A METHODOLOGICAL ANALYSIS OF THE DEVELOPMENT OF INTERNATIONAL TRADE THEORY

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I hereby certify that this material, which I now submit for assessment on the programme of study leading to the award of the degree of Phd , is entirely my own work and has not been taken from the work of others save and to the extent that such work has been cited and acknowledged within the text of my own work

Signed [illegible]

Date December '93.

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PREFACE

This disssertation is concerned with methodology and progress in international trade theory, from 1776 to 1981. It is also concerned with the way in which economic methodologists have analysed the extent of progress in economics in general. 'Mainstream economic methodology,' it is argued, has focused on positivist philosophy of science in the hope of divining an objective definition of scientific progress which could be applied to economics. For Blaug, for example, economic methodology can "provide criteria for the acceptance and rejection of research programs, setting standards that will help us discriminate between wheat and chaff" (Blaug 1992:247).

Two positivist philosophies of science are considered below - Popper's falsificationism and Lakatos' methodology of scientific research programmes. These philosophies are considered in the context of rational reconstructions undertaken by economic methodologists of specific events in the history of economic thought. There are two problems with this 'methodology of economic methodology.' Rosenberg pointed out that philosophers of science have long since given up the search for a unique, objective definition of scientific progress. In persisting in this search, Rosenberg argued that economic methodologists "have, as it were, attached themselves to a degenerating research program"(Rosenberg 1986:136). Another problem is the use of rational reconstructions. Lakatos' meta-methodological framework, the methodology of historiographic research programmes(1971a), suggests that philosophers of science should reconstruct the history of a science according to various theories of scientific rationality The preferred theory of scientific rationality is that which manages to explain most of the choices made by scientists in the development of a particular science The underlying assumption of this historiographic method is that scientists actually used the criteria advocated by the preferred theory of scientific rationality

Lakatos' historiographic method has been much criticised by philosophers of science, who argue that, even if there were an optimal theory of scientific rationality, the methodology of historiographic research programmes would be an inadequate method of discovering such an optimal theory Yet, 'mainstream' economic methodology has been devoted in large part to the rational reconstruction of episodes in the development of economic theory Are these economic methodologists using the much maligned methodology of historiographic research programmes, and therefore subscribing to a degenerating research programme as Rosenberg suggests? This dissertation argues that mainstream economic methodologists are not in fact using the method of rational reconstruction in the way proposed by Lakatos Rather, the failure to reveal a 'closeness of fit' between the actual history of economic thought and positivist rational reconstructions has forced mainstream economic methodologists into a Kuhnian-type analysis of what it is that economists actually do This particular use of rational reconstruction by mainstream economic methodology is not, however, a full-blown, sociological Kuhnian analysis This is because economic methodologists have tended to treat Kuhn's philosophy of science as if it were another positivist philosophy advocating an alternative objective theory of scientific rationality Economic methodology, it is argued, has failed to fully take on board, Kuhn's call for a sociological approach to the analysis of scientific progress However, the method of rational reconstruction in economic methodology is not a degenerating one It has yielded many useful insights into the actual practice of economics

Part two of the dissertation considers the use of rational reconstruction as a means of revealing the underlying definitions of progress used by international trade theorists International trade theory is an interesting case study in that economists themselves have tended to pinpoint international trade theory as a relatively unprogressive branch of economics (until recently) Economic methodologists, too, have suggested that international trade theory is lacking in progress Blaug, for example, has described international trade theory as "a field of economic specialization that seems peculiarly prone to the disease of formalism" (Blaug 1992 190) The arguments presented in Part Two of this dissertation show that this notion of international trade theory arises out of a particularly narrow reconstruction of the history of international trade theory

A modified version of Chapter 6, entitled 'A Lakatosian Approach to Change in International Trade Theory' will appear in *History of Political Economy*, no 3, 1994

The accompanying bibliography contains all works referenced in the text In addition, however, there are also included some works which, while not directly referred to in the text, were used as general sources of information

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PART ONE

TWENTIETH CENTURY PHILOSOPHIES OF SCIENCE
AND THEIR INFLUENCE ON ECONOMIC METHODOLOGY

INTRODUCTION

This part of the dissertation considers the influence of three twentieth century philosophies of science - Popper's methodological falsificationism, Kuhn's scientific revolutions, and Lakatos' methodology of scientific research programmes - on economic methodology

Philosophy of science deals with scientific knowledge on three levels Level one is concerned with the method of a science, with its "intellectual accountancy"(Toulmin 1970 553) At this level, philosophers of science debate *how* scientists might choose between scientific theories At the next level, philosophers of science are concerned with *why* certain criteria for theory-choice might be adopted This epistemological analysis considers the underlying rationale to the method adopted by scientists Method and epistemology together form methodology On the third level, philosophers of science deal with an ontological issue - the relation of scientific concepts to reality Philosophers of science have generally confined the bulk of their analysis to the first two levels "the central problem of philosophy of science is the problem of normative appraisal of scientific theories, and, in particular, the problem of stating *universal* conditions under which a theory is scientific" (Lakatos and Zahar 1976 335) However, implicit in methodological analysis is some ontological stance

McMullin classified different philosophies of science according to the way in which their *methodology* was established (McMullm 1970) "Externalist philosophy of science - PSE" presents an *abstract* theory of how science progresses (McMullin 1970 24) This theory of science gives a set of criteria by which scientists may choose between theories in order to ensure that science progresses - the method PSE is abstract in that "it does not rest upon any analysis of the strategies followed by those who would regard themselves as 'scientists'" (McMullin 1970 24) McMullin divided PSE into two categories PSM where M = methodological, and PSL where L = logic In addition to presenting a method, PSM also presents an underlying justification or rationality for the method it espouses PSL, on the other hand, is concerned solely with the construction of scientific theories and their relation to the formal rules of logic (McMullm 1970 25)

In opposition to PSE is "internalist philosophy of science - PSI" (McMullin 1970 27) PSI is an historic theory of science progresses "The response of these [PSI] writers is to say that rationality ought to be defined by what is found in the history and practice of science rather than set out formally in advance and imposed upon history" (McMullin 1978 239) Thus, PSI "presupposes an already-functioning methodology, whose pragmatic success is a sufficient warrant of its adequacy as a heuristic" (McMullin 1970 27)

Both PSE and PSI use the history of science The externalist philosopher dips into the history of a science, intermittently, looking for examples of actual scientific progress that support his particular theory of science The internalist philosopher, on the other hand, scans the history of a science in order to identify the method actually used In both cases, it is most commonly the history of physics which is the focus of analysis

Of the main philosophies of science considered here, Popper's falsificationism is an externalist philosophy and more specifically a PSM, Kuhn's theory of scientific revolutions is an internalist philosophy, Lakatos' methodology of scientific research programmes is, like Popper's, a PSM These three theories of science have had the greatest influence on modern economic methodology

Such has been the influence of philosophy of science in general on economic methodology that it could be described as a branch of philosophy rather than a branch of economics Modern economic methodology has been criticised for not providing the practicing economist with a set of methodological criteria to ensure progress in economics But philosophers of science have long since recognised that it is "an illusion that there can exist in any science methodological rules the mere adoption of which will hasten its progress" (Klappholz and Agassi 1959 74) This plurality of theories of scientific progress has drawn economic methodologists onto another level of philosophical debate Economic methodologists have now been faced with the task of establishing criteria to allow them to choose, not between scientific theories, but between theories of science or methodologies "[S]etting standards that will help us discriminate between wheat and chaff" (Blaug 1992 247), has involved much more debate on philosophical issues than debate on issues pertaining to the actual practice of economics

Does this mean that economic methodology has not managed to meet its aims? This section is concerned with the following questions i, has economic methodology, in an externalist agenda, given methodological prescriptions which practising economists have followed with some success, and ii, has it, in an internalist agenda, identified a progressive methodology within economics?

CHAPTER ONE

1 1 AN OUTLINE OF POPPERIAN FALSIFICATIONISM

Popper's methodological falsificationism is the prime example of an externalist philosophy of science Falsificationism sets down *a priori* a set of criteria or standards by which the scientist chooses between scientific theories in such a way as to ensure that his science progresses Popper's falsificationism developed out of a criticism of the logical positivist school and their particular treatment of Hume's problem of induction

Logical positivism dominated the philosophy of science in the early part of this century The major players were Schlick, Neurath and Carnap in Austria (known as the Vienna Circle), and Ayer in Britain (Hamlyn 1987 306) Logical positivism makes a distinction between meaningful and meaningless statements on the basis of their relationship to observable phenomena Specifically, scientific statements are meaningful because they are verifiable by observation Metaphysical statements, on the other hand, are meaningless because they are not verifiable by observation Logical positivism is commonly presented as a cohesive, almost dogmatic philosophy In fact, there were many debates within the school (Hamlyn 1987 308) What is presented here, are the elements of logical positivism which Popper specifically objected to Popper's primary objection to logical positivism was that it ignored Hume's criticism of the inductive method of verification

Hume (1748) argued that a scientific statement cannot be verified as true on the basis of compatibility with real phenomena, regardless of the number of times this compatibility is observed The acceptance of the inductive method of verification was, Popper argued, a major flaw in the logical positivist philosophy Accepting Hume's criticism means accepting that all statements are equally conjectural in nature, whether metaphysical or scientific Neither type of statement can be verified as true simply by repeated corroborative observations Popper argued that, given this similarity between scientific and metaphysical statements, the only distinction between science and metaphysics could be that which is made on the basis of method, not of meaning (Popper 1979 1-31) The method Popper proposed was falsificationism He argued that, while scientific statements could not be verified by corroborative observations, they could be falsified by contradictory observations Since metaphysical statements cannot be falsified, this provides a demarcation of method between the two modes of thought

It should be pointed out that Popper was opposed to a specific form of induction, "instantionist induction" (Grunbaum 1976 122) This is the notion that scientific hypotheses can be verified *solely* by repetitions of corroborative observations While this form of inductive validation was acceptable to the logical positivists, it was not acceptable to all inductive philosophers Francis Bacon was one of the first to dispute the validity of instantionist induction (Grunbaum 1976 118) He developed tables of presence, absence and degrees to develop a notion of relative appraisal of theories He conceded that instantiomst induction would not verify a theory, but argued that the relative verity of two theories could be established (Losee 1980 65) This notion of relative appraisal was later adopted by Mill, in his methods of agreement and difference (Mill 1843)

The notion that all knowledge is conjectural means that all are equally likely to be true (Popper 1972 Appendix ii) Taking this notion that all knowledge is conjecture to its extreme results m an instrumentalist methodology Instrumentalism holds that science can never explain natural phenomena, but only predict them The incapacity to explain arises out of the acceptance of Hume's argument that scientific theories cannot be verified by repeated confirming instances This, coupled with the *modus ponens* rule of logic that truth cannot be passed backwards from the initial premises of a theory to its predictions, leads to the conclusion that the only way of choosing between scientific theories is on the grounds of their relative success as predictors Science should therefore be composed of predictive devices, not of causal theories For Popper, instrumentalism places an intolerable limit on the scope of science

> If instrumentalism were true, then all scientific theories would be nothing but computation rules Consequently, there could be no fundamental differences between the theories of the so-called pure sciences, such as Newton's dynamics, and those technological computation rules which we encounter everywhere in the applied sciences and engineering (Popper 1983 113)

Popper described instrumentalist science as "an activity of gadget-making - glorified plumbing" (Popper 1983 122) Giedymin took exception to Popper's interpretation of instrumentalism where "instrumentalists deprive theoretical statements of the descriptive functions" (Giedymin 1976 201) For Giedymin, instrumentalism as an epistemological stance "allows not one but several methodological stances" (Giedymm, 1976 203) Popper, on the other hand, implied that an extreme fallibilist stance could only lead to a instrumentalist methodology Popper, while arguing that all knowledge is conjecture, combined his fallibilism with a realist ontology in order to avoid this extreme fallibilism Popper believed that theoretical concepts *can* refer to real entities In order to demonstrate this, he had to give a set of criteria by which scientists could show one scientific theory to be closer to the truth than others

The Popperian methodology of science is "a method of trial and the elimination of errors, of proposing theories and submitting them to the severest tests we can design" (Popper 1979 16) While the scientist can never confirm the truth of his theory, he may satisfy himself that it is a good theory if it stands up to his best attempts to falsify it Popper's theory of science is based on the following theory of rationality the scientist will not accept confirmations of the predictions of his hypothesis as evidence of the hypothesis' truth-status Rather, he will attempt to falsify his hypothesis by subjecting it to severe tests If these tests prove the hypothesis to be false, then the scientist will reject it Popper did mention the possibility of immunizing a theory against criticism by making *ad hoc* adjustments to the theory in order that it remain unfalsified (Popper 1979 30) Such an action, however, clearly contravenes Popper's theory of rationality, and he excluded it from his model of how science should progress

Popper gave three requirements that a new theory must fulfil if it is to be characterised as a good theory

> The new theory should proceed from some simple, new, and powerful, unifying idea about some connection or relation between hitherto unconnected things or facts or new "theoretical entities"
>
> we require that the new theory should be independently testable
>
> We require that the theory should pass some new, and severe, tests (Popper 1963 241-242)

The criteria upon which Popper judged scientific theories are, therefore, novelty, testability, and success in passing tests All three criteria must be met before the theory can be said to constitute an addition to scientific knowledge If all three are satisfied they ensure that the newly proposed theory is not *ad hoc* Popper defined an *ad hoc* theory as one which seeks to explain a phenomenon by using that phenomenon in the construction of the theory as a theory which employs circular reasoning Popper argued that the criterion of independent testability prevents the acceptance of such *ad hoc* theories as valid The criterion of independent testability asserts that a theory must be testable in a way that is independent of the phenomenon it is attempting to predict The theory must also actually *be* tested It is the actual refutation of a theory which, for Popper, marks scientific progress New theories are built upon the refutations of old theories They both encapsulate and contradict the old, refuted theories

> the new theory, although it has to explain what the old theory explained, *corrects* the old theory, so that it actually *contradicts* the old theory it contains the old theory, *but only as an approximation* Thus I pointed out that Newton's theory contradicts both Kepler's and Galileo's theories - *although it explains them*, owing to the fact that it contains them as approximations (Popper 1979 16)

It is the continual process of conjecture and refutation which gives science its dynamic, in Popper's methodology This process turns on the test to which scientists submit their theories But this test, in turn, assumes the prior acceptance, by the scientist, of some other scientific knowledge as true Without this prior acceptance, the result is an infinite regress Popper conceded this

> Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept* If we do not come to any decision, and do not accept some basic statement or other, then the test will have led nowhere (Popper 1972 104)

For Popper, scientific theories are built upon the foundation of "background knowledge" (Popper 1972 102) The background knowledge of the natural scientist is comprised of "basic statements" and "universal laws" (Popper 1972 102) Universal laws are those regularities perceived in nature, which, although their future existence cannot be guaranteed, are for the most part unquestioned by scientists Basic statements are "singular existential statements" which have themselves been severely tested and are yet to be falsified (Popper 1972 102) The scientific community takes a decision as to which statements are to be included in background knowledge Agreement on basic statements is part of the process of testing a theory "Agreement upon the acceptance or rejection of basic statements is reached, as a rule, on the occasion of *applying* a theory, the agreement, in fact, is part of an application which puts the theory to the test" (Popper 1972 106)

This agreement between scientists to hold some knowledge as foundational smacks of conventionalist philosophy of science Conventionalism stresses a number of criteria, including simplicity, clarity and mathematical precision, to choose between scientific theories These criteria have their roots in the ontological beliefs of the middle ages, such as Occam's belief that nature always follows the simplest course None of the criteria suggested by conventionalism are empirical For this reason, Popper argued that the conventionalist will remain undisturbed by falsifications of his theory "he will explain away the inconsistencies which may have arisen, or he will eliminate them by suggesting *ad hoc* the adoption of certain auxiliary hypotheses" (Popper 1972 80) For Popper, such *ad hoc* adjustments to a theory were the mark of bad science Popper distinguished between falsificationism and conventionalism by arguing that, while he instructed the scientist to hold some knowledge as foundational, the scientist should never hold this knowledge to be true

Individual basic statements are never elevated to the status of universally true statements They remain potentially falsifiable, although their ability to defy severe tests makes them candidates for background knowledge Thus, falsificationism is distinguished from both conventionalism and positivism

> I differ from the conventionalist in holding that the statements decided by agreement are not *universal* but *singular* And I differ from the positivist in holding that basic statements are not justifiable by our immediate experiences, but are, from a logical point of view, accepted by an act, by a free decision (Popper 1972 109)

The background knowledge should only be accepted tentatively by scientists Popper did concede that in practice, "almost all of the vast amount of background knowledge which we constantly use in any informal discussion will, for practical reasons, necessarily remain unquestioned" (Popper 1963 238) Popper pointed to "something like a law of diminishing returns from repeated tests" (Popper 1963 240) By this Popper meant that in order to maintain the severity of tests, scientists should review background knowledge regularly

Relative appraisal, in Popper's methodology, is dependent on the relative extent to which theories are corroborated Popper defined corroboration in the following way

> By the degree of corroboration of a theory I mean a concise report evaluating the state (at a certain time t) of the critical discussion of a theory, with respect to the way it solves its problems, its degree of testability, the severity

of tests it has undergone, and the way it has stood up to these tests Corroboration is thus an evaluating report of past performance (Popper 1979 18)

A theory with a high degree of corroboration does not assume truth-status It can, however, be said to be a better theory than those which have failed tests The theory is better in the sense that it is "a better approximation to the truth" (Popper 1979 47) Popper argued that the degree of corroboration of a theory (relative to some other) is indicative of that theory's verisimilitude It is not, he was anxious to point out, a *measure* of versimilitude of a theory (Popper 1979 103) This would have been too close to another theory of induction, for Popper's comfort (Popper 1979 103) But what of the common situation where the scientist is confronted with two false theories? In this case, falsificationist methodology would appear to have him discard both Theories are, of necessity, *limited cognitive constructions* of real phenomena As such, it is likely that *all* theories will be falsified by at least one piece of empirical evidence Where theories are probabilistic rather than deterministic, there may simply be too much falsification to allow science to progress by discarding *all* falsified theories Popper recognised this, and used the concept of corroboration to develop a means by which scientists might choose between false theories

Popper argued that the concept of corroboration, indicating as it does verisimilitude, allows him to "conjecture that Einstein's theory of gravity is *not true*, but that it is a *better approximation to the truth* than Newton's" (Popper 1979 335) Thus, verisimilitude is to do with the relative appraisal of *false* theories It introduces the notion that it is possible to compare in some way the relative degree of falsity within two theories Faced with a plethora of false theories, versimilitude can give the scientist an indication of which are the best theories to hold on to and which should be discarded

Grunbaum showed that the notion that scientists can make a choice between two false theories conflicts with Popper's earlier insistence that two false theories each have a truth probability of zero (Grunbaum 1976 127) If Popper retained the premise that the truth probability of *all* false theories is zero, then his theory of "quantitative verisimilitude" cannot logically hold, there can be no empirically based method of choosing between two false theories (see also Miller 1985) If scientists deal with theories which are probabilistic in nature and therefore likely to fail at least one test, then falsificationism cannot provide a methodology which ensures progress Verisimilitude is not compatible with falsificationism, and cannot save it

It is difficult to see how the concept of versimilitude can be workable - how scientists can choose between two equally false theories - unless some inductive criteria are permitted Lakatos added these inductive criteria in his methodology of scientific research programmes (Lakatos 1970) This philosophy of science and its impact on economic methodology is examined in chapter three

Popper himself indicated that the theories of the social sciences are false because they are over-simplified (Popper 1976 103) Yet, he argued that they can be relatively assessed in order to establish which are the best approximations to the truth (Popper 1976 103) This indicates that verisimilitude is a particularly important concept with respect to theory-choice in the social sciences (Hands 1991 69) But it also indicates that falsificationism cannot be the optimal methodology for the social sciences Section 1 4 considers how a falsificationist agenda in economics, and particularly in econometrics, reflects these problems in Popper's falsificationism Before an examination of this issue, however, Popperian falsificationism is compared to a methodology developed by a practising economist - Friedman's methodology of positive economics

1 2 ON FRIEDMAN'S METHODOLOGY OF POSITIVE ECONOMICS AND FALSIFICATIONISM

The two main influences on economic *methodology* have been Popper's falsificationism, and Friedman's methodology of positive economics Friedman's paper 'The Methodology of Positive Economics' (1953 hereafter MPE) has been described by Caldwell as a "marketing masterpiece" because of the longevity of the debate it sparked off (Caldwell 1984 226)

Classifying Friedman's methodology is problematic This is compounded by the fact that Friedman was somewhat schizophrenic with regard to the philosophical influences underlying his methodology While Friedman agreed with Boland that his was an instrumentalist methodology (Caldwell 1984 226), he intimated to Hirsch that he could see no sources of conflict between his methodological framework, and that of Popper (Hirsch and de Marchi 1990 6) Frazer and Boland (1983) attempt to rationalise Friedman's position by defining him as a short-run instrumentalist but a long-run Popperian Hands argued that, with the failure of the concept of verisimilitude, Popper's methodology deflates to instrumentalism (Hands 1991 75) If this is so, then the distinction between Friedman (as instrumentalist) and Popper is automatically removed

Given the extent of the debate over MPE, it is surprisingly non-contentious, which perhaps explains the narrowness of the response to it "The philosophical response to this article, which is in fact extensive, is also embarrassing for its concentration on philosophical minutiae" (Ackermann 1983 390) Stanley argued, not so much that Friedman's paper is non-contentious, but that it is ambiguous in the extreme (Stanley 1985 307) Stanley criticised Friedman for his failure to state clearly his methodological position "a simple reference to the literature or a single explicit statement could have avoided the decades of confusion and senseless debate this essay has generated" (Stanley 1985 307) This section examines the main aspects of this debate, in the light of Stanley's objections to it, and considers whether any final conclusions can be drawn on the relationship between the methodology of positive economics and Popper's falsificationism

The core of MPE is represented in the following quotation

> a theory cannot be refuted by comparing its 'assumptions' directly with 'reality' Indeed, there is no meaningful way in which this can be done Complete 'realism' is clearly unattainable, and the question whether a theory is realistic 'enough' can be settled only by seeing whether it yields predictions that are good enough for the purpose in hand or that are better

than predictions from alternative theories (Friedman 1984 237)

There are a number of different methodological issues contained in this quotation Different economic methodologists have picked up on particular issues in order to attempt to classify Friedman's methodology

Boland argued that at the core of Friedman's paper is a recognition of two basic rules of logic (Boland 1979) *Modus ponens* is the rule of logic which states that truth is passed forward in a deductive argument from initial assumptions to conclusions It is the corrollary of the *modus tollens* rule that falsity is passed backwards from the conclusion to at least one of the initial assumptions The implication of *modus ponens* is "*if* your argument is logical, then whenever *all* of your assumptions (or premises) are true *all* of your conclusions will be true as well" (Boland 1979 504) According to Boland, Friedman was arguing that *reverse modus ponens* and *reverse modus tollens* are illogical (Boland 1979 512) According to Boland, Friedman was arguing, i that a theory whose predictions are not falsified by observation is not necessarily based on true assumptions, and ii that a theory whose initial assumptions are false will not necessarily produce false predictions

Friedman was particularly concerned with how an economist might choose between two equally successful predictors In this case, he argued that the choice cannot be made on the basis of *reverse modus ponens* The economist cannot hold the theory with the more realistic assumptions to be the better theory, because truth is not passed backwards from conclusions to premises but rather forwards from premises to conclusions

The emphasis which Friedman placed on prediction prompted Boland to classify him as an instrumentalist (Boland 1979 503) An instrumentalist interpretation of Friedman's MPE was a common one (see, for example, Bear and Orr 1967, Coddington, 1972, Wong, 1973)

In traditional, Berkeleyian instrumentalism, theorists are concerned solely with the predictive accuracy of their theories Theories are used solely as tools or computational techniques for prediction, not as explanatory devices Newtonian mechanics, for example, becomes solely a tool for prediction If these laws of mechanics are concerned only with prediction, their initial assumptions need bear no resemblance to actual phenomena

In MPE, Friedman did stress the importance of the prediction of, as distinct from the explanation for, economic phenomena He argued that "the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience" (Friedman 1984 214) There is quite substantial evidence in MPE of instrumentalist prescription This interpretation of Friedman as an instrumentalist was first suggested by Samuelson (Samuelson 1963 231) Samuelson interpreted Friedman as saying that "the (empirical) unrealism of the theory "itself," or of its "assumptions," is quite irrelevant to its validity and worth" (Samuelson 1963 232) He called this, Friedman's "F-twist" (Samuelson 1963 231) Not only did Friedman believe the truth-status of theories to be irrelevant, Samuelson suggested that Friedman also believed "it is a *positive merit* of a theory that (some of) its content and assumptions be unrealistic" (Samuelson 1963 233, italics not in original).

Samuelson's definition of unrealism differs from Friedman's, and as a result, Samuelson misrepresented Friedmans' methodological position Samuelson equated 'unrealistic' with 'false' in his discussion of the "F-Twist" Friedman, Samuelson argued, "is fundamentally wrong in thinking that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis" (Samuelson 1963 233) Musgrave pointed out that Friedman's error "stems from unclarity about what is stated by a negligibility assumption" (Musgrave 1981 380) Friedman was concerned, not with the "factual inaccuracy" of a theory, but rather with its "descriptive inaccuracy" (Friedmand 1984 218)

Friedman held theories to be descriptively inaccurate, in the sense of being descriptively incomplete a theory is "descriptively inaccurate" where "it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained" (Friedman 1984 218) Thus, for Friedman, theories are necessarily unrealistic and false because they are *incomplete* descriptions This descriptive limitation does not matter, according to Friedman, if the theory is an accurate predictor, because predictive accuracy shows that the theorist has captured all the necessary independent variables in his theory

> A meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in a world of observation *as if* they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important (Friedman 1984 236)

For Musgrave, the classification of Friedman as an instrumentalist is borne out of Friedman's failure to clarify his particular interpretation of the falsity of a theory (Musgrave 1981 380)

Hirsch and de Marchi (1990) argued strongly against Boland's interpretation of Friedman as an instrumentalist, on the grounds that if Friedman were an instrumentalist, he would have discarded theory altogether and concentrated solely on correlations in his practice of economics Hirsch and de Marchi argued that, while on the grounds of MPE alone, Friedman appeared to espouse an instrumentalist methodology, in practice he made every attempt to ensure the realism of initial assumptions (Hirsch and de Marchi 1990 155) Despite the fact that economists will, of necessity, derive unrealistic, "as if" hypotheses, "for Friedman these "as if" accounts are not so many hot-air balloons floated freely aloft They are firmly anchored by the problems and data one starts with" (Hirsch and de Marchi 1990 155)

There is an important epistemological foundation to instrumentalism In contrast to realism, instrumentalism maintains that theories do not pertain to phenomena, they have no truth-status It is difficult to conclude that Friedman adopted this stance in the light of Hirsch and de Marchi's elucidation of the methodology inherent in Friedman's practice of economics

It is perhaps in Friedman's 1944 review of Lange's 'Price Flexibility and Unemployment' that one finds the clearest description of Friedman's methodology "The theorist starts with some set of observed and related facts" (Friedman 1944 618) This presupposes some theory about which facts are related Friedman made this point in MPE "A theory is the way we perceive 'facts,' and we cannot perceive "facts" without a theory" (Friedman 1984 232) The theorist "seeks a generalization that will explain these facts" (Friedman 1944 618) In the following elucidation of Friedman's methodology, it appears that explanation has an equally important role to prediction

> [The researcher] tests his theory to make sure that it is logically consistent, that its elements are susceptible of empirical determination and that it will explain adequately the facts he started with He then seeks to deduce from his theory facts other than those he used to derive it, and to check these deductions against reality Typically some deduced "facts" check, others do not, so he revises his theory to take account of the additional facts (Friedman 1944 618)

Both in this paper, and in MPE, Friedman expressed the belief that theories must be constructed in such a way as to make them potentially falsifiable For Friedman, the "crucial question" is "[w]hat observed facts would contradict the generalization suggested" (Friedman 1944 618) Thus, MPE contains shades of falsificationism However, Friedman was not prepared to reject a probability hypothesis on the grounds of just a single falsification For Friedman, a successful hypothesis is one which is an accurate predictor, *most of the time* But if the occasional falsification is allowed, then how do economists choose between probability hypotheses?

Popper attempted to resolve this issue using the degree of corroboration as an indicator of a theory's verisimilitude (Popper 1972 335) Friedman, on the other hand, adopted conventionalist criteria

> The choice between alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness,' themselves notions that defy completely objective specification (Friedman 1984 215)

Friedman argued that these conventionalist criteria were employed by econometricians in order to solve the identification problem (Friedman 1984 240) Friedman conceded that such a process is "entirely arbitrary" but insisted that it was the only way of "selecting among the alternative hypotheses equally consistent with the evidence" (Friedman 1984 240)

From 1948 until his publication of his re-specification of the Quantity Theory m 1957, Friedman compiled monetary statistics Hirsch and de Marchi argued that this preliminary empirical investigation showed that Friedman was concerned to make "concrete problems and carefully compiled data" the foundation of his analysis (Hirsch and de Marchi 1990 155)

This starting point of the development of a theory is also described m MPE Friedman stressed that "empirical evidence is vital at two different, though closely related, stages in constructing hypotheses and in testing their validity" (Friedman 1984 217) The development of a theory could only begin with "full and comprehensive evidence in the phenomena to be generalized or 'explained'" (Friedman 1984 217) This will ensure that the hypothesis is not "contradicted in advance by experience that has already been observed" (Friedman 1984 217) The next step was to derive from this hypothesis, "new facts capable of being observed but not previously known and checking those deduced facts against additional empirical evidence" (Friedman 1984 217)

This methodology appears to be very similar to Mill's inductive method However, Hirsch and de Marchi argued that Friedman's methodology owes more to American pragmatism, and in particular that of Dewey, than it does to Mill's empiricism (Hirsch and de Marchi 1990 chapter 6)

Friedman's work on monetarism took the form of a curious loop from observation to deduction and back to observation, until the final specification of the theory - the Permanent Income Hypothesis - was tested in 1963

Friedman's 1948 paper provides evidence of his belief m the overriding importance of monetary transmission mechanisms in price and income determination, *prior* to his collection of monetary statistics Friedman cannot therefore be said to be concerned in the first instance with measurement to the exclusion of any notion of an underlying theory By 1948, Friedman was able to conduct an indirect comparison of the predictive ability of the Keynesian income/expenditure model and the Quantity Theory, of price and income changes in three wartime periods This suggests that Friedman did have at least an embryonic version of a theory of demand for money prior to his restatement of the Quantity Theory in 1957 During the late fifties and early sixties, Friedman refined the Quantity Theory and added to it the Permanent Income Hypothesis as an alternative to the Keynesian consumption function In 1963, Friedman and Meiselman undertook a test of the relative accuracy of the predictions of consumption by the income/expenditure model and by the Quantity Theory for the period 1897 to 1958 Theorists were not happy to accept Friedman and Meiselman's conclusion that the Quantity Theory is a more accurate predictor Instead there developed an argument over the validity of the methods used to test the theories, and the specifications of the models used to represent them That Friedman and Meiselman's conclusions led to debate over the nature of tests and models rather than to debate on the relative validity of the theories, is not surprising given the lack of consensus in econometrics over which tests are conclusive and which models are complete specifications of particular theories

Tests cannot be conclusive if economists do not agree as to which are the crucial tests In a reply to Ando and Modigliani, and to DePrano and Mayer, who were the main critics of the Friedman/Meiselman tests, Friedman and Meiselman remarked "[a]mong us, we have produced more measures than there are critics [a]nd all of us harbour serious doubts about the measures we settled on" (Friedman and Meiselman 1966 754) Friedman's concern with predictive accuracy forced him to concede that he may have chosen a test which was biased against the income/expenditure theory and "as a result may have led to too sweeping a conclusion about its lack of conformity with experience" (Friedman and Meiselman 1966 784) Friedman was, of course, fully aware of the arbitrariness with which different models of a theory are adopted "it is an entirely arbitrary subdivision of the process of deciding on a particular hypothesis that is on a par with many other subdivisions that may be convenient for one purpose or another or that may suit the psychological needs of particular investigators" (Friedman 1984 240)

This to-ing and fro-ing between hypothesis and observation, before arriving at an ultimate hypothesis is described by Hirsch and de Marchi "We have to do here, then with a continuous process of inquiry in which observation, the derivation of hypotheses, the testing of implications and the use of revised hypotheses in generating new, testable implications, succeed each other in a never-ending round" (Hirsch and de Marchi 1990 157) This "continuous process of inquiry" sets Friedman's methodology apart from that of Mill Friedman himself noted this distinction (Friedman 1962, quoted in Hirsch and de Marchi 1990 45) In addition, where Mill derived laws from introspection, Friedman attempted to derive them from objective data

Positive economics, then, starts from observed correlations These take the place of the universal regularities of the natural sciences Hypotheses are deductively formed to attempt to predict these regularities These hypotheses are specified as stochastic hypotheses As such, it is accepted that they will be false in general What is hoped for, is to find a particular hypothesis which will be a successful predictor most of the time To this end, a hypothesis is compared to sample data and adjusted where appropriate, to account for any previously omitted variables This process should ensure that no theory is accepted unless it has a reasonable level of success as a predictor Thus, Friedman does appear to be a short-run instrumentalist, as Frazer and Boland argued (Frazer and Boland 1983) However, Friedman cannot be considered a long-run Popperian

The main methodological problem for the 'positive economist' is in choosing between two equally successful predictors In this case, the economist cannot legitimately choose between the theories on the basis of the relative realism of their initial assumptions (In any case, the continual appeal to sample data throughout the development of the hypothesis is likely to ensure that the initial assumptions do reflect real phenomena) The 'positive economist' must, at this stage, resort to arbitrary criteria for theory-choice such as simplicity, clarity, or precision The 'Popperian economist,' on the other hand, would attempt to establish the relative degree of corroboration of each theory in order to indicate the relative verisimilitude of each theory

In practice, however, neither the 'positive economist' nor the 'Popperian economist' is likely to reach this stage of evaluation This is because the potential for several different models to specify a theory makes it impossible to judge between theories on the grounds of how well models predict Thus, Friedman's methodology of positive economics reaches the same impasse as Haavelmo's probabilistic methodology in econometrics Friedman, like Haavelmo, was stuck at a level of theory-development prior to the issue of relative theory-choice For philosophy of science, the only way out of this impasse was to permit inductive criteria in the evaluation of scientific theories (Lakatos 1970) Economic methodologists, however, continued to debate the relevance of falsificationism for economics for nearly two decades more (The last comprehensive study of the Popperian influence in economics was de Marchi (1988) This text had the air of finality about it, notable even in its title, *The Popperian Legacy in Economics*)

1 3 DO ECONOMISTS FALSIFY?

The extent to which Popperian *terminology* has successfully infiltrated almost all fields of economics has been well documented (for example, Blaug 1980, 1992, Caldwell 1982, 1991, de Marchi 1988) However, the extent to which Popperian *methodology* has been adopted by practising economists is less certain It has been argued that economists pay only "lip service" to the methodological prescriptions entailed in falsificationism (de Marchi 1976 109) The most obvious place to look for evidence of falsificationism in economics is the field of econometrics, since it is econometrics which seeks to quantify economic theories and compare these theories to observable economic phenomena This section considers those economists who directly introduced Popperian methodology into economics, and considers the role of falsificationism in econometrics

Popper came to the London School of Economics in 1946 His appointment to the philosophy department was particularly encouraged by the chair of economics at the time, Lionel Robbins Popper professed to have gained a knowledge of the nature of economics from Robbins (Popper 1957 143) In the *Poverty of Historicism* (1957), Popper refered the reader to Robbins' 1935 text, *An Essay on the Nature and Significance of Economic Science* Despite this link between Popper and Robbins, there ıs a conflıct between Popper's methodologıcal prescrıptıons for economıcs and the common ınterpretatıon of Robbın's vıews on the nature of economıcs

Popper held that the only dıfference between economıcs and the natural scıences ıs the extent to whıch successful testıng can be carrıed out

> In physıcs, for example, the parameters of our equatıons can, m prıncıple, be reduced to a small number of constants - a reductıon whıch has been successfully carrıed out m many ımportant cases Thıs ıs not so ın economıcs, here our parameters are themselves ın the most ımportant cases quıckly changıng varıables Thıs clearly reduces the sıgnıficance, mterpretabılıty, and testabılıty of measurements (Popper 1957 143)

Popper acknowledged the dıfficultıes mvolved ın quantıfyıng economıc theorıes, but stıll maıntaıned that attempts should be made sınce, for Popper, falsıficatıon was the optımal form of crıtıcısm Robbıns, on the other hand, ıs often depıcted as stressıng the qualıtatıve nature of economıc theory and as denyıng the possıbılıty of derıvıng quantıfıable and testable economıc theory (for example, Rosenberg 1983 309, de Marchı 1988 144) It ıs dıfficult to see how Popper could have claımed hıs vıews on quantıficatıon and falsıficatıon ın economıcs to be consıstent wıth Robbın's vıews on the nature of economıcs as they are most commonly ınterpreted

Robbins, while he did not place much importance on the quantification of economic theory, was not anti-quantification (O'Brien 1988, see also the debate between Hutchison and de Marchi in de Marchi 1988) There is some evidence in support of O'Brien's interpretation in Robbin's 1938 paper Here, Robbins argued that "the appropriate method of economics is the construction and development of hypotheses suggested by the study of reality and the testing of the applicability of the results by reference back to reality" (Robbins 1938 346) Indeed, he called for more quantification in economics "there is not yet nearly enough quantitative investigation of the applicability of the conclusions to which recent theoretical developments have given rise" (Robbins 1938 347) For Robbins, therefore, economics was fundamentally an empirical science, and a science which could be subjected to empirical testing although he did not distinguish between testing for verification or falsification However, Robbins did advocate a division of labour in economics While he conceded that quantification might be a useful and, indeed, necessary exercise, it was one he was not overly anxious to get involved in

Hutchison's 1938 text marks the introduction of Popperian falsificationism into economics However, Hutchison's particular brand of falsification differs substantially from that of Popper These differences were discussed by Klappholz and Agassi (1959) Hutchison identified two types of scientific statement - those which are falsifiable by observation, and those which are not According to Hutchison, any statement which is not falsifable can only be a tautology (Hutchison 1938 42) For Hutchison, any statement which contains a *ceteris paribus* clause is tautologous This is clearly untrue While such a statement may be untestable if the *ceteris paribus* clause remains unspecified, it is not necessarily tautologous (Klappholz and Agassi 1959 63-64) Hutchison contested that, due to the use of *ceteris paribus* clauses, many economic hypotheses were unfalsifiable

This is perhaps the reason for his application of falsification, not to the predictions of economic hypotheses as in Popperian falsification, but to their initial premises This insistence that every part of a theory must be tested, not simply that theory's predictions, led to Hutchison being classified as an ultra-empiricist by Machlup (Machlup 1978 141) The problem with such ultra-empiricism is that it runs the risk of ending up as a naive form of falsificationism If every part of knowledge is to be tested, then what knowledge is to be used as a test? Hutchison argued that economic hypotheses had to be bolder, to exclude more by eliminating *ceteris paribus* clauses This would certainly be in line with Popper's recommendations However, rather strangely, Hutchison went on to argue that economic hypotheses "need not actually be tested or even be *practically* capable of testing under present or future statistical investigation" (Hutchison 1938 10) It was enough for Hutchison that economic hypotheses be potentially testable This is clearly not compatible with Popperian prescriptions Yet, arguing that theories need only be *potentially* testable gets neatly around the problem, to the ultra-empiricist, of defining the test

Hutchison's call for greater quantification in economics was mirrored by similar calls in the other social sciences in the 1930s The piecemeal statistical work on economic theory and data was, by the 1930s, pulled together to form econometrics

The role of early econometrics was the conversion of economic theory into quantifiable laws The aim was the objective measurement of the parameters of qualitative economic theory (Morgan 1990 229) There was also a suggestion that econometrics could pass an objective critical eye over economic theory There was a confidence m this early period, that "future statistical investigations may lead to verification, revision, or possibly, entire restatement of some economic laws" (Persons 1925 195) It was through econometrics that economics could attain the status of a quantitative science similar in nature to physics

However, Morgan showed how this early confidence soon diminished in respect of demand analysis, when econometricians were faced with the evidence that one of the fundamental laws of economics, demand theory, did not correspond to observed data (Morgan 1990 146) The initial reaction to these discrepancies was to regard economic theory as implicitly correct and to detrend data to bring them closer to the predictions of the theory of demand (Morgan 1990 146) This adjustment process "gradually gave way to the realisation that economic theory had little to say about dynamic elements such as the course of economic change and the timing of economic reactions" (Morgan 1990 146) Instead of attempting to make data conform to the qualitative law of demand, econometricians changed their stance Quantitative models serve as the interface between the economic theory and the observed data Where the theory and the data failed to correspond, econometricians changed the particular model of the theory to make it more fully reflect the data

To a naive falsificationist, this would be interpreted as an immunising strategy designed to save the theory in the face of conflicting evidence However, these early econometricians highlighted a number of problems which prevented the falsification and rejection of qualitative theory It was realised that the conversion of qualitative theory into quantitative model meant that a number of auxiliary assumptions had to be made This raised the question, for every model, as to whether it was a correct specification of the underlying theory Depending on the auxiliary assumptions made, a qualitative theory could be represented by a number of different models This problem of exact specification of the theory was further compounded by the identification problem In the 1920s, econometricians recognised the difficulties involved in *isolating* estimates of single parameters the regression of quantity on price could be an estimate of demand parameters or supply parameters, or most likely of both Econometricians were thus faced with a number of intervening problems which prevented them from directly testing qualitative economic theory What might seem like an immunising strategy was in fact the struggle to find the correct quantitative specification of the law of demand Without some prior agreement as to this specification, econometricians could not test the law of demand to either confirm *or* refute it

Implicit in Popper's falsificationism is the assumption that scientists have agreed, *a priori*, on which are the severest tests of theory This, in turn, presupposes that scientists are agreed on the quantitative form of theory This level of agreement did not exist in econometrics The difficulties which econometricians incurred with regard to the specification of theory, led them to focus more on statistical relations in sample data These investigations were aided by the use of ordinary least squares (OLS) (OLS, though it was developed in the late nineteenth century, only became extensively used by econometricians in the 1940s, 1950s, and 1960s)

Some econometricians, and most economic theorists, saw a danger in this preoccupation with statistical relations m data Koopmans' (1947) critique of Burns and Mitchell's work on the business cycle is indicative of the divisions which this turn in the focus of econometrics created Koopmans, as director of the Cowles Commission, criticised Burns and Mitchell's quantitative analysis of business cycles for the NBER in 1946, as containing nothing but statistical correlations The danger he saw in analysis of this nature was the replacement of causal laws with purely statistical ones That Burns and Mitchell had eschewed the causal laws of economics was the core of Koopmans' attack "the tool-kit of the theoretical economist is deliberately spurned Not a single demand or supply schedule or other equation expressing the behaviour of men or of the technical laws of production is employed explicitly in the book, and the cases of implicit use are few and far between" (Koopmans 1947 163) Koopmans argued that theory was "an indispensable element in understanding in a quantitative way the formation of economic variables" (Koopmans 1947 166) Without an underlying theory, Koopmans argued that Burns and Mitchell were engaged in "methodological quasi-theory", in measurement without theory (Koopmans 1947 165) The tables were turned on Koopmans, when Vining in his defence of the NBER methodology, argued that the Walrasian assumptions underlying the econometric analysis of the Cowles Commission were too weak to be considered a theoretical foundation to such analysis (Morgan 1990 54-55) This distinction between what Koopmans considered to be econometrics on the one hand, and statistical analysis on the other, persists today in the distinction drawn between econometric modelling and autoregressive time-series analysis

