removed the layer or interpretation and categorisation imposed by the summarising author. In this instance the summary articles proved useful bibliographic resources. However it later became important to re-evaluate the role of these papers as, as only became apparent with further participant comprehension, some, Parkin & Laidler's work in particular, were influential substantive contributions to the debate themselves. The quantity of publications was so vast that many participants on the debate read only a fraction of those available, and review articles were attempts by some to address this in a critical light. This practice is not limited only to macroeconomics. Collins (1999) has demonstrated the lack of reading among the core set in gravitational wave research.

As suggested in the previous paragraph, following up bibliographic references proved fruitful. However this only became the case once some participant comprehension was attained. The numbers of references in each meant that attempting to snowball blindly off each in a systematic manner would swiftly avalanche out of control. Only once the fluent demarcation techniques discussed above were available could the citations be used sensibly. As time progressed this became a valuable strategy for discovering relevant literature.

The participants in the debate also drove the identification of relevant literature. Firstly it was essential to be familiar with their work before interviewing them. Where their C.V. was not available on the internet it was requested before the interview. Furthermore a number of the participants were willing to provide me with copies of their articles either in the interview or in advance. Finally interviewees often suggested texts or areas of literature worth exploring. As well as aiding discovering new literature, this also frequently confirmed the validity of the literature search to that point. Rarely did the respondents identify any sections of literature that were not already under the purview of the literature search. This is important as it demonstrates the success of the search of suitable literature in covering the issues considered relevant by the participants themselves. Had the respondents frequently identified sets of literature that had not been covered the sampling techniques used to the conduct the analysis of the debate would have to have been questioned.

3.3.3 Analysis of Economics Publications: In Retrospect

With the benefit of hindsight the literature search receives a positive evaluation. Granted many texts were read that were either not relevant to the thesis or were perhaps too repetitive in form to those already read, given the vastness of the literature. However these mistakes can only be considered part of the attainment of participant comprehension and an inherent element of any task of this nature. Furthermore as a strategy for documenting the Interpretative Flexibility in macroeconomics Chapters Five to Seven stand as testament to its success. Finally the interviews were all conducted with the background knowledge necessary.

However the literature search did have a further impact that could be considered both a benefit and a weakness of the research. It stretched for many months before the interviewing schedule proceeded. It was always essential that the literature was engaged before interviews commenced. However, with the benefit of hindsight, it is obvious that the lengthy pursuit of the literature search was also a product of a nervousness experienced by the novice researcher about conducting the interviews. Many researchers report similar feelings of apprehension in their work, for examples see Walford (1994). Whether the prolonged reading period was a sensible tactic to empower the research and address the issue head-on, or an excuse to hide behind, is a matter of interpretation. Most likely both have resonance. In any case it occurred, and arguably to the benefit of the first two analytical chapters of the thesis.

We will now explore how this was overcome, as we review the interviewing segment of the research design.

3.4.1 Interviews: Sampling

Like the literature research the interview schedule employed snowball sampling guided by participant comprehension. The broad goals of ensuring all views were represented and the inclusion of respondents with all levels of participation in the debate were adopted. Within these goals the identification of interviewees was guided largely by practical concerns.

Originally it was anticipated that the limited resources available for the research would imply the interviewees would almost completely be drawn from those still resident in the U.K.. Locating these macroeconomists began by conducting internet searches on the limited information on authors present in bibliographies, as paralleled in Odendahl and Shaw (2002). Fortuitously, against the trend in macroeconomics publications, one of the Phillips Curve literature review articles mentioned above opted to list first names in their bibliography (Seater & Santomero 1978). Should the macroeconomist still be active in the university sector it is likely that an internet search on their full name and the term 'econ' would provide their homepage or a page like a conference programme including their current institution. The limit of this technique is that retired macroeconomists are difficult to locate. However, the five retired respondents all maintained links to their old departments and were located through these. Initially the internet searches provided ten U.K. based economists. Of these one died before contact was made (Bernard Corry), one declined to be interviewed (Lord John Eatwell), two pulled out during the negotiation process (Max Steuer & David Metcalf), and one was not contacted for an interview (Lord Meghnad Desai). Sometimes after repeated tactful persuasion, the remaining five agreed to be interviewed. Subsequent to this several other U.K. economists were located through alternative means, and five were contacted. Four were interviewed and one did not respond to the request (Patrick Minford).

In 2001 the Annual Conference of the Society for the Social Studies of Science (SSSS) was held in Cambridge Massachusetts. Many distinguished macroeconomists work in the Greater Boston area. Attendance at the SSSS could be combined with data collection. The funding of a trip to North East America provided the opportunity to contact a new league of famous respondents. Six hugely influential U.S. macroeconomists were targeted, including four Noble Prize winners. One of the six did not respond to my request (Robert Barro) and one declined to partake in the research (Paul Samuelson). The remaining four participated enthusiastically in the research.

This experience had a further impact upon the sampling procedure. The American data were compelling and prompted the pursuit of telephone interviews with North American based researchers. Initially rejected as a problematic setting for a qualitative interview, the position shifted to a preparedness at least to experiment with interviews conducted by telephone. Not bound by the limitations of geography this was the first time the sample selection could truly be led by judgements based upon participant comprehension. Ten requests were made and five interviews resulted. The four of the five who were not interviewed did not respond to the request (Armen Alchian, Robert Barro, Robert Gordon, & Thomas Sargent). The remaining one could not agree a suitable time for interview since he was out of their country for an extended period (Michael Parkin). The interview requests were sent with the expectation of a less than complete response rate, and since the sudden explosion in potential respondent numbers, this was no longer a problem. The negative responses were not pursued with as much vigour as earlier had been the case. Again central figures in the debate were pursued for the telephone interviews, this time complemented by those of more average prominence. Appendix One contains a table showing whether each participant was interviewed by phone or face-to-face.

The sampling strategy worked well for the most part. Nineteen interviews were conducted. Of these respondents, two had never published in the debate (Collard & Winnett), two had published only one paper (Copeland & Kuska), three were active in the debate for around five years and then left the core-set (Cowling, Stoney, & Taylor), and the remaining twelve all had lengthy engagements with the debate. Of these twelve, four are Nobel laureates and general macroeconomic theorists (Friedman, Modigliani, Solow & Tobin), three are central innovators of the incarnations of the Phillips Curves (Friedman, Lipsey & Phelps), and the remaining six made notable contributions to the debate (Laidler, Mortensen, Peel, Perry, Sumner, & Zis).

In an alternative split of the respondents, ten published work supporting the Phillips/Lipsey Phillips Curve. A further eight wrote supporting the Friedman/Phelps

Phillips Curve. Finally two were public advocates of the Rational Expectations Phillips Curve. We should note that, since macroeconomists do change their position in the debate, these categories are not mutually exclusive. A table detailing this is in Appendix One, and the information is repeated in the respondent biographies in Appendix Two. The argument could be made that the inclusion of only two respondents supporting the Rational Expectations Phillips Curve is a weakness of the sampling strategy pursued. The position here is that this is not so. However the criticism is sufficiently strong to warrant a defence.

Rational Expectations advocates were, in the main, non-responders or refusers. The small number of Rational Expectations advocates is not due to their neglect in the sampling procedure. Six were contacted. However, even with sometimes frequent repeat attempts, the response rate remained very low. Robert Barro, well known rational expectations theorist pursued for the visit to North East America, was contacted six times, and never once replied. Patrick Minford, the leading British rational expectations advocate, was contacted twice with no reply, although he was willing to cooperate with the research of Evans (1997). Robert E Lucas Jr, the leading rational expectations exponent and Nobel Prize laureate, responded with a photocopy of his autobiography and instructions to read his publications and search the internet for existing interviews with him. He did, however, suggest that if after this there were further questions they could be posed to him, although the suggestion was this would be in written form. This opportunity was not taken up. Thomas Sargent, co-author with Robert E Lucas Jr, did not respond to two requests.

The second defence highlights two aspects of the debate that lessen the apparent severity of the criticism. The fact that only two rational expectations proponents were included in the sample does not mean there are only two respondents able to comment on the rational expectations debate. Indeed, as the most recent cycle of contest and closure, all nineteen respondents were able to comment on it, and in all twelve made contributions to the debate. In this context the rational expectations debate is the best represented of the three.

Furthermore, the rational expectations cycle of contest and closure is studied in a different form from the preceding two incarnations. In the time period explored both the Phillips/Lipsey and the Friedman/Phelps Phillips Curves became the dominant orthodoxy and subsequently contracted. Rational expectations, however, did not contract during this period, and thus can be explored less comprehensively.

The final defence, however, is the strongest. The decision to cease conducting interviews was led by the central methodological tenet of this research, participant comprehension. The last few interviews had felt unsatisfactory, they seemed to contain no surprises. The reason for this is that participant comprehension in the issues being discussed had been accomplished. The interview data no longer surprised because the socialisation process into the aspects of their form-of-life discussed was complete, again the point of 'saturation' as argued by Glaser and Strauss (1967) and the ethnographic literature on leaving the field (Barley 1983, Fine 1983, Maines *et al.* 1980). Once this became apparent it was decided to pause the data collection while analysis was conducted with the view of recommencing should further questions arise. Analysis proved the richness of the existing data and continuing the interviews was deemed unnecessary. It is still the argument here that conducting more interviews would not improve, or even change the research in any meaningful way. The data collected so far give no reason to believe the norms and values experienced by rational expectations advocates are any different from those from other positions. They are simply orientated towards the maintenance of a different paradigm.

3.4.2 Interviews: Negotiating Access

The respondents' contact details were all available in some format on the internet. This provided a range of possibilities for negotiating access. No single technique was used as the specifics depended upon the individual. The initial intention for the U.K. interviews was to e-mail each respondent informing them of the research, and the interest in interviewing them. The e-mail did not request a reply, but instead informed the respondent that they would be telephoned in the next few days to discuss the possibility of a meeting. The idea of constructing a website that the respondents could browse for further information was considered but rejected because it proved difficult to create a suitable generic text that would be useful.

In practice the early interviews were arranged in exactly this way. Later some interview requests were preceded with a mailed C.V. request including a stamped addressed envelope. It was felt that this would soften the respondent to the idea of participating in the research. However in practice these C.V. requests had a poor response themselves.

Once the potential of the North East American interviews became apparent this strategy was suspended. The first reason for this is that the unforgiving time difference problematised the telephoning of the potential interviewees. Secondly, these respondents were higher prestige individuals, and a more formal method of communication was deemed more suitable. Thirdly, four of the six contacted respondents did not have e-mail addresses published on the internet. Contact with departmental representatives of three of them, all at MIT, suggested a fax would be the most suitable method. In practice faxes were sent with a hardcopy following. The telephone interviews were organised through e-mails, faxes and letters, depending on the availability of an e-mail address and the perceived prestige of the individual.

The experience of negotiating access was varied. In some cases the respondents were reluctant and required repeat e-mails and phone calls. A balance needs to be struck when making repeat contacts between persistence and proving rude or irritating. Non-response could represent a decline to partake, busyness, uninterestedness, incorrect contact details, holiday or being away from the department, or a failure of the method of communication. Around a third of respondents also required assurance that their participation would be useful, frequently citing a small scale involvement at the time or the length of time since they have been active in the field. It was necessary to inform them that the views of people with all levels of involvement in the debate was essential to the research.

Also in a number of cases arranging a time for the interview proved problematic. Despite the flexibility of the research timetable many interviews could not be conducted for some time after the initial contact, on occasions stretching into several months.

Although sometimes requiring persistence, the negotiating strategy was suitably conceived and, given the realities of response rates in social science, successful in enrolling participants.

3.4.3 Interviews: Conducting Face-to-Face Interviews

The interviews were intended to be, and were in practice, semi-structured qualitative interviews. The longest stretched for nearly three hours (George Zis) and the shortest slightly over an hour (David Peel). Most ran for around an hour and a half. The interviews were all conducted in the respondents' departmental office, except one held in their home (Franco Modigliani). All were recorded on minidisc.

Semi-structured interviews were adopted for a plethora of reasons. Their applicability was obvious throughout, and no alternative interview technique was seriously considered. As a research project informed by participant comprehension of a historically dislocated period, semi-structured interviews provided the best window into the norms and values experienced by the participants during the Phillips Curve debate. The literature on the strengths and weaknesses of this interviewing style is broad (Atkinson 1990, Coffey & Atkinson 1996, Denzin & Lincoln 1994, Gilbert 1993, Hammersley & Atkinson 1995, Spradley 1980, Warren 2002). The consensus on semi-structured interviews, and the reason why they are used here, is that they provide the opportunity to gain an account of the values and experiences of the respondent in terms meaningful to them. The agenda brought to each interview by the researcher ensures all necessary topics are discussed, while allowing the interviewee to introduce issues they conceive as important. Through prompting and probing the interviewer can elicit detailed responses to their questioning, and challenge the respondent to make explicit their taken-for-granted norms. In the context of this research the semi-structured interview fit well with the practical limitations of geographically dispersed interviewees who could not spare lengthy periods of time.

The interviews with economists were elite interviews. Whether elite is defined in terms of social position relative to the researcher conducting the interview in these instances, or relative to the average citizen in Anglo-American society, they are still clearly in a position of power and raised social stature. Indeed Zuckerman (1996) defines her interviews with Nobel Laureates as engaging an ultra-elite.

There is an establishing literature on conducting elite interviews. Charles Morrissey highlights the importance of flexibility in time tabling interviews with members of busy elites (Morrissey 1970). Susan Ostander prioritises the adoption by the interviewer of a dominant position in the interview through non-verbal cues and direct questioning (Ostander 1993). Others stress the importance of expecting gate-keeping questions, as frequently the elite status is based upon the possession of knowledge and prestige (Cassell 1988, Dexter 1970, Hunter 1993). Odendahl and Shaw (2002) provide a very full exploration of the elite interviewing literature. Their discussion highlights the breadth of social groups often defined as elites in sociological research. With such breadth comes an equivalent range of issues confronting researchers and strategies for addressing them. Scientists would fit into what they call the professional elite, a category still sufficiently wide to include the clergy, lawyers and celebrities. Even as a subset of a wider literature, the elite interviewing texts still consider a broad range of participants with individual constraints and preferences.

Zuckerman (1996) provides a detailed account of the methodological issues closely related to those faced here in her study of Nobel Prize winners. As noted above she considers her respondents to be an ultra-elite because they are considered an elite group among other high ranking individuals and organisations that many of the studies noted by Odendahl and Shaw (2002) would consider elite groups in their own right. Subsequently, to use Zuckerman's terms and definition, a subset of my interviews were conducted with an both members of the elite and ultra-elite. Many of the constraints and solutions faced by Zuckerman echo those of this thesis, including the problems of locating specific individuals as opposed to representatives of a social group, and the importance of studying up on their histories and works in preparation for the interviews. Zuckerman, however, did receive more active indications of positive support for the interview than in the case of this thesis, examples being offers to book hotel rooms and detailed travel directions. Furthermore, Zuckerman often faced a greater degree of gate-keeping questions testing her knowledge of their science than was the case in this thesis. Zuckerman notes that such testing questions were a continuous presence throughout many of her interviews.

However despite the similarities in respondent, the one particular contribution that stood out from the broad elite interviewing literature, and has been central to informing the approach adopted in the interview setting, was based on research with a different subsection of Odendahl and Shaw's (2002) professional elite; Anglican clergy-men and women. Alan Aldridge (1993) argues that many methodological discussions of elite interviewing focus upon endemic differences between the researcher and participant. Instead he suggests an awareness of both commonalties and divergences better supports effective interviewing. Aldridge's research discusses how in the localised setting of the Anglican clergy gender has a great significance due to the sensitivity of the ordination of women into the priesthood at the time. An awareness of issues like this, and their negotiation in the localised setting, is essential to attaining rapport. The paper elaborates this point by discussing the nature of prestige. Elite status is considered a product of localised social negotiation, and not a stable social hierarchy. Thus an understanding by participants and interviewer of the cultural positioning of each other, both in difference and similarity, facilitates a better interview. Aldridge opted to employ unstructured interviews. He considered the more apparent scientistic nature of the structured interview would cause tension by pointing to socially poignant differences between social science and theology. Equally he argues there are many cultural similarities between clergy men and women and social scientists. Here he identifies class specifically. There are also differences among the clergy that need consideration. For example Aldridge, as a male researcher studying attitudes towards the ordination of women, focused upon negotiating gender positions. Good rapport then, according to Aldridge, must be negotiated through a balanced awareness of the characteristics of both interviewer and interviewee.

In the case of this research the similarities are plentiful. Both interviewee and interviewer were social scientists sharing a prevailing liberal academic attitude. Their are shared expectations of research processes manifest in expectations of research competency and intellectual curiosity. All interviewees were also middle class Caucasian men, as is the researcher. This demographic is not surprising, indeed, Breit and Hirsch's (2004) work collating auto-biographical texts of eighteen economics Nobel Laureates has an entirely male group of participants.

Obvious differences are age and experience. The oldest participant is sixty five years older than the researcher, and the youngest was maybe twenty years senior. Odendahl and Shaw (2002) note that a big age difference can make it difficult for the interviewer to be taken seriously. Furthermore, from Nobel Prize winners to established lecturers, the status gap in the interview setting was also pronounced. This is considered another barrier by Odendahl and Shaw.

In all research the interview setting is inevitably a social interaction. Given this the sets of categories actors bring to any interaction will be mediating the interviewing relationship. In the light of the characterisations presented above it appeared clear that, as a young PhD researcher, the existing category the respondents would most likely use to locate the interviewer was either an undergraduate or postgraduate student, a similar argument is presented in Pearson (2002). This is true of even the most famed participants in the debate, as even though academics at the very top of their profession often stop engaging in many of the tasks undertaken routinely by the majority in their profession, such as administration and undergraduate teaching, they still take very seriously their role in PhD supervision (Zuckerman 1996). A relationship mimicking the supervisor/PhD student form had clear benefits for the interview setting, but also needed to be moderated closely. In its favour it instantly makes the interviewee comfortable discussing the topics and prevented the age gap becoming a barrier as feared by Odendahl and Shaw (2002). All the respondents were articulate and free speaking individuals. There was no need for techniques to provoke conversation. Potential problems of adopting the pattern of a lecture/student relationship were tendencies for the interviewee to lean towards teaching the technical issues as opposed to placing values upon them. Certainly in some of the early interviews this was well valued, but could also be a problem that has led to some occasionally lengthy periods of interview being essentially useless in the technical sense of not relevant for the research questions pursued here. Another concern was the potential for the teaching voice to depersonalise the account, and often the interviewer would have to resituate the conversation onto the personal position of the interviewee in the debate, rather than recounting the consensual 'perceived wisdom'.

The final issue is that the many respondents were confident speakers, familiar with adopting the leading role in a conversation and dictating the topic. In numerous instances the respondent would want to draw the conversation away from the Phillips Curve to alternative or contemporary debates. In these instances the interviewer would have explicitly to reassert the topic or lengthy periods of the interview would be off topic. However on occasions it was considered more fruitful to allow the respondent to express these views to, if we can risk dropping into the vernacular, get it out of their system. The interviewer, in the role of the young researcher, listened politely to the deviation and then steered the respondent back to the topic.

Before each interview a schedule was devised. These were specific to the individual in response to their own experience, as paralleled in Zuckerman (1996). However the general template followed the chronology of the debate and the professional life history of the respondent, as discussed by Atkinson (2002). This was preferred as a response to the issues of historical context discussed above. Furthermore to aid the respondent contextualise the debate and provoke memory, copies of a number of their articles were brought to the interview. Often the interviewees reread their texts for the first time in many years. In practice this proved not only to provide a contextual awareness but also increased the participants' enthusiasm for the interview. Several expressed enjoyment in recalling their earlier career in the closing exchanges of the interview.

The format the interview schedule adopted changed through the research process with the progressive attainment of participant comprehension and the growing confidence of the researcher. All research is a lived experience and this thesis is no exception. In the early interviews a gold letter etched Cardiff University A4 ring binder was taken to each containing quantities of photocopies and notes as well as lists of prearranged questions should the interviewer struggle. This symbolism may have aided confidence but was not necessarily of any practical use. By the later interviews social fluency deemed an A5 jotter pad sufficient and the schedule amounted to little more than approximately five topic titles and some specific background information. The researcher's competency was now represented by the interviewer's manner as opposed to the amount of paper brought to the interview.

3.4.4 Interviewing: Conducting Telephone Interviews

The decision to conduct telephone interviews was inspired by the success of the fieldtrip to America. Prior to this they were considered unsuitable as rapport and the subsequent depth of qualitative questioning would be hard to maintain on the telephone. However, it was decided that an attempt to gain this potentially rich data was a worthwhile pursuit. This resulted in five interviews that were largely successful. The academic literature on research methods explores telephone interviewing of non-elite samples, and elite interviews done face to face, but there is no standard reference work on elite telephone interviews. This is not because it has never be attempted before, Wasserman's (2000) work on eminent women scientists being one example making use of telephone interviews. However disadvantages of relying on methodological texts written for non-elite settings is obvious in reading Shuy (2002). Here the advantages and disadvantages of telephone interviewing are discussed. The observation that is perhaps Shuy's most central still resonates in the context of this research, that telephone interviews are usually cheaper and quicker to conduct as they remove geographical limitations. Indeed this is more pronounced in this research than in many of the cases discussed by Shuy as here the geographical boundary is the Atlantic Ocean and vast areas of the U.S.A. and Canada. However, many of Shuy's benefits of telephone interviewing have little utility in this research, examples including researcher safety and the standardization of questions.

Given the state of the elite-telephone interviewing literature, a commentary on the issues raised in the use of telephone interviews in this research is provided below. Each issue is considered in turn.

Interruption: Due to the lack of visible cues any out-of-turn utterance became a direct interruption. Frequently the utterance was not heard properly, as the respondent was also speaking, and the question would be ignored where they would not have been in a face-to-face setting. However it was not perceived that the participants considered such interruptions as rude.

Topic Control: The status of unsolicited utterances as interruptions altered the nature of probing and shaping the conversation. Small inflections would either be ignored or undermine the fluidity of the conversation entirely.

Lack of Visual Communication: The above points are compounded by the lack of visual communication. Firstly, interruptions were not observed before they were spoken. Secondly, the normal role of non-verbal communication in directing the conversation towards the listeners needs was absent. For instance in face-to-face communication the speaker can respond to expressions of interest or confusion by the listener. The later is especially relevant in elite interviewing of this sort as the subject matter is often highly specialised and expressed in a multitude of different terms. Sometimes clarification on technical meanings was forfeited rather than risk undermining the fluidity of the speech.

Articulation: There was a need for a clearer articulation of the questions than that necessary in face-to-face communication. Fragmented or improvised sentences were less tolerable than situations where hand and facial gestures can consolidate their meaning. However the need to pause and articulate also caused problems. Lacking the visual cues to demonstrate the articulation process the respondent would take the instant non-response as a request to continue to speak.

Holding the Telephone: A hands free telephone was not available for the interviews. Holding the telephone introduced more unanticipated problems than any other influence. Having only one spare hand problematised writing notes and reading preparatory literature. This is compounded as the hand typically used to hold the telephone is also used to write. Other seemingly insignificant issues proved consequential, for example removing the pen lid and having a drink prepared suitably in advance.

Controlling the Environment: Unlike the face-to-face interviews that were held in the respondent's space, the researcher could organise his space as he felt comfortable in advance. However there is no control over the space of the respondent. It was decided that at the beginning of each telephone interview the respondent was asked if he was ready, or if he would prefer to be called back after a short amount of time.

This allowed them to prepare his environment so he to could be comfortable for the duration of the interview.

3.4.5 Interviewing: In Retrospect

The interview process was intended to provide the researcher with participant comprehension of the values macroeconomists associated with the use of theoretical and empirical resources and political influence in the Phillips Curve debate. There is no greater testament to the attainment of this than the decision discussed in the sampling section to stop collecting data. At this point the interviews were providing no further information or surprises. The responses offered were either predictable in exact terms, or in a range of potential categories. Although initially this was wrongly interpreted as a failure by the interviewer it was quickly realised that this signified his socialisation into the macroeconomists' world view on these issues. Participant comprehension had been achieved. Although using different language, this has parallels with Glaser and Strauss (1967), and the ethnographic literature on leaving the field (Barley 1983, Fine 1983, Maines *et al.* 1980).

3.5.1 Analysis

The leading principle of analysis has been discussed at length throughout this chapter. The attained participant comprehension has informed the arguments presented in this thesis. To remind ourselves, participant comprehension is the immersion of the researcher in the practices and values of the participant. It is the socialisation of the analyst into the form-of-life experienced in the research setting. Although the ideal form employed to pursue participant comprehension is active engagement, as opposed to unobtrusive observation, in the research setting, Collins (1984) argues an interview is a suitable proxy. Indeed, in this case where historical distance obstructs access to the research field itself, interviews are the optimal obtainable method.

Participant comprehension involves a set of implications for the use of transcripts not present with other similar techniques, such as ethnographic participant observation or interviewing (Collins 1984). Under these methods the field notes and interview transcripts are considered the data upon which the analysis is based. This is not the case with participant comprehension, as the data collected is the experience of participant comprehension. Instead the interview transcripts are used to illustrate the data and provide ideas for considering the experience of participant comprehension.

The transcripts and recordings were used in two ways as a source of inspiration for analysis. Firstly they were read and listened to in their entirety repeatedly. This aids the understanding of the accounts of specific issues within the wider context of the interview as a whole. Furthermore the transcripts were coded into broad themes. The nature of these themes changed as the research progressed. Examples of codes used are 'expressions of making own empirical work', 'expressions of evaluating others empirical work', 'expressions of contact with computer technology', 'expressions of the passage of tacit knowledge', 'expressions of value neutrality', and 'expressions of policy influence'. However these categories were not used in the systematic patterns of code and retrieval typically associated with qualitative research (Mason 1996). The codes were used in a more simplistic format than the frequently complex and sweeping algorithms employed by Computer Assisted Qualitative Data Analysis (CAQDAS) programs (Fielding 2002, Seale 2002). The codes were used to prompt and question the development of analytical frameworks in which the data could be characterised, but not produce the framework itself. The analysis was continuous throughout the data collection period. The simultaneity of analysis and data collection allowed a dialogue between both activities as discussed by Charmaz (2002) in her discussion of grounded theory. Early suggestions of findings can inform further research questions and allow space for points to be clarified. Furthermore the codes used in analysis can also be refined in response to the data collection process. The codes listed above are framed by the researcher's location in the Social Studies of Science field. Charmaz calls this the use of 'sensitizing concepts' in defining codes and frameworks of understanding, a practice that must be pursued reflexively to ensure the maximum benefits are realised. Acknowledging their role allows better informed decisions regarding each concepts continued use or abandonment.

The limited role of code and retrieval strategies lay the burden for analysis clearly in the creative thinking process of the analyst. Reflecting on the comprehension gained and input from the Social Studies of Science literature provided potential frameworks that could operate within economics. Once an idea became apparent, thought experiments regarding its applicability were possible. Adopting the role of a naïve realist as stressed by Collins & Yearley (1992a, 1992b) granted the transcripts, both as codes or considered in completion, autonomy to question or lend support to the framework. In instances where the data did not clearly conform to the framework as it stood judgements would be made regarding whether the framework required modification or rejection. An example is a point raised by interviewee Mike Sumner's discussed in Chapter Nine regarding the acceptance by the 'Manchester School', to which he belonged, of the Friedman/Phelps Curve before the theoretical device detailed in this thesis as the 'problem solving mechanism' was in operation. In this case the judgement was made that a additional theoretical concept, regarding the ongoing production of marginalised macroeconomic work, was considered a suitable amendment that encompassed Sumner's point and improved the analytical framework. A contrary example is present in Chapter Eight where the political interest model employed by Shapin to explore Edinburgh Phrenology is rejected after its incongruence with the data proved immense (Shapin 1975, 1979). These are both prime examples of the reflexivity needed when employing sensitizing concepts as stressed by Charmaz (2002).

A final element to the analysis was the conformation of quotation validity by the respondents themselves. Excluding James Tobin, each quote published here was cleared by the interviewees as representative of their opinion. They were not, however, asked to comment on the interpretation.

3.5.2 Analysis: Dealing with Quotations

In interview each respondent was informed that they would be contacted regarding any of their quotations used in any publication including this thesis. They were offered anonymity on the quotes and the opportunity to make any comments on the content they wished. Each was informed that at least three attempts to contact them would be made regarding the quotations and that if they did not respond within a month it would be assumed they were happy for the quotations to be published. All agreed to these terms.

When the participants were contacted for the thesis most responded swiftly and positively. Some required repeat contacts. Most noted some changes they would like made to the quotations. The quotations had already been formalised to remove unnecessary repetition and short comments tangential from the point. However some respondents requested further tidying of the text. Others requested clarification be made to the context of the quote. These requests were happily granted as they did not undermine the sociological analysis of the quotations.

Prof. James Tobin dies between his interview and the completion of the thesis, so could not be consulted about the extracts from his interview. All his quotations appear edited to remove excessive repetition and deviations from the point. Other than that they appear as in the transcript. Verhoeven (1992) faced a similar issue when publishing material with the late Erving Goffman. The death of an interview participant prior to publication of quotations requiring clearance cannot be a rare event in social research as a whole. However what makes these cases distinct is the notoriety of the respondent and the attribution of the quotations.

3.5.3 Analysis: In Retrospect

The best criterion for success of the analysis is in its findings. It is the contention of this thesis that it has addressed directly the stated aims of the research proposal and the introduction. The analysis has proved interesting, focused and novel. Although numerous alternative locations of exploration could have, and in some cases were, developed as analysis, the central issue of accounting for the cycle of contest and closure in macroeconomics remained central to the text as presented here.

The attainment of participant comprehension in the issues of interest is clear, and the analysis process has formalised its insights to the best of the researcher's ability.

3.6.1 Ethical Considerations

This research has been conducted in accordance with the spirit of the Statement of Ethical Practice of the British Sociological Association throughout (British Sociological Association 2002). The statement contains detailed guidelines on the ethical commitments of social science researchers. Here we will discuss only those that have bearing upon this research. Researchers have the ethical obligation of professional integrity. In accordance with this the research has reported the findings accurately and truthfully. It has always recognised the boundaries of professional competence and has made it clear when these have been approached.

Researchers have an ethical obligation to the research participants. Full consent was always attained from each interviewee. Each was made aware that they might be quoted in publications and that they would be contacted before this occurred. They were aware they could refuse to partake or stop the recording when they wished. Each was aware of their right to anonymity. The interview recordings have been held in secure locations to maintain confidentiality.

Researchers have an ethical obligation to their peers. The research has been conducted to ensure good relations between the interviewees and the professional reputation of sociology. This is to ensure any subsequent research with the same respondents, or those in communication with them, is not jeopardised.

Researchers have an ethical responsibility to their funding body. The research has been conducted to the high professional standards expected by the funding body, in this case the Economic and Social Research Council. The thesis will be delivered within the stipulated time limit and in line with the accepted research proposal.

3.6.2 Ethical Considerations: In Retrospect

In some respects the ethical considerations faced in this researchers were less pronounced than in other cases. Examples would be instances where the researcher is faced with placing themselves in physical or emotional harm (Hudson 2003), or where the researched group are emotionally vulnerable (Scott 2003), young (Wilby 2003), or in sensitive social positions (Hall 2003). However the research presented here dealt with increased issues of informed consent because the participants were named and well known. It is felt that this issue was sufficiently tackled by two mechanisms. Firstly, each respondent was clearly informed that they may be quoted in the interview setting. Secondly, every quotation included in this thesis, excluding those by James Tobin as discussed above, have been explicitly cleared for publication in this thesis by the respondent. They were contacted by e-mail or fax with written versions of each quotation and allowed to comment upon, and as noted above where they wished edit, the quotations in their own time. Given these protections in the context of an intelligent and literate set of participants it can confidently be argued that informed consent has been achieved.

4. The Story of the Phillips Curve Debate

4.1.1 Introduction

In this chapter we chart the progress of the Phillips Curve debate, and provide technical explanations of the main concepts in operation. It is essential that the reader understands the purposes of this chapter, and equally what it does not purport to do, so that they do not misinterpret its contents. Firstly it must be stressed that this is not a full historical account of the period, nor does it need to be. Instead it provides the backdrop against which the analysis in the remaining chapters is conducted. For this reason there is also no attempt at sociological analysis in this chapter. It exists simply to provide the reader with an understanding of the technical issues discussed later in the thesis and provide some historical and political context. The topics explored are only those that are used later in the thesis, and subsequently those issues in the debate that do not form the substance of detailed analysis are only briefly considered. To attempt otherwise would necessitate a thesis in itself. Furthermore, where an exploration of the technical aspects of an argument are an essential element to a theme raised later in the thesis, the specifics are limited in this chapter to avoid repetition.

Achieving this objective encounters an inherent tension in the presentation of the Phillips Curve story. This is the trade-off between simplicity and accuracy. It is assumed that many of the readers of this thesis will have no prior knowledge of economics. It is to these readers that this section is mostly orientated. It is essential that readers of this thesis understand some of the technical macroeconomics discussed. However the reader is certainly not required to have a fluency in the subject, or what Collins and Evans (2002) call 'Interactional Expertise'. The discussion in this chapter has been positioned on this spectrum closer to simplicity than accuracy. It is written to provide the reader with the level of understanding necessary to appreciate the sociological themes developed in later chapters.

There are further issues associated with the portrayal of an accurate account. The experience of researching this thesis has demonstrated that competing accounts of events exist both amongst the respondents and the historians of economic thought who do strive to produce accurate accounts. Just two of the many examples of inconclusive debates in the history of economic thought literature are, firstly, that considering the role of Keynesian thought in British economic policy in the 1950s (Hall 1989, Weir 1989, Howson 1993, Peden 1990, and Booth 2001), and secondly, that concerning the nature of the oral tradition of the Quantity Theory at the University of Chicago (Patinkin 1973, Laidler 1998, Tavlas 1998, Hammond 1999). Since debates over the correct representation of the history of economic thought exist in these contexts it would be unreasonable to expect such disagreements to be consolidated in this thesis.

A further decision on the presentation of this chapter worth making explicit is the exclusion of any mathematical symbolism. Although macroeconomists do engage in algebraic representation of their theories there is no need for it here. This is because, firstly, there are often numerous competing forms an algebraic representation can take. Also it would certainly be an unfair expectation to assume all readers would be fluent in such expressions. Finally the lack of mathematics at this stage does not devalue the readers understanding of the economics discussed or the sociological analysis that follows.

For those seeking a more detailed account of either the technical or historical details alternative sources are referenced in the text. Initial suggestions for further reading are: on the history of economic debate, Backhouse (1985); on American economic history in the period under investigation, French (1997); on British economic history in the period under investigation, Cairncross (1992); and as an introductory guide to the technical aspects of the debate, Snowdon, Vane and Wynarczyk (1994). Furthermore, since any generalizing or accounting action inevitably involves some loss on accuracy, the curious reader is also directed to the original publications referenced throughout.

The chapter is structured according to four cycles of contest and closure, starting with the pre-Phillips Curve position and followed by the three incarnations of the Phillips Curve. Each section discusses both the important technical aspects of each theory with some wider intellectual context and the wider Anglo-American economic policy concerns of the time. These policy discussions focus upon those policy issues concerning inflation and unemployment, the two main variables at stake in the Phillips Curve debate, although wider issues are also included where valuable. To repeat the above point, these sections are intended to furnish the readers understanding but not provide definitive accounts. We start by locating the historical precedents to the Phillips Curve debate.

4.2.1 Keynes and the Classicists

The nineteen thirties saw the emergence of a radically new orthodoxy in macroeconomics. John Maynard Keynes' seminal 1936 book *The General Theory of Employment, Interest and Money* introduced a set of agendas and frameworks that changed macroeconomics for decades to come. Keynes' central propositions were that economies do not automatically tend towards lower levels of unemployment when left alone, and thus governments should use the tools of taxation and government expenditure, known as fiscal policy, to control the overall level of demand for goods and services in an economy, known as aggregate demand, to create an acceptable level of unemployment in the economy. These ideas stood in contrast to the multitude of various positions present in economics thinking prior to Keynes (Blaug 1991, 1992, Morgan and Rutherford 1998a). It is in the context of the fall of these pluralist economics, to the benefit of Keynesian macroeconomics, that the Phillips Curve debate is played out.

The Phillips Curve explores the relationship between the level of unemployment in an economy and the level of inflation. This thesis explores the rise of three separate configurations of the relationship, that we will term here as, firstly, the Phillips/Lipsey Phillips Curve, secondly, the Friedman/Phelps Phillips Curve, and thirdly, the Rational Expectations Phillips Curves. Caution is used with these terms as they are not necessarily those used by participants. This will be expanded upon when each term is discussed in turn below. Furthermore, before exploring these three configurations we should discuss the explanations of inflation that dominated before the first Phillips Curve publication in 1958.

4.2.2 Keynes' Explanation of Inflation

Before Keynes General Theory inflation was often explained in terms of the amount of money in an economy and the speed with which it circulated. Such positions were known as versions of the Quantity Theory of Money, and some of those most influential in the Anglo-American context were those of Fisher (1911) and Pigou (1923) (Backhouse 1985). However a Keynesian position on inflation appeared in Keynes (1940). The inflation of the time was explained by the tension between the reduced quantity of goods to purchase due to their use in the war and the increased purchasing potential in the economy due to the money added to the system to pay for the war effort. Because all the resources in the economy were fully employed the rise in demand for goods could only be met with increases in prices as no more goods could be produced. This, of itself, only produces a one-off rise in prices, so to explain the continuous rise in prices implied by inflation a lagged looping mechanism as money passed between wage earners and profit makers was introduced. The difference between the potential spending ability of an economy and its ability to supply the goods was known as the inflationary gap, and theories of this type became know as inflation gap theories. Other versions of inflation gap theories were also developed, another, more complex, example is Hansen (1951) which introduced two gaps to deal with both the goods and labour market (Backhouse 1985).

Towards the end of this period authors have typified the explanations of the pre-Phillips Curve era as being 'demand-pull' or 'cost-push' theories of inflation, see

Bronfenbrenner and Holzman (1963) and Samuelson and Solow (1960) as examples, although it is less clear that the proponents of either would have used this terminology of themselves. Demand-pull refers to a situation where the demand in an economy exceeds the supply for goods causing prices to rise, as reflected in the inflationary gap theories. Cost push explanations of inflation imply that inflation is caused by wage, plant or raw material price increases being passed on to the consumer. Although this terminology is potentially problematic because the labels were created after the event, they do provide useful descriptions of complex sets of ideas and will be referred to on brief occasions in this thesis. Furthermore, their explanations are important in this thesis because the respondents use them in interview quotations.

4.2.3 Anglo-American Policy Context Between World War II and Phillips' 1958 Publication

In terms of the economic policy context during these debates the British always prioritised low unemployment, as declared in the 1944 White Paper on Employment Policy (Dow 1964). At the time this was assumed to mean a rate of 5%. However in practice the rate was often closer to 2% encouraging the Labour Government publicly to adopt the 3% target by 1951, a target met almost continuously until the mid-1970s. The ease with which these targets were met switched the focus to the massive balance of payments problems inherited from the war effort (Dow 1964). In terms of controlling inflation the Labour government used almost exclusively budgetary methods, most notably the withdrawal of purchasing power. Churchill's Conservative government of 1951 followed suit with the addition of raising interest rates, but shifted to an inefficient flexible monetary policy when Eden took office in 1955. Interestingly in 1957 a move was made by Thorneycroft, then Chancellor of the Exchequer, to use the Quantity Theory of Money that pre-dates the debates of the time in a bid to tackle inflation. However he resigned soon after as the government refused to back the level of cuts in government expenditure he had requested (Cairncross 1992, Middleton 2000). The 1950s also saw attempts to instigate incomes policies in the U.K., as Churchill's Conservatives tried to secure Trade Union agreement to a wage policy (Jones 1987), a move later repeated by Eden who also established the

Council on Prices, Productivity and Incomes. However both Prime Ministers met little success in these pursuits (Cairncross 1990).

The United States post war situation differed greatly to that of the U.K.. Financing the war had caused a massive Keynesian expansion of liquidity which consumers were willing to spend. In addition public facilities and foreign demand for American goods were blossoming (Hogan & Graham 1990). However this post-war prosperity brought with it inflationary pressures which were exacerbated by the defence spending for the Korean war in 1950 (French 1997). This inflation is significant in the history of American economic policy as it led to the 1951 Treasury/Federal Accord. Prior to this the Federal Reserve was committed to maintaining a low interest rate peg on Government bonds to help finance America's involvement in World War Two. In order to achieve this it surrendered control over the size of the money stock. The Accord removed this commitment and again allowed monetary policy to be used for general economic stabilisation (Hogan & Graham 1990). Its use was gradually implemented by Eisenhower in the mid-1950s, most notably with the lowering of the discount rate, meaning the rate at which the central bank lends to the banking system, in 1957. Under Eisenhower the economy slowed down and unemployment rose with a number of small recessions in the late 1950s.

4.2.4 Two Versions of the Keynesian Synthesis

By the 1950s and 60s Keynes' economics had branched into two separate forms. One introduced the Hicksian IS-LM model representation of Keynes' ideas, where IS is Investment-Savings and LM is Liquidity-Money Supply. This version of Keynesian economics became known as the neo-classical synthesis, and it is with this that our story begins. Associated with writers such as Paul Samuelson, Robert Solow, James Tobin and Franco Modigliani, all now Nobel prize winners, and the latter three participants in this research, the neo-classical synthesis was connected with the American Universities of the north east. Understanding the nature of the IS-LM model is not necessary here, however, it should be noted that in this model, due to an idea known after its developer as the Pigou effect, it became again conceivable that an economy could achieve full employment when left alone, given flexibility in the labour market. This discrepancy between the neo-classical synthesis and Keynes principle of markets not returning to full employment was uncomfortable for some. In time it would be the Phillips/Lipsey Phillips Curve that proved to remedy this discrepancy.

The second form taken by Keynesianism rejected Hick's IS-LM model and remained committed to the fundamental positions as outlined in Keynes writings themselves. These researchers, for example Joan Robinson and Nicholas Kaldor, centred in Cambridge University, where Keynes himself had been based, and adopted the role of representing what they perceived as the true Keynesianism. The Cambridge economists remained influential through the sixties, but never reached the heights of their American colleagues working with the neo-classical synthesis. Cambridge economists will feature several times in our story as partial objectors to the Phillips/Lipsey Phillips Curve, accepting some of its features while advocating a stronger role for the cost push element to the understanding.

4.3.1 The Phillips/Lipsey Phillips Curve

Throughout this thesis the understanding of the relationship between unemployment and inflation that first appeared in 1958 and became the accepted view through the 1960s will be termed the Phillips/Lipsey Phillips Curve. However, here it is essential to make the first of several warnings over the use of the terminology. Firstly it would be wrong to assume that all forms of the relationship termed as the Phillips/Lipsey Phillips Curve are uniform. Indeed, as we shall see, Phillips' and Lipsey's individual constructions themselves vary greatly, and throughout the relationship's period of dominance a continual process of development and adaptation was widely conducted. Secondly, it must be noted that in the macroeconomics literature itself the relationship may not always be referred to under this name. It is most frequently termed simply the 'Phillips Curve'. However, this is problematic, as the same term is in some instances also used to refer to subsequent variations of the relationship that for the purposes of this analysis require separation. Subsequently the name Phillips/Lipsey Phillips Curve has been chosen here after the main two inventors of the relationship, and refers to a range of relationships that are linked only by the central commitment to the relationship being non-linear.

4.3.2 Bill Phillips

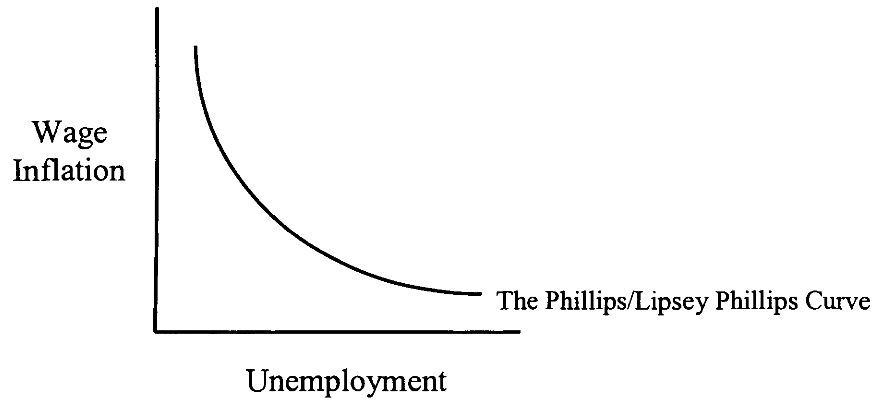
In 1958 Alban William (Bill) Housego Phillips published his most notable contribution to the economics literature, 'The Relationship Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861 – 1957', the relationship that would go on to be known as the Phillips Curve (Phillips 1958). The paper was embraced with noticeable haste by enthusiastic Keynesians of the day as a replacement for the inflationary gap explanations, and to rectify the full employment issue posed by Pigou.

Professor Phillips was an engineer by trade. He was born in Te Rehunga, Dannevirke in New Zealand, but moved to London in the late 1930's. Here he was accepted into the Institute of Electrical Engineers, but soon moved to a degree in Sociology at the London School of Economics (Wulwick, 1996). During his undergraduate years Phillips employed his training as an engineer to design and physically build a hydraulic model of the economy (Phillips 1950). He won an assistant lectureship in the economic department for his efforts on the machine, which is still housed today in the British Science Museum.

Let us focus on exactly what Phillips' 1958 paper says. It is an empirical study of the years 1861 to 1957, excluding the war years. He looked at the movements of the unemployment rate and the rate of change of wages. There are two things to note about the variables used. The unemployment variable is intended as a proxy for aggregate demand, meaning the total level across the economy of consumers' demand for goods and services. Phillips was not interested in the role of unemployment in its own right, but only in its role as a proxy for the aggregate demand that Keynes had stressed in his general theory. Secondly, since it is a strong element in the inflationary process, the rate of change of wages is often considered representative of inflation.

Phillips found that the two sets of data were inversely related, so as one increased the other would fall. The levels at which the two traded off against each other provided a curve when plotted on a graph. This is represented in its simplest form in figure 1:

Fig 1. A Basic Phillips/Lipsey Phillips Curve



There are several things to note about this observation. Firstly, by the standards of the period, the time span of Phillips' data was considered massive, while the stability of relationship proved surprising. Secondly the relationship implied that the Pigou challenge would not hold, as the flattening out of the curve as it approached zero unemployment empirically demonstrated that the economy would not tend to full employment. Thirdly, the relationship would come to represent a policy menu of choice for governments. As we can see from figure 1, the relationship shows that if a government allows inflation to rise, then there is a pay off in a reduction in unemployment. Equally, should a government wish to lower inflation, they could do so by increasing the unemployment level. In effect this allowed governments to choose their preferred position for the economy along the range of possibilities represented by the curve. In the 1960s governments did indeed use the Phillips Curve in this manner to define government policy (Jones 1987).

There are two perceived weakness of Phillips research that we need to note. Firstly Phillips provided no theoretical explanation of why the relationship should be of this form. Macroeconomics in this period had a large theoretical element, and the lack in Phillips' work of such an account was regarded with suspicion by many in the field. Secondly Phillips had used noticeably unorthodox mathematics in the production of his account. The details of this unorthodoxy are elaborated in Chapter Five on how macroeconomists use data. Equally the theoretical ineptitude is discussed at length in the following chapter on how macroeconomists use theory. For now it is sufficient to know that these weakness existed, and as we shall see, they were successfully addressed by Richard Lipsey.

4.3.3 Dick Lipsey

Richard (Dick) G. Lipsey received his B.A. from University of British Columbia in 1951, M.A. from Toronto in 1953 and PhD from London School of Economics in 1957, the same time Phillips was working on his leading paper also at the LSE. Lipsey held a chair in Economics at the LSE until 1964, when he became chairman of the Department of Economics at the now University of Essex. His paper was published in 1960 (Lipsey 1960). In the two years between Phillips' paper and Lipsey's the pair remained friends as Phillips supported Lipsey's efforts to reformulate his relationship.

Lipsey repeated Phillips' work, with the same data, but using the form of regression analysis that was quickly becoming the norm in macroeconomics. His empirical endeavours reaped very similar results to Phillips', and he subsequently continued to develop a theoretical basis for the relationship. By doing this Lipsey reformulated an interesting but unconventional macroeconomic idea as a mainstay of macroeconomic orthodoxy. It is with this that we really witness for the first time the standard Phillips/Lipsey Phillips Curve.

First let us take his empirical work to demonstrate what an orthodox economic regression, the leading research tool throughout the period studied here, actually is. Lipsey took Phillips' data for unemployment and the rate of change of money wages for the period 1861 to 1957, to see if there was a trend line in their patterns of movement. Phillips had suggested that there was a pattern, that in years with high unemployment the rate of change on wage rates was low. To discover the nature of the trend line Lipsey performed a regression on the data, the accepted technique for identifying trend lines. The most common form of regression used at this time, and through out much of the period studied in this thesis, was ordinary least squares (OLS) regression. The line is made by reconfiguring its position across the data until the sum of the distance above or below the trend line of every variable is minimised. This produces an equation that shows the proportions to which the variables relate to each along the span of the trend line. To have an equation like this that is deemed close enough to the data to be mathematically significant is to do orthodox macroeconomics in this period.

On a theoretical level, Lipsey looked first at the individual industry Phillips/Lipsey Phillips Curves and then the impacts of aggregating them into an economy scale relationship. He first assumed the accepted notion that frictional unemployment existed in each market, meaning unemployment arising from people moving between jobs, seasonality and retraining. This creates the curve shape of the Phillips/Lipsey Phillips Curve. Then Lipsey aggregated the individual industry curves together to create the economy wide curve. By assuming the unemployment levels were different in each industry, Lipsey's aggregation increased the height and non-linearity of the curve. This remained the acceptable theoretical account of the Phillips/Lipsey Phillips Curve throughout its dominance.