Econometricians continued to be frustrated by the seemingly unbridgeable gap between economic theories and observed data, and the methodological problems this gap caused In 1944, Haavelmo presented an alternative methodology, which approached this gap from a new angle (Morgan provides a comprehensive analysis of the impact of Haavelmo's probabilistic methodology on econometrics in Morgan 1990 chapter 8)

Haavelmo believed economic theory to be too inexact ever to correspond in a deterministic way with observed data This was something which had been recognised by econometricians working on dynamic demand theory ten years previously Haavelmo's proposal was that economic theories be rewritten as the "hypothetical probability models" they were, and the gap between theory and observation be reconsidered in this light (Haavelmo 1944 52) This reconstruction of economic theory would, Haavelmo argued, be more on keeping with the probabilistic nature of the statistical methods of analysis, and would allow statistical analysis to be used as the test of theories (Morgan 1990 243)

With regard to testing, an important feature of Haavelmo's re-specification of economic theories as probability hypotheses is the argument that the sample data, too, are subject to error in collection (Haavelmo 1944 18, quoted in Morgan 1990 246) Haavelmo's answer to this probabilistic nature of sample data was to make allowances for sample errors within the probability hypothesis itself As Morgan pointed out, there is nothing new in this "indeed, it provides a good description of the ad hoc statistical practices of the early econometricians" (Morgan 1990 246) What Haavelmo suggested was simply an alternative way of re-specifying the model to bring it closer to the observed data, by taking account not only of errors in the specification of the relation between the theory and the model, but also of errors between the population and the sample data

The errors in statistical models would be explicitly accounted for by probability laws, and the analysis of these probability errors would provide a way (using Neyman-Pearson tests) of choosing between models Haavelmo showed that this methodology does not in fact solve the problems encountered by the early econometricians with regard to model-choice, since "the same observable results may be produced under a great variety of different probability schemes" (Haavelmo 1944 49) A range of different models could be subject to the same probability laws, and therefore, could all conform to observed sample data

While Haavelmo's work may *appear* to mark "the shift from the traditional role of econometrics in measuring the parameters of a given theory to a concern with testing those theories" (Morgan 1990 257), it is clear from his presidential address to the Econometric Society in 1958, that Haavelmo believed no great change in the focus of econometric study to have occurred as a result of his work Econometricians still held their role to be the carrying out of "general 'repair work' upon the logical consistency of theories", rather than the carrying out of tests on theory (Haavelmo 1958 354)

Apart from the practical problems of any potential falsificationism in econometrics, Haavelmo's re-specification of economic theory as probability hypotheses presents another limit to any Popperian stance in econometrics The specification of economic theory not as deterministic but as probabilistic hypotheses, involves the acceptance that all economic theories are false over some range of observations The focus for methodology in this case, then, is the setting of criteria by which economists might choose between two false theories Popper recognised that physical theories, too, might be probabilistic in nature, and introduced the concept of verisimiltude to provide a means by which choices could be made between false theories However, this concept cannot be reconciled to Popper's falsificationism (see section 1 1) Thus, Haavelmo's probability method excludes Popperian falsificationism as a means of theory-choice in econometrics Despite the fact that Haavelmo's methodology excluded falsificationism in econometrics, the Popperian influence in economics persisted, reaching its zenith in the 1950s

It was clear, by the 1950s, that econometrics could not be used to test economic theories, without some *a priori* methodological criterion to confirm a particular model as fully representative of a qualitative hypothesis Despite this acknowledged difficulty in testing economic theory, Darnell and Evans identified a renewed confidence m the scope of econometrics in the 1950s and 1960s (Darnell and Evans 1990 40) They relate this new-found optimism to Friedman's 1953 paper, 'The Methodology of Positive Economics,' and to Lipsey's 1963 text, *An Introduction to Positive Economics* Friedman's paper is the subject of the previous section Lipsey's work is considered below

By the late 1950s, a group of economists at LSE were beginning to become familiar with Popperian falsificationism Their interest in this methodology arose mainly from their dissatisfaction with the anti-empirical stance of Robbins The main protagonists of this group were Richard Lipsey and G C Archibald Each was concerned with a different aspect of empiricism in economics "If quantification was Lipsey's mission, reconsidering theory with an eye to testing was Archibald's" (de Marchi 1988 145) Lipsey was concerned with the establishment of the universal laws of economics to confirm causal laws with statistical evidence Archibald was concerned more directly with the introduction of Popperian criteria for theory-choice into economics

Archibald (1959) focused on the theory of the firm This turned out to be an unfortunate choice He pointed to the methodological schism in this field, between those who continued to analyse firm behaviour within idealistic neoclassical models, and the industrial economists, like Hall and Hitch, who took a more inductive approach Both approaches were, according to Archibald, methodologically unsound "[O]n the one hand we had those who paraded their 'realism' - 'this is how businesses actually work' - and were indifferent to the arguments that their theory was indeterminate and therefore irrefutable, on the other hand we had those who stuck to 'rational' theory, and appeared more and more indifferent to reality" (Archibald 1959 61) Archibald set about trying to derive testable predictions from Chamberlain's theory of monopolistic competition What he found was that the theory was incomplete the definition of the group, the relation between the firm's dd curve and the industry DD curve, the response of firms to changes in price, were all too loosely modelled to form any useful quantifiable predictions The range of auxiliary assumptions that would have been necessary to produce a quantifiable form of Chamberlain's theory would have made it impossible to identify the source of any refutation (de Marchi 1988 156)

Undetered by these obstacles to falsificationism, Archibald attempted to engage Friedman and the rest of the Chicago school m a Popperian-style critical debate about the problem of gleaning testable predictions from monopolistic competition theory (de Marchi 1988 154) He highlighted a discrepancy between Friedman's methodological stance (see section 1.3), and Friedman's treatment of monopolistic competition

> We should expect to find the Chicago critics endeavouring to discover what predictions monopolistic competition yields, comparing the predicitons with those of perfect competition and monopoly, and finally addressmg themselves to such empirical testing as seemed necessary But we do not find this at all Rather we find that much of their argument has the *a priori* character that we would associate with a very different methodological school (Archibald 1961 3)

Archibald criticised the Chicago school for criticising the theory of monopolistic competition on the grounds that its initial assumptions were too complex and unrealistic They should, according to Friedman's own methodological prescriptions, have been focusing instead on the validity of the predictions of the theory If there were no testable predictions to be gleaned from the theory, then Friedman's own methodological stance outlined in 1953 would suggest that the theory is worthless

Stigler defended this "internal criticism" of the theory on the grounds that "the probability that a theory will yield useful predictions is reduced by logical weaknesses in its construction" (Stigler 1963 64) Yet Friedman himself had argued in 1953 that, "the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience" (Friedman 1984 214) Archibald's highlighting of this inconsistency between Friedman's methodological stance and his treatment of the theory of monopolistic competition provoked very little response from the Chicago school Archibald's attempts to introduce a little Popperian critical rationalism into economics failed (de Marchi 1988 153), so too, for Archibald, had the notion of falsificationist economics He argued that too much of economic theory was "incurably irrefutable" for a falsificationist methodology to work (Archibald 1966 279) Archibald reverted to Mill's solution to Hume's problem of induction, he argued that the only method of testing possible in economics was the relative appraisal of theories in the light of observable evidence (Archibald 1966 279)

Lipsey's method was, from the start, much closer in spirit to Mill than to Popper Lipsey worked on data to isolate correlations, then developed a theory to provide causation, then derived from this theory a set of predictions, and finally tested these predictions Lipsey's work on the Phillips curve broadly followed this pattern (Darnell and Evans 1990 44) Lipsey encountered the same problems as Archibald and the econometricians had done, and by 1966 had rejected falsificationism as a methodology for economics (de Marchi 1988 161)

Darnell and Evans' description of econometric practice in the 1960s, is similar to that outlined by Morgan in respect of econometric practice in the 1930s (Darnell and Evans 1990 48) Econometricians placed a great deal of faith in OLS, particularly in the investigations of macroeconomic behavioural variables The emphasis was, according to Darnell and Evans, on confirmation rather than on falsification (Darnell and Evans 1990 47) As in the 1930s, where models performed badly, they were adjusted in an *ad hoc* way, in order to fit the observed data Once again, the distinction between causal and statistical laws was blurred as econometricians attempted to establish the universal laws, the background knowledge, of economics

Economists do falsify, but what they falsify and reject are models of a theory, rather than the theory itself The conjecture and refutation that occurs in econometrics is limited to particular specifications of a theory However, Darnell and Evans pointed out that this is an important prelude to the testing of theory (Darnell and Evans 1990 67) The constant revision of models, ideally, should bring the econometrician to a model which is accepted as fully representative of the theory

At this stage, there is some hope of directly testing the theory The problem, however, is that there still exists the possibility of pre-test bias Each rejection of a particular model of a theory involves a decision to accept the validity of the data used to test the model Implicit in this decision is the assumption that there have been no sample errors Ultimately, the accuracy of a model can only be accepted "as a matter of faith" (Darnell and Evans 1990 72) Econometricians must be prepared to accept the assumption that there are no sample errors, in order that a test of the ultimate model be recognised as a test of the underlying theory Few econometricians are willing to do this Yet, without some form of *a priori* agreement as to the status of certain tests, falsification is impossible Without this agreement, the constant re-specification of models appears pointless Hendry's tongue-in-cheek description of this process of model re-specification indicates this "the search correction process is terminated at an arbitrary point often incorrectly determined by the *insignificance* of some test, or perhaps more usually by fatigue" (Hendry 1985 36)

This consensus problem facing econometricians stems from the Duhem/Quine argument Quine developed Duhem's argument that theories are in fact bundles of statements, some of which are analytic and some, synthetic (Quine 1953). Since there is "no sharp boundary" between these statements, the problem for the falsificationist is deciding which statements must be rejected upon falsification of the theory (Losee 1980: 192). Another problem in testing identified by the Duhem/Quine argument is that the testing of a theory involves the use of statistical techniques, themselves derived from statistical theories. This considerably complicates the process of falsificationism, since there could be a number of different sources from which the falsification emanates.

Leamer suggested adopting Bayesian techniques in econometrics in order to "take the con out of econometrics" (Leamer 1983). Bayesian analysis would, he argued, provide econometricians with an *a priori* set of criteria with which to judge models and ultimately allow for theory-appraisal. Adopting Bayesian criteria involves a fundamental change in the way in which econometricians generally consider probability. The general definition of probability is the frequency with which an event is observed in repeated trials. The Bayesian definition of probability, on the other hand, is essentially a reflection of the belief of the individual researcher as to the likelihood of the event being observed. The researcher's belief is not necessarily derived from the evidence of repeated samples. While the Bayesian technique offers *a priori* criteria to the econometrician, these criteria are too subjective to be thought of as Popperian. Bayesian criteria are conventionalist rather than empirical in nature.

There is a fundamental conflict between Bayesian *a priori* criteria and Popperian *a priori* criteria

Econometrics in the 1980s has had a rather narrower focus than that with which it began in the early part of this century The main focus now is forecasting The forecasts of classical linear regression models have been challenged by time-series techniques, principally Box-Jenkins analysis Econometric models still contain some vestiges of economic theory, although to arrive at a quantifiable model some auxiliary assumptions will undoubtedly have had to be made Time-series analysis, on the other hand, takes no explicit cogniscence of qualitative economic theory In time-series, the forecast of a variable depends solely on past values of that variable "it is in essence no more than a sophisticated method of extrapolation" (Kennedy 1985 205) Much of current econometrics has been concerned with which of the two is the better predictor This emphasis on prediction as opposed to explanation of economic phenomena suggests that econometrics might be pursuing an instrumentalist methodology

Gilbert has disputed the claim that "economists never reject theories, or at least not on the basis of econometric evidence" (Gilbert 1991 137) While Gilbert conceded that economists test models, not theories, he argued that "the outcome of these tests may have some bearing on our views about the validity of the underlying theories" (Gilbert 1991 138) Gilbert argued that "in demand theory we are indeed only testing the appropriateness of particular empirical models, while in consumption analysis tests of empirical models are genuine tests of the underlying theory" (Gilbert 1991 142) The ability to test consumption theory directly is a result of the nature of the modifications made to the theory in order to render it empirically testable

> Both demand theory and the theory of the consumption function simplify, as must any theory, but in demand theory the simplifications are motivated by the need to reduce the scale of the modelling enterprise, whereas in consumption, they are motivated at least in part by a desire to generate a particular set of implications In demand theory, these simplifications are part of the empirical model, while in consumption theory they are part of the theoretical model It follows that m testing demand theory we are testing for the appropriateness of an empirical model, and that our tests have the character of specification tests, while in consumption theory tests of the empirical model are tests of the theoretical model (Gilbert 1991 161)

Gilbert's analysis suggests that there might be some possibility of Popperian-style testing of at least some economic theories However, McElroy, in her comment on Gilbert's paper disputed Gilbert's argument that tests of consumption theories have been tests of theories and not of models

> the fundamental insight underlying the rational-expectations approach (differential responses to anticipated and unanticipated events), much less the

> fundamental insights underlying the PIH (permanent income and permanent consumption), are not at all at issue (McElroy 1991 174)

While Gilbert's analysis might highlight a limited ability to test theories, econometricians are far from establishing the sort of consensus necessary in order to define a crucial experiment It also appears that the concerns of the majority of econometricians have shifted in such a direction as to make it unlikely that this consensus will develop in the near future

1 4 ON SITUATIONAL LOGIC AND THE METHODOLOGY OF NEOCLASSICAL ECONOMICS

At the end of section 1 1, it was argued that Popper held critical rationalism to be the method of progress for both science and metaphysics Of the many forms which criticism may take, Popper's view was that falsificationism is optimal The demarcation between metaphysics and science specified by Popper was one of method - scientific propositions could be falsified, but metaphysical propositions could not But what of the social sciences, and economics in particular?

In the *Poverty of Historicism*, Popper presented the difference between economics and the natural sciences as being one of degree (Popper 1957 143) He implied that falsificationism is the optimal form of criticism in both, although he acknowledged that falsificationism might be more difficult in economics due to the changing nature of parameters This brief paragraph in the *Poverty of Historicism* has been the warrant for economic methodologists stressing the importance of falsification in economics

Economic methodologists have paid much more attention to Popper's falsificationism, than they have to Popper's externalist philosophy of the *social* sciences, his situational logic Popper maintained that situational logic is in fact the methodology of neoclassical economics (Popper 1976 102) However, economic methodologists appear to be unimpressed with Popper's elucidation of situational logic It has been described as "very sloppy" (Blaug 1985b 287), "confused or deliberately elusive" (Latsis 1983 133) and "vague and seemingly inconsistent" (Hands 1985 85)

The neglect by economic methodologists of situational logic matters little on a practical level if falsificationism and situational logic entail the same methodological prescriptions Popper maintained that falsificationism is applicable not just to economics, but to all the social sciences "[A]ll theoretical or generalising sciences make use of the same method, whether they are natural sciences or social sciences" (Popper 1957 130, see also 1966 222) Interestingly, as a warrant for his unity of method argument, Popper cited both Hayek and Menger (Popper 1957 136-137) (Both Hayek and Popper were anxious to point out that historicism is an inadequate method for a science Popper first presented *The Poverty of Historicism* as a paper in a seminar series run by Hayek in 1936 in the London School of Economics, and refered to Hayek's earlier arguments in Hayek (1933) Despite the obvious cross-influences of Popper and Hayek in respect of scientific methodology, it does not appear that Popper was particularly influenced in his perceptions of neoclassical economics by Hayek When discussing the methodology of neoclassical economics, Popper cited only Robbins (1935))

Caldwell argued that one way in which to make sense of this insistence on a unity of method across the sciences, is to interpret Popper as meaning that all theories "share the same structure" (Caldwell 1991 14n) Thus, Caldwell held that when Popper talked about the unity of method he was referring to the fact that both natural and social science should be based on hypothetico-deductive method Yet, Popper clearly meant more than this when he proposed a unity of method

> The only course open to the social sciences is to forget all about the verbal fireworks and to tackle the practical problems of our time with the help of the theoretical methods which are fundamentally the same in *all* sciences I mean the methods of trial and error, of inventing hypotheses which can be practically tested, and of submitting them to practical tests (Popper 1966 222)

Is it possible for the unity of method thesis to be a unity of methodology thesis? Popper argued that economics *can* progress using falsificationist rules Yet he also said that economics has its own distinct methodological framework - situational logic (Popper 1966 97) He further suggested that situational logic be extended to the other social sciences (Popper 1976 102) If the unity of method thesis is to be upheld, then falsificationism and situational logic must entail the same methodological prescriptions Upon investigation, however, it is apparent that these two methodologies do not entail the same prescriptions This leads to a paradox This section investigates the nature of this paradox If there is no unity of methodology - if situational logic makes prescriptions which are different from those made by falsification - then this has important implications for those methodologists who stress the importance of falsification in economics In addition to the question of the unity of methodology, this section assesses the extent to which situational logic is representative of the methodology of neoclassical economics

Popper's methodological rules for both the natural and the social sciences are based on the "rules of critical discussion" (Popper 1979 17) These rules form Popper's critical method Falsificationism instructs the natural scientist to hold some of his knowledge - background knowledge - as foundational This background knowledge is then combined with particular premises to form theory While the background knowledge generally goes unquestioned for periods of time, Popper warned that the natural scientist must be aware of the tentative nature of background knowledge and submit it to testing in order to ensure its continuing approximation to the truth The analysis of the paradox begins with a search for a concept analogous to background knowledge in situational logic

According to Popper, situational logic provides the mechanics of the critical method for neoclassical economics In situational logic, Popper introduced two concepts - situational analysis and the rationality principle The situational analysis is formed from "the initial conditions describing personal interests, aims and other situational factors, such as the information available to the person concerned" (Popper

1966 265) This situational analysis is a description of the social situation of the individual at the time in which a particular form of behaviour occurred

Of itself, the situational analysis, as described by Popper, says nothing about why a certain course of action arises from the situation so described One is left with a gap between a *description* of the range of possible actions inherent in the situational analysis, and an *explanation* as to why one course of action, out of the range of possibilities, was chosen Popper claimed that while other methodologies in the social sciences appeal to the laws of psychology in order to explain why a particular course of action was chosen, that such an appeal should be unnecessary in all the social sciences Popper bridged the gap between description and explanation via the "rationality principle" (Popper 1966 265)

Popper defined the rationality principle as "the trivial general law that sane persons as a rule act more or less rationally" (Popper 1966 265), yet insisted that the rationality principle involves no "psychological assumption" of what constitutes rational behaviour (Popper 1966 97) Popper maintained that the rationality principle has "little or nothing to do with the empirical or psychological assertion that man, always, or in the main, or in most cases, acts rationally" (Popper 1985 359) The rationality principle is therefore nothing more than the "principle of acting appropriately to the situation" (Popper 1985 359) The rationality principle involves no general definition of rational behaviour, and for that reason, "lets through as rational most social, economic, political, problem-solving and even neurotic behaviour" (Latsis 1983 132) It does not involve the social scientist making an objective, *a priori* assumption as to what constitutes rational behaviour

The rationality principle and situational analysis provide a foundation against which the Popperian social scientist will test his social theories Popper himself however gave no example of the mechanics of situational logic In order to demonstrate the kind of methodological direction given to economics by situational logic, Barro's Ricardian Equivalence Hypothesis (1976) is examined using Koertge's interpretation of the explanation schema under a situational logic framework (Koertge 1979 87)

Situational Analysis	included in the information which agents hold about the economy is the knowledge that sales of Government debt have increased
Rationality Principle	agents act appropriately to their situation as they see it
Hypothesis	when sales of Government debt increase, agents will reduce their expenditure because they believe higher bond sales now will result in higher taxes in the future
Explanandum	agents reduce their expenditure when the level of Government debt increases

In this example, the explanandum is not realised The weight of empirical evidence suggests that in fact economic agents do not reduce expenditure when the level of Government debt increases In order to find out why Barro's Ricardian Equivalence

Hypothesis does not accurately predict the behaviour observed, Popper instructed the economist to review either his hypothesis or his situational analysis

However, Popper instructed the social scientist *always to retain* the rationality principle The economist should never conclude that economic agents are, in fact, acting *inappropriately*, given the situation as the economist describes it "My thesis is that it is sound methodological policy to decide not to make the rationality principle accountable but the rest of the theory that is, the model" (Popper 1985 362)

This methodological rule in situational logic appears to be in direct conflict with the instructions Popper gave to natural scientists under falsificationism There is no piece of their knowledge that natural scientists may hold to be above falsification Natural scientists are instructed to test *all* of their background knowledge intermittently in order to ensure that it remains a good approximation to the truth Yet social scientists under situational logic are instructed to retain the rationality principle regardless of its approximation to the truth Hence, the background knowledge of the natural scientist, and the rationality principle and situational analysis of the social scientist, are not analogous

Popper's demarcation criterion between science and non-science is based on the falsifiability of the theory whose scientific status is in question The fact that Popper instructed social scientists to hold a part of their theories *un*falsifiable, implies that, by his own demarcation criterion, social theories which use situational logic are not scientific Yet, it is not at all clear from what Popper said about economics in the *Poverty of Historicism*, that this is a conclusion he intended

This analysis suggests that there are methodological differences between Popper's falsificationism and his situational logic It is difficult to see how a unity of method argument can be sustained in any way other than by arguing, as Caldwell did, that the theories of the social and natural sciences are constructed in the same hypothetico-deductive way

Caldwell suggested that an alternative resolution to the paradox might be found in Popper's critical rationalism (Caldwell 1991 25) If, by unity of method, Popper meant that some form of critical analysis may be applied in both the natural and social sciences, then according to Caldwell, the paradox ceases to exist (Caldwell 1991 25)

Popper described the principle of critical rationalism as the insistence that "our adoption and our rejection of scientific theories should depend upon our *critical reasoning* combined with the results of observation and experiment" (Popper 1983 32) Critical rationalism entails the acceptance of an alternative point of view and a willingness to accept criticism of our own point of view A critical rationalist point of view is not confined to the criticism of *scientific* theories Popper held critical rationalism to be a fruitful method for analysing all ideas, whether scientific or metaphysical, although the optimal form of criticism, falsification, is open only to scientific propositions

If the unity of method is taken as meaning that both social and natural scientific theories can be assessed using some form of critical analysis which falls short of falsificationism, then the paradox ceases to exist Both social and natural scientific theories can be critically assessed using the same tools, except for the case of falsification which is the preserve only of the natural sciences

What are the tools of critical analysis that are applicable to both sciences? According to Caldwell, critical analysis at this sub-optimal level, does not lay down any *a priori* rules for theory-appraisal "*[T]he level of criticism will depend on the problem to be solved and the nature of the material under investigation*" (Caldwell 1991 25) Caldwell acknowledged that "empirical criteria are the strongest and whenever possible they should be used" (Caldwell 1991 25) However, he argued that where falsification is the optimal criticism m the natural sciences, in the social sciences, optimal criticism can be obtained where the rationality principle is left intact and either the hypothesis or the situational analysis, or both, is questioned (Caldwell 1991 25)

Thus, the umty of method thesis, for Caldwell, *can only refer* to the application of critical rationalism in the natural and social sciences and not to the application of falsification This solution to the unity of method paradox creates another paradox why have economic methodologists spent so much time on applymg falsification to economics, when this is not the optimal form of criticism in social science?

The crux of the unity of method paradox as it is described above is that the instruction to social scientists not to deny the validity of the rationality principle is not consistent with Popper's instruction to natural scientists to test every part of their scientific knowledge This explanation of the paradox assumes that the rationality principle is an empirical concept, analogous to the background knowledge of natural science If they are not equivalent concepts, then it is not clear that the paradox persists

Popper introduced the rationality principle as the animator of the social scientist's situational analysis But what plays the role of animator in the natural sciences? According to Popper, in the natural sciences "if we wish to "*animate*" the model, that is, if we wish to represent the way m which the various *elements* of the model act upon each other, then we do need universal laws" (Popper 1985 358) The implication is that the rationality principle and the universal laws have the same function But if this is so, why are they subject to different methodological prescriptions?

Popper maintained that the rationality principle is "clearly false" (Popper 1985 360) This implies that it takes a form which is empirically testable The fact that it is false does not appear to have been, for Popper, a good reason for rejecting the principle It may, he argued, still be "a good approximation to the truth" (Popper 1985 362) On the other hand, Popper does not advocate testing the rationality principle to *ensure* proximity to the truth Since Popper was prepared to admit that the rationality principle is false, he denied any suggestion that he has attempted to make the principle *a priori valid* by preventing social scientists from testing it

The only thing which seems clear from Popper's description of the rationality principle is that, while the rationality principle and universal laws may play the same *role*, it is clear that they are not of the same *nature*

How exactly does the rationality principle animate the scientist's situational analysis? How does it fill the gap between the situational analysis and the behaviour which scientists observe? It appears to explain everything and nothing On the one hand, the notion that the scientist must accept that individuals always act appropriately to their situation, is strong methodological advice It constantly throws the burden of proof onto the scientist's hypothesis or onto his situational analysis On the other hand, without a definition of what constitutes appropriate behaviour, the rationality principle can tell the scientist nothing about *how* the decision to act, manifested in the behaviour that the scientist observes, *arises out of* the situational analysis he describes Indeed, Popper described the rationality principle as "almost empty", as "a kind of zero principle" (Popper 1985 359)

This emptiness is precisely why Popper developed the rationality principle To attempt to bridge the gap between description and explanation by using an empirical rationality principle would involve the adoption of some behavioural rule on the part of the social scientist It is this which Popper wanted to avoid

Popper argued that while psychology is a social science, it is not the basis of all social sciences (Popper 1966 97) Latsis, in the most comprehensive analysis of situational logic by an economic methodologist, concluded that the rationality principle, as described by Popper, can only be interpreted as a "bridge between the decision do something at *t* and the actual performance of the behaviour at *t*" (Latsis 1983 134) The *explanation* for the decision to act must already be incorporated in the situational analysis, if Popper's interpretation of the rationality principle is to be maintained Latsis concluded that, if all the goals, aims and motives for the decision to act are contained in the situational analysis, the rationality principle is not necessary to the social scientist (Latsis 1983 135)

The rationality principle appears then to be best described as "a byproduct of a methodological postulate" (Popper 1985 360), the aim of which is to avoid psychologistic explanations of behaviour m social sciences other than psychology It is false which implies that it is testable Yet it "does not play the role of an empirical explanatory theory" (Popper 1985 360) If the rationality principle is not an explanatory theory then it is difficult to see how it bridges the gap between decision and action in a way that is useful for the social scientist, hence Latsis' conclusion Latsis argued that the situational analysis could be framed in such a way as to yield an explanation for a particular form of behaviour without including a rationality principle at all (Latsis 1983 135) This could be done by including some motivational assumption in the situational analysis (Latsis 1983 135) Yet, this involves the making of behavioural assumptions, which is what Popper was expressly attempting to avoid

Why did Popper employ the rationality principle at all if he held it to be empty? Latsis argued that the rationality principle was consistent "with a certain ontology about the relation between mental states and behaviour" which Latsis finds in Popper's 1967 paper, 'Of Clouds and Clocks' (Latsis 1983 136) The rationality principle was devoid of any psychological assumption, and had to remain so in order to be compatible with Popper' views on psychologism

In 'Of Clouds and Clocks,' Popper examined the impact of the Copenhagen interpretation of quantum physics on physical determinism (Popper 1979 210) Popper distinguished between the different levels of control within physical systems On the one hand, there is the cast-iron control represented by the clock, within a "regular, orderly, and highly predictable" physical system (Popper 1979 207) On the other hand, there is the "highly irregular, disorderly, and more or less unpredictable" physical system represented by the cloud (Popper 1979 207) In the latter, the control is plastic With plastic control, it is no longer clear which part of the system is controlling the other (Popper 1979 249) Popper gave the following example of plastic control "The soap bubble consists of two subsystems which are both clouds and which control each other without the air, the soapy film would collapse without the soapy film, the air would be uncontrolled it would diffuse" (Popper 1979 249) Popper maintained that despite this two-way control, it would be possible to identify the controlled system and the uncontrolled system The air is the controlled system "the enclosed air is not only more cloudy than the enclosing film, but it also ceases to be a physical (self-interacting) system if the film is removed" (Popper 1979 249)

Latsis pointed to an analogy between Popper's description of different types of control in physical systems and the plastic control which mental states have on behaviour (Latsis 1983 140) Latsis believed that the rationality principle "does not have the status of a universal theory because Popper's ontology does not allow him to represent the connection between mental states and behaviour as a causal one" (Latsis 1983 140) A rationality principle which embodied a behavioural assumption would be indicative of "'cast-iron' control between mental states and behaviour" (Latsis 1983 140) Yet, without assuming some form of cast-iron control, how can social scientists make predictions?

In 'Of Clouds and Clocks,' Popper argued that no cast-iron controlled physical system actually exists "all clocks are clouds, to some considerable degree - even the most precise of clocks" (Popper 1979 215) On the other hand, "our clouds are not perfectly chance-like, since we can often predict the weather quite successfully, at least for short periods" (Popper 1979 229) Implict in this, is the argument that social systems and physical systems are both cloudy, although social systems might be considerably more cloudy and therefore less predictable than physical ones A study of Popper's ontological argument appears, then, to lead back to the belief that the difference between social science and natural science is one of degree It is a small jump from this position, backward to the unity of method thesis

What seems necessary is a reformulation of the rationality principle which solves the unity of method paradox in a way that is compatible with Popper's ontological stance on plastic control The following is such an attempt

The rationality principle is re-interpreted as a principle with two components - an empirical, animatory component and a methodological component The animatory component instructs the social scientist to make an objective, *a priori*, conjecture about what it is that constitutes appropriate behaviour in his model, that is, to form his model in terms of cast-iron control

This conjecture forms a link between the situational analysis described by the social scientist and the social behaviour he is attempting to predict or explain In other words, this conjecture animates the social scientist's situational analysis The animator enters the explanation schema of the social scientist in the same way that universal laws enter the explanation schema of the natural scientist As with the universal laws of the natural scientist, the social scientist's animator should be periodically tested for its approximation to the truth, if the social scientist is to achieve the optimal level of criticism for his hypotheses If the social scientist finds that his animator is no longer a good approximation to the truth, then he rejects it and formulates another conjecture to animate his situational analysis Because social systems are clouds, not clocks, the cast-iron animator is likely to require regular testing It is this commitment to constant testing of the empirical animator which allows for the recognition of plastic control

The rationality principle, as it is presented here, also contains a methodological component This component instructs social scientists always to retain the rationality principle, that is, to always construct social laws on the basis that there is consistency of behaviour of individuals placed in the same situation This, Popper asserted, allows one to conclude, "admittedly I have different aims and I hold different theories (from, say, Charlemagne) but had I been placed in his situation thus analysed - where the situation includes goals and knowledge - then I, and presumably you too, would have acted in a similar way to him" (Popper 1976 103) Note that Popper's description of the rationality principle here implies cast-iron control If we were to be placed in the same position as Charlemagne, we would not exhibit any control upon the situation, and would respond in the same way as he Popper's critique of Marxism also suggests cast-iron control from the situational analysis to observed behaviour Popper criticised

Marx's economism with the notion that once the ruled classes become the ruling classes, they will behave in the way of the ruling classes regardless of what they were before (Popper 1966 131) This surely implies that the position of ruler exhibits cast-iron control on whoever is ruler?

In the interpretation of the rationality principle presented here, Popper's instruction never to reject the rationality principle, is taken to apply to the methodological component of the rationality principle, but not to the empirical animatory component Situational logic, under this interpretation, works in the following way The rationality principle enters the explanation schema of the social scientist as an animator, along with a situational analysis and the social scientist's particular hypothesis On finding that his predictions or explanations are false, the social scientist can test the animator, or the situational analysis or the hypothesis, or all three The rationality principle *qua* methodological principle remains A rationality of behaviour is always assumed, though the social scientist might not have hit upon the accurate one

Given that the methodology of neoclassical economics provided Popper with the inspiration for situational logic, the methodology of neoclassical economics should provide the best test of the re-interpretation of the rationality principle as a dualistic concept

It is clear that neoclassical economics makes explicit use of a rationality principle And this rationality principle *does* appears to contain two different components In neoclassical economics, individuals are assumed to act in a way appropriate to the situation But neoclassical economics also incorporates a definition of what it is that constitutes rational behaviour, namely utility or profit maximisation This forms the animator in the situational logic of neoclassical economics

Koertge's (1979) explanation schema can be adapted to show how this alternative interpretation of the rationality principle corresponds to the kinds of methodological questions which arise in economics

Situational analysis	economic agents are in situation X which has x particular characteristics (eg the firm is in a perfectly competitive market)
Animator	economic agents act appropriately to the situation, where appropraite behaviour is defined as profit maximisation given constraints (eg the firm will always attempt to maximize profits)
Scientific hypothesis	given situation X economic agents will do Y to maximize profits (eg the firm in a perfectly competitive market will set price equal marginal cost in order to maximize profits)
Explanandum	economic agents do Y (eg firms set their prices equal to marginal cost and maximize profit)

Suppose economic agents do Z (eg the firm sets its price above marginal cost) The economist's prediction is falsified There are three possibilities

1 the economist's analysis of the economic agents' situation is inaccurate (he has wrongly identified the market so that the firm maximizes profit by setting price above marginal cost)

2 the economist's interpretation of appropriate behaviour is inaccurate (firms do not always attempt to maximize profit)

3 the economist's hypothesis is wrong (firms do not do Y to maximize profit)

In most cases the economist will, on the falsification of his hypothesis, reassess either his hypothesis or his situational analysis This is in accordance with Popper's methodological prescription But is it not possible that the economist has cause to question his animator, that is, his definition of the appropriate behaviour for firms?

In respect of the nature of the utility maximisation hypothesis, Koertge asked, "What if an agent deliberately set out to minimize expected utility? Would the resulting action count as a rational one?" (Koertge 1979 30) Under the interpretation presented here, the answer is yes What *is* questioned is the economist's definition of the rational behaviour contained in the animator

If one accepts this interpretation of the rationality principle as the combination of a metaphysical component and an empirical, animating component, then the prescriptive paradox between situational logic and falsificationism ceases to exist, *in principle* This is done by assuming a minimum psychological assumption, but that assumption can be contradicted by observed behaviour, and changed Both natural and social scientists search for laws, without having to assume that such laws exist Both formulate laws by postulating consistencies in the behaviour they observe, and both should, in the Popperian tradition, attempt to falsify their laws and more frequently, the hypotheses derived from these laws The unity of method thesis therefore remains intact This reformulation does nothing, of course, to shelter falsificationism or situational logic from the charge that verisimilitude does not stand up as a method which is compatible with falsificationism

The reformulation of the rationality principle raises an interesting question to what extent does the maximisation principle constitute an empirical animator for situational logic in economics?

There is, now, a considerable amount of agreement among economic methodologists as to what the maximisation principle is not It is not testable Agassi applied Popper's demarcation criterion to the maximisation principle, and argued that the principle is metaphysical (Agassi 1971 52) Hutchison had previously suggested that if the principle were not empirical, then it must be a tautology (Hutchison 1938) Agassi pointed out that a non-empirical statement need not necessarily be a tautology "the informative content need not be zero - it can be too low for empirical tests but still too high for tautology" (Agassi 1971 52) However, Agassi pointed out that the way in which the maximisation principle is continually protected by economists, is in danger of reducing the maximisation principle to a tautology "It looks as if we always defend the theory by qualifying it again and again in the face of counter-evidence" (Agassi 1971 51) This has converted the maximisation principle into an accounting convention "income equals expenditure both in properly balanced books and in perfect competition of necessity" (Agassi 1971 51) The danger is that further defence of the maximisation principle will reduce it the tautology, "firms do as firms do" (Agassi 1971 51)

Boland also held the maximisation principle as a metaphysical principle (Boland 1981 1035) He also criticised the continual defence of the maximisation principle "One would be better off maintaining one's metaphysics rather than creating tautologies to seal their defence" (Boland 1981 1035) Caldwell argued that where utility or profit remains undefined, then the maximisation principle is untestable (Caldwell 1983 826) He argued that Boland made the principle tautologous by defining it as "all consumers maximise something" (Boland 1981 1034) Caldwell went on to assess the attempt by Samuelson to make the maximisation principle empirically testable, by defining utility (Caldwell 1983 824) The problem with the revealed preference approach is that it "requires that assumptions be made concerning the stability of preferences of the choosing agent, as well as the states of information confronting him Since the content of these assumptions are subject to change but are not themselves directly testable, test results are not unambiguously interpretable" (Caldwell 1983 824-825) The problem of testability which Caldwell highlighted is the same as that facing econometricians, namely the identification problem This is a problem for the utility maximisation principle, but not for the profit maximisation principle The work on industrial organisation of the 1950s and 1960s set about replacing the profit maximisation principle with a sales maximisation principle (Baumol 1959), and later, a growth maximisation principle (Marris 1963,1966)

These alternative models of firm behaviour are based on the assumption of the separation of ownership and control in the modern corporation Where the shareholder no longer has control over the dividend he is paid, then these models predict that sales or growth become the major priority for the corporation The problem with these models is that they entail an assumption about utility maximisation on the part of the managers of the corporation They simply replace one kind of utility maximisation, that of the shareholders, with another kind of utility maximisation, that of the managers One still ends up with an untestable utility maximisation principle

The neoclassical maximisation principle is not of the same nature as the animator described in the situational logic model above It does provide a bridge between the situational analysis, and the observed behaviour, and as such turns the model into one of cast-iron control However, it does not allow for the possibility of plastic control, since it is not specified as a testable principle The model remains one of cast-iron control, which is, according to Latsis, precisely what Popper wanted to avoid (Latsis 1983 140) It appears then that Popper's situational logic cannot be the method of neoclassical economics, if one accepts Latsis' interpretation of Popper's rationality principle

CONCLUSIONS

Falsıficatıomsm, although advocated by some promınent economıc methodologısts (eg Hutchıson 1938, Blaug 1980, 1992), has been a faılure ın the practıce of economıcs

The prıncıple reason for thıs faılure ıs that the gap between theory and observatıon has proved too great for falsıficatıomsm to be properly applıed The attempted quantıficatıon of economıc theory has revealed a number of obstacles to testmg A varıety of models can purport to be complete representatıons of a theory A further problem ıs caused by the dıfficulty m ısolatıng economıc parameters Testmg m economıcs has a dıfferent focus than ın Popperıan methodology Testmg ın economıcs tends to be testıng of models, not of theorıes (Caldwell 1984 493)

Falsıficatıonısm provıdes crıterıa by whıch scıentısts may choose between theorıes In economıcs, the use of such crıterıa could only be consequent upon economısts havıng agreed on whıch models are the best representatıons of whıch theorıes The level of consensus requıred ın order to adopt falsıficatıonıst crıterıa has not yet been achıeved In addıtıon to thıs, the fundamental premıse of economıc theory, the maxımısatıon prıncıple, has been made unfalsıfıable by successıve modıficatıons Thıs metaphysıcal foundation is at odds with Popper's call for an empirically tested background knowledge On the other hand, the maximisation principle says too much to be equated with the empty rationality principle

The fact that economists have been unsuccessful in their attempts to falsify economic theory should not be taken as a critique of falsificationism To criticise falsificationism, one would have to show that falsificationist criteria do *not* enable the economist to make choices between economic theories

> it is not an effective argument against falsificationism to simply point out that it is difficult to get clean tests of hypotheses, that decisive refutations are rare That problem always exists The argument must be against Popper's insistence that *nevertheless* refutations should be taken seriously, and that when one occurs, certain theory adjustments are forbidden (Caldwell 1991 7)

Economic theories are probability hypotheses As such, they are likely not to hold outside of a specified range of observations They are, therefore, false in general For Popper, the choice between two equally false theories can be made by appraising the degree of corroboration of each theory The concept of corroboration gives the scientist an indicator of the verisimilitude, the truth-likeness, of a theory The problem is that verisimilitude is not compatible with the essence of falsificationism which is that all false theories have a zero truth-probability If the recommendation is that the scientist discard all false theories, why should he then be instructed to compare false theories? Popper's methodology does not allow for a choice to be made between false theories unless one accepts the validity of the concept of verisimilitude, accepting verisimilitude, in turn, necessitates a rejection of falsificationism This inconsistency is a substantial criticism of Popper's philosophy of science Popper argued that falsificationism is equally applicable to the natural and social sciences Situational logic, the method of choosing between false theories in the social sciences, also fails on the basis of this inconsistency between falsificationism and verisimilitude

Not only can falsificationist criteria *not* be applied in economics, it can also be argued that they *should not* be applied Falsificationism does not allow economists to choose between false theories, yet, this is exactly the type of comparison economists will be forced to make Friedman, in his methodology of positive economics, argued that such decisions between false theories should be made on the basis of conventionalist criteria The only alternative to this conventionalist methodology is to embrace induction The extent to which economists and economic methodologists have done this is examined in chapter three in the context of Lakatos' methodology of scientific research programmes

Has there, then, been no Popperian legacy for economic methodology? Caldwell suggested that Popper's critical rationalism has had a positive influence both on economic methodology and on economics m practice (Caldwell 1991 27) Yet, what is critical rationalism but the instruction that scientists be more critical of their own work and more tolerant of the work of others? Without the falsificationist criteria for theory-choice, critical rationalism "becomes an unobjectionable but rather empty set of rules which at best excludes dogmatism but says little more that is positive" (Nola 1987 455) However, in a social science which tends toward dogmatism, the importance of this legacy should be underestimated

CHAPTER TWO

THE STRUCTURE OF SCIENTIFIC REVOLUTIONS IN ECONOMICS

Faced with the difficulties of implementing falsificationism in economics, economic methodologists turned back to the philosophy of science In their preoccupation with Popper, economic methodologists failed to notice that the orthodoxy of the philosophy of science was under attack The orthodoxy of the philosophy of science was the view that criteria for theory-choice could be made without any reference to the theories the scientist was choosing between Thus, the attack was an attack against the externalist philosophies of science This attack reached its "happy and serendipitous culmination" with the publication in 1962 of Kuhn's *The Structure of Scientific Revolutions* (Wartofsky 1976 729) Kuhn's text created "a split between two philosophies of science - one of which had become all but canonical, in its ahistorical mode, the other of which threatened to usurp this hegemony, with a peculiar link to history of science" (Wartofsky 1976 722)

Kuhn's primary concern was with the history of natural science, but not with the mere chronicle of historical facts Kuhn set out two tasks for the historian of a science

> On the one hand he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented On the other, he must describe and explain the congeries of error,

> myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern scientific text (Kuhn 1970a 2)

For Kuhn, "the process of scientific discovery , or of changing theoretical structures, is inherently a part of its broader environment" (Dow 1985 27) It is the conversion of a chronicle into a subjective discourse on motive, purpose and event, that characterises Kuhn's analysis as a philosophy rather than as a history of science It is, in McMullin's (1970) classification, an internalist philosophy of science Thus, it presupposes the existence of an already functioning methodology within a science and attempts to elucidate that methodology The purpose of internalist philosophy of science is not to set down, *a priori*, criteria for theory-choice, but rather to elucidate the criteria for theory-choice already at work in science

Given the failure of the falsificationist methodology in economics, it might, in retrospect, have seemed a sound methodological move for economic methodologists to turn to the consideration of what economists actually do, and away from what orthodox philosophers claimed they ought to do The extent to which Kuhn's approach has served a useful one for economic methodologists is explored in section 2 2 Section 2 1 gives a brief outline of Kuhn's methodology

2 1 AN HISTORICIST EXPLANATION OF SCIENTIFIC DEVELOPMENT

Kuhn accepted Popper's assertion that science progresses in a process of conjecture and refutation However, the history of science showed that scientists did not always reject refuted theories For Popper, the reluctance of a scientist to reject a theory refuted by empirical evidence could only be described as irrational in terms of Popper's particular theory of science Kuhn attempted to explain why scientists might not reject refuted theories To do this, he did not posit an alternative externalist philosophy of science His aim was, rather, to elucidate the theory of science used by scientists

Kuhn explained the tenacity of refuted theories in a sociological theory of scientific development Kuhn saw science as developing through a series of evolutionary cycles, with the dynamics of each cycle being of the same nature Each cycle begins with what is the most famous (and overused) of Kuhn's concepts - the paradigm Kuhn defined the paradigm in a myriad of different ways Masterson (1970) outlined twenty-two definitions in Kuhn (1970a)

The paradigm is a metaphysical heuristic concept giving direction to scientists, it is a series of laws and theories which scientists use as the foundation of their knowledge, it is a methodological tool-box, it is a set of standards to which scientists must adhere Paradigms combine all these functions into an entire scientific tradition Kuhn's examples of paradigms include Ptolemaic astronomy, Aristotelian dynamics and Newtonian dynamics The word paradigm appears to be too all-encompassing to have any real analytical force 'Paradigm' had been so extensively adopted to describe a range of different concepts, that by 1970, Kuhn admitted that he had lost control of the word (Kuhn 1970b 272) In the postscript to the second edition of *The Structure of Scientific Revolutions*, Kuhn attempted to regain control over the concept in order to reestablish its methodological significance

Kuhn acknowledged the variety of meanings delineated by Masterson, but argued that many of these are as a result of "stylistic inconsistencies" and can therefore be discarded (Kuhn 1970a 181) Kuhn equated the paradigm to a "disciplinary matrix" (Kuhn 1970a 182), made up of the following components The paradigm or matrix will contain "symbolic generalizations" (Kuhn 1970a 182) These form a frame of reference for the development of theory within the paradigm These symbolic generalisations take the form of laws, but inherent in them is a particular definition of the concepts as used within the law Kuhn showed this dual function of symbolic generalisations by posing the following question "Did Einstein show that simultaneity was relative or did he alter the notion of simultaneity itself?" (Kuhn 1970a 184)

The second component of the paradigm is the existence of shared beliefs or commitments to particular problems by those scientists working within the paradigm (Kuhn 1970a 184) This suggests a third component - that of a system of "shared values" (Kuhn 1970a 185) Finally, the paradigm must contain a set of specified exemplars (Kuhn 1970a 187) It is these exemplars which "provide the community fine-structure of science" (Kuhn 1970a 187) Kuhn placed a lot of emphasis on the importance of common exemplars, it is this which ensures that all the scientists within a particular paradigm see things in the same way (Kuhn 1970a 193)

The paradigm, therefore, sets down for the individual scientist, not only the problems or puzzles to be solved, but also the method by which these problems or puzzles ought to be solved This puzzle-solving activity, Kuhn defined as normal science

> research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice (Kuhn 1970a 10)

Normal science is presented as the manifestation of a heuristic given to science by the adopted paradigm It involves the creation of theories to solve set problems in the form dictated by the accepted set of exemplars, in a way that is consistent with the symbolic generalisations of the paradigm, and that does not contradict the shared values of the scientific community which espouses the paradigm It involves empirical investigation "to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which [the paradigm] had previously only drawn attention" (Kuhn 1970a 27)

In the process of normal science, scientists are concerned with upholding rather than with refuting the paradigm They construct their theories and models to fit in with the pronouncements of the paradigm - "the aim of normal science is not major substantive novelties" (Kuhn 1970a 35) The paradigm provides the scientist with the answer, and it is his job to formulate the question If the scientist fails to resolve his theory to the paradigm, then "only his ability not the corpus of current science is impugned" (Kuhn 1970b 4) Normal science, Kuhn asserted, "accounts for the overwhelming majority of the work done in basic science" (Kuhn 1970b 4) The majority of scientists, according to Kuhn, are not in fact engaged in Popperian-type quests for falsifications, but rather in testing theories for compatibility with the prevailing paradigm

The scientific revolution of the title of Kuhn's text, occurs when there is a change in the accepted paradigm of the scientific community This change is ultimately wrought by the discovery of anomalies in the existing paradigm If this is so, where is the difference between Kuhn's description of progress in science and Popper's prescription for progress in science?

Kuhn argued that single refuting instances will not be enough, by themselves, to warrant the overthrow of a paradigm When faced with individual anomalies, Kuhn argued that scientists will "devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict" (Kuhn 1970a 78) In its extreme, the constant modification of a paradigm will lead a dogmatic persistence in the acceptance of the paradigm whatever the impact of anomalies, an outcome which Kuhn called "professionalisation" (Kuhn 1970a 64) Professionalisation leads to "an immense restriction of the scientist's vision and to a considerable resistance to paradigm change" (Kuhn 1970a 64) Science falls short of this development when a new paradigm rises to challenge the accepted paradigm

It is only when the "anomalies and counter-instances" are worked into an alternative paradigm that the established paradigm is overthrown During a period of "crisis", some scientists begin to examine the nature of the anomalies to the established paradigm (Kuhn 1970a 76) "A failure that had previously been personal may then come to seem the failure of a theory under test" (Kuhn 1974 801) It is only during this period of crisis that scientists begin to question the adequacy of their theories as opposed to the theories of other scientists However, scientists do not, according to Kuhn, reject the established paradigm until they have an alternative paradigm of equal stature to replace it with

> At the start a new candidate for paradigm may have few supporters, and on occasions the supporters' motives may be suspect Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like to belong to the community guided by it And as that goes on, if the paradigm is one destined to win its fight, the number and strength of

> the persuasive arguments in its favour will increase More scientists will then be converted, and the exploration of the new paradigm will go on at last only a few elderly hold-outs remain (Kuhn 1970a 159)

Kuhn argued that the new paradigm will differ from the old one in a very fundamental way it will propose new problems to solve and new methods of solving them Given these fundamental differences between paradigms, Kuhn spoke of the necessity of a *gestalt-switch* among scientists, a conversion from belief in the primacy of the old problems and methods, to the primacy of the new problems and methods (Kuhn 1970a 103)

Because they deal with different problems in different ways, Kuhn concluded that paradigms are incommensurable "The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (Kuhn 1970a 103)

Kuhn gave the following reasons as to why competing paradigms are incommensurable "The proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve Their standards or definitions of science are not the same" (Kuhn 1970a 148) But this definition of incommensurability as a dispute over standards is a narrower one than the definition of incommensurability which Kuhn finally proposed

> Since new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed But they seldom employ these borrowed elements in quite the traditional way Within the new paradigm, old terms, concepts, and experiments fall into new relationships, one with the other The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools (Kuhn 1970a 149)

This "misunderstanding" is the source of the incommensurability of competing paradigms The notion that paradigms are incommensurable should not be interpreted as meaning that competing paradigms cannot be compared, since "the very rationale for introducing the notion of incommmensurability is to clarify what is involved when we do compare alternative and rival paradigms" (Bernstein 1983 82) Thus, "when Kuhn claims that 'mass' means something different in classical and relativistic mechanics, thus rendering these theories incommensurable, he is not appealing to history but to an analytical criterion of difference in meaning" (Giere 1973 291)

Kuhn attempted to clear up the general misunderstanding over the meaning of incommensurability by starting with what he felt to be the most common misinterpretation of the impact of incommensurability

> the proponents of incommensurable theories cannot communicate with each other at all as a result, in a debate over theory-choice there can be no recourse to *good* reasons, instead theory must be chosen for reasons that are ultimately personal and subjective, some sort of mystical apperception is responsible for the decision actually reached (Kuhn 1970a 198-199)

For Kuhn, scientific theories *can* be compared, but "debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof" (Kuhn 1970a 199) On what grounds then can the choice between incommensurable theories be made?

For Kuhn, normal science takes up most of the time of the scientist Inherent in the activity of normal science is the recognition of the "incompleteness and imperfection of the existing theory-data fit" (Kuhn 1970a 148) In normal science, there is an attempt to establish a closer fit by adjusting theory to fit the data It is this process of adjustment which makes falsification an inadequate methodology in practice "if any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (Kuhn 1970a 145) Popper recognised this probabilistic nature of scientific theories and proposed verisimilitude as a method of choosing between equally false theories (Popper 1972 335)

While Kuhn acknowledged that "it makes a great deal of sense to ask which of the two actual and competing theories fits the facts *better*", the incommensurability poses a barrier to the resolution of this question (Kuhn 1970a 147) For Kuhn, "the competition between paradigms is not the sort of battle that can be resolved by proofs," because "the proponents of competing paradigms are always at least slightly at cross-purposes" (Kuhn 1970a 148) Thus, the debates about the particular merits of one paradigm over another are circular since "each group uses its own paradigm to argue in that paradigm's defense" (Kuhn 1970a 94)

Yet, scientists do choose between paradigms, although their conversion might take some time Why conversions occur, Kuhn argued, has "no single or uniform answer" (Kuhn 1970a 152) Kuhn considered a range of possible reasons "the sun worship that helped make Kepler a Copernican","idiosyncrasies of autobiography and personality", "the nationality or prior reputation of the innovator" (Kuhn 1970a 152-153) In addition, Kuhn argued that the new paradigm is likely to succeed if it "displays a quantitative precision strikingly better than its older competitor" (Kuhn 1970a 154) And novel facts are "particularly persuasive" (Kuhn 1970a 154) It appears than anything might determine the conversion from the old paradigm to the new, but the relative degree of corroboration of each The working out of the degree of corroboration comes after the conversion "it is only much later, after the new paradigm has been developed, accepted, and exploited that apparently decisive arguments are developed" (Kuhn 1970a 156)

Kuhn placed conventionalist criteria at the centre of the conversion "these are the arguments, rarely made entirely explicit, that appeal to the individual's sense of the appropriate or the aesthetic - the new theory is said to be 'neater,' 'more suitable,' or 'simpler' than the old" (Kuhn 1970a 155) The essence of conventionalism is that the definitions of 'simple,' 'neatness,' 'fruitfulness,' and so on, have been established by the scientific community, and thus are applied by every scientist in the same way Kuhn, on the other hand, argued that "such reasons function as values and they can thus be differently applied, individually and collectively, by men who concur in honoring them" (Kuhn 1970a 199) Where concepts like 'simplicity,' 'neatness,' and so on, are applied with different meaning within different paradigms, then these conventionalist criteria, too, fail to explain why choices between paradigms occur For Kuhn, the choice between paradigms comes down to persuasion "the debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof" (Kuhn 1970a 199) The persuasion of the holders of the old paradigm that the disciplinary matrix of the new paradigm "can solve the problems that have led the old one to a crisis" (Kuhn 1970a 153), must occur before this can *actually be shown* to be the case The conversion "must be based less on past achievement than on future promise A decision of that kind can only be made on faith" (Kuhn 1970a 157-158)

It is after the persuasive tactics of the holders of the new paradigm have managed to ensure conversion from the old paradigm, that the logical relation of the old to the new paradigm is explored Kuhn argued that this resulted in a history of science which is "linear or cumulative" (Kuhn 1970a 139) The fact that the disciplinary matrix has changed when a conversion to a new paradigm occurs, is hidden by this new history The source of this new history is to be found in textbooks "From the beginning of the scientific enterprise, a textbook presentation implies, scientists have striven for the particular objectives that are embodied in today's paradigms" (Kuhn 1970a 140) Scientific revolutions are therefore made invisible by the desire of the scientists working within the present paradigm not only to direct the future development of their science, but also to represent the past Thus, the history of a science as presented by current scientists, will in fact be a reconstruction of that history which seeks to identify, in the past, the seeds of the present set of exemplars

Bernstein argued that this tendency to rewrite history of science as a cumulative process is propagated by empiricist philosophers (Bernstein 1983 83) His example is the common argument that Einstein's theory of dynamics was a more complete version of dynamics than was Newton's which is contained within Einstein's theory as a special case For example, this is Popper's argument against Kuhn's description of incommensurability

> It would thus be simply false to say that the transition from Newton's theory of gravity to Einstein's is an irrational leap, and that the two are not rationally compatible On the contrary, there are many points of contact and points of comparison it follows from Einstein's theory that Newton's theory is an excellent approximation (Popper 1970 57)

Popper's argument is that the shift from the Newtonian to the Einsteinian system can be described in terms of a process of conjecture and refutation, as all good science can In other words, Popper reconstructs this history of physics in terms of his own particular theory of scientific rationality In Popperian terms, the Einsteinian system is preferable to the Newtonian because it solves anomalies that the Newtonian system cannot solve and incorporates the Newtonian system as a special case

This argument does not hold in Kuhn's analytical framework Where paradigms are incommensurable, one cannot be held as a special case of the other In the Kuhnian framework, the differences between paradigms in their understanding of particular concepts suggests that the decision to shift from one paradigm to another cannot be made on the basis of objective criteria such as those suggested by Popper

Kuhn's philosophy of science met with a substantial amount of criticism from positivist philosophers of science whose main argument was that there is a (discoverable) underlying rationale to the scientific process The main aspects of this criticism are to be found in the collection of papers presented at the International Colloquium in the Philosophy of Science, published in Lakatos and Musgrave (1970)

Kuhn's argument that science did not progress according to a single, universally held, set of objective criteria was interpreted as an argument that that science grows on the basis of irrational decision-making by scientists Their decision-making can only be irrational in the light of the argument that paradigms are incommensurable In the light of Popper's arguments against historicism and psychologism (Popper 1966), it is not surprising that he argued strenuously against Kuhn's assertion that scientific growth is borne out of decisions which are based on the relative abilities of revolutionary scientists to persuade their 'normal' colleagues (Popper 1970 57-58) Lakatos, too, denied that 'mob psychology' played a role in "the world of articulated knowledge" (Lakatos 1970 180) Kuhn denied this charge of irrationalism in his reply to these papers "I do not for a moment believe that science is an intrinsically irrational enterprise" (Kuhn 1970b 143) What Kuhn was arguing was that the community-wide concepts used to judge between paradigms are understood in different ways by different members of that community Thus, the decision to change from one paradigm to another is essentially a personal one (see Kuhn 1970a 199, quoted above) For Kuhn, this explains why the conversion of an entire scientific community to a new paradigm can take a long time

If scientific rationality is a personal, as opposed to a community-wide, concept, then the next step is to investigate the extent to which the concept of rationality differs between scientists, to investigate the criteria which scientists themselves use when choosing between theories This leads into the sort of sociological investigation of scientific growth which Popper argued so strenuously against "The suggestion that we can find anything [in psychology or sociology] like 'objective, pure description' is clearly mistaken" (Popper 1970 58)

The notion of the incommensurability of paradigms is closely connected to the notion that scientific decisions are based on irrational premises If paradigms are fundamentally incomparable, as Kuhn was interpreted as having said, then the choice between them can only be irrational Watkins doubted that incommensurability would describe the relation between two paradigms within the same science He argued that incommensurable paradigms are not necessarily incompatible, they do not necessarily lead to scientific revolution "Biblical myths and scientific theories are incommensurable [but] they are compatible and can peacefully co-exist just because they are incommensurable" (Watkins 1970 36) On the other hand, Watkins argued that

> if the Ptolemaic system is logically incompatible with the Copernican system peaceful co-existence is not possible they are *rival* alternatives, and

it was possible to make a rational choice between them partly because it was possible to devise crucial experiments between them (Watkins 1970 36)

Thus, for Watkins, two paradigms that are incompatible cannot also be incommensurable Toulmin, too, criticised Kuhn's concept of incommensurability, it had "an element of rhetorical exaggeration" (Toulmm 1970 43) Kuhn, argued Toulmm, "went too far by implying the existence of discontinuities in scientific theory far more profound and far less explicable than any which ever in fact occur" (Toulmm 1970 41) Toulmm pointed to the fact that many scientists *were* able to give reasons as to why they changed paradigms, and few cited the persuasive abilities of their colleagues (Toulmm 1970 44) Kuhn appears to have taken at least some of this criticism on board, for in *The Essential Tension* (1977), he had adopted a softer definition of incommensurability, which allowed a scientific revolution to have elements of continuity as well as disjointedness This revision to the concept of incommensurability, and therefore to the nature of scientific revolutions, has particular significance in respect of Kuhnian analyses of the growth of economic knowledge

It has been argued that "these concessions considerably dilute the apparently dramatic import of Kuhn's original message" (Blaug 1992 30) However, the notion that there are elements of continuity and disjointedness as a science shifts from one paradigm to another is only interpreted as a weakening of Kuhn's argument if one considers

Kuhn's argument in an objectivist light In other words, the fact that there can exist a great deal of commensurability between competing paradigms, is only troubling if Kuhn were attempting to put forward an objective theory of science based on the notion of incommensurability between paradigms The Kuhnian framework has in fact been interpreted in this narrow sense by positivist philosophy of science It was not, however, Kuhn's aim to find an alternative theory of scientific rationality Rather, he argued that searching for an objective rationality of science was a wasted exercise

> To suppose that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land (Kuhn 1970b 264)

The whole point of a sociological reconstruction of the history of science, is that the historian should have no preconceived notions about how science should progress, or indeed, whether it does progress Kuhn argued that "the philosopher's reconstruction is generally unrecognizable as science to either historians of science or to scientists themselves" (Kuhn 1977 15) This is because the philosopher's preoccupation with a particular theory of scientific rationality distorts his portrayal of scientific development

> Both historians and scientists can claim to discard as much detail as the philosopher, to be as concerned with essentials, to be engaged in rational

> reconstruction Instead the difficulty is the identification of essentials To the philosophically minded historian, the philosopher of science often seems to have mistaken a few selected elements for the whole and then forced them to serve functions for which they may be unsuited in principle and which they surely do not perform in practice, however abstractly that practice be described (Kuhn 1977 15)

The description, by Kuhn, of his history of science as a rational reconstruction seems to be something of a paradox, given his argument that an objective theory of science cannot exist However, a Kuhnian rational reconstruction is a history of a science which seeks to identify the definitions of rationality employed by scientists themselves This is distinct from the positivist notion of a rational reconstruction as an interpretation of the history of science from the point of view of a particular, objective theory of scientific rationality

Since the definitions of rationality which scientists employ are, according to Kuhn, likely to change over time, the history of science is more likely to display a pattern of disjointedness rather than the pattern of continuity which is suggested by positivist rational reconstructions

There are three types of rational reconstruction of the history of a science The first is a positivist rational reconstruction, which superimposes upon the history of a science a particular objective theory of scientific rationality The second is the Kuhnian sociological rational reconstruction which attempts to discover the definitions of rationality expounded by scientists themselves within a particular science The third rational reconstruction is also identified by Kuhn It is the reconstruction of a science presented by scientists themselves in their attempt to show continuity over the lifecycle of their science (Kuhn 1970a 138) The concept of rational reconstruction is raised again in chapter 3 2 in the context of Lakatos' methodology of historiographic research programmes The following section is concerned with the way in which Kuhnian concepts have been imported into economics

2 2 SCIENTIFIC REVOLUTIONS IN ECONOMICS

Kuhn said relatively little about the social sciences in general, and nothing at all about economics His few comments on the social sciences suggest that he did not believe them to have developed sufficient consensus to be described as paradigmatic "it remains an open question what parts of social science have yet acquired such paradigms at all History suggests that the road to a firm research consensus is extraordinarily arduous" (Kuhn 1970a 15) However, he strongly objected to the notion that social scientists should set about *establishing* paradigms "If, as Feyerabend suggests, some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle solving, they are badly misconstruing my point" (Kuhn 1970b 245) Kuhn's aim was not to elucidate some objective methodology whereby sciences might progress, but rather to argue that such an objective methodology does not in fact exist However, economic methodologists have tended to apply Kuhn's philosophy of science in the same way that they applied Popper's Instead of engaging in sociological rational reconstructions of the history of economic thought, economic methodology has, in the main, attempted to superimpose Kuhnian concepts upon the history of economic thought in the form of a postivist rational reconstruction The main concern has been with identifying paradigms and revolutions in the history of economic thought

As was the case with Popper, economists and economic methodologists eagerly adopted Kuhnian *terminology* "appeal to paradigmatic reasoning has quickly become a regular feature of controversies in economics and 'paradigm' is now the by-word of every historian of economic thought" (Blaug 1976 149) A common view is that economics, alone of the social sciences, has the appearance of a Kuhnian science (for example, Kunin and Weaver 1971 391) This assertion is normally based on the existence of the neoclassical tradition, which bears considerable resemblance to the Kuhnian concept of paradigm It contains symbolic generalisations - the laws of demand and supply, the law of diminishing returns It also involves shared beliefs and values - for example, the maximisation principle, the belief in the efficiency of the market, and in the ability of the general equilibrium framework to elucidate this efficiency Finally, it has a set of exemplars - the predictions which have been gleaned from general equilibrium models in the past

It would seem that something bearing a strong resemblance to a paradigm exists within economics But the growth of knowledge occurs, according to Kuhn, when there is revolution, when one paradigm is supplanted by another The identification of a paradigm alone is not enough for economic methodologists whose concern is with whether the development of economic thought can be described in Kuhnian terms

Bronfenbrenner outlined three possible contenders for Kuhnian revolutionary status in economics "the laissez-faire revolution, the utility revolution and the macroeconomics revolution" (Bronfenbrenner 1971 150) Bronfenbrenner conceded that, while "none of these three revolutions would rank - for a noneconomist, at least - with the Copernican, Newtonian, and Darwinian Revolutions in astronomy, physics and biology they are the best economics has to offer" (Bronfenbrenner 1971 138-139)

The laissez-faire revolution is an obvious contender for revolution Smith, with *The Wealth of Nations*, provided a disciplinary matrix that was lacking in the mercantilist agenda (Katouzian 1980 12) Yet, the very admission that a disciplinary matrix did not exist prior to Smith, shows that the laissez-faire revolution cannot have been a revolution in the Kuhnian sense For Kuhn, a scientific revolution is borne out of battle between two paradigms, if the mercantilist doctrine was not a paradigm, then the victory of the laissez-faire paradigm cannot be the result of a Kuhnian scientific revolution There is also the fact that much of Smith's work involved a synthesis of the disparate theories of previous writers In this sense, Smith's work cannot be held as incommensurable with what preceded it

The utility or the marginalist revolution is also commonly given as an example of revolution m economics Bicchieri pointed to the fact that continuity was as much a feature of the shift from classical political economy to neoclassical economics as was disjointedness

> Marginalism is more general than classical economics, and it succeeds in solving some of its predecessors anomalies Other anomalies, on the contrary, are of no interest to the marginalists as there occurred a shift of emphasis and certain phenomena which were important to the classics ceased to be regarded as relevant (Bicchieri 1989 238)

The fact that marginalism answers some of the anomalies of the previous paradigm suggests that the shift in paradigms can be explained in rational terms by the philosophies of Popper or Lakatos However, there are also elements of disjointedness between the two paradigms which resulted, principally, in the Kuhnian loss of growth theory Thus, Bicchieri concluded "There is a measure of continuity and progress, since more problems are raised and solved by the new theory of value, while most old problems are retained and new solutions are offered to them, but there is neither cumulative growth nor complete preservation of content" (Bicchieri 1989 238) The marginalist revolution therefore does not correspond completely to Kuhn's original description The two paradigms in question cannot be said to be incommensurable

The macroeconomics revolution was also borne out of an attempt to explain the unresolved questions of the accepted paradigm Thus there is, in this revolution too, an element of continuity Blaug argued that Keynesian theory made too much use of the methods of classical macroeconomics to be considered an alternative Kuhnian paradigm (Blaug 1976 161) Moreover, Blaug argued that "the tendency of economists to join the ranks of the Keynesians m increasing numbers after 1936 was perfectly rational" (Blaug 1976 163) It was made, according to Blaug, on the basis of its ability to resolve the problem of persistent unemployment and as such, the revolution can be explained wholly in Lakatosian terms (Blaug 1976 163) (The various Lakatosian analyses of the Keynesian revolution are considered in chapter 3 2)

Bronfenbrenner, too, argued that in spite of the methodological cases which each of these three events in the history of economics brought about, they cannot be described as revolutions in the sense that Kuhn meant He pointed out that, while crises did occur within particular schools of thought and antitheses have been proposed, "what has developed out of the conflict between thesis and antithesis is, m most cases, some sort of synthesis which comprises the normal science, the orthodoxy, the paradigm, or the *Schule* of the next generation or two" (Bronfenbrenner 1971 141) The argument appears to be that in a Kuhnian revolution, such a synthesis would be impossible, due to the incommensurability of competmg paradigms Bronfenbrenner admitted that his three examples can only be described as revolutions, if one is prepared to depart from the Kuhnian [1970a] meaning of the concept of revolution Coats, too, argued that the nature of revolutions in economics differs from that described by Kuhn in respect of the natural sciences (Coats 1969 293)

Bronfenbrenner analysed paradigm-change in terms of a Hegelian dialectic as a process of thesis, antithesis and synthesis He argued that, whereas crisis in Kuhnian analysis leads to competition between two incommensurable paradigms and the eventual succession of one over the other, in Bronfenbrenner's dialectic crisis leads to the eventual synthesis of two opposing points of view (Bronfenbrenner 1971 150) He outlined two principle differences between his dialectic and Kuhnian analysis "Our dialectic allows 'outmoded' ideas longer lives in economics than Kuhn grants them in the natural sciences" (Bronfenbrenner 1971 150) The dialectic approach also allows for recognition of the fact that, in economics, "important advances tend to be major *accretions* without any corresponding rejections of existing paradigms" (Bronfenbrenner 1971 150)

The most obvious example of synthesis in respect of the three revolutions chosen by Bronfenbrenner, is the neoclassical synthesis of Keynesian and classical macroeconomics Bronfenbrenner argued that the fact that such a synthesis could take place, suggests that the two paradigms are not incommensurable However, in Leijonhufvud's view, this synthesis is synthetic

> The 'neoclassical synthesis' proposed a reconciliation of 'Keynesianism' and 'orthodoxy' on a purely formalistic plane Substantively, each of the two world-views that were thus wrenched into the logical appearance of

> consistency was basically uncompromised by the adopted formula Behind the formal screen, they stood poles apart (Leijonhufvud 1976 98)

In Leijonhufvud's view, the work done to establish the microfoundations of Keynesian macroeconomics is simply an attempt to formalise the synthesis, it establishes no foundational common ground between the two paradigms (Leijonhufvud 1976 98) Thus, for Leijonhufvud, there is much less consistency between the two paradigms than Bronfenbrenner's analysis suggests This, however, does not mean that the switch from one to the other can be described as a Kuhnian revolution, quite the contrary If the two paradigms do conflict, then there exists the possibility of deciding on rational grounds which is preferable The two paradigms are therefore incompatible rather than incomparable or incommensurable

Loasby explored the development of the non-neoclassical theory of the firm, using a Kuhnian analysis (Loasby 1971) Loasby traced the development of the non-neoclassical theory of the firm, from Sraffa (1926), to Chamberlain's theory of monopolistic competition, and the development of the managerial and behavioural theories of the firm Despite the existence of two alternative paradigms on the firm, Loasby pointed out that no revolution had occurred (Loasby 1971 882) Loasby implied the following explanation for the absence of a revolution "It [the non-neoclassical theory] has no answer to the questions of efficiency or stability as those questions are traditionally posed" (Loasby 1971 882) This suggests that the two paradigms are incommensurable However, in the Kuhnian framework, the incommensurability of paradigms is no obstacle to scientific revolution *per se* If the paradigms are incommensurable and no revolution has taken place, this would imply that the proponents of the non-neoclassical theory have so far failed to persuade the economics community that the particular questions they have posed are worth more time and effort than the questions posed by the neoclassical theory

These studies point to an absence of Kuhnian revolutions m economics, on the grounds that there is a degree of commensurability between different schools in economics The suggestion among these studies is that Kuhn's structure of scientific revolution fails to describe the development of economic thought

Economic methodologists *have* been able to identify normal science in economics Normal science arises because of the incongruities between theory and data Kuhn outlined a range of activities each forming part of the process of normal science The paradigm prompts scientists to examine more closely the phenomena which the paradigm "has shown to be particularly revealing of the nature of things" (Kuhn 1970a 25) Examples from astronomy include the "stellar positions and magnitude, the periods of eclipsing binaries and of planets" (Kuhn 1970a 25) Another normal science activity is the development of "special apparatus to bring nature and theory into closer and closer agreement" (Kuhn 1970a 27) An example, again from astronomy, is the development of telescopic equipment to "demonstrate the Copernican prediction of annual parallax" (Kuhn 1970a 26) The final strand to normal science is the "empirical work undertaken to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which it had previously only drawn attention" (Kuhn 1970a 27) In addition to empirical work, this third strand also includes "theoretical problems of paradigm articulation" (Kuhn 1970a 33) Kuhn held that "during periods when scientific development is predominantly qualitative, these problems dominate" (Kuhn 1970a 33)

Popper regarded normal science as "a danger to science" (Popper 1970 52) For Popper, the normal scientist is "a victim of dogmatisation" who fails to see the scope of his science (Popper 1970 53) For Kuhn, on the other hand, normal science is the creative task of paradigm-articulation which precisely involves the discovery of the scope of a particular paradigm Argyrous explored the work undertaken on the Permanent Income Hypothesis (PIH) and the Life Cycle Hypothesis (LCH) in the context of Kuhn's definition of normal science (Argyrous 1992) Argyrous examined the attempts to empirically test the PIH by Houthakker and Eisner in the 1950s (Argyrous 1992 239-241) Houthakker's results did not corroborate the PIH Yet, this did not lead to a rejection of the PIH, but rather provided the stimulus for further research into the nature of consumption function Eisner's work provided more favourable results for the PIH, and subsequent studies have been concerned with the further articulation of Friedman's original argument This work on the consumption function closely resembles the empirical work which Kuhn identified as being a part

of the process of normal science In chapter 1 3 above, it was argued that instead of rejecting theories on the grounds of incompatibility between an empirical model of a theory and observed data, econometricians are much more likely to reexamine the links between the theory and possible representative
models Friedman's work on monetarism was described m chapter 1 2 as a curious loop from observation to deduction and back to observation This process is in fact part of what Kuhn called normal science, it is the empirical articulation of an underlying paradigm

Bronfenbrenner argued that the revolutions he identified in economics were not Kuhnian revolutions, because of the element of synthesis which occurred between two competing paradigms This synthesis suggests that the two paradigms are not incommensurable Bronfenbrenner found it more appropriate to analyse revolutions in economics using a Hegelian dialectical approach which allowed for this tendency to render compatible two competing paradigms This dialectic approach is, however, compatible with Kuhn's softened version of incommensurability which appears m *The Essential Tension* (1977) Here, Kuhn allowed for the fact that the new paradigm may address at least some of the issues dealt with by the precedmg paradigm This softening of the concept of incommensurability implies an acceptance that the history of science displays both disjointedness *and* continuity Thus, the revolutions depicted above may well constitute Kuhnian revolutions under this reformulation However, in searching for Kuhnian concepts like paradigm, revolution and normal science, economic methodology seems to have missed the main point of Kuhn's argument Kuhn was not concerned with supplying an alternative theory of scientific development in order to provide a rational reconstruction of the history of science Rather, he argued that the history of science should be a sociological reconstruction, a reconstruction which considers the definitions of progress adopted by scientists themselves, however irrational these definitions might appear to the positivist Somewhat ironically, it is the failure of positivist philosophies of science to provide adequate rational reconstructions of the development of economic thought, rather than a desire to undertake Kuhnian-type analyses of the development of economic thought, which has forced economists to consider the definitions of rationality actually employed by economists themselves This point is considered further in chapter 4 below

Kuhn's work placed the philosophy of science at a crossroads There existed the possibility of reverting to the orthodox philosophy, the externalist philosophy Alternatively, there existed the possibility of exploring further the idea of a sociology or psychology of science Dow has suggested that "the reactions of economists to Kuhn's approach is determined to a considerable extent by their prior methodological stance" (Dow 1985 33) That economic methodologists, in the main, followed the former route, is not therefore surprising, given the general methodological stance that economics was a positive science, more like the natural than the social sciences The influence of positivist philosophy on economic methodology led methodologists to consider the postivist response to Kuhn's attack, namely Lakatos' Methodology of Scientific Research Programmes (MSRP) (Lakatos 1970) The influence of MSRP on economic methodology is the subject of the following chapter

CHAPTER THREE

THE METHODOLOGIES OF SCIENTIFIC RESEARCH PROGRAMMES AND ECONOMIC METHODOLOGY

3 1 THE LAKATOSIAN APPROACH TO SCIENTIFIC PROGRESS

The methodology of scientific research programmes (MSRP) can be interpreted as a response of the orthodoxy to Kuhn's thesis that there can be no objective, community-wide criteria for theory-choice Lakatos attempted to reinstate the notion of objective criteria, while at the same time acknowledging the validity of Kuhn's argument that scientists do not reject all falsified theories Suppe depicted Lakatos as attempting to "steer a middle course between two extremes" (Suppe 1977 704) On the one hand, there was the orthodox, positivist view of science as "a rational enterprise concerned with obtaining objective knowledge" (Suppe 1977 705) On the other hand, there were the "young Turks including Hanson, Feyerabend and Kuhn" with their view of science as "a social phenomenon in which science became a subjective and, to varying degrees, an irrational enterprise" (Suppe 1977 704-705)

Both Popper and Kuhn noted that, in order to make scientific theories testable, they have to be combined with auxiliary assumptions, and that this makes scientific theories probabilistic in nature This leads to a particular problem with the falsificationist thesis if the theory is found to be incompatible with observed facts, exactly what should be rejected, theory or auxiliary assumptions? This problem, which is commonplace in econometrics, is known as the Duhem-Quine thesis (Quine 1953) The Duhem-Quine thesis also involves a recognition that immunising strategies can be made within any axiomatic system, to make that system compatible with the facts Given this, the choice between scientific theories on the basis of their relative corroboration by observed facts, becomes a "'tackers' race'" (Worrall 1978b 331) the preferred theory is the one which is adjusted to take account of observed facts first

The methodology of scientific research programmes (MSRP) focuses on the nature of the modifications which scientists make to their axiomatic systems when faced with refutations It classifies modifications in such a way as to distinguish between those modifications which are progressive, and those which are not In this way, MSRP provides an alternative, objective, criterion for choice between false theories a criterion which is based on the way in which scientists choose to modify their axiomatic systems in the light of conflicting empirical evidence

Lakatos acknowledged that, in order to apply falsificationist principles without an infinite regress, it would be necessary for the scientist to take some *a priori* methodological decisions He listed the extent of decision-making that the Popperian scientist would have to engage in, prior to testing his theories (Lakatos

1970 106-112) Firstly, the scientist must decide that some knowledge constitutes unproblematic background knowledge The scientist "makes unfalsifiable by *fiat* some (spatio-temporally) singular statements" (Lakatos 1970 106) These statements are ‘basic’ or ‘observational’ statements, "but only in inverted commas," to acknowledge the fact that they remain potentially falsifiable by some future test "This decision is then followed by a second kind of decision concerning the separation of the set of *accepted* basic statements from the rest" (Lakatos 1970 106) This foundation gives the scientist some knowledge against which to test his theory The theory is rejected if it conflicts with the ‘observational’ statements held unfalsifiable by the scientific community The third decision involves "specifying certain rejection rules which may render statistically interpreted evidence ‘inconsistent’ with the probabilistic theory" (Lakatos 1970 109) Fourthly, the scientist must make a decision with regard to *ceteris paribus* clauses The factors in the *ceteris paribus* clause must be specified and tested, and a decision must be taken as to whether the clause should become part of the unproblematic background knowledge of the scientist This, for Lakatos, is the most ‘dramatic’ of the decisions to be taken by the scientist "he has to promote one of the hundreds of ‘anomalous phenomena’ into a ‘crucial experiment’, and decide that in such a case the experiment was ‘controlled’" (Lakatos 1970 111)

Lakatos held these decisions to be too risky they are "*too firm*" and "*too arbitrary*" (Lakatos 1970 114) In MSRP, Lakatos presented a methodology which reduced the extent of *a priori* decision-making for the scientist "it places no restrictions on the way a theory may be modified in the event of a clash between theory and evidence, however, once the modified theory has been produced, MSRP's rules will tell whether or not the new theory constitutes progress over the old" (Worrall 1978b 333) This retrospective characterisation of MSRP suggests that it is an internalist philosophy of science, a philosophy which describes rather than prescribes, the criteria for scientific progress In fact, it contains both prescriptive and descriptive elements

For both Popper and Lakatos, scientific progress is denoted by the elucidation of novel facts For Popper, the elucidation of empirically corroborated novel facts by a theory is an indicator of its versimilitude "an accidentally very improbable agreement between a theory and a fact can be interpreted as an indicator that the theory has a high verisimilitude" (Popper 1979 103) For Lakatos, it is the elucidation of empirically corroborated novel facts which creates the demarcation between science and non-science "a theory is 'acceptable' or 'scientific' only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts" (Lakatos 1970 116) Lakatos argued that verisimilitude had to be provided with an inductive foundation

> Only such an 'inductive principle' can turn science from a mere game into an epistemological rational exercise, from a set of lighthearted sceptical gambits pursued for intellectual fun into a - more serious - fallibilist venture of

approximating the Truth about the Universe (Lakatos 1971a 101)

Lakatos used the notion of corroborating instances to construct an "index of verisimilitude" (Zahar 1982 37) For Lakatos, presumably, the probabilities of *all* false theories are not zero, as Popper assumed them to be If the probabilities of false theories can be ranked, then some criterion can be adopted which enables a choice to be made between theories

Lakatos accepted the Kuhn/Duhem argument, that theories are developed within networks rather than as singular entities "series of theories are usually connected by a remarkable *continuity* which welds them into *research programmes*" (Lakatos 1970 132) A research programme is "a series of theories where each subsequent theory results from adding auxiliary clauses to the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor" (Lakatos 1970 118)

The concept of a research programme is more structured than Kuhn's 'paradigm', in that Lakatos outlined more clearly how theories, auxiliary clauses, and methodological rules interact to form the disciplinary matrix Lakatos also outlined how a research programme might develop through time A research programme is held to be "*theoretically progressive* if each new theory predicts some novel, hitherto unexpected fact" (Lakatos 1970 118) If these novel facts are also empirically corroborated, then the research programme is also "*empirically progressive*" (Lakatos 1970 118) In contrast to falsificationism, MSRP only requires that a research programme "display an *intermittently progressive empirical shift*" (Lakatos 1970 134) In other words, the scientist need not be continually concerned with the empirical corroboration of his novel facts This suggests that there are periods of research within a particular research programme which focus on the elucidation of theoretical novel facts alone

In accepting verisimilitude as a method of theory-evaluation, Lakatos necessarily reduced the importance of falsification Koertge argued that "Lakatos' position is in fact an *inversion* of Popper's basic views" (Koertge 1978 269) The Lakatosian scientist is certainly less concerned with the falsification of the novel predictions of his theory, and more with the extent to which they are confirmed by empirical evidence While not denying that refutations are a part of the scientific process, Lakatos did believe that they are largely "irrelevant" (Lakatos 1970 136) Rather, "it is the 'verifications' which keep the programme going, recalcitrant instances notwithstanding" (Lakatos 1970 137) To discard the first specifications of a theory on the assumption that the refutation is not caused by faults in the background knowledge or the data, is, for Lakatos, too risky Lakatos argued that, far from ensuring progress, refutations can actually be detrimental to a developing research programme "To give a stern "refutable interpretation" to a fledgling version of a programme is dangerous methodological cruelty" (Lakatos 1970 151) Lakatos continued, "it may take decades of theoretical work to arrive at the first novel facts and still more time to arrive at *interestingly testable* versions of the research programmes" (Lakatos 1970 151) By refuting theories at the first opportunity, falsificationism could result in the rejection of potentially progressive theories MSRP would allow the scientist to make a choice between two theories even where neither had been falsified The only requirement for establishing progress is that a theory, T_2, account for the successes of its rival, T_1, and predict more novel facts than T_1

A research programme is degenerative, not because of anomalous observations *per se*, but rather when it fails to deal with these anomalies by the generation of further novel facts (Lakatos 1970 118) In this way, a research programme may retain a theory which has been refuted, and still be classified as progressive Lakatos argued that "it may be rational to put the inconsistency into some temporary, *ad hoc* quarantine, and carry on" (Lakatos 1970 143) This course of action is rational *if* the programme continues to generate novel facts This distinction between degenerating and progressive research programmes highlights the demarcation criterion used by Lakatos "a theory is 'acceptable' or 'scientific' only if it has excess content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts" (Lakatos 1970 116)

Since ultimately a research programme is deemed progressive when it generates empirically corroborated novel facts, it must contain some form of background knowledge against which to empirically corroborate these facts This knowledge forms what Lakatos called the "hard core" of the research programme Lakatos outlined the hard core of Newton's research programme as being Newton's three laws of dynamics and the law of gravitation (Lakatos 1970 133) The hard core appears to be analogous to the background knowledge of the Popperian scientist However, Lakatosian scientists take a methodological decision *not* to test the propositions of the hard core of their research programmes, while the Popperian scientist is instructed to only tentatively accept the validity of his background knowledge MSRP is, therefore, much more overtly conventionalist than Popper's falsificationism

> From conventionalism, this methodology [MSRP] borrows the licence rationally to accept by convention not only spatio-temporally singular 'factual statements' but also spatio-temporally universal theories (Lakatos 1971a 101)

In order to prevent the testing of the hard core, each programme comes equipped with a "negative heuristic" (Lakatos 1970 133) The negative heuristic does not "allow 'refutations' to transmit falsity to the hard core as long as the corroborated empirical content of the protecting belt of auxiliary hypotheses increases" (Lakatos 1970 134) As long as the research programme continues to predict novel facts, the hard core remains immune from criticism

The hard core becomes hardened over time, as scientists sift through theores in order to decide which are foundational The propositions of the hard core, once hardened or finalised by the scientists working within the programme, would appear to be permanent The negative heuristic would appear to provide an impregnable defence against refutation Worrall argued against this interpretation of a permanent hard core "MSRP would be mad to give this advice, since if it were consistently followed it would endow the first research programme in any field with an eternal monopoly" (Worrall 1978b 333) This would be too much of a concession to conventionalism However, while it is possible to abandon the hard core, Worrall argued that this is "an enormous undertaking which may involve, amongst other things, the development of entirely new mathematical techniques" (Worrall 1978b 334) It is analogous to the abandoning of a Kuhnian paradigm The abandonment of the existing programme, for this is what an abandonment of the hard core essentially entails, creates "a theoretical void" (Worrall 1978b 334)

For the most part, Lakatosian scientists are concerned with the derivation of auxiliary hypotheses within the framework of the programme Yet, this does not make them Kuhnian scientists engaged in the process of normal science By Kuhn's definition, normal science does not entail the prediction of novel facts

In addition to the negative heuristic which protects the hard core, the research programme will also contain a set of positive heuristics which instruct the scientist on the kinds of problems it is his function to solve while working within that programme "The positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants of the research-programme, how to modify, sophisticate, the 'refutable' protective belt" (Lakatos 1970 135) Any auxiliary hypotheses incorporated into the programme must be consistent with the methodological instructions laid down by the positive heuristic, for the programme to remain progressive This adherence to the positive heuristics is what gives a programme its internal cohesion The "heuristic power" of the research programme is defined as the ability of the programme to generate novel facts by following the directives of its positive heuristics (Lakatos 1970 155)

Lakatos outlined three types of *ad hoc* auxiliary hypothesis, the incorporation of which, would render a research programme degenerative *Ad hoc*$_1$ hypotheses are "those which have no excess empirical content over their predecessor" (Lakatos 1971a 125) This is equivalent to Popper's definition of an *ad hoc* hypothesis as one which provides no mdependently testable novel facts *Ad hoc*$_2$ hypotheses are "those which do have such excess content but none of it is corroborated" (Lakatos 1971a 125) In other words, these hypotheses do generate novel facts but they are refuted by empirical evidence Finally, *ad hoc*$_3$ hypotheses are "those which are not *ad hoc* in these two senses but do not form an integral part of the positive heuristic" These last are hypotheses which do predict empirically corroborated novel facts, but whose methodology is not compatible with that of the positive heuristics of the research programme

Scientific progress, according to MSRP, rests upon the empirical corroboration of novel facts which are derived in a way which is compatible with the positive heuristics of the research programme The definition of what is considered to be a novel fact under MSRP has been modified somewhat since Lakatos' original definition, and no clearcut definition exists Carrier provided an outline of the debate over the nature of scientific novelty (Carrier 1988)

Lakatos' original definition of a novel fact was one which was "inconsistent with previous expectations, unchallenged background knowledge and, was forbidden by the rival programme" (Lakatos and Zahar 1976 375) Zahar pointed out the problem which arises if *only* previously unknown facts count as novel facts "We should, for example, have to give Einstein no credit for explaining the anomalous precession of Mercury's perihelion, because it had been recorded long before General Relativity was proposed" (Zahar 1973 101) Zahar argued that it should be permissable to include known facts as novel facts, *provided* those facts "did not belong to the problem-situation which governed the construction of the hypothesis" (Zahar 1973 101) In other words, the same fact cannot be used as an initial assumption of a hypothesis, and at the same time used as corroborative support for that hypothesis Zahar's modification of the definition of novelty provided by Lakatos was criticised on the grounds that it personalised the process of scientific discovery (Carrier 1988 208) In other words, novelty was defined in terms of the knowledge held by the individual scientist at the time he made a particular discovery Worrall argued that novelty should not be assessed in personal terms, but rather on the basis of the relationship between the purported novel prediction, the theory from which it is derived, and the heuristics of the programme within which the theory is developed (Worrall 1978 326)

There are, then, two types of novel predictions those which predict previously unknown facts, and those which provide novel explanations of known facts In order to render a programme progressive, both types must be empirically corroborated *and* be derived in a way that is consistent with the positive heuristics of the programme

The Lakatosian scientist makes two types of decisions - decisions between theories *within* a research programme, and decisions *between* research programmes

Under MSRP, the choice between two theories, T_1 and T_2, depends on the extent to which there is empirical support for the novel facts put forward by each If the novel facts of T_1 are corroborated by empirical facts, e, which in turn contradict the novel facts of T_2, then the scientist will choose T_1 (This is similar to Mill's method of agreement and dıfference) The Duhem-Quıne thesıs argued, however, that "ıt ıs always possıble to produce a T'_2, suffıcıently sımılar to T_2 to be essentıally the "same" theory, whıch *does* entaıl e" (Worrall 1978b 322) To what extent, then, can MSRP offer a crıterıon for theory-choıce whıch allows progress to be determıned ın the face of the Duhem-Quıne thesıs?