4.3.4 Responses to the Phillips/Lipsey Phillips Curve

The relationship was very quickly accepted, aided by an equivalent article based on US data by the extremely influential macroeconomists Paul Samuelson and Robert Solow (1960). Perhaps as many as 95% of the profession would have claimed to believe it existed in the mid 1960s. However, the consensus did not mean that researchers were happy to leave the relationship untampered with. In subsequent years an enormous research effort and quantity of papers were devoted to improving the fit of the relationship. In practice this meant creating regression equations that were more convincing than Lipsey's, or those of any other researcher in the field. An account of how this is achieved forms the core part of Chapter Five. For now we provide only a simple explanation.

To improve the fit of their regressions, macroeconomists would firstly invest in cleaning their data to remove external impacts. These researchers did not have the luxury of laboratory settings, and subsequently have to use data fraught with external influences such as wars, changes in government polices, and changes in labour practices. Many macroeconomists spent time discovering ways to minimise these impacts in their data. The second, most dominant, form of research, was to reconfigure the regression equation itself. The regression equation shows the proportions by which the variables included in the data relate to each other on the trend line. By introducing other variables into the data, either as an addition to the existing two, or as a replacement, the extent to which the trend line fits the data could be increased. The breadth of variables available for this are limited only by the imagination, but some of those that received heavy attention, include the dispersion of unemployment (Lipsey 1960, Archibald 1969, Thomas & Stoney 1972, Hines 1971, Archibald, Kemmis and Perkins 1974, Gordon 1972), price change (Bhatia 1961, Perry 1964, Lipsey 1960, Eckstein & Wilson 1962, France 1962, Archibald 1969, Archibald, Kemmis and Perkins 1974), productivity (Vanderkamp 1972, France 1962, Kuh 1967,), profit (Kaldor 1959, Lipsey and Steuer 1961, Eckstein & Wilson 1962, Bhatia 1962, Schultze & Tryon 1965, Hamermesh 1970, Kuh 1967, Watanabe 1966, Pierson 1968) and Trade Union power (Hines 1964, 1968, 1971, Purdy & Zis 1974, Ashenfelter & Johnson 1972, Schmidt & Strauss 1976, Thomas & Stoney 1970, Throop 1968, Thomas 1974b, Pierson 1968, Hamermesh 1970, 1972, Ashenfelter, Johnson & Pencavel 1972, Godfrey 1971, Johnston & Timbrell 1973).

These efforts, however, are most effectively characterised as operations of precision on the Phillips/Lipsey Phillips Curve, and were not attempts radically to discredit the relationship. However, towards the end of the 1960s voices were heard that did ask serious questions of the relationship, and in time would rise and replace it.

4.3.5 Anglo-American Policy Context Between Phillips' 1958 Publication and the Late 1960s

The period over which these modifications to the Phillips/Lipsey Phillips Curve were conducted has become considered a golden age in British economic history

(Cairncross 1992). Unemployment stayed low while inflation caused problems only when judged by the standards of the time but could only be considered meek when compared to the problems of the 1970s. The major economic issue facing the succession of Conservative governments was the balance of payments deficit. (Middleton 2000) Budgetary controls remained the dominant tool for economic stability. In the early 1960s inflationary concerns were considered best tackled through incomes policies, for example the enforced short-lived public sector pay-pause in 1961 that failed to gain union support (Jones 1987) and Wilson's Labour government of 1964's Declaration of Intent that did bring the unions on board (Cairncross 1990). However these measures, and others such as the creation of the National Board for Prices and Incomes, were only fairly successful in restraining inflation, and only until 1967 (Cairncross 1990). It was in this year that the government finally devalued the pound against a background of political resistance spanning three previous exchange crises in the three previous years (Blackaby 1978). In effect, the need to counter the continuing balance of payments deficit superseded the control of inflation as the devaluation reignited the upward trend in inflation through rapid exports growth.

Across the Atlantic U.S. economic policy pursued strong fiscal activism and expansion under Kennedy's presidency. With unemployment over 6% in 1961 the administration launched its 'New Economics' based more squarely on new Keynesian principles of maximising growth over smoothing out fluctuations (Hogan & Graham 1990). The centrepiece of the policy was wide-spread personal and business tax cuts, designed to promote economic growth and reduce unemployment. The proposal was initially resisted but was passed on a wave of sympathy after Kennedy's assassination (Hogan & Graham 1990). At the time it was the biggest tax cut in US history. Johnson's presidency placed even further importance on fiscal activism as monetary policies were considered as moderately inflationary. Over the course of the 1960s U.S. unemployment dropped from 6.7% in 1961 to 3.5% in 1969 (French 1997). However, in 1966 the economy started to shown worrying inflationary signs and Johnson stood accused of acting late in trying to resolve it.

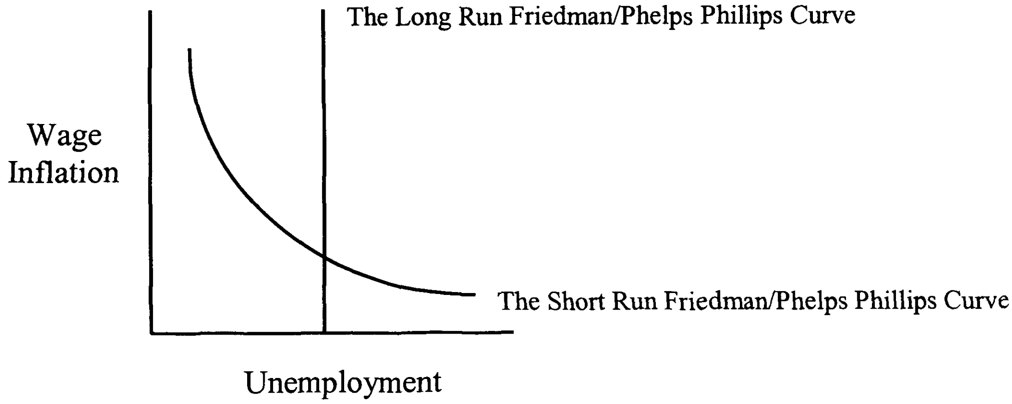
4.4.1 The Friedman/Phelps Phillips Curve

It was just after the emergence of this inflation that the second incarnation of the Phillips Curve first appeared in public. Two macroeconomists, Edmund Phelps and Milton Friedman, both working independently outlined theoretical positions with similar conclusions (Friedman 1968, Phelps 1967). They had very different intellectual backgrounds, with Friedman working within a framework unusually close to that of classical macroeconomics and the Quantity Theory of Money and Phelps as a neo-classical Keynesian. To some macroeconomists their divergent backgrounds and similar conclusions made the Friedman/Phelps Phillips Curve even more convincing.

It is time for the regular warning over terminology. Here we use the term Friedman/Phelps Phillips Curve to encapsulate a heterogeneous collection of theoretical constructions. Furthermore the term Friedman/Phelps Phillips Curve is rare in macroeconomic writings. In publications the relationship features under numerous names, most notable the Natural Rate of Unemployment and NAIRU (Non-Accelerating Inflation Rate of Unemployment), but also the adaptive expectations augmented Phillips Curve, the Monetarist Phillips Curve, the vertical long run Phillips Curve or even simply the Phillips Curve.

In this instance we have adopted the term Friedman/Phelps Phillips Curve to refer to specifications of the relationship between unemployment and wage inflation that adhere to one defining characteristic. All the relationships assert that in the short run the Phillips Curve is as the Phillips/Lipsey Phillips Curve tells us. However Friedman/Phelps Phillips Curve advocates tell us that this ignores the role of wage expectations in the long run. In the long run, they postulate, the relationship is a straight vertical line, as represented in figure 2.

Fig 2. A Basic Friedman/Phelps Phillips Curve



Let us imagine that the government makes a determined attempt to lower unemployment by injecting money into the economy to create jobs. On the Phillips/Lipsey Phillips Curve this would work because unemployed workers would be willing to work at the new higher wage levels. Of course this is at the expense of inflation. However with the Friedman/Phelps Phillips Curve the workforce would soon realise that although their wages have risen in nominal terms, i.e. the physical number of pounds they receive has increased, the actual spending power gains are an illusion because prices have also risen due to inflation. Subsequently as the workers' expectations of the changes in price level gradually become realistic they leave their jobs and the unemployment rate returns to the original position, the long run vertical Friedman/Phelps Phillips Curve.

The Friedman/Phelps Phillips Curve advocates typically adopt an adaptive expectations scheme. This means economic agents' expectations of the future price level are derived from extrapolations from recent history. Subsequently a change in government policy on inflation will fool economic agents for a short while. This is where we see the Phillips/Lipsey Phillips Curve. However soon economic actors will incorporate this new knowledge into their expectations and the change of policy will have little effect. We need to make this point explicit as shortly we will be contrasting adaptive expectations to the mechanisms espoused by the Rational Expectations Phillips Curve.

Before introducing the architects of the Friedman/Phelps Phillips Curve we must point out that the position of the vertical line is not considered a constant position. It can be changed by what is known as supply side policies, including altering the level of competitive forces in a market and changes in unemployment benefit. These changes differ from the demand management prescriptions associated with the Phillips/Lipsey Phillips Curve because instead of trying to create jobs directly they attempt to foster conditions that make the labour market function more efficiently, i.e. they seek to move the long-run curve along the x-axis and closer to the origin.

4.4.2 Milton Friedman

Milton Friedman received his B.A. from Rutgers University in 1932, his M.A. from the University of Chicago in 1933 and PhD from Columbia University in 1946. He then returned to Chicago where he maintained an intellectually marginalised anti-Keynesian position through to the dawn of the Friedman/Phelps Phillips Curve, when his ideas attained wide acceptance. This culminated in him winning the 1976 Nobel Prize in Economics and he is today arguably the most famous living economist.

His contribution to the Phillips Curve debate came in his 1967 Presidential Address to the American Economic Association (Friedman 1968), where he outlined the theory described above. This was actually a brief excursion away from his usual work and, although he continues to support this theoretical construction to today, he did not pursue it further with his research. However it was widely accepted and further pursued by other writers and supporters of his position. In contrast to Friedman, the co-architect of the Friedman/Phelps Phillips Curve would invest heavily in establishing and developing the relationship.

4.4.3 Ed Phelps

Edmund Phelps received his B.A. from Amherst College in 1955 and M.A. and PhD from Yale in 1957 and 1959. His PhD thesis actually explored and supported the Phillips/Lipsey Phillips Curve relationship. However by 1967 he had published his first paper in the Friedman/Phelps Phillips Curve tradition, and after several other notable contributions published an edited collection of his own and other papers on the subject in 1970 (Edmund Phelps 1967, 1970). These works bore the burden of detailing and promoting the Friedman/Phelps Phillips Curve.

Phelps developed what became known as the micro foundations of the Friedman/Phelps Phillips Curve. He explored how the dynamics of contract setting and job searching operated to produce the adaptive expectations scheme. Although the micro foundations literature is not homogeneous, we can articulate a loose and generalistic version to provide an understanding. In these models the individual is placed under a set of assumptions. Firstly they are assumed to maximise the present value of lifetime income. Secondly it is assumed that searching for a new job is impossible while employed elsewhere. Subsequently there is a trade-off based upon how much income is lost by being out of work against how much will be gained by waiting for the best paid job to arise. The balance point of these two interests is set at the acceptance wage, and the individual will accept any work that pays above it and thus finish their period of job search.

Given the adaptive expectations scheme outlined above, where individuals base their expectations of price changes on the pattern experienced in the previous time period, people are slow to attain realistic expectations. This creates Phillips/Lipsey Phillips Curves in the short run and the vertical Friedman/Phelps Phillips Curve in the long run because if an unexpected one-off increase in inflation occurs the individuals will be offered a sudden rush of jobs paying above the acceptance wage. Subsequently more workers will accept employment and unemployment will go down. Here we have moved along the curve of the Phillips/Lipsey Phillips Curve. However, the increased wage is only increased in money terms, but not in real terms. As the time period passes the individuals will realise that they have not made the real term gains they expected and will leave their jobs. Subsequently, once they attain realistic expectations, unemployment returns to the same rate it occupied previously. We have now returned to the point where the vertical Friedman/Phelps Phillips Curve has been throughout.

4.4.4 Responses to the Friedman/Phelps Phillips Curve

Like the Phillips/Lipsey Phillips Curve, the Friedman/Phelps Phillips Curve would quickly become the orthodox view, with perhaps 80% of the profession believing it by 1973. However, also like the Phillips/Lipsey Phillips Curve, the research effort turned to modifying and perfecting the relationship. In the version above the individual speculates on a range of currently available wages. Writers pursuing this line included Alchian (1970), McCall (1970), Mortensen (1970), Gronau (1971) and Salop (1973). However others based their argument on individuals having perfect knowledge of all the available money wages, but misperceiving changes in the price level effecting their real value. These include Friedman (1968), Almonacid (1971), Lucas (1973) and Sargent (1973). Yet more writers based their arguments around individuals who speculate over time in the usual rate of increase of wages, but do not engage in job search. Authors include Lucas (1972) and Weiss (1972). Furthermore, great quantities of empirical work to test for the existence of the relationship were produced, some examples include Gordon (1970, 1971, 1972,), Vanderkamp (1972), Parkin, Sumner and Ward (1976) and Wachter (1976). Most of the work of this period tended to confirm the relationship, and suggested ways in which it could be better specified. However, by the late nineteen seventies the Friedman/Phelps Phillips Curve was itself experiencing a massive fall in support.

4.4.5 Anglo-American Policy Context Between the Late 1960s and the Mid 1970s

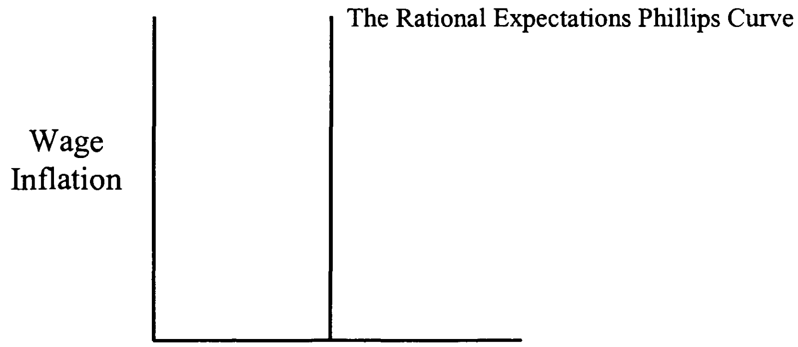
The 1967 British devaluation was followed by two more years of anxiety over the balance of payments spurred by an unanticipated strength in imports (Cairncross 1992). By the early 1970s the effects of the devaluation was being felt and the balance of payments problem finally alleviated. However, at the same time the unemployment rate that had stayed so comfortably low during the 1960s slowly began to rise as the 1970s began. As well as unemployment, inflation had also started to rise much faster after the cessation of the 1960s incomes policies. In 1972 the Conservative government went against it's previous reasoning and reengaged the unions in unsuccessful rounds of incomes policy discussions as the concerns over stagflation increased (Middleton 2000). The rising inflation also spurned the government into targeting an increased rate of economic growth, with the promise that the fixed exchange rate would not stand in the way. This promise soon had to be acted upon as the exchange rate was floated in 1972 (Blackaby 1978). However, these economic concerns were soon dwarfed by the major economic incident of the period, the quadrupling of oil prices by the OPEC cartel in late 1973. Record levels of inflation gripped the U.K. economy as the balance of payments raced into deeper and deeper deficit. Wilson's newly elected government failed to tackle the issue and lost the confidence of financial markets (Cairncross 1992). By 1975 inflation was at its worst, although it began to show signs of softening due to rises in unemployment and the unions new found willingness to co-operate more enthusiastically with incomes policies (Cairncross 1990).

In the U.S. the Nixon administration inherited an inflationary economy in 1969. For the first two years Nixon used the established Conservative tools of tightening tax and spending and restrained the supply of money to get a small budget surplus. An acceptance existed that his would cause increases in unemployment but it was hoped fiscal policies could minimise the effect. However they were failing and by 1971 unemployment reached 6% and inflation still caused concern (Hogan & Graham 1990). Encouraged by this the Democrats started making threatening noises about the upcoming election that intimidated Nixon into a change of course. In 1971 Nixon announced his 'New Economic Policy', a set of reforms including a ninety-day pay freeze and the establishment of a Pay Board and Price Commission to stifle inflation. Furthermore taxes were reduced and the money supply expanded while the devaluation of the dollar through ending its convertibility to gold addressed Nixon's balance of payment problems (French 1997). 1972 reaped the rewards of these policies as unemployment and inflation began to fall, and Nixon won a land-slide re-election. However, as with the U.K., 1973 held harsher times for the U.S. as the OPEC price rises caused massive inflation. Ford succeeded Nixon in the same year but the problems remained as his fiscal and monetary policies only further increased inflation and unemployment (Hogan & Graham). It was not until 1975 that signs of improvement started to emerge.

4.5.1 The Rational Expectations Phillips Curve

The third and final version of the relationship between unemployment and inflation that we will be looking at is the Rational Expectations Phillips Curve. This first arose as an addition to the Friedman/Phelps Phillips Curve in the early 1970s. It is most closely associated with Robert E Lucas, as well as a number of other writers, including Thomas Sargent, Leonard Rapping, Patrick Minford, Robert Barro and Neil Wallace. The central tenet is that, unlike the backward looking adaptive expectations of the Friedman/Phelps Phillips Curve, individuals have rational expectations. This means that people base their expectations of future price changes on all the available information and will, on average, be correct in all but a few select instances. Subsequently the Rational Expectations Phillips Curve is always vertical and acts like the long run Friedman/Phelps Phillips Curve at all times. Figure 3 illustrate this form.

Fig 3. A Basic Rational Expectations Phillips Curve



We should note that the relationship employs a similar micro foundation in contract and search theories as the Friedman/Phelps Phillips Curve.

Again a terminology warning must be issued, although less severe in this case. This relationship is commonly referred to as the Rational Expectations Phillips Curve, although it is also known as the Rational Expectations Augmented Phillips Curve, the New Classical Phillips Curve, and simply just the Phillips Curve. The name New Classical Phillips Curve exists because the writers associated with it were sometimes called the New Classical School.

4.5.2 Responses to the Rational Expectations Phillips Curve

Like the two incarnations of the Phillips Curve proceeding it, the Rational Expectations Phillips Curve was widely and swiftly adopted by the majority of the profession. Perhaps 80% of macroeconomists would have believed it existed by 1980. Again work on it turned to contending the correct form the relationship took, while broadly accepting its truth, for most of the following decade. Important contributions to this include Lucas (1972, 1973, 1975), Sargent (1973), Sargent and Wallace (1975, 1976), Barro (1976), Minford and Peel (1982a, 1982b).

4.6.1 Understanding the Story of the Phillips Curve Debate

This chapter has sought to provide a background to the analysis that follows. We must stress again that the chapter has not attempted any sociological analysis. Furthermore it has not provided a full history of the relationship. There is simply insufficient space and reason to do so here. Instead it has provided the reader with an account of issues raised in the next seven chapters. This is all it has attempted to do, and this, we hope, it has achieved.

5. How Macroeconomists use data

5.1.1 Introduction

This chapter will demonstrate how macroeconomists use data in their own work. Illustrations of a number of mechanisms used by macroeconomists to whittle potential interpretations of the data available into compatibility with their preconceived expectations are provided, and a model is developed that formalises these processes. By doing so the chapter starts to fulfil the essential task of documenting Interpretative Flexibility in a scientific controversy as dictated by the Empirical Programme Of Relativism (EPOR) (Collins, 1981a, 1983a, 1992), as discussed in Chapter Two. This is a necessary procedure in EPOR because the remaining two stages, the exploration of how closure is achieved and how closure relates to wider social structures, are premised upon the findings of the first stage. This is a fundamental procedure in any SSK study. Subsequently this chapter, and the following two, make an essential contribution to the theoretical and methodological premises adopted in this thesis.

Interpretative flexibility is an inevitable product of what Collins (1992) calls the 'experimenters' regress'. The regress demonstrates the problem of circularity involved in attempts to establish the existence of a phenomenon. The researcher cannot reliably state that a phenomenon exists until they have a suitable method for detecting it. However, they cannot know if they have a suitable method for detecting it until they have tried to use it and obtained the correct outcome. However they cannot know what the correct outcome is until they know whether the phenomenon exists. Collins (2004b) uses the example of detecting gravitational radiation to demonstrate this. The circularity of the experimenters' regress produces interpretative flexibility because the lack of a concrete position at any point in the exploration of a phenomenon allows numerous claims of successful combinations of methodological prescriptions and accounts of the phenomenon without providing a unified criterion by which to judge their success. The discussion that follows demonstrates how macroeconomic data have interpretative flexibility. It shows the methodological techniques used by macroeconomists to locate a single interpretation from the infinite possible due to the experimenters' regress. The chapter also illustrates how the particular interpretation chosen is often one that fits with a prior pre-conception of what the answer should be. Firstly, we shall explore some interview quotations as an introduction to the topic. Secondly we will explore three examples of interpretative flexibility in macroeconomics. Thirdly we will note some similarities with work in the History of Economic Thought literature. Finally we will review what has been discussed in this chapter and locate it within the overall argument of the thesis.

5.1.2 Discussions about using Data

The empirical macroeconomics of the Phillips Curve in almost all cases involves the construction of regression equations that predict patterns of behaviour that match as closely as possible those exhibited by real world data. The regression equation is reconfigured with additional variables to maximise its predictive capability. Here is a typical account from the interview data from US macroeconomist Robert Solow:

> Robert Solow: "You could certainly improve the Phillips Curve as an empirical regularity by introducing other variables, especially since in the U.S. the relationship seemed to be much less tight than in the U.K., much less stable than the relationship Phillips claimed to have found. We were interested in trying to improve it by making it a more reliable tool, by introducing new variables"

However, as well as configuring the regression equation, inevitably part of a macroeconomist's work is to also configure the data they are compared to. Both processes of configuration introduce Interpretative Flexibility into the macroeconomists' work. Sufficient Interpretative Flexibility is introduced, it is argued here, to permit the legitimate acceptance of radically opposed results based upon the same initial data. Hinting at this, Edward Kuska:

> Edward Kuska: "If you work a bit you can get results out of econometric exercises that come out in the direction you want them to come out, you know, if you're careful how you fiddle with the data. I think people do this unconsciously"

The claim that this is an unconscious action will be challenged as this chapter develops. An initial indication of Kuska's politeness on the topic, and further evidence of Interpretative Flexibility, comes from Peter Stoney:

> Peter Stoney: "[I] was the computer operator stroke data gatherer stroke bug finder or pointer outer of possible errors in the formulations etceteras, so I became very familiar with the ease with which results could change by altering just a few input elements in the data, or by changing the formulations of the equations, just a little bit here and there could produce significant differences in the results"

And later, commenting on the final regression equation chosen for publication from his work with Leighton Thomas on an unemployment dispersion variable:

> Interviewer: "So the paper was an operation in finding the equation that gives the best fit for dispersion?"
>
> Peter Stoney: "Yes, but I wouldn't claim that it was the only equation, you know [that] there weren't other variants that might have been possible, but it was one that gave a good fit so, you know, 'let's get it published boys' you know 'looks good, Taylor series' you know 'cogent', 'good fit', but there were loads of equations that didn't, I've forgotten what they were now, I was the computer bloke that used to take these great reels of flexi-writer tapes across to

> the computer lab at Salford where I was, Leighton used to come across from Manchester to pick up the results and talk about it 'oh no that didn't work let's try this' you know, it was a bit like that, a bit trial and error really, then we came up with this one that fitted and 'great, let's try it with the publisher', so then it was Victor Morgan, who was then Professor of Economics, he was the editor of the Manchester School [journal], said 'oh it's great' you know 'we'll take it' "

And commenting on some specific mechanisms by which Interpretative Flexibility can be achieved, David Laidler:

> David Laidler: "If you want to go back to the late sixties, early seventies you would do things like mess around with how many lag dependent variables you put into a relationship to see if you can tidy up its stability, you might worry about using moving averages of the data instead of point observations. What else might you do, if you had a relationship that didn't fit, you might also look carefully at the data and see if there was one big jump in the series you could get rid of just by adding a dummy variable, um, basically beat the data to death that way"
>
> Interviewer: "Do you think that that was acceptable behaviour?"
>
> David Laidler: "Oh sure, I mean it's, er, well, it depends what you mean by acceptable"
>
> Interviewer: "Amongst other economists"
>
> David Laidler: "Well it was certainly accepted, I mean, I think what you wouldn't do was make up the data, actually falsify the data, that would obviously be outrageous"

Laidler's quote shows him explaining the central topic of this chapter: that is to articulate the exact mechanisms by which macroeconomists orient their data towards their expected outcome. To this end, we will first develop six categories of action enacted in empirical work, and then observe their practice in three clusters of published journal articles engaging topics within the Phillips Curve debate.

5.2.1 Categories of Action Employed in Macroeconomic Empirical Work

The categories shortly to be explained account for sections of a fluid process of data handling. The representation here asserts a linearity and discreteness to each category that are overly formalistic compared to their lived implementation. The following sections will demonstrate that some actions witnessed in macroeconomic empirical work cross the boundaries asserted by this classification. However, once this is accepted, the categories provide a clarity of insight into what is a complex and specialist social process.

5.2.2 Choosing the Macro Data-Set

The first category of action engaged in by macroeconomists during empirical work is the choice of the macro data-set. This refers to the initial choices regarding upon which data-set to base the empirical investigation. There can be a wide number of options available. Examples are whether to use income or wage data, whether to use data collated monthly or quarterly, what time period the data should cover, and should there be competing sources of data applicable to the same variable, which one to adopt. An attempt to list exhaustively the variants of macro data-set choice would prove daunting and perhaps fruitless. The constraints faced vary by instance, and the number of data concerns available for debate are bounded only by the analysts' imagination. However, in the empirical section of this chapter, the potency for the outcome on macroeconomic research of this category will become indisputable.

5.2.3 Transforming Data

The second category is the transformation of data. This, like the next, is a set of actions by which macroeconomists reconfigure their data sources into a form that allows communication with their theoretical and methodological tools. Specifically in this context, transforming the data as it appears on external data sources acquired from a public information body into a form suitable for analysis. A simple example would be collating monthly unemployment figures from country A to produce annual figures, so they can be compared to those of country B where only annual figures are available. However these transformations can also consist of complex operations removing weighted proportions of the data thought to represent external influences or unsuitable data, or calculating the rate of change of variable X from variable X data.

5.2.4 Developing Proxies

While transforming data reconfigures the data to produce quantitatively useful variables from other quantitative variables, developing a proxy constructs a quantitatively useful variable for a phenomenon that is not in any practical sense naturally accessible to quantitative methods. Subsequently variables available for measurement that are considered to vary in accordance with movements in the variable sought after are used as a proxy. The main example seen throughout the Phillips Curve debate is the horizontal axis of the Phillips Curve itself, unemployment, used as a proxy of aggregate demand. With any choice of proxy there are debates over its suitability, and often a range of options are available. Each possibility is then open to transformation in the same way as any other quantitatively available variable.

5.2.5 Micro Data-Set Choices

The choice of macro data-set is not the final decision concerning which data are included in the empirical work. Further decisions regarding the micro data-set choices can be made. These decisions exclude selective elements from the data, possibly results from a particular industry or the omitting of a specific time period within the data-set. Often this is the case in European data during World War One and Two, but can also be used for other years specifically chosen for a particular piece of research. The decision to make micro data-set choices is based upon judgements suggesting mitigating circumstances identifying the omitted data as atypical. Frequently the inclusion of those data would propel the results away from the analyst's expected outcome.

5.2.5 Identifying Special Effects

The identification of special effects has kinship with the micro data-set choices. Both are strategies to counter sections of data that do not conform to the analyst's expectations. Whilst micro data-set choices remove the offending data from the analysis, the identification of special effects makes recourse to the historical literature to explain away the impact of the inconvenient data. Entirely mathematical papers will suddenly pause their algebraic formulations to engage in a narrative on a particular historical incident that identifies the particular data as in some way different and subsequently deflates the negative impact of the results without actually changing the statistical results, as discussed by McCloskey (1985, 1990a, 1994, 1998). Examples of such historical incidents are changes in government and policy, industrial unrest, or war and civil unrest.

Special effects can, however, manifest themselves in a wholly mathematical form without omitting the offending data outright. As suggest above by David Laidler, the impact of periods of government policy change can be softened with the inclusion of a dummy variable. Here the variable co-efficient can be set to one during periods when a policy perceived as a cause of interference is in operation and zero when it is not. This dummy variable is intended to neutralise the impact of the policy from the final results.

5.2.6 Interpretation of the Data

The sixth and final category exposes the processes by which the results are interpreted and discussed. Empirical macroeconomic papers do not contain just a single test. Large numbers of similar tests run with differing configurations of the equation and data occur. Usually the publications portray adjustments that progressively improve the strength of the relationship. These tests are located within an unfolding discussion of the results ending with a conclusion. Throughout the discussion the macroeconomists are able to add nuances to the interpretation of their results through prioritisation and emphasis. The tests that are most confirmatory of their initial expectations are always the most prized and advertised results, where as those less emphatic can be relegated to footnotes, if reported at all. Discussions of policy implications can be based exclusively on the best fit result, omitting any reference to less flattering results.

5.3.1 Empirical Demonstration of the Categories: Three Clusters of Papers

To illustrate these categories of action in practice we will examine three clusters of papers. First Bill Phillips' own 1958 paper will be discussed, with the emphasis placed on Phillips' use of data. Then this will be compared to Dick Lipsey's equally influential 1960 paper, where Lipsey reconfigures Phillips' relationship in a manner compatible with the newly emerging macroeconomic orthodoxy at the time. The juxtaposition of these papers allows insights regarding both why Phillips' paper was unorthodox, and the requirements of a discussion for it to become compatible with the framework of orthodox macroeconomics. As throughout this discussion, the emphasis will lie with the collection, transformation, and interpretation of data.

The second cluster of papers examined consists of three papers testing the key sector hypothesis as formulated by John Eatwell, John Llewellyn and Roger Tarling (1974), and two further replications of their work by Leighton Thomas (1976) and Laurence Copeland (1977). There are numerous reasons why these papers have been chosen. Firstly they represent an instance of replication, and as such all the papers are rehearsing similar actions with similar availability of data. They also differ from the Phillips and Lipsey papers in that their influence was far more limited within macroeconomics. They also draw a range of conclusions, with two of the papers representing differing positions within the Keynesian orthodoxy, which by this time was troubled, and the third a product of the progressively influential monetarist position of the Manchester school.

The third cluster of papers also originate in the unsettled times of the Seventies, and illustrate a different point from the previous groups. This group is based around a set of papers published from the Manchester school criticising the use of data in a set of papers advocating Trade Union militancy as the leading predictive variable for wage inflation. The determination of George Zis and co-writers David Purdy and Bob

Ward (Purdy & Zis 1974, Ward & Zis 1974) to undermine a number of these writers illustrates a range of different resolutions to the challenges posed by data that can lead to equally different conclusions.

5.3.2 Cluster One: Bill Phillips' and Richard Lipsey's Phillips Curve papers

Both Phillips and Lipsey were based at the LSE, and used their two papers to establish the format for the understanding of the relationship between inflation and unemployment for at least a decade. The position adopted here is that Lipsey's paper represents the orthodoxy of 1960's Anglo-American macroeconomics. It is not the intention of the section to present an abstract account of characteristics associated with this orthodoxy, and subsequently demonstrate Lipsey's congruence with them. By being accepted on such a wide reaching level Lipsey's paper itself stands as an enactment and embodiment of the orthodoxy, in effect creating the standards by which orthodoxy in the field came to be judged. The sociological insight gained here lies in the actions made by Lipsey that allowed essentially a very similar notion to that provided by Phillips to move from the realm of the idiosyncratic to acceptance among their peers as an essential element within the Keynesian synthesis. Phillips' methodology will be outlined, and then further elaborated and scrutinised in relation to the use of data in Lipsey's paper.

Phillips' contribution is the empirical observation of an inverse relationship between percentage change in an index of wages and the percent level of unemployment. The theoretical implication of this is an inverse relationship between inflation and aggregate demand. This is because percent change in an index of wages was often interpreted as a determinant variable for inflation and unemployment was used as a proxy for aggregate demand. The development of a proxy is at the very heart of Phillips' paper.

His observation is drawn from three data-sets of UK peacetime statistics, 1861-1913, 1923-1939, and 1948-1957. This is part of Phillips' macro data-set choice. He then divided the 1861-1913 data into six groups of observations based upon the unemployment rate. The six groups were 0-2% unemployment, 2-3% unemployment, 3-5% unemployment, 5-7% unemployment, and 7-11% unemployment. Each year was placed in one of the unemployment level brackets. For each bracket Phillips took the average percent change in wages. This produced six constructed observations. The construction of variables in this manner is clearly a form of transforming the data, as Phillips was producing new data from an existing data source to test. However, transformation of this sort was far from orthodox, and this break from tradition carries through into how Phillips used the data.

Phillips entered four of the six constructed variables into a form of equation that by design contains one unknown value. Phillips then entered various random numbers into the equation representing the unknown value until the function produced a curve upon which the remaining two constructed variables sat. This done, Phillips checked the fit of his new curve to the original 1861-1913 data. In practice the test of fit was made by studying the picture of the curve produced from the equation and comparing it to the original scatter diagram of the 52 observations. Phillips argued the data fitted well. The constructed curve was then placed upon the 1923-39 data, again inspected, and considered reasonably close. Finally the curve was placed over the 1948-57 data. Here Phillips saw his greatest level of fit, better than even the 1861-1913 data the curve was derived from. All but one observation sat closely to the curve. To Phillips, this justified the acceptance of a stable and predicable relationship between unemployment and the rate of increase of money wages.

Lipsey's paper acknowledges the influence of Phillips on its findings, as well as Chris Archibald, again at the LSE, who is credited by Lipsey as having a substantial impact on the paper, to the extent of becoming a co-author of the section on theoretical development. It represents an attempt by a group of economists at the LSE to bring Phillips' observations onto the mainstream through testing for the relationship with Ordinary Least Squares regressions (OLS) on data very similar to Phillips'. In doing so Lipsey rejects several of Phillips' subsidiary hypotheses regarding additional observations made by Phillips, while retaining the general wage inflation/unemployment relationship. The paper then proceeds to develop a theoretical explanation of the relationship based upon the aggregation of individual markets, and finally subjects the new model to testing and further discussion.

Lipsey describes Phillips' methodology without ever explicitly labelling it unorthodox. The first change comes in the form of the theoretical and mathematical representation of the relationship. Unhappy with the six constructed variables, Lipsey preferred to test directly from the original 52 observations. He then prepares to run standard regressions on the data. Lipsey runs Phillips first data-set of 1862-1913 through his regression model. Phillips' choice of macro data-set was to use figures from the Phelps Brown–Hopkins index (1950), which lists figures from five countries for four major variables, including money wage rates in the U.K.. The Phelps Brown–Hopkins index does collate changes in money wages, however Phillips was interested in the rate of change of money wages. Phillips had to transform the data from its initial form to one useful for him. To do this Phillips took half the first central differences as the best approximation for the rate of change. Lipsey discusses this point in a footnote, comparing it to an alternative method for transforming the data. He notes that Guy Routh (1959), an early critic of Phillips' paper, argued the differences in the two methods were so small that it made little difference which technique was employed. We must note, however, that this process did occur.

The decisions relating to the macro data-set are not limited to this. Phelps Brown and Hopkins based their statistics for the money wage rate variable on previous estimates by Wood (1909) and further work on Wood's statistics by Bowley (1937). Lipsey reproduces Phillips' use of these data. Phillips found that for the 1881-1886 period the relationship between unemployment and rate of change of money wages in the Phelps Brown–Hopkins index did not exhibit the same level of stability as it did throughout the rest of the pre-war period. Phillips re-estimated these five years substituting different data for this period taken from Bowley's data-set. This substitution of data resulted in a pattern that did indeed confirm the relationship as Phillips expected. To justify this he notes "[i]t seems possible that some peculiarity may have occurred in the construction of Wood's index for these years." (Phillips 1958, p291) Phillips' next sentence continues to note that for the remainder of the period for 1886–1913 Bowley's results do not demonstrate the same level of regularity obtained by the Phelps Brown–Hopkins index. This point, however, is not mentioned again in Phillips' article. Clearly here Phillips is making creative use of the data resources available to him through a set of actions relating to decisions on how to construct the macro data-set. The guiding principle for the decisions was the greatest compatibility with the relationship expected from the data. This is confirmed when repeated in Lipsey's paper, where, although dealt with swiftly, the matter is addressed. Here the issue is set out and the question asked regarding which of the two is the most legitimate data source for this period. Lipsey concludes...

> "[i]n the absence of any evidence favouring one series rather than the other, we cannot eliminate one merely because it does not conform with our hypothesis. Therefore, although Bowley substitution for the years 1881-85 is used on the subjective grounds that it seems more plausible, ..." (Lipsey 1960 p5).

Lipsey then notes that for this period the Phelps Brown-Hopkins series estimates are included in footnotes. These results, however, are not discussed again. They are filtered out through their neglect in the interpretation of the data.

In a further footnote to this section Lipsey references Routh (1950) who argued the data-sets should not be substituted in this way, although Lipsey does not include any details of why this argument is made. The point demonstrated here is that both Phillips and Lipsey found it acceptable to reconfigure their data to the extent of introducing five years from an alternative data source if it increases the fit of the relationship, in Lipsey's case in the face of existing criticism on this issue.

Lipsey runs an Ordinary Least Squares (OLS) regression on the modified data and finds 64% of the rate of change of money wages is associated with the unemployment rate. Next he tests the relationship between the rate of change of money wages and the rate of change of unemployment. This is a test looking for the existence of what in time would come to be known as 'Phillips Loops'. These loops exist in the data across periods of around ten years, and will be discussed in the next chapter. OLS on a new wage equation including a rate of change of unemployment variable found 50% of the variance not associated with unemployment were associated with its rate of change.

Before concluding on this testing of Phillips' original hypotheses, Lipsey has a section a paragraph long regarding a single paragraph in Phillips' original paper. Two points of interest arise here, firstly as a case of the special effects, and secondly demonstrating how Lipsey's methodology stands in contrast to Phillips'. In his paper, Phillips notes how visually the graph for the 1893–1896 period depicts a lesser angle of increase in the relationship than in other periods, inferring a lower rate of increase in the rate of change of wage rates than expected. Phillips accounts for this by referencing the economic history literature, claiming this period experienced a rapid growth of employers federations, who at this time had a tense relationship with Trade Unions regarding the introduction of an eight hour day, with resultant industrial activity. Without manipulating the data, Phillips has again deflated the result lying contrary to his expectation through a tale of mitigating circumstances. The desired outcome is a nullification of the problematic result. In this case at least, numeric data can be overridden by historical narrative.

However this is not born out in Lipsey's response to what he labels an *ad hoc* explanation put forward by Phillips. Instead it is argued that despite the history of the period there is no empirical evidence for exceptional downward pressure on wages. At maximum the deviations from the normal pattern were one percent, a figure Lipsey considers less than significant, and thus not needing special explanation. Later, however, it will become apparent that Lipsey is not opposed to special explanations of his own.

Up to this point Lipsey claims to have established the existence of a relationship between the rate of change of wages and both the rate of unemployment and the rate of change of unemployment, in the period 1862-1913, with possible weak evidence of the involvement of prices. He now turns his attention to the remaining years in the data-set, 1919 to 1957. He identifies three hypotheses worthy of exploration, (i) that changes in the money wage rates continue to be explained mainly by changes in unemployment, rate of change of unemployment, and rate of change of prices in the new data-set, (ii) that the relative explanatory power of the three variables is unchanged, and (iii) that the exact relation between the four is unchanged so that the equation developed previously predicts well in these data. The first is supported by the data but the second and third are refuted.

Lipsey's first manipulation is to remove the war years plus the first post-war year, 1919-57 becomes 1920-39 & 1947-57. This is a macro data-set choice, and reflects the same decision made by Phillips. The reasoning references the heavy war time controls making the period inappropriate for study. Lipsey then performs OLS regression on this data-set, both with only unemployment as the independent variable, and then including all three independent variables. He attains what he refers to as "startling" results (Lipsey 1960, p25). Lipsey is startled by the extent to which the expected relationship is weakened compared to the previous results, especially as the cost of living becomes the most important variable. This result leads Lipsey to identify some serious problems with the dataset. He highlights four years experiencing extreme values for the rate of change of money wages, justifying their identification through referencing the high proportion of variance in the overall period associated with them. He notes any variable that has an impact in these years will be represented highly in the regression coefficient regardless of its impact across the other years. Lipsey argues this implies the four extreme years should be dropped from the data-set. This is a micro data-set decision. In a footnote he considers two of the years, 1920 and 1947, suitable for dropping for the same reason as the war years. However he is less certain about the remaining years, 1921 & 1922, for which no justification is offered in the paper except that they do not conform to his expectation of the results. On his concern over this, Richard Lipsey:

> Richard Lipsey: "Yeah, concerned, yes, if I remember I thought that the best thing to do is to do them both ways and see what happens, and it seemed to me that there were very special circumstances in the immediate post First World War data because [the labour unions] got locked in the cost of living indexes during the war which had never existed before and then they were removed after their unfortunate experience in about 1922, so you have an institutional circumstance that was important for wages that existed in those few years that never existed before or after, so [it] seems to me the thing to do is say 'look, this really does bias everything and let's do it both ways and you can make your choice', but my choice would be to remove it because of these clearly very specific circumstances, but I would very strongly argue then as now that if you're going to do it you've got to do it both ways so people can see what difference it does make."

However in the paper it is only 1920 in the post World War One period that is considered legitimately excluded for the special effects reason. Again here the data-set is being modified to exclude those years least compatible with the relationship

Lipsey is testing for. In the first instance the war years are removed from the data-set because of an intuitive notion of the history of the time. In the second the data-set is altered in order to remove extreme results from the regression.

The manipulations of this form continue until the closing pages of the article. His results demonstrate that the Phillips/Lipsey Phillips Curve relationship does not hold in the years 1934-39. Lipsey offers two possible special effects in operation here, changes in the cost of living and increased inequality between sectors in unemployment rates. Furthermore he notes that for the period 1948-57 the data required transformation to be comparable with the earlier time periods. This is because of changes in coverage of unemployment provision, and thus the numbers included in the unemployment statistics. Lipsey opts to raise the unemployment figures by 20%. He refers to the previously mentioned critique of Phillips' paper by Guy Routh that argues the figures should be raised by 12.5%. He then most surprisingly notes that "[t]he most accurate adjustment probably lies somewhere between those two figures". Here Lipsey has made a transformation to the data that even he claims is probably not the most accurate available.

There have been two sets of insight to gain from the analysis of Phillips' and Lipsey's papers. Firstly from comparing the differences between the two we gain a sense of the makeup of an orthodox paper. Although similar in conclusion, Lipsey's paper optimised methodological values congruent with the emerging mainstream position in macroeconomics. Lipsey, like so many authors in the Phillips Curve debate to follow, employed regression analysis on the original data-set. He also made an effort to produce a theoretical framework in which his observations were tested. These are the essential ingredients that allowed Lipsey's paper to save the Phillips/Lipsey Phillips Curve from the realm of the curious oddity and embed it firmly in the centre of mainstream orthodox macroeconomic thought.

Secondly we are observing the six categories of data handling in operation. The macro data-set choices have centred upon which years to include in the data-set and the substitution of one data-set for another for a troublesome five year period. The process of data transformation is prevalent throughout both papers, including Phillips' generation of rate of change of money wage rates from level of money wage rate data, and Lipsey's 20% upward adjustment in the unemployment rate for the 1948-57 period. The Phillips/Lipsey Phillips Curve itself is a proxy with the horizontal axis being a proxy for aggregate demand. Micro data-set choices are seen in the exclusion of 1920-22 & 1947 by Lipsey and special effects to explain away problematic results by Phillips for 1893-6 and Lipsey for 1934-9. Finally the interpretation of data is evident in cases where the results do not comply with their expectations, as both writers make an effort to account for this. Equally, less favourable results are omitted from the conclusions of the paper. It should be emphasised that these are not critical comments, but a descriptive account of normal economic practice, as prescribed by EPOR.

Now attention will turn to the second cluster of papers, where further examples will demonstrate further nuance in the implementation of the six categories of data handling.

5.3.3 Cluster Two: Testing the Key Industry Hypothesis

The three papers testing the key industry hypothesis are Eatwell, Llewellyn and Tarling (1974), Thomas (1976), and Copeland (1977). The general principle informing the key industry hypothesis is that the rate of wage change in a key group of leading industries determines wages in all sectors. A refined form of this is proposed, tested and accepted by Eatwell and colleagues, then subsequently replicated and refuted by both Thomas and Copeland. The Eatwell *et al.* (1974) paper is based within the Cambridge Keynesian tradition, and argues the Phillips/Lipsey Phillips Curve is better reconfigured when using the Key Industry variable instead of unemployment on the horizontal axis. The Thomas paper is from the Keynesian neo-classical synthesis tradition and takes issue with Eatwell and colleagues by arguing for a Phillips Curve in the Phillips/Lipsey form. The Copeland paper originates within the monetarist group at the Manchester Inflation workshop, which although at this time was still considered the underdog by itself and others, would soon lead the way towards the new monetarist orthodoxy.

The analysis here is interesting for two reasons. Firstly we can see how three papers approach very similar data-sets in differing ways, and secondly see how both the Thomas and the Copeland papers reject Eatwell and colleagues' claims while drawing opposing conclusions of their own.

Eatwell *et al.* (1974) begin their paper by drawing a distinction between two causes of inflation, 'special' and systematic effects. They define the special effects as infrequent and usually one-off shocks to the system, such as social conflict, devaluation, or significant changes in state intervention in the bargaining process. Unlike the second set of influences, these are considered too unsystematic to be suitable for investigation of this sort, and subsequently Eatwell and colleagues make a determined effort to minimise their impact on the data-set. This is achieved through basing the sample in a strategically chosen time period, essentially embodying a special effects explanation in the macro data-set choice. Eatwell and colleagues include fifteen industrial nations in their study for the period 1958-67. They argue this is a period of relative stability in these economies, with only the collapse of an incomes policy in the Netherlands and Israel's devaluation as examples of special effects. These, for now, are allowed to remain in the data-set.

Once the sample is developed, Eatwell *et al.* (1974) discuss how to represent the theoretical construction of the key industries in the available data. This is the development of a proxy, as the theoretical concept of the 'key industries' is not an obviously available measurable phenomenon. After discussing the inherent difficulties in defining the key industries, and how it has been achieved in other papers, they opt initially to use the three industries with the highest productivity rates as a proxy. Later an attempt is made to use alternative formulations for deriving the leading sectors, based upon those industries with 'distinct' higher productivity, using several definitions of distinct. Eatwell *et al.* (1974) claim these differing computations have limited effect on the conclusions drawn, and usually result in a slightly higher rate of influence for the leading industry variable.

Eatwell *et al.* (1974) then turn their attention to defining a data-set to represent the long run trend in money wages. In a footnote Eatwell and colleagues note that earnings as opposed to wages should be used, due to plant level bargaining forcing a distance between the actual rate being paid and the conventional measure of wages. Later this is shifted to average rates of growth of earnings as opposed to the rate of earnings because in many instances the three industries composing the leading sector are all based in manufacturing, which experiences high structural shift in the labour force. Here Eatwell and colleagues are transforming the data as a response to intuitive speculation and experiential knowledge of the practices within the field from which the data were collected.

We witness another instance of transformation when Eatwell *et al.* (1974) calculated their productivity growth rates from the arithmetic difference between the growth rate of output and the growth rate of employment for each country. Both of these data-sets came from UN publications, with the output data from the Index of Industrial Production and the employment data their General Industrial Statistics data-sets. This reinforces our sense that the macro data-sets used are chosen and manipulated from a number of available data-sets.

These data-set choices also constrain the analysis. To test the key industry hypothesis an analyst needs a clear sense of the boundaries between one industry and another. The boundaries are not obvious and could be configured in numerous ways. However, in practice the most workable definitions are based upon the availability of data. Eatwell and colleagues do not provide a full account of how the limited formats of available data impact upon the construction of these groupings in each country, but do indicate that, in this instance, the transformation involves collating smaller data from similar industries and calculating from a constructed combination of data sources.

Before performing their regressions Eatwell and colleagues note that the average rate of growth for earnings in the Netherlands is considerably different from the rest of the sample as the rate of growth of earnings is much greater than the average rate of growth of productivity in the top three, a position that is uniformly found in the opposite in all other cases. The Netherlands had earlier been isolated as experiencing special effects in the form of the collapse of an incomes policy during the sample period. Eatwell and colleagues argue that this is reason to cast doubt over any results including the Netherlands data. In response they opt to perform all the regressions twice, both with and without the Netherlands data. Here we see another response to the special circumstances issue faced by many papers in the Phillips Curve debate. This response is much closer to Lipsey's micro data response than Phillips' to the critique of substituting Bowley for Phelps Brown-Hopkins series data from the years 1881-6. In both cases the papers do continue physically to do multiple regressions, including and excluding the disputed manipulation to the data, but only consider the variation more in keeping with their expectations when analysing the data and developing conclusions.

Eatwell *et al.* (1974) run regressions on the leading sector rate of productivity growth against rate of growth of wages as a test of the key sector hypothesis, and a regression using the average rate of productivity growth across all sectors for purposes of comparison. They conclude that the evidence supports the key industry hypothesis above the average rate of growth across all sectors data, with 83% of the international variation in wage inflation being associated with the three sectors with the highest rate of growth of productivity. However, their conclusions are not the concern here. Instead we are interested in how they took a range of published data sources and manipulated them into a format that confirmed their expectations of the key industry hypothesis. Now attention will turn to the two subsequent papers that, despite their own differences, both rejected Eatwell and colleagues' claims.