It was John Worrall who developed MSRP, after Lakatos' death He agreed wıth Feyerabend's crıtıcısm that verısmılıtude (as adapted under MSRP) faıls to provıde "an objectıve epıstemologıcal ratıonale" for theory-choıce where scıentısts can make these kınds of adjustments

> If MSRP ıs to be *more than* a sımple descrıptıve generalısatıon of scıentısts' past preferences, ıt must gıve ıts methodologıcal rules an at least tentatıve and conjectural underpınnıng of a *general epıstemologıcal kınd* (Worrall 1978b 326)

Worrall held that MSRP does, m fact, provıde such an epıstemologıcal ratıonale, ın ıts argument that theory-choıce ıs ultımately a "*heurıstıc*-relatıve affaır" (Worrall 1978a 51) Koertge (1971) had argued that MSRP was a socıologıcal analysıs of hıstory of scıence, an alternatıve to Kuhn Worrall denıed thıs "No psyches or socıal structures need be ınspected one needs to look only at theorıes, facts, and heurıstıcs" (Worrall 1978b 326)

When faced with two equally empirically corroborated theories, Worrall argued that the Lakatosian scientist will judge between theories on the basis of their relative consistency with the heuristics of the programme He will "distinguish between genuine and *ad hoc* explanations" (Worrall 1978b 323), and choose those theories which comply with the methodological regulations laid down by the heuristics of the programme

The choice between research programmes is dependent on the same criteria that is, on the relative extent of empirical corroboration, and the internal consistency of programmes *vis a vis* their positive heuristics

As a research programme develops, it may produce novel facts which contradict those predicted by another research programme "As the rival research programmes expand, they gradually encroach on each other's territory and the *n*-th version of the first will be blatantly, dramatically inconsistent with the *m*-th version of the second" (Lakatos 1970 158)

Where there are two competing research programmes, R_1 and R_2, R_2 will be the preferred programme if it "explains the previous success of its rival and supersedes it by a further display of *heuristic power*" (Lakatos 1970 155) By heuristic power Lakatos meant "the power of a research programme to anticipate theoretically novel facts in its growth" (Lakatos 1970 155n) R_1 is, then, *less* potentially fruitful in terms of novel facts and is therefore less progressive than R_2, even though R_1 may itself be deriving novel facts

Despite the preference for progressive over degenerative research programmes, MSRP states that scientists should hesitate before rejecting a degenerating research programme It is always possible that a degenerating programme will self-rejuvenate and start to predict novel facts once again Such a rejuvenation involves an *internal* revolution, a revolution *within* the research programme "When a research programme gets into a degenerating phase, a little revolution or a *creative shift* in its positive heuristic may push it forward again" (Lakatos 1970 137) This "creative shift" involves the introduction of hypotheses which modify the positive heuristics so that the programme can begin to predict novel facts once again

However, these hypotheses are obviously inconsistent with the positive heuristics of the degenerating programme, since they cause the positive heuristics to be modified As such, they are defined by Lakatos as *ad hoc*$_3$ It would appear then that, while some *ad hoc*$_3$ hypotheses render a programme degenerative, others shift the programme out of its degenerative state back into a state of progress Lakatos acknowledged this possibility with *ad hoc*$_3$ hypotheses "I now see that any "creative shift" is *ad hoc* in my sense" (Lakatos 1971b 176) It would clarify the issue to add to Lakatos' list of *ad hoc* with the following definition An *ad hoc*$_3$ hypothesis which is applied to a degenerating programme, and initiates a modification of that programme's heuristics in such a way that it provides a necessary creative shift, is an *ad hoc*$_4$ hypothesis It would seem reasonable to assume that *ad hoc*$_1$ and *ad hoc*$_2$ hypotheses cannot produce creative shifts in degenerating programmes, because they do not entail empirically corroborated novel facts It is only, therefore, a hypothesis which predicts a novel fact, albeit in a way inconsistent with the degenerating programme's heuristics, that can possibly produce a creative shift in that programme Thus, scientists can be justified in continuing to work within R_1, in spite of the fact that it is less progressive than R_2, in the hope of a creative shift in R_1

How can the Lakatosian scientist choose between "empirically equivalent" research programmes? Clearly, where the novel facts of two research programmes are empirically corroborated "then neither programme has 'superseded' the other" (Worrall 1978a 63) As in the choice between empirically equivalent *theories*, Worrall held that it is possible to choose between empirically equivalent research programmes on the basis of internal consistency of each programme with its heuristics Both R_1 and R_2 may have empirically corroborated novel facts, but where the novels facts of R_1 are *ad hoc*$_3$, then R_2 is the more progressive programme

Worrall pointed to another type of decision - the assessment of two research programmes, where each has some empirically corroborated content which the other does not have Clearly, the choice of one over the other will entail a loss of content

(Kuhnian loss) In this case, Worrall argued that a consideration of the relative internal consistency of each research programme with respect to its heuristics "provides a clear rationale for the preference of one programme over the next even if a loss of explanatory content is involved" (Worrall 1978a 63) Worrall argued that, where the extent of empirical corroboration cannot distinguish between theories, the relative heuristic power of research programes is an equally objective criterion upon which to base theory or programme preference Moreover, the "heuristic power" of a programme can be taken as an indicator of that programme's potential for novel fact prediction Thus, Worrall's heuristic criterion is an objective alternative to Kuhn's act of faith, in the decision to work within a particular research programme/paradigm

MSRP, with Worrall's additions, seems to defend the orthodox position in the philosophy of science from the Kuhnian attack It provides an answer to the Popperian dilemma that scientists do in fact retain false theories However, it stops short of a psychological or sociological explanation for the retention of false theories, by allowing for the reintroduction of inductive criteria for theory-choice The notion that the more progressive programme is the one with the greater empirical content allows for a rationalist position that there is continuity in the history of science In the case where two programmes are empirically corroborated to the same extent, heuristic criteria provide the basis for a choice between them Finally, it is rational for the scientist to continue to work within a degenerating research programme in the hope of a future creative shift

In addition to providing criteria by which scientists could choose between competing scientific theories, Lakatos also provided criteria by which philosophers of science could choose between competing theories of scientific rationality or methodologies In Lakatos' methodology of historiographic research programmes (MHRP), the actual history of a science is used to test the competing methodologies through the method of rational reconstruction

In Lakatosian terms, a rational reconstruction describes only the interpretation of the history of a science according to a particular theory of scientific rationality This is a narrower definition than thatemployed by Kuhn, who argued that all history, whether told by philosophers, historians, or scientists themselves, will be a reconstruction of historical events (see chapter 2 1)

Lakatos conceded that certain historical events will always remain incapable of explanation by any theory of scientific rationality "no rationality theory will ever solve problems like why Mendelain genetics disappeared in Soviet Russia in the 1950s" (Lakatos 1971a 102) Despite this, Lakatos argued that his methodology of historiographic research programmes provided criteria which would enable the methodologist to have an objective choice between theories of scientific rationality (1971a 102)

The methodology of historiographic research programmes (MHRP) works in the following way While the rational reconstruction contains all the historical events that the particular theory of scientific rationality can explain, the historian should "indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction" (Lakatos 1971a 105) The preferred theory of scientific rationality is that which explains most of the historical events in the development of a science In assessing a particular theory of scientific rationality, the methodologist is therefore using a "resemblance criterion" (Burian 1977 31) He is comparing the rational reconstruction of the history of a science with the actual history of a science The closer the correspondence between the two, the better the theory of scientific rationality MHRP, therefore, involves the quasi-empirical use of the actual history of a science as an arbiter between the rational reconstructions of different theories of scientific rationality (Zahar pointed out that Popper had proposed a meta-methodological framework similar to MHRP in *Die Beiden Grundesprobleme*, which he wrote in 1930-31 (Zahar 1982))

Lakatos, using MHRP, concluded that Popper's falsificationism represents progress on previous theories of scientific rationality because it "enabled the historian to interpret more of the *actual* basic value judgements in the history of science as rational" (Lakatos 1971a 117) Popper's theory internalised more of the history of science, and in addition, "predicts (or, if you wish, 'postdicts') novel historical facts, unexpected in the light of extant (internal and external) historiographies" (Lakatos 1971a 118)

There are a number of underlying assumptions of MHRP which call into question the resemblance criterion as a means of choosing between theories of scientific rationality MHRP assumes that "*all methodologies function as historiographical theories and can be criticised by criticising the rational historical reconstructions to which they lead*" (Lakatos 1971a 105) However, it is not the aim of all theories of scientific rationality to describe the actual historical events of science If the goal of the externalist philosopher of science is to provide a description of how science *ought* to be done, then where is the onus on him to ensure that his theory of scientific rationality provides a close account of the actual historical events of science? If the resemblance between his rational reconstruction and the actual history of science is weak, then he need only retort that "the 'science' under discussion falls short of what 'science' ought to be" (McMullin 1970 24) However, McMullin argued that "a practitioner of PSE cannot be wholly unconcerned about serious divergences between his own account of the nature of science and the course science has actually followed" (McMullin 1970 28) In practice, externalist philosophers of science *have* looked to the history of science for examples of scientific progress which support their particular theories of scientific rationality There is a "burden of proof on the philosopher to show that his 'rational reconstructions' of the epistemological problems faced by scientists are relevant and applicable to real theories" (Burian 1977 8)

What exactly does the corroboration of a rational reconstruction by the history of a science say about the theory of scientific rationality underlying that reconstruction? MHRP assumes that a high degree of resemblance between a rational reconstruction and actual history means that the criteria suggested by the theory of scientific rationality are in fact the criteria adopted by scientists themselves Radnitzky disputed the validity of this assumption

> The situation appears to be analogous to the justification of a technology which is based merely on observed correlaions for which no causal explanation has been given, and with reference to which it is thus not known whether or not the variables are causally related (Radnitzky 1976 517)

Radnitzky argued that an historical appraisal of a theory of scientific rationality must be accompanied by some discussion as to why the rational reconstruction is compatible with the history of a science "to justify M [a particular methodology], to give good reasons why M leads to success, the methodologist has to embark on 'praxio-logical' argumentation" (Radnitzky 1976 517-518) In other words, it must be proven that scientists actually adopted the criteria advocated by the theory of scientific rationality

In using history of a science in the quasi-empirical way suggested by MHRP, there arises the problem of whose interpretation of science to employ as arbiter Lakatos held that the value judgements of the scientific elite comprise the history which should be used to test rational reconstructions (Lakatos 1971a 117) But in a science made up of competing research programmes, which scientific elite's value judgements should be used to formulate the history of the science? What of Kuhn's argument that the history told by scientists themselves will also be a rational reconstruction (Kuhn 1977 15)? Kuhn hightlighted the tendency of the scientific elite to rewrite the history of their science in order to stress the compatibility between the present paradigm and those which dominated in the past (Kuhn 1970a 140) If this is the case, then how can the comparison of the philosopher's rational reconstruction with that of the scientist say anything about the worth of the theory of scientific rationality underlying the philosopher's rational reconstruction?

Shearmur has argued against the comparison of rational reconstructions to the "moves made by a 'scientific elite'" on the grounds that

> this has the consequence of taking us away from Popper's Weberian pluralism to something closer to Hegel's monism m historiography, in which there is just one story to be told (Shearmur 1991 44)

In other words, inherent in MHRP is the notion that there is a single rational way of practising science It is at this meta-methodological level that the distinctions between Kuhn and Lakatos are at their most obvious Lakatos is often portrayed as a compromise between Popper and Kuhn In fact, MHRP perpetuates the positivist myth that there is a single, optimal methodology for the natural sciences, a myth which both Popper and Kuhn were anxious to dispel

Hall agreed with Lakatos that, "we would generally be inclined to accept scientists' own basic value judgements about what is good science and what isn't" (Hall 1978 157) But how is a list of 'good science' to be drawn up? Nola highlighted two problems (Nola 1987 473) Firstly, "criteria are needed for individuating members of the scientific elite whose judegements are then used to generate the basic list" (Nola 1987 473) Secondly, "we must ensure that their answer is not tainted by an appeal to any methodology to ascertain whether or not some theory is a piece of great science" (Nola 1987 473)

Hall did concede that "nobody would want to say that *all* of scientists' judgements about science are correct" (Hall 1978 157) Yet, assessing rational reconstructions on the grounds of their compatibility with the value judgements of the scientific elite does involve "the *a priori* assumption that the best example of present (and past) scientific method cannot be improved upon" (Koertge 1976 366) If this is the case, what is the need for the philosophy of science?

Lakatos' methodology of historiographic research programmes has received a substantial amount of criticism If the actual history of a science is taken to be the rational reconstruction of that science by scientists themselves, then it is unclear what the comparison of the scientist's rational reconstruction with that of the philosopher's reconstruction says about the philosopher's rational reconstruction It is also unclear how the relative compatibility or resemblance of different philosophers' reconstructions can constitute a criterion by which to judge theories of scientific rationality

This conclusion poses a particular problem for economic methodology The past ten years have seen a plethora of Lakatosian analyses of the history of almost all branches of economics These analyses have mainly consisted of Lakatosian reconstructions of the history of economic thought The suggestion, generally implicit, is that because MSRP comes closer to the actual history of economics, it is a better methodology for economics than Popperian falsificationism This is a meta-methodological conclusion based on Lakatos' MHRP Yet, if one takes into consideration the arguments against MHRP, this conclusion is somewhat tenuous The following section considers some of the difficulties and confusions which have arisen in the application of Lakatosian analysis to economics Specifically, it considers the debate as to the nature of the novelty inherent in Keynesian economics

3 2 NOVEL FACTS IN ECONOMICS

The falsıficatıonıst 'experıment' ın economıc methodology was not a success m that ıt faıled to provıde a feasıble set of objectıve crıterıa for theory-choıce ın economıcs However, the preoccupatıon wıth falsıficatıonısm had embedded twentıeth century economıc methodology ın externalıst phılosophy of scıence

After the faılure of Popper's verısımılıtude, "Lakatos has to choose between epıstemologıcal anarchısm and mductıvısm" (Andersson 1986 241) The choıce was the same for economıc methodologısts Judgıng by the amount of Lakatosıan studıes on economıcs, the majorıty of economıc methodologısts followed ın the tradıtıon set by the London School of Economıcs, and adopted MSRP (Schabas wrote "Neıl de Marchı ınforms me that there are some seventy artıcles by economısts addressmg the Lakatosıan model alone"(Schabas 1992 196n) Thıs gıves an ıdea of the amount of resources devoted to Lakatosıan analyses of economıcs) Thıs adoptıon of MSRP m the late 1970s, ıs ındıcatıve of a lag between economıc methodology and the phılosophy of scıence (Rosenberg 1986 129) By thıs tıme, phılosophers of scıence had gıven up attemptıng to defıne concepts lıke excess content and novel fact, and had begun to look beyond objectıvısm (Rosenberg 1986 135) It ıs not perhaps surprısıng that economıc methodology, ın the maın, faıled to follow thıs shıft ın the phılosophy of scıence There was the fact that MSRP was m the LSE tradıtıon as mentıoned above, but there was also the fact that there are distinct parallels between MSRP and Friedman's methodology of positive economics MSRP provided a "less-bizarre soundmg replacement for Friedman's unrealism-of-assumptions methodology (de Marchi 1991 6) It provided epistemological justification for the retention of false and unfalsifiable assumptions, it described the to-ing and fro-ing between theory, model and data as legitimate work within the protective belt, it suggested that the acceptance of those theories that predict well most of the time, might be more than instrumentalism As with Popper and Kuhn, practising economists adopted Lakatosian *terminology*, and economics is now replete with research programmes, as it once was with paradigms Nearly every possible research programme in the history of economics has been investigated for Lakatosian novel facts

Despite this, Rosenberg has argued that in adopting MSRP, economic methodologists "have, as it were, attached themselves to a degenerating research program" (Rosenberg 1986 136) Has the time invested in applying MSRP to the history of economics been wasted time? This section considers the difficulties in applymg Lakatosian concepts m economics, as they arose m the debate about the nature of Keynesian economics, principally between Blaug (1976,1990,1991a) and Hands (1985b,1990)

Blaug (1976) was principally concerned with the extent of Kuhnian revolution in economics He held that the level of continuity (lack of incommensurability) between

Keynesian macroeconomics and classical macroeconomics marked the Keynesian revolution, not as a revolution in the Kuhnian sense, but rather as a move from a degenerating to a progressive research programme (Blaug 1976 162) For Blaug, the Keynesian method was still closely tied to that of the preceding neoclassical programme

> Keynes leaned heavily on the concepts of general equilibrium, perfect competition, and comparative statics, making an exception only for the labour market, which he seems to have regarded as being inherently imperfect and hence always in a state, not so much of disequilibrium as of equilibrium of a special kind (Blaug 1976 161)

Blaug identified the hard core of the Keynesian programme as highlighting the possibility of "pervasive uncertainty and the possibility of destabilizing expectations", the auxiliary hypotheses are "the consumption function, the multiplier, the concept of autonomous expenditures, and speculative demand for money, contributing to stickiness in long term interest rates", the heuristic is embodied in "national income accounting and statistical estimation of both the consumption function and the period-multiplier" (Blaug 1976 162)

Blaug identified "novel aspects of Keynes", these were "the tendency to work with aggregates to concentrate on the short period and, thirdly, to throw the entire weight of adjustments to changing economic conditions on output rather than prices" (Blaug 1976.162) These are, however, changes in heuristics governing macroeconomics, as distinct from the novel facts of the Keynesian programme In addition, the Keynesian programme, for Blaug, fulfilled the requirement that it predict novel facts (Blaug 1976 162) MSRP gives two definitions of a novel fact The first was the original Lakatosian definition, that a novel fact is simply a fact hitherto unknown A later modification by Zahar led to the acceptance as a novel fact, that which explained a known phenomenon in a novel way This latter is accepted as a novel fact, provided that the known phenomenon is not used in the construction of the explanation

For Blaug, the Keynesian theory provided the first non-*ad hoc* explanation for persistent unemployment "Its principal novel prediction was the chronic tendency of competitive market economies to generate unemployment" (Blaug 1976 162) For Hands, reviewing Blaug's paper among other Lakatosian analyses in 1985, the explanation for persistent unemployment could *not* be interpreted as a Lakatosian novel fact "Is it true that the concept of unemployment was not used in the construction of the theory? No, Keynes fails here also Blaug makes it quite clear that *The general theory* was written *precisely* to explain unemployment" (Hands

1985b 8-9)

Hands' argument was that since Keynes' theory was developed to explain persistent unemployment, it used unemployment as an initial assumption and therefore could not also explain the phenomenon in a novel way For Hands, the theory in question had to explain the known phenomenon as a by-product This fits in with Zahar's explanation of why he modified Lakatos' earlier definition (Zahar 1973 101) Zahar argued that even though General Relativity explains Mercury's perihelion, Einstein could be given no credit for this explanation under Lakatos' original definition, since Mercury's perihelion had already been documented (Zahar 1973 101) The difference between Einstein's explanation for Mercury's perihelion, and Keynes' explanation for persistent unemployment, is that General Relativity did not set out to explain Mercury's perihelion, rather the explanation is a by-product On the other hand, Keynes' theory set out explicitly to explain persistent unemployment, The phenomenon of persistent unemployment *per se* cannot, therefore, be considered a novelty Lakatos conceded that "a new research programme which has just entered the competition may start by explaining 'old facts' in a novel way," but argued that it must eventually produce "'genuinely novel' facts" (Lakatos 1970 156) His example was that of Bohr's theory of wave mechanics, which explained the already-observed Balmer formula

> Balmer merely 'observed' B_1 that *hydrogen lines obey the Balmer formula* Bohr predicted B_2 that *the differences in the energy levels in different orbits of the hydrogen electron obey the Balmer formula* (Lakatos 1970 156)

Thus, to be 'Lakatosian novel,' Keynes would have had to do more than provide a new interpretation of an old fact Hands argued that there are no other components of *The General Theory* which can be considered as independently novel The marginal propensity to consume, liquidity preference, and the marginal efficiency of capital cannot be considered as novel, because they are "*used explicitly in the construction of the theory*" (Hands 1985b 9) For Hands, there is only one possible source of Lakatosian novelty in Keynesian theory "it could be argued that the Phillips curve and the ensuing related literature represented a novel, and temporarily corroborated fact for *The general theory*" (Hands 1985b 9) Hands argued that a Lakatosian rational reconstruction *cannot* explain economists' enthusiasm for Keynesian theory He therefore concluded his argument to be "a negative appraisal" of MSRP (Hands 1985b 13)

Ahonen (1989, 1990) attempted to defend Keynes, Blaug, and MSRP against Hands' arguments He used Lakatos' argument, which came in turn from Popper, that the preferred theory is the more 'general' one (Lakatos 1970 124)

> Einstein's theory is better than Newton's theory *because* it explained everything that Newton's theory had successfully explained, and it explained also *to some extent* some known anomalies and, in addition, forbade events about which Newton's theory had said nothing, moreover, *at least some* of the unexpected excess Einsteinian content was in fact *corroborated* (Lakatos 1970 124)

In parallel, Ahonen argued that Keynes' theory was more general than its classical predecessor, on the grounds that, "it explained everything that classical theory had explained, plus phenomena which had become anomalous in classical theory (such as involuntary unemployment and the coexistence of unemployment and equilibrium) It also forbade the neutrality-hypothesis of money, which classical theory had taken for granted" (Ahonen 1989 259) For Ahonen, the novelty of Keynesian theory lies in liquidity preference (Ahonen 1989 262) Hands had argued that liquidity preference could not be a novel fact, since it was used in the construction of the Keynesian theory Ahonen criticised this definition of novelty which Hands had used

> What does Hands mean by saying that a fact is used when 'constructing' a theory? Obviously he cannot mean its use as both explanans and explanandum, because that would imply circle reasoning It appears that the

> awareness of an empirical fact is equivalent to using it in the construction of a theory (Ahonen 1989 263)

Hands' rejoinder to Ahonen was to agree that the definitions of novelty he had used were bad, but he held "the problem is, these *are* the Lakatosian definitions of novelty" (Hands 1990 73) Hands correctly interpreted Lakatosian novel explanation as coming from an unexpected explanation for a well-known phenomenon Ahonen was mistaken in his argument that Hands had made awareness of a fact equivalent to the use of that fact in the construction of a theory Hands denied that Keynesian theory is more general than classical theory and therefore the more preferable

> Keynesian theory does not apply to a broader class of phenomena than classical theory, both Keynesian and classical theory are 'about' developed capitalist economies The difference is not in what the theories are about, the difference is in how the two theories characterize equilibrium, classical theory requires full employment for equilibrium, Keynesian theory does not (Hands 1990 74)

In Lakatosian terms, the issue of which theory is more general is a moot one, if neither theory predicts novel facts The universality of a theory is not the thing which makes it progressive Given the dearth of novel facts in Keynesian theory, any generality it may possess over the classical theory, is necessarily *ad hoc*

Blaug conceded that "the concept of 'novel facts' is not unproblematic" (Blaug 1991a.172) He went on to define novelty in a stronger way than either Hands or Lakatos had done "it does not include facts which are known before a research program is launched, particularly if these facts are deliberately used in the construction of the program" (Blaug 1991a 172) Zahar's modification only required that well-known facts not be used in the construction of the theory which explains them, Blaug's definition holds all well-known facts to be excluded from novelty Blaug acknowledged the mistake he made in putting forward persistent unemployment as a novel explanation by the Keynesian theory "this may have been a Kuhnian 'anomaly,' but it is not a 'novel fact' in the sense of Lakatos" (Blaug 1991a 172n)

Blaug, however, insisted that there *is* Lakatosian novelty in Keynesian theory

> The principal novel prediction of Keynesian economics is that the value of the instantaneous multiplier is greater than unity and that the more than proportionate impact of an increase in investment on income applies just as much to public as to private investment, and indeed just as much to consumption as to investment spending (Blaug 1991a 182)

The multiplier, Blaug argued, was "an unexpected implication" of Keynes' particular formulation of the consumption function The consumption function itself, along with liquidity preference and the investment function, Blaug conceded, could not be interpreted as novel facts since they had been used in the construction of the theory (Blaug 1991a 187) However, he argued that

> Nevertheless, interpreted in a particular way thay can be employed to predict novel facts, such as that a cut in money wages cannot have a significant effect on aggregate demand, that an increase in government expenditure will have a large, or at least a more than proportionate, effect on aggregate demand, and that a given increase in government expenditure financed by an equal increase in tax receipts will raise national income by the same amount as that given increase - the balanced budget multiplier is unity (Blaug 1991 187)

By elucidating the novel facts of Keynesian economics, Blaug defended Lakatosian appraisal, at least in this particular instance MSRP *can*, for Blaug, explain the shift from classical macroeconomics: "it was therefore perfectly 'rational' in the strict sense of Lakatos for economists in the 1930s to have adopted Keynesian economics" (Blaug 1991a 188)

Hands, in his comments on Blaug's paper (which originally appeared in Italian in 1987), conceded that the government expenditure multiplier is indeed a Lakatosian novel fact (Hands 1990 76) Having granted this much, Hands went on to raise a *meta-methodological* question "Why would we want to accept the position that the sole necessary condition for scientific progress is predicting novel facts not used in the construction of the theory?" (Hands 1991a 78)

Blaug attributed Hands', by this stage ambiguous, position as being due to a failure to separate MSRP from Lakatos' meta-methodological framework, MHRP (Blaug 1990 103) MHRP judges methodologies by their ability to 'internalise' the history of a science, that is, by their relative ability to show the development of a science to be consistent with their particular theory of rationality

In his first paper, Hands had argued that since there were no Lakatosian novel facts to be found in Keynesian economics (commonly held as an example of progress by economists), MSRP was not appropriate to the analysis of the history of economics "it will be demonstrated that the MSRP's strictly empirical criteria of 'progress' makes a Lakatosian rational reconstruction of the most successful episodes in the history of economic thought virtually impossible" (Hands 1985b 2) If there are no Lakatosian novel facts tobe found in Keynesian economics, then according to MHRP, MSRP fails to show this particular development in economics to be consistent with the theory of rationality underlying MSRP

Five years later, Hands conceded that there *are* indeed Lakatosian novel facts in Keynesian economics But he argued that these novel facts alone do not explain the success of the Keynesian theory "I still do not accept this novelty as the general *reason for the progress* of the Keynesian revolution, nor do I believe that it is appropriate to argue that such novelty 'accounts for the rapid approval' of the profession" (Hands 1990 77) Hands did not say exactly what factors he believed to be instrumental in explaining the Keynesian revolution, but he did discuss, in general terms, the criteria economists (along with all other scientists) use to choose between theories

> We in economics and those in every other branch of science choose theories because they are deeper, simpler, more general, more operational, explain known facts better, are more corroborated, are more consistent with what we consider to be deeper theories and for many other reasons (Hands 1990 78)

In 1990, Hands' argument shifts from being an argument against MSRP to being an argument against MHRP His argument is *now* that the validity, or otherwise, of a methodology cannot be established on how well the theory of rationality underlying that methodology corresponds to the actual history of economics The discovery of novel facts in Keynesian economics is not, for Hands, enough to recommend MSRP as a method of appraisal in economics The existence of novel facts in Keynesian economics says nothing about the value of MSRP as a method of appraisal in the history of economics The ability of MSRP to internalise events in the history of economics is a correlation, it does not necessarily imply any causal relationship

Hands made the same argument in respect of MHRP as Radnitzky had done (Radnitzky 1976) But this is a criticism of that meta-methodology which Hands first used to criticise MSRP

Blaug's answer to Hands' question, why economic methodologists should use MSRP, betrays the influence of the orthodox philosophy of science on economic methodology

> In terms of MSRP, the answer is because 'scientific progress' is progress in achieving 'objective knowledge' and the only way we can be sure that we have acheived objective knowledge is to commit ourselves to the prediction of novel facts (Blaug 1990 103)

Implicit in Blaug's argument is that economic methodology requires such an objective criterion However, in his earlier paper, Blaug too had raised the question as to whether the criterion supplied by MSRP was indeed appropriate for defining progress in economics

> MSRP may not fit the history of economics economists may cling to 'degenerating' research programmes in the presence of rival 'progressive' research programmes while denying that the 'degenerating' programme is in

need of resuscitation because they are suspicious of hard data, inclined to assign low priority to the discovery of novel facts (Blaug 1976 176)

If there is not a good fit between MSRP and the history of economics, and if novel facts alone cannot explain progress in economics, why, one might ask, has so much time and effort been spent by economic methodologists on appraising different economics research programmes? There is a general belief that these appraisals have been useful, even in Hands (1990) "despite the fact that novel predictions have been few and far between, hard cores, and heuristics abound, and Lakatos' general link between empirical prediction and theoretical progress has helped initiate a serious historical examination of the role of econometrics and testing in economic theory" (Hands 1990 79) Blaug implied that it was useful to analyse the history of economics in terms of MSRP, *precisely to find out why* MSRP does not describe progress in economics (Blaug 1976 176)

In his comprehensive review of the Lakatosian analyses of the history of economics to date, de Marchi, in contrast to Blaug and Hands, argued that there is a "very considerable overlap in perspective between Lakatos' methodology and the 'official' methodology of mainstream economics" (de Marchi 1991 2) Moreover, this overlap represents "something like a natural fit and signals a deeper underlying agreement about the nature and ideal practice of science" (de Marchi 1991 3) De Marchi argued that the link between the history of economics, and MSRP, is more than a series of coincidental correlations

> Economists' scepticism about data mining, their sensitivity to the problems of specification and their discomfort with black-box 'testing', which looks at predictions but leaves the structure and explanatory power of the rival theories obscure, suggest that they share much the same view of the good scientist (de Marchi 1991 4)

De Marchi argued that the Lakatosian rational reconstructions of the history of economics provide a causal link between MSRP and actual progress in economics These analyses provide the sort of "'praxio-logical' argumentation" which Radnitzky held to be necessary in order to validate MHRP as a meta-methodology (Radnitzky 1976 518) Thus, de Marchi used MHRP to argue that MSRP is a good methodology for economics, despite the numerous arguments put forward by philosophers against this type of meta-methodological conclusion (see chapter 3 1)

According to de Marchi, economists *explicitly* set about trying to discover Lakatosian novel facts Their discovery is not a by-product of the application of some alternative criteria for progress, as is suggested by Hands (Hands 1990 77) Implicit m de Marchi's argument is an acceptance that MHRP is a valid method of appraising methodologies, and that the rational reconstruction provided by MSRP comes closest to describing the actual underlying rationality in economics

De Marchi reviewed Lakatosian analyses of the history of economic thought, outlining the particular points of emphasis in these analyses "*theoretical progress*" as distinct from empirical progress in the history of economics, "*how RPs interact*", "*defining and identifying novel facts*" (de Marchi 1991 12-14) De Marchi conceded the difficulty that Blaug and Hands had in identifying Lakatosian novel facts in Keynesian economics, but he argued

> the final gloss by Lakatos, incorporating as novel also the first precise explanations of known facts, has been ignored in the discussions to date, even though, as I suspect, this may be the most obviously relevant to modern economics (de Marchi 1991 14)

'The final gloss' to which de Marchi referred, is Lakatos' recognition that new research programmes may begin "by explaining 'old facts' in a novel way" (Lakatos 1970 156) But this explanation alone is *not* progress for the research programme the new programme must go on to produce "'genuinely novel'" facts (Lakatos 1970 156) Lakatos' example of the development of Bohr's theory of wavelengths clearly shows that "mere theoretical reinterpretation" of old facts is not enough (Lakatos 1970 156)

This 'final gloss' simply leads back to the old problem of identifying genuinely novel facts in economic research programmes

De Marchi pointed out what he sees as failings in the Lakatosian analyses to date For example, he noticed a "reticence about spelling out the hard core and heuristics" of economic research programmes (de Marchi 1991 16) Without this clarification, much of this Lakatosian analysis "looks like an exercise in renaming" (de Marchi 1991 17) There is also the failure to specify exactly what constitutes a research programme in the history of economics De Marchi argued that these incomplete analyses "cannot be seen as tests of the appropriateness for economics of MSRP" (de Marchi 1991 17) De Marchi pointed out that Lakatosian analyses have shown theoretical progress to be of more importance in economics than empirical progress, that economists are more concerned with the generation of theoretical novel facts, and less with their empirical corroboration De Marchi suggested that "if testing is not as important as economists' claims would imply, then perhaps it is also the case that there is (epistemic) value elsewhere than in deduced test implications alone" (de Marchi 1991 16) De Marchi linked this notion that the source of novelty in economic research programmes may lie somewhere other than in the generation of empirically corroborated novel facts, to Lakatos' "late amendment to the notion of novelty," which de Marchi interpreted as permitting theoretical reinterpretations as novelty (de Marchi 1991 16) This amendment has been shown above to be nothing more than an affirmation of Lakatos' original definition of novelty Novelty may lie somewhere other than in novel facts in economic research programmes, but this novelty cannot be classified as Lakatosian novelty

In one of the first applications of MSRP to economics, Leijonhufvud argued that, in economics, "genuinely novel predictions are relatively rarely made, what the 'progressive' economist is usually engaged in is trying to incorporate more 'things that have been well-known for a long time' into a logically consistent structure" (Leijonhufvud 1976 78) This suggests that neoclassical economists might define progress as the ability to incorporate more observed facts into the general equilibrium framework of analysis, and that this definition of progress has much more significance for neoclassical economists than any of the objective definitions of scientific progress put forward by positivist philosophy of science

The Lakatosian analyses to date have revealed that "novel facts may provide a 'clincher' every now and then, like Halley's comet, but they are nowhere near the whole story" (Hands 1990 79) A Lakatosian analysis is, therefore, a useful starting point in the process of discovering what constitutes novelty and progress in economics Lakatosian rational reconstructions should be valued as much for the *discrepancies* they reveal between Lakatosian concepts and the practice of economics, as for the points of *correspondence* The danger is in distorting the history of economics to try to derive a closeness of fit between MSRP rational reconstructions and the history of economic thought "bringing Lakatos to economics promises to yield new insights, bringing economics to Lakatos promises little more than a series of artificially generated congruencies" (de Marchi 1991 18)

Much of work done by economic methodologists in applying Lakatosian concepts to economics is confused and incomplete Confusions arise because of a failure to distinguish between Lakatos' methodological framework, his MSRP, and his meta-methodological framework, MHRP Incompleteness arises from a failure to articulate clearly the research programmes and the novel facts in economic thought

What did the work on Lakatosian concepts in economics hope to achieve? Much of the work reviewed by de Marchi takes the form of Lakatosian rational reconstructions of the history of economic thought Implicit in these analyses is the assumption that a closeness of fit between the history and its rational reconstruction says something good about the compatibility of the methodology of economics and the theory of scientific rationality underlying Lakatos' MSRP In other words, economic methodologists appear to have been using MHRP to validate both the methodology of economics and MSRP However, not all economic methodologists agree that there is a goodness of fit between Lakatosian rational reconstructions and the history of economic thought (eg Blaug 1976, Hands 1990) Yet, this type of research is still encouraged The suggestion from these economic methodologists is that an analysis of the incongruencies between Lakatosian (and other positivist) rational reconstructions and the history of economic thought constitutes an analysis of what it is that economists actually do Thus, the failure to find a closeness of fit between positivist rational reconstructions and the history of economic thought has forced economic methodologists into a search for the definitions of scientific rationality and progress employed by economists themselves In other words, they have been forced into a Kuhnian-type analysis

CHAPTER FOUR

CONCLUSIONS

Economic methodology has been principally concerned with the method of rational reconstruction, and in particular with the reconstruction of episodes in the development of economic thought, using positivist philosophies of science as the underlying theories of scientific rationality

Popperian rational reconstructions of the history of economics have shown that economists do not use falsificationist criteria when choosing between theories The conversion of qualitative economic theory into testable theory requires the adoption of additional assumptions in order to derive quantifiable models A single theory may be represented by a number of differently specified models Testing is confined to these models While models may be refuted and rejected, the underlying qualitative theories rarely are Economic theories are probability hypotheses, and as such may be false over some range of data, while still being good predictors in the range to which they relate For this reason, econometricians are reluctant to reject a theory on the grounds of falsification *per se*

The background knowledge of economists does not provide an objective test of economic theory Observable data are subject to sample-error, and therefore are not accepted by economists as objective tests of theories In addition, the universal laws of the economics background knowledge are metaphysical in nature They have been systematically protected from any empirical testing, rather than explicitly subjected to testing as Popper instructed Section 1 2 shows that the methodological consensus necessary to implement Popperian falsificationist criteria does not exist in econometrics The methodology of econometrics appears to be much closer to instrumentalism than to falsificationism

Falsificationism instructs the scientist to reject all falsified theories But where theories are probability theories, then falsification does not necessarily warrant rejection Scientists, in this case, are likely to have to make decisions between false theories Falsificationism fails to provide objective criteria for choice between two equally corroborated, but false, theories Verisimilitude, Popper's attempt to remedy this deficiency, can only work if an inductivist method of verification is permitted If such a method of verification is permitted, than falsificationist criteria become superfluous Lakatos showed this in his methodology of scientific research programmes These internal deficiencies suggest that falsificationist criteria would not lead to progress in economics

Popper claimed that his situational logic was in fact the methodology of neoclassical economics However, neoclassical economics uses a minimum behavioural assumption, the profit maximisation hypothesis, in order to establish cast-iron control of a situational analysis on behaviour Popper denied that his situational logic involved such a behavioural assumption The rationality principle in neoclassical economics is not empty, as Popper insisted it should be Popper also implied that situational logic entailed the use of falsificationist criteria If this is accepted, then the same problems identified above in respect of falsificationism apply to situational logic

Using MHRP (albeit implicitly), economic methodologists have concludedthat Popperian falsificationism is not an appropriate methodology for economics Popperian rational reconstructions fail to correspond to what the elite of economics put forward as examples of progress in economic thought However, this position would seem to be tenuous in the light of arguments criticising MHRP as a meta-methodology If one accepts these arguments, then Popperian falsificationism could still be a valid methodology depsite its failure to describe progress in economics This would seem to be Blaug's position However, there is a more serious criticism to be levelled against Popperian falsificationism It is that without a method of choosing between two false theories, falsificationism is a deficient methodology for any science

Economic methodologists have tended to adopt a positivist attitude to Kuhnian philosophy of science In other words, they have attempted to identify Kuhnian concepts - normal science, scientific revolution, paradigms, and so on - in the history of economics They have treated Kuhnian philosophy of science as though it were an alternative theory of scientific rationality The failure to identify scientific revolutions in economics has led to the suggestion that Kuhnian philosophy of science is inappropriate for describing progress in economics Again, this is a conclusion which seems to be based on MHRP Mainstream economic methodology has remained focused on this positivist approach to scientific development There has been significantly less of the sociological analysis suggested by Kuhn than of the positivist analysis provided by Lakatos, in economic methodology

Lakatos' MSRP was a positivist answer to Kuhn's suggestion that the search for objective methodological criteria was a waste of time MSRP offers a solution to the Duhem-Quine thesis, it allows for the metaphysical foundations of a science, and justifies the retention of false theories It offers criteria for theory-choice, on empirical grounds in the discovery of novel facts, and on heuristic grounds It is an externalist philosophy, but one which takes account of the actual methodological difficulties which scientists face