Turning to the second paper of our cluster of three, Copeland (1977) also runs two regression types, a 'leading sector' regression and an 'average productivity in all sectors' regression. However, unlike Eatwell and colleagues, he concluded the average rate of productivity growth provided an unambiguously superior explanation of the rate of change of money wages. After running the tests in several formats he found the leading industry variable could never explain more than 30% of the movement in the rate of change of money wages, compared to the 83% claimed by Eatwell *et al.* (1974). How is this possible? In a footnote Copeland attributes the differences to the substantially different methods of collecting and transforming the raw data. He directs the reader to the appendix, where two reasons for the differences are detailed. Firstly Copeland made a different macro data-set decision by including four years worth of data, 1968 to 1972, too recent to be included in Eatwell *et al.* (1974). To minimise the bias of this the data were transformed to a common level of disaggregation between the new and old data. Secondly, nine of the fifteen countries disaggregated male from female respondents. The two sets of writers made different transformation decisions on this, as Eatwell and colleagues chose only to include data referring to men while Copeland aggregated both sexes, weighting the ratio between men and women on employment rates. Outside of these two changes, all the remaining macro data choices are the same as those featured in the Eatwell, Llewellyn and Tarling paper.

Copeland also used a different specification of the key sectors proxy. He saw no reason to follow Eatwell and colleagues in identifying three as having any more merit that any other number of industries, and instead used a varying number of industries, from one to four, in each case. In principle this re-specification increases the chances of Copeland finding results that lend support to the hypothesis.

A final noticeable difference between the two is the use of special effects. Copeland chose to include the Netherlands in the regressions on which his main conclusions are made, although he does include the results excluding the country. He comments that Eatwell *et al.* are only able to achieve positive results for the Key Industry hypothesis when the Netherlands was excluded. He does not provide a justification for including the Netherlands data in his conclusions, while noting that there is precedent for papers trying to establish Phillips/Lipsey Phillips Curve style analysis omitting the country. Including the Netherlands in this way, of course, lends further support to Copeland's arguments.

Copeland's paper continues by introducing another paper and developing another set of tests, that will not be commented upon here. However, in conclusion he argues that any small impact that wage leadership may have in the inflationary process can only hold in the short run, which is coherent with the view that the labour market only plays a role in the transition of inflation and not its causation. This alludes to a construction of inflation compatible with the monetarist conception of the natural rate of unemployment and the Friedman/Phelps Phillips Curve.

As with the previous examples Copeland's results conformed to his expectations. As with all the papers discussed up to this point, Copeland configured the six categories of data handling in such a way to ensure this occurred. This paper was Copeland's first, and was written under the guidance of Michael Parkin, a leading British macroeconomist then at Manchester. Recalling his introduction to the macroeconomics profession, and the goal orientated nature of macroeconomic empirical work, Copeland explains:

> Laurence Copeland: "I walked in green as grass and Mike [Parkin] said to me 'your first job Laurence' he said 'go and get that paper Eatwell, Llewellyn and Tarling in *Review of Economic Studies* and replicate their results' I said 'OK, I'll get on with it' he said 'OK I want it by the end of the week', I took that seriously you know, I was totally green, in fact it took me about three months to replicate their results, so that was my first job, replicate their results and show that their results were crap, which they were"
>
> Interviewer: "That's what you were asked to do?"
>
> Laurence Copeland: "Yeah, I think that was the words they used probably knowing Mike Parkin"

Copeland then, like many macroeconomists, was actively engaged in providing the best possible arguments against an opposing view.

Another macroeconomist actively engaged in providing the best possible arguments against Eatwell and colleagues was Leighton Thomas. He adopted a converse position to Copeland, and in rejecting Eatwell *et al.* advocates an alternative explanation that is closer to the Phillips/Lipsey Phillips Curve. Thomas casts doubt on Eatwell *et al.'s* findings by claiming eight of the fifteen countries they studied had earnings growth rates in the top three sectors below that of the average for all sectors. He highlights Sweden and Japan as particular examples of this. Thomas claims Eatwell *et al.*'s findings are misleading, and that the variations found between countries can be explained by variables traditionally included in the Phillips/Lipsey Phillips Curve, in particular the level of unemployment and the extent of unionisation.

Thomas criticises Eatwell *et al.'s* macro choice of data. He notes that any cross-country comparison is inherently problematic due to the lack of comparable data. He points to differences in how unemployment statistics are compiled in Europe and the United States. For his macro data choice he adopts Maddison's (1964) data for 1950-60 that has been adjusted for these differences. Maddison's data include nine of the fifteen countries in Eatwell *et al.* (1974), and Thomas then adds three more countries where the government compiled figures are compatible with the U.S. format. Thomas uses transformation techniques to further increase the macro data-set to extend the data-set up to 1967 by adjusting the official figures for these countries by the ratio of the difference between the official figures for the years covered by Maddison and the official statistics for those years. These manipulations are claimed to increase the comparability of the data between countries, allowing for a better test of the key industry hypothesis.

Thomas constructs the leading sectors rate of productivity growth variable in the same way as Eatwell and colleagues. He regresses the twelve nations' average earnings growth twice, once against Eatwell *et al.*'s key industry variable and then against percent of the labour force unemployed, the classic Phillips/Lipsey Phillips Curve component. The unemployment regression has a higher level of determination associated with it than the key industry hypothesis, although both are significant. He then regresses with both variables together and finds the key industry variable ceases to be significant. Thomas then turns his attention to the issues of excluding the Netherlands. He accepts that the productivity variable performs better without the inclusion of the Netherlands, but argues against its exclusion. To Thomas the special effects motive does not stand up to scrutiny, as the higher than expected growth in earnings exists both before and after the collapse of the incomes policy. Therefore the policy collapse cannot be justified as an explanatory variable. He continues to argue that if you use the unemployment variable there is no need for any special explanation, as here the Dutch data give similar results to the other nations.

Thomas now introduces a variable for the percentage of the labour force that is unionised. He notes that few people have used this in empirical studies of wage inflation, and identifies two of his own earlier works that stand as exceptions

(Thomas, 1974a & 1974b). He tests for this and finds that in combination with the unemployment data a high degree of explanation is achieved.

To finish, Thomas notes that Eatwell *et al.*'s results may be better explained by the argument that both the rate of wage inflation and productivity gains may be the result of excess demand pressure, reflected in the unemployment statistic. Thus Thomas suggests Eatwell *et al.*'s identified relationship is not one of the rate of change of productivity impacting upon the rate of change of wages, but instead both variables responding to changes in unemployment. In effect, something very close to the Phillips/Lipsey Phillips Curve.

In the discussion of the second cluster of papers we have seen yet further examples of the six categories of data handling. There have been macro data-set choices in selecting the number of years as well as Eatwell *et al.*'s intent to pick years that minimise the impact of special effects. The papers have been riddled with data transformation, from Eatwell *et al.*'s combining of varying national statistics into uniform comparable industry groupings to Thomas' adjustment of 1960-7 government figures by Maddison's ratio of adjustment for U.S. comparability. Again a proxy is at the heart of the analysis, as Copeland took issue with Eatwell *et al.* over the correct construction of the key industries variable. Debates over micro data-set choices have been central to deciding the outcome of each paper due to the contested legitimacy of excluding the Netherlands data. Special effects have spearheaded both the micro and macro data-set choices, although have been less pronounced as a stand alone category. Finally, again the interpretation of data has revolved around the legitimacy of including the Netherlands data in the conclusion.

Where as the first cluster of papers contained two publications orientated towards establishing the same phenomenon, and the second cluster concerned a debate between three competing conclusions from similar data sources, the third cluster explores two papers orientated towards critiquing an established idea. The methodical deconstruction of one group of macroeconomists' empirical work by another will provide our final insight into the lived practice of data handling.

5.3.4 Critiquing the Use of Data in Trade Union Studies

Purdy & Zis (1974) subjects the empirical work of Bertie Hines to critical scrutiny (Hines 1964, 1968, 1969, & 1971). Hines was the first to heavily commit to supporting the hypothesis that Trade Union pressure on wage rates is a significant independent cause of rising prices. Ward and Zis (1974) make further tests to contribute to the conclusions previously drawn, and extend their criticisms to other writers in the area. All three authors were based at Manchester University, with varying links to the Manchester Inflation Workshop, the hub of British Monetarist thought in this period.

Hines argues that many writers either dismiss the possibility that unions may affect the rate of change of money wages independently of demand, or use unsatisfactory methods of identifying Trade Union pressure. His preferred measure is an index of union militancy. This is not directly measurable itself, so Hines suggests the rate of change in the proportion of the labour force belonging to trade unions as a proxy. The conclusion drawn by Hines is that, since the period before World War One, unemployment, taken as a proxy of excess demand, has become progressively less, and 'institutional' forces progressively more important in determining the rate of change of money wage rates. In particular, union militancy has emerged in the post-war period as a key factor in determining the pace of wage inflation.

Purdy and Zis set their task as highlighting the problems with Hines' data and suggesting appropriate adjustments. Hines' first investigation measured Trade Union density as the ratio of Trade Union membership in the U.K. to the labour force of Great Britain. The labour force data used included the total working population. Purdy and Zis re-estimated the simple regression change in wages for change in Trade Union membership after making several transformations of the labour force data. They change labour force of Great Britain to the wider U.K. data-set, they then remove employers, self-employed and armed forces from the working population, as these groups would not become unionised. They also remove the unemployed. Upon estimating for the post-war period Purdy and Zis find the adjustments do not seriously change Hines' results. In fact the explanatory power of the change in membership variable is improved when employers, self employed and armed forces are removed. Purdy and Zis account for this by recourse to a special effect by referencing historical changes in the definition of Trade Union membership. They conclude that this demonstrates the bias introduced by Hines' mis-specification of the labour force variable is negligible in the post-war period.

However Purdy and Zis claim Hines' transformation creates a mis-specification in the inter-war period that is far more serious. In Hines' data the labour force series is based upon interpolation from a straight line time trend fitted to just two observations in the total occupied population taken from the 1921 & 1931 censuses. Purdy & Zis argue the use of a liner interpolation for a variable known to fluctuate is problematic. The difficulties with this data-set are compounded because the operational definitions employed changed between the two censuses. A final criticism made is that Hines' inter-war data includes the unemployed, managers, employers and the armed forces who would not be eligible for Trade Union membership. Purdy and Zis instead advocate using the numbers insured under the unemployment insurance scheme to represent the labour force eligible for Trade Union membership.

The interesting point here, and the reason why examining a paper orientated towards criticising the empirical work of another is so fruitful, is that Purdy and Zis were unpacking Hines' use of the six categories of data handling. In this paper they pay particular attention to Hines' use of transformation in constructing his data-set. It is worthy noting that Purdy and Zis did not argue the problems exist in Hines' work because of excessive modification to his data. Instead they claimed the data-set required further transformation to ensure true representation was achieved.

Having identified and resolved the problems in Hines' data-set Purdy and Zis recalculate the relationship, in this instance to a great difference. The proportion of variance explained by changes in union membership drops to under a third of that explained by Hines' militancy index. In addition they find auto correlation in their regressions, a common statistical error where the residuals are correlated. This is found to be removed when the level of union membership is included as well as its rate of change. However, the inclusion of the level of Trade Union membership variable further weakens the strength of the rate of change of union membership explanation.

Purdy and Zis take their data-set yet further away from Hines' with a macro data-set change, updating the index from the early 1960's to 1969. They claim their regression co-efficient is not dissimilar to Hines' inter-war estimate, thus challenging Hines argument that the co-efficient on change in the level of union membership has undergone a permanent increase in the post-war era. They soon embark on more refutations of Hines, particularly Hines (1964), and engage in more critique of his transformation of data. Explaining these will not add anything qualitatively different from the analysis developed so far, but does lend quantitative support to the arguments, while further illustrating the breath of potential alternative specifications of data. Thus as a brief outline, Hines is criticised for basing his inflation variables on the Gross Domestic Product in 1921-58 and the Retail Price Index for 1959-61, when the RPI is more appropriate throughout. He is also criticised for using end-of-December wage series for his index of hourly wage rates, while his price series consisted of annual averages. By doing so Hines encountered issues of differing base lines and greater data smoothing in the annual averages.

Ward & Zis (1974) extends the critique by conducting further in-depth tests of the Trade Union hypothesis. Their macro data-set choice is to compare Belgium, France, Germany, Italy and the Netherlands with annual data for the period 1956-71. For the purposes of comparison they also present some data for the U.K., but do not attempt a full scale investigation of the U.K..

Ward and Zis tested for impacts upon wage inflation from a number of variables. Included were three measures of Trade Union militancy, the level of unemployment, the proportional rate of change of the cost of living, and the expected cost of living. The three versions of Trade Union militancy all used strike activity as the proxy, but differ in the form used to measure it. The choice of strike frequency, number of workers involved in strikes, days lost through strikes for measuring strike activity demonstrates that to construct a proxy a macroeconomist must make two sets of judgements. Firstly what real world data would make the best proxy for the phenomenon under study and then how best measure that proxy. Ward and Zis felt that if strike activity is a reliable proxy for union militancy then there should be a close correlation between each pair of measures. This was not altogether the case and this led them to suspect that the choice of measurement used will impact upon the eventual conclusions drawn. This conclusion is entirely in line with the argument made here.

They construct a data-set for all three measures for all countries, excluding one for the number of strikes in Germany, where the data were unavailable. This is far from the only restriction placed upon their work by the availability of data. They note that the data include strikes initiated for any reason, not just wage related claims, and suspect that this might again skew the results of the data. Furthermore they can only test Hines' rate of change in union membership variable for Germany and the Netherlands as these were the only nations where the data could be found. As a final complication Ward and Zis could not find data that excluded employers, the self employed and the armed forces, the groups who did not engage in union activity. As we have seen data issues of this nature, and the decisions made to counter these problems, impact upon the final results gained.

Ward and Zis could have responded to these problems with a range of creative strategies. They could change their adopted stances on any one or more of the six categories and potentially alter their findings. In practice they configured their decisions in the way described in this section. The argument here is that it is no co-incidence that with this configuration Ward and Zis were able to conclude that only Italy showed notable influence for Trade Union militancy, and then only for two of the three measures. Armed with broadly the same data, other writers, for example Bertie Hines, would be able to conclude in the opposing direction. Indeed as we saw above, in the case of Purdy and Zis (1974) this is exactly how events transpired.

5.3.5 Discussions of Economists' use of Data in the History of Economic Thought Literature

There are a number of contributions to the History of Economic Thought literature that are in keeping with the argument presented in this chapter. However there are also differences in the arguments. As illustrated in Chapter Two, these differences are usually found in the conclusions and are typically due to the differing goals of the field. Let us explore some examples.

Two very clear examples of this are the papers written directly on the use of data in constructing the Phillips/Lipsey Phillips Curve by Nancy Wulwick (1989, 1996). In many ways these papers pre-empt the analysis featured in the first section of this chapter. Both explore the specifics of how Phillips and Lipsey performed their statistical analysis. However although Wulwick provides essentially the same data displayed in the same way, she does not offer the same conclusions. This is because working within the history and methodology of economics she approaches the data with a different background and interests. Let us briefly explore each paper in turn.

The 1989 paper explores Phillips' paper exclusively. Similar to the arguments made here, Wulwick demonstrates that Phillips' methodology did not utilise the regression analysis that was quickly becoming the norm in macroeconomics at the time. Also, in parallel to the analysis here, the paper explores how Phillips moulded his data and configured his statistical techniques to ensure the results fitted his expected outcome. By doing this Wulwick has offered a demonstration of Interpretative Flexibility in macroeconomics. However this is not made explicit and the full implications are not drawn out. The central difference is that in the Wulwick paper the unorthodoxy and inherent moulding of the data are seen as actions against the norm of macroeconomics and a concern for its methodology. Instead, as we have shown, this type of action is indeed the norm, and that many macroeconomists are both aware of Interpretative Flexibility and accept it.

The 1996 paper again provides a detailed explication of Phillips' statistical technique, this time joined by the same for Lipsey. True to the spirit of a History of Economic Thought paper, detail is at its heart, and the lively discussion including quotes from correspondence with macroeconomists provides a more human account of the time. However, as with the 1989 paper, the methodological interest again draws the conclusion away from the form taken here. Wulwick attempts to replicate both Phillips' and Lipsey's papers, and concludes that it is only possible for Phillips' work. This is again presented as an exercise to further the pursuit of economic objectivity by demonstrating the looseness of the massively influential paper. Why, Wulwick asks, if the paper has been read and replicated so many times has nobody realised Lipsey's results do not add up? The most heinous crime Lipsey is accused of is not basing his empirical work on the data he claimed to have. Wulwick demonstrates this by noting that, for example, Lipsey claims to have used only the Phelps Brown and Hopkins price index data for observations up until 1939, however the Phelps Brown and Hopkins data only go up to 1938. Wulwick claims that, if the wider profession had been aware of these discrepancies at the time, the Phillips/Lipsey Phillips Curve might not have received such wide acceptance. This thesis would argue against this as, as with the critique of the 1987 paper, these forms of manipulations are familiar to macroeconomists and are not subject to scorn. Indeed good data manipulation is considered good macroeconomics.

The 1996 paper does, however, have one extra conclusion that is interesting and pertinent in this context. Wulwick claims that her ability to replicate Phillips' results but not Lipsey's reflects the different backgrounds of the two researchers. Before coming to macroeconomics Phillips was an engineer, and was accustomed to maintaining thorough records of his data sources and calculations. This was not the case for Lipsey, whose background in macroeconomics did not habituate record keeping. This distinction explores an interesting sphere of socialness in macroeconomists' use of data.

Another account of macroeconomists' use of data is provided by Kim, De Marchi and Morgan (1995). They look at a number of early papers on Rational Expectations. This includes contributions by Robert Lucas Jr. and Thomas Sargent, two significant contributors to the Rational Expectations argument who are discussed later in this thesis. Kim, De Marchi and Morgan argue that Lucas and Sargent were looking for confirmations of particular characteristics of their empirical models in their data-sets. Kim *et al.* unpack the statistical methods used in several of the authors papers and show how judgments are made that impact upon the results and interpretation of the tests. These strategies include labelling failing models 'not operational' and the shifting assumptions in tests from one that failed to another that gave strong results. They note in one instance that what Lucas (1973) claimed to be a successful test was no more than a statement that the theory was not contradicted by a specific test on one specific implication of one specific version of it.

Kim *et al.* show more congruence with the arguments proposed in this chapter than Wulwick's (1989, 1996) conclusions. Instead of deeming the manipulations of the data a methodological weakness particular to these specific tests, Kim *et al.* recognise that these practices are a common feature of econometrics. However, they do continue to make the methodological point that, for a number of reasons, tests using the level of tight focus found in these papers cannot be used to justify the wider theories in the way the authors claim they do.

Further acknowledgement of the type of practices discussed in this chapter are evident in Backhouse and Morgan's (2000) discussion of whether data-mining should be considered a methodological problem. Here the techniques of reconfiguring their mathematics, labelled data-mining by its detractors, allows the econometrician to understand the strengths and limitations of the phenomena. The processes they label data mining have much in common with the six categories of action employed in macroeconomists work detailed throughout this chapter. They characterise both the public attitude of econometricians and the philosophies of science prevalent in the 1940s to the 1960s as outspokenly against practices of data-mining. Backhouse and Morgan also concede that these practices are an inevitable and frequent occurrence in econometric research. They also argue that similar practices are evident in the natural sciences and in fact can be offered as positive strategies for the econometrician to follow.

They criticise the view of econometrics as a process of theory testing, instead proposing that such research is most fruitfully considered an arena for the analyst to interact with the phenomena they are studying. They come to this conclusion with reference to explorations of the social construction of knowledge in the natural sciences. Collins' (1974, 1995, 2001) notions of tacit knowledge and replication are used to demonstrate that the idealised 1940s-60s philosophical rules operate as loosely in the experimental settings of physics laboratories as they do in econometrics. Backhouse and Morgan develop an example from radio-astronomy to demonstrate the benefits of manipulating the available data in order to find its boundaries and limitations. Establishing such boundaries can be a finding in itself. Furthermore, the processes of data manipulation can spur further theoretical ideas.

Backhouse and Morgan, of course, are primarily interested in the methodology of econometrics, and subsequently the paper is not concerned only with exploring the processes that make sciences work, but also with providing insights into how it can be improved. To this end they suggest the econometrician should not be embarrassed by their data-mining, indeed through its considered usage it can improve their research. By understanding how experimental techniques work in practice they can be used to better effect.

Mirowski (1995) also contrasts traditional perspectives of economic methodology on the empirical practices of econometrics with positions similar to that adopted here. He draws upon writers within the Social Studies of Science literature, which he terms the Critical Postmodern Movement, to argue there is no fixed hierarchical relationship between theory and empiricism as the traditional view would have us believe. He stresses that modern econometrics is a fragmented discipline employing multiple heterogeneously applied statistical formulations. Like Backhouse and Morgan (2000) Mirowski argues that econometrics could be improved if these realities are accepted.

Finally numerous accounts of similar patterns of data manipulation in the decades prior to the Phillips Curve debate are provided by Morgan (1990). Just one example is the relatively unproblematic estimation of demand curves in the 1920s. To solve the correspondence problem of linking their data to the theoretical model economists would make changes to their data to create a well-defined market, a group of buyers at a particular stage of the marketing process, and a sensibly defined time unit as required by demand theory. Such changes to the data are echoed in our account of the empirical strategies employed during the Phillips Curve debate.

The literature discussed here certainly lends support to the arguments of this chapter. They describe similar empirical strategies from before, during, and after the period studied here. However their conclusions are always both methodological, and orientated towards improving economists' research practices. Subsequently they do not use these observations as evidence for Interpretative Flexibility in macroeconomics, and the associated conclusions that go with it. We now discuss and expand upon these conclusions.

5.4.1 Understanding how Macroeconomists Handle Data

Let us draw together the dynamics presented in this chapter back into the single argument. We have seen how the six categories of data handling are embedded throughout macroeconomists' empirical work. Macro data-set choices are endemic in any empirical work. Data transformation is a constant factor as macroeconomists seek suitable data sources and configure them to their needs. The development of proxies is frequent throughout macroeconomics, and inherent in Phillips Curve research, where unemployment is used as a proxy of aggregate demand. However we have seen this is far from the only instance, as both Trade Union militancy and key industries were proxied in the select number of papers discussed here. Analysts are always afforded the flexibility of micro data-set choices and special effects explanations. Finally, further flexibility is found in the priorities given in the interpretation of data. The History of Economic Thought literature demonstrated that these dynamics are also present in other debates and other time periods.

The configuration of the six categories of action is orientated towards a desired result. This is obvious in all three clusters of papers. The researchers entered the research process with an expectation of their outcome and whittled away alternative results through the configuration of the data. If initial runs of the regressions did not confirm their expectations then the six categories are reconfigured in an attempt to improve the fit. This process can be repeated until the desired conformation is achieved.

The mechanism that accommodates this process lies within the relationship between the identification of problems requiring attention with the data, and the prior expectation of the nature of a correct response. Identification of problems with the data can come from two sources. The first is the existing macroeconomic literature and common practice. Certain norms develop within macroeconomics that analysts may abide by, for example eliminating auto-correlation. Furthermore macroeconomists may argue for specific methodological precepts as part of a localised debate, for example Guy Routh on Phillips' work. However, macroeconomists need not follow these norms should they prefer not to, for example Richard Lipsey on Routh's critique of Phillips.

The second source of identifying problems with the data-set is the one central to the argument here. Macroeconomists will recognise problems with their data-set and the configuration of their variables directly because the results to not conform to their expectations. The criteria for a successful test is when it conforms to their prior expectation. If this is not fulfilled then the empirical research is not complete. As the Peter Stoney quote in the introduction to this chapter shows, empirical analysis is a process of constructing the very best fit for the data achievable. The skill of empirical macroeconomics lies in the creativity with which the six categories are configured to produce the optimum result. Good macroeconomics *is* good data manipulation.

It should be noted that this notion is not purely a technical maxim but a moral one as well. Good data manipulation is not only good macroeconomics because it achieves high R squared figures. There is no sense of deceit or corruptness in the actions accounted throughout this chapter. Good data manipulation is also morally good and morally correct macroeconomics.

Here we return to the Edward Kuska quote also in the introduction to this chapter. He claimed that macroeconomists could ensure the results to their test fitted their expectations, but noted that this was an unconscious action. At that point issue was taken with Kuska over this, because, as has been illustrated in this chapter, at least some macroeconomists are fully aware they are seeking the optimum results for publication. However, perhaps Kuska's quote should be interpreted in an alternative fashion. Perhaps Kuska is alluding to an unconsciousness to any immorality associated with the behaviour. Kuska refers to the processes of data handling as fiddling the data. The term 'fiddling the data' suggests an immoral act in the traditional discourse of science akin to data-mining as discussed by Backhouse and Morgan (2000). Kuska, perhaps as a protective action on behalf of his colleges, is using unconsciousness to distance macroeconomic empirical work from any immorality. However he need not. The David Laidler quote exposes the format of macroeconomic morality on this issue. After listing several forms of data transformation, Laidler claims these actions are acceptable to macroeconomists, while maintaining a distinction between these manipulations and falsifying the data. To do that is labelled outrageous. Kuska's rhetoric and subsequent response alludes to a difference between the moral scheme associated with empirical work in generic science and that operational amongst macroeconomists from the late 1950s till the early 1980s. The difference being the permissibility of intentional manipulation of the data to produce the strongest conformation for an argument. Macroeconomists are aware of this in their own work, and as we shall see in Chapter Seven, they are also appreciative of such manipulations in the work of others, and mobilise a set of strategies to account for them.

In terms of the dynamics directing the changes in macroeconomic orthodoxy the processes discussed in this chapter appear impotent. This is because the natural conclusion from the Interpretative Flexibility of macroeconomic empirical work is that data alone cannot settle debates. This is because the data can be disputed and reconfigured to produce a variety of outcomes. However there is still a positive conclusion to be made. The Interpretative Flexibility of data acts as a lubricant for changes in macroeconomic orthodoxy as it removes any resistance by empirical results to accepting a new position. Should, for some external reason, macroeconomists change their positions on the Phillips Curve debate they will not be restrained by the data, as it can be reconfigured to conform to the new expectation. The role of Interpretative Flexibility as a lubricant will be present throughout this thesis, and will contribute well to the final account of why the cycle of contest and closure in macroeconomics occurs so quickly. This conclusion is clearly not present in any of the History of Economic Thought arguments discussed above which reflects the different orientations of the analysts.

As a caveat to this discussion it must be noted that not all papers fit the goal orientated manipulation of data model described here. A minority of empirical research is carried out without a prior conception of the outcome leading the analysis. Instead the research is driven by curiosity or perhaps a need to publish. Indeed an example may be Phillips' paper itself. Unfortunately Phillips' motives for his paper are no longer accessible. Speculation from interviewees in this research, some of whom knew Phillips well, tends towards suggesting his curve did arise from such loose speculation. It is the case that he had no background in the area which may lessen any commitment to a particular position in the debate. Either way it matters little to the conclusions drawn here. Such papers are a minority, and very few achieve anything approaching the influence of Phillips' in wider macroeconomic debate. Equally whether they are orientated towards an expectation or not the researchers still must engage with the six categories of data handling.

5.4.2 How Data Handling Relates to the Rest of the Thesis

This chapter has illustrated how macroeconomists use empirical resources to confirm their own expectations. It is the first of three chapters that undertakes the essential task in any Sociology of Scientific Knowledge research project to document the Interpretative Flexibility of the debate, as dictated by the EPOR. The next chapter, Chapter Six, explores similar territory regarding theoretical resources. Both chapters discuss only how analysts produce their own work. Chapter Seven extends the analysis to how macroeconomists evaluate the empirical and theoretical work of others, given the processes illuminated in this and the next chapter.

This chapter has spoken frequently of macroeconomists orientating their work towards a desired outcome, but has remained silent on the origin of this expectation. This is the topic of the remaining three chapters, and it is here where the discussion turns to the role of political interest.

6. How Macroeconomists use Theory

6.1.1 Introduction

In this chapter we will explore how macroeconomists develop their own theoretical models. As with the previous chapter, this further contributes to the demonstration of the Interpretative Flexibility in the Phillips Curve debate that is an essential characteristic of research in the Sociology of Scientific Knowledge (SSK) and a necessary stage of the Empirical Programme Of Relativity (EPOR). In the previous chapter we discussed how Collins' notion of the experimenters regress underpinned the principle of Interpretative Flexibility. The same argument is made for this chapter. However it is also worth noting Daniel Kennifick's (2000) exploration of the theory of gravitational forces developed the 'Theoreticians' Regress', an analogous concept orientated towards theoretical pursuits. Both support the inevitability of Interpretative Flexibility. Two clusters of papers will demonstrate how theories are built to produce the results the macroeconomist expects. The analysis has similarities to the previous chapter on the use of empirical resources. However there are differences, and where these arise they will be made explicit. We commence by articulating the theoretical argument of the chapter, then applying it to some empirical examples, and finally exploring similarities with the History and Methodology of Economic Thought literature.

6.1.2 Macroeconomic Theory as a Building Process

Macroeconomic theory is a creative act. Good macroeconomic theory configures elements of accepted macroeconomic thinking in a format that reproduces the patterns of macroeconomic behaviour witnessed in empirical work. There are numerous macroeconomic theoretical tools and many more ways to combine them. Successfully to create a theoretical model that predicts correctly requires initiative, technical competence, and patience.

Firstly let us expand upon the meaning of macroeconomic theoretical tools. Each tool is a concept embodying a perceived macroeconomic force. They represent a pressure that has the ability to impact upon the macro economy. A simple example would be demand, a theoretical tool representing people's desire to buy a good or service at a range of prices. The presence of demand exerts a pressure on a theoretical model that may, for example, result in the formation of new companies to provide for this demand. The exact response in the model depends upon how the analyst configures all the theoretical tools within it. To be accurate, a tool does not create the force it embodies, but instead transfers the force exerted upon it by another force. Perhaps in the case of demand an example would be the changes in the wage level tool that provide its force. A model can contain many tools that are ascribed varying strengths and are configured in such a manner that the shifts of influence between them claim to produce the same patterns of behaviour as the real world phenomena they model.

In this way the theoretical models are built by combining the available tools to make predefined patterns of behaviour. The discourse of building is purposely employed, as the construction of a machine is an insightful metaphor for the practice we observe. The discourse of theoretical tools employed here is not one used by macroeconomists themselves. In the terminology of macroeconomic theory they are known as concepts or assumptions.

Soon we shall witness how small changes in the configuration of the available theoretical tools can produce widely varied outcomes. The ability of macroeconomists to introduce one or more of the available tools at their discretion to permit the production of the anticipated pattern of behaviour will be illustrated, as well as the potential for them to develop new tools of their own. First we shall discuss three issues that can, in a nuanced and heterogeneous manner, embody values that constrain macroeconomists' theoretical work.

6.1.3 Fitting the Data

The first is the fit to the data. In the large majority of cases this is the guiding criterion of success. An observed empirical relationship in macroeconomics is not considered understood until the theoretical model can produce a pattern of behaviour that mimics the observation. An inability to achieve this frequently leaves macroeconomists uncomfortable with their empirical work. Phillips' original 1958 publication is an excellent example of this, and we can observe how the discomfort mobilised an effort to accommodate the observed pattern in the existing theoretical scheme.

However, there are instances where an analyst may place their theoretical convictions above the empirical data, and argue the interpretation of the data is incorrect as they contravene valued theoretical precepts. This most usually occurs when macroeconomists judge the work of others. As was shown in the previous chapter, should a macroeconomist have a theoretical precept that their own data does not realise then the empirical work will typically be considered to have fallen short of the criterion of success leading to subsequent reconfiguration of the data. This is less severe than an analyst placing their theoretical convictions above the empirical data because the mathematics are still in at the developmental stage. In accordance with this the discussion of theoretical concerns outweighing empirical work is left to the following chapter.

6.1.4 Linkages to Existing Theory

Any orthodox theoretical position has a conventional configuration for a number of theoretical tools. An example in Keynesian theory would be the decisive role of aggregate demand in determining real output. This places restrictions on an analyst's development of a new theoretical scheme to account for an empirical phenomenon. For their explanation to remain within the orthodox terms of an existing school of thought the theory developed must respect the conventional configuration of theoretical tools. The accepted configuration is then complemented with the addition of supplementary tools configured to produce the desired pattern of behaviour. If a theory fails to do this then it will not be acceptable to those aligned with the conventional school of thought.

6.1.5 The Intuitive Property of the Idea

Frequently a new theoretical construct is lent support if it is intuitive to the analyst. As we shall see, macroeconomists frequently speculate on the values and behaviour of economic actors engaged in the phenomena they are theorising. These speculations often inform the format their models take. However, only in rare cases are these anymore than speculation or reflection on personal experience. The research effort does not involve explicit attempts to measure such values. Instead they act both as guides for realism and as inspiration for usable creative theoretical mechanisms. However the role of intuition is heterogeneous and flexible. As we will see later in the chapter, in some instances the requirement of intuitiveness is disregarded altogether.

6.2.1 Empirical Demonstration of Theoretical Model Building: Two Clusters of Papers

We now explore these processes in two clusters of papers. The first will be a set of three papers that in part develop theoretical accounts of the 'Phillips loops', an observation made in the data by Phillips in his original 1958 paper (Phillips 1958). The cluster consists of Phillips own quickly dismissed theoretical explanation, Lipsey's subsequent 1960 account that soon became the orthodox Keynesian explanation, and an alternative 1966 Keynesian account by Edward Kuska that passed almost unnoticed (Lipsey 1960, Kuska 1966).

The second cluster of papers contains two publications, both widely read, one supporting the Phillips/Lipsey Phillips Curve and the other the Friedman/Phelps Phillips Curve. The First is Edmund Phelps' own highly influential 1968 paper that introduced his ideas to the economics profession, and the second is James Tobin's 1972 publication of his Presidential Address to the American Economic Association, that develops a theoretical argument defending the Phillips/Lipsey Phillips Curve stance (Phelps 1968, Tobin 1972).

6.2.2 Cluster One: Bill Phillips on the Phillips Loops

The Phillips Curve loops are small loops spotted by Phillips in his time series data. They are not visible in the trend line calculations, i.e. the graphical representation popularised as the familiar Phillips Curve. The loops only become apparent when the observations are charted sequentially year by year. They form continuous anti-clockwise loops around the Phillips Curve trend line. Thus the theoretical developments here are a direct response to properties found in the data. The economists regard the discovery of a systematic pattern in the data as something necessitating theoretical explanation, and the actions discussed in this section are an orientation towards this need. This demonstrates that actors within the debate are motivated into a set of actions by the contents of empirical material.

First we will consider Phillips' rationalisation of the observed loops. Being his own discovery, he was first to make an attempt at a theoretical explanation. However his analysis on this issue, which is entirely contained within one paragraph, would soon be considered ad hoc and insufficiently rigorous to be acceptable.

Phillips noted the values for rate of change of wages tended to be above his curve when unemployment was falling, and below his curve when unemployment was rising. This, he suggested, results from, firstly, changes in the level of competitive bidding by employers for labour, and, secondly, workers' ability to push forward wage demands. In times of rising business activity the demand for labour will increase, and thus the pool of unemployed labour will be shrinking, so the employers will be bidding up the wages offered to prospective workers to ensure their employment requirements are fulfilled. Equally as business activity declines so does the demand by employers for labour. Unemployment will be increasing, and thus employers will be less willing to increase wages and the workers will be in a less sturdy position from which to negotiate for them. Thus the observation for the rate of increase of wages in a year of increasing business activity will be above that for a year with an equal unemployment rate but decreasing business activity. This, to Phillips, produced theoretical loops matching those present in the data.

To Phillips his account complies with the three issues detailed above. The model produces loops as seen in the data. It is also based upon the commonly accepted theoretical tools of demand and supply. The original component in Phillips' work is that for a given level of unemployment the rate of increase of wages will be different depending upon the direction of business prosperity. The introduction of this theoretical tool is legitimised for Phillips by an intuitive notion of businesses employment motivations in differing economic climates. These characteristics are common in theoretical work. However that does not imply homogeneity in the specific patterns produced or the theoretical construction developed.

6.2.3 Cluster One: Richard Lipsey on the Phillips Loops

As in the previous chapter on the role of data in the Phillips Curve debate, it is fruitful to draw comparisons between the interesting yet quirky work of Phillips and the new orthodoxy of Dick Lipsey. Firstly Lipsey opted to measure the observation. He found 82% of the variance in rate of change in wages is associated with a combination of variations in unemployment and the Phillips loops. The loops can account for 50% of the variance not already associated with unemployment. This establishes the Philips loops as something worthy of explanation through a means more akin to the statistically orientated approach Lipsey was advocating for macroeconomics. Lipsey's theoretical work was not a process of speculatively reconfiguring theoretical tools to observe the end results. It was a determined effort to create a configuration that produced the loop shape pattern.

Lipsey's theory of the Phillips loops is tightly connected to his general theory of the Phillips Curve. It is rooted in the traditional Keynesian understanding of inflation, particularly as characterised by Hansen (1951), where the movement from dis-equilibrium within the economy (i.e. excess demand) towards equilibrium causes inflation. In addition to this, the Phillips Curve observation implies the speed of the movement towards equilibrium is dependent upon the amount of excess demand, as proxied by unemployment. Characteristically of the orthodoxy Lipsey's paper would come to represent, he is keen to stress the links to wider accepted theory.

First let us look at Lipsey's theoretical base for the Phillips/Lipsey Phillips Curve itself. Lipsey's choice of theoretical tool is an 'adjustment function', an equation that dictates the speed at which prices adjust to dis-equilibrium within each individual market. His second is frictional unemployment, meaning unemployment from moving between jobs, which provides the function with a non-linear shape in periods of dis-equilibrium. The third theoretical tool is his most creative move, noting that the aggregation of numerous individual market level Phillips/Lipsey Phillips Curves is necessary to create the economy wide Phillips/Lipsey Phillips Curve. The aggregation of individual markets ensures several key characteristics of the Phillips/Lipsey Phillips Curve are realised in the theory. For example, the Phillips Curve's non-linearity is ensured by the presence of frictional unemployment in the individual markets. Only in the unlikely instance of excess supply, and thus no frictional unemployment, in every constituent individual market would the aggregate Phillips Curve be linear.

The basis for the loops is also provided for through the theoretical tool of aggregation of industries' unemployment levels. An uneven dispersion of unemployment between the individual industries shifts the Phillips/Lipsey Phillips Curve up and down as the dispersion increases and decreases. In this interpretation the loops become a function of systematic variations in the degree of upward displacement. Periods of economic recovery effect different markets at different times, while falls in demand, at least during the early stage of a recession, are more evenly distributed.

What Lipsey has done here is locate the inverse relationship between the rate of change of wages and unemployment, i.e. the trend line Phillips/Lipsey Phillips Curve, in each individual market. Thus it becomes a micro-economic phenomenon. The loops do not exist in the individual markets. These only appear in the aggregation of the micro-markets into the economy wide model, and the source of the loops lies in the aggregation, not a property of the individual wage bargaining processes, as Phillips had argued.

Let us reiterate the notion of theoretical development as a building process. Lipsey's combination of standard market analysis, frictional unemployment, and differing unemployment levels between industries *made* a model that *made* loops. All of these concepts had existed before, but had never been combined in this way. This is because loops on a Phillips/Lipsey Phillips Curve had never required an explanation before. The metaphor is of building and making an interpretation that produces a pattern of behaviour comparable to that witnessed. The theory made the loops, linked their production to the existing body of theory, and did so in an intuitively compliant way. Now we will see the same mechanisms in operation in the third paper of the cluster.

6.2.4 Cluster One: Kuska on the Phillips Loops

The Kuska paper is an exclusively theoretical paper, concerned entirely with providing an alternative Keynesian explanation to that offered by Lipsey. It did not become widely read and the theory presented here did very little to lessen Lipsey's aggregation hypothesis' position within the profession.

Kuska develops a set of models, adding new theoretical tools at each stage, to produce a model the produces loops like that found in Phillips' data. The first model assumes a downward linear function, linking percentage rate of change of money wages to frictional unemployment minus unemployment. This produces a Phillips/Lipsey Phillips Curve shape without any loops. This model is constructed in the same way Lipsey devised his micro-market Phillips/Lipsey Phillips Curve, which also did not contain any loops. It is the frictional unemployment tool that causes the dis-equilibrium position represented by the non-linearity. However, Kuska does not identify this as a micro-economic structure, leaving it in the realm of the wider economy.

Kuska's theory introduces the possibility of loops in his second, separate, model, through his next theoretical tool, the assumption of sticky wages. Stickyness was an established Keynesian idea that introduces dis-equilibrium into a system through inferring time lags due to a number of potential causes, such as limited information or contract constraints.

The assumption of no growth and thus constant supply is used to simplify the model and aid the creation of the loops. As this model as yet does not include the Phillips/Lipsey Phillips Curve relationship, it is simply movements of demand and supply in the labour market. Kuska speculates that given time workers and unions will find an average wage of their services in the market. When the wage falls below the average wage workers will raise their resistance to it, and employers will be aware of the probable affects on morale and be less likely to lower the wage level. When the wage is above the normal level firms may still not wish to respond as they will be focusing on long run profits, and know that the change in wages is only short run. Thus, through this intuitive hypothesising, Kuska concludes that in addition to the effect of excess demand in the simple demand and supply model, when the wage is below the stationary-state equilibrium wage there is a negative influence on the rate of change of wages, and vice-versa.

This configuration of tools provides clockwise ellipse shape loops, but not the more twisted shape loops witnessed in Phillips' data. When the first tool used, the unemployment equation of model one, is introduced it produces the negative relationship between rate of change of wages and frictional unemployment. The graph is an ellipse that traces counter clockwise loops over time. This is the same orientation as found by Phillips. Next, as in model one, the differences between frictional unemployment and unemployment during periods of negative excess demand are introduced, and the final tool of additional random errors added to break the smoothness of the curve. The model then adopts the shape found by Phillips.

Again we witness the three issues at play. Kuska's paper linked the observed data to existing theoretical positions in a manner intuitive to the author. This was achieved through making a system of assumptions that are followed to their logical conclusion and provide a Phillips/Lipsey Phillips Curve and its accompanying loops. As in the previous two, assumptions are made as to the nature of the wage bargaining process, and of the manner in which the general economy in which it exists operates. Kuska describes the background to his paper:

> Edward Kuska: "[The Phillips/Lipsey Phillips Curve] was a very popular issue at the time, Phillips was still here [at the LSE], he was my supervisor when I was a graduate student, and Lipsey had written a paper on the Phillips Curve that was quite well received, and there was an awful lot of empirical work being done on it here and in the States and, some of the empirical work ... had difficulties with the loops around the basic curve that you would get from standard theory"

Providing a very clear account of the construction of an economic theory, and the role of intuition in it, he continues:

> Edward Kuska: "So I thought about it and it seemed to me that most people['s] expectations are highly influenced by what's happened in the past, and you expect things will go back, if they deviate they will go back to what they were before, it's not always the case, but that is the assumption I worked on, and if you put that into the model there's what you might call regressive expectations, that when the wage rate was above the sort of normal wages rates that would give you zero unemployment or effectively full employment; if unemployment was below, if the wage rate was too high then it would go back down to that level and if it was below that then it would go back up to it so it's a sense of stability of expectations, and once you put that in then you develop the loops and they go in the right direction because that sort of gliding effect of expectations changes the current relationship between wage rates and unemployment, and it seemed to me to be a better explanation than the explanations that I read in the literature so I sent it off"
>
> Interviewer: "In the paper you develop it very much as a mathematical model, but it was the concept of it that occurred to you first, and you placed the mathematics around it?"
>
> Edward Kuska: "Yeah"

Exploring the process of creating theory initially in an abstract form, but later in direct comment on his own paper, he states:

> Edward Kuska: "It's like kids learning how to deal with the world, you keep asking why why why, and you make hypotheses and the kid has to deal with the real world and [there's] something he can't understand, he makes a hypothesis, tries it, you know if he thinks he can walk on water he tries it, he sinks, so he knows that hypothesis doesn't work and he tries something else, and doing research is exactly the same, it's usually formalised a bit more and if like in this case I would have first of all, I can't even remember how I got it, but I presumably thought that there must be some sort of inertia there in people's expectations, [they] were backward looking in some sense, and then you try it and it's like walking on water, if you sink, and you usually start [with] the simplest notion and then if that works then you go on and see if you can complicate it a bit to see if that works and then you continue to complicate it until usually you run out of mathematical technique, either yourself or you make it complicated enough [that there] just isn't the mathematics to do it"

And a few moments later:

> Interviewer: "So does that imply that you came up with the other versions that didn't work?"
>
> Edward Kuska: "I honestly can't remember, but I mean economists aren't any more honest than anybody else, if you're trying to get a paper published and you have something that you say works in one case and you try it in a slightly different case and it doesn't work, and you try the different case and it does work, the temptation is to not mention the one that doesn't, so if you have a progression of complications that do work that's what gets published … I can't remember, it's too long ago, but I'm sure I'm sinful enough that if one of them didn't come out I probably would have put it aside"
>
> Interviewer: "Well I'd have thought that that would have been the stuff of economics, that's what it is about, trying things, experimenting, seeing if they work"
>
> Edward Kuska: "Well that's true … the way of doing theory must be the same pure across the sciences, the difference in economics is that it's so difficult to prove anything that you can go on doing abstract theory and pretty much believe in what, you know, if you've got prejudices you get results out that fit with your prejudices"

Kuska confirms that macroeconomic theorising is a very creative process. It is about developing a story that appears intuitively sound and accounts for observed phenomenon in a way that is coherent with already accepted theoretical positions. In Phillips' case it was a simple demand and supply model, in Lipsey's it was the aggregation of individual markets with differing levels of unemployment, and in Kuska's it was the introduction of an expectations based lag in a normal market system that produced the loops. Furthermore the Kuska quotes demonstrate the process of trial and error evident in the handling of data discussed in the previous chapter. Finally he alludes to the flexibility with which theoretical resources can be employed to produce a desired outcome.

The theoretical development in these papers has fallen into a clear pattern. Now it is time to complicate the pattern with the introduction of the second cluster of papers, no longer concerned with the loops, but instead with the shape of the Phillips Curve itself.

6.2.5 Cluster Two: The Shape of the Phillips Curve: Edmund Phelps

Phelps' paper uses theory differently from those discussed so far. Phillips, Lipsey and Kuska built a theoretical model that predicted a pattern resembling an observed empirical relationship. Phelps argued the empirical observation must be wrong, as there is a set of theoretical tools not used by Phillips/Lipsey Phillips Curve advocates that predict a different outcome. The central tenet to these theoretical tools is the role of peoples' expectations in the labour market.

Phelps worked on the theory presented in his papers of this time during a sabbatical year at the London School of Economics. Referring to this, we can see clear evidence that his theoretical work was goal driven in this quote:

> Edmund Phelps: "I knew already then that I went there hoping and planning and expecting to develop some sort of a model of the connection between unemployment and inflation that would involve expectations"

The insight to be gained from the Phelps case is the continual work and reconfiguration a theoretical construction requires. Similar to the data handling discussed in the previous chapter, should the conceptual work not produce a theoretical representation of what was sought to be explained, the macroeconomic theorist reconfigures the theoretical tools available to them until it does. Commenting directly on this, Phelps explains:

> Edmund Phelps: "I even made a joke about it that sort of became famous, I quoted a joke about an audience in an Italian opera house applauding the Italian tenor in such a strenuous way that he repeated his aria, and then they applauded and applauded and he repeated his aria again and so on, and then he said 'I can't, I can't sing any more I won't be able to finish the opera' and somebody in the audience [shouts] 'you'll keep on repeating that aria until you get it right', ha ha, so I was unconsciously saying, I'm still thinking about this and I probably will have to rewrite this paper many times before I get it completely down"

Phelps comments on the experience of developing theory in this way, while discussing why he worked on the papers alone:

> Edmund Phelps: "There was just so much that I didn't understand, so many confusions to be dispelled, I ask[ed] myself if I could find anybody to [work with], when I endured this torture of eight hours a day of just staring at the blackboard with a pad day after day after day, who'd want to do that? I had to be almost crazy to do this, so I ended up pretty much working by myself, and it [was] eight or nine months later that I gave the first presentation of it for some colleagues at the University of Pennsylvania"

Phelps, then, was determined to establish the problems in the Phillips Curve literature as it stood, and invested a large amount of time in reconfiguring the theory until it predicted as expected. So let us explore exactly some of his theoretical work, to explore this mechanism in practice.

Phelps first published his ideas most fully in his 1968 paper 'Money-Wage Dynamics and Labor-Market Equilibrium' (Phelps 1968). This paper is both long and detailed, and a full exposition of its technicalities is beyond the level of exploration necessary to establish the sociologically interesting points. What follows is far from a complete account of the paper. In fact, the paper is lengthy to the extent that the detailed analysis here will not get as far as his explanation of why the Phillips Curve is vertical. Instead we will focus on the first section developing his initial construction of the short run Phillips Curve, which in the Friedman/Phelps model retains the shape identified by Phillips.

Phelps explores the wage negotiation process on an individual firm level. He assumes considerable variety in the nature of the jobs available. Furthermore he also assumes imperfect information, which means workers and firms do not have complete knowledge of all the available jobs and their wage rates, and all the unemployed workers and the wage they are willing to work for. Subsequently firms and workers experience search costs when dealing with the labour market. This means that even when there are unemployed people looking for work, and available vacancies for them to fill, unemployment can persist. The level of unemployment and the number of vacancies become Phelps' main explanatory theoretical variables. Each of these assumptions is a theoretical tool that he uses to make his model.

For another tool Phelps develops a mechanism termed the 'turnover rate'. This shows how lower levels of unemployment cause higher levels of inflation. If a firm pays lower wages than other firms in the industry, a fall in unemployment will cause an increase in the amount of their workers quitting their existing jobs, as workers look elsewhere for better paid jobs with less competition from unemployed people for those positions. This leads to the theoretical tool of the 'quit rate'. A higher quit rate imposes higher costs on a company, as output must decrease with a subsequent loss of revenue, and there are recruitment and training costs of replacing workers. Further speculation upon this suggests to Phelps that if the quit rate is high enough firms will want to increase the difference between the wage it pays and the average wage paid elsewhere, as the savings from lower turnover costs will more than pay for the extra wage bill. For a final tool Phelps aggregates this process to produce a general rise in wages.

Phelps explores how the rate of vacancies also has an influence on wage rises. His first argument is that more vacancies in an economy lead to a higher quit rate as the quitting workers expect a smaller period of unemployment. His second is that if a firm has vacancies it will raise its wage, or increase the difference between the wage it pays and the wage paid elsewhere. The size of this wage increase depends on the number of vacancies in the firm, the size of the unemployment pool, the number of workers employed elsewhere, and the size of the labour force.

For Phelps the conclusion of all this is that the general wage rate will increase if all firms in an economy want to pay higher wages than the other firms competing for the same labour. Firms will want to do this when unemployment is low and vacancies are high. It is assumed that these wage changes happen slowly due to administrative and psychological costs of changing wage rates. These renegotiations occur periodically, but if they are staggered across time by firm then the changes on the general level will be smooth. These rationalisations imply the two major facets of the Phillips/Lipsey Phillips Curve. Firstly the curve is negatively sloped, because decreased unemployment puts pressure on the lowest paying firms to increase their wages, compounded by the increase in vacancies. Secondly the curve is convex. This is because as the unemployment rate is decreased the vacancy rate must increase at an increasing rate to keep the curve linear. Of course, Phelps continues to develop his model in section two of the paper by introducing adaptive expectations that make the long run vertical Friedman/Phelps Phillips Curve. However, as noted above, to continue an account of this at this level of detail would require much space at a low dividend in new sociological insight.

The description of section one alone is a clear demonstration of how macroeconomic theorists construct intuitively plausible notions of how economic activity occurs, and rationalise it into a set of mechanisms that produce an outcome in line with their expectations. These specific mechanisms described represent choices made from a wide selection of possible theoretical tools, and there are further choices to be made in the extent and manner these tools are employed. Phelps comments that the endless reconfiguration of the task necessary led the theory of the paper to be done in a:

> Edmund Phelps: "Tremendously sloppy and primitive way, in part because when you are playing a sort of, pivotal role in a paradigm shift, there are so many influences operating on you simultaneously and even though you know where you want to go you sometimes don't succeed in shaking off all those influences that you'd really be better off casting aside"
>
> Interviewer: "Could you list some of these?"
>
> Edmund Phelps: "Well for example a fairly disastrous mistake in tactics in the 1968 paper was I imagined that each firm would be very uncertain about the situation that it found itself, so it would only move one little step at a time, so instead of jumping its wage rate in response to a demand jump it would just start moving its wage rate up slowly little step by step, well that was a recipe for ah, that caused me to overlook certain things that I wouldn't overlook if I faced the possibility of a jump, so the paper was at points probably lacking in transparency, [it] didn't have as much clarity as would have been desirable, and the other thing was that I had chosen a mechanism underlying the wage setting that was already fairly sophisticated, and its ideal analysis would have required using the mathematical tools of Pontryagin, which I didn't know at that point, so I only used the tools of dynamic programming"

This demonstrates explicitly the process of continual reconfiguration and rebuilding of the model to ensure it makes the expected outcome. Phelps made these decisions precisely because they conform to his expectations, just as Eatwell, Llewellyn and Tarling, Thomas, Copeland and all the other macroeconomists discussed in the pervious chapter made decisions regarding how best configure their statistical manipulations based on their conformation with their expectations. For the final empirical demonstration of this process, we now discuss a response to Phelps' work by James Tobin.

6.2.6 Cluster Two: The Shape of the Phillips Curve: James Tobin

In the light of the emerging Friedman/Phelps Phillips Curve literature, James Tobin took the opportunity of his 1971 Presidential Address to the American Economic

Association to discuss the work and develop a theoretical argument of his own that protected the established Phillips/Lipsey Phillips Curve model (Tobin 1971). He claimed that as late as 1971, the Phillips/Lipsey Phillips Curve was still an empirical finding in search of a theory. To address this, Tobin describes a theory acknowledged as similar to Lipsey's industry disaggregation theory discussed earlier in this chapter, with the modification of a wage floor.

Like Lipsey, Tobin claims that economy wide relations between employment, wages and prices are aggregations of the experience of many markets all of differing form. However, the work moves beyond that of Lipsey's by introducing two new theoretical tools with the idea of an equilibrium and disequilibrium component in the setting of the wage level in each individual industry. We need not concern ourselves too deeply with this, suffice to say the disequilibrium component is considered a short run phenomenon that occurs due to economic shocks, for example new tastes in consumer behaviour, new technologies of production, and issues of the mobility of labour. When the economy is in this state of shock the Phillips Curve will be as the Phillips/Lipsey Phillips Curve inclines us to believe.

The equilibrium component refers to the long run position where the number of unemployed equals the number of vacancies, excluding frictional unemployment, i.e. those moving between jobs. Tobin concedes that in this situation wage setting occurs as Phelps claims it does, with employers raising their wage to a competitive level with similar firms when they are experiencing vacancies, and that as expectations become realistic a vertical Friedman/Phelps Phillips Curve will exist.

However, Tobin introduces what he claims to be a rather minor modification to the theory as discussed to his point that can preserve the Phillips/Lipsey Phillips Curve trade-off in the long run. This modification is a floor on wage change in industries experiencing unemployment, meaning a wage level that the agreed wages cannot fall below. Tobin suggests that this is possible because wage contracts are often negotiated on an annual or greater time span, and as such wages cannot go below that level. Given how Phelps explains that firms look to give a competitive wage in relation to other firms, this wage can act as an economy wide floor. The model predicts a long run Philips Curve that is very flat for high unemployment and becomes very vertical at a critically low rate of unemployment.

Here is Tobin creatively using the available theoretical tool of a floor in pricing to build an extra component onto Phelps' existing model that makes the relationship between unemployment and inflation once more look like that claimed by Phillips and Lipsey. It is couched in intuitive terminology regarding the length of contract setting. If we were to use the form of reasoning developed in this chapter to ask why Tobin used a floor wage when Phelps did not, then we would say that Tobin employed this particular theoretical resource because it made a Phillips/Lipsey form relationship and Phelps did not for the same reason.

Although perhaps such concluding analysis is premature, as Tobin continues to acknowledge a criticism of his theory. He notes that for a permanent floor to exist in a single market would imply people confuse money vales for real values, a phenomenon demonstrated as only short term by Friedman and Phelps. Without this money illusion the model still makes a Friedman/Phelps style relationship. However, Tobin has spent longer rummaging through his theoretical toolbox, and has found another trick that prevents this. He argues that the floor need not be permanent in any single market, it could give way to wage reduction when enough unemployment has persisted long enough. But with intersectoral shifts of demand, markets are always changing roles, and subsequently another market can adopt the wage holding role for a short time. With this creation, Tobin has once more constructed a model that makes a Phillips/Lipsey Phillips Curve exist, if not in the absolute long run, for a very considerable amount of time, as the economy is experiencing continual sectoral shifts. On this Tobin remarks:

> Interviewer: "Are you saying that by [introducing a wage floor] theoretically you maintain a long run Phillips Curve?"
>
> James Tobin: "Yeah you could say it would maintain a long-run Phillips Curve, that's true, ... because with time in each market the wage will go down, it's true it takes time, but when market A gets back into equilibrium then market B is still out, a new market is out of equilibrium and [it] will take time ... maybe forever for the whole economy to be in equilibrium at once"

And later:

> Interviewer: "You are saying that that actually is something that happens in the economy?"
>
> James Tobin: "Yeah that happens in the economy ... if everything quietens down then everything will be the way the monetarists say, but I did some simulations of an economy like that in which there was a long run Phillips Curve because of that effect and it would depend on how fast you make the adjustments take place"

Many agree that the shape of the Phillips Curve does depend on how fast the adjustments take place. By introducing a wage floor, Tobin made the adjustments happen very slowly, sufficiently slowly to maintain a trade-off possibly forever. By introducing adaptive expectations, Phelps made the adjustments happen more quickly, subsequently making the relationship a trade-off in the short run and vertical in the long run. And equally, by introducing rational expectations, rational expectations theorists made the adjustments happen instantaneously, making the relationship a vertical one in all instances. The sociological insight, of course, is that all of these relationships were 'made', built from an array of existing theoretical tools combined in a certain form that produces the expected result.

After this thorough empirical demonstration of how macroeconomists construct theories, we return to some theorising of our own.

6.3.1 How Data Handling and Theory Construction Differ in Macroeconomics

Both the empirical and theoretical Interpretative Flexibility have commonalties. Both are goal orientated, and both are subject to continual modification to attain a best fit explanation for the phenomenon in question. However there is a distinct difference. In principle empirical work is bound by the standardised methodology of regression.

This is presented as a tool to ensure accuracy and scientific rigour. Despite this intention, we demonstrated in the previous chapter that in practice it does not limit the scope for Interpretative Flexibility. Yet it does impact upon the manner in which the flexibility is actualised. As we observed, the standardisation produces six mechanisms allowing Interpretative Flexibility, the macro data-set choice, transformation, development of proxies, special effects, micro data-set choices, and interpretation of the data. These categories are used to limit the wide possibility of potential interpretations to one singular interpretation. This was characterised as a process of whittling away alternative readings of the data. The lack of this standardisation in theoretical work means such clearly identifiable locations are not available. This is the first reason why the whittling metaphor is not appropriate here.

Instead, theoretical development is bound only by the creativity with which an actor can employ and combine any of the wide range of available theoretical concepts, or create their own. This leads us to a second difference between the Interpretative Flexibility of data and theory, and the second reason why this characterisation is inappropriate. As we have shown the creativity of theoretical development has little in common with whittling away possible accounts. Instead we have developed a characterisation of building a conceptual machine, where the existing cogs and pumps of demand and supply or wage stickyness can be built and combined in such a way that produce the patterns apparent in the data.

This is not to argue for creative impotency in macroeconomic empirical research. Here also inventive solutions to the whittling process are sought. However it is a reductive task of removing possible interpretations, not creative in the spirit of building bridges or theoretical chains between existing theoretical knowledge and new empirical observations. It is the reductive/constructive nature of empirical and theoretical macroeconomic solutions to Interpretative Flexibility that distinguish one from another.

6.3.2 Intuition in Making Macroeconomic Theory

The argument developed so far in this chapter has been that macroeconomic theorists pick selectively from an array of available theoretical tools to make what is to them an intuitively acceptable account that produces an expected pattern of results. The range of available tools is large, and more can be created or borrowed from other disciplines (Mirowski 1989a, 1994). There is no tightly limited pool of tools to chose from that may limit the flexibility allowed for the theorist. However, to this point it may appear that the notion of intuitiveness may introduce a restrictive component to how macroeconomists make theory. Let us take this opportunity to dispel this perception. Intuition is in itself a flexible phenomenon that can be configured in many ways. On this, Robert Solow:

> Robert Solow: "It seems to me that ... the normal evolution is: here is a counter intuitive result in economics; so you check up on it and it turns out, yeah, that's right, things do work that way in spite of what you previously thought. Then you try to understand how did earlier thinking go wrong? what was omitted? what was mis-specified? what was incorrect? and after a while the counter intuitive result becomes intuitive, for the reason that you now understand the mechanism"

Solow provides a particularly rigorous sounding account involving checks and balances regarding shifts in intuition. However, there are no reasons to assume changes in what appears intuitive cannot be more fluid than this. There is no argument for why intuition introduces a rigidity in macroeconomists' use of theory. Intuition may be required, but its form is malleable and contingent.

Furthermore the location of the intuitiveness is available for contest. An example would be the role of rational expectations on the Phillips Curve. Rational expectations proponents find the notion that economic agents optimise the available evidence when making financial decisions, and subsequently have rational expectations, lends the theory intuitive support. However, regardless of this, an opponent of the Rational Expectations Phillips Curve may find the implication of a short run vertical relationship between unemployment and inflation as wholly counterintuitive. Intuition has a role for both analysts, but the differing locations of the intuitive/counterintuitive notion allows flexibility in interpretation. This serves as further evidence to the contingent and malleable form taken by intuition in macroeconomic theorising.

6.3.3 Discussion of Economists use of Theoretical Models in the History of Economic Thought Literature

As with the previous chapter, there are parallels to be made between the argument presented here and those present in the History of Economic Thought literature. Again frequently the conclusions drawn are different, and this represents the differing analytical focus. However some accounts do provide supportive evidence for the mechanisms discussed here.

One contribution that quite noticeably resonates with the arguments presented here is that of Nancy Cartwright (1999). Cartwright is keen to disband an existing view that science and nature assumes an ordered, hierarchical, and elegant structure. Instead she purports the existence of the 'Dappled World', a messy, disproportioned jumble. The boundaries between scientific disciplines are equally messy, with overlaps and gaps, and ragged edges. This, however, is often ignored, instead preferences tend towards anticipating ordered sciences representing the ordered laws of an ordered nature.

Cartwright wants to make the world better, and argues a proper understanding of science, as opposed to the idealised notion of a unity of knowledge and ordered knowing, will contribute to this goal. As an example of the benefits of the Dappled World account she takes the example of breast cancer treatment. She notes that in the vast majority of cases endogenous oestrogen levels are the dominant determining factor in the occurrence of the disease. Furthermore, it is well known that lifestyle patterns have a strong impact upon endogenous oestrogen levels in the body. Yet both emphasis and funding are overwhelmingly placed upon genetic solutions to breast cancer at the expense of research into controlling lifestyle choices. Cartwright argues this is a product of the preference for an ordered aesthetic for science. Genetics promises the overarching laws of the hierarchical science. This contrasts to the specific and context dependent knowledge of lifestyle issues. However, Cartwright supposes, while genetics is privileged women continue to die.

The congruencies between Cartwright's arguments and those of this chapter become obvious when she discusses where the belief in laws comes from. She insists that such regularities do not come from nature but instead from the 'nomological machines' employed by the sciences. The metaphor of a machine for exploring the construction of knowledge was clear in this chapter. To Cartwright a nomological machine is a fixed arrangement of concepts and factors with predisposed capacities that in a suitable environment provide, with repeated operation, the regularities recognised as laws. However these machines rely entirely on *ceteris paribus* assumptions. Should any external influence enter the system the regularity will be lost. Cartwright takes examples from physics and economics to illustrate her arguments, starting with the example of 'Kepler's problem' which asks why Mars takes an elliptical orbit with the sun at its focus. Within the constraints of a nomological machine Newton was able to solve the problem with reference to the specific type of interaction with the gravitational pull.

Before discussing examples from economics Cartwright notes that economists tend to talk of models instead of laws and theories, an observation also made in Morgan (2002). Cartwright suggests that this could reflect the fact that economists are not intending to produce over-arching laws, but instead explore regularities in reduced special cases. As an example from economics Cartwright identifies the Game-theoretic model of Hart and Moore (1991) as a good blue print of the nomological machine. The model provides three aspects of the information necessary to make a nomological machine. Firstly it lists each part that forms the machine, including their properties. Secondly their model describes how these parts are to be assembled. Thirdly the rule for calculating what should result from their joint operation once assembled is made explicit. The model is less successful at describing the remaining two important aspects of a nomological machine, firstly, what counts as shielding, by which Cartwright means the mechanisms that prevent external inputs impacting upon the machine, and secondly, how the machine is set running.

Regardless, Cartwright postulates that the *ceteris paribus* conditions describe the structure of the machine that allows regularities to be found. If the conditions are not met, or an external force acts upon it, the regularities will not be found. Central to Cartwright's argument, and that which resonates so well here, is the amount of fine-tuning of the nomological machines necessary to produce these regularities. The fine-tuning identified by Cartwright is directly analogous in the economics context to the analysis presented in this chapter. For a further consideration of *ceteris paribus* conditions in four modes of economic analysis; mathematical modelling, econometrics, simulations and experiments; see Boumans and Morgan (2001).

Cartwright provides two further examples from economics, Anand and Kanbur's (1995) research into the relative success of Sri Lankan health and education programmes and Christopher Pissarides' (1992) study of the circularity on deskilling through long term unemployment causing further unemployment. Cartwright shows that Pissarides requires sixteen assumptions for his model to predict as required. Without constructing his model in this particular way the desired result would not be achieved.

Another exploration of how economic models operate is provided by Mary Morgan (2001). Morgan argues that models help us understand the world by telling stories about it, whether it be the real world or a hypothetical one. An economic argument is structured by the model in which it is presented, but its application is achieved through a narrative sparked by a question, an external fact, or an imagined event. Morgan acknowledges the contributions of Gibbard and Varian (1978) and Deirdre McCloskey (1990b), both of whom start to unpack the role of stories in the use of models. However she is not fully satisfied with either of the accounts and argues that the economic models should not be characterised as either a metaphor or a structure since neither can describe how models function. Models require both structures and stories. Morgan argues models need to be 'questioned', to make use of their 'internal dynamic'. Answering the question using the 'deductive resources' of the model usually means telling stories. The story is started by a question or a change in any aspect of the model.

The concept of a story is a suitable mode of analysis for the use of economic models in this context for three reasons. Firstly the operation of a model follows the sequence of narrative, it starts with a question which sets in motion a number of reactions that eventually arrive at a final outcome. Secondly causal connections run through the interpretation of the models application. Thirdly the interpretation of the workings of the model is usually articulated by linkages to dynamics in the real world.

Morgan's first demonstration of the role of stories in making use of models is with a simple demand and supply model. She follows this with an exposition of her argument through a paper by Meade (1937) that tries to provide a model for Keynes' arguments about equilibrium and the impact upon employment of changes in variables. Morgan illustrates how Meade uses narrative sequences of events that are connected in his model to explore the impact of changes in individual variables e.g. interest rates, the money supply, money wage rates, and the proportion of income saved. Meade uses an eight equation structure in his model which is inevitably explored through a story narrative. His eight equations are derived from seven assumptions based upon his interpretation of Keynes' work and the policy issues of the day. Morgan's point is that we cannot understand the model by the mathematical structure – the seven assumptions and eight equations – alone. We also require the stories that give meaning to the model through its application.

She follows this by looking at the role of story telling in two further models, Samuelson (1939) and Hicks' (1937) paper that introduced the IS/LM model. She notes that Samuelson used simulation methods and analytical solutions models which contrasts with Meade's comparative static method. However in both cases story telling was essential in connecting the abstract structure to the real world.

A further exploration of economic models is provided by Robert Sugden (2000), himself a theoretical economist. To Sugden models are not abstractions from, or simplifications of, the real world. Instead they describe counterfactual worlds constructed by the modeller. He argues that the gap between the two can only be filled by inductive inference, and we can have more confidence in such inductions the more credible the model is as an account of what could have been true.

His empirical exploration of a model considers a paper by George Akerlof (1970), the paper known for introducing asymmetric information to economics. Sugden describes how Akerlof's paper quickly claims to speak about a range of issues, including the labour market, business in underdeveloped countries, and insurability amongst others. However as the paper progresses other utterances are more coy on the extent to which these topics will actually be discussed in detail. To Sugden this vagueness is a product of the inevitable distance between the counterfactual world and the real one. Like many theoretical papers, Akerlof seeks to comment on the real world. However, there is an unspoken acknowledgement that the model cannot fully justify this leap of faith from model to the real world. It is this leap of face that Morgan (2001) takes through the use of stories as linkages between abstract models and the real world.

Sugden shows that Akerlof chooses to explore the market for cars, not because of its realism, but, because of its ease. Sugden highlights some of the literary devices to shift between accounts of the real world and that represented in his model. Akerlof acknowledges that he uses unrealistic assumption on the car market, but justifies them as simplifications which allow him to focus on the real market. For example he avoids using an assumption of risk aversion to prevent complex algebra confusing the central point. Sugden notes that in his construction of the model Akerlof provides no evidence of the form or structure of the real markets he is modelling.

Sugden, of course, is primarily interested in the methodological implications of his discussion. To this end he argues the gap between the counterfactual worlds and the real world can be bridged by inductive inference. However in our context the paper is more interesting as a further demonstration of the inherent configuration and reconfiguration of macroeconomist theoretical tools.

6.3.4 Understanding how Macroeconomists Construct Theories

The construction of macroeconomic theory operates differently to the use of macroeconomic data. However, both exhibit a dynamic that allows the macroeconomist to attain results that conform to their initial expectations. Indeed, the process of doing macroeconomic work is configuring the available theoretical and empirical resources in such a manner that best conforms to their expectations. On this, Mike Sumner:

> Mike Sumner: "Economics is essentially about abstracting from unnecessary complications, and a priori what are essential features and what are unnecessary complications is not something that's easy to decide in many cases"

This section has shown that frequently the decision as to what is necessary and what is not is led by whether or not the complication adds or detracts from the explanatory power of the theoretical construction.

We have developed a characterisation of building a theoretical machine. By choosing from the available theoretical tools the macroeconomist can configure their theory in such a way that it makes patterns fitting with their expectations. The models are mediated by, but not restricted by, a sense of intuitive validity. Should a theoretical construction not produce the accepted patterns, the macroeconomist can introduce further tools until it does.

Perhaps almost too incestuously, the best real world example of this metaphor is arguably a machine made by Phillips himself. Drawing upon his experience as an engineer, in 1949 Phillips designed and built a hydraulic model of the macro economy, with flows of liquid representing flows of money and other economic variables (Phillips 1950). By using various configurations of pipes, tanks and valves, Phillips made the machine to conform to the expectations of macroeconomic theory. Just as with making macroeconomic theory itself, Phillips would have worked hard to combine the available tools of engineering in a suitable manner to produce the patterns of behaviour expected of it.

As is the case with the Interpretative Flexibility of macroeconomic data, the processes discussed here introduce lubrication to the cycle of contest and closure. This is because theoretical resources cannot settle a debate alone. Instead theory can be reconfigured in a variety of forms. Subsequently should, due to an external pressure, a macroeconomist choose to change their position in the Phillips Curve debate they would not have been restrained by theory. They could adopt a different theoretical construction and still retain a sense of coherence and correctness. The Interpretative Flexibility of theory provides this, and removal of this tension can only aid any explanation of why the cycle of contest and closure occurs at such a high frequency. This conclusion does not feature in analysis of economists use of theory in the History and Methodology of Economic Thought literature.

6.3.5 How the Construction of Macroeconomic Theory Relates to the Rest of the Thesis

This and the previous chapter have illustrated how macroeconomists develop their own empirical and theoretical work in accordance with their prior expectations. Both chapters have contributed to the demonstration of Interpretative Flexibility in the Phillips Curve debate. As discussed in Chapter Two, this is an essential stage of EPOR and any case study in SSK. It is essential because the remaining two stages of EPOR explore how this Interpretative Flexibility is overcome in a socio-political context. The next chapter, Chapter Seven, continues the documentation of Interpretative Flexibility by extending the analysis to how macroeconomists evaluate the empirical and theoretical work of others, given the process illuminated in this and the previous chapter.

Both this and the previous chapter have spoken frequently of macroeconomists orientating their work towards a desired outcome, but again have remained silent on the origin of this expectation. This is the topic of the remaining three chapters, and it is here where the discussion turns to the role of political interest and the second and third stages of EPOR.

7. How Macroeconomists Evaluate Theory and Data

7.1.1 Introduction

The previous two chapters have explored how macroeconomists conduct their own empirical and theoretical work. This chapter concentrates on how macroeconomists judge the empirical and theoretical contributions of others. This chapter is the third and final chapter that demonstrates Interpretative Flexibility in the Phillips Curve debate. As with these previous two chapters, by doing so it fulfils an essential role in the Empirical Programme of Relativism (EPOR) that is necessary in any research in the Sociology of Scientific Knowledge. Firstly we shall discuss how data are assessed, then theoretical work, and finally how both are brought together. All three cases will clearly demonstrate the form assumed by Interpretative Flexibility in macroeconomics.

7.2.1 How Macroeconomists Evaluate Empirical Work: Finding Support

This section argues that the existence of empirical support for a macroeconomic idea depends upon where the analyst looks. Equally the existence of an empirical refutation of an argument also depends on where the analyst looks. In effect macroeconomists can choose to prioritise one data source type over another. Indeed, the interview data demonstrates that macroeconomists often cannot even agree whether supportive data exists or not. Let us begin with comments on the Phillips/Lipsey Phillips Curve, from Edmund Phelps and George Perry:

> George Perry: "The [Phillips/Lipsey] Phillips Curve was a very strong empirical regularity, ... there's a very striking level of empirical regularity in the cyclical data"

> Edmund Phelps: "When I saw the data I immediately accepted it, and wished that I had done something like that, ha ha"

However, Milton Friedman argues that this strong empirical regularity does not imply the existence of a Phillips/Lipsey Phillips Curve. On this:

> Milton Friedman: "The [Phillips/Lipsey] Phillips Curve developed because of the accident that Britain for a long time had a pretty stable monetary policy, so that long term expectations were stable, and as a result what is essentially a short term relationship looked like a long term relationship, but it didn't develop out of any political context without that empirical setting, ... the price level in Britain in 1939 was roughly as it was in 1739, and if that hadn't been the background that data wouldn't have been there that Phillips could have summarised in his curve"

Friedman has explained away the Phillips/Lipsey Phillips Curve interpretation by reference to mitigating circumstances in the British context. He did, however, at least accept that there were data to support his adversary's claims, only that it was wrongly interpreted. This acceptance is not always present, on the Friedman/Phelps Phillips Curve, Richard Lipsey:

> Richard Lipsey: "I've always felt that was just an assumption lacking in any empirical verification"

Willing to provide such an empirical verification is Milton Friedman:

> Milton Friedman: "The empirical evidence, you've got to distinguish between before and after, as of today the empirical evidence for the [Friedman/Phelps Phillips Curve] ... is the success of its predictions, as of today the best empirical evidence is probably the Nineteen Seventies as the most extreme empirical evidence, as of the time the [Friedman/Phelps Phillips Curve] was put forward the empirical evidence for it was of course much weaker. In general what you do [is] you conjure up a hypothesis that seems to fit the facts as you know them, but you can't have too much confidence it in until it is subjected to tests, until you can make predictions from it, to see if those predictions are confirmed or contradicted. In the case of the [Friedman/Phelps Phillips Curve], I believe the evidence I would have cited at the time ... would have been the historic experience of the past, and the difference between what happened in the US between 1879 to 1896 when prices were declining on the average and from 1896 to 1913 when prices were rising on the average and when the rate of economic growth was roughly the same in both periods, which would be inconsistent with a strict [Phillips/Lipsey] Phillips Curve"

By looking to a certain historical period Friedman is able to find empirical support for his ideas. He also notes that the experience of the 1970s provides the best support for the Friedman/Phelps Phillips Curve. He contextualises this as retrospective evidence, and in terms of his development of the theory it is. However in terms of the period of its wider acceptance, the early 1970s, this evidence would be available to macroeconomists. Yet by looking elsewhere, contradictory evidence can be found, Franco Modigliani:

> Franco Modigliani: "Friedman's arguments are defeated by what happened in Europe, now I don't know whether the money supply rose regularly or not, but it certainly is a fact that unemployment grew and grew and grew, it's only been declining very recently"

And later:

> Franco Modigliani: "Some, such as the European Central Bank, use the vertical Phillips Curve to justify the mass unemployment they are fostering in Europe; they seem to hold that the critical unemployment rate is even now at least 9%.

> That if you had 6% unemployment there will be a lot of inflation, which is stupid, as long as unemployment is above 3 or 4 in Europe you will not get a lot of inflation, you have had times in which you did, but that is due to the oil crisis, these people are still unable to distinguish between unemployment that's due to too much demand and unemployment that's due to cost push"

Modigliani, like Friedman commenting on the Phillips' data, has found a mitigating circumstance, here the OPEC price rises of the 1970s, to identify Friedman's data as a special case, and subsequently disregards it.

Finally we have similar quotes concerning the Rational Expectations Phillips Curve. In a conversation with David Peel:

> Interviewer: "Did you feel that there was empirical support for using the rational expectations scheme?"
>
> David Peel: "Yes yes, I feel the sort of empirical work especially coming out of the States was supportive of it"
>
> Interviewer: "In relation to what it says about the Phillips Curve as a vertical phenomenon?"
>
> David Peel: "Well yes, I mean there were papers there, McCallum 76 comes to mind, *Econometrica*, but their work seemed to be able to support, the empirical work, the outcomes of the empirical work seemed consistent with the hypothesis that expectations could be modelled in an unbiased and at least weakly efficient way, so I found that attractive yeah"

Further to this, Dale Mortensen felt the data for the Rational Expectations Phillips Curve better fit the data for the 1970s than the Friedman/Phelps Phillips Curve:

> Dale Mortensen: "Many people also viewed the Seventies as a time when [the Rational Expectations Phillips Curve] ideas were verified in the data, alright maybe afterwards there's been a re-evaluation"
>
> Interviewer: "What data?"

> Dale Mortensen: "Oh the period of rising unemployment and inflation, how could you explain that, the old Phillips Curve ideas could not"
>
> Interviewer: "Could it be explained by the Phelps type model?"
>
> Dale Mortensen: "A Phelps type model, well, but it could more easily be explained by the fact that people realised the inflation rate was going up instead of, real rates were going up so that simply forward looking agents knew what was going on more than the models did"

Conversely, on the empirical validity of the Rational Expectations Phillips Curve, Phelps and Laidler:

> Edmund Phelps: "I wasn't persuaded about rational expectations, it seemed to be a bit over board, going way too far"
>
> Interviewer: "What, theoretically?"
>
> Edmund Phelps: "Theoretical, no. Empirically, empirically just crazy"

And Laidler:

> Interviewer: "The idea of Rational Expectations augmented Phillips Curve, did you have a lot of time for that?"
>
> David Laidler: "Nope, never"
>
> Interviewer: "Why not?"
>
> David Laidler: "Well because it seemed to me to be empirically unsupportable, the Lucas aggregate supply Phillips Curve has got one really firm prediction, which is 'quantities move in response to prices', and if quantities move in response to prices the data should have prices moving in front of, or at least at the same time, as quantities, and the data don't show that, the data show that quantities move first, so that seemed to rule that out immediately"

Clearly macroeconomists feel it is legitimate to ignore or undermine the empirical work of others. This can be achieved by referring to an alternative data source, finding mitigating circumstances explaining others findings, or simply not accepting a data source is valid. Through this they maintain their sense of empirical validity in their own arguments.

7.2.2 How Macroeconomists Evaluate Empirical Work: Operating with Interpretative Flexibility

Chapter Five demonstrated the Interpretative Flexibility of macroeconomic data. There has been some implicit and some explicit recognition of its existence by interviewees themselves. In this section we review how some macroeconomists manage with this recognition, beginning with a very illuminating discussion from Richard Lipsey on how networks of trust permeate his evaluations of empirical data. When asked how he judges empirical papers whilst knowing Interpretative Flexibility allows massive changes in the results, he replied:

> Richard Lipsey: "Ha ha, well ok it essentially it's a sociological answer, without mentioning names there are some people I trust and some people I don't trust, people I don't trust I would worry that they've mined the data, done twelve different regressions and only published the one that's good, and then secondly there's genuine mistakes of people I do trust, where they can make all kinds of things that can cause you trouble, but there's two quite different levels. If I'm reading a paper by someone who I don't trust then I'm generally very very sceptical, and if it mattered to my research I'd try my best to try and duplicate it, then of course the opposite ones, the problems with those is that you can make an unconscious mistake in myriad directions and one of the big problems of this I think is that it's too difficult to duplicate experiments, and secondly if you do duplicate experiments successfully no one wants to publish it, as soon as you get a [natural] scientific experiment everyone and his uncle rushes out to duplicate it, you know, you can be sure that 100 people have tried, so anyway I think it's very difficult and you just have to make an assessment and where it really matters you've almost got to redo it yourself"
>
> Interviewer: "Ok how does somebody train, trust, to use your term, you said you trust certain groups of people, are these groups of people linked by an institution or you know or"
>
> Richard Lipsey: "No I meant individuals"
>
> Interviewer: "Individuals"

> Richard Lipsey: "Yes, I don't think I said groups did I? No I don't think there's any one school of economists that has any more unreliableness than others, everybody has a preconception to start with"
>
> Interviewer: "Ok, so how do you know when you trust somebody, do you have to work with them personally, or reputation?"
>
> Richard Lipsey: "No no, you certainly don't have to work with them personally, I guess it's partly reputation, partly what you've made, if they're established researchers, someone whose been around for along time, have they done stuff that has been suspect and there has always [been] stories about particular people from their research assistants, that they would tell them to go away and keep on making regressions until they got one that supported their hypothesis, and they didn't want to see them until they got the right regression, and once you hear that as long as it's from a reliable source it certainly makes you worry, and you know one of the big problems in the subject is, the way we go about things is we fit to data and we may try ten different things and then we take one that fits the best and then we report the t-statistics as if they were genuinely tests, well they're not genuine tests if you've already mined the data and picked only the regression you know that gives you the best results, or the ones you like, well we're full of that kind of thing so its, in other words its a subject that's very difficult to do in really objective, or something close to objective, it's getting worse and worse, much worse than the sciences, and there's even more room to personalise, and it's hard to duplicate, that's the other thing and all of this means I have to make a kind of personal judgement, there's enough people in the subject today that if somebody produces data for some outlandish theory it's almost certain that soon enough someone will come along and question it, and they'll get into debate about it, and that was much less true, there was 10% as many people in the profession back then"

In environments based upon trust, you also often discover suspicion. Commenting on the work of Bertie Hines, a macroeconomist he disagreed with, is Laurence Copeland:

> Laurence Copeland: "His big thing was he used to come up with t-ratios of nine and ten and this that and the other, and we always used to say how the bloody hell does he get these t-ratios on strikes variable you see"

A t-ratio of nine would suggest a very significant relationship indeed. To Copeland, the very strength of Hines' results provided sufficient stimuli to question them. Although it should be noted that Copeland would object to Hines' arguments with more normal positive t-ratios as well. David Laidler elucidates another area where suspicion about data can arise:

> David Laidler: "When I wrote the demand for money book one of the things I learnt is that when you read a paper you look at the numbers in the tables to see if the numbers actually square with what the authors say they show"
>
> Interviewer: "What, you mean the results of their regressions?"
>
> David Laidler: "Yeah, and just, you know, oh how can I put it, some one will say 'well this is a highly statistically significant result de da de da de da', you look at the table and its got a t-statistic of 1.8 or something like that, yeah people sometimes claim more for what the numbers show than what they really support"
>
> Interviewer: "Do you think you ever did that?"
>
> David Laidler: "Oh I wouldn't be surprised, but I hope I didn't do it consciously, but yeah yeah probably"

The evaluation of macroeconomic empirical work is performed through a set of trust relationships, permeated by mistrust and suspicion. Given a context of mistrust, or even without it, macroeconomists can legitimately ignore or devalue empirical findings. To find empirical validation for their own positions, they need only prioritise locations where it is available over those where it is not.

The following sections deal with similar themes regarding the ways in which macroeconomists evaluate theoretical work.

7.3.1 How Macroeconomists Evaluate Theoretical Work: Defending a Position

In this section we will explore how macroeconomists evaluate theoretical ideas. The previous chapter demonstrated that the construction of theoretical arguments resembles building a machine, where specific theoretical components are combined and balanced in such a way that they make patterns of a desired form. Similar mechanisms of selective use and weightings of theoretical components are visible in how theoretical papers are evaluated. The data are largely drawn from arguments made by Franco Modigliani and Robert Solow, both Noble prize winners associated with the New Keynesian Synthesis of the 1960s, against the Friedman/Phelps and Rational Expectations Phillips Curves that superseded their ideas, and responses by proponents of these curves. Let us first look at the period of the fall of the Phillips/Lipsey Phillips Curve and the emergence of the Friedman/Phelps Phillips Curve.

As we saw in the earlier discussion of data, the 1970s oil price rises can be used by supporters of the Phillips/Lipsey Phillips Curve to defend its existence in a theoretical way. On this, Franco Modigliani:

> Franco Modigliani: "You have had a lot of cost push inflation through the 70s and the 80s all connected with the oil crisis"
>
> Interviewer: "Not just inflation but stagflation?"
>
> Franco Modigliani: "Right, the issue of stagflation is that you get inflation through the cost push and then the attempt to prevent the inflationary spiral leads to stagflation, because in fact, wages being rigid, when you clamp down on the money supply you get unemployment ... I mean the Philips Curve becomes not irrelevant, but it's completely overwhelmed by a different mechanism"

Modigliani does not concede that the Phillips/Lipsey relationship ceased to exist in the 1970s, but instead was swamped by external pressures. He suggests that the attempts by the government to minimise the inflation were firstly not strong enough to succeed, and secondly, due to wage rigidity, the cause of unemployment. Given the unusually strong opposing forces of the oil price increase, and government policy intended to fight it, the Phillips/Lipsey Phillips Curve was overpowered, and the theory is protected.

Modigliani also provides us with a theoretical challenge to the Friedman/Phelps Phillips Curve:

> Franco Modigliani: "If you take the vertical [Friedman/Phelps] Phillips Curve as is, ... on the left you get accelerating inflation, on the other side you get accelerating deflation ... ok now the problem is that if you believe this then at some point inflation becomes negative, I don't believe in that, I think that inflation is never, hardly ever, is negative"
>
> Interviewer: "Why do you believe that?"
>
> Franco Modigliani: "Because wages are rigid downward, in other words the whole point is Keynesian asymmetric rigidity, which is rigid down not up, very flexible up and rigid down, and therefore you cannot believe that if I have enough unemployment, inflation will go negative"

Again Modigliani draws upon the notion of downward wage rigidity to counter the Friedman/Phelps argument. The two theoretical notions stand opposed to each other. In this instance Modigliani has more faith in the theoretical might of downward wage rigidity and subsequently rejects the Friedman/Phelps position. As we will see in a very similar interchange between Robert Solow and Milton Friedman, it is not necessary to prioritise the theories in this way:

> Robert Solow: "There were a lot of reasons why [the Friedman/Phelps Phillips Curve] never struck me as very plausible, but perhaps the most transparent one is that it suggests an experiment: suppose the natural rate of unemployment in some economy is five percent. Then what the theory says is that if the actual unemployment rate in that economy stays at something higher than five percent, maybe five and a half percent or maybe even six percent for a very long time, cash wages and prices will fall, and they will fall faster and faster as time goes on. I find that utterly unbelievable as a description of a real capitalist economy, and I've never found any proponent of the natural rate theory [Friedman/Phelps Phillips Curve] who is prepared point blank to say 'yes, that is what I believe, and I think that if that experiment was performed that would be its outcome'. Instead what they say is 'oh well of course the natural rate of unemployment is not a number to five decimal places, it's an interval and range'. Within that range, then, the natural rate theory [Friedman/Phelps Phillips Curve] doesn't hold. Now if the range is very very narrow then I would just repeat the question, do you believe that if the unemployment rate got maybe a quarter percentage point above that range that eventually we'd have galloping deflation; if not then we have to widen that range and if you keep doing it then eventually we've got a real range in which the natural rate theory [Friedman/Phelps Phillips Curve] doesn't hold. That kind of argument just led me never to be willing to believe the natural rate

> theory [Friedman/Phelps Phillips Curve]. Well I think for a long time I may have been the only person I knew who wasn't prepared to accept it"

Solow explores the theoretical implications of the Friedman/Phelps Phillips Curve and finds an implication he finds unsupportable. He is even prepared to argue that he could not find a Friedman/Phelps Phillips Curve proponent willing to argue the implication was supportable. He later admitted to putting this proposition to both Friedman and Phelps, both of whom, he claimed, admitted the natural rate was not a point, but that it was a range. However, today Friedman is less willing to concede this position:

> Interviewer: "You mentioned Bob Solow earlier, I interviewed him a short while ago and he explained to me his argument for why he never felt convinced by the natural rate [Friedman/Phelps Phillips Curve] idea, I'd like to repeat it to you and hear your thoughts on it, essentially he comes up with a theoretical idea saying that if you were to assume that the natural rate were five percent in a particular country, the natural rate theory would suggest that if the actual rate of unemployment was somewhere above this five percent level, say six percent, for a sustained period of time then wages and prices would have to deflate progressively faster as time goes on"
>
> Milton Friedman: "That's right, and that's why unemployment would not stay at six percent"
>
> Interviewer: "Well he felt that that was not a description of a real capitalist economy"
>
> Milton Friedman: "No it's not, because unemployment does respond, if you have a country in which the natural rate is as best you can judge it five percent, unemployment reaches six percent, then either your estimate of the natural rate is wrong or else unemployment will not stay at six percent, people set in motion a series of events namely declining prices and wages which will lead to unemployment falling from six percent back to five percent, I agree with him"
>
> Interviewer: "Yeah"
>
> Milton Friedman: "It would be inconsistent with the theory to have unemployment stay at six percent"
>
> Interviewer: "Well he said that he would not believe that you would get galloping deflation as a response to this"

> Milton Friedman: "You would get deflation, of course, but it would not be galloping deflation, because as inflation decreases the unemployment rate would decline, and as the unemployment rate declined inflation would slow down"
>
> Interviewer: "He said that he was quite confident that people would not agree with the galloping [deflation], he said that he believes the theory would lead to a galloping deflation"
>
> Milton Friedman: "Well you should ask him for some empirical evidence, he is saying that despite what is happening to real wages, the workers would still hold stubbornly to the same nominal wage, that interpretation denies the importance of real as opposed to nominal"
>
> Interviewer: "So that theory's still running round the real/nominal distinction?"
>
> Milton Friedman: "That's right"
>
> Interviewer: "Ok, would you say that the natural rate is an exact number or an interval?"
>
> Milton Friedman: "No of course not"
>
> Interviewer: "It's an interval?"
>
> Milton Friedman: "No, I shouldn't say that, the natural rate is an exact number yes, but we don't know it"

Friedman disagrees with Solow's account on two points. He says firstly that it is impossible for unemployment to stay above the natural rate of 5%, and claims from Solow to suggest it would rely on the workers sticking to the same nominal wage, which Friedman believes they would not. Solow and Modigliani use wage rigidity here to argue that the wage would remain high. The second issue is that Friedman argues the natural rate is an exact point, but it is beyond the abilities of the profession to measure it exactly. In both cases small theoretical tweaks and interpretations maintain or corrode the Friedman/Phelps position. Where Modigliani and Solow prioritise the notion of wage rigidity, and subsequently question the Friedman/Phelps position, Friedman prioritises the real/nominal variable distinction, which lends support to his arguments. By selecting a theoretical construction to prioritise, macroeconomists defend or attack each others models.

Franco Modigliani also provides a theoretical debunking of the Rational Expectations Phillips Curve:

> Franco Modigliani: "I think that it's a very nice theoretical idea that has no empirical relevance, almost none, and the reason for that is that rational expectations supposed that everybody has the same expectations and will act [on these expectations], and so on, and that expectation comes true. It's nonsense because the expectation depends on your model, ok, in other words what happens to Y if I expect Y0 depends on the mechanism which is a function of the model, I have a model with rigid wages and therefore the mechanism is very different from Robert Lucas who has a mechanism for flexible wages, therefore we get very different expectations, so how can we have the same? There are as many expectations as there are people"

And another critique from Bob Solow:

> Robert Solow: "The arguments for rational expectations were always logical arguments, [a rational expectations proponent would say] 'come now, can you imagine that people whose livelihoods depend on the decisions that they make now are not exploiting every bit of data that they can find?' ... and I might then argue yes, but it's costly to find and analyse data, and why would someone engage in calculations that are so expensive that they are unlikely to pay off. They say 'ah yes, but rational expectations sort of subsumes that, the theory of rational expectations says that you exploit data optimally, of course you take account of cost'. That seems to me to be implausible"

Modigliani's argument suggest that people will not share their predictions of what will happen in the economy, as they have different models of how the economy works, and Solow's is that it would cost too much for them to do thorough research to gain realistic explanations. Addressing this directly is rational expectations theorist David Peel:

> David Peel: "Clearly most people don't know about the models, but the obvious mechanism would be public forecasts, i.e. that forecasts from informed groups are published very cheaply so we can get the data from the

> Treasury for the price of a daily newspaper, now that clearly embodies much more information in it than a simple adaptive forecast"

Peel theorises a mechanism by which Modigliani's and Solow's arguments do not hold. He admits that his example of Treasury publications may not go quite far enough to provide the level of perfect knowledge rational expectations presupposes, but he is willing it argue it has sufficient proximity to protect the theory. Solow, however, prioritises the search costs involved in acquiring perfect knowledge, and subsequently limits the validity of the rational expectations model. Both Solow and Modigliani have highlighted implications of the rational expectations theory and accounted theoretical reasons why they are fallacious. Alternatively, Peel has chosen to invest time and work in corroborating reasons why such weaknesses may be avoidable. Thinkers form both sides of the debate are prioritising the components of the theory that support their overall position, and equally importantly, placing less priority on those that may not.

Finally in this section we explore a few alternative reasons exhibited for accepting or rejecting a theoretical construction. Firstly a quote demonstrating how Robert Solow feels legitimate in rejecting an argument because it is difficult to actualise, in this case effectively measuring what expectations are:

> Robert Solow: "I don't like expectations based arguments, not that I think they are wrong, if I thought they were wrong that would be easy. But I think expectations are important in economic life, and yet they are something we don't know anything about. That's uncomfortable, that's a very uncomfortable situation"

As well as the practical measurement issues, some respondents discussed how policy issues related to macroeconomic theories can influence their acceptance. On this in others work, David Laidler:

> David Laidler: "Phillips and Paish's original work was mainly geared to discovering what level of unemployment was compatible with price stability, and the answer they got was about two and a half percent, or a little more, and this was regarded as outrageously high and politically unacceptable. The argument was that numbers like that were politically unacceptable, I think there were some people who regarded that as a sufficient reason for cutting off the debate"
>
> Interviewer: "In what way?"
>
> David Laidler: "Well in saying 'if your model is telling me we need a two and a half percent unemployment rate to keep prices stable then there must be something wrong with your model, and it certainly can't be applied to policy because two and a half percent unemployment rate is politically unacceptable', I'm not defending the logic of that argument, but it is an argument that you hear from time to time"

And on his own work, when asked why he felt rational expectations was better than the adaptive expectations associated with the Friedman/Phelps Phillips Curve, David Peel responded:

> David Peel: "Well I think it gave more sensible policy advice basically, that the implications of analysis of a monetary policy would make more sense studied in structures where agents were smart than where they were assumed to be dumb"

Clearly some evaluation of the policy implications impacts upon macroeconomic decision making. However, here it is premature to engage in a detailed discussion of the mechanisms as they stand. This is the topic of the following three chapters. For now it is sufficient to acknowledge that policy implications, and practical usage of a theory, can be used by a macroeconomist to accept or dismiss a theoretical construction.

In this section we have seen that macroeconomists can find reasons to save theoretical ideas they believe in, and find flaws in those they do not. It is a matter of introducing certain theoretical concepts that defend their own position, and applying different weight to the mechanisms that may be shared with those they disagree with. The final section on the evaluations involved in theoretical judgements explores other issues shown to impact upon theoretical decision making.

7.3.2 How Macroeconomists Evaluate Theoretical Work: Socialisation and Social Networks

This section explores some additional issues that impact upon macroeconomic theoretical debate. First off is an enlightening quote from Edward Kuska, a macroeconomist who moved to the LSE in the 1960s, on the role of previous life experiences on forming macroeconomic understandings.

> Edward Kuska: "I came from the States where the market system worked, I grew up in the Forties and Fifties, and the Fifties was the golden age in the United States, wealth was increasing very rapidly so to me the market economy worked, and when I came over here I was just stunned that a lot of the economists, you know, some of them had gone through World War Two and the aftermath, and they felt that it didn't work, you know, it had to be very closely sheparded to get any decent results, and that meant government control"

And a similar, but more explicitly political discussion from Richard Lipsey, originally from Canada:

> Richard Lipsey: "I came to the LSE, to England, a pretty strong believer in markets, it's a very mixed position, but in some sense it was, right rather than left, I wasn't interventionist, and then the experience in Europe, to a great extent in England and travelling around Europe made me acutely aware of market failures instead of market successes, and I started to think about poverty and other things that were just intractable if you left them to the markets, so I became much more what you'd call a Labour Party sympathiser, I was never comfortable with socialist economic policy but I was very comfortable with a Labour government social policy, so I believed in a sort of core of the market economy but there was a lot more room for intervention than I thought when I was doing my MA in Toronto"
>
> Interviewer: "Ok, and this was from your experience of moving into Europe, is this sort of European academic intellectual climate?"

> Richard Lipsey: "It's not such an academic thing, it's just looking at big cities and London slums, and travelling around in Europe, and watching farmers, and the difference between agriculture in Germany and agriculture in France, and social conditions were very different, if you crossed the border in 1953, it was just a world of difference between still almost Nineteen century horse driven French agriculture and Germany who'd been defeated by the war [and was] already mechanised with tractors, I said well this isn't just economics doing this, this is people attitudes, so I began to think there were a lot more forces at work than a sort of narrow view of it"

Both quotes demonstrate how the North American upbringing of Lipsey and Kuska shaped their political and theoretical understanding of the economy. The relative stability of the North American context led to less questioning of the market orientated explanations. Lipsey explicitly identifies a sense that poorer social conditions initiated an interest in variables explaining economic and social behaviour beyond an efficient market mechanism. However, as indicated above, at this point it would be premature to elaborate upon the political context of this quote, as this topic is addressed explicitly throughout the next three chapters.