De Marchi held that there is a good fit between Lakatosian rational reconstructions and the history of economics (de Marchi 1991 2) Not all economic methodologists would agree (for example, Blaug 1976, Leijonhufvud 1976, Rosenberg 1986, Hands 1990) Section 3 2 shows that, while Lakatosian rational reconstructions correspond more closely to the history of economics than Popperian and Kuhnian rational reconstructions, there is still a problem in reconciling Lakatosian novelty with what is commonly regarded by the economics community as novel This difficulty suggests that Lakatosian rational reconstructions are failing to internalise certain theories which economists themselves consider to be novel

The irony is that the failure to find a resemblance between the actual history of economics and Lakatosian rational reconstructions has forced economists into an examination of how economists themselves define novelty Economic methodologists, like Blaug (1976) and Hands (1990), have argued that in spite of the failure to find Lakatosian novel facts, Lakatosian rational reconstruction of the history of economics is nevertheless useful for this reason Thus, the failure on the meta-methodological level to find the optimal theory of scientific rationality has forced economic methodologists to undertake a Kuhnian-type analysis on the methodological level While these analyses are not full-blown sociological analyses in the format suggested by Kuhn, they nevertheless do provide useful insights into the actual practice of economics and the underlying criteria of progress which economists use

Part two of this dissertation is concerned with the development of international trade theory It compares different philosophical rational reconstructions and their conclusions about the nature of progress in international trade theory (Kuhnian concepts in international trade theory are identified, but this is not to suggest that Kuhn's philosophy of science provided an alternative theory of scientific rationality to Popper's or Lakatos') These rational reconstructions are also compared to the rational reconstruction of the history of international trade theory written by international trade theorists themselves

International trade theory is an interesting choice for this type of study, because it is not one of the primary examples of progress put forward by the elite of economics Historians of economic thought, too, have tended to assume that progress in international trade theory, at least until the early 1980s, has been limited Blaug, for example, has described international trade theory as being "peculiarly prone to the disease of formalism" (Blaug 1992·190)

The pure theory of international trade is to found in all international trade textbooks Its principle characteristic is that of a two country/two good/two factor, general equilibrium model of trade It is argued in the following chapters that the development of international trade theory has been reconstructed by mainstream international trade theorists to emphasise the development of this model Because this rational reconstruction stresses the neoclassical elements of the history of international trade theory, I have called et the neoclassical rational reconstruction This should not be interpreted as a suggestion that neoclassicism provides an objective theory of scientific rationality in the same way that falsificationism or the methodology of scientific research prgorammes do (Popper, in his situational logic, did argue that neoclassical economics used a methodology which was applicable to all the social sciences The problems inherent in situational logic are discussed in chapter 1 4) The definition of rational reconstruction employed here is not the narrow, Lakatosian one, but rather the broader, Kuhnian one Kuhn held that every interpretation of history is a rational reconstruction, be it a philosopher's, an historian's or a scientist's interpretation (Kuhn 1977 15) Rational reconstruction, in the Kuhnian sense, is not therefore a reconstruction which always suggests some objective theory of scientific rationality

The neoclassical rational reconstruction of international trade theory emphasises the development of the general equilibrium model of international trade The history of international trade theory that is presented is therefore continuous rather than disjointed This rational reconstruction omits a substantial amount of work which was not in the neoclassical tradition, but which is now being held as significant by international trade theorists

The aim of the analysis in part two of this dissertation, is not, as would be the case in MHRP, to find a closeness of fit between any particular philosophical rational reconstruction and the rational reconstruction of the elite of international trade theory The purpose of the analysis is not to arrive at any conclusion as to which might be the preferred theory of scientific rationality out of a particular selection Rather, the analysis is more a Kuhnian one Kuhn argued that the philosopher's rational reconstruction will differ from that of the scientific community, because of an "identification of [different] essentials" (Kuhn 1977 15) It is hoped that an analysis of the distinctions between the philosophers' rational reconstruction and the neoclassical rational reconstruction of international trade theory will produce a greater understanding of the nature of progress in international trade theory It is the existence of gaps between philosophical rational reconstructions and the neoclassical rational reconstruction which forces an examination of what it is that trade theorists actually do

PART TWO

A METHODOLOGICAL ANALYSIS OF THE DEVELOPMENT OF INTERNATIONAL TRADE THEORY

CHAPTER FIVE

THE DEVELOPMENT OF INTERNATIONAL TRADE THEORY IN THE CLASSICAL PERIOD

Schmitt gave the standard textbook definition of the distinction between classical, neoclassical and Keynesian economics

> Economics can be said to have evolved in three stages In the first, political economy considered that production was prior to exchange and not determined in exchange In the second stage, economics is founded on the general equilibrium of supply and demand, exchange thus encompasses production In the third stage, production is again pre-eminent all exchange pertains to production (Schmitt 1986 105)

Implicit in this definition is the notion of a methodological distinction between classical political economy on the one hand, and neoclassical economics on the other neoclassical economics operates within a static general equilibrium framework which is in some way different from the framework adopted by classical political economy This distinction is not one accepted by all historians of economic thought The argument that there is a continuity of method from the classical to the neoclassical period was forcefully put by Hollander in respect of Ricardo (Hollander 1987) Ricardo, Hollander argued, "frequently dealt with disturbances (demand changes, innovation, taxation) within a static framework Conversely, Walras extended his own static analysis in the *Elements* to deal with growth" (Hollander 1987 433) Blaug argued that, in the 1930s, "Ricardo was regarded as the virtual inventor of the method of comparative statics and a prime example of the tendency of orthodox economists to emphasize long-run equilibrium values at the expense of any consideration of short-run, disequilibrium adjustments" (Blaug 1985 4) Consider, however, these reconstructions of Ricardo's methodological aims, in the light of Kuhn's argument that the history of a science will tend to be reconstructed in terms of the exemplars of the latest paradigm

> Partly by selection and partly by distortion, the sceintists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific (Kuhn 1970a 138)

There have also been attempts to show continuity from classical political economy to neoclassical economics by using a Lakatosian analysis Baranzini and Scazzieri argued that classical political economy and neoclassical economics constituted two distinct research programmes, but with considerable overlap between them (Baranzini and Scazzieri 1986 5) This suggests that there was an overthrowing of classical political economy in favour of the different hard core propositions of neoclassical economics Blaug, on the other hand, argued that there was no such change "it is evident that the marginalists adopted the 'hard core' of classical political economy but they altered its 'positive heuristic' and provided it with a different 'protective belt'" (Blaug 1976 161) This suggests that the shift from classical political economy to neoclassical economics was only a creative shift within a single research programme, as opposed to a shift from one programme to another This creative shift resulted in a methodological change, but not in a change in the basic laws which underlie economic theory Under this interpretation, the marginalist revolution becomes a revolution in method only The argument that economists tend to view *heuristic* changes, as opposed to any other changes, as signs of progress, is repeatedly made below, in respect of the development of international trade theory

The Pure Theory of International Trade as it is presented in international trade textbooks implies a continuity of methodological purpose in trade theory from the classical era into the twentieth century The Pure Theory presents a static, general equilibrium model of international trade, the origins of which are traced back to Ricardo It is the development of this static theory which a neoclassical rational reconstruction of the development of international trade theory would stress However, in addition to this static theory, a dynamic theory of international trade also developed, the origins of which can be found in Smith and also in Ricardo This chapter explores the development of the static and the dynamic theories of international trade through the classical era It considers the extent to which the twentieth century, neoclassical reconstruction of the development of international trade theory ignores the dynamic theory of international trade, and because of this, misrepresents the history of international trade theory in the classical era Another issue is the extent to which positivist reconstructions can explain the persistence of the dynamic theory as the dominant explanation for patterns of international trade until the 1930s

5 1 THE ORIGINS OF THE STATIC THEORY OF INTERNATIONAL TRADE

Until recently, the standard interpretation of the development of international trade theory in the classical period, was a strictly neoclassical one A prime example of neoclassical rational reconstruction in relation to the development of international trade theory can be found in Chipman (1965) Chipman began his historiographic analysis with the law of comparative advantage, first proposed by Torrens in 1809, and developed by Ricardo in 1817 (Chipman 1965 479-481) One could be forgiven for assuming that Smith had no role to play in the development of international trade theory at all From a neoclassical perspective, Smith's model was not important The principle reason for this is that Smith did not make the assumption that factors of production are immobile It is this which gives *raison d'etre* to the development of a separate theory of international trade

Chipman presented Ricardo's theory of international trade as a two country/two good/one factor model, a precursor to the Heckscher/Ohlin two country/two good/two factor model of the 20th century (Chipman 1965 479) According to Chipman, Ricardo made the necessary assumptions to fully delineate a neoclassical model of trade "Ricardo rather glossed over the question of the interdependence of industries, treating them as integrated, producing one output and using one primary input (labour)" (Chipman 1965 479) For Chipman, the main issue is the failure of Ricardo to give the conditions under which the international terms of trade are established, and he turned almost immediately to Mill (1848) and his elucidation of the law of reciprocal demand, which, not surprisingly given his neoclassical interpretation, Chipman considered to be "one of the greatest achievements of the human intellect" (Chipman 1965 486)

This interpretation of Ricardo's contribution to the development of international trade theory suggests that the hypothesis that Ricardo was in fact more of a neoclassical economist than a classical economist is an accurate one In chapter 7 of his *Principles of Political Economy* (1817), Ricardo outlined his supposedly neoclassical theory of international trade But how much of the modern neoclassical Ricardian model of international trade is in fact a 20th century invention?

Ricardo began his discussion of international trade by pointing out that the gains from trade are the gains to consumers

> No extension of foreign trade will immediately increase the amount of value in a country, although it will very powerfully contribute to increase the mass of commodities, and therefore the sum of enjoyments (Ricardo 1965 128)

The argument that such gains exist, independently of any changes which trade may induce in production, is at the core of the static theory of trade Thus, the static theory of ınternatıonal trade does ındeed appear to have orıgınated wıth Rıcardo's novel fact But how much of the modern neoclassıcal analysıs dıd Rıcardo employ, ın order to derıve thıs novel fact?

Rıcardo dıd not fully agree wıth Smıth that the maın benefıt of ınternatıonal trade ıs capıtal accumulatıon An ıncrease ın profits was not a foregone conclusıon of ınternatıonal trade

> If, ınstead of growıng our own corn, or manufacturıng the clothıng and other necessarıes of the labourer, we dıscover a new market from whıch we can supply ourselves wıth these commodıtıes at a cheaper prıce, wages wıll fall and profıts rıse, but ıf the commodıtıes obtaıned at a cheaper rate, by the extensıon of foreıgn commerce, or by the ımprovement of machınery, be exclusıvely the commodıtıes consumed by the rıch, no alteratıon wıll take place ın the rate of profıts (Rıcardo 1965 132)

It would appear, then, that Rıcardo played down the dynamıc effects of trade, hıghlıghted by Smıth (Smıth's dynamıc theory of ınternatıonal trade ıs examıned ın the followıng sectıon) Contrary to Smıth, Rıcardo argued that "the rate of profıts ıs never ıncreased by a better dıstrıbutıon of labour" (Rıcardo 1965 133) In order to valıdate a separate theory of ınternatıonal exchange, an assumptıon of ınternatıonal ımmobılıty of factors of productıon ıs requıred

> If the profits of capital employed in Yorkshire, should exceed those of capital employed in London, capital would speedily move from London to Yorkshire, and an equality of profits woould be effected, but if in consequence of the diminished rate of production in the lands of England, from the increase of capital and population, wages should rise, and profits fall, it would not follow that capital and population would necessarily move from England to Holland, or Spain, or Russia, where profits might be higher (Ricardo 1965 134)

Directly following this argument, Ricardo put forward the law of comparative advantage, in terms of the well-known example of trade in cloth and wine between England and Portugal (Ricardo 1965 134-136) Ricardo showed that where Portugal had an absolute advantage in the production of both cloth and wine, she could still gain from trade "This exchange might even take place, notwithstanding that the commodity imported by Portugal could be produced there with less labour than in England" (Ricardo 1965 135) The standard neoclassical interpretation of Ricardo's model depicts the gain for Portugal in terms of the labour savings made However, Ricardo was not thinking in terms of a single factor model

> Though she [Portugal] could make the cloth with the labour of 90 men, she would import it from a country where it required the labour of 100 men to produce it, because it would be advantageous to her rather to employ her capital in the production of wine, for which she would obtain more cloth from England, than she could produce by diverting a portion of her capital from the cultivation of vines to the manufacture of cloth (Ricardo 1965 135)

The fact that Ricardo did not himself employ a single factor model has been pointed out by several writers (Findlay, 1984, Gomes 1987, Maneschi 1992) Another distinction between the neoclassical 2x2x1 model, and Ricardo's arguments in his chapter 7, is the issue of specialisation in production The neoclassical model predicts complete specialisation by both countries in one of the traded commodities The argument as to whether Ricardo implied complete specialisation focuses on the following footnote

> It will appear then, that a country possessing very considerable advantages in machinery and skill, and which may therefore be enabled to manufacture commodities with much less labour than her neighbours, may, in return for such commodities, import a portion of the corn required for its consumption, even if its land were more fertile, and corn could be grown with less labour

> than in the country from which it was imported (Ricardo 1965 136)

From this, it would appear that Ricardo did not predict complete specialisation as an outcome of trade It also adds credence to the argument that Ricardo's theory of trade was a multi-factor one (Maneschi 1992 428) However, Ricardo continued

> Two men can both make shoes and hats, and one is superior to the other in both employments, but in making hats, he can only exceed his competitor by one-fifth or 20 per cent , and in making shoes he can excel him by one-third or 33 per cent , - will it not be for the interest of both, that the superior man should employ himself exclusively in making shoes, and the inferior man in making hats? (Ricardo 1965 136)

Viner argued that, in the second part of the footnote, Ricardo is referring to trade between individuals as distinct from trade between two countries, and thus, it cannot be inferred that Ricardo predicted complete specialisation between *countries* as an outcome of trade (Viner 1955 452, quoted in Maneschi 1992 428) This issue of whether Ricardo predicted complete specialisation cannot be resolved on the basis of this footnote alone But there is evidence elsewhere in Ricardo's writings on international trade to suggest that Ricardo was in fact thinking in terms of incomplete specialisation as an outcome of international trade On the evidence of chapter 7 outlined above, Maneschi made the following argument

> It is hard to escape the conclusion that the Ricardian trade model is a multi-factor one, with circulating capital an indispensible concomitant of the employment of labour, and the production of agricultural goods involving land and hence being subject to diminishing returns to labour (Maneschi 1992 428)

A prior argument has been made that Ricardo put forward two models of trade, the static neoclassical one represented in most trade textbooks, and a dynamic theory of trade based on his theory of growth (for example, Findlay 1984, Mumy 1991) The most concise version of Ricardo's dynamic theory of trade is to be found in his 1822 pamphlet, *On Protection in Agriculture*

The crux of Ricardo's dynamic theory of international trade is the law of diminishing returns to the production of corn as production is expanded

> It appears then, that in the progress of society, when no importation takes place, we are obliged constantly to have recourse to worse soils to feed an augmenting population, and with every step of our progress the price of corn must rise, and with such rise, the rent of the better land which had been previously cultivated, will be increased (Ricardo 1965 212)

Ricardo pointed out that international trade would prevent the price of corn from rising

> it [higher price of corn] would not have existed if the same return had been obtained with less labour, - it would not have existed if, by the application of labour to manufactures, we had indirectly obtained the corn by the exportation of those manufactures in exchange for corn (Ricardo 1965 212)

Ricardo made two arguments against the assertions of those in favour of the corn laws on the grounds that the English farmer paid higher wages than those on the Continent (Ricardo 1965 213) He argued that if a rise in English wages compared to those on the Continent *were* to produce a rise in prices, it would produce a rise in the prices of all goods, including corn (Ricardo 1965 213) Since relative prices would remain unchanged in this case, Ricardo argued that the farmer would be just as well off under trade as without it, *if* the effect of higher wages was to increase prices

> If a quarter of corn be raised from 60s to 75s , or 25 per cent by a rise in wages, and a certain quantity of hats or cloth be raised in the same proportion by the same cause, the importer of corn into England would lose just as much by the commodity which he exports, as he would gain by the corn which he imports (Ricardo 1965 214-215)

Of course, the fundamental premise of Ricardo's law of profits is that an increase in wages will not increase price, but rather will reduce profit, and cause a decline in the accumulation of capital Ricardo's conclusions as to the validity of the arguments put forward in favour of the corn laws were the following

> No one class of producers, then, is entitled to protection on account of a rise of wages, because a rise of wages equally affects all producers, it does not raise the price of commodities because it diminishes profits, and, if it did raise the price of commodities, it would raise them all in the same proportion, and would not therefore alter their exchangeable value (Ricardo 1965 215)

Ricardo argued that wages were being kept high in England, because the price of corn was high due to diminishing returns to its production, corn being one of the "necessaries of the labourer" (Ricardo 1965 237) High wages would result, according to Ricardo's law of profits, in low profits

> In this view of the law of profits, it will at once be seen how important it is that so essential a necessary as corn, which so powerfully affects wages, should be at a low price, and how injurious it must be to the community generally, that, by prohibitions against importation, we should be driven to

the cultivation of our poorer lands to feed our augmenting population (Ricardo 1965 237)

For Ricardo, protection had resulted in consumer losses, "diminishing the sum of our enjoyments" (Ricardo 1965 237) There was also the possibility of a flight of capital "we offer an irresistible temptation to capitalists to quit this country, that they may take their capitals to places where wages are low and profits high" (Ricardo 1965 237) On the other hand, there was the argument of those in favour of the corn laws, that the free importation of corn would ruin English farmers Ricardo did not accept that the importation of corn would result in complete specialisation

> From all the evidence given to the Agricultural Committee, it appears that no very great quantity could be obtained from abroad, without causing a considerable increase in the remunerating price of corn in foreign countries To raise a larger supply, too, those countries would be obliged to have recourse to an inferior quality of land, and as it is the cost of raising corn on the worst soils in cultivation requiring the heaviest charges, which regulates the price of all the corn of a country, there could not be a great additional quantity produced without a rise in the price necessary to remunerate the foreign grower (Ricardo 1965 265)

Ricardo's dynamic theory of international trade differs from the model of trade he presented in chapter 7 of *Principles* In his arguments on protection in agriculture, Ricardo made use of the earlier concept of absolute advantage, rather than his own principle of comparative advantage The static Ricardian model of trade in its modern formulation incorporates an assumption of constant returns to scale, whereas Ricardo's dynamic theory deals with the impact of diminishing returns to scale in the production of corn Mumy has criticised Ricardo for failing to establish a link between his dynamic and his static theory of international trade

> But what about a country that trades with England because it has a comparative advantage in agricultural production? Wouldn't the argument applied to England work in reverse because the agricultural margin is extended, thus lowering the profit rate and increasing landlord incomes? (Mumy 1991 92)

But this effect of diminishing returns to agricultural production was *not* something which Ricardo failed to identify (Ricardo 1965 265, see quote above) Incomplete specialisation was the outcome in Ricardo's dynamic theory of international trade in goods subject to diminishing returns This, coupled with the footnote to chapter 7 (Ricardo 1965 136), casts doubt on the modern assumption that the most accurate interpretation of Ricardo's model of international trade is one based on a straight-line production function It would have been impossible for Ricardo to show that there would be mutual gains from trade in a two country/two good model, if one of the goods was produced under diminishing returns and if complete specialisation was to be the outcome (Steedman and Metcalfe 1979 99-100) *If* Ricardo had insisted on the preservation of a prediction that complete specialisation would be the outcome of international trade in all cases, then Mumy would be correct to argue that "the generality of Ricardo's claim that foreign trade is highly beneficial to a country is seriously undermined" (Mumy 1991 88-89) However, it is not at all clear that complete specialisation was an outcome of international trade that Ricardo held to be valid in the face of diminishing returns to agricultural goods

In conclusion, it seems that the standard, one factor model of trade attributed to Ricardo, is not an adequate representation of Ricardo's discussion of international trade "The Ricardo of pure trade theory is a pale shadow of the real one" (Findlay 1984 186) Meneschi has argued that a multi-factor model would be a more accurate representation (Maneschi 1992 428) In addition, it is argued above that Ricardo did not assume complete specialisation in the case of diminishing returns to factors in the production of a traded good If this interpretation of Ricardo's arguments in chapter 7, and in the pamphlet, is accepted, then the straight line production function is a misrepresentation of Ricardo's argument

Why is the 20th century, static Ricardian model of international trade at variance with Ricardo's theory of international trade? Findlay suggested conventionalist reasons for this divergence "The very neatness and elegant simplicity of the chapter 7 analysis seems to have diverted attention from the more complex, but also in my opinion very rich and deep ideas contained in the *Essay*" (Findlay 1984 186) (The *Essay* to which Findlay refers, is Ricardo's *An Essay in the Influence of a Low Price of Corn on the Profits of Stock*, 1815) Blaug made the following comment in his discussion of neoclassical interpretations of Ricardo's work "we have travelled a long way from what Ricardo actually said to what Ricardo must have meant if he cared as much as modern economists do about the internal consistency of economic models" (Blaug 1985c 9) This comment is particularly appropriate to the way in which the history of international trade theory is written in international trade textbooks This history is written as though Ricardo had a fully developed notion of neoclassical objectives In fact, while there may be hints of the neoclassical objectives which were later to be found in Ricardo's chapter 7, these hints were not woven together in any cohesive fashion

The richness of Ricardo's theory of international trade can be compared to the narrowness of the neoclassical interpretation of his theory, using Lakatosian analysis From a Lakatosian perspective, there is little novelty m the neoclassical, Ricardian model The principle of comparative advantage had already been stated by Torrens in 1815 The static model predicts that complete specialisation would be the outcome of international trade, and that there would be mutual gains from international trade where specialisation based on comparative advantage occurred Ricardo himself showed that incomplete specialisation was the outcome of international trade where goods are subject to diminishing returns (Ricardo 1965 265), thus showing complete specialisation to be a special case That the principle of comparative advantage is a major source of progress in the development of international trade theory, has been questioned on occasion "the sole addition of consequence which the doctrine of comparative cost made [was the fact that] imports could be profitable even though the commodity imported could be produced at less cost at home than abroad" (Viner 1955 441)

Ricardo's dynamic theory, on the other hand, put forward a number of novel predictions At the core of Ricardo's dynamic theory of international trade were the law of diminishing returns to agricultural production and Ricardo's law of profits From these propositions, Ricardo made the following predictions in respect of the effect of international free trade Without the free importation of corn from France, the level of money wages in England would remain high, and profits low This would induce capital outflows Free trade in corn would reduce the money wage, since corn was the principle commodity purchased by the labouring classes This free trade would not result in the complete specialisation of English agriculture out of corn, due to the diminishing returns to agricultural production Blaug has shown that several of these predictions were in fact falsified by empirical evidence throughout the first half of the 19th century (Blaug 1986 94) Ricardo's dynamic theory failed to explain how economic growth persisted throughout the 1830s and 1840s despite the continuation of the corn laws (Blaug 1986 94) The corn laws failed to produce a rise in the price of corn (Blaug 1986 105) However, Blaug conceded that in the years following the repeal of the corn laws, several of Ricardo's predictions were empirically corroborated

> A larger quantity of grain was imported in the decade after 1846 than in all the thirty-one years between Waterloo and repeal, yet there was no ruinous drop in wheat prices or in acreage under cultivation In fact, the period between repeal and the 1870s was the golden age of British farming (Blaug 1986 105)

Ricardo's dynamic theory contains more novel predictions than the 20th century neoclassical representation of Ricardo's theory of comparative advantage Some of these novel predictions of the dynamic theory were, in addition, empirically corroborated Thus, in Lakatosian terms, it is Ricardo's dynamic theory which is the more progressive of the two, and should have been the one to survive into the 20th century However, Ricardo's dynamic theory was not compatible with neoclassical concerns about exchange It was not so easily adapted to suit the neoclassical mode of analysis Ricardo's principle of comparative advantage survived, not because it was novel, but because it could be made heuristically compatible with the neoclassical mode of analysis This neoclassical modification of Ricardo's theory of comparative advantage into a two country/two good/one factor model of international trade significantly narrows the potential scope of Ricardo's original theory It suggests that there was a continuity in the development of international trade theory, which in fact did not exist except in the most implicit and disjointed of forms

The neoclassical rational reconstruction gives an erroneous portrayal of continuity in international trade theory Mirowski criticised the notion of rationally reconstructing history for justifying the persistence of such erroneous histories

> The Lakatosian method of 'rational reconstruction' is in fact a thinly disguised blueprint for the justification of the status quo in any intellectual discipline, because it freely advises the historian to ignore any contradictory evidence which might call into question a presumption of pure and unhindered progress in a science (Mirowski 1987 296)

Clearly, in international trade textbooks, the criterion of progress is the ability to explain the development of international trade theory in terms of neoclassical methodology, regardless of any empirical considerations Thus, there developed in the early 20th century a static theory of international trade which purported to have its roots in Ricardo's theory of international trade This static theory was a neoclassical alternative to the dynamic theory of international trade which was the main mode of analysing international trade issues until the 1930s The roots of this dynamic theory of international trade can be traced back to Adam Smith, whose theory is the subject of the following section

5.2 THE ORIGINS OF THE DYNAMIC THEORY OF INTERNATIONAL TRADE

A neoclassical interpretation of Adam Smith's theory of international trade represents the principle of absolute advantage in a two country/two good/one factor model. The level of technical advancement differs across the two countries, and this determines the relative cost of production in labour units of each good in each country. In equilibrium, there is complete specialisation. The principle of absolute advantage is an almost intuitive argument in support of free trade.

> Whether the advantages which one country has over another, be natural or acquired, is in this respect of no consequence. As long as the one country has those advantages, and the other wants them, it will always be more advantageous for the latter, rather than to buy of the former than to make. (Smith 1976, Vol 1 480)

This is not as bold a prediction as that derived from the 'Ricardian,' neoclassical model of international trade. With the principle of comparative advantage, Ricardo produced the unlikely prediction that there can still be mutual gains from trade, even where one country has an absolute cost advantage in respect of both goods in the

2x2x1 model In Popperian terms, comparative advantage is the bolder, riskier, and therefore the preferable prediction

As was noted above in respect of Ricardo, this interpretation of Smith's theory of international trade, common to most textbooks, is a 20th century construct The superiority of the principle of comparative advantage means that only a few paragraphs are devoted to Smith (for example, Sodersten 1980), or else Smith's theory of trade is omitted altogether (for example, Chipman 1965) Smith, it seems, is chastised for failing to discover comparative advantage (Bloomfield 1975, Myint 1983) This neoclassical interpretation fails to internalise all of Smith's theory of international trade It cannot explain the development of Smith's dynamic theory of trade, just as it cannot explain or rationalise *Ricardo's* dynamic theory of trade West argued that "after Smith, foreign trade *and* domestic economy were subsumed as branches of the static general equilibrium analysis" (West 1990 27) In respect of international trade theory, this is not accurate Smith's dynamic theory of international trade persisted as the primary mode of analysis during the 19th century, and arguably, into the 20th century with Ohlin (1933) An alternative rational reconstruction of the impact of Smith's dynamic theory of international trade is therefore necessary

Hollander conceded that there was a dynamic theory of international trade to be found in *Wealth of Nations*, but focused on the extent to which Smith's theory can be made compatible with its 20th century, neoclassical interpretation (Hollander

1973 275-283) The essence of Hollander's rational reconstruction of Smith is to show that Smith was in fact a general equilibrium theorist In respect of international trade theory, Hollander pointed to the similarities between Smith's theory and the 20th century factor proportions explanation for patterns of international trade

> Smith took it for granted that in a new country the peculiar advantage would lie in the production of farm produce, because of the large supply of cheap land available And that the advantage of Europe lay in manufactured produce, because of the relative cheapness of labour and high cost of land (Hollander 1973 283)

Myint, though he disputed Hollander's interpretation of Smith's theory of trade on several counts, agreed that "Smith was able to conduct his trade analysis on the basis of all three factors - land, labour, and capital - and this enabled him to anticipate the modern Heckscher-Ohlin approach to international trade" (Myint 1983 511, see also Bloomfield 1975 459) Of course, if Smith's theory of trade involves the use of more than one factor, then the standard 2x2x1 interpretation is a misrepresentation But was Smith thinking in terms of a multi-factor model of trade where trade patterns are determined solely by factor quantities across countries? O'Donnell pointed out that Smith did not focus solely on quantitative differences of factors across countries, but also on qualitative differences (O'Donnell 1990 194) Moreover, O'Donnell argued that Smith explicitly "*played down*" the quantitative differences in factors across countries (O'Donnell 1990 194), in his introduction to *Wealth of Nations* (Smith 1976 1-2) Here, Smith argued that, whatever the factor endowment of a country, its rate of capital accumulation would depend on "the skill, dexterity, and judgement with which its labour is generally applied" and "upon the number of those who are annually employed in useful labour, and that of those who are not so employed" (Smith 1976 1-2)

Myint pointed out that "Smith identified China's potential advantage in trade as consisting in the export of manufactures" and that "this conclusion accords well enough with the modern factor proportions theory" (Myint 1983 516) This suggests that the reasoning behind Smith's conclusion that China's advantage is in manufactures, was an assumption that China was relatively abundant in capital as opposed to labour However, Smith's 'strategic trade policy recommendations' for China come, not from an assumption about factor proportions, but rather from an argument based on technology transfer

> A more extensive foreign trade could scarce fail to increase very much the manufactures of China, and to improve very much the productive powers of its manufacturing industry By a more extensive navigation, the Chinese would naturally learn the art of using and constructing themselves all the different machines made use of in other countries, as well as the other improvements of art and industry which are practised in all the different parts

> of the world Upon their present plan they have little opportunity of improving themselves by the example of any other nation, except that of the Japanese (Smith 1976, Vol II 202)

As O'Donnell has argued, the outcome might be similar to that under the factor proportions theory, but the "*logic* of neoclassical theory" is missing (O'Donnell 1990 194)

Myint, in his defence of Smith against the charge of failing to have discovered the principle of comparative advantage, argued that Smith provided "a richer and more realistic model of the domestic economy than would have been possible within the restrictive framework of a comparative cost theory" (Myint 1983 525) Rather than being modified so as to fit a static, neoclassical view of international trade, Myint argued that Smith's theory "should be considered as an attempt to study the longer-run mutual interaction between foreign trade and domestic economic development, essentially involving an increase in the total volume of the resources and a rise in their productivity" (Myint 1983 510) The extent to which Smith's theory of international trade spreads over the whole of *Wealth of Nations* is, for Myint, indicative of the importance of international trade in Smith's theory of economic development (Myint 1983 513)

Myint identified two dynamic theories of international trade in *Wealth of Nations*, the 'vent-for-surplus' theory and the 'productivity' theory (Myint 1958,1983) The productivity theory is Smith's argument about the impact of international trade on the level of output and the productivity of labour

> By means of it [foreign trade], the narrowness of the home market does not hinder the division of labour in any particular branch of art or manufacture from being carried to the highest perfection By opening a more extensive market for whatever of the produce of their labour may exceed the home consumption, it encourages them to improve its productive powers, and to augment its annual produce to the utmost, and thereby to increase the real revenue and wealth of the society (Smith 1976 Vol I 469)

This impact of international trade on the productivity of labour amounts to a pushing out of the production possibilities frontier of neoclassical theory (Myint 1958 318) Smith was, therefore, making the point that the domestic reallocation of resources which international trade would prompt, would generate not only static, consumer gains, but also dynamic, productivity gains He conceded that the reallocation of resources prompted by the introduction of foreign trade might negatively affect certain sectors of the domestic economy

> If the free importation of foreign manufactures were permitted, several of the home manufactures would probably suffer, and some of them, perhaps, go to ruin altogether, and a considerable part of the stock and industry at present in them, would be forced to find out some other employment (Smith 1976, Vol II 481)

However, Smith argued that "there are other collateral manufactures of so similar a nature, that a workman can easily transfer his industry from one of them to another" (Smith 1976, Vol II 493) In order to facilitate the swift adjustment of industry to the removal of protection, Smith held that the regulations which impinge on the free movement of labour should be abolished, "so that a poor workman, when thrown out of employment either in one trade or in one place, may seek for it in another trade or in another place, without the fear either of a prosecution or of a removal" (Smith 1976, Vol II 493)

The neoclassical model of absolute advantage would predict that the domestic economy would tend towards specialisation of one good as it realised more economies of scale through the greater division of labour In equilibrium, both countries would be completely specialised in one good There is some evidence that Smith believed underdeveloped countries should specialise completely in agriculture

> It has been the principal cause of the rapid progress of our American colonies towards wealth and greatness, that almost their whole capitals have hitherto been employed in agriculture Were the Americans, either by combination of by any other sort of violence, to stop the importation of European manufactures, and, by thus giving a monopoly to such of their own countrymen as could manufacture the like goods, divert any considerable part of their capital into this employment, they would retard instead of accelerating the further increase in the value of their annual produce, and would destruct instead of promoting the progress of their country towards real wealth and greatness (Smith 1976, Vol I 388)

An underdeveloped country would, according to Smith, specialise in agricultural production because the returns to agriculture in terms of the labour employed is greatest "After agriculture, the capital employed in manufactures puts into motion the greatest quantity of productive labour, and adds the greatest value to the annual produce" (Smith 1976, Vol I 387, this point is made in Myint 1983 520) Smith did concede that there would be diminishing returns to agriculture, but argued that the extent of surplus generated would create enough wealth to counteract these diminishing returns (Myint 1983 516)

In respect of the developed countries of Europe, however, the implication is that they would continue to produce and to expand their agricultural production, while exporting manufactures

> The most opulent nations, indeed, generally excel all their neighbours in agriculture as well as in manufactures, but they are commonly more distinguished by their superiority in the latter than in the former (Smith 1976, Vol I 10)

This failure to specialise on the part of developed countries suggests that they were not fully utilising all their resources prior to the opening up of international trade That international trade allows for the expansion of production so as to utilise previously unused factors is the essence of Smith's vent-for-surplus theory of international trade The assumption that a country's resources are not fully utilised prior to international trade introduces a methodological conflict between Smith's theory of international trade and the Ricardian, neoclassical model In respect of Smith's vent-for surplus theory

> Introducing foreign trade will not, therefore, require any transfer of resources away from domestic production there is a *net gain* In sharp contrast in this regard is Ricardian trade theory according to which resources are initially m

full employment so that the introduction of trade involves a *reallocation* of activity (Hollander 1973 269)

This is a contradiction which Ricardo noted (Hollander 1973 274) If unemployed resources existed, Ricardo asked

"Could not this portion of the productive labour of Great Britain be employed in preparing some other sort of goods, with which something more in demand at home might be purchased? And if it could not, might we not employ this productive labour, though with less advantage, in making those goods in demand at home, or at least some substitute for them? (Ricardo 1965 294-295)

This point was reiterated by Mill, in his criticism of Smith's vent-for surplus theory (Mill 1892 393)

The implication of the vent-for surplus theory that resources are not fully utilised prior to trade, also conflicts with the productivity theory which suggests that it is the reallocation of fully utilised resources facilitated by international trade which generates further division of labour m those for which the country has an absolute advantage Myint attempted to reconcile these two theories of international trade (Myint 1983) He conceded that the formal model of absolute advantage had to assume the constant full employment of factors, "otherwise there would be no point m insisting on the efficient allocation of the available resources" (Myint 1983 521) Myint argued that Smith, on the other hand, "required a conceptually 'open-ended' model of the domestic economic system in which the *incomplete development* of the internal economic organization would leave room for its long-run productive potentialities to be brought out more fully by the forces introduced by foreign trade" (Mymt 1983 522)

Smith considered the argument that "landed nations" of Europe - England and France - would gam from restricting their imports from the "mercantile states" - Holland and Hamburg (Smith 1976, Vol I 190) Smith argued that free trade would lead to the "improvement and cultivation" of the landed nations, and would encourage the production of a surplus in agriculture (Smith 1976, vol I 191) Accordmg to the neoclassical theory of absolute advantage, the landed nations would develop an absolute advantage in agriculture and specialise completely As Mymt pointed out, "this is not how Smith would see the situation" (Mymt 1983 524) For Smith, the generation of a surplus in agricultural output would have the following beneficial effects

> The continual increase of the surplus produce of their land, would, in due time, create a greater capital than what could be employed with the ordinary rate of profit in the improvement and cultivation of land, and the surplus part

of it would naturally turn itself to the employment of artificers and manufacturers at home (Smith 1976, vol I 191)

This 'natural' development of the industry of the landed nations would, Smith argued, eventually lead to an absolute advantage in manufactures "The artificers and manufacturers of such mercantile states, therefore, would immediately be rivalled in the market of those landed nations and soon after undersold and justled out of it altogether" (Smith 1976, vol I 191) But would not protection lead to the same development of manufactures m the landed nations? Smith did not agree

By raising up too hastily a species of industry which only replaces the stock which employs it, together with the ordinary profit, it would depress a species of industry which, over and above replacing that stock with its profit affords likewise a neat produce, a free rent to the landlord It would depress productive labour, by encouraging too hastily that labour which is altogether barren and unproductive (Smith 1976, vol I 193)

Myint focused on the Smithian distinction between productive and unproductive labour in order to attempt to clarify what Smith meant by full employment

But Smith's notion of 'full employment' of labour would allow for the possibility of increasing output, even in the short run, by recruiting the extra

> labour for productive uses from the existing pool of 'unproductive' labour He then assumed that there was a considerable amount of unused or underutilized land to produce the extra agricultural output not only in the colonies but also in the developed 'landed nations' of western Europe (with the exception of the highly advanced 'mercantile states' such as Holland or Hamburg) (Myint 1983 525)

Thus, Myint reconciled the productivity and the vent-for-surplus theory by arguing that neither involved an assumption of the full utilisation of resources in the common meaning of the term In essence, international trade gives rise to increased division of labour, which in turn increases the level of domestic resources, this increase in domestic resources generates surplus production which is in turn disposed of through international trade It has been argued that in neoclassical terms, the productivity theory is concerned with the shifting outward of a country's production possibilities frontier (Myint 1958 318), while the vent-for-surplus theory suggests that a country is operating below its production possibilities frontier (Evans 1989 2) Myint's interpretation of Smith's dynamic theory of international trade suggests a production possibilites frontier which can be continually shifted outwards, if a country engages in international trade Only at a very advanced stage of development, would a country's actual *and potential* resources be fully utilised, resulting in complete specialisation

As with Ricardo, Smith's theory of international trade is closely tied to his theory of economic growth Also as with Ricardo, Smith's theory of international trade differs markedly from the neoclassical reconstruction of it Smith did not assume one factor of production Nor did he argue that complete specialisation would be the outcome of free trade There is no equilibrium in Smith's theory of trade - factors of production are endlessly accumulated A neoclassical rational reconstruction omits most of Smith's theory of international trade It creates a historical picture of continuity in international trade theory which does not exist As with Ricardo's principle of comparative advantage, Smith's principle of absolute advantage is retained because it can be made compatible with the heuristics governing the static neoclassical theory of international trade Yet, Smith's dynamic theory was not discarded as soon as the principle of comparative advantage was elucidated Indeed, it remained the primary mode of analysing patterns of international trade until the 1930s (O'Brien 1975 54) Where then are the novel predictions of Smith's dynamic theory of international trade which would justify its persistence?