Lipsey and Kuska, of course, were both discussed in the previous chapter, where their theoretical work on producing Phillips Loops was explored in detail. We return to how these papers were accepted by the wider audience in the following two quotes from Kuska, that demonstrate the importance of a macroeconomists profile in getting their work established. Commenting on the acceptance of Lipsey's explanation:

> Edward Kuska: "[Richard Lipsey's explanation for the Phillips loops was] for a while, I think, the only explanation of the loops, and he had made a name for himself, he had published several papers before that were really very good papers, and became a professor here I think at the age of thirty three, which in those days was unheard of, so he had sort of star quality, and his text, he wrote a text book soon after that, and it became a very popular text book, and the guy had charisma, and I think that also helps a bit. But my impression was that it was the only explanation for sometime and because the paper was considered to be a good paper, you know, people aren't going to think about it themselves, if they're not going to do research in that area, they sort of take the whole package, so I would have guessed most people would probably have said 'well this is quite a plausible explanation of what went on' "

And on his own explanation of the loops:

> Edward Kuska: "I don't think anyone ever read it."
>
> Interviewer: "Really, why not?"
>
> Edward Kuska: "I don't know. I think it was published in *Economica*, and I didn't have a name. If it had been published in *American Economic Review* it would have gotten read, I don't know, and it may have been slightly mathematical for people working in the field at the time, I just don't know but if you looked up references to it I think there'd be very few"

This dynamic increases in influence as the debate progresses, as during the period studied the macroeconomic profession grew at a high rate over time, both in numbers of staff and publications. This polarises the density of exposure experienced between the more and the less established writers.

A final quote that provides a quick insight into the role of friendship and social networks in macroeconomics comes from Robert Solow:

> Robert Solow: "Since Friedman and Phelps are both friends of mine, naturally if I wanted to try and argument out on anyone I would try it out on them"

Social relationships mediate debate in macroeconomics, and tends to prioritise high profile individuals. Also socialisation into political cultures impacts upon macroeconomists' prioritisation of theoretical resources.

7.4.1 How Macroeconomists Combine Theoretical and Empirical Resources

This section looks at the decision making process involved when theoretical and empirical resources tell different stories. Illustrations will be provided to show firstly how macroeconomists can prioritise one resource over the other, and usually prioritise the resource that most conforms to their preferred result. To start us on this, two quotes from George Perry discussing Phillips' original 1958 paper:

> George Perry: "Now it always bothered me, it bothered me from when I was writing my thesis, that [the Phillips/Lipsey Phillips Curve] was really what looked like a very strong empirical regularity, but with really very little in the way of a coherent model to make sense of it"
>
> Interviewer: "And that was a concern for you?"
>
> George Perry: "Oh yeah, absolutely"

However, this concern over the theoretical basis did not prevent Perry from believing the relationship existed:

> George Perry: "There's such a strong empirical regularity here that I don't have any problem in accepting it as true, but I still would like to be able to understand it fully"

And on a similar note, George Perry's PhD supervisor in this period, Robert Solow:

> Robert Solow: "That didn't bother me at all, because a natural way to proceed intellectually is to see an empirical regularity and then ask yourself why does it work this way? He [Phillips] saw an empirical regularity; I thought it would be interesting to look at a background theory for it, but also for me then, and possibly also now, informal theorising was OK, it's a very common way to proceed in economics"

Both Solow and Perry have prioritised an empirical finding over theoretical weakness. They were prepared to invest a research effort in providing a theoretical justification for the relationship as opposed to using the lack of a theoretical basis to discredit the relationship. However, macroeconomists can equally prioritise theoretical resources.

This illuminating quote from David Peel explicitly acknowledges the Interpretative Flexibility of data demonstrated in Chapter Five, and how theoretical resources are mobilised to address it.

> Interviewer: "When reading an empirical paper, how do you know if it's any good or not, what do you look for in a good paper?"
>
> David Peel: "Well one looks at the quality of the theory, you know most papers are going to be basically borrowing somebody else's hypothesis that I read, there's a few key papers that clearly are innovative, but the majority are going to be presenting results on different data-sets, one looks basically at the quality of the econometrics often actually"
>
> Interviewer: "The methodological?"
>
> David Peel: "The methodology and then the actual econometrics, the statistics of it, as to whether I give any weight to the results or not"
>
> Interviewer: "Do you mean how you look at the mathematics and whether you think they use the techniques properly?"
>
> David Peel: "Yeah, I mean in the context of Macro I have basic priors now, and that doesn't mean that I'm not prepared to change my opinion, for instance papers for instance on asset markets that purport to say that you can make lots of money following a particular trading rule, I'm deeply suspicious of, as an example, but yeah obviously it's about the quality of the hypothesis"
>
> Interviewer: "Ok if you have, as you do get, empirical papers that disagree on an issue, how do you know what to believe?"
>
> David Peel: "Well as you say the methodology, the econometrics, you know where it's been careful in employing statistics, there's something called co-integration which involves non stationary series and people will be claiming that certain variables are co-integrated, which I often simply don't believe because there's nothing in theory to say they should be co-integrated, and since the tests have often got low power I'd describe that as bad work basically"
>
> Interviewer: "OK, because?"
>
> David Peel: "Well if it doesn't fit in with any known theory that I don't know, obviously my theory could be well limited, but where I think it's reasonable I would evaluate results through that filter basically"
>
> Interviewer: "OK, do you feel that in empirical papers the results can vary quite widely depending on very basic constructions within the equations, things like lag structures and things like that can have a wide impact upon eventual r square ratios, r square results things like that?"

> David Peel: "Well yeah that's clearly possible, I mean two researchers armed with the same data-set may well come up with different results, but then that goes down to basic econometric expertise and theoretical priors, you know, if economics had no theoretical priors to impose on data then maybe we should just give all our data to statisticians and give up, but somebody once said if you give a series to a statistician and you give the same series to an economist the economists may ask what is it, while the statistician may not, the question 'what is' because we have priors about it, we have lots of exclusion restrictions, we don't typically put variable X, let's call it rain fall, in the equation explaining consumption, we don't have to test whether it's zero because we know it is zero ... so I think economics is really using theory intelligently to put restrictions on data, which is why there's an art in it, which is why two people armed with the same data-set could often get different results"

Peel speaks of intelligently placing restrictions on how you interpret and manage data. Clearly the notion of intelligent use has congruency with the enactment of priors, implying previous expectations of which patterns the data should present. Should a paper fall short of this it is probable that Peel will reject the research. It is relevant to note that this is only probably, as Peel did claim he is prepared to change his mind. A full discussion of when, how and why macroeconomists change their minds feature in Chapters Nine & Ten, however, we can begin to see from this quote that a great many contingent judgements need to be made regarding prioritising resources and specific locations within those resources, and of the expectation of what picture the data or theory need provide.

The argument is not that some macroeconomists, for example Perry and Solow, prefer empirical resources, whist others, for example Peel, prefer theoretical ones. It is that macroeconomists will prioritise the resources that best support their position. Indeed, at one point a macroeconomist may prioritise empirical resources, and at another prefer a theoretical basis for their position. On this, we discuss two sections of interview with Dale Mortensen, a writer originally associated with the Friedman/Phelps Phillips Curve literature, who later accepted the rational expectations position. On his work on the micro foundations Friedman/Phelps Phillips Curve literature:

Interviewer: "OK, to what extent did you consider your work an empirical or theoretical endeavour?"

Dale Mortensen: "I very much thought of it as empirically motivated, that the purpose was to try and understand the world in a more sophisticated way, and I continued to work on problems of the labour market, not so much inflation but unemployment"

Interviewer: "Certainly, so you would be swayed by, I mean if you were in a certain situation where empirical results did not conform with a theoretical position you would be much more likely to question the theory than the data?"

Dale Mortensen: "Yeah right, yeah, both right, given the data became persuasive"

Interviewer: "How does data become persuasive?"

Dale Mortensen: "In the usual way, as much as you can replication and, you know the sort of different angles of the data that are mutually consistent"

Interviewer: "OK"

Dale Mortensen: "In macroeconomics though, this is very difficult, the problem is that we don't have much data that's independent, macro data is highly time dependent, interdependent and correlated, so the notion of having sort of independent observations is really impossible, you begin to question this, that's why in some sense micro data ... is more dependable"

However, the empirically led account is not reproduced when discussing his change to the rational expectations position:

Dale Mortensen: "I simply felt that from a scientific point of view it provided a tool that allowed you to, oh well it just made the setting fit together better, that wasn't to say that scientifically models that have strong forms of Rational Expectations are better, but they do provide a baseline that at the time we didn't have, I guess [that's] where my support was strong for rational expectations, [to] have a baseline that you can better understand."

Interviewer: "Sorry, what do you mean by a baseline?"

Dale Mortensen: "Well if you really believe that misperceptions are important, start off with a model where there aren't any, alright and then illustrate explicitly how introducing expectations will change the result, we didn't have that bench mark at that time and we did afterwards"

Interviewer: "Ok, so you're saying this is a theoretical endeavour?"

Dale Mortensen: "Yeah, as a way of, as a tool"

Interviewer: "And how about the empirical support for rational expectations?"

Dale Mortensen: "Well that was less obvious, but you can fool all the people some of the time and some of the people all of the time, ok, but not all the people all the time, and understanding that point within a theoretical structure was"

Interviewer: "Ok, um, you were saying earlier that you were generally much more empirically led in you work and yet with the movement to rational expectations it was the [theoretical benefit]"

Dale Mortensen: "It was a theoretical innovation yes, alright I'm not particularly, that's not necessarily important invariably"

The juxtaposition of the Perry, Solow, Peel and Mortensen quotes establishes that macroeconomists do prioritise either theoretical or empirical resources above the other. The decision of which to prioritise depends upon a set of judgements led by the preference for the outcome of the research. The potential to do this clearly increases the flexibility macroeconomists have in advancing support for their own position.

Two specific examples of this demonstrate a mechanism by which macroeconomists can combine theory and data to attack a contested theoretical construction. First we revisit a quote already discussed in this chapter, from David Laidler:

Interviewer: "The idea of the Rational Expectations augmented Phillips Curve, did you have a lot of time for that?"

David Laidler: "Nope, never"

Interviewer: "Why not?"

David Laidler: "Well because it seemed to me to be empirically unsupportable, the Lucas aggregate supply Phillips Curve has got one really firm prediction, which is quantities move in response to prices, and if quantities move in response to prices the data should have prices moving in front of, or at least at the same time, as quantities, and the data don't show that, the data show that quantities move first, so that seemed to rule that out immediately"

Laidler is writing from a Friedman/Phelps Phillips Curve position and attacking rational expectations, now let's observe a similar attack in the opposite direction from David Peel:

> David Peel: "At that time I was taking an increasing interest in the efficient markets hypothesis, and essentially if markets are efficient then effectively expectations have got to be formed in a smart way, otherwise at least at a simple level there would be opportunities to make abnormal profits, and I saw no evidence in the empirical literature that that was the sort of feature that we observed, so all of these things were driving me to believe in a smarter way of forming expectations"

Both writers identify specific theoretical implications from the theory, in Laidler's case quantities responding to prices, and in Peel's opportunities to make abnormal profits. Empirical evidence is then provided to show the falsity of that implication. Subsequently the theory is rejected. This is a very similar mechanism to how Modigliani and Solow were identifying specific implications of a theory and providing theoretical justifications for why they do not hold. However, in these cases, explicit priority is given to the empirical work to overrule the theoretical proposition.

In neither case do the authors object to the concept of using theory, or the normal mechanisms by which they are used. Both are, in fact, using conventional theoretical methods of constructing the paradox between the theoretical and empirical findings. What they are objecting to is the specific form of the theoretical construction, i.e. exactly how the available concepts of the macroeconomists tool box have been configured and balanced to produce an expected pattern of results. Given a specific construction of theoretical concepts, and not forgetting the lessons from Chapter Five, a specific construction of the empirical evidence, each has chosen to prioritise the resource that best conforms to their established position.

Clearly then, the prioritising of theoretical or empirical resources is yet another location of Interpretative Flexibility available to the macroeconomist. This next quote from Keith Cowling demonstrates yet more complexity in how macroeconomists can consolidate empirical and theoretical findings. Commenting on his work looking for Phillips/Lipsey Phillips Curves in Ireland, he notes:

> Keith Cowling: "I noticed that the results were coming out in two blocks, those in those sectors which were structured in more or less competitive terms and those which were more or less oligopolistic, and where they usually confronted powerful unions, and there were two quite different sorts of results coming out, traditional Phillips Curves coming out of the competitive industries where wage inflation is related to the amount of excess demand for employment, and the other block the concentrated block where wage inflation appeared to be related to the profitability of the industry and the cost living index and so on, and you could see the unions in the oligopolistic structures giving rise to a different form of Phillips Curve, something more like Kaldor had been talking about, so I put the Kaldorian debate in the context of different sort of industries being interpreted in terms of different Phillips Curves mechanisms, and I thought that was interesting er which people hadn't really noted before, they'd set one against the other, Kaldor against Phillips or whatever"

Here Cowling has taken what at the time were two opposed theoretical arguments, the Phillips/Lipsey Phillips Curve, and a similar shaped curve using different explanatory variables associated with Nicholas Kaldor, and argued both exist in different industry contexts. This configuration of the two theoretical resources accommodates both within the empirical findings. This is an example of yet further flexibility in how the relationship between theory and data can be handled, and highlights the level of creativity involved in macroeconomic thinking.

The final quote in this chapter is from Robert Solow, and provides another insight into how economists use theory and data:

> Robert Solow: "In doing work I was prepared to accept [the Friedman/Phelps Phillips Curve] hypothesis, because if that wasn't the issue, you just needed a Phillips Curve, why add hassles that you can avoid. But I would at any time if asked have said 'well I don't think that's really right, I think that possibly you have something much closer to a vertical Phillips Curve in periods of very rapid inflation, my own guess is that if you have a varying small rate of inflation, Phillips Curves estimated from data like that would [be more like the Phillips/Lipsey Phillips Curve]' "

Solow is saying that although he does not believe the Friedman/Phelps position to be true, he was prepared to use it during its orthodoxy to avoid criticism in instances when the form of the Phillips relationship was not the matter of debate. This implies a level of flexibility where a macroeconomist can use a theoretical construction whilst still arguing it is false. The insight that the commitment to a Phillips/Lipsey Phillips Curve is less pronounced when it is not the issue at stake is also vital, and one that will be explored at greater length in Chapter Ten.

7.5.1 Understanding Interpretative Flexibility in Macroeconomics

The previous three chapters have demonstrated the existence of Interpretative Flexibility in macroeconomics. This is an essential task in any SSK research and completes the first stage of EPOR. Chapter Five explored how relatively minor changes in the construction of statistical manipulations can provide radically different empirical findings. Supporting evidence was found in the History of Economic Thought literature. The changes were located in six specific mechanisms, the macro data-set choice, data transformation, the development of proxies, micro data-set choices, identifying special effects, and the interpretation of data. Each mechanism is able to restrict the available interpretations of the data. They are aligned in such a manner that the results provide the best possible fit to the analysts expectations, and a process of continual reconfiguration is performed should the results initially not conform.

Chapter Six demonstrated a similar dynamic for how macroeconomists construct their own theoretical models. However in this case the dynamic is more creative, not focusing on restricting the available interpretations, but using the flexibility of model making to build a model that predicted patterns congruent with the analysts expectations. Through an effective configuration of the available theoretical tools, macroeconomists can produce a wide variety of patterns, and again will enter into a process of continual reconfiguration of the model until it acts as required. Again supportive arguments were discussed from the History of Economic Thought literature, most notably Cartwright (1999).

Given these vast levels of Interpretative Flexibility in how theory and data are constructed within the development of a single economic paper, this chapter uncovered further locations of Interpretative Flexibility in how macroeconomists judge the body of work in aggregate. This was initially divided into empirical and theoretical work.

When dealing with empirical work, macroeconomists can easily prioritise research that provides support for their existing position. Given what we saw in Chapter Five it is no surprise that research with a range of alternative conclusions does exist. However this is compounded when analysts can pick and choose data from any geographical and temporal location. This follows to the extent that macroeconomists can even deny the existence of empirical support for a theory, whether its proponents claim it or not.

Regarding theoretical work, macroeconomists can prioritise theoretical constructs to differing degrees, favouring those that support their position. Frequently an analyst will highlight a specific theoretical implication of a theory and demonstrate that it does not hold. This demonstration often involves the addition of another of the macroeconomists theoretical tools to reverse or irritate the dynamic central to the achievement of their opponents preferred outcome. Furthermore they will then not engage in a research effort to undermine their own finding, as they do not consider it necessary.

With the aggregated analysis of theoretical and empirical resources we can see that enormous quantities of Interpretative Flexibility exist. This is extended to an even further degree with a consideration of how macroeconomists combine empirical and theoretical resources. Again by exploring particular implications of a notion, analysts can mobilise either specifically chosen empirical or theoretical resources to contest or support a finding. By prioritising one over the other in a situation they can deflate the compulsion to acknowledge weakness in their own position. Macroeconomics is rife with Interpretative Flexibility, and it exists at all levels of macroeconomic work.

Another theme present in the previous two chapters is relevant here. This enormous level of Interpretative Flexibility in macroeconomics provides ample lubrication for the cycle of contest and closure. By this we mean that a macroeconomist who decides for some reason to change their position in the Phillips Curve debate is not restricted by either theoretical or empirical rigidities. We have seen that both can be constructed in such as way as to support a range of possibilities. In the previous chapters the conclusion was limited to saying either empirical or theoretical resources alone do not confine a macroeconomists position. This chapter has shown that further to this the two combined are also unable to prevent the construction of a coherent belief in alternative positions in the Phillips Curve debate. This argument is not present in similar studies from the History of Economic Thought literature.

This suppleness of the cycle of contest and closure does not provide an explanation of why shifts in macroeconomic orthodoxy occur. However it does supply additional insight into why the shifts transpire as speedily as they do. In the following chapters we will develop further explanations of why the changes are so swift and describe the dynamics that instigate these shifts.

It would be easy to mistake the arguments of the last three chapters. Let us state clearly what they do *not* argue. To some the discussion of macroeconomists shaping empirical and theoretical resources to confirm their expectations may suggest that they are cheating. This is not the case. Academic debate is contested amongst macroeconomists, who, as we have see in this chapter, are aware of the Interpretative Flexibility permeating the subject. The types of manipulations discussed so far are not performed without the knowledge of those who read the publications. They are endemic within the normative practices of macroeconomics and are enacted by those on all sides of the debate. This thesis has made no claims that there is any deceit between macroeconomists. Chapter Five has already discussed the morality of data handling in macroeconomics. There are norms regarding what is permissible and the actions described here fit within them.

However we can argue that there is a level of misrepresentation in the public presentation of macroeconomic work. The public face of macroeconomics is one of strict value neutrality and macroeconomists are reluctant to display the mechanisms discussed here. The dynamics that perpetuate this, and the moral discourse enacted to justify it, are central to Chapter Nine on value neutrality. We will see how macroeconomists construct the ethicality of their work so this is not perceived as a moral concern, and how the cultural context of liberal democracy interacts with this situation.

7.5.2 How Interpretative Flexibility Relates to the Rest of the Thesis

This and the previous two chapters have conducted a thorough exploration of how Interpretative Flexibility exists in macroeconomics. This is the first stage of EPOR. They have also argued that this provides lubrication for the cycle of contest and closure, while remaining silent on the origins of the macroeconomists' expectations of their work and how periods of transition between orthodoxies are initiated. The following three chapters explore differing roles for political culture in explaining this, completing the second and third stages of EPOR.

8. Political Interpretative Flexibility

8.1.1 Introduction

Macroeconomics deals with political issues. Many of the variables used in macroeconomic analysis are frequently used in political discourse. Terms like employment, international trade, Trade Union strength, workers' rights, inflation and interest rates are part of political and macroeconomic discourse. The Phillips Curve is a fine example of this, representing the relationship between two of these variables. This chapter explores the relationship between macroeconomic ideas and their political connotation.

The previous three chapters demonstrated that both empirical and theoretical resources are insufficient to settle macroeconomic controversy. The Interpretative Flexibility inherent in each allows the provision of empirical and theoretical support for opposing and competing conclusions. This chapter explores the possibility that social and political interests have a decisive role in closing macroeconomic debate.

The model for a social interests explanation is taken from Steve Shapin's (1975, 1979) account of 18th Century Phrenology in Edinburgh. Shapin's work has been chosen because, as discussed in Chapter Two, this and Barnes (1977) are the seminal contributions to the Edinburgh School's 'social interest theory' position. This is valuable because it is well known among Social Studies of Science scholars and represents an explicit account of the social interest model. The theory has become so ingrained in the modern Social Studies of Science literature that it is rarely represented as visibly as in these early works. Examples include Mello and Freitas' (1998) study of the proposed emergency use of a car fuel made of methanol, ethanol and gasoline in Brazil, and Rasmussen's (2004) exploration of the integration of academic and industrial pharmaceutical research in interwar America.

Shapin (1975, 1979) argues the shifting nature of Scottish society produced a political culture that was both shaped and reinforced by the debates concerning the acceptance of Phrenology as a scientific practice. To the newly established bourgeoisie the Phrenological claim that differences in the physical construction of people's brains led to diversity in people's skills and character lent scientific legitimacy to their political interest in promoting the emergent division of labour. Conversely groups keen to promote traditional social hierarchies in Scotland saw their values expressed through the old Scottish Common-Sense philosophical explanation of the mind, and invested resources in sustaining the field.

A similar explanation in macroeconomics would demonstrate alternative constructions of the economy representing particular political and social interests, and then illustrate that power shifts amongst these interests map onto the fast paced cycles of contest and closure.

However the argument here is that a social interest explanation cannot account for the experience of macroeconomics. This is because, like the interpretation of theory and data, the political significance of a macroeconomic idea is subject to Interpretative Flexibility. This chapter will demonstrate the dynamics that allow this to be the case, and then explore the implications for macroeconomic debate.

8.1.2 Political Interpretative Flexibility

Political Interpretative Flexibility does not mean that macroeconomic theories are apolitical. The empirical section of this chapter will show that many macroeconomists considered their political goals were expressed through the version of the Phillips Curve they supported, and saw the promotion of that Phillips Curve as a political activity. Instead Political Interpretative Flexibility means that a single scientific notion has flexibility in the political goals it embodies. Subsequently, in the case of the Phillips Curve debate, we find two implications. Firstly each incarnation of the Phillips Curve is used to express multiple political ideologies. Secondly macroeconomists with the same political background adopt opposing constructions of the Phillips Curve to represent their social interests. In effect macroeconomic arguments are not apolitical but any-political.

For macroeconomists the exact relationship between political ideas and macroeconomic ideas is complex, heterogeneous, and contingent. Some macroeconomists do not perceive any political connotation to their work. Others claim no political allegiances in or out of macroeconomics. Yet others identify clear political interests in their work. Of these a further subset assert that their support for a particular version of the Phillips Curve is a deliberately ideologically motivated act. These positions will be demonstrated empirically shortly. For now the vital insight is that the pattern of linkages between political ideology and the three versions of the Phillips Curve is not systematic. Despite the existence of ideologically motivated participants in the debate, there is no consistent association between any version of the Phillips Curve and a set of social interests. Since a systematic link is missing, shifts in the wider political power struggle do not produce analogous changes in macroeconomic orthodoxy. Social interest theories of the sort so fruitfully employed in the characterisation of Phrenology in 18th Century Edinburgh fail to provide equivalently insightful accounts of mid 20th Century Anglo-American macroeconomics.

We now turn to the empirical demonstration of Political Interpretative Flexibility.

8.2.1 Claims of Apolitical Macroeconomics

As stated above, some macroeconomists expressed political neutrality for either their theories or their personal beliefs. As a swift example, Peter Stoney:

> Interviewer: "How would you characterise your political position in the early 70s, late 60s, when [your Phillips Curve] papers were being published?"
>
> Peter Stoney: "Inchoate, undeveloped, not clear, it's only recently through the last fifteen, twenty years that I've become more clear about that kind of issue, yeah, didn't really have any political definition, clear definition, in my mind if that's what you're asking"

And Dale Mortensen:

> Interviewer: "Do you think the political impact of [your work on the Phillips Curve] is obvious, or is it ambiguous in terms of policy choices?"
>
> Dale Mortensen: "In terms of policy choices, well no it's probably somewhat ambiguous, it depends on how you look at it, the idea of [the Friedman/Phelps] Phillips Curve does not imply that there isn't room for short run counter-cyclic policy and the costs and benefits of that policy"

And elsewhere in the interview:

> Dale Mortensen: "Frankly I was more interested in making a name for myself, ha ha, and the political impacts of the work weren't the first order of consideration"

The role of apolitical discourse is discussed in detail in the following chapter. Within the context of the argument for Political Interpretative Flexibility we need only understand that some macroeconomists perceived no clear political connotation of their work and no political motivation to their actions.

We now turn to the more engaging demonstration of Political Interpretative Flexibility regarding the Phillips/Lipsey Phillips Curve.

8.2.2 Political Interpretative Flexibility in the Phillips/Lipsey Phillips Curve

To begin our exploration of Political Interpretative Flexibility in the Phillips/Lipsey Phillips Curve we will hear the reaction of one of its architects to the political response within the macroeconomics community. Commenting on a number of staunchly left wing Cambridge macroeconomists, Richard Lipsey:

> Richard Lipsey: "I was quite surprised, I was quite good friends with the Cambridge people, Nicky Kaldor and Joan Robinson, Nicky in particular was a good friend, they and Richard Kahn were violently opposed to the Phillips paper. I always had trouble understanding the strength of their opposition, and of course the basis of it was they thought the numbers were such that it would encourage the government to make the trade-off choosing a little more unemployment in order to get a little less inflation, and they would have liked the number to have been 10% unemployment to get rid of inflation, then no one would do it, so then it became very clear to me, but only after I saw the hostile reaction that it got, that it had these strong political implications"

The Cambridge macroeconomists objected to the Phillips/Lipsey Phillips Curve because to them it embodied the legitimisation of a less than full employment economy. They feared the ratio for the trade-off between unemployment and inflation was weighted such that governments might consider persistent unemployment permissible for large anti-inflationary gains. Politically these thinkers are on the far left, associating themselves with forms of Communism, and found the Phillips/Lipsey Phillips Curve too right wing to be acceptable.

However, the Phillips/Lipsey Phillips Curve is also associated with left wing thinkers, although less left wing than the Cambridge macroeconomists. Lipsey himself claims allegiance to the left, as do Samuelson and Solow, the leading figures in the U.S. in the Keynesian Neo-classical synthesis and publicists of the Phillips/Lipsey Phillips Curve internationally, Solow describing himself as:

> Robert Solow: "Someone on the left wing of the Democratic party, then and now"

Furthermore these three authors are not alone. We will articulate later how the Phillips/Lipsey Phillips Curve is expressive of mainstream left wing thought. For now we will continue with a discussion by Laurence Copeland.

We first discussed Copeland in Chapter Five where his empirical critique of the Key Industry Hypothesis forwarded by Eatwell, Llewellyn and Tarling was examined. Their paper supported the use of the Key Industry variable as a superior explanatory variable to unemployment on the horizontal axis of the Phillips/Lipsey Phillips Curve. Here he is referring to the same papers, and has already identified himself as right wing:

> Laurence Copeland: "There was a political background to this in the sense Eatwell, Llewellyn & Tarling were, you know, lefties, don't ask me how lefty or the what the theology of the left was or where they fitted on that spectrum in those days, but they were certainly left wing and that went, this Trade Union leadership and all the rest of it, went with a broadly sort of anti-market attitude, we didn't see it in those terms but they saw it in terms of this sort of right versus left, business market verse anti-market"
>
> Interviewer: "Do you think that that came through very strongly in their paper?"
>
> Laurence Copeland: "...you must remember that it wouldn't have to, because everybody knew who was working in the area, even those who were not working in the area, even the bloody politicians and all the rest of it were aware of the fact that there was cost push and demand pull, and the conventional wisdom in those days was all cost push, I mean you ask the bloke in the street he'd say cost push, he wouldn't use those terms but he'd describe it in those terms, everybody was cost push it was only us weirdoes at Manchester who were right in both senses of the word"

Here Copeland's account identifies a strict political distinction between the left and the right, and associates these groups with positions in the Phillips Curve debate. Copeland also describes the distinction as being very explicit and public. We continue the discussion with reference to the political make-up of the Manchester University Economics Department, where the Friedman/Phelps Phillips Curve promoting group of which Copeland was a member came to be called the Manchester School, or the Inflation Workshop:

> Interviewer: "There were left wing thinkers at Manchester though?"
>
> Laurence Copeland: "Oh Christ that's what I said, I mean within the department we knew we were unorthodox, the department was, you could divide the department into three, not two, the department was in three segments and it was almost, you could go round the department, it was like when I was a kid at school you knew who was a [Manchester] City supporter and who was a [Manchester] United supporter, you know, it was exactly like that, everybody was one of those three groups, I could have gone through the whole bloody lot right down to RAs and PhD students the whole lot. You were always one of three groups. There were the Marxists, who no doubt saw themselves as being variegated, you know Trotskyites and Neo-Ricarians, bloody Maoists and shit knows you know, but basically Marxists, and then there was a central group of old fashioned Keynesians and then there was the group around the inflation workshop, the Manchester School"

Copeland's account continues to construct strict linkages between Key Industry and Trade Union power oriented Phillips/Lipsey Phillips Curves and the left and the Manchester Schools Friedman/Phelps Phillips Curve advocacy and the right. However, as the interview probes deeper Copeland begins to question the boundaries he had previously been describing through a discussion of George Zis and David Purdy, two Manchester colleges who shared his views on the Phillips/Lipsey Phillips Curve:

> Laurence Copeland: "Well George Zis is a Marxist, George is a Marxist who let's say is one of a group of several Marxists I know in the British profession who are let's say flexible"
>
> Interviewer: "OK"
>
> Laurence Copeland: "Now don't ask me how they square it with what they do, that's not my affair, you know, a man's religion is his own affair, but em, that was a fact, so George worked both sides of the park, I mean, you see George is not quantitative, ... George is alright, but as I say how he squares it with his Marxism, he did a paper with David Purdy and it was published in the *Manchester School [of Economic and Social Studies]* I think, remember this, that when it comes to subjects like the influence of Trade Unions on inflation,

> the left was ambiguous, for some purposes they wanted to make it cost push, for some purposes they didn't, now don't ask me how they square that with this and all the rest of it"

Copeland's initial account of steadfast linkages between political interests and macroeconomic theories is amongst the most stringent of its type in the data collected. Despite this he still introduces ambiguities into the relationship. By identifying Marxists who support the Friedman/Phelps Phillips Curve, Copeland also provides a striking contrary position to the leftist construction of the Cambridge macroeconomists. Copeland never questions the ties between the right and monetarism, but does identify distinctions within left wing political interpretations.

These differences are in the political connotation of what Copeland terms cost push inflation. Cost push explanations of inflation stipulate that inflation is a product of passed on increases in production costs, usually wage costs through Trade Union influence. The Political Interpretative Flexibility for the left wing is in attaching a positive or negative judgement to Trade Union influence causing inflation. A positive left wing interpretation of accepting a cost push explanation is the implication that Trade Unions work well in representing workers' pay and conditions. Furthermore, for those cost push advocates who go so far as to exclude unemployment as an explanatory variable altogether, the theory suggests an impotency for incomes polices in controlling inflation, which means workers' wages will not be systematically repressed. However, on the negative side the argument that Trade Unions cause inflation negates the benefit of wage rises and could lead to calls for a reduction of Trade Union power.

The analytical insight here is not that all left wing thinkers were in continual paradox over these concerns. Neither is the argument that cost push inflation is politically neutral for the left. Indeed for many it may be obvious to each individual thinker whether advocating cost push is politically advantageous or disadvantageous. Several sources have confirmed that Bertie Hines, a left winger also discussed in Chapter Five as the advocate of the Trade Union militancy variable, considered the cost push explanation an embodiment of his political ideology. Challenging Hines in Chapter Five were George Zis and David Purdy, the left wingers Copeland acknowledges muddy his tightly drawn connections between politics and macroeconomics, two writers who were avid critics of cost push explanations.

We will hear from George Zis in the following section. First we will articulate one way in which the Phillips/Lipsey Phillips Curve expresses mainstream left wing political ideology. This formulation was alluded to by Franco Modigliani throughout his interview, although never articulated fully in a single quotation. The line of argument relates to the role of the relationship in the wider Keynesian framework.

The pre-Keynesian era exhibits a plurality of approaches to economic issues (Morgan and Rutherford, 1998b, Backhouse 1998). However, one central strand running through the neo-classical period is the prevalence of Equilibrium Analysis and Business Cycle theories (Backhouse 1985). Discussions of employment were subsumed within the wider discussions of business cycle theory, a position that would be reversed during the Keynesian era (Backhouse 1985). Business cycles were explained through a range of explanations. Examples include periods of innovation in Schumpeter (1912) and Robertson (1915, 1926), over-production of capital goods in Tugan-Baranovsky (1894), fixed capital formation in Cassel (1918), monetary factors in Hawtrey (1913, 1919, and a combination of many of these factors in Mitchell (1913, 1927) and Pigou (1912, 1927) (Backhouse 1985). Excluding perhaps Pigou's later work, this body of theory placed little emphasis on unemployment (Backhouse 1985). It is considered a product of fluctuations in the business cycle and will inevitably drift either side of a low equilibrium point. The pursuit of full employment was far from the policy agenda.

This, however, altered with the emergence of Keynesian economics. Here unemployment is caused by insufficient aggregate demand and is a variable that should be understood and controlled directly and not only as a result of the business cycle. By prioritising unemployment in this way Keynesian theory can be seen to represent left-wing interests in tackling the economic hardships of the population and offering arguments for big government interventionist policies in attaining these ends. It provided legitimacy for the argument that economies should be controlled by increasing government expenditure in social services and job creation, known as stimulating aggregate demand. This has clear resonance with mainstream left-wing political thought. This, of course, does not mean that left wing political ideologies cannot be represented through the business cycle theories listed above. Indeed, the notion of Political Interpretative Flexibility suggests this will inevitably be the case. This example is only to outline one of the many potential constructions of the political implications of Keynesian theory and its linkages to left-wing political ideology.

There were a range of perceived issues with Keynes' General Theory. One such perception concerned the flexibility of money wages. In the General Theory Keynes begins by assuming money wages are fixed. Then, in chapter 19, he relaxes this assumption, assuming it makes little difference (backhouse 1985). Keynes theory is based upon the idea that the economy will not naturally return to equilibrium, as is implied by Say's law. However, as Franco Modigliani argued, the theory only holds if money wages are assumed fixed (Modigliani 1944). If money wages are assumed to be flexible, then any unemployment will cause money wages to fall. Thus, Modigliani argued, workers' wages will become cheaper and firms will be able to employ more workers. This will continue until full employment is reached. Should this occur the equilibrium has been restored just as Say's law would predict. The entire theory, according to Modigliani, hinged on the assumption that money wages were fixed. He claimed that such as assumption proved hard to maintain. However, without it the Keynesian economic theory would collapse as Keynesians could not find an alternative explanation of why unemployment would not return to equilibrium. This changed with the arrival of the Phillips/Lipsey Phillips Curve. The perception was that by positing wage inflation as a product of unemployment and the dispersion of unemployment between industries, the Phillips/Lipsey Phillips Curve suggested the labour market could be in constant disequilibrium. This prevents the fulfilment of Say's law and solves this perceived problem in the Keynesian synthesis.

By completing the Keynesian theoretical structure, the Phillips/Lipsey Phillips Curve provided legitimacy for large scale government expenditure targeting the unemployed and poor in an economy. Through this quite lengthy set of linkages the Phillips/Lipsey Phillips Curve is constructed as a pivotal axiom in a wider framework of theories that express mainstream left wing political ideology. For Modigliani then, the Phillips/Lipsey Phillips Curve had left wing connotations.

A final quote from Keith Cowling, whose work supplemented the Phillips/Lipsey Phillips Curve with additional variables:

> Interviewer: "OK, do you reckon these papers then politically, well how would you call it, politically conservative or"
>
> Keith Cowling: "No, but infinitely flexible really"
>
> Interviewer: "What do you mean?"
>
> Keith Cowling: "Flexible in the political sense, they could be used by various people for their own devices"

At this point we can conclude that with the Phillips/Lipsey Phillips Curve exhibiting excessive right wing implications for the Cambridge group, being firmly to the left for Copeland, mainstream left for Solow and Modigliani, politically flexible when including cost push variables for left wingers and in any form for Keith Cowling, the evidence of Political Interpretative Flexibility is mounting. However the omission at this point is an empirical demonstration of a right wing expression of the Phillips/Lipsey Phillips Curve. The fact that this was not instantiated in the data does not undermine the concept of Political Interpretative Flexibility, on the condition that an articulation could be imagined. Through certain combinations of aggregate demand control and the impacts upon welfare spending the argument here is that one could be constructed. However, instead of dabbling in speculation our time is more productively spent exploring a case where the empirical support for Political Interpretative Flexibility is even more pronounced, the case of the Friedman/Phelps Phillips Curve.

8.2.3 Political Interpretative Flexibility in the Friedman/Phelps Phillips Curve

The last section has already demonstrated Laurence Copeland's association between the Friedman/Phelps Phillips Curve and right wing political opinion. A further supportive quote for this comes from left winger Richard Lipsey. When asked if the Friedman/Phelps Phillips Curve had an obvious political connotation, Lipsey responded:

Richard Lipsey: "Oh yeah, yeah, I mean it was basically anti any form of stabilisation policy, whether monetary or fiscal, it said that monetary policy should be directed at the price level and the macro unemployment should just be left to the market, very strong political implications about macro economic policy"

Interviewer: "Towards the right you're saying?"

Richard Lipsey: "Well I guess you'd call that the right, yeah, but I don't have any problem saying monetary policy should be relatively concerned with the price level, but I am for saying fiscal policy is a pretty blunt instrument, from which I say we must learn to do better at macro policy, and they say we shouldn't have any macro policy, but I think it was very cogent, we learned from it, but it definitely had political implications which are in a loose sense right wing anti-interventionist in terms of macroeconomic policy"

Interviewer: "And small government?"

Richard Lipsey: "Yeah"

Lipsey's account identifies a clear political interpretation for the Friedman/Phelps Phillips Curve. He couches it as an expression of anti-interventionist ideology. He associates this with right wing thought, while introducing locations of agreement with his own position and a reconfiguration of the interpretation that increases the congruence with leftist political ideology. First, let us explore how the Friedman/Phelps Phillips Curve embodies right wing priorities.

The Friedman/Phelps Phillips Curve's vertical position dictates the long run rate of unemployment in an economy with a given structure. Here there are three policy options available to the government. Firstly they could have accelerating rates of inflation to maintain a low stable unemployment rate. Here inflation would have to become continuously higher in order to prevent a change in the unemployment level. The notion of a trade-off between an infinitely increasing inflation rate and a stable unemployment rate is deemed unattractive. Inflation is considered a negative impact upon an economy because by forcing increases in interest rates leading to shorter pay-back periods, business' cash flow positions are weakened thus stifling investment.

The danger posed to business of ever-increasing inflation leads to the rejection of this policy choice.

The second policy choice is to employ what economists term supply side policies to move the Friedman/Phelps Phillips Curve to a lower level. This implies that the steady long run level of unemployment is at a lower level than it previously would have been, and that the long run position is reached more rapidly. Supply side policies are policies that alter the physical behaviour of economic agents. To list the dominant forms these policies take demonstrates their congruence with right wing political thought. Tax cuts are high on the agenda, intended to increase people's incentive to increase the ratio of work time to leisure time. Monopolies, including state monopolies, are targeted for the introduction of competitive forces. Policies are developed to limit Trade Union power, as these are deemed as prohibitive to the free movement of the market. Benefits are cut to the unemployed to increase their incentive to work, and labour mobility is promoted to encourage the unemployed to relocate in areas where jobs are available.

The third policy choice is simply that the position of the Friedman/Phelps Phillips Curve must be endured. Thus unemployment will exist at a certain rate, and it will be assumed to stay at that rate indefinitely. No attempt to reduce it will be made as this will only lead to short term gains with permanent increases in inflation.

This policy position clearly has congruence with the ideals of right wing thought. Business interests are prioritised over social interests and typically left wing institutions like Trade Unions and the welfare state are dismantled. Such policy prescriptions are associated with the Republican political writings of Milton Friedman (1962).

Political Interpretative Flexibility would imply, however, that there are left wingers who also consider their political ideologies expressed through the Friedman/Phelps Phillips Curve. An example is Edmund Phelps himself. On this Dale Mortensen:

> Interviewer: "Were you aware of any writers that you felt were doing politically motivated work?"
>
> Dale Mortensen: "I think probably Phelps was"
>
> Interviewer: "How?"
>
> Dale Mortensen: "Motivated by it, he felt more than others that … the policy makers were not taking into account the potential for future inflation in what they were doing, the potential consequences"
>
> Interviewer: "OK, so sort of reins the importance of future inflation within the political agenda"
>
> Dale Mortensen: "Right"
>
> Interviewer: "Would you say that that would cling to a specific party political position?"
>
> Dale Mortensen: "Not necessarily, it was more within the policy debate generally, it wasn't democratic or republican"

Mortensen claims a non-party political basis for Phelps' political motivation. However he does acknowledge a political motivation in the work. The ideological origins of Phelps' research are better revealed in his own reflections on the issue:

> Edmund Phelps: "I would be a living example of somebody who arrived at a theoretical perspective that has a lot of kinship with Milton Friedman, but on a whole range of policy things I don't agree with Milton Friedman …"
>
> Interviewer: "How would you characterise your political position [in the Nineteen Seventies] briefly?"
>
> Edmund Phelps: "Oh, probably just a mainstream Keynesian, a mainstream Democrat, I mean I still believed in using anti-cyclical tools of the government to fight recessions and to fight unsustainable booms and I was relatively moderate on the inflation side … I had the activism of Keynesians, I still retain the activism of Keynesians"

In the following quote Phelps is discussing the taxation implications of running supply side policies. By negative income taxation, he is referring to income subsidies paid to those living below the poverty line. This quotation is particularly interesting because Phelps alludes to the Interpretative Flexibility of the notions he believes in while accounting for them.

> Edmund Phelps: "I favoured a negative income tax when I was a young, very young economist myself but then later on I felt well wait a minute, why should we just splash this money on people independently of whether they work? Why don't we, I know you may say this is a more conservative idea, and it is and it isn't, wouldn't it be much better if instead we paid subsidies to employers to employ low wage people to pull up their wage rates and to pull up their employment? And then they would be self supporting and they would be more fulfilled individuals and they'd be happy and productive and it would be a revolutionary change"

By doing this Phelps is suggesting the use of supply side policies to lower unemployment. This is akin to investment to create high levels of aggregate demand as seen in many Keynesian schemes. To Phelps the negative income tax is a means of providing the unemployed with jobs beyond those created by the free market. Here Phelps is constructing supply side policies in such a way that they are expressive of his left wing political orientation. There is another way in which Phelps does this:

> Edmund Phelps: "I wrote a book in which I said, 'look four percent inflation per year is not so bad, and there might be certain things to recommend it' "

The Friedman/Phelps Phillips Curve suggests that any attempt to reduce unemployment below the natural rate will result in ever increasing inflation. By constructing an account in which inflation is not necessary an unquestionable negative event for an economy he negates the assumption present in right wing accounts of the Friedman/Phelps Phillips Curve that unemployment must not be tackled through stimulating demand because it risks inflation. For Phelps reducing unemployment does not cause a negative effect. It will cause inflation, but inflation is no longer deemed unbearable so-long as it remains a stable low level inflation. Thus the Friedman/Phelps Phillips Curve remains compatible with the welfare state and the other state apparatuses attacked as inflationary in right wing variants of the relationship.

Let us now return to George Zis, discussed already in this chapter by Copeland, on the politics of his papers criticising Hines' Trade Union militancy paper:

> George Zis: "Myself and [David] Purdy were well to the left of the labour party, I mean I have always personally identified with Greek Communism so in that sense I never espoused reliance on the market mechanism, but again this so called Keynesian analysis of inflation intellectually looked very, very weak"

Like Phelps, Zis is rejecting the free market anti-stabilisation solutions read into the Friedman/Phelps Phillips Curve by right wing thinkers. Unfortunately the George Zis interview occurred early in the research process before the notion of Political Interpretative Flexibility was articulated. In fact the experience of conducting the interview had a role in providing inspiration for the idea. Subsequently his political investment in the theory is not discussed any further. However we can say that to him the Friedman/Phelps Phillips Curve was not incompatible with his Greek Communist ideology.

There is another way in which the Friedman/Phelps Phillips Curve can be constructed without the supply side implications at all. The theory argues that the Friedman/Phelps Phillips Curve is only vertical in the long run. In macroeconomics the concept of the long run refers to the length of time all factors take to adjust to the most recent change. It is commonly argued by Keynesians that this is so long that it need not concern us. Subsequently the Phillips/Lipsey Phillips Curve, and its political connotations, remain in the Friedman/Phelps model, be these a left or right wing construction of the Phillips/Lipsey Phillips Curve.

Finally we will observe a final quote by a Friedman/Phelps Phillips Curve advocate on ideological motivations in the Phillips Curve debate, David Laidler:

Interviewer: "Do you think that there were people who were trying to express directly political views, and were making theories and using data in such a way that supported political opinion?"

David Laidler: "Oh what a good question. I suppose the answer is yes and no … when you are concerned with economic policy and someone comes up with a position you don't agree with, and seems to leave out evidence that you think is absolutely critical, the first reaction is usually to think that this is an ideologically motivated piece of work that was being written for ulterior motives, I mean I think that's just human nature to react that way, but I've been treated that way by other people often enough ha ha"

Interviewer: "You mean people assume your work is ideologically motivated?"

David Laidler: "See my work in exactly that way… if you turn to the proceedings of the 1972 BAAS conference, which was edited by Joan Robinson, … you'll find she goes out of her way to be extremely nasty and hang ideological motives around my neck in her editor's introduction"

Interviewer: "She was accusing you of ideologically motivated work?"

David Laidler: "Yeah, and I'm sure I was accusing Joan of that as well, ha ha, no question about it"

Interviewer: "Do you think that what you were doing was ideologically motivated work?"

David Laidler: "Well you can look at the paper … no I didn't think so"

Laidler locates macroeconomic debate in a politically motivated discourse, but declines to implicate his own involvement. This reminds us that even in the context of Political Interpretative Flexibility macroeconomics is also pervaded with the norms of value neutrality. How this relationship functions will be discussed in the next chapter. For now we simply conclude that like the Phillips/Lipsey Phillips Curve, the Friedman/Phelps Phillips Curve is expressive of both right and left wing political ideologies. Political thinkers from opposing positions can construct the relationship as expressive of their political ideology. As we will see, this is also true of the Rational Expectations Phillips Curve.

8.2.4 Political Interpretative Flexibility in the Rational Expectations Phillips Curve

The Rational Expectations Phillips Curve suggests that there is no relationship between unemployment and inflation in either the long or short run, i.e. that the Friedman/Phelps Phillips Curve long run position holds in all instances. The only instance in which an economy could deviate from the long run vertical position is one where the government lied to the public about their policy actions. Here the public's expectations of changes in the price level will be incorrect for a short period. However, quickly they will realise their expectations were incorrect and they will be adjusted accordingly. A government cannot continually lie to the public, as the public will soon learn to expect the government to act in this way and will build this knowledge into forming their price expectations.

This being the case, the right wing construction of the Friedman/Phelps Phillips Curve is applicable here. Moreover in the case of the Rational Expectations Phillips Curve the set of propositions are stronger because the long run logic is assumed applicable in all circumstances. The leading proponents of rational expectations in both the US and UK are well known for their right wing positions, Robert E Lucas and Patrick Minford. However, as Political Interpretative Flexibility predicts, this does not mean all rational expectations advocates are right wing. On Lucas and his frequent co-author Thomas Sargent, rational expectations convert Dale Mortensen says:

> Dale Mortensen: "I mean you can go through it, there are no two people more politically different than each other than Bob Lucas and Tom Sargent, their politics are 90 degrees to each other, but their methods as researchers are similar"

Just as in the U.S., frequent rational expectations co-authors Lucas and Sargent are political opposites, in the U.K. the prominent research team of Minford and David Peel experienced far reaching political differences. In this lengthy articulation of the issues at hand, Peel provides an account of the political connotation of the Rational Expectations Phillips Curve, firstly as right wing, then as apolitical, and finally his own position as expressive of left wing ideology. Responding to whether the relationship had an obvious political implication, Peel revealed:

> David Peel: "Well I suppose this one did, that's why it was so challenged, maybe it was put in that form because the proponents of it wanted it to be taken that way ... everything got rolled together so a word like monetarist became a dirty word to some, ... I would have thought of myself as a Monetarist in that I believed in a vertical Phillips Curve, certainly in the long, run but for instance I thought of myself as a socialist, so my views on defence expenditure, health or whatever, taxes, were totally irrelevant to that position, but I think a lot of people thought that if you took up a position on rational expectations it implied that you were somehow a right winger and pushing a certain view, but those things are never logically related"

We continue:

> Interviewer: "So to make it clear you're saying that it's completely sensible and not incompatible for you to be a socialist and believe in rational expectations"
>
> David Peel: "Not at all, not at all"
>
> Interviewer: "That's what you're saying or you don't think it's a contradiction?"
>
> David Peel: "Not a contradiction at all ... it's simply a proposition about money stock and inflation, there's nothing to say per se about your views on health or what the appropriate ratio of government expenditure to income are, it's simply a proposition about control of inflation"

Peel's acknowledges a public perception of connections between the Rational Expectations Phillips Curve and the right. He asserts his disagreement with the claim through a discourse of objectivity. However as the interview continues he is willing to reinstate political connotations for the relationship:

> Interviewer: "OK, so you've said you don't feel there's any link between rational expectations and a political position, do you think..."
>
> David Peel: "Well there needn't be, there have been people who have clearly been associated with rational expectations who've been very political, Patrick Minford is an example, there are many others"

> Interviewer: "And have you seen, say, the Friedman style Phillips Curve or even the original [Phillips/Lipsey] Phillips Curve, do you think there have been political advocates that have been using these for their positions as well?"
>
> David Peel: "Well it's, ha ha ha, funnily enough you see if properly interpreted even the zero trade-off idea [rational expectations], which caused a lot of problems ... actually from a left wing perspective could have been taken on board and used much more effectively, because essentially what it was saying [was] there was no long run trade-off between inflation and unemployment, now remember what economists of a so called middle ground or left wing persuasion were saying [now referring to the second formulation of the Phillips Curve], remember what policy advice was based on that. It was, to quote Frank Page, that we need to keep a pool of unemployment to moderate inflation, having a long run trade-off implies you can only keep inflation down by maintaining unemployment at a high enough rate to keep it down, what the zero trade-off position is saying, particularly under [rational expectations], is that there's essentially no meaningful trade off, so you don't need to keep pools of unemployment to keep inflation down, what it's saying is that if you want to reduce unemployment you don't do it by inflating, you do it by having active labour market policies that bear on real factors such as mobility of labour, housing, unemployment benefits, retraining, the whole cacophony of policies that make the labour market function more smoothly, but you can't just fix it by simply inflating or having a pool of unemployment, now you know if having a pool of unemployment is a good left wing idea then I wouldn't particularly want to be associated with it"

Peel's account identifies clear left wing connotations for the Rational Expectations Phillips Curve. The promise of eradicating pools of unemployment provides expression for the leftist political goal of full employment.

As with the previously discussed incarnations of the Phillips Curve relationship, the rational expectations version experiences Political Interpretative Flexibility. It can be constructed as expressive of multiple political agendas. We now turn to the implications of this widespread phenomenon for the arguments of the thesis and the pattern of contest and closure in the Phillips Curve debate.

8.3.1 Understanding Political Interpretative Flexibility in the Phillips Curve Debate

This chapter has demonstrated the existence of Political Interpretative Flexibility in all three versions of the Phillips Curve. Both symptoms have been illustrated, each incarnation of the Phillips Curve is expressive of multiple political ideologies, and macroeconomists with the same political beliefs can consider these beliefs best expressed by differing versions of the Phillips Curve. Due to the structure of this chapter this second symptom has not been discussed empirically until now. Through comparison of the discussions of each incarnation it is clear that all right wingers, all left wingers and all Marxists have not supported the same Phillips Curve. In many of these cases the embodiment of a political ideology in the theory has also been established.

We have also touched upon Trade Union variables in the Phillips Curve debate, in discussions from Copeland and Zis. However an exploration of the Political Interpretative Flexibility of Trade Union militancy versions of the Phillips Curve has not been conducted to the same level of detail as for the Phillips/Lipsey, Friedman/Phelps, and Rational Expectations Phillips Curves. The analysis of Copeland's quotations did make some progress towards this, but was not as complete an account. This is because there are no proponents of the Trade Union militancy versions of the Phillips Curve participating in this research. The methodology chapter noted that John Eatwell was contacted several times, but did not agree to be interviewed. However this does not mean that these versions of the Phillips curve are not subject to Political Interpretative Flexibility. Contrary to this, the argument of Political Interpretative Flexibility implies that it is inherent in all constructions of knowledge, both theoretical and empirical, and from any discipline. To claim otherwise would stand in contradiction to the philosophical tenets that underpin this thesis. The suggestion that there is something inherent in any idea that leads to a particular political connotation grants agency to the idea. This is akin to adopting the teleological model rejected by Bloor (1973, 1976) where truth has a role in its own discovery, as discussed in Chapter Two. Here political connotations would have a role in their discovery. This is in direct contradiction to the methodological relativism of the Sociology of Scientific Knowledge and the constructivist agenda. In some instances it may not be immediately obvious how a particular idea can be constructed to have multiple political connotations. However it is an in principle argument that, given the time to construct appropriate networks of values, any political position can come to be embodied by any knowledge claim.

8.3.2 Social Interest Models of Explanation in the Phillips Curve Debate

Just as empirical and theoretical resources have been shown unable to settle macroeconomic debate and explain the cycle of contest and closure, political interests also fail to provide impetus for opening and closing debates. This is not because Phillips Curve theories are apolitical, but because the patterns of linkage between political ideology and macroeconomic concepts are not systematically distributed. Unlike Phrenology in 18^{th} Century Edinburgh, shifts in the wider political culture do not impact upon consensus in macroeconomics through the social interest dynamic exemplified by Shapin (1975, 1979). This is because a wider political shift in any direction will not spur an increased public profile or strengthen the research effort of a particular Phillips Curve construction. Since the expression of political ideology is diffuse amongst opposed macroeconomic positions the increased power associated with shifts in the dominant political position is equally dispersed and thus diluted.

The argument that political interests are insufficient to account for the cycle of contest and closure is a negative one. This position declares what is not, not what is. However, again as with empirical and theoretical resources, a positive contribution to the explanation of the swift transitions between orthodoxy is available.

8.3.3 Political Interpretative Flexibility as a Lubricant to the Cycle of Contest and Closure in Macroeconomics

The argument is now familiar and well rehearsed. However it is still worth reiterating. As with empirical and theoretical resources, Political Interpretative Flexibility implies that as well as being insufficient to cause shifts in macroeconomic orthodoxy, political interests are also insufficient to prevent such shifts. When a shift between macroeconomic orthodoxies begins the ideological interests were too weak a restraint to impede it. This is for two reasons. Firstly, on an individual level, macroeconomists who do express their political views through their work are able to reconfigure their construction of the political connotations into compliance with their acceptance of the new incarnation of the Phillips Curve. An example of this is Phelps, whose position on negative income taxes shifted in such a way that constructed supply side policies as expressive of left wing values. The second reason why political interests do not repress shifts in orthodoxy under conditions of Political Interpretative Flexibility highlights the role of young macroeconomists in shifts of orthodoxy. This is a topic argued at greater length in Chapter Ten. For now we need only accept that central to establishing a new orthodox position is enrolling the support of the new generation of macroeconomists. These are less entrenched in old theories and are more willing to explore new research agendas. We can see that if strong systematic linkages between political positions and incarnations of the Phillips Curve existed then the flexibility for young politically motivated macroeconomists to adopt the latest position would be curtailed. This is because young right wing macroeconomists would be more inclined to adopt the position systematically constructed as right wing, as with left wingers. The lack of these linkages removes further tension and adds further lubricant to the cycle of contest and closure.

8.3.4 How Political Interpretative Flexibility Relates to the Rest of the Thesis

As suggested in the introduction, this thesis can be separated into two sections in two ways. In one, the first three analytical chapters discussed empirical and theoretical resources and the remaining three discussed political influence on the data. In the other the first four chapters consider how the forms of Interpretative Flexibility add suppleness to the cycle of contest and closure, and the remaining two explicate the driving forces that shape shifts between macroeconomic orthodoxies. As such it is this chapter that fits within both groups by being associated with Interpretative Flexibility and political influence.

As an Interpretative Flexibility chapter it has fitted well with the mode of analysis followed in the previous three chapters. As a chapter on political influence it differs to the remaining two on this topic. While this chapter argued politics does not influence macroeconomic debate through a social interests model, the following two chapters illustrate an alternative mechanism through which political influence exacts a decisive role in macroeconomic debate. The next chapter, Chapter Nine, considers the role of the macroeconomists construction of self as value neutral in the legitimacy of liberal democracy. Chapter Ten builds upon this analysis by exploring the interaction between macroeconomics as a problem solving discipline and the immediacy of policy needs.

9. Macroeconomists' Construction of Self as Value Neutral and the Legitimacy of Liberal Democracy

9.1.1 Introduction

The previous chapter discussed how macroeconomics can represent political ideologies, although in practice it does so in a way that is contingent and unpredictable. In this chapter we explore another interaction between the roles adopted by macroeconomists and the political sphere, the difference being that in this instance the results are most predictable and almost unanimous. This chapter explores the relationship between the legitimacy of liberal capitalist democracy and the macroeconomists construction of self as value neutral. It is here, and in the following chapter, that we perform stages two and three of the Empirical Programme of Relativism (EPOR).

We start by exploring what macroeconomists mean by value neutrality, and will swiftly demonstrate that in practice they do not conform to any classical notion of neutrality. However, the argument of this chapter is that although the set of actions engaged in the name of value neutrality do not achieve the goal of objectivity, they do have a significant impact upon the cycles of contest and closure observed in macroeconomics. This is because, due to the construction of self as value neutral, academic macroeconomists disengage themselves from the policy setting agenda. The full ramifications of this for shifts in orthodoxy are articulated in the next chapter on macroeconomics as a problem solving discipline. This chapter facilitates this analysis by firstly drawing upon interview data to elucidate the process by which the construction of self as value neutral is actualised and secondly by drawing upon the Social Studies of Science and History of Economic Thought literature to demonstrate macroeconomics role in lending legitimacy to liberal democracy.

9.1.2 Macroeconomists' Construction of Self as Value Neutral

The discourse of value neutrality pervades macroeconomics. Central to the construction of value neutrality adopted by macroeconomists is the distinction between normative and positive science. Here the value neutral role means an analyst may illustrate the ramifications of various policy options available to government, but may not exercise any influence on which policy is adopted. On the normative positive distinction, a very public advocate, Milton Friedman:

> Milton Friedman: "It's extremely important to make that distinction because people who differ on ultimate values may, nonetheless, agree on the positive side, so people may have exactly the same opinion of what the effect of doing A will be, but nonetheless the values of one person may lead him or her to want to do A and the values of the other person may lead him or her not to want to do A, so to treat economics as a science it needs to be a positive science, in a sense it's a contradiction in terms to talk of a normative science, what's normative is a matter of philosophy or of ethics but not of science"

Another, more mechanical, account of value neutrality comes from David Peel. When asked about the role of the macroeconomist as value neutral he replied:

> David Peel: "Yeah, I think I can sympathise with that, I think in terms of the economic system and we have theories of consumption and investment and et-cetera, you put that together and we have a model of the macro economy, now

in principle then if we pull certain leavers, if we increase government expenditure, reduce taxes, whatever, then we'll get certain outcomes, that's as far as I see it, having that model, the actual policy is about the objective function of the policy maker, I, for instance, may have been against defence [expenditure] for years, I may have wanted all sorts of things, but I don't see that as being basically a problem to me because I see the macro model"

These forms of discourse are typical amongst macroeconomists, and there are multiple examples in the data. However there is obvious difficulty squaring these crystalline images of the value neutral macroeconomist with the behaviour discussed in the previous four chapters. Indeed, in the previous chapter we discussed a lengthy quote from Peel himself exposing left wing political priorities expressed through the Rational Expectations Phillips Curve. On this contradiction it is interesting to hear the accounts of two macroeconomists who eventually rejected the role, firstly from one of the architects of the Phillips/Lipsey Phillips Curve, Richard Lipsey. When asked if he adopted the value neutral role of the macroeconomist he responded:

Richard Lipsey: "Yeah, absolutely strong believer in that, until I went into the government in 1963, the National Economic Development Office (NEDO), I became an advisor for a couple of years there, and I came back to [my earlier colleagues] and gave a seminar where I said this is just nonsense, the way you present a thing, what you choose to leave out, what you put in, the words you couch it in, the stress you put on the qualifications, you know it's going to effect the way people are going to make decisions and there's no way out of this"

Interviewer: "You were talking here to academic economists?"

Richard Lipsey: "Yeah, that's when I really first formalised it and I gave it to a seminar, but basically I gave it because [the value neutral] position that you give neutral advice and other people choose just is unreal, it's not reality, every bit of advice you give is loaded in one way or another, you try to prevent things but what you cannot do is give completely neutral advice, you can't separate completely the objective from the subjective, ... but until I went into NEDO and saw economists giving advice to people who weren't economists I was a strict believer in this, but it didn't stand up to more than a few months of empirical testing"

Secondly a leading UK author on the Friedman/Phelps Phillips Curve, David Laidler:

Interviewer: "You said earlier that during your time in the Seventies you would have considered what you were doing to be economic science and dispassionate"

David Laidler: "Oh yeah sure I would, sure I would"

Interviewer: "So that was your idea of what an economist was, to be the dispassionate scientist?"

David Laidler: "Oh listen, I was a student of Karl Popper's at university, I believed in the dispassionate testing of theories, it took me quite a little while to figure out that that wasn't what people actually do and even when I..."

Interviewer: "Sorry, so what do people actually do?"

David Laidler: "Well I think that what people do is that they generate their theories for one reason or another and they try to get their own theories established. You don't get papers written by people who say 'Here I have this good idea but when I tested it it turned out to be wrong', what they've usually done is beat their computer to death until it gives them positive results and I think the scientific attitude, if you like - the kind of Popperian model - turns up at the level of the intellectual market place in which we try and defend our own theories and refute other people's. And there is an audience out there called graduate students and the interested lay public who look at the debate and make their own minds up on the basis of it"

Interviewer: "And this is what you do as well?"

David Laidler: "Um, this is what I now think I was involved in, but I think that if you talk to Laidler circa 1972 I would have said 'This is science that I am doing and if my results happen to show that economic policy in Britain is badly conceived, well that's not a political point to be making, that's a scientific point to be making, and I do hope that the politicians will listen' "

Both Laidler and Lipsey express a concrete commitment to the value neutral role during the periods of their biggest contribution to the Phillips Curve debate. Equally they both accept that their own actions at the time did not in any real sense conform to their ideals. Laidler proceeds to declare that in practice macroeconomists batter their data into submission and only engage in the discourse of value neutrality in the public sphere. However Laidler's claims seem too strong on this point. There is full agreement that macroeconomists engage in purposeful manipulations of their data to attain predefined results. However the suggestion that the commitment to value neutrality is purely a mask adopted to retain credibility under the public gaze is too simplistic. To repeat, the argument here is that many macroeconomists do have a genuine commitment to value neutrality. Furthermore they engage in actions they consider compliant with this goal. The way macroeconomists conduct their research and frequently the position they adopt in the debate is at least in part a result of their construction of self as value neutral. The notion of value neutrality is intrinsic in shaping the cycle of contest and closure. This of course does not mean that macroeconomics is the objective science so many of its practitioners say they strive for.

In the following section we illustrate both the nuances and the heterogeneous application of the value neutral construction of self in practice.

9.1.3 The Value Neutral Construction of Self in Practice

What follows is an exploration of opinions from macroeconomists who identify with the drive for value neutrality. We will see how the respondents acknowledge the inevitable negotiation of normative positions while enacting strategies to maintain the value neutral framework. We commence with a short quote from Keith Cowling, who, when asked about the extent to which he felt his role was to attempt to collate the evidence as best he can to demonstrate his existing view point, responded:

> Keith Cowling: "No no, I'm not that unscientific, I do take a scientific approach, but I also have a view of the world, you can't not have a view of the world when the world is economics because we are surrounded by these forces all the time, we come to judgements based on our perceptions… but I try to be dispassionate"

Cowling provides an account of macroeconomists as inevitably located within the normative and subjective discourses about macroeconomics, but engaged in purposeful action to minimise its impact upon their work. The argument that economists are inevitably dealing with normative issues in found in Myrdal (1958). He argues that value judgements and judgements of facts are logically inseparable, a position labelled the 'strong non-neutrality' thesis by Mongin (2002). The Cowling quote also suggests a boundary issue. It is locating the boundary that identifies the level of inevitable normative influence that is permissible while maintaining the value neutral construction of self. In essence, what level of political involvement violates value neutrality. Pondering this, George Perry:

> Interviewer: "Do you feel that there is room for political expression while being an economist?"
>
> George Perry: "Well I think it depends a little bit on what you mean by political, if you mean if a Democrat is in office I would express different views

> about economics than if a Republican was in office, I don't think I'm guilty of that, if you mean the things that I emphasis in economics reflect my political ideas more broadly, I think the answer to that is yes, if you go back to the old [Phillips/Lipsey] Phillips Curve story, one concern that I had was that shifting the emphasis to a focus on inflation rather than 'let's think about targets for real variables like unemployment', I happen to think that low unemployment benefits people and benefits the poor by providing a kind of labour market where employers have to take a chance on employing people who maybe aren't on paper as qualified as they might otherwise be, and so forth, so in general a high pressure economy is good for a more egalitarian society, and I think helps take people into the mainstream who would otherwise have a harder time, so for a whole range of reasons that's one of the things that I think ought to concern us in policy making, and so when attention was shifted entirely away from that I probably had more resistance to the idea that you forget about that and focus on inflation and everything else will take care of itself than I would of as a purely intellectual exercise"

To Perry the boundaries of what is and what is not political and subjective are more complex in practice than the discourse of value neutrality suggests. The idea of changing one's beliefs in response to the electoral political climate is deemed beyond the bounds of objective science. However, defending an argument because of a political interest represented by it is perceived as acceptable. This illustrates the availability for judgement making in constructing how value neutrality should be actualised.

The second valuable insight from Perry's comments is the acknowledgement of moral standards denying the permissibility of shifting intellectual positions with changes in political power. By labelling this action as one worthy of guilt he constructs it as sinful behaviour. It makes clear that a moral boundary does exist, and that on one hand shifting intellectual position and on the other defending an argument due to its political implications lie on either side of it.

At this point it is useful to introduce two analytical frameworks that we can use to explore the macroeconomists construction of self. The first is Gilbert and Mulkay's (1984) notion of the empiricist and contingent repertoires. The empiricist repertoire is used in formal interpretative contexts such as journal articles and employs an empiricist representation of scientific action. The contingent repertoire includes accounts of personal actions, judgements and group membership. These are almost never seen in formal interpretative contexts. This contrasts to informal discourses where both repertoires feature.

Gilbert and Mulkay, of course, work within the Discourse Analysis tradition discussed in Chapter Two. The theoretical premise underlying this position limits the applicability of these concepts in this context. This is because, as discussed in Chapter Two, Gilbert and Mulkay argue the multi-vocality and context dependence of discourse means analysts can only research the interpretative strategies used by respondents in developing their accounts of social action. The researcher has no grounds upon which to comment on the actions undertaken by their recipients beyond these interpretative strategies. The counter-arguments against this are outlined in Chapter Two and need not be repeated here, suffice to recall that the position adopted in this thesis is in line with the counter-arguments presented by researchers in the SSK tradition. Subsequently in this context we can and do make claims about participants actions that stretch beyond the limitations of the Discourse Analysis approach. As Collins (1983b) suggests, there is no inconsistency in using Discourse Analysts' conclusions, but equally there is no compulsion to adopt their methods.

For these reasons we can also use Erving Goffman's (1959) concepts of frontstage and backstage to understand the macroeconomists' construction of self. Goffman uses a dramaturgic approach to research, meaning employing a theatre analogy for everyday life. Actors are engaged in the 'impression management' of frontstage social interaction, implying the presentation of self as congruent with social expectations placed upon them. In the backstage actors assess their frontstage performance and plan their future presentation of self in their next frontstage encounter. The backstage is not necessarily the preserve of the individual, as actors can operate in teams that present a frontstage impression to other groups. Of course, the individuals would still maintain a frontstage/backstage distinction within the group itself. It is in the context of such a macroeconomics team that we employ the frontstage/backstage distinction here. Goffman's dramaturgic approach is used fruitfully elsewhere in the Social Studies of Science literature, for example Hilgartner's (2000) exploration of dietary recommendations in the United States.

In the frontstage the performance of macroeconomists is as Laidler described in a previous quotation, they present themselves as value neutral actors and do so through engagement in the empiricist repertoire (Gilbert and Mulkay 1984). Their public persona is in most cases one of proud objectivity. However the Cowling and Perry quotes begin unpacking the complexities of the backstage using the contingent repertoire. Commitment to a sense of value neutrality is not absent in the backstage, as is in keeping with Gilbert and Mulkay's belief that both repertoires can co-exist in informal discourse. Both quotations demonstrate attempts to locate subjectivity in an objective framing. However both accept subjectivity exists. The issue is how best to manage subjectivity while maintaining consistency with a commitment to value neutrality. On this, two quotes from Mike Sumner, one of the most vocal advocates of the value neutrality position among the respondents:

> Mike Sumner: "When you've accepted the [Friedman/Phelps Phillips Curve] there might well be directions in which you'd be led by your political beliefs, but at the end of the day the question of which policies are most fruitful is not something that can be settled by belief"
>
> Interviewer: "That would be settled by?"
>
> Mike Sumner: "The evidence, what are the costs and what will they achieve"
>
> Interviewer: "It would be political to start prioritising things in advance though?"
>
> Mike Sumner: "Yeah, I mean what you would look at might well be conditioned by your political or more general social beliefs, the view you took about equality of opportunity as a desirable objective might lead you to rule

> out certain possibilities and concentrate on others, but if you find your preferred options are not going to do the job in hand, or will do so at prohibitive cost, I think most economists would say 'oh well, that's the way the world is' "

Sumner's account clings tightly to the rhetoric of value neutrality. However he still accepts subjective inputs exist. He places the onus on methodological processes to weed these out, an interpretative strategy terms the 'truth-will-out device' by Gilbert and Mulkay. Furthermore he notes that the role of the value neutral macroeconomist is to accept these findings and with them sacrifice the prior political belief. We can of course question in practice whether empirical evidence would be accepted in this way, given what we already know about the Interpretative Flexibility of data. Nevertheless to Sumner the concept of changing his opinions as the manifestation of value neutrality is meaningful, and is illustrated with even more clarity in the following excerpt:

> Michael Sumner: "There is a popular perception in some quarters ... that academic economists make their mind up and then do the research that verifies it. I like to be able to demonstrate that 'look, this is what I used to think in 1968, and by 1972 I'd changed my mind because the evidence appeared to change', partly because I think it's an essential feature of any science and partly I think it illustrates the separation of economics from the kind of things you were talking about earlier, political opinions, social attitudes and so on"
>
> Interviewer: "Which is important?"
>
> Sumner: "Yeah I think so"
>
> Interviewer: "Because?"
>
> Sumner: "Oh well because I think if we don't make that distinction then we may well back the wrong horse and go up a blind alley"

Interviewer: "In what way, what do you mean?"

Sumner: "Well if we ignore the evidence or choose not to let it change our beliefs then we'd still be flat earth-ers wouldn't we? If you go back far enough, we might still believe in a long run [Phillips/Lipsey Phillips Curve] trade-off"

Again Sumner associates value neutrality with being data responsive and a preparedness to change position on an issue. His account goes so far as to display pride in his own acceptance of these values. In the backstage area Sumner's investment of faith in the methodology of macroeconomics provides consistency in his construction of self as value neutral. Political interests are deemed permissible because the techniques of macroeconomics will neutralise them, another firm application of the truth-will-out device. The role of statistical methodology will be discussed in detail later. For now let us conclude this section.

Macroeconomists employ a frontstage rhetoric of value neutrality that draws upon an empiricists repertoire. The central tenet to this is maintaining a distinction between positive and normative macroeconomics, and locating themselves exclusively within the former. However the preceding four chapters have demonstrated how the process of macroeconomic research is permeated with value judgements beyond the model of objectivity they parade in public. We have explored how this inconsistency is negotiated in the backstage area. While remaining committed to a sense of value neutrality, we have witnessed Cowling express the pursuit of value neutrality as something to strive for, but only achievable as a close approximation. Perry constructed the meaning of value neutrality with a different boundary from that found in frontstage accounts of what is permissible. Sumner also accepted subjective inputs to macroeconomic work, but invested faith in the methodologies used to eliminate their effect in any final conclusions. We can see from this that the backstage accounts are typified by nuance and negotiation. The definition of value neutrality is formulated in such a manner that it makes forms of normative influence permissible.

However the key analytical insight gained here is not that macroeconomists claim to be value neutral when they are not. The central argument to be taken from this, drawn upon later in the thesis, is that macroeconomists do express a moral commitment to value neutrality and they do enact a set of actions in its name. The following section will discuss how the origins of the value neutral construction of self lie in the legitimation of liberal democracy followed by a discussion of the difference it makes to macroeconomic debate.

9.2.1 The Macroeconomists' Construction of Self as Value Neutral and Liberal Democratic Politics

An empirical exploration of the historical association between value neutrality and legitimacy in liberal democracies is beyond the remit of this thesis. Instead we draw upon the work of Yaron Ezrahi (1990), whose work covering the development of all sciences provides an excellent insight into the processes underlying the cycle of contest and closure in macroeconomics.

Ezrahi's analysis explores the dawn of liberal democracy and the processes by which this new political system established a moral legitimacy for its system of rules and power distribution. It required a discourse that the population would accept as permitting authority to be centralised amongst the small group of leaders. Ezrahi argues that where as the monarchist system acquired its legitimacy through reference to widely accepted religious narratives, the new political institutions of liberal democracy would come to depend upon a foundation of external rationalisations provided by the cultural resources of enlightenment science.

In essence Ezrahi argues that the leaders of liberal democratic societies would claim legitimacy for their authority in the eyes of the population by reference to an external notion of truth. To counter suspicions that they were acting in self interest they would rely upon the cultural resources of fact and objectivity to demonstrate their decisions had a grounding in an externally rationalised location. The trust placed in science as a path to de-personalised knowledge was subsequently lent also to the liberal democratic state. Just as the monarchist state would act with legitimacy because God said they were right to, the new leaders would act because science says so.

We can explore the dynamics that brought this situation about in more detail by considering Ezrahi's comparison between the values of liberal democracy and those associated with the monarchism it replaced. Central to the legitimacy of the later is the de-personalisation of political power, while retaining authority for the individuals who participate in governing bodies. To Ezrahi the use of science itself, and the adoption of scientific modes or description, is essential to this re-negotiation of role. Let us discuss the three roles identified by Ezrahi for science in this legitimising political power in a de-personalised context. We will focus on examples from his work that explore examples from economics.

The first role for science is the reconciliation of the requirement for order while preserving a commitment to social freedom. The second concerns the partial de-personalisation of political action while maintaining the status of agents as individuals who are responsible for their own actions. The third Ezrahi considers is the need for the public acceptance that those with political power act in the interests of those they represent, and subsequently that they are accountable publicly.

The first role can be addressed in several ways. One of particular interest in this context identified by Ezrahi is the framework offered by Adam Smith to legitimate the acceptance of freedom instead of control as the route to socially desirable outcomes. Smith is famous for the notion of the invisible hand. His theory of balances and forces towards centrality in the pricing system of free markets provides moral legitimacy for economic freedom and self serving actions. However, these self serving actions are to be carried out in a framework of rules and laws limiting the range of acceptable actions to those such as mutually agreed exchange, but not theft. This is exactly the logic expressed in liberal democratic capitalism. However when provided by Smith, with the cultural association to science and all that brings, the system is afforded the authority of reasoned investigation, and a legitimacy routed far beyond the opinions of those with political power. Instead the observation of laws and regularities invokes a discourse of naturalism, externalising the burden of justification from the political system itself. The framework, of course, by which rationalisation can offer external legitimacy is associated with the development of enlightenment thought and Newtonian Mechanics themselves. For more on Smith's negotiation of positive and normative value premises in economics, see Young (1990).

Economics has a role in Ezrahi's second role for science; the de-personalisation of political action with maintenance of individual responsibility. Smith's invisible hand and the wider framework of economics at the time constructs a model in which occurrences at the societal level result from aggregate individual actions. A person is capable of acting in a specific location, and being responsible for their actions, while the wider social consequence is unanticipated and dislocated. Furthermore, the wider social consequences are not deemed the result of a single individual's actions, as they may be with a monarch, but are due to the wider flows of multiple dispersed human actions.

To Ezrahi the issue of responsibility further benefits from the employment of the discourse of science in a different manner. This is because issues of responsibility and accountability, Ezrahi's third role for science, use the tools of science themselves as instruments of external authority. By this we mean the notion of dispassionate observation as a route to the truth. This value can be witnessed in the practices of democratic election, prosecution through legal frameworks, and many other social tools of accountability. Particularly in the case of court trials, the discourse of rationalising the decision making process and externalising the conclusion beyond the realm of individual bias is readily activated.

Throughout the Phillips Curve debate, and still today, the legitimising role of objective science holds strong in liberal democratic society. Dispassionate observation still lends authority to the decision making process in many spheres of social life, for example democratic elections and prosecutions through legal frameworks. Equally in the Phillips Curve debate the perceived objective nature of constructions such as the Friedman/Phelps Phillips Curve were employed as external rationalisations of economic policy decisions, for example Thatcher's implementation of the Medium Term Financial Strategy (MTFS) in the U.K. (Maynard 1988). Should the policy makers be criticised for pursuing policies that permitted unchallenged long term unemployment the external rationalisation provided by Friedman (1968) and Phelps (1967) would be evoked to deflect blame away from the political process. Instead the observation of laws and regularities creates a discourse of naturalism that externalises the burden of justification from the political system itself.

It is to this end that the culture of value neutrality remained robust in macroeconomics. Let us note that the actions of macroeconomists need not produce truly objective science. The actual output of their work or the form of objectivity adopted by macroeconomists are not of great importance in this context. This is why the subtle negotiations present in the backstage construction of value neutrality matter little. The essential element is the maintenance of the perception of value neutrality amongst the electorate and subsequently the lending of legitimacy to the wider political culture.

While Ezrahi provides an exploration of the mechanics by which science adds legitimacy to liberal democracies, Steve Shapin (1996) provides an account of the early history. In the context of medieval till Seventeenth century Europe, Shapin describes the social, political, and religious upheaval that led to the role of science as lender of legitimacy. The Protestant Reformation of the sixteenth century features prominently in Shapin's account. The unity of the church had provided moral legitimacy to the wider culture for centuries. In the Reformation scepticism arose about the cultural legitimacy of the then contemporary forms for knowledge provision.

The passing of this period also saw a change in the demography of scientists. Until the Seventeenth century many natural philosophers were clerics, and the institutions of science, including Universities, were related to the church. However as the patterns of literature consumption altered more gentlemen became interested in learning science. Indeed Shapin mentions an ethical guide for gentlemen of the time that advocated the learning of science as well as involvement in civic duties.

Shapin describes the role of Francis Bacon as an example. Bacon, the lord chancellor of England and court counsellor to Queen Elizabeth and King James I, partook in an explicit effort to highlight and propagate the expansion of state power through the enrolment of learning cultures. He argued for the production of knowledge to become an enterprise ran by the state. Bacon feared the appearance of intellectual resources based upon the Protestant notions of each individual's ability to establish truth and falsity for themselves. Without the mediation of priests in knowledge production these intellectual paths may arrive upon conclusions contrary to the well being of monarchic rule. To Bacon the success of scientific inquiry, free from these individualistic tensions, would be to enact a methodological preference for collective labour in knowledge production. These sentiments were echoed across Europe, and in accord with them, societies such as the Royal Society of London and the Florentine Accademia del Cimento were established. An interesting comparison between the publications found in the journal of the Royal Society of London and those of the French Académie Royale des Sciences has been conducted by Gross, Harmon and Reidy (2000). They demonstrate the tendency for French socio-political structures to produce greater influence on measurement and explanation in scientific research compared to the British preference for the collection of facts.

Shapin argue such institutions are important for three reasons. Firstly they were an alternative sphere of learning to the Universities, and opted to move away from the hierarchical structure of Universities at the time. In this they concerned themselves foremost with the production of new knowledge, not just the reinforcement of established ideas through teaching. The new knowledge was to be orientated towards the goals of civic society. Secondly they operated on the collective labour model identified above. Thirdly it drew upon the norms of gentlemanly interaction in ensuring order and calm in knowledge production. Vocal arguing and dispute did not feature in the expected actions of a scientific society member. Part of this gentlemanly code of interaction was the refusal to discuss disruptive topics. Thus the contentious matters of politics or theology were prohibited from discussion. The criteria by which a topic was deemed contentious concerned whether the subject fitted into the category of normative, relating to judgments, or the category of rational accessibility, the preserve of science. In this environment it was deemed possible for gentlemen to disagree without a challenge to the social legitimacy of knowledge production.

Shapin is keen to note that this science was not in opposition to Christianity. Instead it was used to further provide legitimacy for it. By illuminating the mechanical complexities of the natural world, for example to construction of a fly's eye, science was not deemed to be undermining notions of God's power, but instead praising God's infinite creativity. Only one as powerful as God could produce such a complex mechanical organisation. Oslington (2001) provides an illuminating account of the negotiations of this position in political economy in the 1800s. He describes competing accounts of how political economy represents religious interests from Nassau Senior and John Newman. Senior argues that political economy promotes religious well-being because it promotes wealth, which in turn promotes knowledge, which in turn promotes religion. Newman takes issue with this claim arguing to make such a statement is beyond the boundaries of political economy and instead tramples on the ground reserved for ethics and theology. Instead Newman argues all knowledge forms a circle where the sciences are co-dependent upon each other. However it is essential that each science should remain within its disciplinary boundaries. He suggests that within this circle the use of economics is limited without ethics and theology. Science and political economy then, were providing cultural authority to both the state and the church. The disinterested nature of the gentlemanly codes spoke to values of objectivity. Shapin notes that the greater the levels of disinterestedness and objectiveness, the greater the moral legitimacy afforded to the church and state would be.

Shapin's text, however, come no closer to the modern era than this discussion. So for a comment on the role of science and technology through the industrial revolution and into the modern day we turn to Jurgen Habermas (1970). Habermas' discussion of the role of science and technology appears in his discussion of the rational society. He discusses Weber's notion of rationalisation, as an attempt to understand how scientific-technical progress forms the institutional framework of modern societies. He further notes Marcuse's (1968) critique that these changes are not a process of rationalisation itself, but the adoption of rationalisation as a specific form of political domination. To Habermas, this domination manifests itself as the class structure witnessed in liberal democracies and a product of the industrial revolution. Where Marx identified the exploitation of the proletariat as the engine of social change and power struggle, Habermas claims the process of rationalisation has de-politicised the majority in society. This leaves the movement towards revolution stifled in liberal democracies. It is science and technology that have been used to lend legitimacy to liberal democracy and the class system it engenders.

Habermas is less detailed than Ezrahi on the exact mechanisms by which this occurs. However, he does make similar comparisons to earlier times and earlier political structures where the cultural resources of myth and religion formed the basis of the authority assumed by the ruling powers. The erosion of the legitimacy taken by these is Weber's notion of secularisation, where they take on the characteristics of subjective value systems. Habermas argues they become replaced by notions bourn of the critiques of religious dogma, laced with scientific character. The difference between these notions of science and earlier forms of investigation is the identification of technical control as a central value. This knowledge is technically exploitable towards continual economic growth, and instils the value of progression central to the authority of liberal democracy. Like Ezrahi (1990), Habermas points to the development of the mechanistic world view of the seventeenth century as central to this process, and identifies the role of the belief in the market as a locus of authority for liberal democracies.

A discussion of the changing notion of objectivity in economics is provided in an edited collection by Morgan and Rutherford (1998a). Drawn together by an introductory discussion by the editors (Morgan and Rutherford 1998b), the collection charts the development of objectivity in American economic circles from the late nineteenth century into the post-war period. They argue the economic academic climate in the pre-war era was characterised by pluralism, implying the use of a variety of methods and ideas. This is supported by Backhouse (1998) and Biddle (1999). Morgan and Rutherford are keen to stress that this pluralism embraces more than just American Institutionalism, a movement in economics that stressed the importance of the social and historical institutional context upon which economic actions were played out, and became intent on ratifying empirical research in economics through institutions such as the National Bureau of Economic Research (NBER) (See Yonay 1994 and Rutherford 1999).

Rutherford and Morgan use examples from the edited collection to demonstrate the changing standards in economics for demarking research as 'scientific'. In the 1920s many researchers from heterogeneous groups considered themselves scientific. For the institutionalists this implied employing the empirical and experimental techniques of the natural sciences. However, in the pluralist economics of the period many alternative constructions were in operation and the claims to represent scientific standards were subject to continued debate (Backhouse 1998). Despite this debate, Morgan and Rutherford are able to identify three aspects of the notion of scientific work in economics that are shared by the majority of American economists. The first is the focus on concrete or practical real world discussions as opposed to abstract discourse. The second is the identification of correct scientific practice in the researchers personal attributes, such as their professional integrity, as opposed to their choice of method. The third congruence is the emphasis on even-handedness in asserting objectivity. Being scientific did not refuse the economist the opportunity to comment on policy decisions or forwarding the case for particular policy regimes. Instead the research was to be fair and respectful towards competing claims, and prioritise the social interest in policy advice.

This contrasts with post-war neo-classical economics where objectivity is assumed both at the level of investigation and at the level of beliefs and policy advice. Morgan and Rutherford identify two processes that have fuelled this change. Firstly the objectivity based on personal attributes and skills was replaced with an alternative construction of objectivity based upon the application of specific methods, i.e. mathematics and statistics. Between the wars this was most evident in agricultural economics, the NBER of the Institutionalists noted above, and developments in econometrics, including the Cowles Commission. Goodwin (1998) argues that technical expertise in statistical methods expanded in the interwar period as economists were positioned in Government departments. They employed these techniques not just on economic variables, but also in analysis of fighting techniques such as bombing raids and firing patterns. A similar, but more critical argument is in Mirowski (2002). Goodwin also stresses the role of the conflicting pressures on institutions of higher education, business leaders, and social science foundations in limiting the role for forms of economics outside of the neo-classical economics in the post-war era. The role of mathematics and statistics in macroeconomics will be discussed in more detail later in this chapter.

The second process underlying the shift between constructions of objectivity in Morgan and Rutherford is the growth of faith in the 'market solution' and free competition in American intellectual circles. This belief was not widely held by economists in the late nineteenth century, as demonstrated by Bateman (1998) and Mayhew (1998), both contributors to the edited volume. Only in the post-war era were competition and individualism considered representations of objectivity and value neutrality. Morgan and Rutherford argue that by 1960, the time of the early Phillips/Lipsey Phillips Curve publications, the American Neo-classicalism and a construction of objectivity based upon methodological detachment were well established.

We will return to Morgan and Rutherford's arguments later in the chapter. First we will outline one further force for change in the construction of objectivity in economic proposed by Julie Nelson (1993). Nelson explores the influence on economic objectivity and method of biases that are usually invisible because they seem natural and self-evident to the community of male researchers. The period studied coincides with the early period considered by Morgan and Rutherford (1998a), 1880s-1930. Nelson analyses the differences between the 1885 Statement of Principles of the American Economic Association, an 1888 revision of these, and the Scope of the Society statement adopted by the Econometric Society in 1930. She argues that the three texts demonstrate an increased value in detachment as objectivity over time. This is contextualised with the observation that a perception of oneself as separate or detached is a particular masculine style of self-identity. The suggestion is that wider beliefs about the constructions of masculinity and femininity have impacted upon this shift. Nelson does not argue that there has been any change in the association between masculinity and detachment. Instead the claim is that the masculine self-identity was placed under progressive threat during this period combined with the simultaneous reduction in the traditional avenues for the expression of masculinity. As evidence Nelson points to the sentimentality and piety of nineteenth century culture as a process of feminisation, and the decline of physically demanding occupations in which men could prove their manhood. Given these pressures alternative expressions of masculinity were sought, and Nelson suggests the push towards detachment in economics is one.

However, in the context of the argument presented here such renegotiations of the construction of objectivity in the pre-history of the Phillips Curve debate are not central to the argument. It is not contradictory with Ezrahi's (1990) claims on the role of value neutral sciences in legitimising liberal democratic society if the construction of objectivity changes. The contributions of Shapin (1996), Habermas (1970), Morgan and Rutherford (1998) and Nelson (1993) have added insightful colour to the historical narrative presented here, but do not include the time periods covered by the data collected as part of this thesis. Now we have established the reasons for, and form taken, by the macroeconomists construction of self as value neutral we explore the impact it has upon macroeconomic work.

9.3.1. The Differences Made by the Value Neutral Construction of Self

There are two ways the construction of self as value neutral has impacted upon what macroeconomists do. Firstly it has led to the rise in the use of statistical methodology, as suggested by Morgan and Rutherford (1998b), and secondly it has disengaged the macroeconomists from the policy making circles in which the problems of macroeconomics are set. Both will be discussed in turn, although the full ramifications of the latter will be left to the following chapter.

9.3.2 Quantitative Methodology as Value Free Macroeconomics

The period explored in this research saw an explosion in the use of statistics and testing orientated mathematics. This was furnished by advancements in macroeconomists' knowledge of statistical technique and the availability of computer technology. However the data presented here show it is still most closely associated with a sense that mathematical testing was more value neutral, or rigorous (Weintraub 2002), than other forms of forwarding the macroeconomic research effort. On the position of statistical work, Peter Stoney:

Peter Stoney: "I think there was a tendency for econometric work to be regarded as hallowed ground as it were, so if you came up with something [through statistical techniques] which was apparently cogent that intrinsically it had merit, I think that it's subsequently been realised that that's a somewhat naive not to mention somewhat arrogant view, I think that there was certainly ... a feeling in those days that econometric work was somehow better than non-econometric work and there was a certain youthful enthusiasm in the approach, I think there was certainly a feeling that if it wasn't quantitative then it wasn't much good"

Interviewer: "And you had this feeling yourself?"

Peter Stoney: "Well I'm bound to yes to be frank to be quite truthful about it yes, I mean I don't hold that view anymore"

Interviewer: "Why did you believe it at the time?"

Peter Stoney: "Well it was just part of the general wave and surge of optimism for econometric work, I mean it came from American people, Franklin Fischer and Goldberger, they'd written these textbooks that were full of mathematical symbols and techniques which had been quite undeveloped, they'd existed but they'd been undeveloped in terms of application to economic problems, so there was a feeling that this was the great hope of solving problem[s]"

Chapter Five argued that Lipsey's Philips Curve paper was influential because it used standard mathematical testing techniques early in their popularisation. The paper emerged from the ethos of the Measurement and Methodology Seminar group based at LSE (for more on this see De Marchi 1988), of which Lipsey was a member, as was Edward Kuska:

> Edward Kuska: "There was a seminar that all the junior members of staff ran and they were sort of revolutionary seminars rejecting everything that the older members of staff thought was important, or at least they thought at the time, and Max [Steuer] and [Richard] Lipsey, and Chris Archibald was here at the time, and they were all roughly the same age and they all went to the methodology seminar, you know, with the belief that if they got the methodology right [they could achieve great things]"

And later:

> Interviewer: "And it was orientated towards discussing new methodologies which at the time would have been model making and statistical?"
>
> Edward Kuska: "Yeah Popper ... had just publish[ed] *Conjectures and Refutations*, and so my impression was that they were supporters of Popper's position and they thought you made hypotheses and you go out and test them, and the testing was really important, that you shouldn't just create castles in the air, so they were all keen to do that and they thought that this methodology would change the world"

A useful framework for understanding the links between the enthusiasm for statistics in macroeconomics and value neutrality has been developed by Theodore Porter (1995). The general thrust of Porter's work is that the standardised methodology of statistics has been adopted across sciences and administrative systems because the universality of the techniques embodies trust. The techniques lend authority to their user because their common standards are deemed able to impede any space for personal judgement in their application. Clearly Chapter Five has shown this promise to be fallacious. That, however, does not prevent statistics delivering more stable trust relationships, even if it cannot deliver value neutrality.

Clearly Porter's work has congruence with Ezrahi's (1990). Both explore the role of the cultural association of science to depersonalised truth in lending legitimacy to governments, bureaucracies, and in Porter's case knowledge production itself. Porter's focus also differs in that he emphasises the standardisation of mathematical techniques as central to trust relationships.

One specifically pertinent example from Porter's work is the development of experimental psychology, the social science that took the standardisation of statistics most passionately to heart. The wide scale adoption of quantitative methodology in experimental psychology occurred in the 1930s and 40s, twenty years before macroeconomics. Porter notes that, as in other cases he discusses, the forces initiating the adoption of statistical techniques did not originate within the research community itself, but instead with the pressure of public exposure. He extends themes threaded throughout his work on the role of disciplinary weakness in motivating the adoption of standardised methodologies in Psychology. Institutional frailties and intellectual disunity are pointed to as the locations of such weakness. In this context the inflexibility of quantitative methods was adopted to compensate for the lack of a secure community. The commitment ran so deeply that young researchers in the field would be socialised into feel guilt for redefining their hypothesis after exploring the data. A comparison between this account and the experience of macroeconomics illuminates some of the issues operating in how and why statistics came to represent value neutrality in the Phillips Curve debate.

Similar to the last point in the discussion of Porter is the quotation already featured in this chapter from Mike Sumner in which he expressed a level of pride in being able to demonstrate his objectivity through his data-responsive rejection of a previous belief in the Phillips/Lipsey Phillips Curve over only four years. The investment of personal pride in conforming to the professional expectations of value neutrality placed upon him has congruence with the motivations of the young Psychological researchers identified by Porter. However, as we have seen from Chapter Five on the use and constant remoulding of data in macroeconomics, the manner in which these motivations are expressed is noticeably different. Chapter Five demonstrated that the redefinition of hypotheses was an accepted practice throughout the Phillips Curve debate. While the exact configuration taken by the moral positions associated with the handling of data are clearly different in the two cases, the pride felt by the researcher through the meeting of professional standards of statistical use resonate in both situations. As noted previously when discussing the contributions of Morgan and Rutherford (1998) and Nelson (1993), the exact formation taken by the construction of objectivity is not important to Ezrahi's (1990) argument, so long as a construction is in operation. To Porter's argument for it is important that the moral judgements aspire to the use of statistical methodologies, which is the case in both disciplines, while again the exact configuration within this does not face such requirements.

Porter's discussion of intellectual disunity translates less well into the macroeconomic case. This thesis has shown that in any single temporal location studied in this thesis macroeconomics is rarely characterised by anything other than overwhelming consensus. Of course, as we have seen the content of the arguments around which the majority rally shifts with astonishing regularity. There are several specifics of the macroeconomics case that may shed light on this. Firstly the argument can be made that the experience of the Great Depression brought the findings of macroeconomic thought into question to an extent not witnessed before. This faced the profession with an unprecedented collapse of public trust, and an associated increase in public and political exposure. The existing cultural association of macroeconomics as a source of externally rationalised knowledge became challenged.

Porter's notion of policing the boundary of a discipline lends further interesting insights to the analysis when alternative sources of the boundary issues are explored. It is the case that over the period studied the macroeconomics profession expanded in numbers dramatically. This may have introduced boundary issues in terms of attempts to keep new researchers out. Furthermore, it would increase the likelihood of researchers not having personal links to each other, exacerbating the mistrust discussed by Porter, and the networks of trust discussed by Lipsey in Chapter Seven.

There is a second issue about potential violations of the boundary. Macroeconomists inhabit a discourse that is widely shared with everyday public and political discourses. Subsequently the boundary disputes may not arise from internal discontinuities. Instead they may see their source in the perception of a personal legitimacy to contribute to the debate on the behalf of political, media and public voices. This would have been compounded, if not created, by the atmosphere of distrust in the post Great Depression era.

The role of the Great Depression is also discussed by Morgan and Rutherford (1998b). They argue that the Great Depression created a climate where American economists were required to diagnose and treat the problems facing the economy. Furthermore the wartime planned economy also placed demands upon economists to become more interventionist in policy debates. However the trend towards economist producing information suitable for economic planning was most pronounced during the New Deal. This certainly demonstrates congruencies with Porter's (1995) argument about the increase in public accountability in psychology prior to the widespread adoption of significance testing. Morgan and Rutherford associate the increase in these tasks with the increase in the use of simple mathematical models and statistical techniques. Their linkage is based upon the observation that these methods were well suited to the task, although they also note that other tools had been used in previous times without problem. Quite why, then, the expansion in the use of statistical techniques over these other techniques should be connected to the increases in interventionist politics is unclear. Regardless Morgan and Rutherford assert that the experiences of the New Deal did not improve the public trust in the economics profession as those involved shared in the failure of the interventionist approach at the time. Not until after World War II and their successes in planning the wartime economy did the interventionist economists receive public accolade for their work.

Morgan and Rutherford do stress that the type of mathematics associated with interventionism was not the sophisticated neoclassicalism of the American Keynesians who first accepted so enthusiastically the Phillips/Lipsey Phillips Curve. They are keen to separate out the historical precedents of the rise of mathematical techniques and neoclassical macroeconomics. Instead the mathematics they describe is microeconomic employing a 'tool kit' of simple models that can be applied in a wide range of situations. Interestingly they suggest the timing of the rise of neo-classicalism is puzzling, but venture to ascribe its popularity to the American Cold War context. Neo-classicalism is said to embody the values of the free market, and the Cold War provided the impetus to harden attitudes in that direction. However they do question the relevance of this to the European context. The impact of McCarthyism is forwarded, and they call upon another contribution to the edited collection by Perry Mehrling (1998) that suggests the use of mathematical representations help hide ideological positions. He makes comparisons between pre-war debates in monetary thought and their counterparts in the post war era. By locating the post-war debates in technical language, Mehrling argues, the authors can deflect attention away from the fact that the debates were actually about the appropriate role of money in American society. The technical language minimised the audience to the limited few with the social fluency to recognise this content. Morgan and Rutherford liken this account to the work of Porter (1995). The pressures placed upon Keynesian and other economists during McCarthyism are also discussed by Leeson (1997d) and Solberg and Tomilson (1997).

The application of Porter's (1995) ideas to the macroeconomics case and Morgan and Rutherford's (1998b) claims are not entirely incompatible, although they do stress different dynamics in the proliferation of statistical techniques. Morgan and Rutherford do note that during the Great Depression economists experienced a massive increase in their role in aiding interventionist policy. Furthermore they stress that through this, and the New Deal, American economics struggled for public support of the level attained in the pre-World War I era and the unity between pluralist economics and the Protestant church in the Social Gospel Movement (Bateman 1998). Although the Social Gospel Movement did reassert itself during the New Deal, the perceived failures of the interventionist policies dented any rise in public trust associated with it. This remained the case until the post-World War Two optimism and support for interventionist economics. Furthermore Morgan and Rutherford accept Porter's (1995) arguments on the benefits of ascribing objectivity to statistical methods in cost-benefit analysis, although they do not appear to use this as a causal force in their account.

We can further access these claims with reference to some empirical data for the pre-Phillips Curve debate era provided by Jeff Biddle (1999). Biddle studies the usage of statistics in journal articles by rank and file members of the economics profession from 1900 to 1950. He notes that previous research on the emergence of statistical and mathematical techniques in economics has always taken its focus as famous names and early pioneers of the field. Biddle lists Morgan (1990), Mirowski (1989b), Epstein (1987), Qin (1993), Wulwick (1992) and Klein (1997) as examples. Biddle stresses the importance of exploring the majority as opposed to the innovating minority, as only through these will the analyst be able to understand the actual position of mathematical techniques in the economics profession. Biddle is completely correct of course, since, being innovators, the famous names in the field were atypical individuals.