A lot of Smith's theory of international trade is inductive in nature, deriving its predictions from empirical observation (Smith did not, however, use empirical observations to verify his predictions) An example would be Smith's prediction that international trade would not result in complete specialisation in the landed nations This is deduced, not from a set of basic premisses, but rather from observations as to the extent of agricultural production in landed nations This is quite unlike the deductive method of the neoclassical representation of Smith's theory of absolute advantage Given the inductive nature of several of the predictions of Smith's theory, it is impossible to apply the term 'novel prediction' in the sense in which it was meant by Lakatos In the Lakatosian framework, as in the Popperian, novelty can only be used in the context of predictions derived from deductive systems

Where then are the (Lakatosian) novel predictions of Smith's theory of international trade? Implicit in Smith is the argument that there are mutual gains from trading according to the principle of absolute advantage In order, however, to establish this novel prediction, it would have been necessary to show that complete specialisation is the outcome of international trade While in his vent-for-surplus theory, Smith did not argue that complete specialisation is a result of trade according to international absolute advantage, in his discussion of the productivity gains from trade Smith did suggest that a reallocation of resources might take place Moreover, he argued that such a reallocation would be more easily facilitated by a removal of the restrictions on the domestic movement of labour Thus, the prediction that there will be mutual gains from international trade can be said to be a novel prediction of Smith's theory However, the observation Smith made that complete specialisation was *not* the outcome of international trade did not shake Smith's theory For Smith, the reallocation is incomplete, because only an incomplete specialisation is necessary to generate division of labour to the extent that a surplus is created and capital accumulated Thus, under international trade, landed nations remain specialised in both agriculture and manufactures, while underdeveloped countries will develop a manufacturing industry out of the surplus resources they generate from their exports of agricultural produce

The notion that international trade will result in both an increase in the resources of a country, and in the fuller utilisation of previously unused resources of that country, are the two main novel predictions from Smith's dynamic theory of international trade In arguing that Smith's dynamic theory of international trade persisted, it should be pointed out that it was the productivity aspect of this theory which persisted Trade theorists of the 19th century and early 20th century focused on the extent to which international trade, through specialisation, would encourage the development of increasing returns to scale in manufacturing This, and the assumption of diminishing returns to agricultural production, form the main propositions of the classical theory of international trade This classical theory would therefore predict an imbalance in economic growth between developed and underdeveloped countries This would not have been a result of Smith's dynamic theory alone, given that in Smith's theory diminishing returns to agriculture do not outweigh the positive effects of specialisation in agriculture

A neoclassical rational reconstruction ignores the dynamic theorising of these early trade theorists, and focuses instead on the further development of a static theory of international trade The next significant development in the static theory of international trade was in 1848, when Mill proposed his principle of reciprocal demand Between 1869 and 1873, Marshall put forward a geometric representation of Mill's principle, and introduced the method of comparative statics to international trade theory These developments are the subject of the following section

5 3 THE DEVELOPMENT OF THE STATIC THEORY OF INTERNATIONAL TRADE - MILL AND MARSHALL

Ricardo's principle of comparative advantage provided only one half of a static theory of international trade What was missing from Ricardo's discussion was some way of determining the international terms of trade This missing half was supplied by J S Mill in chapters 17 and 18 of his *Principles of Political Economy*, published in 1848

Mill was much more explicit than either Ricardo or Smith on the method he believed appropriate to political economy Mill's contribution to the development of international trade theory should therefore be considered in the light of his methodological discussions

Mill argued that political economy should adopt the deductive method of the natural sciences "the method *a priori* in Political Economy, and in all the other branches of moral science, is the only certain or scientific mode of investigation" (Mill 1874 331) Mill was a positivist, though he saw the role for induction "not as a means of discovering truth, but of verifying it" (Mill 1874 331) Like Ricardo, Mill derived abstract models based on the notion that wealth maximisation is the sole motive behind individuals' behaviour It was in this domain of *a priori*, abstract science that Mill held the laws of political economy to be exact (de Marchi 1986 91) However, in the application of these laws to the real world, "disturbing causes" to the laws are revealed (Mill 1874 330) Mill stressed the importance of empirical testing in order to elucidate disturbing causes to the laws of political economy "the discrepancy between our anticipations and the actual fact is often the only circumstance which would have drawn our attention to some disturbing cause which we have overlooked" (Mill 1874 330) The laws of political economy can be made more exact *in their application* if the disturbing causes, too, are subject to definable laws In this case, Mill argued, "the nature and amount of the disturbance may be predicted *a priori*, like the operation of the more general laws which they are said to modify or disturb" (Mill 1874 330)

Only the disturbing causes which "operate upon human conduct through the same principle of human nature with which Political Economy is conversant, namely, the desire of wealth" can "be brought within the pale of the abstract science if it were thought worth while" (Mill 1874 330-331) Where the disturbing cause is due to "some other law of human nature it can never fall within the province of Political Economy, it belongs to some other science" (Mill 1874 331) Because there will always be disturbing causes which are non-economic in nature, the predictions of applied economics will never be wholly accurate

> mankınd can never predıct wıth absolute certaınty, but only wıth a less or greater degree of probabılıty, accordıng as they are better or worse apprısed what the causes are, - have learnt wıth more or less accuracy from experıence the law to whıch each of those caues, when actıng separately conforms, - and have summed up the aggregate effect more or less carefully (Mıll 1874 336)

Lıke Popper, Mıll argued that a crıtıcal attıtude was necessary

> All we can do more, ıs to endeavour to be ımpartıal crıtıcs of our own theorıes, and to free ourselves, as far as we are able, from that reluctance from whıch few ınquırers are altogether exempt, to admıt the realıty or relevancy of any facts whıch they have not prevıously eıther taken ınto, or left a place open for ın, theır systems (Mıll 1874 336)

But Mıll, ın contrast to Popper, argued that the method of ınductıon *ıs* vıable as a method of analysıng the truth-status of hypotheses Mıll's methodology also holds that the basıc laws of polıtıcal economy are ıntrospectıvely derıved by the theorısts, and are therefore psychologıcal ın nature Popper made a strong attack on psychologısm as a method for the socıal scıences (Popper 1966 90-99, see also sectıon 1 4)

Mill did not advocate the rejection of economic theories where these theories are falsified Blaug implied that this is an irrationality on Mill's part "Mill cannot bring himself to equate a failure to verify a prediction with a refutation of the underlying theory" (Blaug 1992 59) However, de Marchi argued that "it is not correct to regard the tendencies of Mill's economic *science* as inexact laws Rather, they are encompassing and wholly accurate as far as they go, but their domain is artificial and limited" (de Marchi 1986 92) De Marchi's interpretation suggests that Mill, while conceding that disturbing causes exist for all theories in their application to the real world, would argue that these theories are nonetheless accurate within the context of the axiomatic system within which they were constructed

In Lakatosian terms, Mill laid down the following heuristics for political economy

PH1 use the deductive method to develop the theories of political economy, using introspection to establish the basic laws

PH2 compare the predictions of theories with observed data

PH3 any divergence between these predictions and the observed data should be analysed in order to elucidate the disturbing causes

PH4 theories may have to be modified in order to take account of disturbing causes, if the disturbing causes are economic in nature

The extent to which Mill adhered to his own methodology in respect of international trade theory is now examined

In chapter 17, Mill repeated Ricardo's principle of comparative advantage, with its allied assumption that factors of production are internationally immobile Mill presented international trade as a substitute for factor mobility, where both would result in "a more efficient employment of the productive forces of the world" (Mill 1892 392) In addition, Mill reiterated Smith's dynamic argument that international trade would expand the market for domestic production and allow for economies of scale However, Mill denied Smith's argument that surplus production on the basis of these economies of scale would be generated in the absense of international trade (Mill 1892 394) For Mill, economies of scale would not be realised prior to the generation of excess demand through international trade

In chapter 18, Mill was concerned with the determination of international values He argued that the international price of a traded good would be determined by "the cost of its acquisition", or, "the cost of production of the thing which is exported to pay for it" (Mill 1892 395) Mill was, therefore, discussing a barter, or pure theory of international trade

> The value, then, in any country, of a foreign commodity, depends on the quantity of home produce which must be given to the foreign country in exchange for it In other words, the values of foreign commodities depend on

the terms of international exchange (Mill 1892 395)

Mill argued that international prices were determined, not by the cost of production as was the case with domestic prices, but rather they were determined by "an antecedent law, that of supply and demand" (Mill 1892 396) Since "the supply brought by the one constitutes his demand for what is brought by the other, supply and demand are but another expression for reciprocal demand" (Mill 1892 402)

Mill outlined the process by which the international price ratio is established "when two countries trade together in two commodities, the exchange value of these commodities relatively to each other will adjust itself to the inclinations and circumstances of the consumers on both sides, in such a manner that the quantities required by each country, of the articles which it imports from its neighbour, shall be exactly sufficient to pay for one another" (Mill 1892 398) Exactly where this exchange value settles would depend on the "inclinations and circumstances" of the consumers of each country These inclinations and circumstances are in modern terms denoted by elasticity of demand Mill argued that there would be limits to the extent of variation of the ratio of exchange "the limits within which the variation is confined, are the ratio between their costs of production in the one country, and the ratio between their costs of production in the other they may exchange for any intermediate number" (Mill 1892 398)

Mill argued that the country most likely to benefit from a favourable ratio of exchange is "the country for whose productions there is in other countries the greatest demand, and a demand the most susceptible of increase from additional cheapness" (Mill 1892 400) In other words, the country with the greater elasticity of demand for imports, gains most

> It gets its imports cheaper, the greater the intensity of the demand in foreign countries for its exports It also gets its imports cheaper, the less the extent and intensity of its own demand for them The market is cheapest to those whose demand is small (Mill 1892 401)

With the elucidation of the principle of reciprocal demand, the notion that there would be mutual gams from international trade with incomplete specialisation could now be fully derived from the prnciple of comparative advantage It is not surprising that from a neoclassical point of view, Mill's theory of international trade is considered as "one of the greatest achievements of the human intellect" (Chipman 1965 486) With the elucidation of the equilibrium international price ratio, the static theory of international trade became wholly dependent upon an assumption of constant costs The extent to which this assumption was a necessary precondition of establishing a trading equilibrium is revealed in Marshall's geometric representation of Mill's principle of reciprocal demand

Mill reiterated Smith's dynamic (productivity) theory of international trade, and examined the impact of changes in technology on the international terms of trade He first considered a technological change which creates a new product for export In this case, Mill argued that the country's terms of trade would improve He then considered the effect of a change in technology which reduces the cost of production In this case, Mill argues that the terms of trade would deteriorate Specifically, the terms of trade would fall to a greater degree than the fall in costs if foreign demand for the good is less than one (Mill 1892 402-403)

Mill went much further than other trade theorists of the classical period in developing the static theory of international trade While other classical writers focused on the dynamic aspects of international trade, paying only lip-service to the principle of comparative advantage, Mill's discussion of international trade was almost exclusively in static terms With the elucidation of the principle of reciprocal demand, he developed the following hard core propositions of the static theory of international trade

HC1 trade patterns are determined by the comparative cost of production between countries

HC2 the international price ratio is determined by the interaction of international demand and supply

HC3 there are mutual benefits from free trade between two countries

In conjunction with these hard core propositions, Mill also proposed some auxiliary hypotheses

AH1 the extent to which a country gains from international trade depends on the elasticity of foreign demand for its exports, relative to its own demand for imported goods

AH2 rich countries gain least from trade, because their elasticity of demand for imports is greater

Mill, like Smith and Ricardo before him, argued that free trade would bring mutual benefits to trading partners He showed this, not in an inductive way as Smith had done, but rather as a prediction deduced from his basic premises Mill did, however, concede that while mutual gains from trade were the outcome of his abstract model, in terms of applied political economy a policy of protection might be justified in a particular circumstance "The only case in which, on mere principles of political economy, protecting duties can be defensible, is when they are imposed temporarily (especially in a young and rising nation) in hopes of naturalising a foreign industry, in itself perfectly suitable to the circumstances of the country" (Mill 1892 593) This admission was, for Mill, a disturbing cause to his abstract model of international trade, but it did not cause him to abandon his abstract model

Blaug has argued that Mill's methodology resulted in nothing more than the addition of *ad hoc* adjustments in the face of refutations to elements in the Ricardian theory of economic growth (Blaug 1992 65) According to Blaug, Mill 'saved' Ricardian theory "by adopting various 'immunizing stratagems,' of which the chief one was to empty the appropriate *ceteris paribus* clauses of whatever specific content they may once have had" (Blaug 1992 65) Blaug listed the anomalies to the Ricardian theory of economic growth which were well-known at the time at which Mill was writing *Principles* (Blaug 1986) The decline in the birth rate from the 1820s onward, falsified Malthus' hypothesis that higher money wages would result in a higher rate of population growth (Blaug 1986 95) It was also apparent at the time, that in spite of the persistence of the corn laws, the price of corn was declining (Blaug 1986 105) Despite these recorded refutations of the Ricardian theory of economic growth, Mill's *Principles* "retained the Ricardian system without qualifications" (Blaug 1992 65) For Blaug, as a falsificationist, this failure to reject the Ricardian system can only be interpreted as a degenerative move

Blaug argued that Mill, "having defended the Malthusian theory of population as analytically 'correct,' was forced to concede that the census reports did not uphold the theory" (Blaug 1986 99) In terms of Mill's methodology, however, such an acknowledgement would not involve the rejection of the theory The divergence of the abstract laws from empirical observation do not indicate that the abstract laws are false, but rather that they may require modification in the light of hitherto unsuspected economic disturbing causes, or that there are non-economic disturbing causes which prevent the abstract laws explaining fully the observed phenomenon De Marchi conceded that Mill was not always concerned with the elucidation of the disturbing causes "It cannot be said that Mill always attempted to test his theory against the facts Mill was sometimes willing to live with a gap between his deductive theory and the facts" (de Marchi 1970 272) With respect to international trade theory, Mill considered the impact on the two country/two commodity model of removing the initial assumptions "Those who are accustomed to any kind of scientific investigation will probably see, without formal proof, that the introduction of these circumstances cannot alter the theory of the subject" (Mill 1892 399)

Mill argued that transportation costs may change the international price ratio between two countries for two goods, but it will not prevent an international price ratio being established (Mill 1892 400) He also argued that the principle of reciprocal demand would still hold in a multi-*good* model "the exports of each country must exactly pay for the imports, meaning now the aggregate exports and imports the things supplied by England to Germany will be completely paid for, and no more, by those supplied by Germany to England This accordingly will be the ratio in which the produce of English and the produce of German labour will exchange for one another" (Mill 1892 400) Nor, Mill argued, would the extention of the model to a multi-country model have any impact on the principle (Mill 1892 401) Thus, in the case of the conclusions of international trade theory, Mill held there to be *no* disturbing causes One must conclude from this, that Mill believed the static theory of international trade which he developed, to explain fully the empirical facts

Implicit in Mill's static theory and its extensions, is the assumption of a labour theory of value This was how Mill was able to show a mutual gain from international trade Without some assumption of proportionality between price and cost, it becomes impossible to argue as a logical consequence of the model that there are mutual gains from trade, or that the patterns of international trade is determined by differences in the relative cost of production across countries (Steedman and Metcalfe 1979 99)

In a methodology like instrumentalism, which is wholly concerned with prediction, the veracity of initial assumptions like the labour theory of value is irrelevant This is the point argued by Friedman, in the *Methodology of Positive Economics* (Friedman 1984) Mill, however, purported to be concerned, in his *applied* political economy, with explanation It was to this end that he advocated the elucidation of the disturbing causes to the principles of *abstract* political economy Despite this methodological instruction, Mill himself made no attempt to analyse the impact on the static theory of international trade, of removing the assumption of a labour theory of value In the case of international trade, it seems that Mill was indeed "willing to live with a gap between his deductive theory and the facts" (de Marchi 1970 272) Thus, the static theory of international trade as developed by Ricardo and Mill, is unprogressive according, not only to falsificationism, but also to Mill's own methodology

Most of the classical political economists continued to make an exception of international trade theory "Economists who in general would deny that prices are necessarily proportional to labor costs may have fallen back on the labor cost formula when expounding the theory of international trade because of the aid this formula provides in avoiding - or evading - serious logical difficulties in appraising from a welfare point of view the consequences of trade" (Viner 1955 491) Eventually, a real cost alternative to the labour theory of value was established, which was in essence, nothing more than "a strong presumption of rough proportionality between market prices and real costs" (Viner 1955 491) According to Viner, the following methodological rule was adopted "propositions which depend for their validity on the existence of such rough proportionality are not for that reason to be regarded as invalid unless and until evidence is produced tending to show that in the particular situation under examination no such approach to proportionality between prices and real costs exists" (Viner 1955 491) Since classical theorists were not predisposed to searching for instances in which proportionality fails to hold, this methodological rule amounts to little more than a warrant to ignore gaps between deduced theory and the facts

This is not meant to suggest that classical international trade theorists *ignored* empirical facts, but rather to point out that little attempt was made to analyse the relationship between the static theory and the facts For example, Ohlin was later to express his surprise at finding "a chapter in Bastable [1887] dealing with the international movements of capital, without a single word being said to explain how far these movements affect the fundamental arguments of the foregoing chapters," which dealt with the static theory of international trade (Ohlin 1933 589) The static theory continued to be developed in the late nineteenth century, but the empirical facts of international trade were analysed in a looser framework which was closer to Smith's dynamic theory of international trade This juxtaposition of two distinct methodologies without any attempt at integration, is obvious in Marshall's analysis of international trade

In a neoclassical reconstruction, it is Marshall's geometric interpretation of Mill's principle of reciprocal demand which would be given prominence in a discussion of the importance of Marshall in the development of international trade theory Marshall's offer curve analysis of the principle of reciprocal demand was developed between 1869 and 1873, and was published privately in 1879, in a paper, *The Pure Theory of International Trade* Marshall later published a revised version of this paper, as an appendix in his *Money, Credit and Commerce* (1923) Marshall placed most of his geometric and algebraic analyses in the appendices of his major texts, lest these techniques "lead us astray in pursuit of intellectual toys, imaginary problems not conforming to the conditions of real life" (Marshall 1925 84)

Along with most of the latter classical political economists, Marshall replaced the Ricardian labour theory of value with a real cost theory Marshall abandoned Senior's notion of capital as abstinence from consumption with the notion of 'waiting' or delayed consumption (Roll 1962 397) His real cost theory was therefore just as subjective as that adopted by Senior (Roll 1962 402) The Marshallian synthesis preserved the Ricardian notion of a tendency for the price of factors to equal their marginal productivity in the long run "interest would tend to be identical with the marginal sacrifice involved in saving, wages with the marginal disutility of effort" (Roll 1962 401) This, Marshall coupled with a neoclassical analysis of demand

The debt which Marshall owed to Mill, in his analysis of demand and supply, is exemplified by Marshall's geometrical development of the principle of reciprocal demand In Marshall's model, the terms of trade are determined by the rates at which the units of productive power, or "bales," of one country exchange for those of the other (Marshall 1923 330) Taking two countries, England and Germany, Marshall outlined a table of the number of bales England would sacrifice in order to obtain a specific number of "*G* bales," and vice versa (Marshall 1923 330) The rate of increase of England's offer of its bales in respect of a constant increase of German bales offered, initially increases and then falls The same is true in respect of Germany's offer of bales to England

Marshall transformed this table into a graphic representation of the two countries' offer of bales, namely, the offer curves of each (Marshall 1923 331) *G* bales are represented on the y-axis, and *E* bales on the x-axis The decline in the rate of increase of bales offered for exchange produces an intersection of the two offer curves This is the geometric representation of Mill's equilibrium international price ratio

Marshall measured the gain from trade in terms of the surplus amount of bales embodied in the exports a country would have been willing to exchange, for the amount that country actually imports at its equilibrium terms of trade This is an application of Marshall's notion of consumer surplus to trading countries "the surplus is the greater, the more urgent is *G*'s demand for a small amount of *E*'s goods and the more of them she can receive without any great movement of the rate of interchange in her favour" (Marshall 1923 339-340) The gains from trade are measured, therefore, by the relative slopes of the offer curves of the trading partners In fact, Marshall overestimated the extent of the gain, since he assumed the trading partner's offer curve to remain unchanged whether the country exchanges exports determined by the equilibrium terms of trade, or exchanges the maximum amount of exports it is willing to exchange (Viner 1955 541-546)

The slope of each offer curve is a reflection of the elasticity of demand of one country for the products of the other "if *E* has some important exports which are nearly indispensible to *G*, while *G* has none which are nearly dispensible to *E*, then *OG* will be nearly vertical in the neighbourhood of *O* [the origin], but *OE* will not be nearly horizontal in the neighbourhood of *O*" (Marshall 1923 332) Marshall held that under the "ordinary (or 'normal') conditions of international trade neither country is in urgent need of the greater part of her imports from the other, and the demand of each is very elastic in the neighbourhood of the equilibrium point" (Marshall 1923 332) In normal cases, each point on one country's offer curve (each offer of bales) will correspond to a single point on the trading partner's offer curve, in spite of any difference in the slopes of the two offer curves (Marshall 1923 332) Abnormal cases, where there are two possible corresponding points on the trading partner's offer curve, occur where the trading goods are subject to "exceptional demand" or to "exceptional supply" (Marshall 1923 332) Exceptional demand, Marshall defined as the case where "the markets of a country for foreign wares may be so inelastic as to be completely glutted by moderate supplies, in so much that any further increase of the supplies, forced on the market, will compel them to be sold for a diminished aggregate return" (Marshall 1923 333) The case of exceptional supply is that where the produce of one of the trading partners is subject to increasing returns to scale

Marshall's attempt at incorporating the case of 'exceptional supply' into his static, geometric representation, was essentially an attempt to integrate his static and dynamic discussions of international trade Marshall's dynamic theory of international trade did away with Smith's notion of a vent-for-surplus Marshall argued instead that absolute advantage would depend on the size of the home market

> No country has ever attained leadership in manufacture for export, without previously developing manufacture on a rather large scale for domestic consumption but the export trade affords exceptional opportunities for dealing on a large scale, and this, in turn, tends to promote manufacture on a large scale (Marshall 1923 351)

In Marshall's theory, therefore, a country will have realised increasing returns to a particular good *prior* to the introduction of international trade This will give an indicator of that country's absolute advantage, before international trade is established This differs from Smith's dynamic theory, where international trade generates the division of labour, which in turn creates an absolute advantage

In Marshall's theory, the realisation of increasing returns to scale would increase the "content of the bales" of the country (Marshall 1923 354) This would have no impact on the offer curve of the country, but rather would affect the slope of the offer curve of the tradıng partner (Marshall 1923 354) The result of the realısatıon of ıncreasıng returns by one country would be that "the other country may be wıllıng to take an ıncreased number of them [bales] at a rate of ınterchange whıch ıs nomınally (though not really) less favourable to her" (Marshall 1923 354) As the extent of ıncreasıng returns realısed by one country ıncreases, the offer curve of the tradıng partner wıll change slope There may be a *decrease* ın the number of bales whıch the tradıng partner ıs wıllıng to trade, for any gıven number of bales offered Thıs decrease causes the offer curve to become negatıvely sloped, and ın turn, generates more than one poınt of ıntersectıon wıth the offer curve of the fırst country Marshall's ınabılıty to show whıch of these ıntersectıons would be a stable equılıbrıum led hım to conclude that "*[d]ıagrams representıng the case of Exceptıonal Supply, ın whıch the exports of a country show strong general tendencıes to Increasıng Return, are deprıved of practıcal ınterest by the ınapplıcabılıty of the Statıcal method to such tendencıes*" (Marshall 1923 354)

Marshall's attempt to ıntegrate some dynamıc aspects ınto the statıc theory of trade, although ıt faıled, marks a departure from other classıcal polıtıcal economısts who sımply ıgnored the gaps between the two theorıes Marshall's analysıs of the case of exceptıonal supply could be ınterpreted ın terms of Mıll's methodology as as an attempt to modıfy the statıc, abstract theory of ınternatıonal trade, ın the face of an ımportant dısturbıng cause

Although unable to handle increasing returns within the confines of the two country/two commodity model, this did not prevent Marshall from dealmg with increasing returns within the dynamic theory of international trade Marshall recognised the effect of specialisation in the export of agricultural produce on economic growth - "the more America exported her raw produce in return for manufactures, the less the benefit she got from the Law of Increasing Return (ie that manufacture on a large scale is more economical than on a small)" (Marshall 1925 261) This is the first hint of a recognition that, where there are decreasing returns to scale, it may, contrary to the conclusion implicit in the static theory, matter where a country's comparative advantage lies "It was to England's sagacity and good fortune in seizing hold of those industries in which the Law of Increasing Return applies most strongly, that she owed in a great measure her leading position in commerce and industry" (Marshall 1925 266) Marshall did not, however, attempt to reconcile this conclusion to the static theory

Rather than use this argument in support of protection, Marshall used it to show that free trade was in fact still preferable to protection in the case of America He argued that if America had not used protectiomst policies to build up a specialism in agriculture, its absolute advantage in certain artisan skills could have been developed, allowing America to benefit from increasing returns to scale (Marshall 1925 261) Marshall showed that the existence of increasing returns in manufacture reinforced

List's arguments in favour of a temporary policy of protection in certain circumstances (Marshall 1925 258) However, he argued that protection should not be relied upon indefinitely He pointed to the mismanagement and corruption which, for him, are inevitable consequences of a protectionist regime Marshall's conclusion that free trade is not universally beneficial but is better than corrupt protectionism, is similar to Krugman's (1987) argument that free trade, while it cannot be universally defended as optimal, is a second-best policy in the light of such outcomes as retaliation

Marshall's method was strongly influenced by Mill It was Marshall who introduced the *ceteris paribus* clause as a method of abstracting from disturbing causes (Marshall 1920 366) He was aware of the implications of such abstraction "The more the issue is thus narrowed, the more exactly can it be handled but also the less closely does it correspond to real life" (Marshall 1920 366) Marshall stressed the importance of making exact statements in the abstract, but at the same time, loosening these arguments in order that they reflect reality "With each step exact discussions can be made less abstract, realistic discussions can be made less exact than was possible at an earlier stage" (Marshall 1920 366) The importance which Marshall placed upon this loosening of the abstract models of political economy is reflected in his tendency to place all mathematical formulations into appendices

In respect of the static theory of international trade, Marshall was more involved in the development of techniques of analysis rather than in the generation of novel facts

The facts derived from his offer curve analysis - a country's terms of trade varies directly with its elasticity of demand for imports, the more elastic the demand for imports, the greater the volume of international trade, tariff retaliation eliminates the gains from trade - are all to be found in Mill, although in a less technical form While Marshall was engaged in normal science in the static theory of international trade, he did produce novel facts in respect of the dynamic theory of international trade, principally, in his implication that where there are increasing returns to scale it does matter in which goods a country's absolute advantage lies This argument, in turn, provided a justification for temporary protection, provided this protection serves to build up the industry in which a country has an absolute advantage

Marshall's failure to incorporate increasing returns into his static model of international trade brought a warning on the limitations of the static method of analysis This is not a limitation of which other classical political economists remained unaware "This assertion [comparative cost] is only true if all retarding elements - all those hindrances which arise from cost of carriage and customs duties - are neglected, and then only if the inquiry is confined to two countries" (Bastable 1897 16) Bastable argued that, while the principle of comparative advantage proposed some unexpected predictions, these are in fact "exceptional cases"(Bastable 1897 19) The more usual basis for international trade, Bastable argued as the case where a country "is able to procure commodities which it is absolutely unable to produce itself - tropical spices furnish a good example" (Bastable 1897 19)

Classical political economists recognised the predictive strength of the static theory of international trade, but they were also aware of the disturbing causes to the theory Few held Mill's view that the propositons of the static theory remain unaffected by the removal of its limiting assumptions They elucidated the disturbing causes to the static theory, and then proceeded to analyse international trade and to address the policy debate over free trade versus protection in terms of absolute advantage, as Ricardo had done (A good example of the failure to make use of the static theory of international trade in respect of policy issues is Cairnes' essay, *Fragments on Ireland*, 1873)

The limitations to the static theory allowed for the persistence of two theories of international trade, the static theory developed by Ricardo and Mill, and the dynamic theory begun by Smith These two theories continued to be developed in tandem, until the 1930s when the static theory began to dominate analysis of international trade The next chapter considers the development of the static theory of international trade, focusing specifically on the attempts to incorporate into this static theory, the phenomena of increasing returns and product differentiation

CHAPTER SIX

THE TREATMENT OF IMPERFECT COMPETITION IN THE INTERNATIONAL TRADE SUB-DISCIPLINE

6 1 THE RELATIONSHIP BETWEEN THE INTERNATIONAL TRADE SUB-DISCIPLINE AND THE GREATER NEOCLASSICAL RESEARCH PROGRAMME

By the turn of the century, two dominant theories of trade existed - the dynamic Smithian theory of absolute advantage, and the static Ricardian theory of comparative advantage The Smithian theory was an attempt to analyse the role of international trade in the context of economic growth and development The Ricardian theory focused on the effects of international trade on economic welfare Within the static theory, there was a period of heuristic refinement which began with Marshall's offer curves, and was continued by Edgeworth, Lerner and Leontief among others, in the early part of this century However, few empirically corroborated novel facts were produced out of this new mathematical analysis of international trade

Despite this, the static theory of international trade came to dominate analyses of international trade, culminating in the 'Samuelson shift' in the 1940s, where static analysis took over completely The extent to which static theory came to dommate the analysis of international trade theory, in spite of the lack of empirically corroborated novel facts, is an mdicator of the importance trade theorists have placed on heuristic as opposed to empirical progress One could argue that their choice is essentially a Lakatosian one that, out of heuristic strength should come predictive power This chapter examines the attempts of international trade theorists to derive the predictions of the dynamic theory of international trade from their static framework Specifically, it deals with the attempt to incorporate increasing returns and product differentiation into the static theory

Different fields of economic inquiry can be linked in a fundamental way Remenyi (1979) explored the nature of these links by modifying MSRP, mto what he called, "the theory of core demi-core interaction" (Remenyi 1979 33) Remenyi argued that there is a single research programme m economics - the neoclassical research programme All branches of applied economics he referred to as "sub-disciplines", each of which has its own "demi-core" (Remenyi 1979 33) These sub-disciplmes are m essence mini-programmes - "[t]he demi-core is to the sub-discipline what the hard core is to the SRP [in this case, the neoclassical research programme]" (Remenyi 1979 33) These sub-disciplines sit in the protective belt of the neoclassical programme where they are classified as either progressive or degenerative to the neoclassical programme Thus, the demi-core of a sub-discipline is not fixed, and can "drift into open conflict with the hard core" (Remenyi 1979 34) In the case of conflict, Remenyi cited the defence mechanisms of the negative heuristic of the research programme The "oversight principle" alerts theorists to anomalies arising in the sub-disciplines, a kind of early warning system (Remenyi 1979 35) On the discovery of anomaly, two mechanisms are put into action The "Errant Hypothesis" (the academic response) attempts to disprove the anomaly (Remenyi 1979 35) The EH involves "critical research to evaluate the attack and if possible demonstrate the source of 'error'" (Remenyi 1979 35) The "Institutional Response will operate to isolate the heretics from the mainstream of the discipline" (Remenyi 1979 35)

According to Remenyi, the positive heuristics of the neoclassical programme permeate every part of the entire system of sub-disciplines "In almost messianic fashion they direct economists to go out and preach the dictates of the hard core in every conceivable field of political economy" (Remenyi 1979 47) The positive heuristics produce a "Bravado Impulse", in that economists are "blind to the prospect that anomalies might be encountered" (Remenyi 1979 36) Allied to this is an "Absorptive Reaction" which is "the natural tendency to absorb into an SRP all core-supporting facts and knowledge, plus the equally natural *propensity to learn*" (Remenyi 1979 36)

Perhaps not surprisingly, Remenyi's methodology bears the hallmarks of Mill's methodology The neoclassical research programme constitutes the abstract laws outlined by Mill The protective belt is the application of these laws The disturbing causes do not allow the abstract laws to be rejected in Remenyi's methodology, it is the negative heuristic which prevents rejection of the propositions of the neoclassical hard core The main difference is that in Mill's methodology there is scope for the modification of the abstract laws in the face of disturbing causes, although in respect of international trade theory, Mill seemed reluctant to make such modifications (see chapter 5 3) In Remenyi's methodology, on the other hand, the negative heuristic appears to protect the neoclassical hard core indefinitely

Remenyi's modification makes it easier to elucidate the links between the international trade sub-discipline and other fields of economic research The static and dynamic theories of international trade can be redefined as elements within the international trade sub-discipline And this sub-discipline can be analysed in terms of its dependence on the neoclassical hard core and heuristics This analysis sheds much light on the rise to dominance within the sub-discipline, of the static theory of international trade

In Remenyi's methodology, no sub-disciplme can be independent of neoclassical heuristics without being classified as a degenerative element in the protective belt of the neoclassical programme This raises the question, was the international trade sub-discipline ever independent of these neoclassical heuristics Did the international trade sub-discipline ever constitute a degenerative element in the neoclassical research programme? To answer this, it is necessary to delineate the hard core and the heuristics of the neoclassical programme, and to assess their impact on the development of international trade theory

There have been several outlines of the neoclassical programme (eg Backhouse 1988, Weintraub 1985,1988, and Remenyi 1979) The following is not intended as an exhaustive list of neoclassical characteristics, but it provides the most important features given the context of this analysis

HC1 Consumers have rational preferences

HC2 Producers seek to maximise profits

HC3 "Choices are made in interrelated markets" (Weintraub 1988 214)

HC4 Perfect competition is allocationally optimal

HC5 "Stable Pareto-efficient equilibrium solutions can be defined for any and all markets relevant to economic research and analysis" (Remenyi 1979 59)

The neoclassical programme contains the following heuristics

PH1 Construct models in which an equilibrium exists

PH2 "Test equilibrium for stability, if it is unstable search for the stable solution" (Remenyi 1979 60)

PH3 Investigate how economic systems shift from one equilibrium position to another

PH4 "Always act on the premise that economic welfare is a direct function of economic efficieny and that social welfare is a direct function of economic welfare" (Remenyi 1979 60)

NH1 Do not test the propositions of the neoclassical hard core

In addition to the above, a number of auxiliary assumptions are required to uphold the perfectly competitive equilibrium solution (Latsis 1972)

AA1 Both producers and consumers have perfect knowledge

AA2 There is freedom of entry and exit onto all markets

AA3 Products in industries are homogeneous

The link between the Ricardian model of trade and neoclassical methodology is obvious The Ricardian model led, for example, to the hunt by Mill and Marshall, in the last century, for the conditions of a stable competitive trading equilibrium The Ricardian model was expanded into a general equilibrium model with the inclusion of the factor proportions theorem by Ohlin The search for the stability conditions persisted, through the work of Samuelson, until the late 1960s As Weintraub has pointed out, however, the neoclassical hard core and heuristics outlined above only 'hardened' in the 1950s, with the elucidation of the proof for stable, Pareto-efficient equilibria (Weintraub 1985 112) One would expect therefore that each step in the hardening process of the neoclassical hard core would send shock-waves through the sub-disciplines of its protective belt In the case of the international trade sub-discipline, however, the demi-core remained virtually unchanged from the end of the classical period until the late 1970s

DC1 Patterns of international trade are explained by pre-trade comparative cost differences between countries

DC2 Free trade maximises the overall welfare for trading partners

DC3 The equilibrium level of international trade is determined by the interaction of international demand and supply for traded goods

The development of the general equilibrium methodology during the marginalist revolution, posed no methodological problem for the static theory of international trade which had always been presented in terms of a (incomplete) general equilibrium model The static theory adopted the new methodology with little problem, substituting the problematic real cost assumption with the concept of opportunity cost (Haberler 1930) The search for stable equilibria had been a focus of international trade since Mill's determination of the international terms of trade Marshall showed the conditions for stable equilibrium in his offer curve analysis The dynamic theory of international trade could not, by definition, be subsumed into this static framework, although there were a few attempts to incorporate dynamic aspects of international trade into the static, general equilibrium theory

This chapter is concerned with the attempts to explain international trade under increasing returns to scale and/or product differentiation *within* the neoclassical tradition, and specifically with the constraints imposed upon the sub-discipline by the heuristics of the neoclassical programme in this respect Section 6 2 is concerned with the treatment of increasing returns and product differentiation by trade theorists prior to the publication of Robinson's and Chamberlin's theories of competition Section 6 3 assesses the impact of Robinson's and Chamberlin's theories on the neoclassical tradition Section 6 4 examines the impact of these theories on the international trade sub-discipline during its 'Samuelson shift'

6 2 THE EARLY TREATMENT OF IMPERFECT COMPETITION WITHIN THE INTERNATIONAL TRADE SUB-DISCIPLINE

As was shown in chapter 5 3, Marshall attempted to incorporate increasing returns to scale into his static offer curve analysis of international trade, but found it impossible to establish a stable equilibrium where two countries' offer curves intersected more than once

Marshall attempted to derive a method of dealing with increasing returns to scale m a way that would be compatible with perfect competition The problem was to show that a cost advantage realised by individual firms within an industry would not lead, ultimately, to monopoly Marshall argued the existence of increasing returns to scale which are external to individual firms, but which are internal to the industry as a whole

> The economic use of expensive machinery can sometimes be attained in very high degree in a district in which there is a large aggregate production of the same kind, even though no individual capital employed in the trade be very large (Marshall 1920 271)

In the case, then, of external economies, a single firm cannot realise increasing returns to scale, and the perfectly competitive equilibrium is preserved Marshall's definition of external economies has been widely used by international trade theorists as a way of incorporating increasing returns into the static theory, although Marshall himself did not incorporate external economies into his offer curve analysis

The difficulties inherent m the incorporation of increasing returns into the static theory of international trade were elucidated in the debate between Graham (1923,1925) and Knight (1924,1925)

In Graham's model there are two countries, A and B A has a comparative advantage in the manufacture of watches, produced under mcreasing returns B has a comparative advantage in wheat, produced under decreasing returns Using this model, Graham attempted to show that, where there are non-constant returns to scale, it may, contrary to the implication in Ricardo's theory, matter a great deal where a country's comparative advantage lies Marshall had hinted at this novel fact in his analysis of the pattern of international trade between America and England (see chapter 5 3) This problem with the prediction of mutual gains from trade, in the face of decreasing returns to scale, was largely ignored by subsequent trade theorists until the mid 1980s, when it became the focus of attention in work on strategic trade policy

Graham argued that the opening up of trade would, according to comparative advantage, force B to specialise in wheat production, and this specialisation in a decreasing returns industry could result in welfare losses for B

> It may well be disadvantageous for a nation to concentrate in production of commodities of increasing cost despite a comparative advantage in those lines, it will the more probably be disadvantageous to do so if the world demand for goods produced at decreasing cost is growing in volume more rapidly than that for goods produced at increasing cost, while at the same time competition in the supply of the former grows relatively less intense as compared with competition in the supply of the latter (Graham 1923 213)

Knight argued that Graham failed to specify the *nature* of the increasing returns in the watch industry He pointed out that if the increasing returns are internal to specific firms, then the pre-trade production of watches in B would be monopolistic prior to the opening up of trade And in this case, Knight argued, there would be no reason to suppose that, after trade, B's watch-maker should lose the economies of scale he realised before trade was established He pointed out that in the event of non-reversible increasing returns to scale, there would be no incentive for this watch-maker to switch into the production of wheat after trade Knight concluded as Marshall had done that where the watch industry is subject to internal increasing returns to scale, the pattern of international trade is indeterminate The argument for protection cannot be justified

Knight conceded that, under an assumption of external economies, Graham's conclusion holds (Knight 1924 331) The argument for protection would seem to be validated However, Knight questioned the *empirical* validity of this assumption He insisted that, in order to validate his conclusion about protection, Graham had to first establish "that in a significant proportion of cases industry really operates under decreasing cost, *without tending towards monopoly*, the case of monopoly being expressly excepted" (Knight 1924 331) Knight also considered the theoretical validity of the external economies assumption He argued that although one industry might display external economies, this must be as a result of internal economies elsewhere in the system The implication of Knight's argument is that only a partial analysis of trade under increasing returns is possible

Marshall identified what could be interpreted as an anomaly to the principle of comparative costs, namely the mutual exchange between countries of goods categorised within the same industrial classification

> Belgian steel on its way to England, often crosses English steel on its way to Belgium, but the consignments are likely to be of different qualities, and to

be used for dıfferent purposes (Marshall 1923 104)

Several years later, Taussıg ınvestıgated the extent of "cross trade" ın the US trade of ıron and steel (Taussıg 1931 191) Taussıg found cross trade to exıst between developed countrıes across many ındustrıes "[W]e fınd the perplexıng phenomenon that commodıtıes apparently of the same sort are both brought ınto the country and sent out from ıt Cotton goods, woolens, sılks, ıron manufactures are among both the exports and the ımports of the Unıted Kıngdom (Taussıg 1931 191) The puzzle for Marshall and for Taussıg was ın fındıng an explanatıon for thıs pattern of trade whıch dıd not contradıct the comparatıve cost proposıtıon of the demı-core Accordmg to the Rıcardıan prıncıple of comparatıve advantage, countrıes would specıalıse ın ındustrıes ın whıch they had a pre-trade comparatıve cost advantage Where two countrıes had pre-trade comparatıve cost advantages ın the same ındustry, then the prıncıple predıcts that there would be no trade between them Cross trade appears to be an empırıcal refutatıon of the Rıcardıan demı-core proposıtıon

Both Marshall and Taussıg related thıs type of trade to product dıfferentıatıon The ımplıcatıon was that where ındustrıes are composed of dıfferentıated products, then each of these dıfferentıated products ınvolves a dıfferent productıon functıon If one adhered to the tradıtıonal, classıcal method of ıdentıfyıng ındustrıes as composed of fırms whıch are close substıtutes m productıon rather than ın consumptıon, then a sıngle ındustry composed of dıfferentıated goods could be redefıned as a number of separate industries Under this definition, the pattern of international trade could still be determined by pre-trade comparative cost differences For example, Taussig considered the case of cross trade between America and Germany in sewing machines He argued that while America had an advantage in the production of mass-produced standardised sewing machines, Germany had an advantage in the production of specialised machines (Taussig 1931 199) These advantages stemmed from the different factors and technology necessary to both types of machine, so that this cross trade can be reinterpreted as inter-industry trade

Ohlin raised the possibility that international trade might be determined by consumer preferences "English and Czek boots for ordinary wear cannot be called identical, nor can one say that the former are 'worth', for example, 10 percent more than the latter If their price is 10 percent higher, a certain number of people will prefer one kind and the rest the other If the price increases to 20 percent some people will continue to buy English boots, if it disappears others will still continue to buy English boots" (Ohlin 1933 95) Even if England has no comparative advantage in the production of boots, international demand will ensure that this industry survives Ohlin clearly recognised the impact this hypothesis has on the underlying assumptions of the static theory "It has hitherto been assumed that a country will export things it can make cheaper than other countries and import the rest That statement clearly assumes that the goods are identical in quality, as soon as this condition changes the relationship between prices and international trade becomes more complicated" (Ohlin 1933 95)

There is, however, no suggestion by Ohlin that trade determined by consumer preferences could conflict with the comparative advantage proposition, nor, indeed, any suggestion that it could conflict with Ohlin's factor proportions explanation for comparative advantage In this regard, Ohlin was guilty of the same neglect of which he accused Bastable, namely, a disregard for the impact of empirical facts on the theory of international trade (Ohlin 1933 589)

Ohlin was more explicit in defining the relationship between increasing returns to scale, comparative advantage and the factor proportions theorem He argued that increasing returns provided an explanation for pre-trade comparative cost differences between countries, that was an alternative to his own factor proportions theory (Ohlin 1933 106) Thus, where two countries have identical factor proportions there may still be trade between them, if one country has a pre-trade comparative cost advantage arising out of the realisation of increasing returns Inter-industry specialisation will in this case depend on the relative extent to which increasing returns have been realised across countries (There is a clear link between Ohlin's conclusions on increasing returns and comparative advantage, and those of Linder (1961), see chapter 7 2)

In respect of his analysis on trade within regions, Ohlin took this argument a stage further, and concluded that even in the case where there are no pre-trade comparative cost differences between regions, international trade may still be an outcome if increasing returns exist (Ohlin 1933 54) This international trade would be prompted, not by the *existence* of increasing returns in one part of a region, but by recognition of the *potential* of increasing returns for whichever part of the region is most successful in extending its market through trade (Ohlin 1933 54)

Ohlin noted that, in this case, an equilibrium pattern of trade is indeterminate "The character of this trade will be entirely a matter of chance, if factor equipment is everywhere the same, for it is of no consequence whether a certain region specialises in one commodity or another" (Ohlin 1933 55) This novel prediction clearly conflicts with the prediction of comparative advantage The novelty of Ohlin's prediction was overlooked by subsequent trade theorists who stressed the importance of Ohlin's incorporation of Heckscher's factor proportions theorem into a Casselian-type general equilibrium model of international trade Yet, like Marshall, Ohlin had placed his static analysis of international trade in an appendix to his text

The failure to derive a model of international trade based on the potential for increasing returns, is indicative of the growing links between welfare theory and the static theory of international trade during the 1930s

Developments in the static theory during this period were mainly to do with developments of the techniques of analysis These techniques allowed the movement towards a competitive trading equilibrium to be geometrically shown Haberler introduced the concept of opportunity cost into the static model (Haberler 1930) This allowed for the techniques of welfare theory to be fully incorporated into the static model of international trade Haberler derived the production possibilities frontier, and in 1933, Leontief provided a proof of the derivation of a community indifference curve from individual indifference curves In addition, Leontief derived Marshallian offer curves from community indifference curves, thus providing a link between the standard tools of analysis in both sub-disciplines During this period, the static theory began to dominate in explanations of patterns of international trade

Lerner considered the existence and stability of a trading equilibrium under increasing returns (Lerner 1932,1934) He constructed a composite "production indifference curve", which gave the production potential in a two-country/two-good model (Lerner 1932 331) Lerner did extend his analysis to the case where both goods are subject to increasing returns He concluded that the only stable trading equilibrium under increasing returns would be where each country would become an international monopolist in the production of a single good (Lerner 1932 332) In the case where only one good was subject to increasing returns, the country with the comparative advantage in the production of this good would specialise completely, since there would be no incentive for domestic producers to specialise in the non-increasing returns good The other country would, Lerner argued, remain diversified (Lerner 1932 331) Lerner's predictions as to the outcome of international trade in the static model under increasing returns were generally upheld until the 'new theories of international trade' of the late 1970s and early 1980s It was not until this time that the models of imperfect competition outlined by Robinson (1932) and Chamberlin (1933) had any significant impact on the static theory of international trade

6 3 THE NEOCLASSICAL THEORIES OF IMPERFECT AND MONOPOLISTIC COMPETITION

Marshall had sought to reconcile the notion of increasing returns and the perfectly competitive equilibrium by establishing the phenomenon of external economies The introduction of external economies had two benefits It provided a "safeguard against the common error of assuming that wherever increasing returns operate there is necessarily an effective tendency towards monopoly" (Young 1928 527) It also simplified "the analysis of the manner in which the prices of commodities produced under conditions of increasing returns are determined" (Young 1928 528) However, Marshall failed to show how external economies might arise in actuality

In England, there was a much greater attachment to the neoclassical, static method initiated by Ricardo and developed by Marshall, than in Germany or in America Yet, it was from Cambridge, England, that the most dramatic critique of the neoclassical theory of competition was to come Sraffa argued that increasing returns, no matter how defined, could not be made compatible with competitive equilibrium (Sraffa 1926 196) However, he also held that increasing returns introduced into a competitive structure need not necessarily lead to the establishment of a monopoly