Biddle has three main conclusions. The first is that, as a proportion, the number of articles using mathematics over the fifty year period remained fairly constant. Biddle doubts that that is a controversial finding, but suggests his second conclusion does contradict some of the more established positions. The existing accounts of statistical history argue that the 1940s was a crucial decade when the work of Haavelmo (1944) and others started the 'probablistic revolution'. Morgan (1990) is a keen proponent of this position and argues Haavelmo's prescriptions quickly became the standard view. However, Biddle says there is no evidence of this before 1950, even among those already committed to statistics. Biddle's third conclusion is that a noticeable change occurred in the 1920s where economists shifted from using simple tables of statistics to calculating means, correlations and regressions. This started in the 1920s and shows a constant move towards the later up until the 1950s. However, it must be remembered that this was only a shift in the balance of the articles among the relatively unchanged proportion of texts employing statistics.

Biddle's second conclusion is the most interesting in our context. Haavelmo's (1944) argument, circulating as early as 1941, postulated that theories should be expressed as models that embodied hypotheses about the probabilistic relationship between random variables in a population. His sample of articles shows that there were a small number of elite innovators doing this, notably at the Cowles Commission, but the main research tools for the majority of the profession remained averages and frequency tabulations. Until 1950 the field was relatively un-touched by the probabilistic revolution.

Over the period 1900-1949 Biddle shows that more authors processed their figures instead of using raw data, and the techniques used became more complex. He notes a move away from a rhetorical style of justification towards an objective one presenting the results as the end product of a disinterested analysis, as in keeping with the suggestions in Morgan and Rutherford (1998b). As above, Biddle uses Porter's (1995) terminology characterise this, speaking of the replacement of a rhetoric of expert judgement with one of mechanical objectivity. Biddle also explores the Psychology example discussed above, and, as here, considers the impact of the relative strength of the economics discipline compared to Psychology in the adoption of mechanical objectivity. He cites Rucci and Tweney's (1980) similar study in Psychology that shows in the late 1930s 25% of all articles used probability based 'critical ratio' significance testing. This compares with almost no references to statistical significance in economics by 1949. Biddle demonstrates that economists had access to the information, and provides as examples the Survey articles published in the *Journal of the American Statistical Association* by Hotelling (1930) and Rider (1935). Morgan (1990) has provided a number of reasons why economists prior to the 1940s did not want to use such techniques. These are technical reasons based on the applicability of their data, including the aggregate nature of their data and the independence assumption. To Biddle, the late adoption of tests of statistical significance in economics relative to psychology is a product of the relative strength of the economics community, and the more complex regression techniques were held back after 1950 because of the expense of the computers required for the tasks.

We can add another issue to the explanation of the noticeable time delay between the distrust embodied by the Great Depression and the New Deal and the advent of institutions such as the LSE's Measurement and Methodology seminar group in the late 1950s. Here we concern ourselves with rigidities in the well documented flows of tacit knowledge in sciences (Collins 1974, 2001). The insights gained from the tacit knowledge literature cast Biddle's claim that economists had access to information on statistical testing through survey articles in a different light. Collins' work on the development of the TEA laser demonstrated that the transfer of knowledge needed more than written descriptions. For his respondents the creation of a working laser required close interaction with those who have already achieved successful results. The knowledge was more subtle than step-by-step instructions. The construction of a working laser required constant modification and judgments to the scientists work. Indeed, Chapter Five demonstrated that similar judgements were endemic in established statistical use in the Phillips debate. Furthermore this ignores the judgements necessary in operating the complex and slow computer technology used in their production. We can identify several rigidities to the flow of tacit knowledge at in this period.

First is the disparate geographical spread of macroeconomists. Unlike Collins' TEA laser groups, who were located in several localised pockets, macroeconomists were less formally grouped by specialism. This is perhaps a reflection of the relatively solitary nature of macroeconomic research, where research groups centred around a single topic were rare, as were grants to fund such activities. Subsequently the close quarters contact central to the transition of tacit knowledge is stifled. Instead interview data shows macroeconomists' reliance on two locations of tacit knowledge transition. The first is visiting seminar speakers addressing their department. This is clearly far from the movements of personnel and the frequent communications between the TEA laser groups, and in practice holds little significance compared to the second location. This is the direct interaction with the small number of institutions with a stock of statistical knowledge, one notable example already mentioned by Biddle (1999) and Morgan and Rutherford (1998b) is the Cowles Commission/Foundation, at Chicago and then Yale, although other examples can be found in the U.S., and in Europe including centres in Norway and the Netherlands, and Leeds and Cambridge in the U.K..

Many of the respondents who wrote on the Phillips/Lipsey Phillips Curve identified specific periods spent in such institutions which furnished their statistical understanding. Furthermore they noted this experience was rare amongst their colleges. Robert Solow spoke of his employment by Massachusetts Institute of Technology specifically because of his quantitative knowledge. In addition both James Tobin and Edmund Phelps were at the Cowles Foundation. In the U.K. context both Keith Cowling and Jim Taylor discussed how their years spent at the American Universities with links to the Cowles Commission/Foundation equipped them with the close quarter interaction necessary. Taylor spent a year at the University of Pennsylvania under Laurence Klein, an ex-Cowles Commission researcher and future Nobel Prize winner for contributions to statistical modelling in macroeconomics. On this, Jim Taylor:

Interviewer: "Did you learn Econometrics whilst you were in Pennsylvania?"

Jim Taylor: "I hadn't been heavily involved in computing or using computer programs at that time and I was influenced by a number of people in using statistical packages and in using techniques that I had not used before, basically auto-regressive techniques, I hadn't done very much stats or econometrics ... so I had to learn what was appropriate to use in certain circumstances. So by looking at papers that were currently being produced by people in this area I learnt that there were techniques. There was a package called TSP, a Time Series Package. This was when computers were just coming in in a big way, and I was carrying boxes of punch cards around with me. You had the program and you had the data so you carried boxes, you know literally boxes with the program that would actually set everything up before you actually run it on your data-set, and it could take hours to actually get your results out, especially if any of the cards got mashed up in the process. They often got damaged so you had to find the card that got damaged and replace it and so on. Anyway I started to use a computer in a big way in the USA and I was helped by other visitors, from Japan in particular, and I was helped by PhD students, in fact I had a PhD student allocated to me to help me ... with that and he was doing econometrics, and his economics was much better than mine. Anyway so I learnt from other people how to use the computer and how to use statistical packages and you just sort of learn by asking other people and by reading up manuals and looking at textbooks and so on"

Taylor's discussion notes the paucity of his own statistical teaching prior to attending Pennsylvania, and the transition of tacit knowledge through geographical relocation both in his own presence in the U.S. and visits made there by those from elsewhere. He also touches on the second rigidity in the uptake of statistical work in macroeconomics, the physical machines required to do it. Many respondents talked of the inadequacy of computers, both in terms of availability and scope. Each machine was large and slow, and could only complete one run at a time. The shortage of necessary skills and opportunities to use the computers instilled a debilitating friction in the adoption of quantitative methodologies. Subsequently the strained trust relations formed in the Great Depression could only be challenged by widespread standardised methodology in mainstream macroeconomics in the late 1950s and 1960s.

The adoption of quantitative knowledge was faster in the U.S. than the U.K.. Again we can turn to Porter's arguments of standardisation replacing trust relations to account for this. He compares the maintenance of trust relations in the U.S. and U.K. government. He notes that the U.S. government required greater levels of objectivity to attain sufficient public credibility compared to the UK. This is because the U.S. government had weaker trust relations due to a greater geographical spread covered by these networks and a bigger recruitment pool of staff compared to the tight knit community drawn from the British public school system. These arguments translate well to U.S. and U.K. macroeconomic circles and are given extra weight in the light of Lipsey's trust networks quote in Chapter Seven. The greater geographic and demographic dimensions of the U.S. macroeconomic profession hastened the weakening of existing trust relationships.

Weintraub (2002), as discussed at length in Chapter Two, prioritises the role of the changing visions of mathematics by mathematicians and economists in the adoption of the varying forms of statistical and mathematical techniques. He associates the movements between the competing visions of mathematics with shifting constructions of mathematical rigour. He argues that on way rigour was ascribed to mathematics was through empirically testable hypothesis by mathematicians such as Votterra and Edgeworth at the beginning of the twentieth century, and again with the emergence of econometrics in wider macroeconomics in the period we are discussing here. An alternative vision of mathematical rigour was provided by formalists such as Debreu and von Newmann, which privileged the consistency of an axiomatic base for economic modelling. These shifting notions of rigour can be associated with the changing constructions of objectivity discussed by Morgan and Rutherford (1998b).

We have seen that the drive to quantitative methodology was spurred by the pursuit of stable trust relations in macroeconomics. The increased spotlight endured by the profession in the post depression era undermined existing trust relationships and ignited public questioning of the value neutral construction of self amongst macroeconomists. The standardised methodologies of statistics embodied trust and value neutrality through shifting the burden of analysis from the researcher to the technique, and the rigour of testable hypotheses. As such quantitative methodologies would be adopted widely in macroeconomics, but only once the obstacles of shared tacit knowledge and computer proliferation had been overcome.

As well as the appropriation of quantitative techniques, the value neutral construction of self had another profound impact upon the actions of macroeconomists in the Phillips Curve debate. This is treated in the main in the following chapter, however we can take the opportunity to introduce it here, the dislocation of macroeconomists from the policy setting arena.

9.3.3 Value Neutrality and Macroeconomists' Dislocation from Agenda Setting in Policy Making Circles

We have shown that macroeconomists consider it their role to not engage in normative political action through their work. We have demonstrated that the form this takes is available for negotiation, and is heterogeneous between respondents. This and the previous chapter have shown some try to effect the political agenda by researching issues they consider neglected in policy circles, for example Perry and Laidler. However the contention here is that, even with incidence of this, macroeconomists still act in accordance with the classical representation of value neutrality by removing themselves from agenda setting in the policy making arena. Institutionally the dislocation is very clear. The majority of research-active macroeconomists worked outside government circles. Those who did have opportunities to interact with policy makers retained university positions. Furthermore even within this capacity they remained as advisors on policy as opposed to making policy choices. Autonomy over the agenda of policy goals remained in the political sphere set by government and wider social influence. This is especially apparent during periods of economic crisis. As much as a macroeconomist may wish to argue the government should be prioritising the needs of the unemployed, or international trade, or central bank autonomy, their arguments would have little impact in the face of contrary pressure from public and political voices. Decisions on these issues remained within the political apparatus of governments and elections. The full ramifications of this claim, and the associated discussion of related literature, will be discussed in the next chapter.

9.4.1 Understanding the Macroeconomists' Construction of Self as Value Neutral

This chapter has shown that macroeconomists have a commitment to a construction of self as value neutral. This construction is organised around a distinction between normative and positive work and denies the macroeconomist a role in policy making decisions. In the name of this construction macroeconomists engaged in a set of actions that did have impacts upon macroeconomic debate. However the previous four chapters have demonstrated that the attainment of objective research was not one of them. Despite this these actions were instrumental in the workings of the cycle of contest and closure in macroeconomics.

We employed Goffman's (1956) frontstage/backstage distinction and Gilbert and Mulkay's (1984) empiricist and contingent repertoires to elucidate the operation of the value neutral construction of self. The public frontstage performance is of a pure and successful value neutrality couched in the terminology of the empiricists repertoire. The backstage, conversely, witnesses subtle negotiations of the inherent normative element in any cultural activity. Here the role of standardised mathematical technique is apparent as the universality of the methodology is entrusted with eradicating any normative element. We discussed Porter's arguments in relation to this analysis. The argument was made that the Great Depression and the New Deal generated a distrust of macroeconomics that encouraged the adoption of statistical testing. This coincided with the demands placed upon economists to use basic mathematical techniques in guiding interventionist government policy. However rigidities in the spread of tacit knowledge and the availability of computer technology forced a lag in the eventual wide spread adoption of complex statistical techniques.

Ezrahi's (1990) work on the legitimacy of liberal democratic politics enabled an explication of why the frontstage representation existed as it did. At its inception the new political culture sought legitimacy for the distribution of power and rules it embodied through the externally rationalised knowledge of scientific discourses. Their employment depersonalised the decision making processes. For this to work the sciences had to exemplify the value neutral position. This was further pronounced in macroeconomics as the linkages between the subject matter and the political process were particularly obvious. By pursuing value neutrality in various forms macroeconomists were able to comply with their legitimising role.

9.4.2 How the Macroeconomists' Construction of Self as Value Neutral Relates to the Rest of the Thesis

There remains only one analytical chapter developing new arguments for this thesis. That chapter, and this one, perform stages two and three of EPOR. The next chapter assumes the dislocation of macroeconomists from the policy debate discussed here as its starting point. It will locate this within a Kuhnian framework of problem solving to make explicit the process that instigate shifts in the cycle of contest and closure. This is followed by a concluding chapter drawing together the insights from this and the previous chapters on Interpretative Flexibility in macroeconomics to give a complete depiction of how all these processes operating together produce the fast paced cycles of contest and closure witnessed in the Phillips Curve debate.

10. Macroeconomics as a Problem Solving Discipline

10.1.1 Introduction

This is the chapter that finally bears the burden of answering the central research question of the thesis as set out in the introduction. Here an explanation of why macroeconomics experienced changes of orthodoxy at a frequency far above that witnessed in the majority of other sciences is forwarded. This is not to say the preceding five chapters shed no light on this issue. Indeed they are all essential building blocks in the analysis explicated in this chapter, and have completed the work of the first stage of the Empirical Programme of Relativism (EPOR) (Collins, 1981a, 1983a, 1992) and made contributions towards the second and third. However Chapters Five to Eight explored cultural dynamics that lent fluidity to the cycle of contest and closure, while remaining quiet on the driving force that instigates these changes. The beginnings of the explanation was developed in Chapter Nine. This chapter applies the framework provided by Thomas Kuhn (1970) to develop a theoretical account of this cycle of contest and closure. The account is consistent with the data presented in this thesis. However it also draws upon wider theoretical frameworks demonstrated empirically in other sciences by the Social Studies of Science literature and contributions from the History of Economic Thought literature to lend support to the account in this case. Some of the claims may not be fully substantiated by the limited dataset collected for this thesis. However they do not contradict the data, and supportive evidence is provided from the existing literature.

Despite this, the account presented in this chapter remains a theoretical exercise in producing an account of how wider socio-cultural forces fuel the cycle of contest and closure in macroeconomics. This is the prescription of the third stage of the EPOR. Like any argument, further research can be conducted to further add veracity to the account. The chapter argues that the engine powering the cycle of contest and closure in macroeconomics is the interaction between the immediacy of macroeconomic policy needs, macroeconomics' role as a problem solving discipline, and the macroeconomists' construction of self as value neutral. This chapter will trace a history of the Phillips Curve debate demonstrating how, at each cycle of contest and closure, these three issues set in motion the Interpretative Flexibilities discussed in Chapters Five to Seven and initiated the birth of a new macroeconomic orthodoxy.

To start this chapter we will discuss those elements of the classic work by Thomas Kuhn on science as a problem solving enterprise. However this will be kept as brief as it possible to allow the main explanatory weight be taken by the subsequent application of these ideas to the Phillips Curve debate.

10.1.2 Kuhn's Problem Solving Sciences

Kuhn famously argued that competing paradigms were incommensurable to the extent of being beyond comparative evaluation with a single criterion (Kuhn 1970). However, his meanings for paradigm and the form adopted by incommensurability were varied and open to interpretation. (Doppelt 1978, Masterman 1970). In the later empirical section of this chapter the specific characteristics of macroeconomic paradigms are developed. The two mechanisms of incommensurability useful in this context, as identified by Doppelt, are firstly the agenda of problems to be solved and secondly the criteria of adequacy for scientific explanation. The relationship between these is the focus of this chapter.

Kuhn argues that rival paradigms frequently provide differing explanations for a phenomenon because they are orientated towards achieving different goals. Furthermore they may not even seek to explain the same phenomenon. Different problems provide different sets of criteria with which successful science is judged.

The two cannot be disentangled, because the measurement of good science becomes the achievement of a positive step towards solving the accepted problem. To illustrate the pivotal dynamics of this mechanism we shall briefly discuss one of Kuhn's examples, the one he describes as possibly the fullest example of a scientific revolution, the shift from a chemistry of mutual affinity to one of fixed proportions. A comparison between this and macroeconomics will prove fertile ground for explaining the cycle of contest and closure.

In the 18th to 19th century chemists believed elementary atoms were held together by mutual affinity. This construction proved useful in exploring many interests in the natural world, and affinity theory was widely employed to investigate the concerns of the day. However, affinity theory began to lose its acceptance as new concerns and avenues of investigation arose. Affinity theory distinguished between physical mixtures and chemical compounds. In practice this distinction left a large range of intermediate cases that did not easily fit the definition, but by convention were considered compounds. However, this definitional awkwardness did not prevent chemists at the time accepting and using the theory, since they were not overwhelmingly concerned with the intermediate range. This remained the case until John Dalton began to employ chemistry to solve problems traditionally associated with meteorology.

Dalton's interest lay in the absorption of water by the atmosphere and of gases by water. Such phenomena are in the range of intermediate cases. This limited affinity theory's use for Dalton's research because they were awkwardly classified as compounds in the absence of a better category. Dalton subsequently developed a framework in which atoms were related to each other in whole number ratios. Upon publication Dalton's work was considered maverick and widely attacked. However over time the law of fixed proportions proved fruitful over a range of uses, and, eventually, became widely accepted. Kuhn notes that it certainly were not the data alone that lead to the rejection of affinity theory because, although many of experimental reactions did comply with the whole number ratio construction, many others did not. Kuhn argues this provided the task of normal science for the next generation of chemists, the task of beating nature into the shape expected by Dalton's fixed proportion theory.

Dalton's criterion of scientific adequacy was the explanation of the absorption of gases by water. Let us compare this to the agenda of Berthollet, a French chemist who supported affinity theory. Berthollet would also have had a specific research topic to explore. However part of his criterion of scientific adequacy for any topic was that the findings conformed to the laws dictated by affinity theory. If a finding did not conform to this then the experiment would be deemed a failure. This plunged chemistry into a controversy not about whether affinity theory or fixed proportions theory were correct, but about which set of problems, and thus criteria of adequacy, were most worthy of exploration. The central issue for our analysis is that this negotiation occurred between chemists for a long time before Dalton's theory became dominant.

10.1.3 Kuhnian Dynamics in Macroeconomics

Kuhn's explanation can account for many of the dynamics witnessed in the Phillips Curve debate. However, there is one central difference between the experience of 19th century chemistry and 20th century macroeconomics, and this is the main focus of this chapter. In the chemistry case it is the scientists themselves who develop, negotiate, and maintain both the problems for, and the standards of, adequacy for science. In contrast, macroeconomists, acting as value neutral and distant from policy making decisions, have dislocated themselves from the mechanisms where the problems their profession address are derived. It is not macroeconomists, but those in policy making positions, who define the problems and subsequently the criteria of adequacy for macroeconomics. This does not imply that macroeconomists are unaffected by policy making circles. Indeed this chapter argues they are very closely connected since it drives their agenda. Simply that they do not have a role in the agenda setting mechanisms of the policy circles during the periods of political crisis and macroeconomic paradigm shifts in the Phillips Curve debate.

Policy making circles have the autonomy to set the problems addressed because of the macroeconomists' construction of self discussed in the previous chapter. Here we saw macroeconomists distancing themselves from the policy setting arena in the pursuit of objectivity. Clearly the previous chapters demonstrated that this attempt does not make them value neutral in any real sense. Nuances in the application of value neutrality necessary to negotiate inherent social interests do allow some acknowledged political influence in their work. However the flexibility introduced here is insufficient to counter the force of problems identified in the policy setting arena.

In the chemistry case Kuhn argued Berthollet and Dalton engaged in a contest over which set of problems were the most important for their science to address. This debate took time, but could only occur because the two men and their discipline had the autonomy over their goals and so could defend their positions. By dislocating themselves from agenda setting in the policy making arena macroeconomists have sacrificed this autonomy. The argument of this thesis is that in macroeconomics there is no opportunity for a prolonged period of contestations over the correct goals, because macroeconomists do not set their own goals. Once the policy circle has identified a new macroeconomic concern the decision is made. Macroeconomists and macroeconomics must address it or become irrelevant. It is during periods of economic crisis that policy circles shift the goals of macroeconomics with sufficient pace to deny macroeconomists the opportunity to successfully contest the change.

The policy making circle operates on a faster time scale than is expected for the sciences. Whether the length of a period of accountability for government economic policy is measured by the four to five yearly election or the day by day public scrutiny of journalists, the time frame is short. Policy goals exact an immediacy upon their achievement not witnessed in the majority of sciences. Macroeconomics, however, through allowing others to set the research agenda has shackled itself to a higher frequency turnover of problems than many other sciences. Again, without the ability to defend the relevance of their existing problems and their associated criteria of adequacy, macroeconomists have to reinvent their ideas at regular intervals to remain convincing to those around them.

Let us now explore these dynamics using the data, starting with the Phillips/Lipsey Phillips Curve.

10.2.1 The Phillips/Lipsey Phillips Curve Cycle of Contest and Closure as a Problem Solving Phenomenon

As part of the Keynesian synthesis the Phillips/Lipsey Phillips Curve is a response to the problems of the Great Depression. The political crisis caused by the soaring unemployment demanded economists provide an explanation and a set of policy tools to elevate the situation. There was a perception among some that the pre-Keynesian economics could not provide such an explanation or policy tools, as exemplified in the following quote from James Tobin:

> James Tobin: "Well what was special about [Keynes'] General Theory was that it had an explanation that ordinary economics did not have for the depression, and it also had a solution that ordinary economics didn't have either, so that was a real eye opener for many people of my generation, so it looked to us as if the Classical orthodox economic theory [of] Marshall and [others], however good it was for other things, it had no macroeconomic truth to it at all, so it had no explanation of the question [of the Great Depression], you know, [a] convincing one"
>
> Interviewer: "It didn't address the issues of the day?"
>
> James Tobin: "It did not address the issues of the day and it was continuing with being completely blind to the problems of demand, as if we were still were still back in the [Nineteen] Twenties"

There are two issues to be noted here. Firstly Keynes' explanation of the Great Depression is not the only imaginable explanation. U.S. deflationary monetary policy (Friedman and Schwartz 1963), changes in consumption demand (Temin 1976) and a lack of world leadership on protectionism (Kindleberger 1973) have all been offered as explanations in later years. Indeed, Blaug (1991) asserts pre-Keynesian theory could easily account for the unemployment with reference to market imperfections, for example overvalued currencies and real wages being held above the market-clearing level. Secondly the Keynesian construction was unable to account for a number of alternative macroeconomic phenomena. However despite this Keynesian macroeconomics was speedily adopted by a great number of economists.

Morgan and Rutherford (1998b) argue that the "Great Depression demanded that economists take up the challenge of diagnosing and treating the illness of the economy" (Morgan and Rutherford 1998b p. 11). However they continue to argue that it was not until the New Deal, and the priority on interventionist economics, that this demand dramatically widened. The data collected in this research makes few comments on the New Deal. However Morgan and Rutherford's position is compatible with the account presented here. In the quotation above they argue the Great Depression caused the demand for economic work. This is not exactly the dynamic described here. Instead it is argued that the political decision to tackle the Great Depression instigated the demand. It could be argued that the Great Depression, and the associated pressures placed upon governments, necessitated this political decision. This may well be the true. However it remains the case that, in the account presented here, it is the political decision, not the depression it responds to, that shifted the goals of economics. The importance of the political decision is clearly pronounced in Morgan and Rutherford's identification of the New Deal as central to increasing demand for economists. The new era of policy making exacted new demand upon economics, and with it an associated new set of criteria for judging success. The details of the New Deal will be discussed below with reference to a contribution by Barber (1990).

The inability of the pluralist range of pre-Keynesian economic theories to account for the events of the Great Depression was not identified by the economists of the day themselves as the problem economics needed to tackle in the same way Dalton considered the explanation of the absorption of fluid by gases. The interest in the failings of economics was not internally created and promoted. Instead its identification came with the advent of rising unemployment and the political decision to tackle it. That is not to say economists were blind to the problems until policy makers made new demands of the profession, nor that macroeconomists did not feel the need to prevent rapidly escalating unemployment. Instead, in a time where policy makers were desperate for some credible and socially legitimate explanation of the social upheaval, it was the macroeconomists' perceived role as value neutral advisors to provide this explanation.

There are two reasons why this situation created the swift rejection of earlier theory and the equally rapid adoption of Keynesian ideas. Firstly the macroeconomists themselves could not contest whether or not rising unemployment should be the problem to which their science should be orientated, as is the case in most sciences. Whereas in many cases it would be reasonable to assume that each scientist's vested interests in their work would inject a rigidity and subsequent attempt to maintain the current position of the science, the vested interest in macroeconomics had little authority compared to the demands placed upon the profession by policy makers. Subsequently time spent in other sciences disputing the right and correct goals is circumvented in macroeconomics. Secondly there is little willingness to do the repair work to maintain the existing order. The equivalent to this in the Dalton case involved finding ways and explanations that made existing readings of the percentage relationships between particles fit the Dalton's fixed proportion theory. This is what Kuhn calls the generation long task of beating nature into shape, the work of normal science. The pre-Keynesian economists, of course, were not afforded the luxury of a generation to beat the observed facts of the Great Depression into their existing mould. Even though, as noted above by Blaug (1991), they could provide explanations based upon market rigidities, such explanations were not deemed sufficient to restore political legitimacy to the crisis hit liberal democratic structures of governance. Indeed it was the onset of political crisis that prevented these ideas from continuing to represent socially legitimate abstracted knowledge upon which the legitimacy of liberal democracy could be established. Given the Interpretative Flexibility of economic arguments described in Chapters Five, Six and Seven, the pre-Keynesian economists surely could have provided such suitable explanations given time. However this time was not available because they lacked the autonomy to set their own problems and subsequently the standards by which their work was judged. Instead the economic policy sector demanded an immediacy of response, a call to which Keynes responded most effectively.

There are two technical remarks requiring clarification in the closing sentences of the previous paragraph. The first regards the Interpretative Flexibility of economic arguments. It is an in-principle argument that Interpretative Flexibility would have allowed pre-Keynesian economists to reconfigure their arguments to account for a any set of phenomena. However it does not follow that they necessarily would have attained the social position for them to be used to once again represent the scientific model that provides liberal democracies with their legitimacy, as argued by Ezrahi (1990). That is a result of social negotiations beyond the Interpretative Flexibility of economic knowledge. Furthermore, although such Interpretative Flexibility would exist, it is unlikely that it would have been actualised through the process detailed in Chapters Five, Six and Seven. This is because there were numerous different methods used in economics in this period (Morgan and Rutherford 1998a), and associated with them would have been different constructions of 'rigour' (Weintraub 2002). Subsequently the ways in which Interpretative Flexibility would be manifest would also be different. The secondly issue of clarification regards the claim that Keynes responded most efficiently to the new demands of the policy circle. It should be made clear that this efficiency is a social efficiency and not a comment upon the content of his economics. The latter would be against the tenets of methodological relativism (Collins 1981a). We can know that Keynes responded with the most social efficiency because it was his arguments that attained the social position of orthodoxy.

Additional literature from the History of Economic Thought lends further support to the argument that it is crisis that leads change in economic policy and the acceptance of wider theories. This can be found in both the American and British contexts for the period of the increasing acceptance of the Keynesian orthodoxy during the Great Depression. We will start with two articles on examples from the United States, through the presidencies of both Franklin D Roosevelt and Harry S Truman.

William J Barber (1990) considers the Roosevelt years of 1933-45 where ideas in the style of Keynes' first entered the American policy arena. In the first three years of his administration Roosevelt listened to two main schools of macroeconomic thought as presented by Rexford Guy Tugwell on one side and George F Warren and Frank Pearson on the other. Tugwell advocated the end of laissez-faire to be replaced by centrally planned, but not publicly owned, forces of economies of scale. Warren and Pearson's form of monetarism looked to raise the general level of prices by increasing the money supply. An example policy of this type is the 'gold purchase program' of 1933. These policies were judged at the time to be working until the recession of 1937-38, the first recession since a limited recovery following the Great Depression. This recession occurred while the economy was still far short of full employment, an outcome unfitting with the conceptions of either Tugwell or Warren and Pearson. To Barber this economic crisis was the turning point in the fiscal revolution that precipitated the adoption of Keynesian ideas. However, interestingly, Barber argues the initial push in this direction came from within government departments, where, with little influence from Keynes' writings, officials promoted the policy of strong deficit spending to enhance purchasing power. This policy is congruent with Keynes' logic, but was developed independently, and its successful implementation in 1938 was crucial in gaining support for Keynes General Theory. In the context of the research presented here the importance of this is the role of economic crisis in turning the wheels of macroeconomic theoretical development, and enlightening the growth of the Keynesian synthesis following the Great Depression.

Robert M Collins (1990) uses a similar analysis to elucidate the development of theories of 'Growthmanship' by the next President, Harry S. Truman. Collins explains how the crisis created by financing World War Two shifted the goals of policy from the concerns of managing economic scarcity as embodied by the New Deal's National Recovery Administration and Agricultural Adjustment Administration to those of promoting economic expansion. The push towards economic expansion was most keenly advocated to Truman by Leon Keyserling, a career bureaucrat who lived isolated from and resentful of academic macroeconomic circles (Goodwin 2001, Brazelton 2001). The pursuit of 'Growthmanship' did lead to policies promoting industrial expansion and the establishment of the Council of Economic Advisors. However, the goals of policy changed again with the declaration of the Cold War & the Korean War in 1950 and expansion lost favour. Collins' conclusion reflects on the relationship between economic policy and macroeconomic theory, arguing that in this case neither led the other throughout the period. Nevertheless it was the policy sphere that took the crucial first step in response to the crisis of funding World War Two and thus framed the economic policy agenda to which economists of all disciplinary persuasions were thus orienting their work. This fits well with the argument presented here.

A further two papers illuminate the same dynamic in the British context. Firstly Peter Clarke (1990) discusses the abandonment of the Gold Standard and free trade. He explains that the British Treasury of the 1920s held firm to the belief that their role should be kept at a minimum as the economy was best left to monitor itself. This concept was expressed most succinctly by the Gold Standard which adjusted smoothly between equilibrium positions and caused little incentive for concern. This was until the 1920s when the adjustment process broke down. Initially a discourse of wage inflexibility in the British economy was mobilised to account for the discrepancies. However as the world slump gained a tighter hold on the British economy, and British exports would not sell at any price, this argument faltered and the equilibrium economics of the Gold Standard would be rejected for a disequilibrium analysis closer to that of Keynes. Again it was economic and political crisis that led the changes in macroeconomic theory.

The second paper by Barry Supple (1990) also considers the withdrawal of free trade and the instigation of protectionism in the British economy, although in this instance the analysis traces the arguments as far back as 1880. Supple is keen to demonstrate the number of anti-laissez-faire voices that were ignored prior to the on-set of depression in the 1930s that inspired the re-evaluation discussed above by Collins. As examples he cites non-governmental voices in the 1870s, the Tariff Reform Movement led by Joseph Chamberlain in 1903, and the government instigated 1929 Balfour Committee, all which questioned the sense of free-trade but were ignored in policy making decisions. Ultimately Supple argues it was the collapse of exports in the coal, iron, steel, shipbuilding and cotton industries that broke the hold of classical macroeconomics and led to the abandonment of free trade in the early 1930s. The essential point is that even in the face of pro-protectionist voices it was not until the government was faced with economic crisis that their goals and the dominant economic doctrines supporting them shifted.

Both the original analysis presented here and the discussion of literature above are of course only the story of the rise of Keynesianism. The Phillips/Lipsey Phillips Curve itself would not appear for the best part of twenty years, and in a very different political context. The late 1950s and early 1960s, when the Phillips/Lipsey Phillips Curve rose to prominence, is accepted as a time of strong influence for macroeconomists in economic policy circles (Backhouse 1985, Bleaney 1985). Indeed in the American context Barber (1989) argues the strongest links between academic economists and policy makers were during the Kennedy administration in

1960-63. As further evidence, Stein (1996) identifies the 1964 Revenue Act, popularly known as the Kennedy-Johnson Tax Cut, as the greatest ever success of the Council of Economic Advisors (CEA). However, Stein also adds ambiguity to the situation by highlighting President Johnson's 1965 refusal to follow the Council's advice on funding the Vietnam War. Schultze's (1996) account of the CEA also supports this position.

This strong relationship does not in anyway undermine the argument made in this thesis, as the development of the Phillips/Lipsey Phillips Curve can be considered an act of Keynesian normal science. As addressed in Chapter Eight the Phillips/Lipsey Phillips Curve solved a perceived theoretical problem in the Keynesian synthesis regarding the flexibility of wages. Initially highlighted by Franco Modigliani's 1944 PhD thesis, Keynes' rejection of Say's Law only held if money wages were assumed fixed (Modigliani 1944). If money wages were assumed to be flexible, then Say's predictions of prices returning to an equilibrium level would be realised in Keynes' theory. It was accepted by many Keynesians at the time that the Phillips/Lipsey Phillips Curve finally solved this contradiction.

However, there is another way in which the development of the Phillips/Lipsey Phillips Curve can be considered apart from its role as an act of normal science. This version is entirely supportive of the argument for the strong relationship between macroeconomics and the policy circle in this period. In this instance the policy problem solving mechanism is observable, however in this case the problems set are less radical, as are any changes in the criteria of adequacy. Subsequently the Keynesian orthodoxy is able to address the issue in suitable time to maintain the legitimacy of the policy making sector, demonstrating the strength of the policy/academy relationship. A quote from Robert Solow highlights this.

> Robert Solow: "One of the reasons I was interested in the Phillips Curve ... in the American context was that in the middle fifties ... there was an intense discussion of inflation in the U.S. In the very minor recession in 1954 and still fairly minor but somewhat deeper recession in 1957, counter to everybody's expectations, the price indexes not only did not fall but actually continued to rise. It had been previous experience that in recessions the price level fell; yet here were two recessions in a row in the US where the price level didn't fall, in fact [the price level] rose, ... That phenomenon was called creeping inflation

> … There was a lot of interest in it and I was interested in it too, and one wanted to understand why that had occurred and the [Phillips/Lipsey] Phillips Curve offered a way of getting an answer"

The Phillips/Lipsey Phillips Curve provided an explanation of the creeping inflation experienced in the two preceding recessions. As a political problem the economic fallout of creeping inflation was insufficient to reconfigure the questions asked of macroeconomics, and Keynesian macroeconomists thus had enough time to perform the normal science of reconstructing their understanding to account for it. The newfound dominance of the Phillips/Lipsey Phillips Curve over the previous inflationary gap explanation could occur without breaking down the existing wider Keynesian frame work, or the strong relationship between economic policy and macroeconomists. With the criteria of adequacy not profoundly changed, and the space to carry out the necessary repair work maintained, many of the theoretical axioms trusted since the Nineteen Forties remained.

Indeed, the problem solving account can tentatively be forwarded to further our understanding of why macroeconomists enjoyed a strong relationship with the policy making circles in this period. As Morgan and Rutherford (1998b) and Barber (1990) have argued, economists were brought closer to the policy making circles during the fallout of the Great Depression and the New Deal to help provide solutions to the economic crisis. In Barber's account it is clear that the move towards Keynesian style analysis originated within government departments, independently from Keynes' influence. Leeson (1997d) argues in the nineteen sixties the Keynesian arguments were used to justify and implement Kennedy's 'New Economics' of big government interventionist economic policy. Bleaney (1985) also contends that some Keynesians became close advisors to Kennedy, although he questions the extent to which his policy decisions were actually guided by these principles. The are two things to note about these cases. Firstly, in both of these eras, the policy circle was sympathetic to the views of economists. The problem solving account would suggest that this is because the policy circles agenda demanded this input from the economists. The autonomy to make the suggestions they did was not socially negotiated by a strong economics, but granted by an expectant policy circle. Only those economists who were promoting ideas that had congruence with the political agenda were entertained.

It is not the case, for example, that Milton Friedman was granted a strong relationship with the Kennedy administration. This is because his arguments were contrary to the policy goals identified by the policy circle. The second point to note is that the Kennedy-Johnson presidencies did not experience political crises equivalent to the Great Depression or the 1970s stagflation. This allowed macroeconomics to settle into the short period of normal science during which the Phillips/Lipsey Phillips Curve was established. Should there have been such a political crisis in this period, it is doubtful that the economists would have maintained their strong position in relation to the policy making circle. The macroeconomists were only able to establish this relationship because it was in the interests of the policy making circles, as was reflected in the currently operable set of macroeconomic problems and their associated criteria of adequacy.

Another issue, suggested by our reading of Bleaney (1985), requires attention. As mentioned above, he argues that Kennedy's economic policy often did not actually follow Keynesian prescriptions, even though many of his advisors were Keynesians. Whether this is the case or not is not an issue for this thesis. Furthermore the truth of Bleaney's claim is also not of consequence for the problem solving account. The reason for this stresses an important dynamic in the argument presented here. Whether an economic orthodoxy does actually address the problems set by policy circles, or whether their prescriptions are adopted by policy circles, is not important to the argument. Firstly, to engage in such discussions would break the principle of methodological relativism (Collins 1981a) guiding this research, as it would necessitate dealing with the 'truth' of the economic claims. Moreover, the actual use by policy circles of the economic arguments has no impact upon the ability of economics, as part of a wider network of sciences, to lend legitimacy to liberal democratic political structures. As Ezrahi (1990) argues, liberal democracies appeal to the authority of science to justify their rules and power structures, just as monarchist systems of power appealed to religion. The essential characteristic for achieving this is the perceived embodiment of depersonalised knowledge, as represented by the scientific method in it various constructions, and the commitment to value neutrality. The truth or falsity of the knowledge claims, and the particular construction of scientific method adopted, are irrelevant so long as they do not inhibit the public trust invested in the systems of science as depersonalised knowledge. It is argued here that this trust is being questioned in the moments of political crisis that have lead paradigm shifts in economics.

As a final comment on this era we turn to George Zis, commenting on the decreasing prominence of the advocates of Trade Union militancy variables in the Phillips/Lipsey Phillips Curve:

> George Zis: "It was in a sense coming towards the end of the debate, certainly the case for incomes policies had become very weak, and of course with the Labour party in the mid Seventies saying now inflation is our major target, there was a complete shift on how we are going to control inflation and once you have moved onto how you control inflation, given that incomes policies have not worked, policies that aim to ease inflation arising from social conflict just went by the by, so you started thinking about international co-operation"

Zis' claims the government adoption of a position on inflation had a role in marginalising the Trade Union militancy variable advocates. This is because the policy implications espoused by Trade Union theories were no longer in the gaze of the policy circle. The irrelevance for policy created the irrelevance for macroeconomics. They were failing to address the problems their discipline had been (re)orientated towards.

We now turn to the second incarnation of the Phillips Curve, the Friedman/Phelps Phillips Curve.

10.2.2 The Friedman/Phelps Phillips Curve Cycle of Contest and Closure as a Problem Solving Phenomenon

Signs of stagflation began in the late Nineteen Sixties, but by the early Nineteen Seventies they proved economically crippling. Stagflation describes the simultaneous rapid increase in inflation and unemployment, the direct opposite of the simple predictions of the Phillips/Lipsey Phillips Curve. However the argument here is not that it is the perceived logical opposition between the Phillips/Lipsey Phillips Curve and the observed phenomena that cemented its demise. We should note that, for almost twenty years, the Keynesian orthodoxy survived and prospered with the perception of the more profoundly logically crippling contradiction of the wage flexibility issue that the Phillips/Lipsey Phillips Curve eventually solved. Instead the Keynesian orthodoxy could not survive the sudden shift of problem and criteria of success placed upon it by economic policy makers. The immediacy of policy needs and macroeconomist's loss of autonomy to set the problems of their field implied that Keynesian macroeconomists were denied, firstly, the right to contest the change in problems set, and, secondly, carry out the repair work necessary to maintain the synthesis. Instead policy needed a quicker response, and separately Milton Friedman and Edmund Phelps were able to provide it.

We should note that it is possible to account for the observed stagflation of the 1970s within a Keynesian framework. Some macroeconomists, such as James Tobin and Robert Solow, provided such accounts, usually involving references to the one-off shock of the OPEC oil price rises, and remained convinced of the relevance of the theories they worked to develop ten years previously. However these macroeconomists and their colleagues became marginalised in the profession as their ideas became less popular. These dynamics are discussed in this account from James Tobin:

> James Tobin: "In the United States the policies were pretty Keynesian, there was still this undercurrent, Friedman and so on fighting against it and they did win so to speak, because there was the OPEC and inflation in the seventies and people who were more on the traditional Keynesian side thought that was explained by the OPEC countries and what they did to their monopoly of oil, but according to the Friedman side, monetarism and so on, that's not true, it was all because of bad economic policy, bad monetary policy that was based on this illusion that the [Phillips/Lipsey] Phillips Curve was a reliable concept that you could use in making policy"
>
> Interviewer: "So given the fact that there were two plausible explanations, OPEC or that the [Phillips/Lipsey] Phillips Curve was unreliable, why do you think people decided to agree with the Friedmanite prescription?"
>
> James Tobin: "Well I think because there was a revolution against having so much inflation all the time, and the accusation that the Keynesians caused the inflation, and people who believed in the [Phillips/Lipsey] Phillips Curve

> caused the inflation, was credible to a lot of people, now of course there is a self interest of business and industry in having less intervention into the economy, but why they want less macroeconomic intervention, a difference in monetary policy is not so clear, so it's a good question, I don't know"
>
> Interviewer: "So if you were somebody who might buy into the idea that you were saying, that Seventies stagflation was due to the failure of the [Phillips/Lipsey] Phillips Curve, wouldn't that just make people against certain uses of Keynesianism in developing economic policy as opposed to giving up on Keynesianism as a form of science?"
>
> James Tobin: "No I don't think that distinction would be so clear in the political realm"

In this discussion Tobin identifies several mechanisms that conform to our mode of explanation. Firstly he argues that monetarism was no more than an undercurrent during the Keynesian dominance. He also describes both the monetarist and Keynesian standing on stagflation, demonstrating the existence of a coherent Keynesian explanation. Furthermore he identifies the strength of political will to address the issue of stagflation. At this point he also introduces a discussion of the perceived vested interests of business in the outcome of the debate, a point that in the light of the earlier discussion of Political Interpretative Flexibility can be understood as a contingent, yet influential, political construction bolstering the political will. Finally, and importantly, he notes that in policy circles the distinction between rejecting a single element of the Phillips/Lipsey Phillips Curve and rejecting the entire Keynesian synthesis was not apparent.

This, of course, is only Tobin's own position within the debate. With the benefit of symmetrical analysis it is possible to challenge his last point. It is not the case that the political realm per se is unable to make the distinction between rejecting a section of, as opposed to the entire, Keynesian synthesis. This is exactly what occurred in the case of creeping stagflation in the 1950s. Instead, the heightened situation of a shift in the problems faced by economic policy circles, combined with the immediacy of their needs, overrides the possibility for the repair work necessary to the Keynesian synthesis during this period of political crisis. As with creeping inflation, when the problems are insufficient to radically alter the problems macroeconomics faces, the distinction between a section and the whole synthesis is available to policy makers.

This is the case when the overarching theory can, or at least should, be capable of addressing the problems within a culturally acceptable time frame. The intense political need for an account of the stagflation did not allow the time to do this or provide for the distinction between a part and the whole of the Keynesian synthesis.

10.2.3 Reflections on Macroeconomic Paradigms

We can comment on the form adopted by macroeconomic paradigm shifts. One interesting facet of the data is that paradigms in macroeconomics are never totally dispelled from the profession. Instead they become marginalised often to return at a later time. The Phillips/Lipsey Phillips Curve is a case in point. A number of its architects, Lipsey, Solow, Tobin and Perry, are still committed to it. While these writers, as Keynesians, remained on the periphery through the Neo-Classical dominance of the Nineteen Seventies, they did indeed experience a re-emergence in the late Nineteen Eighties. This is associated with writers including Gregory Mankiw and David Romer, although, as James Tobin explains, their Keynesianism is different from that of the past:

> James Tobin: "Well theory changes with experience and with the work of theorists, so-called Keynesian theory is very different now from what it was in 1940 or 1950, so that's changed a lot"

For a fuller exploration of the growth of a marginal paradigm we take the case of Milton Friedman. His thinking can be considered the re-emergence of the Quantity Theory of Money that became attenuated and marginalised throughout the Keynesian period. Indeed Friedman himself believed in and worked with these doctrines while the Keynesian theories dominated. When asked in interview about his position in the field during the 1940s, when he was writing critiques of Keynes' General Theory, he replied simply:

> Milton Friedman: "Yes I would have been in the minority"

In Edward Kuska's account of the early 1960s:

> Edward Kuska: "Almost everybody was a Keynesian in those days. Milton Friedman and the Chicago school were trying to convince people that money mattered, but they were having a really hard time of it"

And later:

> Edward Kuska: "Milton Friedman, I think the *New York Review of Books* refused to review his books in the Nineteen Fifties because they thought that he was just off the wall, I mean it was, the discipline had gone that far towards Keynesian economics, so yeah it was all Keynesian"

Within two decades Friedman would become the most influential macroeconomist in the Anglo-American context, and soon after be awarded the Nobel Prize for Economics.

Many of Friedman's ideas, such as the notion that money mattered, had roots in the Quantity Theory of Money, and are argued most thoroughly in Friedman (1956) and Friedman & Schwartz (1963). A decade later, during the dominance of the Friedman/Phelps Phillips Curve, the latter publication was recognised as a central text to the monetarist doctrine, and a major blow to Keynesianism. However, like Eddington's 1919 experimental proof of Einstein's theory of relativity, the accolade placed upon this text was not bestowed until after wider debates, most prominently that concerning the failure of the Phillips/Lipsey Phillips Curve were settled (Collins & Pinch 1998). Indeed, from his seclusion in the margins, it is in this text that Friedman constructs an account of why the phenomenon witnessed by Phillips in his original 1958 paper is explicable from within Friedman's own framework:

> Milton Friedman: "The [Phillips/Lipsey] Phillips Curve developed because of the accident that Britain for a long time had a pretty stable monetary policy, so that long term expectations were stable, and as a result what is essentially a short term relationship looked like a long term relationship, but it didn't develop out of any political context without that empirical setting, ... the price level in Britain in 1939 was roughly as it was in 1739, and if that hadn't been the background that data wouldn't have been there that Phillips could have summarised in his curve"

This shows that while working in a marginal paradigm the actions of macroeconomists looks very much like that of normal science. As with Tobin's account of the impact of OPEC upon the 1970s stagflation rate, explanations are still constructed for the observed phenomena of the time, and those that confounded them in previous times. The difference between normal science in the dominant and marginal macroeconomic paradigms is that the dominant paradigm has many more people enrolled in its performance. As the Seventies passed into the Eighties Friedman's ideas lost their prominence. As his ideas again returned to a marginal position he remained committed to them, and in that he was not alone.

Edmund Phelps is an interesting case for the paradigm characterisation. In some ways his experience problematises the notion, while in others he conforms to it. The tension highlights the often confused and complex application of theoretical labels in macroeconomics. Firstly Phelps has throughout his career considered himself a Keynesian. However one of his greatest contributions, his role in the development of the Friedman/Phelps Phillips Curve, became widely accepted during the monetarist dominance. Today he is often associated with the Neo-Keynesianism mentioned above. Phelps then, a self-considered Keynesian, clearly did not experience a marginal position during the Keynesian lull. His work has had an existence outside of the Keynesian synthesis both through its links to the rise of monetarism, and the impact of another dormant paradigm in the Anglo-American context, the influence of Austrian economics (see below). Tracing through his early career illuminates how this set of cultural associations arose.

Phelps' first paper, produced only months after Phillips' original 1958 paper, accepted and found further evidence to support Phillips' hypothesis (Phelps 1961). The paper came from his dissertation at Yale, and had personal input from James Tobin. From the conclusions and influences we can see that this paper is clearly within the Phillips/Lipsey Phillips Curve accepting Keynesian synthesis of the 1960s, as Phelps testifies himself:

> Interviewer: "This was within a fairly sort of standard Keynesian [Phillips/Lipsey] Phillips Curve accepting"
>
> Edmund Phelps: "Yes, it was a fairly standard framework, I didn't question the stability of the relationship between the size of the unemployment rate and the rate of inflation, I was at a more primitive level of enquiry you might say, ha ha"
>
> Interviewer: "Yeah sure, ok"
>
> Edmund Phelps: "But I was vaguely aware even then that there was an issue about the stability"

Phelps' comment about stability refers to his later work on the relationship, arguing that the Phillips/Lipsey Phillips Curve only holds in the short run. The pertinent point is that this vague awareness stems from a marginal influence, that of Austrian macroeconomics. In Phelps' case this exists through the influence of William Fellner and Henry Wallich. In the following lengthy but important extract, Phelps is discussing how the awareness he identified above informed his later works from the late sixties onwards, works that were central to the development of the Friedman/Phelps Phillips Curve:

> Edmund Phelps: "So I was aware of the idea that ultimately, within a given structure of the economy, there is only one sustainable unemployment rate that is roughly like the vertical [Friedman/Phelps] Phillips Curve idea, who were those people, Abba Lerner in 1947/1948 in a symposium called 'The Discussions of the Inflationary Gap', in which Lerner said [he] didn't think there was any long run relationship between the rate of inflation and the rate of unemployment, and then this view came to be argued for again in the 1950s by William Fellner and Henry Wallich, who were both at Yale, and there was no way that I couldn't have picked up from them at Yale that thesis"
>
> Interviewer: "Because?"

Edmund Phelps: "Because I took courses with both of them, and so when I introduced expectations into the inflationary mechanism I was fully prepared to find that the relationship would sort of evaporate, that there would be no steady inflation rate that went within a steady unemployment rate of inflation, if the unemployment rate was too low the inflation rate would simply rise and rise and rise and rise"

Interviewer: "Sure, Lerner, Fellner and Wallich, they, would they have been writing within fairly orthodox Keynesian traditions?"