Sraffa held that each firm could influence the price it charged through product differentiation and in this way would prevent a firm with competitive advantages in the form of increasing returns from establishing an equilibrium Sraffa called these industries which displayed both product differentiation and (internal) increasing returns "multiple monopolies" (Sraffa 1926 195) He held that product differentiation would produce an equilibrium, but that the relationship between price and cost would vary across firms in the industry "The conclusion that the equilibrium is in general determinate does not mean that generalising statements can be made regarding the price corresponding to that equilibrium, it may be different in the case of each undertaking, and is dependent to a great extent upon the special conditions affecting it" (Sraffa 1926 195) Sraffa presented an obvious challenge to competition theorists, namely to establish the conditions for equilibrium in industries that were multiple monopolies

Robinson's *The Economics of Imperfect Competition* (1933), was a work which was firmly in the neoclassical welfare theory tradition Robinson was concerned, not with firm behaviour, but with the allocation of resources under non-competitive equilibria She was concerned to protect the demi-core proposition of the welfare sub-discipline which states that perfect competition is an optimal, albeit mythical, system of resource allocation Imperfect competition was so-called because it is sub-optimal In order to show how imperfect competition failed to produce optimal allocation, Robinson devised a model whose assumptions differed from perfect competition in only one respect In Robinson's model of imperfect competition, consumers perceive some difference in the goods produced by firms in the same industry Removing the assumption of perfect elasticity of substitution between products produced a downward sloping demand curve In order to preserve the concept of industry, Robinson had to assume that all firms in her industry have identical production functions In Robinson's model, there is therefore no actual product differentiation

Yet, if there is no product differentiation, are consumers in fact making *irrational* choices between products which were in fact homogeneous? Such an outcome would clearly conflict with the hard core proposition that consumers display rational preferences between goods Robinson was undoubtedly aware of the impact of such a conclusion She argued that "[t]his problem can be evaded if we assume that the imperfection of the market arises solely from differences in transport costs or from such differences between consumers in their preferences for particular firms as cannot be altered by the action of the firms themselves" (Robmson 1932 545) This is not a very satisfactory solution, but it had the vital effect of allowing the concept of industry, as it had been classically defined, to persist Moreover, she was able to assume that no individual firm had control over the demand curve of the industry Thus, the marginal revenue curves facing each firm in the industry are identical

Robinson's next task was to establish the conditions for equilibrium in this industry "The equilibrium of the industry thus requires a double condition Marginal revenue must be equal to marginal cost, and price must be equal to average cost" (Robinson 1932 547) Where price is equal to average cost, there is no incentive to leave or to enter the industry. An outcome of this double condition is that the industry would be in equilibrium where each firm has excess capacity It was exactly this conclusion which Robinson required, in order to show that this form of competition was an imperfect allocator of resources, despite being a stable equilibrium

Robinson's theory can be described as an Errant Hypothesis, as outlined by Remenyi (1979 35) The theory of imperfect competition was an attempt to deflect the anomalies raised by Sraffa (1926) Robinson protected the hard core proposition of the neoclassical research programme, and the demi-core of the welfare sub-discipline, by showing that a deviation from perfect competition would lead to a misallocation of resources through the creation of excess capacity This interpretation of the methodological significance of Robinson's theory explains its popularity among her Cambridge colleagues, despite the obvious inadequacies of her model

Robinson managed to "preserve by sleight of hand the concept of the industry" (Shackle 1967 51) Her reasons for the display of consumer preferences in a homogeneous market were inadequate

> Joan Robinson's demand functions have no analytical roots Her demand curves fall simply because she tells them to do so By this device she virtually assumed that the major theoretical problem had been solved, without actually solving it (Andrews 1966 22)

These inadequate assumptions were nevertheless necessary for the preservation of the predictions of the welfare sub-discipline

Chamberlin's aim, in *The Theory of Monopolistic Competition* (1933), was not to support the neoclassical welfare propositions but to find an alternative to them

> The theory of competition, by its very nature, eliminates the monopoly elements completely, thus erasing a part of the picture and giving an account of the economic system which is so false that in most cases it could not even be called an approximation to it (Chamberlin 1962 206)

Chamberlin, in contrast to Robinson, emphasised anomalies to the neoclassical theory of competition "He was offering a full theory of competition, not of imperfections from a perfect ideal" (O'Brien 1983b 35) Chamberlin's theory was, therefore, part of a different sub-discipline to that of Robinson's Where Robinson was concerned with the preservation of welfare theory, Chamberlin was anxious to develop a theory of firm behaviour The irony is that in the process, Chamberlin came up with the same tangency solution to describe the outcome of competition in an industry with many small firms (the large group case) as Robinson did

Chamberlin introduced the notion of product differentiation into a model of competition among a large number of competing firms He acknowledged that this inclusion blurrs the concept of the industry, so he discussed competition in terms of groups rather than industries (Chamberlin 1962 69) He considered competition in the large group (monopolistic competition) and in the small group (oligopoly) case Chamberlin's large group case was made up of firms with varying degrees of monopoly power, reflected by the slope of the demand curve for their individual product Thus, Chamberlin did not attempt to preserve the neoclassical notion of an industry made up of identical firms, each with no influence over market price, in the way that Robinson had done He willingly deviated from the neoclassical heuristic in his attempt to provide a more realistic model of firm behaviour

The Institutional Response mechanism from the neoclassical programme which Chamberlin's model prompted is well-documented (for example, Shackle 1967 62, Loasby 1971 878) Chamberlin spent the remainder of his academic life trying to establish the difference between his theory of monopolistic competition and Robinson's theory of imperfect competition He argued that Robinson's imperfect competition is not an alternative theory of competition, in the way that monopolistic competition is (Chamberlin 1962 206)

The Errant Hypothesis mechanism, the academic response, was to criticise Chamberlin's model not on the grounds that it gave a less realistic account of competitive forces, but rather because it was too nebulous to produce any empirically testable hypotheses (See chapter 1 3 for an outline of the debate between Archibald and the Chicago school on the theory of monopolistic competition) Lip service is paid to the theory of monopolistic competition "the theory of monopolistic competition is tucked away in every text, but its relevance and its implications are ignored" (Solo 1976 47)

Both Loasby (1971), using a Kuhnian analysis, and O'Brien (1983b), using a Lakatosian analysis, concluded that Chamberlin's theory marked a substantial departure from the neoclassical theory of the firm, and that there is a significant difference between the work of Robinson and Chamberlin "the function of the analysis, in relation to both theoretical issues and their views of the world, is very different for the two authors" (Loasby 1971 876)

In contrast, Latsis (1972) who examined monopolistic competition using a Popperian situational logic analysis interspersed with Lakatosian and Kuhnian terminology, placed Chamberlin's theory firmly within the neoclassical tradition Latsis described both perfect competition and monopolistic competition as coming from the same programme "the neoclassical programme of situational determinism" (Latsis 1972 208) This programme has the following hard core "(i) Profit Maximisation (ii) Perfect Knowledge (iii) Independence of Decisions (iv) Perfect Market" (Latsis 1972 209) The positive heuristic of this programme is comparative statics (Latsis 1972 212) In his argument that both perfect and monopolistic competition fit into this framework, Latsis appeared to have confused monopolistic competition with imperfect competition - "[t]he firms under monopolistic competition produce goods which are different in the eyes of the consumers but which do not demand any special knowledge or advantage. on the part of the producer who is responsible for their differentiation" (Latsis 1972 214) Yet, it is precisely the ability of individual firms to influence demand for their product which distinguishes monopolistic competition from imperfect competition

Latsis argued that perfect competition and monopolistic competition have the same situational analysis - "that optimizing behaviour (yielding merely subsistence profits in equilibrium) is the only way of avoiding elimination from the industry" (Latsis 1972 214) As was pointed out in chapter 1 4, Latsis held that the rationality principle must be empirical if it is to serve any purpose (Latsis 1972 228) In neoclassical economics, the profit maximisation hypothesis exerts cast-iron control on the behaviour of firms Given that both imperfect competition and monopolistic competition are based on an assumption of profit maximisation, they are simply showing the different reactions of firms, according to whether their situational analysis is that outlined by Chamberlin or that outlined by Robinson The rationality principle is the same in both cases Latsis held that the Chamberlin/Chicago controversy was one of number of "mere family quarrels between slightly different variants within the same programme" (Latsis 1972 222)

In Lakatosian terms, neither Robinson's or Chamberlin's models can be considered progressive Robinson's model was specified in such a way that it was anomalous to the hard core proposition that consumers make rational choices Chamberlin's model questioned the hard core proposition that pure competition optimises social benefit, he argued that the increased social welfare from increased variety under monopolistic competition might be greater than any loss in terms of excess capacity Since both models conflict with a neoclassical hard core proposition, they are *ad hoc* Chamberlin's and Robinson's simultaneous elucidation of conditions for equilibrium in imperfect competition/monopolistic competition are the novel facts in both models Therefore, each is classified as *ad hoc*$_3$ to the neoclassical theory of the firm

This conclusion with regard to the models of imperfect competition suggests that, from a Lakatosian perspective at least, these models should not have been incorporated into the static theory of international trade, since they would have been *ad hoc*$_3$ to it And, indeed, except in a few instances, they were not However, in the late 1970s, several models of international trade were proposed, which were based on the models of Robinson and Chamberlin Far from being rejected as *ad hoc* to the international trade sub-discipline, they were hailed as its rejuvenator The following section examines the analysis of increasing returns and product differentiation within international trade theory, in the period following the publication of Robinson's and Chamberlin's models, up to these 'new theories of international trade' of the 1970s and early 1980s

6 4 ATTEMPTS TO MODEL INTERNATIONAL TRADE IN A STATIC IMPERFECTLY COMPETITIVE MODEL

Very few theorists considered the impact of Robinson's or Chamberlin's models on the analysis of international trade This is strange when one considers the lengthy discussions of Ohlin and Marshall, among others, on the impact of increasing returns and product differentiation on the pattern of international trade This lack of response is indicative of the extent to which the static theory of international trade, with its neoclassical foundations, had come to dominate the international trade sub-discipline The dynamic theory, while still used as a basis for policy prescription, was not the standard model of trade to be found in the textbooks of the 1940s Another, perhaps more fundamental, reason for the lack of response by trade theorists, is that these models of imperfect competition were partial analyses, whereas the static theory of international trade from the time of Ricardo presented a general analysis

Up to 1941, there was no attempt to analyse international trade within an imperfectly competitive model, although several trade theorists discussed in a less formal way the implications of Robinson's and Chamberlin's conclusions for the predictions of the static theory (eg Beach 1936, Anderson 1937, McDiarmid 1938) Beach highlighted the extent to which the link between prices and costs of production are broken when the assumption of homogeneous products is removed (Beach 1936 108) Implicit in Beach is the conclusion that the acknowledgement of monopolistic competition would warrant an entirely new proposition upon which to base the determinantion of patterns of international trade, although he himself made no attempt to establish such a proposition Anderson (1937) considered the impact of monopolistic competition on the free trade proposition He argued that free trade would still be the optimal trade policy in the face of product differentiation Protection would only serve to limit the market of domestic producers of differentiated products The only producers who may lose from free trade would be those who produced varieties closely resemblmg imported varieties (Anderson 1937 163) McDiarmid (1938) was more explicit than Beach had been about the problem which product differentiation caused for the comparative advantage proposition In a three-country/two-good model, McDiarmid reiterated Ohlin's argument that patterns of international trade could be determined by the potential for increasing returns to scale (McDiarmid 1938 126)

In 1941, a trade theorist presented a model of international trade under monopolistic competition Implicit in this model was the belief that cross trade necessitated a new theory of international trade, that it was more than simply the statistical phenomenon observed by Marshall and Taussig Lovasy (1941) derived a two country/one good model, where the good is differentiated into a number of varieties Lovasy used a modified version of Hoteling's (1929) location theory model, placing the varieties of the product along a scale according to the elasticity of substitution of consumers for each variety Lovasy did not go any further in the specification of utility functions of consumers She did note, however, that the introduction of more varieties through international trade would change the elasticity of substitution between varieties m each country Lovasy considered the possibility of international trade in a number of different cases

1 where country A has a cost advantage in all varieties, although A does not produce all varieties

2 where there are no cost differences between country A and country B, but consumers in A and B have different tastes

3 where all the varieties produced in country A are of a superior quality to those produced in country B, and there is no cost difference between the two countries

4 where all the varieties produced in country A are of a superior quality to those produced in country B, and A varieties are more expensive than B varieties

None of these patterns of trade between A and B are incompatible with comparative advantage, as Marshall and Taussig had shown in their analysis of cross trade However, Lovasy took the analysis of cross trade a step further She argued that "foreign trade is *caused* by the mere fact of product difference with standardization of the products and no cost or price difference international trade would not take place at all" (Lovasy 1941 582) The demi-core proposition of comparative advantage predicted that where production functions are identical across countries and where there are no pre-trade comparative cost differences between countries, there will be no international trade Lovasy, on the other hand, argued that trade could occur between countries that are identical in every respect including factor endowment, on the grounds of product differentiation This international trade is patently *not* of the same nature as the cross trade described by Marshall and Taussig The pattern of international trade which Lovasy predicted was not cross trade, but intra-industry trade Intra-industry trade is *not* compatible with the demi-core proposition of comparative advantage, because it assumes that the same costs are faced within the industry in both of the trading partners The distinction between intra-industry trade and cross trade did not come to the attention of trade theorists for another 34 years, in Finger (1975) The significance of this distinction between cross trade and intra-industry trade is highlighted in the debate on the validity of the 'new theories of international trade' proposed in the late 1970s

Lovasy does not appear to have been aware of the the significance of her conclusions for the static theory of international trade Nevertheless, her work did contain several novel facts which could, had they been given any serious consideration by trade theorists at the time, have changed the direction of international trade theory markedly The following analysis shows how similar Lovasy's conclusions are to the conclusions of the 'new theories' which led to a radical change in the international trade sub-discipline

To a sub-discipline whose main preoccupation was to find the conditions for competitive trading equilibria, Lovasy's model was too indeterminate to be anything other than *ad hoc*[3] It conflicted with the neoclassical heuristics in that it was a two country/one good model, with no additional assumptions about the factors of production The incorporation of Lovasy's model into the international trade sub-discipline would therefore have rendered the international trade sub-discipline and by association the neoclassical programme, degenerative

The concept of intra-industry trade did not reappear in the international trade sub-discipline until the 1970s Trade theorists continued to try to integrate the phenomenon of increasing returns into the static model of international trade In particular, static theory focused on the impact increasing returns would have on the factor price equalisation theorem (Samuelson 1948,1949,1951,1953)

Both Matthews (1949) and Meade (1952) used Lerner's analysis of international trade under external economies Lerner had argued that complete specialisation by both countries was the only stable outcome where both traded goods were produced under conditions of increasing returns (Lerner 1934) Since Samuelson had argued that incomplete specialisation was a necessary condition for factor price equalisation (Samuelson 1949), it was necessary to refute Lerner's prediction in order to show that factor price equalisation would occur in a competitive trading equilibrium under increasing returns

Matthews however argued that "complete specialisation is inevitable only if the production frontier is more sharply convex than the country's indifference curves" (Matthews 1949 153) He held that there could be a stable equilibrium with incomplete specialisation under increasing returns, if one trading partner was substantially larger than the other (Matthews 1949 153)

Meade concluded that, excepting Matthews' case of comparative size differences between the trading partners, there could be no stable equilibrium with incomplete specialisation in the presense of increasing returns (Meade 1952 40) Meade reiterated Lerner's geometric representation of international trade under increasing returns, and added to it Matthew's exceptional case This became the standard treatment of increasing returns within the static theory of international trade

It was generally accepted that, due to complete specialisation, international trade under increasing returns would not result in factor price equalisation Laing proved this to be the case in 1961 In 1964, Kemp concluded that the only case in which there would be factor price equalisation would be where both countries had identical factor endowments and produced each product to the same scale Of course, under these conditions there would not be any trade, according to comparative advantage (Kemp 1964 122) Kemp did, however, argue the possibility of *relative* factor price equalisation under increasing returns He held that the outcome of relative factor price equalisation would depend on whether the production frontiers of both countries intersected after trade (Kemp 1964 125-7) However, he argued that, while there might be a number of price ratios at which relative factor price equalisation is observed, "[t]he probability that consumer preferences will dictate the establishment of one of those commodity price ratios is slight indeed" (Kemp 1964 126)

The post-Lerner analysis of international trade under increasing returns yielded little in the way of novel facts, either theoretical or empirical The limitations imposed by the neoclassical methodology were apparent to all working in the area "while there will be trade, there is very little else we can conclude about the pattern of trade in the increasing returns situation We have no way of knowing, for example, which country will export which good We know that one country will specialise in X and one in Y, but except for that, the production function is indeterminate Of even more importance, we cannot even be sure that both countries will gain from trade" (Melvin 1969 393) Negishi was more succinct "[w]hen these assumptions [constant returns] are relaxed, we must confess that the subject is in a mess" (Negishi 1972 73)

The problem with analysing trade under increasing returns was not in establishing the compatibility of the phenomenon with the demi core of the static theory of international trade As Ohlin had shown in 1933, there was no intrinsic conflict between the comparative advantage proposition and the existence of increasing returns The problem was one that had been noted much earlier by Marshall, namely that static models imposed severe limitations on the analysis of dynamic phenomena such as increasing returns (Marshall 1923 356)

The phenomenon of intra-industry trade was raised again during the 1960s, with the publication of empirical studies which showed the extent to which countries with the same factor endowments were trading in goods belonging to the same industries (eg Dreze 1961, Verdoorn 1963, Kojima 1964, Balassa 1966, 1967) It should be noted that trade theorists did not always distinguish between cross trade and intra-industry trade In many cases, the empirical data related to cross trade as distinct from intra-industry trade To reiterate, cross trade is trade in varieties which have different production functions Intra-industry trade, on the other hand, assumes that there are no production differences in traded varieties

The lack of distinction between intra-industry trade and cross trade is obvious in the outline given by Grubel in 1970 to show the eclectic approach which trade theorists took to trade within the same industrial grouping Grubel outlined three different approaches

1 The Heckscher/Ohlin theorem which explained "intra-industry trade" according to pre-trade comparative cost advantages across countries in respect of specific sub-industries

2 Linder's theory which explained intra-industry trade in the context of national income differentials (see chapter 7 2)

3 Technology gap theories, which explained intra-industry trade in the context of

the product life cycle and the international transfer of technology (see chapter 7 2) (Grubel 1970 38-43)

The Heckscher/Ohlin theorem cannot explain intra-industry trade, but it *can* explain Marshall and Taussig's cross trade Further evidence that Grubel mistook cross trade for intra-industry trade is to be found in his model of international trade where the traded good is differentiated

Grubel analysed international trade in the context of a two country/one good model, where there were three varieties of the single good (Grubel 1970 45) Grubel concluded, as Lovasy had done, that the pattern of international trade would depend on the elasticity of substitution between different varieties of a single good produced in both countries The difference between Grubel's analysis and that of Lovasy, is that Lovasy considered the possibility of trade on the basis of product differentiation alone Grubel, on the other hand, assumed that factor inputs would vary across varieties Given this assumption, Grubel was able to predict which countries would specialise in which varieties, using the Heckscher/Ohlin theorem But Grubel was able to make this prediction only because he assumed pre-trade comparative cost differences in respect of different varieties across countries The international trade Grubel identified was therefore cross trade, not intra-industry trade Grubel's model was in essence a more formalised version of Marshall and Taussig's arguments While his analysis has a different perspective to Grubel's, Gray's (1973) model of international trade under monopolistic competition does not preclude a comparative advantage explanation of international trade either

Despite some not insignificant problems with empirical evidence on cross trade/intra-industry trade, by the late 1970s there appeared to be a fairly substantial consensus of opinion that the HOS theories were inadequate to explain intra-industry trade Corden's view was a widely-held one "It is desirable that there be developed a rigorous general equilibrium model with economies of scale, possibly embodying some dynamic elements and allowing for more than two products - and yet (ideally) remaining as simple as the popular geometric expositions of the H-O-S model" (Corden 1978 10) What Corden was asking was that further work be carried out within the static theory of international trade, in order that the phenomenon of increasing returns be adequately dealt with The inclusion of increasing returns into the static theory would have necessitated a change in the neoclassical heuristics of that theory

In his response to Corden's paper, Krugman took a much stronger anti- factor proportions line "the evidence on intra-industry trade does more than downgrade conventional factor proportions theory it provides considerable positive support to one particular alternative theory, which combines factor proportions with economies of scale and differentiated products" (Krugman 1978 13) Krugman argued that it would be possible to construct a model of trade under monopolistic competition, and cited Gray (1973) in this respect (Krugman 1978 14) Moreover, Krugman held that "a model which combines scale economies and factor proportions makes some substantive predictions which seem to be borne out in practice" (Krugman 1978 14) Krugman based this conclusion on a model he had already derived and which was published the following year (Krugman 1979)

In his 1979 paper, Krugman derived a two country/one good model where the single good was made up of several varieties the production of which were subject to increasing returns Krugman showed that trade could be stimulated by the potential that exists for increasing returns through the extension of the market "Trade need not be as a result of international differences in technology or factor endowments Instead, trade may simply be a way of extendmg the market and allowing exploitation of scale economies, with the effects being similar to those of labour force growth and regional agglomeration" (Krugman 1979 479) This prediction first appeared in Ohlin (1933) and later in in Lovasy (1941) Negishi (1969) also produced a model which incorporated economies of scale, on the grounds that "it is preferable to develop an endogenous theory of trade since there is a possibility that international trade and specialization as such creates the comparative advantage" (Negishi 1969 132) Negishi went on to show that gains from trade between countries which are identical are upheld where Marshallian external economies are held to be irreversible

In Krugman's model, all varieties of the traded product enter the utility function of each consumer symmetrically Under this condition, the opening up of trade increases the scale of production for each firm as well as the range of varieties available in each country Krugman held that international trade could be stimulated by the demand for additional varieties by consumers, irrespective of whether pre-trade comparative cost differences exist Krugman was, however, unable to predict which countries would produce which varieties after the opening up of trade Again, this argument is not new. Exactly the same point was made by Lovasy (1941) What is important here is that Krugman was dealing with intra-industry trade, not with cross trade or two-way trade as his immediate predecessors had done This was the first mention of the possibility of international trade in the good of a single industry where there were no cost differences across trading partners in respect of the good, since Lovasy (1941) Krugman, like Lovasy, was unable to make any predictions about what country would produce what varieties after trade links were established

Lancaster (1980) specified an alternative model to that of Krugman, but arrived at the same conclusions. Lancaster's model was a two country/*two* good model, where one good was a differentiated manufactured good, and the other a standardised agricultural product Thus, it was an attempt at a more general version of international trade under monopolistic competition In Lancaster's model, only one variety of the manufactured good enters the utility functions of consumers, along with the agricultural product

Where the consumer's most preferred variety is not available, he might be persuaded to accept another variety on the basis of a price differential between this variety and his most preferred variety In Lancaster's model, thetrading partners are identical in every respect Every consumer in one country is matched in the other country by a consumer with identical preferences Lancaster concluded that "intra-industry trade will certainly occur when the economies are absolutely identical in all respects *and* can persist under conditions of comparative advantage" (Lancaster 1980 174, my italics) The pattern of trade would, according to Lancaster, depend on the elasticity of substitution among all the product varieties The agricultural product was assumed to be a non-traded good Lancaster also predicted that international trade could occur where there was the potential for economies of scale but no pre-trade comparative cost differences between two countries

Lovasy, Krugman and Lancaster arrived at the same prediction, namely that international trade could occur without any pre-trade comparative cost differences between countries (Lovasy is not acknowledged in either Krugman 1979 or Lancaster 1980) However, while Lovasy's prediction was ignored, the Krugman and Lancaster models were seen as the rejuvenators of international trade theory A Lakatosian analysis can shed some light on this rather paradoxical treatment of what was essentially the same set of novel facts

Lovasy, by predicting the possibility of intra-industry trade where no pre-trade comparative cost advantages are present, introduced an anomaly to the static and the dynamic theories of international trade, both of which were based on the principle of comparative advantage In a Lakatosian framework, such anomalies are tolerable, provided that the research programme (or in this case, sub-disciplme) continues to generate empirical novel facts that are derived in a way that is consistent with the heuristics of the programme (sub-discipline) Under these conditions, Lakatos argued "it may be rational to put the inconsistency into some temporary, *ad hoc* quarantme, and carry on with the positive heuristic of the programme" (Lakatos 1970 143)

During the Samuelson shift, the links between the static theory of trade and the hard core of the neoclassical programme strengthened, to the extent that the static theory dominated the international trade sub-disciplme One could in fact argue that during this period, the international trade sub-discipline went from being a quasi-independent sub-discipline to being simply a set of hypotheses within the protective belt of the neoclassical programme, such was the preoccupation among trade theorists with proving the existence of a trading equilibrium (de Marchi 1976) Given the strength of the neoclassical heuristics at this time, the reaction or rather lack of reaction to Lovasy's theory is explicable in Lakatosian terms Under MSRP, the incorporation of Lovasy's *ad hoc*$_3$ theory would have rendered the static theory degenerative

Yet, Krugman's and Lancaster's models, although they reiterate the novel facts found in Lovasy (1941), were treated very differently How can MSRP explain this dichotomy?

During the 1970s, the international trade sub-disciplme began to display signs of a creative shift The emphasis on the existence and stability of competitive trading equilibria waned, and trade theorists turned to the problem of providing an explanation for actual international trade patterns The empirical work on mtra-mdustry trade is an example of this change in direction Such a change of direction in turn necessitated the modification of the restricting neoclassical heuristics adopted by the international trade sub-discipline The new theories of international trade played a major role in this reorientation of trade theory With the loosening of the heuristic constraint, Krugman's and Lancaster's predictions could be introduced into the sub-disciplme, where Lovasy's could not

The incorporation of the new theories of international trade into the sub-disciplme was not, however, an indication of independence from the neoclassical programme The incorporation of the new theories was facilitated by prior heuristic changes within the neoclassical programme m respect of its treatment of imperfect competition The neoclassical programme has dealt with imperfect competition by devising a series of models, each of which derives equilibrium under different forms of imperfect competition Trade theorists adopted this method on the grounds that "it is better to have a collection of examples that seem to capture what is actually going on than to restrict oneself to a fully integrated theory that does not" (Helpman and Krugman 1985 4) Thus, the impetus for a creative shift, a change in the heuristics of the international trade sub-discipline, came from the changes in the greater neoclassical economics research programme, as much as from any desire on the part of international trade theorists to provide a more realistic explanation of international trade patterns

According to Lakatos, a creative shift changes only the positive heuristics of a programme (Lakatos 1970 137) The Lovasy/Krugman/Lancaster predictions consitituted more than a creative shift in the international trade sub-discipline The principle of comparative advantage is accepted now as an explanation for only a certain number of world trade patterns It has been supplemented by two other demi-core propositions which provide an explanation for international trade which does not occur on the basis of pre-trade comparative cost differences between trading partners

HC1 Inter-industry trade is explained by differences in the comparative costs of production between countries

HC2 Intra-industry trade is explained by the differentiation of products across countries

HC3 Where there are different relative prices across countries, there will be gains from trade from exchanging goods at intermediate prices

HC4 Where there are no pre-trade comparative cost differences across countries, there may still be gains from trade in terms of consumer choice where traded goods are differentiated, or where the expansion of the market allows for the realisation of economies of scale

HC5 Free trade with compensation will increase global welfare

In MSRP, any incorporation of theories into a programme which involve a change in the hard core of that programme, amounts to the generation of a new hard core Using Remenyian terminology, the incorporation of the new theories into the international trade sub-discipline generated a change in the demi-core of the sub-discipline, and therefore resulted in the creation of a new sub-discipline This new sub-discipline is still dependent on the heuristics of the neoclassical programme, in the same way that the old sub-discipline was There is considerable continuity between the old and new sub-discipline, in terms of the issues addressed For example, there have been attempts to establish whether factor price equalisation is an outcome of international trade under various models of trade under imperfect competition (see Helpman 1981) The emphasis in the new sub-discipline is still on the existence, nature and stability of trading equilibria Given this continuity, the shift from the old international trade sub-discipline to the new one cannot be interpreted as a paradigm-shift

The Krugman and Lancaster papers did not generate novel facts This suggests that their incorporation into any international trade sub-discipline cannot be held as progressive in the Lakatosian sense However, it is more accurate to regard these papers as only a part of the development of the new sub-dıscıplıne, a new sub-dıscıplıne whıch dıd go on to generate novel facts Sınce the early 1980s, the ınternatıonal trade sub-dıscıplme has become more empırically-orıented, and several of these novel facts have been empırically corroborated Thus, the ınternatıonal trade sub-dıscıplıne can now be descrıbed as progressıve ın the Lakatosıan sense

6 5 CONCLUSIONS

In the early part of this century, the international trade sub-discipline is principally characterised by the technical refinements of the static theory of international trade While there were few novel facts to be gleaned from this exercise, it is an indication of the importance which trade theorists have attached to heuristic development, as opposed to the generation of empirically corroborated novel facts

The development of the techniques of comparative static analysis, along with Ohlin's completion of the Ricardian two country/two good model, gave rise to the dominance of the static theory within the international trade sub-discipline from the 1930s The problem this presented for trade theorists was in reconciling the adoption of the static theory, with the knowledge that increasing returns had an important role to play in the determination of patterns of international trade Despite Marshall's warning that his comparative static method does not lend itself to analysis under increasing returns, a few trade theorists persisted in the attempt to derive a model of trade under increasing returns Very few novel facts were generated out of this analysis The most significant, in hindsight, is the novel prediction of Lovasy (1941), that international trade can occur without the initial assumption of pre-trade comparative cost differences across countries

MSRP explains the lack of response to Lovasy's novel prediction Lovasy's novel prediction was derived from a partial two country/one good model which conflicted with the general equilibrium heuristics of the static theory of international trade, making her prediction *ad hoc*$_3$ to the static theory at that time In addition, Lovasy's prediction questioned the adequacy of the principle underlying both the static and the dynamic theories of international trade, the principle of comparative advantage

Krugman (1979) and Lancaster (1980) reiterated Lovasy's novel prediction in more formalised models While they may have provided more precise explanations of intra-industry trade, they did not provide any novel facts in addition to that of intra-industry trade Hence their models cannot be considered progressive Yet, these papers marked the beginning of a whole new static theory of international trade

The adoption of the new theories of mternational trade once again indicates the concern which trade theorists place on heuristic development The Krugman/Lancaster models were part of a heuristic shift in the international trade sub-discipline which was to lead to a new sub-discipline They did not m themselves generate novel facts, but these models marked the acceptance within international trade theory of the derivation of partial models of international trade This is a substantial change in perspective from the general equilibrium methodology which had characterised the static theory of international trade since the time of Ricardo The Krugman/Lancaster models were new, not in the Lakatosian sense that they generated novel facts, but in a heuristic sense This change in heuristics allowed for the incorporation of Lovasy's novel prediction into the demi-core (although this novel fact has not been recognised as originating from Lovasy) This change to the demi-core resulted in the development of a new sub-discipline, but a new sub-discipline which displays considerable continuity with its predecessor

It has been argued above that the heuristic change in the international trade sub-discipline was facilitated by heuristic changes that had been previously made within the neoclassical programme There was, in addition, some impetus for change from within the international trade sub-discipline itself The dynamic theory of international trade continued to be developed during the 'Samuelson shift' in the international trade sub-discipline, and the novel facts it generated, placed further pressure on the static, neoclassical heuristics of the sub-discipline The following chapter examines the development of the dynamic theory of trade, and compares this development to that of the static theory under the 'Samuelson shift'

CHAPTER SEVEN

SCIENTIFIC PROGRESS IN THE
STATIC AND DYNAMIC THEORIES OF INTERNATIONAL TRADE

Up to and including Ohlin (1933), the dynamic and static theories were depicted as complementary, though separate theories of international trade There were few attempts to link the two approaches This chapter explores the post-Heckscher/Ohlin theorem development of the two theories, during the 1950s and 1960s Judging from the textbooks of the day, it would appear that the static theory, during this period, dominated in any explanation of international trade patterns This chapter considers whether this dominance of the static theory was warranted by progress in the static theory of international trade, as opposed to that of the dynamic theory The first section of this chapter considers the development of the dynamic theory of international trade, concentrating on three theories, those of Linder (1961), Posner (1961) and Vernon (1966) The second section examines the development of the static theory of international trade during its 'Samuelson shift' The final section uses the theories of scientific rationality explored in Part One of the dissertation, to analyse why the static theory of international trade was emphasised as the principal explanation for international trade patterns during this period

7 1 PROGRESS IN THE DYNAMIC THEORY OF INTERNATIONAL TRADE

During the 1950s, the dynamic theory of international trade does indeed appear to have become relatively isolated, as the sub-discipline became preoccupied with the question of factor price equalisation in the Heckscher/Ohlin model It was not until the early 1960s, that a reconsideration of the dynamic aspects of international trade occurred This reconsideration came from two angles Linder (1961) considered the impact of increasing returns on international trade, while Posner (1961) analysed the impact of technological change on international trade

Linder questioned the relevance of the factor proportions theorem in explaining international trade patterns Linder held that the other reasons which Ohlin gave for international comparative cost differences, like economies of scale and transportation costs, are much more significant in explaining comparative advantage (Linder 1961 17) Linder's theory did not, therefore, conflict with the demi-core proposition of the static theory that international trade patterns are determined by comparative advantage But his model of international trade was not in the static, comparative static tradition It was not consistent with the heuristics of the static theory

Linder's was an attempt to amalgamate theories of growth and international trade, in a way reminiscient of Smith His primary focus was on the extent to which countries are able to reallocate resources when the opening up of trade forces them to specialise in particular lines of production Smith had argued that the reallocation of resources into the specialist industry would depend on the homogeneity of factors of production (Smith 1776 Vol I 493) Linder argued that in certain countries, the scope for this reallocation of resources would be slight and the gains from trade limited (Linder 1961 12-13) He derived two models, one which considered the effect of international trade on underdeveloped countries, and one on developed countries He argued that economic growth would depend on the relative ability of countries to reallocate resources "Countries with an ability to reallocate factors of production are likely to be able to *accumulate* material resources at a rate faster than that at which population increases, and they are thus probably passing through a process of economic growth reflected in per capita incomes" (Linder 1961 49) Moreover, he argued that countries with a faster growth rate are likely to be able to reallocate resources at a permanently faster rate, thus ensuring the persistence of a growth differential between high-growth and underdeveloped countries "Since trade will stimulate growth in growth countries - but not in U-countries [underdeveloped countries] - our theory leads to the important conclusion that *the per-capita income gap as between U-countries and growth countries will grow faster under trade than under autarky*" (Linder 1961 134)

Lınder consıdered ıncreasıng returns to be an alternatıve to the factor proportıons explanatıon for pre-trade comparatıve cost dıfferences, but derıved no formal model In respect of the ımpact of ıncreasıng returns on the development of comparatıve cost advantage, he made the followıng predıctıon "We shall claım that a country cannot achıeve a comparatıve advantage ın the productıon of a good whıch ıs not demanded on the home market" (Lınder 1961 17) Thıs argument ıs hınted at ın Mıll (1892) and ıs explıcıtly made m Marshall (1925) The argument ıs that the sıze of the home market determınes the extent of mcreasıng returns whıch ın turn determınes the country's comparatıve cost advantage Lmder, however, went on to argue that, hıgh-mcome countrıes wıll, prıor to trade, specıalıse ın the productıon of goods the demand for whıch comes from hıgh-mcome earners The level of economıes of scale earned through thıs autarkıc specıalısatıon wıll determıne the hıgh-ıncome countrıes' comparatıve advantage Lmder argued that gıven the preference of hıgh-mcome countrıes for the same types of goods, the extent of ınternatıonal trade would be greater between countrıes wıth the same per-capıta ıncome levels (Lmder 1961 17) Due to the exıstence of ıncreasıng returns, each hıgh-ıncome country would specıalıse ın only a lımıted range of these hıgh-mcoıne goods

Lınder noted that the ıntroductıon of mcreasmg returns would brıng wıth ıt the possıbılıty of dıfferıng productıon functıons at dıfferent output levels wıthın the same ındustry "ıf productıon functıons dıffer ıt may be ımpossıble to dıstınguısh between labor- and capital-intensive industries at all relative prices" (Linder 1961 129) In this case, it becomes more difficult to identify industries as labour or capital intensive, and the HOS theorem is inapplicable, unless industries are divided into labour and capital intensive sub-industries

Linder considered the impact of the increasing returns explanation for comparative advantage on the factor price equalisation hypothesis Under the HOS theories, Samuelson had shown that relative and absolute factor price equalisation would be the outcome of free trade (Samuelson 1948b) Linder predicted that there would not necessarily be any movement to factor price equalisation where international trade was determined by increasing returns rather than by factor proportion differentials across countries Where increasing returns are present in labour-intensive goods, Linder argued, there is no reason why the cost of labour should rise as production is increased "Only to those who are indoctrinated with the factor cost equalization theorem could it seem provocative to conclude that a labor-abundant country, although it takes part in international trade, will have relatively low wages" (Linder 1961 132)

The Stolper/Samuelson (1941) theorem of the static theory, predicts that inter-industry specialisation according to comparative advantage would reduce the returns to the scarce factor of production, as the country would demand relatively greater amounts of the abundant factor in order to facilitate export demand Linder argued that just as there is no reason to suppose that the abundant factor gains where trade is determined by increasing returns, likewise there is no reason to suppose that the scarce factor loses out Linder argues that for developed countries, "as total income increases in consequence of reallocation, the absolute renumerations will increase" (Linder 1961 132)

Linder pointed out that in the HOS theories, international trade is a substitute for international factor movements Where international factor movements cause factor price equalisation then there is no incentive to trade, since comparative cost advantages, under the HOS theories, can only be explained by factor price differentials across countries In Linder's theory, however, "labor and capital movements will *tend to increase trade* by making factor endowments and per capita incomes more equal" (Linder 1961 139) In other words, the international movement of labour and capital, by equalising factor prices across countries, would also equalise per capita incomes across countries This would, according to Linder, expand the volumes of international trade between these countries, as the increased international demand for goods, whose production is subject to economies of scale, would allow for the further development of comparative cost differentials between countries

Linder found it curious that relatively little empirical testing of the predictions of the static theory had been undertaken (Linder 1961 142-143) Linder conducted some rudimentary testing of the predictions of his own theory In particular, he studied the propensity of high per capita income countries to trade with each other (Linder

1961 110) While his results broadly supported his hypothesis that volumes of international trade will be greater between countries with similar per capita incomes, Linder did point to technical problems in his statistical analysis - the incompatibility of international trade statistics, the use of per capita income as a proxy for demand, etc , - and he called for more rigorous empirical specification and testing of this hypothesis He also suggested areas for further empirical research, such as the extent to which trade in certain product categories is expanding or declining as a function of per capita income growth, and the reaction of individual countries to these changing patterns

Linder had a different agenda of problems to that of the static theory of international trade, and this necessitated an alternative methodology The difference between the static theory and Linder's dynamic theory become more obvious, when Linder's theory is represented in Lakatosian/Remenyian terms Linder's theory contains the following demi-core propositions

DC1 Economic growth under trade follows a different pattern to that under autarky

DC2 The dynamic gains from trade for a country are determined by that country's ability to reallocate factors of production

DC3 The level of demand in a country determines the extent of economies of scale derivable, and in turn the comparative advantage of that country

Along with these demi-core propositions, Linder's theory incorporates the following heuristics

PH1 Construct models which outline the links between growth and trade

PH2 Assess the welfare effects of international trade for countries with differing productive capabilities

PH3 Statistically analysis the predictions derived from auxiliary hypotheses

The following auxiliary hypotheses form the protective belt of Linder's theory

AH1 Underdeveloped countries are less likely to be able to reallocate factors as efficiently as growth countries

AH2 The level of trade is likely to be higher among countries with similar per capita incomes, than between countries with different per capita incomes

AH3 The per capita income gap between developed and undeveloped countries will grow faster under trade than under autarky

Linder's theory is more compatible with the dynamic, Smithian theory of international trade It can be interpreted as a progressive development in the dynamic theory, producing as it did a variety of novel facts The novel predictions generated by Linder's theory include the following that the extent of a country's comparative advantage depends on the level of domestic demand for exported goods, that countries with similar per capita income levels will trade relatively more intensively with each other, that international mobility of the factors of production will stimulate, rather than depress trade volumes Some of these novel predictions were empirically corroborated Linder's theory can therefore be considered a progressive shift in the dynamic theory of international trade More evidence of Lakatosian progress in the dynamic theory of international trade at this time can be found in the analysis of international trade patterns and technological development by Posner (1961)

In 1961, Posner argued that the static theory might only be an explanation of short run international trade flows "'comparative cost differences' may induce trade in particular goods during the lapse of time taken for the rest of the world to imitate one country's innovation" (Posner 1961 323) As with Linder, Posner's aim was not to replace comparative advantage, but rather to explain why, when the international transfer of technology is taken in account, the factor proportions theorem "may not provide the whole answer to the questions at issue" (Posner 1961 323) His aim was rather to provide an "explanation of the process of generation of comparative advantage through time" (Posner 1961 328)

Posner argued that the gap in the development of technology across countries means that "a cause of trade exists which is independent of any of the previously existing comparative cost differences" (Posner 1961 323) The introduction of an assumption of variable technological development across countries conflicts with the HOS assumption that production functions are identical across countries "trade, then, may be caused by the existence of some technical know-how in one country not available elsewhere, even though there may be no international differences in relative endowments of factors of production *strictu sensu*" (Posner 1961 324) Moreover, Posner went on to argue that differences in factor endowment across countries might well be a result rather than a cause of international trade (Posner 1961 331) The generation of extra capacity in the country which developed the new technology may involve the flow of foreign capital into this country, making it more capital intensive and its trading partner more labour intensive

Posner identified a number of different lags which determine the length of time for which the country with the new technology retains its comparative advantage in this new technology The imitation lag is the length of time it takes for the firms of the trading partner to adopt the new technology (Posner 1961 331) The foreign reaction lag is "that time which elapses between the successful utilization of an innovation by one firm in the foreign country and the new good's becoming regarded, by producers in the domestic market, as a likely competitor on the same footing as a domestic product" (Posner 1961 333) A demand lag represents the substitutability between the imported new good and the domestic older variety (Posner 1961 333)

Posner put forward two alternative hypotheses on the relationship between the demand lag and the foreign reaction lag The first hypothesis is that the lags would be very similar in size "the more slowly the sales of a new foreign product could be promoted in the domestic market, the less the incentive to imitation, and vice versa" (Posner 1961 333) The second hypothesis argues that the two lags are positively, although not strongly correlated "the foreign reaction lag could be much smaller than the demand lag if domestic producers were more alert to foreign developments than were foreigners to the possibilities of their export market" (Posner 1961 333) Posner favoured the second hypothesis

Posner outlined a model of international trade, where the length of time a country holds on to its technological comparative advantage depends on the size of the net lag, (*L* - _) *L* is equal to the foreign reaction lag + the imitation lag + the learning period, and _ is equal to the demand lag Posner depicted the following no-trade scenario