Edmund Phelps: "Oh no, ha ha, although Lerner was an arch Keynesian in many ways, he must certainly have been somewhat in touch with the Austrian and German literature of the 1930s and 1920s because he spent his youthful days as a student of economics, um I don't know exactly where, but early in his career he was at the London School of Economics and he was writing in the late thirties, so he was familiar with the continental tradition which was not Keynesian, it was pre-Keynesian, and he almost certainly would have picked up the idea that [a Phillips/Lipsey style Phillips Curve would be unstable], and then both William Fellner and Henry Wallich were born on the continent of Europe, Fellner didn't migrate to the United States until he was probably already thirty and then would have passed in, let's say 1940, I'd have to check those years, and also Henry Wallich studied at Berlin in the early 1930s and came over to Harvard I think in the late thirties, so I think Lerner, Fellner and Wallich had all picked up this idea of a vertical Phillips Curve in the nineteen twenties and thirties"

Interviewer: "From the continental tradition?"

Edmund Phelps: "Yeah"

Interviewer: "And brought that into a western [macroeconomic framework], oh that's very interesting"

Edmund Phelps: "So there's always that dissident view that was opposed to Phillips Curve idea"

Interviewer: "And that would be alien in Anglo-American orthodox economics"

Edmund Phelps: "Right, the Keynesians were prepared to embrace the Phillips Curve almost immediately"

Phelps identifies a strain of thought beyond the conceptual framework of the dominant Keynesian ideas he was socialised into. It would be wrong to say Phelps' work is Austrian macroeconomics. However we can say he drew upon what in the Anglo-American context was a dormant paradigm in a manner unavailable to many of his generation. It is also worthy of note that in the Nineteen Eighties and Nineties a form of Austrian macroeconomics did gain ground in the Anglo-American context, associated with writers such as Roger Garrison, although it remained a minority position.

Phelps' use of Austrian macroeconomics suggests paradigms in the discipline need not be incommensurable in their scientific concepts and theoretical language, or the observational data and mode of scientific perception, as Kuhn sometimes suggests. This demonstrates another nuance of macroeconomic paradigms and supports the argument that it is the agenda of problems to be solved and subsequent criteria of adequacy for scientific explanation that provide the incommensurability in this case. As argued in Chapter Two, the first two locations of incommensurability are prioritised by Blaug (1976, 1991) in his rejection of Kuhnian analysis in economics. Phelp's quotation adds further support to the rejection of Blaug's interpretation.

10.2.4 The Properties of Paradigm Shifts in Macroeconomics

There are three mechanisms explaining how paradigms shift in macroeconomics. These relate to the macroeconomist's construction of self as value neutral, the role of new researchers in creating orthodoxy, and the continual increase in the number of macroeconomists and shifts in their focus during the Phillips Curve debate. We discuss each in turn.

In the previous chapter we saw how macroeconomists constructed value neutrality as being data responsive, and a willingness by members of their profession to change their mind should it seem beneficial to their work. The additional understanding developed in this chapter is a better insight into how a particular position becomes identified as beneficial. Work was beneficial when it cohered to the criteria for good macroeconomics, and as is the argument throughout this chapter, this negotiation is located in the social space occupied by policy makers. Subsequently changes in the goals of policy makers were swift to attract the majority of macroeconomists who were not too deeply entrenched in the existing theory to jettison their previous beliefs.

The second mechanism concerns whom macroeconomic debate is attempting to seduce. Contributors to the debate did not act to change the minds of their fellow core set members. Instead, as David Laidler describes, they are attempting to enrol their younger colleagues' fresh into a career in macroeconomics as subscribers to their view. Speaking of one of his papers:

> David Laidler: "This is another way I think the profession works. Among people who were already established in economics and politics I think the reaction was predictable: people like Harry Johnson and Alan Walters liked it, people like Brian Reddaway and Nicky Kaldor ... disliked it intensely, and the graduate students read it and made up their own minds. So I think on balance that's one of the papers that did actually shift the view of macroeconomics in general and inflation theory in particular among the younger British economists"
>
> Interviewer: "You've made several mentions of convincing graduate students. In terms of economic debate, then, to what extent do you think economic debate happens to persuade the other person?"
>
> David Laidler "Oh not at all"
>
> Interviewer: "No, it's to persuade onlookers?"
>
> David Laidler: "It's to persuade whoever's looking on, yes, I won't say I've never been present at a place where someone has changed their minds in the face of an argument, because obviously it does happen, we all know that that happens, but I think that when you get into these big policy debates the debate is a set piece, that's carried on with the aim of persuading the audience"

And on the responses of young macroeconomists, Edward Kuska:

> Edward Kuska: "Young people are looking round for things to publish, you tend to be more revolutionary when you're young, and you want to knock people off their pedestals, so it's young people that go out and lead charges like that, and the old people just by their nature tend to be more conservative and defend the *status quo* in everything I would have thought, and the same in economics so a lot of the new stuff comes from younger people, and then flexible older people accept it if it seems reasonable, some just fight it tooth and nail all the way to their grave"

Kuska's account has resonance with Collins' notion of distance lending enchantment and Donald MacKenzie's uncertainty trough (Collins 1985, MacKenzie 1999). Both describe how the settling of a scientific controversy is always bestowed with the virtues of a more clear and less messy resolution by those further away than by those in the core-set itself. Furthermore, frequently this perception amongst wider groups has a role in the cementing of a new consensus. These dynamics are indeed at work in macroeconomics, although like many other facets of scientific debate, they operate at a much higher speed in this setting.

The third mechanism further amplifies this effect. During the period of the Phillips Curve debate the macroeconomics profession increased dramatically in size. Clearly a continually increasing pool of new academics looking for new topics to research means that the latest popular idea has more available recruits. Those older macroeconomists remaining committed to an earlier argument become progressively marginalised, not only because colleagues turn away from their position, but also as their numbers become proportionately smaller relative to the population of macroeconomists.

Furthermore, macroeconomics is broad enough, with sufficient transferability of skills, for macroeconomists to migrate between localised specialisms. Subsequently some whose work supported one incarnation of the Phillips Curve could stop advocating that version without moving on to the one that followed. Instead their interest moves to another area in the discipline, further marginalising those who remain in opposition. Examples from the data-set are Keith Cowling and Peter Stoney.

The combination of the three mechanisms demonstrates how macroeconomic paradigms expand through a process of mass enrolment. When the macroeconomists of the existing paradigm are unable to make the repair work necessary to address the latest economic problem in the timeframe available many will stop supporting it. Many will adopt a new theory while others will move into a different subsection of macroeconomics. Furthermore, the ever increasing pool of young researchers new to the profession tend to align themselves with the newest popular version.

This leaves little to the imagination regarding the mechanisms by which a paradigm contracts. It finds itself on the receiving end of all the same mechanisms it experienced in its own expansion. The paradigm's inability to address the newly identified economic problems with the immediacy required of it ceases to attract new researchers to the field, it loses its less entrenched followers, and is condemned by the removal of support from the wider community.

Now we have explored the concept of expanding and contracting paradigms it is time to apply them one more time, to the emerging dominance of the Rational Expectations Phillips Curve.

10.2.5 The Rational Expectations Phillips Curve Cycle of Contest and Closure as a Problem Solving Phenomenon

To complete the paradigm-centred analysis it is necessary to identify an economic problem that the Rational Expectations Phillips Curve is a response to, and demonstrate that the Friedman/Phelps Phillips Curve failed in the same goal. In practice there are two related ways the argument can be made. One is that the Rational Expectations Phillips Curve addresses the same problem as the Friedman/Phelps Phillips Curve, the stagflation of the late sixties through to mid-seventies. Here the paradigm shift was instigated as the Friedman/Phelps Phillips Curve proved inadequate to explain the stagflation of the early to late Nineteen Seventies in the culturally available time. The second plausible account is that the second bout of stagflation in the late Seventies through to the mid Eighties led to the paradigm shift. Here the Friedman/Phelps Phillips Curve would have been judged adequate in providing an account of the first stagflation, but was seen to fail when it re-occurred.

The differentiation drawn here is perhaps too subtle a distinction for the paradigm account. With both we can say that sometime around the mid to late Seventies the Friedman/Phelps Phillips Curve was deemed to be failing to provide an adequate account of stagflation. In effect the immediacy of policy in a prolonged or repetitious stagflation meant even the new found Friedman/Phelps explanation could not fit the criteria for scientific success it was selected to fulfil. Subsequently the mechanisms outlined above engaged and the rational expectations paradigm came to prominence.

We can make some brief comments about the period after that explored in this thesis. It is interesting to note that policies drawn from rational expectations, which were meant to lower inflation without any impact upon the unemployment rate, failed. Unemployment continued to rise to its highest rate in the post war period in both the U.S. and the U.K., although both massive inflation rates were quelled. However this unemployment crisis did not immediately break the Rational Expectations Phillips Curves paradigm. Instead it would remain dominant for around five more years. Rational expectations theorists were able to carry out the repair work of normal science, identifying the lack of credibility of the government to speak honestly as preventing the realisation of full rational expectations by the public. This leads to the issue of the nature of political goals, and subsequently the criteria of scientific success placed upon macroeconomists. The policy did stop the massive inflation as it claimed, however it failed to predict the accompanying massive unemployment. A potential area for further research could explore whether the continued dominance of the rational expectations paradigm reflected the varying political priorities placed upon inflation and unemployment by the U.S. and U.K. governments of the eighties.

10.2.6 Paradigms on the Margins

It is now time to address a potential criticism of the argument as presented so far. There may appear to be a conflict between the theory of problem-led paradigms and the evidence for the uptake of the theories themselves. This criticism would point out that often macroeconomists start turning to a new incarnation of the Phillips Curve before the problem we have associated with it arose. For example, one informant, Mike Sumner, explains his shift from believing in a Phillips/Lipsey Phillips Curve to the Friedman/Phelps Phillips Curve, Mike Sumner:

> Mike Sumner: "Well I can only speak for myself, and for me it was just a narrow question of what the economics evidence told me, I changed my view on that before the magnitude of stagflation became apparent"

However this does not derail the paradigms characterisation. This is in fact the normal movement and intellectual growth within the marginalised macroeconomic sphere. Members of the Manchester School, including Mike Sumner, often said in interview that they were well aware that they were adopting a marginal position. Other macroeconomists voicing dissenting opinions have always existed and been active in pursuing new ideas. However many of these macroeconomists would never go on to see the popularisation of their ideas in the way the Manchester School did. This is not because these other macroeconomists' ideas were false while the Manchester School's ideas were correct. It is because the timing of the growth in the Manchester School coincided with the sudden appearance of the breakdown of the Phillips/Lipsey Phillips Curve. Many other groups would have been developing alternative ideas from the forties through to the mid-sixties. However these paradigms never expanded because there was no call for a new theory, the criterion set by the policy making circles agenda was being addressed by Keynesianism. Not until this criterion changed, and Keynesianism struggled to do the repair work in the socially available time, did the activity in the dormant sphere have an opportunity to expand.

Let us take three pertinent examples. Friedman himself was keen to highlight that Phillips' 1958 paper was not the first publication expressing the relationship in that nature. In fact a very similar relationship was posited by Irving Fisher in 1926. Fisher's version, however, never became widely incorporated and was not even read at the time. This was not a statement on Fisher's legitimacy to a knowledge claim on the relationship, or that data could not be provided to support it, since Phillips' own data covered the period back to 1861. Instead there was no political or intellectual need for the relationship at the time. If there had been, and Fisher's notions were the most socially effective addressing it at the time, then it almost certainly would have been elevated to this level, and Bill Phillips' career would have taken a very different path. On this, James Tobin:

> James Tobin: "Irvine Fischer had an article back in the Twenties actually showing the relationship between wage change and unemployment"
>
> Interviewer: "But the Fischer paper never spawned such an [interest], why not?"
>
> James Tobin: "Well it didn't occur at the time, it wasn't a big issue for any diagnostic or policy point of view, so it was in the prosperous times of the Twenties and it was more a kind of curiosity, he didn't make anything out of it from a policy point of view"
>
> Interviewer: "So do you feel then that economics follows social need in a certain way?"
>
> James Tobin: "Oh yeah, there's no question about it, economics takes its problems from the real world and often they are highly related to policy, so the policy debates have a counterpart in theoretical debates and vice versa"

A second example, also from the Phillips Curve debate, is Robert Lucas himself. Although his work on rational expectations did expand greatly in late Nineteen Seventies and Eighties, it is worth noting that he had been publishing on the topic with Leonard Rapping since the late Sixties, so was in fact part of a dormant paradigm simultaneously with Friedman and Phelps (Lucas & Rapping 1969a, 1969b). Furthermore, the idea of rational expectations had existed since 1961 in the work of John Muth, albeit in a microeconomic context. However, even within microeconomics the idea remained dormant for a number of years.

So some ideas are created, only to be ignored and reinvented, when necessary, perhaps decades later, others exist on the margins vying for attention and subsequent expansion, failing only to succeed later, and some never go on to expand at all. For example, before his influential work with the Manchester School, David Laidler produced a model with Bernard Corry that suffered this fate, speaking of this:

> David Laidler: "I'm not very proud of that paper, OK, it's there and there is nothing I can do to remove it from my C.V.. We also did quite a bit of empirical work with that model which was never published in the late Nineteen Sixties"

So the fact that we see the growth of a theory on the margins before the existence of the economic problem that fuels its expansion should come as no surprise. There are always movements and developments on the fringes with small congregations of people forming around an idea. The vast majority, however, do not attain popularity and are never heard of. Again here it is worth mentioning the example of Eddington's 1919 experimental proof of Einstein's theory of relativity, where the accolade placed upon this text was not bestowed until after wider debates (Collins & Pinch 1998). The construction of people like Friedman, Phelps, the Manchester School and Lucas as pioneers with great foresight can only be attributed years later, once the lottery of problem changes has played its part.

10.2.7 Lessons from the Development of the Bicycle

Pinch & Bijker's (1987) discussion of the development of the bicycle adds further insight to our story. This early work in the Social Studies of Technology stresses the multidirectional nature of any artefacts advancement over time. This means that any successful progression in the design of a technology is not the only potentially available version. This fits well with the above discussion of macroeconomic ideas on the margins acting as a pool of alternative constructions of economic problems, ready to be drawn upon in times of crisis. However, in the Pinch and Bijker case the alternatives are shown to be far from marginal in the period of debate.

Pinch and Bijker trace the development of the bicycle from the form today nick-named the 'penny-farthing' to those with more equal sized wheels and rubber air filed tyres. They demonstrate a range of competing designs for the bicycle throughout its history and seek to explain why some succeeded over the others using a symmetrical analysis as employed here. They are relevant in this context because their priority on the multidirectional nature of debate and the role of identifying the ownership of problems inherent to the bicycles continued reinvention has similarities with the argument presented here.

Pinch and Bijker identify various social groups with interests in the development of the bicycle. These groups associate different meanings and values with the bicycle. The competing sets of meanings construct different perceived 'problems' with each design. Examples of the social groups identified by Pinch and Bijker are the cyclists; sub-divided in certain instances by gender and age; the engineers, and the 'anti-cyclists'. They discuss the period when the it was considered incorrect for women to mount the penny-farthing. Instead tricycles were the socially acceptable alternative for women. During this period the dominant users of the penny-farthing were professional men who valued the bicycle primarily for sport as opposed to transport. Both groups had differing values associated with the bicycle which in turn directed competing expectations of the design. These conflicting expectations pitted safety against speed, and subsequently the alternative design solutions to each problem against each other.

The example of the air tyre brings these issues together. It was initially developed as one of many solutions to the problem of vibrations passing from bumpy roads to riders on small wheeled machines. However, such vibrations were only constructed as a problem by certain social groups; women riders and elderly men. The young men riding bicycles valued the vibrations as part of the experience that reinforced their male bravado identity. Closure to the issue arrived when, initially to wide-scale derision, an air tyre bicycle was raced in a sporting context, only to demonstrate itself far quicker than anticipated by the audience of young men. Through this, the air tyre became recognised not only as a solution for the vibration concerns of women and older men, but also as contributing towards creating the fast and excitement sport desired by young male riders.

The comparison to macroeconomics is interesting for both the similarities and differences. Pinch & Bijker's multidirectional analysis clearly resonates in our case. The cycle of contest and closure in macroeconomics could also provide many alternative potential forms, all undisclosed until political crisis takes hold. Furthermore Pinch and Bijker's emphasis on the role problem setting in the development of the bicycle fits has parallels with the analysis presented in this chapter. However there is one central difference. As with Kuhn's chemistry example, the negotiation of the priorities to which the development of the bicycle responded to occurred between a number of groups with competing interests. Unlike Kuhn's chemistry example, but still similar to the macroeconomics case, the story of the bicycle does allow room for the users of the scientific or technical development to set the problems and associated criteria by which it is judged. However, unlike the bicycle case, during periods of political crisis in macroeconomics, there is only one group of users, the policy circle, that have the autonomy to set the relevant problems and associated criteria of adequacy. There is not room for the type of negotiations discussed by Pinch and Bijker, as any other interested groups, including the macroeconomists themselves, lack the autonomy to define the problems. The immediacy of policy needs during periods of political crisis excludes all other parties from the types of social negotiation discussed by Pinch and Bijker.

10.3.1 Understanding the Characteristics of Macroeconomic Paradigms

This chapter has applied Kuhn's notions of paradigms being orientated towards certain goals and the associated criteria of success to the macroeconomics case where the macroeconomists have dislocated themselves from the policy setting arena. We have compared the macroeconomics case to the chemistry example of affinity theory and the theory of fixed proportions. This demonstrated that in the chemistry paradigm shifts the participants in the debate themselves develop, contest, and maintain the problems their discipline is orientated towards. This process is lengthy and can take a generation. However this rigidity is absent in macroeconomic debate. Subsequently any attempt to contest the new goals or standards of adequacy for the discipline by a macroeconomist will result in their marginalisation from the mainstream of the profession.

We have explored the three problems each incarnation of the Phillips Curve addressed. The Phillips/Lipsey Phillips Curve is the final act of the Keynesian normal science that had risen on the political decision to tackle the problems of the Great Depression. The Friedman/Phelps Phillips Curve became the new macroeconomic orthodoxy after another political decision to tackle the stagflation of the early Nineteen Seventies. Finally the Rational Expectations Phillips Curve followed the reoccurrence of stagflation in the late Nineteen Seventies and early Nineteen Eighties.

In each case the immediacy of policy needs during period of economic crisis in response to each political problem prevented the existing orthodoxy from engaging in the processes of normal science to develop an account that explains the phenomenon within their existing framework. Instead the new problem asserted its dominance swiftly and the marginal paradigms competed for their opportunity to become the newest orthodoxy.

A paradigm expands from its marginal position by three mechanisms. We saw that they were firstly macroeconomists constructing themselves as value neutral and, by perceiving the newest incarnation as best addressing the new problems of the field, adopting the new position as their own. Second was the enrolment of young researchers in the field. The third related to demographic shifts in the profession as it expanded in size and macroeconomists shifted between specialisms within macroeconomics.

We have also explored how paradigms exist on the margins. Their work is similar to the normal science of mainstream paradigms. Explanations are produced that explain previously unaccountable phenomena within their existing framework. There are always macroeconomists working on the margins. Many of these will never attain orthodox status, however, because the opportunity of a shift in policy objectives immediate enough to break the existing orthodoxy and orientated towards their position fails to emerge. Furthermore the collapse of an orthodox paradigm does not result in its total demise. Instead it contracts back to a marginal position where the few who remain loyal continue the work of normal science again awaiting their next turn to blossom.

10.3.2 How Macroeconomic Paradigms as Problem Solving Phenomena Relates to the Rest of the Thesis

We have now completed the six main analytical chapters in the thesis. The first four demonstrated the various locations of Interpretative Flexibility in macroeconomics that add lubrication to the cycles of contest and closure. This chapter and its predecessor explored how the legitimising role of value neutrality in liberal democratic political cultures dislocates macroeconomists from the processes that set the goals and criteria for success for their discipline. The following chapter, Chapter Eleven, draws together all six chapters to create a well-rounded account of the macroeconomic cycle of contest and closure.

11. Why Macroeconomic Orthodoxy Changes So Quickly: Summary and Conclusion

11.1.1 Introduction

This thesis has addressed one central question, that is why the cycle of contest and closure in macroeconomics is faster than in most sciences. In the twenty five year period studied, from Phillips' original 1958 publication, until the early 1980s, three separate orthodox positions on the relationship between unemployment and inflation came into the ascendancy. Within ten years each was beginning to lose their dominance. Chapter One argued that swift paradigm shifts are rarely witnessed in other sciences. This chapter draws together the arguments presented so far into one unfolding narrative, and then locates these findings in the context of the Social Studies of Science literature.

This research followed the prescriptions of the Empirical Programme of Relativity (EPOR) (Collins 1981a, 1984, 1992). The thesis has six data led analytical chapters. The first four detailed the Interpretative Flexibility of the Phillips Curve debate, and subsequently completed stage one of EPOR. The three chapters preceding this explored the linkages between the Phillips Curve debate and wider socio-political structures. Each chapter focused on a particular issue that in aggregate provide an explanation of the high frequency turnover of the cycle of contest and closure. In order, the six chapters considered Interpretative Flexibility in data, Interpretative

Flexibility in theory, Interpretative Flexibility in the combination of data and theory, Interpretative Flexibility in political interests, the role of value neutrality in the debate, and macroeconomics as a problem solving discipline.

The analytical section of this thesis began with an exploration of macroeconomist day to day activities and, as it progressed, located these in the wider cultural context. This conclusion retells the story in the opposite direction, starting with the widest socio-cultural influences and then concentrating down to demonstrate how they shape the smallest day to day actions of the macroeconomists. We start by discussing the widest cultural aspect discussed in the thesis; the creation of liberal democracy.

11.2.1 Establishing a Political Culture and Establishing Macroeconomics

Chapter Nine drew links between the birth of liberal democratic society and scientific practices, including economics. Yaron Ezrahi's (1990) work was used to explore how the need for legitimacy in the new political culture was satisfied by the depersonalised knowledge of the perceived objective science. Where monarchist power structures appealed to the authority of religion to justify its rules and inequalities, liberal democratic societies appeal to the authority of the sciences. By representing depersonalised knowledge, scientific discourses can be drawn upon by governments to externalise decision making away from the self interest of the individual. They rely upon the cultural resources of fact and objectivity to demonstrate their decisions have a grounding in an externally rationalised location.

To fulfil their perceived social role as producers of depersonalised knowledge economists adopted a construction of self as value neutral. Their role would be to understand economic phenomena, but not make decisions on which economic policies should be employed. We saw that this construction had a role in two processes, although the attainment of objectivity was not one of them. Firstly macroeconomists dislocated themselves from agenda setting in the policy arena. This was vital to their role as rejecting normative advocacy in policy debates. The second was the widespread adoption of statistical techniques. We benefited from the insights of Theodore Porter (1995) on how depersonalised statistics embodied trust beyond that invested in the researcher employing them. Morgan and Rutherford (1998a) were used to demonstrate that the form adopted by the value neutral construction of self was contingent and a result of social negotiation. However we noted that the mechanisms detailed by Ezrahi (1990) operated successfully irrespective of the particular form of value neutrality used.

We explored the frontstage and backstage construction of value neutrality (Goffman 1956). In the frontstage we see a pristine performance of the norms associated with objectivity expressed through the empiricist repertoire (Gilbert and Mulkay 1984). This is understandable in the context of Ezrahi's analysis where the strict distinction between normative and positive science must remain in the public eye for the wider social system to maintain public confidence. However in the backstage we witnessed an approach more susceptible to nuance and negotiation expressed through the contingent repertoire. The boundaries between normative and positive actions were more loosely enforced than in the front stage. However a clear moral code orientated towards the maintenance of value neutrality still existed in the backstage. It was simply more reflexive towards the inherent normative element in any cultural activity, and relied upon the perceived depersonalised nature of statistical technique to weed out the normative element, a technique known as the 'truth-will-out device' (Gilbert and Mulkay 1984).

Given this socio-cultural context, we can now explore the operation of the relationship between politics and macroeconomics within it.

11.3.1 Movements within a Political Culture and Movements within Macroeconomics

Chapter Ten built upon the tensions relating liberal democracy to the construction of value neutrality. We employed the analysis of Thomas Kuhn (1970) to demonstrate macroeconomics as a problem solving discipline. Three economic problems were identified that preceded each incarnation of the Phillips Curve. Kuhn's argument that individual paradigms are orientated towards addressing different problems, and that with these problems came specific criteria for adequacy of scientific work, fitted well with the macroeconomic case. The novel aspect of the account derived here is that the value neutral construction of self imposed by the need for legitimacy in liberal democratic states has dislocated the macroeconomists from or the sphere in which the problems their science addresses are set. In times of relative political calm and normal science, such as the early 1960s, macroeconomists may be able to influence policy decisions in a way that suggests they do have some autonomy over the policy agenda. However this is always limited to those policy suggestions that cohere with the existing political vision. The relationship is granted and guarded by the policy circle. Should the government be faced with economic crisis comparable to the Great Depression or the stagflation of the 1970s the nature of the relationship would change significantly and it is unlikely that the macroeconomists would retain their influence on the policy agenda.

The dislocation of macroeconomists from the problem setting realm had several interconnected implications for the cycle of contest and closure. This was made explicit by a comparison to a paradigm shift in nineteenth century chemistry (Kuhn 1970) and the development of the bicycle (Pinch and Bijker 1987). In the chemistry example the scientists themselves were engaged in the debate over what the problems their science should address should be. This introduced a tension in the paradigm shift because disapproving chemists could legitimately protest against the shifts and enact strategies to prevent them. The nature of the problems themselves were contested. However in the macroeconomics case this is not so. The nature of the problems macroeconomics addresses is contested in the policy making circles during periods of political crisis. Subsequently macroeconomists themselves cannot pursue a strategy of resistance and expect to remain central to their discipline. Instead the form of contestation witnessed in macroeconomics concerns which of the competing forms of theorising can best address the problems it is given. Most pertinent for understanding the speed of the cycle of contest and closure is the observation that the contestation of the problems chemistry addressed took a sizeable amount of time, that in turn delayed the widespread adoption of the new paradigm. In macroeconomics this is missing, and the cycle of contest and closure had a faster rhythm.

The rhythm is dictated by the policy setting arena, and here lies another interconnected implication. The policy setting sphere experiences a different temporality from that expected for sciences. There is an immediacy to policy needs that sits awkwardly with the usual slower paced processes of normal science. We noted that, whether the cycles of accountability in politics are measured in days for journalistic scrutiny or years for the electoral process, the pace is faster than the frequently generation-length shifts in scientific paradigms. This cycle is further imposed upon macroeconomics in the way few other sciences experience.

A serious change in agenda in the policy making circle instigated by political crisis sets new problems for macroeconomists and with them a new criterion of adequacy for their work. If the current orthodoxy cannot provide an explanation through normal science in the culturally available time then a paradigm shift occurs. Chapter Ten explored the specific processes underlying macroeconomic paradigm shifts. A defining characteristic in this case is that paradigms do not disappear completely but instead exist on the periphery of the discipline. Indeed there is constant activity amongst marginal macroeconomic paradigms, some the remnants of a former dominant position, some future orthodoxies, and some that may never rise in stature beyond marginal status. The activities of these groups are similar to that in the orthodox paradigm. Research efforts are enacted to configure theoretical and empirical resources in a format that provides explanations of past and present macroeconomic concerns within their existing framework. Marginal paradigms, however, have far lower numbers engaged in this.

Marginal paradigms are given the opportunity to expand into a dominant position only when the policy arena dictates new goals that the orthodoxy cannot address in the available time. The marginal paradigm that can best provide an explanation of the problem will expand by attracting macroeconomists with prior allegiance to the failing orthodoxy and new researchers to the field. We also noted that the profession expanded in numbers dramatically during the period investigated. This, combined with the ability of macroeconomists to shift specialism within the discipline, ensures a swift shift of dominance from one paradigm to the next.

Chapter Eight also explored a set of dynamics that under a social interest model could lead movements within a political culture to impact upon movements within macroeconomic orthodoxy. However we saw that in this case the social interest model provides little insight explaining the cycle of contest and closure. This is because of the Political Interpretative Flexibility in macroeconomics, meaning that a single macroeconomic argument can, and frequently does, express the ideals of multiple political positions. Equally, thinkers with the same political position can consider their ideals best expressed through differing macroeconomic arguments. This does not mean that the Phillips Curve is apolitical. We saw that frequently the respondents did identify political motivations in their work and that of others. Instead macroeconomics is 'any-political'.

It is for this reason that the social interest theories are unsuitable in the macroeconomics context. The political linkages between the wider political ideologies of the left and right and incarnations of the Phillips Curve are not systematically distributed. Subsequently a wider political shift in either direction will not deliver increased support or profile for a particular Phillips Curve theory. Since the expression of each political ideology is distributed throughout the Phillips Curve debate any increase in support for the research effort associated with a swing towards one political position is shared on all sides of the debate. This, however, is not the central conclusion of Chapter Eight for the explanation of the cycle of contest and closure.

Political Interpretative Flexibility also provides additional lubrication to the cycle of contest and closure. This operates on two levels. Firstly it allows the established researcher to shift from one paradigm to another and potentially construct both as expressive of their political views. We took Edmund Phelps as an example of this, who discussed his shifting position on the negative income tax. Secondly new researchers to the field can adopt the latest set of ideas without mounting a challenge to their own political interests. Both these examples are best pronounced in comparison to a situation where strict connections between political positions and incarnations of the Phillips Curve existed. Existing researchers in the field would be less likely to change their position in the debate if it meant advocating a position that challenged their political views. Instead they may engage in a research effort to protect their existing position with more rigour than they otherwise might. Equally new researchers to the field would be more inclined to adopt the position associated with their own political opinions, thus hardening old battle lines and not progressing forward quickly. Political Interpretative Flexibility removes this rigidity.

11.4.1 Maintaining a Political Culture and the Day to Day Activities of Macroeconomists: The Interpretative Flexibility of Theory and Data

Chapters Five, Six, and Seven explored the day to day judgements made by macroeconomists in doing their own theoretical and empirical work and in evaluating that of others. There were differences between the format in which Interpretative Flexibility was actualised in each case, and we will address these shortly. First let us reiterate the similarities. In all three chapters we saw how macroeconomists prioritise certain elements of the work over others to conform with their expectations. However, also in all three chapters, we remained silent on how these expectations were derived.

We are now in a position to address this issue. The criterion for adequacy in macroeconomics is a product of the problems identified by the policy circle. Macroeconomists generally configure their day to day work to support the paradigm that best addresses the most recent economic policy concern in the amount of time that is culturally available. When the policy concern has remained stable for a period of time this resembles the normal science Kuhn describes, and focus shifts to broadening and establishing the established framework. Equally, when the policy concern shifts with insufficient urgency to deny the orthodox paradigm time to complete the normal science necessary to construct an explanation for the new concern, macroeconomics still resembles the normal science Kuhn describes. This is what happened with the adoption of the Phillips/Lipsey Phillips Curve. However, when faced with an economic problem sufficiently severe to cause political unrest, the socially available time for the dominant paradigm to perform the normal science is vastly curtailed. The criteria for scientific adequacy set by the policy sphere require an explanation for the perceived economic problem and a range of strategies to conquer it. In addition, and most importantly, the criteria for adequacy embody the temporality of the policy setting arena, implying the immediacy of policy needs is imported into the academic domain, thus imposing a narrow time limit. Should the dominant paradigm fail to orient itself towards this end swiftly enough a new paradigm needs to be selected from the margins to address the new concern. Once this shift becomes apparent the day to day work of the macroeconomist ceases to be the normal science of reinforcing the previous dominant paradigm, and instead becomes the establishment and embellishment of its successor. Subsequently the expectation to which the theoretical and empirical work is orientated shifts.

For example, before the identification of the stagflation of the early 1970s as a major policy concern the majority of macroeconomists active in the area were configuring their work to correspond to their expectation of a Phillips/Lipsey Phillips Curve. Afterwards, once it had become apparent that the Phillips/Lipsey Phillips Curve was no longer compliant with the criteria of success enforced by the policy making sphere, the majority of macroeconomists active in the area orientated the configuration of their work towards the paradigm deemed most quickly addressable to the new criteria of success for the field. This paradigm was the Friedman/Phelps Phillips Curve, and the theoretical and empirical effort of the profession quickly turned to the normal science of defending and substantiating its new orthodoxy.

Let us remind ourselves of the mechanisms described in each of the three Interpretative Flexibility of resources chapters. Chapter Five illustrated the Interpretative Flexibility of data. Three clusters of important macroeconomic papers were discussed, and six categories of data handling were developed. These categories were: choosing the macro data-set, transforming the data, developing proxies, micro data-set choices, identifying special effects and the interpretation of the data. Each could be configured in such a way as to limit the potential interpretations of the data to the specific format that confirmed with the analyst's expectations. We illustrated this with three clusters of papers, Phillips' and Lipsey's work on the Phillips/Lipsey Phillips Curve, the debate over the Key Industry hypothesis, and the critiques of Trade Union militancy studies. Similar arguments were discussed from the History of Economic Thought literature (Wulwick 1989, 1996, Kim, De Marchi and Morgan 1995, Backhouse and Morgan 2000, Mirowski 1995 and Morgan 1990).

Chapter Six explored how macroeconomists configure theoretical resources in their own work. We characterised the process with a building metaphor, where conceptual tools were combined together to link the existing theoretical basis of the paradigm to the patterns derived from the data. We illustrated this with two clusters of papers, Phillips', Lipsey's and Kuska's theoretical basis for the Phillips Loops, and Tobin's reassertion of the Phillips/Lipsey Phillips Curve in the light of the Friedman/Phelps Phillips Curve critique. Again similar arguments were discussed from the History of Economic Thought literature (Cartwright 1999).

Chapter Seven illuminated individual macroeconomists' prioritisation of theoretical and empirical resources when evaluating the contributions of others. Interview quotations were used to make explicit the favouring of confirmatory locations of empirical or theoretical resource over those that challenge the analyst's expectation. Examples included Laidler's identification of the failure of price movements to preceding changes in quantities as predicted by the Rational Expectations Phillips Curve, and Peel's claimed preference for theoretical resources over the findings of data.

We could apply the framework explicated here to all of these examples, but let us explore it in only one context, a specific modification to his data-set by Richard Lipsey. As we discussed in Chapter Five, Lipsey made a micro data-set choice to drop 1920-2 and 1947 from his data-set. This was because he claimed they were extreme variables because they accounted for a high proportion of the variance explained in his regression that showed unexpected results in these periods. He concludes that the exclusion of 1920 and 1947 can be justified by reference to the lingering impact of heavy war time controls, while accepting there is no such justification for 1921-2. After running the regression again without these years his results had a higher level of conformity with the Phillips/Lipsey Phillips Curve he was trying to establish.

Let us contextualise this action in its fullest. Lipsey was operating in a liberal democracy that depended upon a public perception of a depersonalised knowledge base to maintain legitimacy for its rules and power distributions. In this context macroeconomics, a science dealing in visibly political phenomena, needed to adopt a value neutral construction of self. One by-product of this construction was the dislocation of macroeconomists from the policy setting arena where the problems of the discipline are set. At the time Lipsey wrote his paper the most recent economic phenomenon that had been deemed sufficiently politically volatile to change the criteria of adequacy for macroeconomics and import the temporality of the political arena was the Great Depression. This had seen the fall of the plurality of pre-Keynesian economics and the widespread adoption of Keynesianism. Lipsey was writing within the Keynesian tradition. Within the normal science of Keynesianism the Phillips/Lipsey Phillips Curve served two functions. Firstly it provided an answer to the long standing perceived weakness of the Keynesian synthesis regarding the assumption of fixed wages. Secondly it provided insights into two recessions in the late fifties that before the Phillips/Lipsey Phillips Curve could not be easily explained in a Keynesian framework. These recessions, however, did not cause sufficient political turmoil to provoke a stark change in the criteria of success for macroeconomics, or import the temporality of the policy making sphere. Instead a relatively calm process of normal science was allowed. In this context we can say that Lipsey removed the years 1921-2 from his data-set as part of an effort to consolidate a macroeconomic paradigm that had risen to dominance by addressing the issues raised during the Great Depression. In a different cultural context, for example that experienced only twelve years later, it would be unlikely that a macroeconomist would have made that decision in their data configuration. The difference is that the political unrest associated with stagflation had imposed policy setting temporality on macroeconomists and the resultant paradigm shift was in full swing.

11.5.1 The Main Contributions to the Social Studies of Science Literature made by this Thesis

This thesis has made three main contributions to the Social Studies of Science literature. We will discuss each one in turn.

The first contribution to the Social Studies of Science literature is the contribution to the understanding of Kuhn's (1970) concept of problem solving sciences. Chapters Nine and Ten have shown that the standard account of paradigm shifts through Kuhn's problem solving dynamic assumes the autonomy of the scientists to contest, negotiate, and defend their research agenda. Such negotiations usually takes many years to complete, as argued in Chapter One. However through applying the arguments to the Phillips curve debate we have shown that this need not always be the case. Macroeconomists do not have this autonomy since the problems and associated criteria of adequacy to which their discipline is addressed are identified by the another group, those in the policy making arena. Subsequently the temporality of this other group, in this instance the policy circle, is also placed upon the discipline. In the research presented here his temporality exhibited a higher pace than is usually experienced by the sciences, and resulted in the swift cycle of contest and closure witnessed in the Phillips Curve debate.

The second contribution to the Social Studies of Science literature is the introduction of the concept of Political Interpretative Flexibility in Chapter Eight. The Interpretative Flexibility of theoretical and empirical resources has been accepted in the field since the early publications by Collins (1975). However it has never been applied to the political connotations of scientific ideas. As described earlier in this thesis, Political Interpretative Flexibility refers to the potentiality of any scientific concept to be constructed in such a way that it embodies any political position. This finding is useful in the context of social interest theory (Shapin 1975, 1979, Barnes 1977) where political connotations are regularly ascribed to scientific claims. Taking Political Interpretative Flexibility seriously does not undermine social interest theory. Instead it adds further nuance to its explanations and can explain, as in this instance, why they may not be applicable in all contexts.

The third contribution to the Social Studies of Science literature is the display of Interpretative Flexibility in macroeconomics. Chapter One argued that the researchers in the field have produced relatively few studies of economics compared to other sciences. As such a through demonstration of the existence of, and strategies employed to overcome, Interpretative Flexibility in economics is a worthwhile contribution in itself. The few exceptions include Evans (1997, 1999) and MacKenzie (2003). These accounts explore different debates within economics, and Evans, as noted in Chapter Two where both are discussed, demonstrates Interpretative Flexibility in a different way. Even after the contribution of this thesis there is still plenty of room for additional demonstrations of Interpretative Flexibility in economics.

11.6.1 Why Macroeconomic Orthodoxy Changes So Quickly: Final Remarks

Chapter Two agued that the Social Studies of Science literature has largely neglected macroeconomics, while there is a growing literature in the History of Economic Thought associated with the 'naturalistic turn' (Hands 2001, Weintraub 2002) that make interesting, but sometimes tangential, contributions. This research made some small steps towards addressing the gap in the Social Studies of Science literature. At minimum, this thesis has demonstrated that interesting things can be learnt about the dynamics underlying macroeconomic debate by applying modes of thinking developed elsewhere in the science studies literature. Furthermore, and hopefully more poignantly, interesting things can also be learnt about the dynamics underlying these modes of thinking from the Social Studies of Science literature by applying them to the unique context of macroeconomics.

Appendices

Appendix 1: Tables of Methodological Information

Table One: Respondents by phone or in person interview

Collard, David:	Face-to-face interview	
Copeland, Laurence:	Face-to-face interview	
Cowling, Keith:	Face-to-face interview	
Friedman, Milton:		Telephone Interview
Kuska, Edward:	Face-to-face interview	
Laidler, David:		Telephone Interview
Lipsey, Richard:		Telephone Interview
Modigliani, Franco:	Face-to-face interview	
Mortensen, Dale:		Telephone Interview
Peel, David:	Face-to-face interview	
Perry, George:		Telephone Interview
Phelps, Edmund:	Face-to-face interview	
Solow, Robert:	Face-to-face interview	
Stoney, Peter:	Face-to-face interview	
Sumner, Michael:	Face-to-face interview	
Taylor, Jim:	Face-to-face interview	
Tobin, James:	Face-to-face interview	
Winnett, Adrian:	Face-to-face interview	
Zis, George:	Face-to-face interview	

Table Two: Respondents by position in Phillips Curve debate

	Phillips/Lipsey	Friedman/Phelps	Rational Expectations
Collard, David:			
Copeland, Laurence:		X	
Cowling, Keith:	X		
Friedman, Milton:		X	
Kuska, Edward:	X		
Laidler, David:		X	
Lipsey, Richard:	X		
Modigliani, Franco:	X	X	
Mortensen, Dale:		X	X
Peel, David:			X
Perry, George:	X		
Phelps, Edmund:	X	X	
Solow, Robert:	X		
Stoney, Peter:	X		
Sumner, Michael:		X	
Taylor, Jim:	X		
Tobin, James:	X		
Winnett, Adrian:			
Zis, George:		X	

Appendix 2: Short Respondent Biographies

Collard, David:

David Collard has lectured in economics since 1960. His principle research interests have been welfare economics, public sector economics and the history of economic thought. He has not published on the Phillips Curve debate. Today he is a Professor of Economics at the University of Bath. He was interviewed on the 27/2/01 in his office at Bath.

Copeland, Laurence:

Laurence Copeland has lectured in economics since 1976, when he was based at the University of Manchester and the influential Manchester School Inflation Workshop. His only publication in the Phillips Curve debate was his first paper in 1977, criticising the Key Industry Hypothesis. He has since published on other macroeconomics topics and now specialises in finance. Today he is Professor of finance at the University of Wales Cardiff. He was interviewed on the 16/5/01 in his office at Cardiff Business School.

Cowling, Keith:

Keith Cowling has been Professor of Industrial Economics at the University of Warwick since 1969. Before this he worked at the Agricultural Economics

Department at Manchester University, where he wrote most of his Phillips Curve debate contributions. In the mid to late 1960s he published research disaggregating the Phillips Curve by industry and region. After this period his interest shifted into other areas, and today his interests are the deficiencies of monopoly capitalism, economics and democracy, industrial policy, the dynamics of cities and regions and corporate governance and the public interest. He was interviewed on 17/12/01 in his office at Warwick.

Friedman, Milton:

Milton Friedman has been a professional economist since 1934. His work spans the width of macroeconomics, and he is one of the world's most influential living economists. He was awarded the Nobel Prize in economics in 1976 for his achievements in the fields of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy. His greatest contribution to the Phillips Curve debate was his 1967 Presidential Address to the American Economic Association, that was foundational in establishing the Friedman/Phelps Phillips Curve. Aside of this contribution, his work in the area has been limited. Today he is Professor Emeriti at the Hoover Institution, Stanford University. He was interviewed on 8/2/02 on the telephone.

Kuska, Edward:

Edward Kuska was supervised by Phillips as a graduate student, and attended the Measurement and Methodology Seminar group at the London School of Economics. His only contribution to the Phillips Curve debate came in 1977, and was a theoretical account of the Phillips Loops. He subsequently published on a balance of payments theory through the 1970s and early 1980s. Kuska retired in the summer of 2002 and today lives in the United States. He was interviewed on 12/6/01 in his then office at the LSE.

Laidler, David:

David Laidler has lectured in economics since 1961. In 1969 he moved to Manchester University, from the University of Essex, where he and Michael Parkin were awarded a grant to establish the Manchester Inflation Workshop. During this period Laidler made frequent and influential contributions to the Phillips Curve debate, most prominently in promoting the study of the demand for money and the Friedman/Phelps Phillips Curve in the U.K. context. In 1975 Laidler moved to the University of Western Ontario, where he remains today. His current research interests are monetary policy regimes and the development of monetary economics. He was interviewed on 16/1/02 on the telephone.

Lipsey, Richard:

Richard Lipsey has been a professional economist since 1950. He moved to the LSE in 1955 where he remained until 1963. During that time he worked with Phillips and others at the Measurement and Methodology Seminar group to develop the Phillips/Lipsey Phillips Curve. He moved to Essex University for seven years, and then held visiting appointments at seven universities until 1989, when he joined Simon Fraser University where he remains as Professor Emeritus. Throughout this period he has published widely on a range of economic issues. He was interviewed on 25/2/02 on the telephone.

Modigliani, Franco:

Franco Modigliani entered the economics profession in 1941 and by 1944 had published his first groundbreaking paper linking Keynesianism to neo-classical concepts. In 1960 he joined Massachusetts Institute of Technology where he continued to be at the forefront of the Keynesian neo-classical synthesis and wrote widely on macroeconomics, including the Phillips Curve. In 1985 Modigliani was awarded the Nobel Prize for his work on saving and financial markets. He has remained at MIT to today, where he is currently Professor Emeritus. He was interviewed on 6/11/01 in his home in Cambridge, Massachusetts.

Mortensen, Dale:

Dale Mortensen entered the economics profession in 1964. His research interests in labour economics, macroeconomics and economic theory have remained to today. He was an early advocate of the microfoundations of the Friedman/Phelps Phillips Curve literature, and also later advocated the Rational Expectations Phillips Curve. Today Mortensen is the Ida C. Cook Professor of Economics at Northwestern University. He was interviewed on 28/1/02 on the telephone.

Peel, David:

David Peel has lectured in economics since 1972, when he joined Liverpool University for twelve years. At this time he accepted the Friedman/Phelps Phillips Curve, but by the late 1970s became a committed advocate of the Rational Expectations Phillips Curve. He has published extensively on a wide spectrum of macroeconomic topics and is connected with the Liverpool school most prominently associated with Patrick Minford. Today he is Professor of macroeconomics at Cardiff Business School. He was interviewed on 17/4/02 in his office at Cardiff Business School.

Perry, George:

George Perry became a professional economist in 1961 when he served as Senior Economist for the President's Council of Economic Advisers. Following a brief spell at the University of Minnesota he joined the Brookings Institution where today he is a senior fellow. His PhD was supervised by Robert Solow at Massachusetts Institute of Technology on variations of the Phillips/Lipsey Phillips Curve relationship. Perry has continued to publish widely on macroeconomic issues throughout his career. He was interviewed on 13/12/01 on the telephone.

Phelps, Edmund:

Edmund Phelps entered professional economics in 1958. His first work on the Phillips Curve was his PhD dissertation, supervised by James Tobin at Yale, that supported the Phillips/Lipsey Phillips Curve. After eight years at Yale, and the Cowles Foundation, Phelps moved to University of Pennsylvania for five years and then to Columbia University where he remains today. During the mid 1960s Phelps' work on the introduction of expectations to the Phillips Curve created a foundational basis for the Friedman/Phelps Phillips Curve. He has continued to write extensively on these formulations and the breadth of macroeconomic issues, and is now a leading thinker in the post-Keynesian school. He was interviewed on 9/11/01 in his office at Columbia.

Solow, Robert:

Robert Solow has been a Professor of economics at Massachusetts Institute of Technology since 1950. Now Professor Emeritus, he has continued to write widely on all macroeconomic issues. His 1960 publication, co-authored with Paul Samuelson, raised the profile of the Phillips/Lipsey Phillips Curve greatly. Both authors are central architects of the Keynesian neo-classical synthesis, and both subsequently won Nobel Prizes, Solow's in 1987 for his analysis of economic growth. He was interviewed on 12/11/01 in his office at MIT.

Stoney, Peter:

Peter Stoney has been at the University of Liverpool since 1970. At this time he published two contributions to the Phillips Curve debate with Leighton Thomas. The first a critique of Bertie Hines' Trade Union militancy argument, the second exploring the role of unemployment dispersion. Stoney's attentions then shifted to other areas, and he now focuses on regional economics, with specific reference to Economic Policies for Merseyside and the North West. He was interviewed on 3/1/02 in his office at the Department of Economics and Accounting in Liverpool University.

Sumner, Michael:

Michael Sumner entered the economics profession in 1965 and moved to Manchester University in 1970. He published extensively on the Phillips Curve and wider macroeconomic issues during his time at the Manchester Inflation Workshop, in the Friedman/Phelps Phillips Curve mould. He left Manchester in 1977 for the University of Salford, and then the University of Sussex in 1983 where he remains today.

Sumner has continued to publish on inflation and other macroeconomic topics throughout. He was interviewed on 18/12/01 in his office at Sussex.

Taylor, Jim:

Jim Taylor started his professional life as an economist in 1964 at Lancaster University, where he remains today. He published several papers and a book introducing the concepts of hidden and hoarded labour to the Phillips/Lipsey Phillips Curve debate in the early 1970s. These concepts were developed in his work on regional economics, and after his Phillips Curve publications his focus returned to this area. Today he continues to work in regional economics, and indicators of performance in education. He was interviewed on 3/9/01 in his office at Lancaster.

Tobin, James:

James Tobin started working as a professional economist in 1941. He moved to Yale in 1950 and became director of the Cowles Foundation on two occasions. Throughout his career he has published across macroeconomics, and was a leading figure in the Keynesian neo-classical synthesis. Tobin was awarded the Nobel Prize in 1981 for his analysis of financial markets and their relation to expenditure decisions, employment, production and prices. He remained at Yale until his death in March 2002. He was interviewed on 9/11/01 in his office at Yale.

Winnett, Adrian:

Adrian Winnett has lectured in economics since 1970. His research interests have been monetary economics and growth and capital theory, although he shifted focus towards environmental and natural resource economics. As an undergraduate he was lectured to by Phillips, but never himself contributed to the Phillips Curve debate. Winnett joined the University of Bath in 1973, where he remains today. He was interviewed on 1/3/01 in his office at Bath.

Zis, George:

George Zis has spent his professional life as an economist at universities in Manchester. Initially at the University of Manchester he was a member of the inflation workshop, and co-authored a number of papers discrediting Trade Union militancy explanations of inflation. He later shifted his attention towards Phillips Curve relationships in an international context and under different exchange rate mechanisms. He has continued to publish on these topics throughout his career, and is now based at Manchester Metropolitan University. He was interviewed on 5/10/01 in his office at Manchester Metropolitan University.

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