> If the imitating country's entrepreneurs in the relevant industry are particularly quick at imitation, the reaction lags may be very small, and if the learning period is also small, it may be that the imitation lag is smaller than the demand lag, in this case, there will be no trade (Posner 1961 335)

Posner's model of international trade does not explicitly incorporate cross trade, although it could be easily modified to include the phenomenon Posner did comment on the possibility of cross trade in his discussion of inter-regional trade

> Innovations affecting only single products, in an industry where multi-product firms predominate, present an interesting special case The switch of one firm's capacity to the standardized production (by a new technique) of one commodity will automatically leave unsatisfied those

> customers which had previously purchased other products from the innovating firm and this market (often a relatively unprofitable one) will fall to the backward firms (Posner 1961 331)

Like Linder, Posner's theory generated a number of novel predictions, the principal one being that the pattern of international trade changes with the international transfer of technology Like Linder's, it retained the principle of comparative advantage as the underlying explanation for international trade patterns Its principal dispute was, again like Linder's theory, with the factor proportions explanation for comparative advantage Yet, neither of these theories were simply static replacements for the factor proportions theorem They involved the use of a very different set of heuristics to those of the static theory, the most obvious being their emphasis on empirical investigation

In his product life cycle theory, Vernon (1966) went further than Linder and Posner, in that he attacked, not only the factor proportions theorem, but also the principle of comparative advantage He argued that neoclassical analysis had outlived its usefulness "It is doubtful that we shall find many propositions that can match the simplicity, power, and universality of application of the theory of comparative advantage and the international equilibriating mechanism, but unless the search for better tools goes on, the usefulness of economic theory for the solution of problems in ınternatıonal trade and capıtal movements wıll probably declıne" (Vernon 1966 190)

Lıke Lınder, Vernon too had a dıfferent agenda to that of the statıc theory of ınternatıonal trade He retaıned nothıng of the statıc and dynamıc theorıes of ınternatıonal trade Vernon's was a whole new theory of ınternatıonal trade, whıch drew on theorıes of ınnovatıon, ındustrıal growth, marketıng and the behavıoural theorıes of the fırm Vernon approached ınternatıonal trade from the perspectıve of the ındıvıdual fırm and ıts strategy *vıs-a-vıs* export markets The mam strategy optıons for the fırm, as Vernon saw ıt, are to export or to engage ın foreıgn dırect ınvestment In Vernon's lıfe cycle theory, the pattern of ınternatıonal trade ıs dependent on the decısıon to export m preference to dırect ınvestment by ındıvıdual fırms Thıs decısıon, ın turn, depends on the stage of development of the market for the product of the fırm

Vernon outlıned the stages of productıon of a good Each of these stages has specıfıc characterıstıcs whıch ınfluence the decısıon about how to servıce the export market for the good In the fırst stage of productıon, Vernon argued that the product ıs lıkely to be dıfferentıated due both to the adoptıon of a number of dıfferent productıon functıons and a hıgh degree of marketıng (Vernon 1966 195) At thıs stage of productıon, the fırm ıs faced wıth a number of strategy optıons, but due to the precarıousness of ıts posıtıon m the market these decısıons are lıkely to ınvolve *regıonal* as dıstınct from *ınternatıonal* locatıonal or export strategıes In contrast, ın the mature stage of production, Vernon argued that the market for the product is more firmly established and the need for flexibility in production functions declines Vernon held that at this stage, firms become less concerned with the differentiation of their product and more with the accumulation of economies of scale It is at this stage of production that the firm turns to the question of how to expand its market The decision facing the firm is whether to service this expanded market through exporting or through foreign direct investment (Vernon 1966 200)

Vernon held that the main influence on this decision is the cost of foreign labour relative to transportation costs He suggested the possibility of an extreme case where domestic firms might switch all their production abroad and service the domestic market through exports "If labor cost differences are large enough to offset transport costs, then exports back to the United States may become a possibility as well" (Vernon 1966 200) Vernon predicted that foreign direct investment would be more likely to be chosen by a firm involved in labour-intensive industries with standardised production methods He also used his theory of the international product life cycle to provide an explanation of the Leontief Paradox What Leontief's empirical results showed, according to Vernon, was that "the United States [was] exporting high-income and labour-saving products in the early stages of their existence, and importing them later on" (Vernon 1966 201)

Vernon predicted that "at an advanced stage in the standardisation of some products, the less-developed countries may offer comparative advantages as a production location" (Vernon 1966 202) Vernon based this prediction on an assumption that "highly standardized products tend to have a well-articulated, easily accessible international market and to sell largely on the basis of price" (Vernon 1966 203) Vernon went on to describe the likely characteristics of goods exported from lesser developed countries - "[t]heir production function is such as to require significant inputs of labor they are products with a high price elasticity of demand for the output of individual firms products whose production process did not rely heavily upon external economies products which could be precisely described by standardised specifications and which could be produced for inventory without fear of obsolescence" (Vernon 1966 204)

Vernon's demi-core proposition that the pattern of international trade will be determined by developments in the production process would appear to conflict with the demi-core comparative advantage principle of the static theory For Vernon, national comparative advantage in the production of a good is temporary, if it is viewed in terms of national comparative costs of production Where the production function of a good changes over time, the country in which the original production began may lose its comparative cost advantage as the necessity for certain factors decline and the demand for others increase Production then shifts to another country

But is this a source of conflict with the comparative advantage principle? Vernon's argument could be restated in comparative advantage terms a country which is capital intensive will initiate production of a capital intensive good, as the production of that good matures and less new capital and technology are required, production of the good becomes relatively more labour intensive and the country loses its comparative advantage, production of the by now labour intensive good switches to labour intensive countries From this perspective, Vernon's theory is not necessarily incompatible with the principle of comparative advantage or with the factor proportions theorem (This is contrary to the view expressed by Deardorff 1984)

Vernon's approach to international trade is very different from the static theory, and from the theories of Linder and Posner His theory can be incorporated with Linder and Posner into the dynamic theory of international trade, if one accepts the argument presented above that Vernon's theory does not conflict with the principle of comparative advantage Grouping the three theories together in this way suggests that the dynamic theory of international trade, during this period, was characterised by methodological pluralism What ties the three theories together, however, is their support of the comparative advantage proposition and an emphasis on empirical investigation A Lakatosian analysis would suggest, however, that this commonality is not enough to prevent Vernon's theory from being *ad hoc*$_3$ to the dynamic theory Despite its novel facts, it is not consistent with the macroeconomic view of international trade taken by the dynamic theory

7 2 THE DEVELOPMENT OF THE STATIC THEORIES THE SAMUELSON SHIFT

The Heckscher/Ohlın theorem resolved the classıcal problem of how to lınk ınternatıonal prıces to the productıon functıon, and allowed for the development of a complete general equılıbrıum formulatıon of the statıc theory of ınternatıonal trade From a neoclassıcal perspectıve, the elucıdatıon of the Heckscher/Ohlın theorem and the development of technıques of analysıs were the mam events ın the ınternatıonal trade sub-dıscıplme durıng the 1930s Both events allowed for the development of a strong neoclassıcal heurıstıc wıthın the sub-dıscıplıne From thıs tıme untıl the 1970s, those workıng wıthın the statıc theory developed an ıntense preoccupatıon wıth the axıomatıc deductıon of hypotheses based on the general equılıbrıum framework provıded by the Heckscher/Ohlın theorem Thıs set of hypotheses ıs commonly referred to as the Heckscher/Ohlın/Samuelson (HOS) theorıes, m order to acknowledge the work done m thıs regard by Samuelson

From the mıd-1940s, the statıc theory underwent quıte dramatıc formalısatıon Thıs development was dırectly tıed to developments ın mathematıcal economıcs, general equılıbrıum theory, and welfare economıcs Thıs sectıon consıders the mam aspects of that development

In Stolper and Samuelson's 1941 paper, they were concerned with Haberler's (1936) prediction that where factors are immobile across industries there could be substantial welfare losses from specialisation according to comparative advantage (This was a possibility later considered by Linder, although he does not refer to Haberler's prediction)

Stolper and Samuelson agreed that in the two-factor/two-good model, "*international trade necessarily lowers the real wage of the scarce factor expressed in terms of any good*" (Stolper and Samuelson 1969 257) This admission raised a serious question for the demi-core proposition that free trade optimises welfare While it was a relatively simple affair to show that the scarce factor would lose from international trade, it was more difficult to analyse the effect on the scarce factor in a multi-factor/multi-good model Stolper and Samuelson concluded that in a multi-*factor* model, the factor proportions explanation for comparative advantage is unlikely to hold due to the existence of factor intensity reversals The existence of more than two factors increases the possibility that a number of different production functions could be used to produce the same identical good Thus, the definition of industry employed by the HO theorem is no longer appropriate in a multi-factor model of international trade However, Stolper and Samuelson pointed out that irrespective of the causes of pre-trade comparative cost advantages, international trade could still render losses to a particular groups of factors, although the possibility of "diverse patterns of complementarity and competitiveness" means that this loss is not inevitable (Stolper and Samuelson 1969 267) They do concede that these factors could only be identified *after* the process of specialisation has begun In a multi-*commodity* model, Stolper and Samuelson argued that the scarce factor (in their case, labour) will lose from trade, since "there will inevitably be a *relative* substitution of labour for capital *in each line* of production" (Stolper and Samuelson 1969 263)

This admission of welfare loss would seem to conflict with the demi-core of the international trade sub-discipline, were it not for the fact that Stolper and Samuelson make the following assertion "We are anxious to point out that even in the two factor case our argument provides no political ammunition for the protectionist For if effects on the terms of trade can be disregarded, it has been shown that the harm which free trade inflicts upon one factor of production is necessarily less than the gain to the other" (Stolper and Samuelson 1969 267) To protect the free trade policy in the face of the Stolper/Samuelson prediction, it was necessary to introduce a proviso in the free trade proposition

DC4 Free trade (with compensation) increases the welfare of all participants

Stolper and Samuelson provided the justification for the compensation principle by showing that the welfare loss to the scarce factor will always be less than the overall gain from trade in the static two-factor/two-good model Even though this modification of the free trade proposition conflicts with the negative heuristic instruction not to interfere with the demi-core propositions, the modification was made in a way that was compatible with the neoclassical heuristics of the static theory This is a reversal of the Lakatosian conditions for a creative shift In MSRP, a creative shift occurs when there is a modification of the heuristics, but not of the hard core Stolper and Samuelson modified the demi-core, but m a way that was consistent with the heuristics This cannot be considered as a shift which results m the creation of a whole new theory, in the way that Krugman and Lancaster's models were interpreted in chapter 5 4 The Stolper/Samuelson modification did not result in the generation of a whole new theory, but it allowed for further progress in the old theory One could argue, in the light of this, that creative shifts may also occur in a programme, where the hard core is changed m a way that allows for the incorporation of more novel facts, and in a way that is consistent with the existing heuristics of the programme

Stolper and Samuelson's paper noted the tendency identified by Ohlin (1933) towards factor price equalisation, where international trade causes inter-industry specialisation across countries according to relative factor abundance The proof for complete factor price equalisation was to dominate much of the static theories during the 'Samuelson shift' In 1933, Lerner showed that under a competitive tradmg equilibrium there would be relative and absolute factor price equalisation, although this proof was not published until 1952 Lerner's proof was in the form of a geometric analysis Under the assumption of identical production functions within industries, factor price equalisation will, by definition, occur where the production frontiers of the trading partners are tangential to the international price line

Samuelson (1948,1949) gave a mathematical proof for relative and absolute factor price equalisation in the two-factor/two-good model Specialisation according to factor abundance causes demand for the abundant factor to increase relatively more than demand for the scarce factor The price of the abundant factor rises, while the price of the scarce factor falls In the trading partner, the same factor price changes are observed These factor price movements continue until factor prices are equalised in both countries At this point, the pre-trade comparative cost advantages have disappeared for each country and the volume of international trade stops expanding Underlying this proof are a substantial number of assumptions Factor markets must be perfectly competitive, and factors fully mobile within each country Allied to this is the assumption that both labour and capital earn the same in both industries A globally univalent production function is assumed, that is, an assumption that there is a unique relationship between the price of factors and the price of goods Specifically, the factor price must be equal to the marginal productivity of the factor times the price of the good produced There is, in addition, the assumption of homogeneous and identical production functions across countries in respect of each industry Each of these assumptions made it difficult to render the factor price equalisation hypothesis empirically testable

The main issue in this respect was the proof of whether the univalent relationship between the price of factors and the price of goods held irrespective of factor prices, and in a multi-factor/multi-good model Pearce questioned Samuelson's (1949) proof that production functions are invertible irrespective of factor prices (Pearce 1951-2) While Pearce accepted Samuelson's proof of the univalent relationship between goods prices and factor prices in the two-factor/two-good model, he argued that the same relationship could not be said to hold in a multi-factor/multi-good model (Pearce 1951-52 114) In a mathematical appendix to Pearce's paper, James and Pearce showed that in a three-factor/three-good model, factor prices could not be held as uniquely determinable from traded goods prices (James and Pearce 1951-52 119-120) Pearce went on to show that where this univalent relationship did not hold, then it would be possible for the common international price to be a result of different factor prices in trading countries Samuelson in his comment on Pearce (1951-52), conceded the problem with the assumption of identical production functions which Pearce highlighted (Samuelson 1951-52) In his 1953 paper, Samuelson further considered the proof provided by James and Pearce In James and Pearce's proof, they argued that if the relationship between the factor price and the goods price, $p = f(w)$, was univalent, then the Jacobian, df/dw, would have to remain invertible (ie must not change sign) (James and Pearce 1951-52 120) In 1953, Samuelson put forward what he believed was an extension of the univalence conditions to the multi-factor/multi-good case (Samuelson 1953 16) Both Laursen (1952) and

McKenzie (1955) avoided this issue in their analysis of factor price equalisation, by choosing specific forms of analysis Laursen analysed factor price equalisation via log-linear Cobb-Douglas production functions, while McKenzie used activity analysis or linear programming Both of these analyses *assume* univalence in the first instance Given this, McKenzie's 'proof' of univalence (McKenzie 1955 243), essentially puts forward as a novel fact, that which it uses as an initial assumption

The notion of univalence seems to have pre-occupied mathematicians as much as trade theorists (Chipman 1966 30) Nikaido showed Samuelson's proof of univalence in the multi-model to be false (Chipman 1966 30) Some considerable developments in mathematics were necessary to show the conditions under which the Jacobian could be held invertible in the multi-factor/multi-good model Without the specification of these conditions for uniqueness, the factor price equalisation theorem remained unproven in the multi-factor/multi-good case Moreover, the Heckscher/Ohlin theorem could not be extended to the general case either. Thus, it was not until Gale and Nikaido (1965), when the conditions for the invertibility of the Jacobian were isolated, that the HOS predictions could be extended to the multi-factor/multi-good model Where the function p = f(w) is globally univalent, there are no factor intensity reversals and the definition of industry underlying the HOS theories can be upheld (It should be noted that Chipman did raise a doubt as to the validity of the Gale/Nikaido proof (Chipman 1966 39)) As can be seen by this discussion, the debate over the proofs for factor price equalisation was a complex one Yet, it failed to raise any novel predictions

The main novel predictions of the static theory at this time were the Stolper/Samuelson theorem 1941, factor price equalisation in the two-factor/two-good model (Samuelson 1948, 1949), factor price equalisation in the multi-factor/multi-good model (Samuelson 1953), Rybczynski's theorem 1955 The empirical testing of these predictions was not an issue, and indeed, they were formalised in such a way as to render them untestable "the whole discussion is, for better or worse, a supreme example of nonoperational theorizing" (Caves 1960 92) Blaug pointed out the paradox in Samuelson's role in the development of a nontestable theory of international trade, given his methodological suggestion that economists focus on the generation of operationally meaningful statements (Blaug 1992 188)

International trade theorists did consider the disturbing causes to the predictions of the static theory They considered, for example, the effect of unequal numbers of factors/goods, variable factor supplies, specialisation and increasing returns to scale Indeed, Johnson argued that the whole purpose of the factor price equalisation analysis was to elucidate disturbing causes

> The Samuelson factor price equalisation theorem is indeed a splendid proposition, but its chief practical relevance is to direct attention -by the indirect process of theoretical abstraction- to the many reasons why factor

> prices, and more still, incomes per head, are unlikely to be equalised in the real world as we know it (Johnson 1970 19)

Mill stressed the importance of elucidating disturbing causes to the predictions of abstract economic theory (Mill 1874 330, see section 4 3) However, in Mill's methodology, disturbing causes are revealed by comparing abstract predictions with real world pheonomena, not by the process of 'theoretical abstraction' outlined by Johnson

As Johnson argued, the static theory of international trade was not, at this time, "much concerned with the empirical problems of predicting or prescribing which goods will or should be traded by particular countries, or of specifying the characteristics of such goods" (Johnson 1970 10) The agenda of the static theory as it developed during the 'Samuelson shift' of the 1950s and 1960s, was a very different one to that of the dynamic theory of international trade Most textbooks implicitly suggest that a choice was made between the two theories, with the static theory being chosen as the appropriate method of analysing patterns of international trade The next section poses the following question: on what criteria could this choice between the static and the dynamic theories have been based?

7 3 THEORY-CHOICE IN THE INTERNATIONAL TRADE SUB-DISCIPLINE

This section uses the theories of scientific rationality in order to elucidate possible criteria which would have facilitated a choice between the static and the dynamic theories of international trade, as they stood at the end of the 1960s

That falsificationism was not on the methodological agenda of the international trade sub-discipline is evident from the treatment of trade theorists to Leontief's empirical findings on the pattern of U S trade De Marchi (1976) showed that, far from rejecting the factor proportions theorem, trade theorists defended the theorem in the face of Leontief's findings with an *ad hoc* redefinition of 'factor' The Leontief paradox, that U S exports were predominantly labour intensive while the U S was abundant in capital, did not cause the upheaval for the static theory that would be suggested in a Popperian rational reconstruction

De Marchi argued that "the Leontief test, though not perfectly controlled, is probably about as clear an example of a 'crucial experiment' as one is likely to encounter in economics" (de Marchi 1976 113) If it was deemed by the trade theorists of the 1950s as a crucial experiment, then one is forced to conclude that these theorists made a conscious decision not to reject the factor proportions theorem despite the conclusions of the crucial experiment However, given that the reaction to the

Leontıef findıngs was to poınt to problems wıth the procedures employed by Leontıef, ıt seems that theorısts dıd not hold Leontıef's to be a crucıal experıment There was, ın fact, consıderable agreement that the Leontıef results could be overturned wıth the ınclusıon of skılled labour as a component of the U S capıtal endowment (as shown ın the studıes of Kravıs 1956, Kenen 1965, Keesıng 1966) Leontıef, hımself, made several suggestıons as to how hıs data mıght be made more compatıble wıth the factor proportıons predıctıon (Leontıef 1969 126-139)

Crucıal experıments are, for Popper, "experıments whıch could falsıfy and thus elımınate some of the competıng theorıes" (Popper 1972 15) Falsıficatıonısm presupposes some consensus on the part of the scıentıfic communıty as to what constıtutes a crucıal experıment No such consensus exısted ın respect of Leontıef's test, and thıs ıs evıdenced by the extent to whıch the empırıcal work followıng Leontıef was largely concerned wıth reorganısıng the data to fit the factor proportıons theorem

Does a Lakatosıan ratıonal reconstructıon fare any better at provıdıng an explanatıon for the apparent domınance of the statıc theory as the prımary explanatıon for ınternatıonal trade patterns durıng the 1950s and 1960s? MSRP argues that the choıce between theorıes should be made on the crıterıon of empırıcally corroborated novel facts

The static theory, while it may have generated theoretical novel predictions, made little attempt to empirically corroborate these predictions Indeed, the static theory immunised itself from empirical judgement by generating nonoperational hypotheses Its major novel fact was refuted by the Leontief paradox This refutation was overturned by later studies, and this afforded some support for a modified version of the factor proportions theorem There was also some support for the neoclassical, Ricardian model of international trade Empirical investigations by MacDougall (1951,1952), Stern (1962) and Balassa (1963), pointed to the positive relation between labour productivety and comparative advantage There are two points to be made m respect of this evidence Firstly, the Ricardian model, while it *assumed* a link between labour productivity and comparative advantage, did not *explain* this link Thus, in Lakatosian terms, the model uses a novel fact twice, once m its construction and once in its prediction The labour productivity explanation for international trade cannot therefore be considered an empirically corroborated novel fact The second point is that this model does not conflict with the dynamic theory of international trade which is also based on the proposition of comparative advantage If it were to constitute an empirically corroborated novel fact, it would support both theories

The dynamic theory of international trade prompted a significant number of empirical investigations, particularly in respect of Posner's prediction of a link between technological development and the pattern of international trade (for example, Bharadwaj and Bhagwati 1967, Gruber, Mehta and Vernon 1967, Keesing 1966, Bruno 1970, Hall and Johnson 1970, Hufbauer 1970) A number of the theory's novel facts were corroborated A tally of empirical corroborated novel facts would suggest that the dynamic theory of international trade was the more (Lakatosian) progressive at this time A Lakatosian rational reconstruction could, therefore, not explain the perisistence of the static theory of international trade, when a more progressive dynamic theory existed

There is, however, a problem with this conclusion The heuristics of the static theory of international trade did not stress the importance of empirical corroboration Static theorists were not instructed to test their predictions, in the way dynamic theorists were Thus, any methodological judgement between the two theories on empirical grounds is bound to favour the dynamic theory, and to argue therefore that the persistence of the static theory was an irrational move by trade theorists, one that is explained by factors external to any empirically-based rational reconstruction

Vernon proposed that the main reason for the persistence of the static theory is that the "doctrine of comparative advantage and the theory of the international equilibrating process have a simplicity, a strength, and a clarity that are not matched by many branches of economic theory" (Vernon 1970 2) But strength in Vernon's context is not equal to the Lakatosian concept of heuristic strength In Lakatosian terms, heuristic strength or power is the potential ability of a programme to generate *empirically corroborated* novel predictions (Lakatos 1970 155) Economists, however, often mean something else when they talk about the strength of a theory or set of theories It is the strength of a theory *in the abstract*, the ability of a theory to explain in terms of an idealised model of reality This is the first stage in Mill's dual methodology of political economy The second stage is to test these predictions against the real world in order to elucidate the disturbing causes to them It is this second stage that was overlooked within the static theory of international trade during the 'Samuelson shift'

Worrall argued that a choice between theories might be based on their relative internal consistency with their heuristics (Worrall 1978a 63, see section 3 2) There can be no doubt that the static theory was consistent with its neoclassical heuristics, which directed the theory to examine the stability and uniqueness of general equilibrium models of international trade The dynamic theory too, however, was consistent with its heuristic, which directed the theory to empirically test predictions about the pattern of international trade However, the dynamic theory was a collection of methodologically diverse theories, with this single common heuristic It could therefore be argued that the static theory displayed more internal consistency, and was therefore the preferred theory Worrall argued that the heuristic criterion could be used in the case where two theories under consideration had predicted empirically corroborated novel facts to the same extent, that is, where the two theories are empirically equivalent (Worrall 1978a 63) Without this proviso, the heuristic criterion is nothing more than a conventionalist preference for precision, order, clarity, etc The persistence of the static theory can only be explained in these non-empirical terms It is difficult not to agree with Corden's conclusion that the preoccupations of the static theory during its 'Samuelson shift' were nothing more than an "intellectual game" (Corden 1965 31)

A Lakatosian rational reconstruction might explain dominance of the static theory, as adherence to a degenerative theory in the hope that it would eventually undergo a creative shift and begin to generate empirically corroborated novel facts once again As the previous chapter shows, the changes which did occur in the static theory at the end of the late 1970s resulted in the development of a whole new static theory of international trade Is it accurate, however, to argue that the static theory dominated the international trade sub-discipline, in the period before this whole new static theory of international trade emerged?

While in the 1950s there may have been a period where international trade became preoccupied with questions of existence and stability in the two country/two good/two factor model, this preoccupation had diminished greatly by the 1960s The 1960s saw the development of two progressive dynamic theories of international trade, critiques of the methodology of the static theory of international trade, and the development of a new heuristic instructing trade theorists to test their predictions The dominance of the static theory appears only in textbooks of the period The tendency to follow the neoclassical history of economic thought has perhaps led critics to focus on the static theory, rather than on the development of the international trade sub-discipline as a whole Blaug, for example, focused solely on the development of the static theory during the 'Samuelson shift', and not surprisingly, concluded that international trade theory was "prone to the disease of formalism" (Blaug 1992 190) This conclusion perpetuates the myth that the static theory of international trade was the dominant theory of international trade from the 1940s to the end of the 1970s (It is strange that Blaug, given his falsificationist tendencies, did not examine the empirical work on patterns of international trade that occurred during the 1960s and 1970s)

It is only when focusmg on the development of the static theory, that the international trade sub-discipline appears esoteric and unrealistic In fact, the sub-disciplme only allowed neoclassical heuristics to dominate for a relatively short period of time, the 1940s and 1950s By the 1960s, the sub-discipline had reverted to its usual method of using the static theory to explain the basic principles of international trade, while dealing with real world trade patterns and allied policy issues within a dynamic framework The extent to which the empirical evidence on intra-industry trade, gathered during the 1970s, influenced the development of the new theories is examined in the final chapter of this dissertation The conclusion of this final chapter should either corroborate or refute the methodological hypothesis that the new theories of international trade facilitated a convergence of the static and dynamic theories of international trade

CHAPTER EIGHT

THE INFLUENCE OF EMPIRICAL TESTING ON THE DEVELOPMENT OF INTERNATIONAL TRADE THEORY - THE CASE OF INTRA-INDUSTRY TRADE

In the previous chapter, it was argued that a neoclassical rational reconstruction of the history of international trade theory would ignore the shift in methodology towards the empirical which occurred in the international trade sub-discipline in the 1960s, and emphasise instead the development of the neoclassical, static theory of international trade This chapter examines a particular part of that empirical work, namely, empirical investigations into the phenomenon of intra-industry trade

Trade theorists tend to refer to both cross trade as identified by Marshall and Taussig, and the Lovasy/Krugman phenomenon of intra-industry trade, as intra-industry trade Cross trade is not incompatible with the principle of comparative advantage, as explained by the Heckscher/Ohlin factor proportions theorem But it *is* compatible with the principle of comparative advantage Cross trade, as defined by Marshall and by Taussig, is international trade in respect of goods classified within the same industrial group Marshall and Taussig assumed that these goods, while in the same industrial classification, were produced under different production functions It is conceivable, therefore, that one good within an industry could be labour intensive, while another could be capital intensive Under such circumstances, cross trade is compatible with the factor proportions explanation for international trade

Intra-industry trade, as it was defined by Lovasy (1941) and by Krugman (1979), is not reconcilable with the factor proportions theorem Intra-industry trade is trade in varieties of a single product, where there are no differences in the factor requirements for each variety regardless of where they are produced Factor proportions, therefore, cannot explain intra-industry trade Intra-industry trade also runs counter to the principle of comparative advantage Both Lovasy's (1941) and Krugman's (1979) models assume that two countries are identical in every respect Under this condition, no international trade based on comparative advantage would occur However, Lovasy and Krugman both show that intra-industry trade can occur under this assumption Thus, intra-industry trade, as defined by Lovasy and by Krugman, contradicts both the factor proportions theorem and the principle of comparative advantage The failure of many trade theorists to make a clear distinction between these two types of international trade raised an important issue in the methodological debate over whether an alternative theory to the factor proportions explanation for comparative advantage was necessary

In chapter 6 4, it was argued that the new theories of international trade, based on Krugman's model of intra-industry trade, created a shift in the static theory of international trade to the extent that a new static theory of international trade was generated A question was posed as to whether that shift in the static theory was borne out of changes which occurred in the heuristics of the dominating neoclassical programme, or whether the impetus for these new theories came from the empirical evidence on intra-industry trade generated in the 1960s and 1970s This chapter considers whether the empirical investigations facilitated a convergence between the dynamic and the static theories of international trade, and whether the new theories of international trade exemplify such a convergence

8 1 THE MEASUREMENT OF INTRA-INDUSTRY TRADE

Empirical evidence which showed the existence of extensive intra-industry trade was put forward in a number of papers in the 1960s and 1970s They include Dreze (1961), Balassa (1966,1978,1979) Kojima (1964), Grubel (1967,1970), Grubel and Lloyd (1971, 1975), Willmore (1972), Aquino (1978), McAleese (1977, 1978)

Because of the distinction between cross trade and intra-industry drawn above, it is necessary to consider the way in which intra-industry trade has been measured in order to establish whether or not it is intra-industry trade or cross trade which has been measured

The most common index used to measure mtra-industry trade has been (and remains) the measure used by Grubel (1967) and later modified by Grubel and Lloyd (1971)

$GL_i \quad (X_i + M_i) - |X_i - M_i|$ (Grubel and Lloyd 1971 496)

X_i = exports of industry i, M_i = imports of industry i, $(X_i + M_i)$ = the value of total trade, where i = (1, ,n), n being the number of industries at a chosen level of industrial aggregation, $|X_i - M_i|$ = net exports from the industry Grubel and Lloyd derived this measurement from their definition of mtra-industry trade "the value of exports of an 'industry' which is exactly matched by the imports of the same industry" (Grubel and Lloyd 1971 496) Grubel and Lloyd took the 3-digit SITC classification as their definition of industry

While this measure gives a value for intra-industry trade in a given industry, an index is required for comparative industry and country studies

$GL_2 \quad [(X_i + M_i) - |X_i - M_i|]\ 100 / (X_i + M_i)$

(Grubel and Lloyd 1971 496)

This index ranges from 0 to 100 It equals 100 where there is complete intra-industry trade, that is, where $X_i = M_i$ It equals 0 where either X_i or M_i equals 0, that is, where there is complete *inter*-industry trade

Underlying the Grubel/Lloyd definition of intra-industry trade, is an assumption about the extent of intra-industry specialisation across countries The closer that X_i moves to M_i, the greater a country is specialised m the production of goods within the same mdustrial classification as its trading partners If X_i exceeds M_i, then there is still some inter-industry specialisation between a country and ts trading partners Grubel considered the extent of mtra-mdustry specialisation within the EEC, for the years 1955, 1958 and 1963 The proposition of comparative advantage would predict growing specialisation across countries in terms of their national comparative advantage as trade was liberalised On the basis of this prediction, the pattern of international trade should have shown greater export concentration as trade increased among member states (Grubel 1967 377) Grubel compiled variances for the intra-EEC shares of total exports for each member-state, by calculating a trade ratio for each member in respect of each traded industry These ratios were classified according to whether exports were greater than, equal to or less than imports Grubel then calculated the mean and variance of these ratios, for each industry

Grubel argued that if inter-industry specialisation, as predicted by the principle of comparative advantage, was occurring, one would expect to see an increase in the variance of export shares calculated for each member-state (Grubel 1967 377) In contrast, Grubel noted a "profound tendency towards an equalisation of three-digit industry exports and imports among the Common Market countries, evidenced by the decreases of both the means and the variances of the ratios" (Grubel 1967 378) This, Grubel argued, was evidence that *intra*-industry specialisation was occurring The argument that intra-industry specialisation can be inferred from increased intra-industry trade is circular Grubel was, in essence, arguing that a country whose exports and imports are moving closer together within a particular industrial group is engaging in intra-industry specialisation, because (he assumes) intra-industry specialisation causes a country's exports and imports within a particular industry to move closer together The argument is circular because Grubel failed to make any hypothesis about the link between his empirical definition of intra-industry trade, and the notion of intra-industry specialisation It is possible that intra-industry trade is indicative, not of intra-industry specialisation across countries, but rather as indicative of cross-country changes in comparative advantage due, say to changes in technological development across countries

The problem which this theoretical gap creates is considered below in respect of

Aquino's (1978) modification of the Grubel/Lloyd measures of intra-industry trade

Grubel and Lloyd modified further their measure of intra-industry trade, in order to account for variance in the proportions of intra-industry trade across industries Without this modification, the index will overestimate the extent of intra-industry trade as a proportion of total trade The GL2 index must be weighted to take account of the relative importance of industries to the total trade balance

$$GL_3 \quad [E^n(X_i + M_i) - E^n|X_i - M_i| \;/\; E^n(X_i + M_i)] \quad 100$$

(Grubel and Lloyd 1971 497)

Grubel and Lloyd pointed to a problem with this modified version of GL_3, if a country's total trade is imbalanced (Grubel and Lloyd 1971 497) The index will underestimate the extent of intra-industry trade when total exports and imports across n industries is not matched Grubel and Lloyd attempted to remove this distortion "by expressing intra-industry trade as a proportion of total commodity export plus import trade less the trade imbalance" (Grubel and Lloyd 1971 497) This has the effect of increasing the proportion of intra-industry trade by a proportion of the size of a total trade deficit in the case of such a deficit, and decreasing it in the case of a trade surplus This is akin to making an estimate of the proportion of intra-industry trade there would be if total trade were balanced The adjusted index is the following

$GL_4 \quad [E_n(X_i - M_i) - E_n|X_i - M_i| \ / \ E_n(X_i + M_i) - |E_nX_i - E_nM_i|] \ 100$

(Grubel and Lloyd 1971 498)

Grubel and Lloyd next turned to the impact of the level of statistical aggregation on their indices They noted that GL_3 will register more intra-industry trade at the higher levels of aggregation, since the denominator, $E_n(X_i + M_i)$, is unaffected by the level of aggregation (Grubel and Lloyd 1971 498) Aggregation has the following effect "aggregation increases the measure of intra-industry trade by a greater amount the greater the extent to which the terms $(X_{ij} - M_{ij})$ at the less aggregated level are of opposite sign" (Grubel and Lloyd 1971 498) The same effect applies, of course, to aggregation across countries, that is, the measure for bilateral intra-industry trade will underestimate the extent of intra-industry trade, compared to the measure for intra-industry trade across a number of countries (Grubel and Lloyd 1971 499)

Grubel and Lloyd used these indices to calculate the extent of intra-industry trade in Australia They found that the extent of intra-industry trade as a proportion of Australia's total trade, was less than proportions found in respect of industrialised countries in other studies (Grubel and Lloyd 1971 499) However, Grubel and Lloyd argued that certain of the industries in which Australia traded, revealed very high levels of intra-industry trade and that this supported the hypothesis that "the phenomenon of intra-industry trade is not restricted to trade among highly industrialised countries" (Grubel and Lloyd 1971 499)

Grubel and Lloyd noted that the level of intra-industry trade declined rapidly as the degree of aggregation declined (Grubel and Lloyd 1971 500) This shows a serious downside to their measures of intra-industry trade, since it may be the case that intra-industry trade is merely a product of statistical aggregation "It is quite possible for a low level of intra-commodity trade among several 'industries', reflecting the fact the country exports and does not import the products of some of these industries while it imports and does not export the others, to become a high level of intra-industry trade when these industries are aggregated" (Grubel and Lloyd 1971 500)

In order to show that intra-industry persisted at the lower levels of aggregation, Grubel and Lloyd undertook correlations between the 2-digit measure and the 3-digit measure, and between the 3-digit measure and the 5-digit measure, of intra-industry trade in the industries they were investigating They found high correlations, which imply that the extent of intra-industry trade persisted over the different levels of aggregation As they had expected in the light of the problems with the indices which they had noted, Grubel and Lloyd found that "the adjusted averages are generally higher than the unadjusted figures and more so for bilateral than total trade (Grubel and Lloyd 1971 501)

Aquino noted that Grubel and Lloyd made no adjustment to GL_2, for the bias they identified in GL_3 (Aquino 1978 280) The assumption Grubel and Lloyd made, according to Aquino, is that the downward bias in GL_3, is due to the fact that it aggregates the means of GL_2 (Aquino 1978 280) Aquino argued, however, that the bias is just as likely to appear at the level of a single commodity, as it is at the highest level of aggregation (Aquino 1978 280) In other words, the trade imbalance of an individual industry is as likely to impact on the measure of intra-industry trade within that industry, as the total trade imbalance would have on the calculation of intra-industry trade across all industries

Aquino introduced an alternative index, Q_{ij}, which estimates the proportion of intra-industry trade, had there been no total trade imbalance

$$Q_{ij} \quad [E(X_{ij} + M_{ij}) - E_i|X_{ij} - M_{ij}|]/ [E(X_{ij}^e + M_{ij}^e)] \;\; 100$$

where $X_{ij}^e = X_{ij} \quad [1/2\ E_i(X_{ij} + M_{ij})]/ EX_{ij}$

and $M_{ij}^e = M_{ij} \quad [1/2\ E_i(X_{ij} + M_{ij})]/ EM_{ij}$

(Aquino 1978 280)

Aquino conceded that it is likely that all industries will be equi-proportionately affected by the total trade imbalance, but he argued that "in the absense of any information about inter-commodity differences in the strength of the imbalancing effect the best one can do is then to assume that it is equi-proportional in all industries and equal to the overall imbalance" (Aquino 1978 280)

Despite the rather unrealistic assumption used in the construction of this index, Aquino argued that it has an important advantage over over GL_3 and GL_4 In order to show this advantage, Aquino considered the case of two countries one with an equal ratio of exports to imports in its three industries, and one with complete intra-industry trade in two industries (ie exports = imports) and substantially more exports than imports in the third industry Both countries have the same absolute total trade figures, with both having an overall trade surplus

	Case I		Case II	
	X_{ij}	M_{ij}	X_{ij}	M_{ij}
Chemicals	20	10	10	10
Textiles	10	5	40	5
Machinery	40	20	20	20
	--	--	--	--
Total	70	35	70	35

(Aquino 1978 281)

With regard to case 1, it would appear that there is both intra-industry and inter-industry trade, in that exports exceed imports in each of the three industries The GL_3 index is equal to 66 66, which confirms this view However, as Grubel and Lloyd pointed out, this index is likely to contain a downward bias where total trade is imbalanced The GL_4 index is equal to 100, implying that there is in fact complete intra-industry trade in case 1 when the impact of the total trade imbalance is taken into consideration

In case 1, the ratio of exports to imports is the same for all industries In case 2, however, the ratios are not all the same If one accepts the Grubel and Lloyd argument that, where the value of exports is equal to the value of imports, there is complete intra-industry specialisation, then there is greater intra-industry specialisation in case 2 Note, however, that there is no more intra-industry *trade* than in case 1 Thus, it is not surprising that the GL indices are the same for both cases The fact that the indices are equalised across the two cases shows the mistake in inferring, as Grubel (1967) did, the existence of intra-industry trade specialisation from intra-mdustry trade figures alone

Aquino made a different point about the equalisation of the indices in respect of the two cases He argued that in case 2, there is "a clear tendency to specialize in textiles with respect to chemicals and machinery, in this case only a proportion of total trade is intra-industry trade" (Aquino 1978 281) In other words, the fact that the ratios of exports to imports is no longer equal indicates, for Aquino, the development of inter-industry specialisation He argues that GL_4 *over*-estimates the level of intra-industry trade in the second case, because for Aquino, this change in the relative trade ratios must indicate more inter-industry trade than in case 1 The value of the Q_{ij} index "would be in fact equal to 100 in case 1 (no inter-industry specialization hence all trade is intra-industry trade) and to 57 1 in case 2 (42 9% of trade stems from inter-industry specialization and the remaining 57 1% is intra-industry trade)" (Aquino 1978 282) Thus, Aquino argued that while GL_3 is a downward biased measure, GL_4 is an upward biased measure where total trade is imbalanced (Aquino 1978 281n) He implied that an index should reflect the fact that export/import ratios can differ across industries, since, this, according to Aquino, is an indicator of inter-industry specialisation

However, according to the Grubel/Lloyd definition of intra-industry specialisation, there is more *intra*-industry specialisation in case 2, in that there are more industries in which the value of exports is exactly offset by the value of imports Thus, Grubel and Lloyd's indices do not reflect the very thing they purport to be measuring, namely the extent of intra-industry specialisation

Thıs sectıon has focused on the Grubel/Lloyd ıneasure of ıntra-ındustry trade because they were and contınue to be extensıvely used (In respect of Irısh ıntra-mdustry trade, the Grubel/Lloyd measures were last used ın NESC 1989) Yet, these measures are not unproblematıc Thıs sectıon ıdentıfıes a problem wıth the Grubel/Lloyd ındıces, ın respect of the lınk drawn between ıntra-mdustry trade and mtra-mdustry specıalısatıon The followıng sectıon consıders these ındıces ın respect of the dıstınctıon drawn between ıntra-mdustry trade and cross trade The questıon to be answered ıs the followıng do the Grubel/Lloyd ındıces provıde a measure of a pattern of ınternatıonal trade whıch cannot be explaıned by the factor proportıons theorem, or even by the prıncıple of comparatıve advantage?

8 2 INTRA-INDUSTRY TRADE - A STATISTICAL ABBERATION?

In 1975, Grubel and Lloyd published the first textbook on intra-industry trade As in their earlier paper, they concluded that the factor proportions theorem could only explain certain types of intra-industry trade for example, intra-industry trade which arises out of different transportation costs across countries and within countries, intra-industry trade due to seasonality of production in agricultural products, and intra-industry trade which is essentially the re-export of slightly modified products (Grubel and Lloyd 1975 71-82) Specification of these different types of intra-industry trade indicates that intra-industry trade, for Grubel and Lloyd, has a much wider definition than for Lovasy and for Krugman

Grubel and Lloyd identified three different types of intra-industry trade, where the traded goods are close substitutes in either production or consumption The first was trade in goods which are close substitutes in consumption but not in production This is the Marshall/Taussig cross trade Grubel and Lloyd pointed out that this type of intra-industry trade is compatible with the principle of comparative advantage "Input requirements for the production of different types of furniture (wood and steel) are so different that the principle of comparative advantage can be found importing and exporting simultaneously two products within the same group" (Grubel and Lloyd 1975 87) They stressed that in this case, "the intra-industry trade phenomenon is simply the result of statistical aggregation" (Grubel and Lloyd 1975 87) While Grubel and Lloyd did not expressly say so, it is clear that this type of intra-industry trade is compatible with a factor proportions explanation of intra-industry trade if the industrial classification is narrowed Where the industrial classification is narrowed, this type of trade pattern becomes inter-industry trade

A second type of intra-industry trade highlighted by Grubel and Lloyd, is trade in goods which have "rather similar input requirements but low substitutability in use" (Grubel and Lloyd 1975 86) They argued that in this case, intra-industry trade arises out of "technical peculiarities" such as the manufacture of joint products (Grubel and Lloyd 1975 88) This type of intra-industry trade, they argued, can be compatible with the principle of comparative advantage and with factor proportions (Grubel and Lloyd 1975 88)

Finally, Grubel and Lloyd identified intra-industry trade in respect of goods with similar production functions and high substitutability in consumption This is the Lovasy/Krugman definition of intra-industry trade, which is incompatible with both the principle of comparative advantage and the factor proportions theorem Grubel and Lloyd argued that this type of intra-industry trade "can be explained by relaxing either the assumption that production functions are identical across countries or the assumption that there are no economies of scale" (Grubel and Lloyd 1975 89) However, the relaxation of the assumption of identical production functions dilutes this type of intra-industry trade into the first type of intra-industry trade identified by Grubel and Lloyd In other words, the Lovasy/Krugman intra-industry trade becomes the cross trade of Marshall and Taussig, and is compatible with both the principle of comparative advantage and the factor proportions theorem Removmg the assumption that there are no economies of scale in respect of the varieties of the good does, on the other hand, allow for international trade without the existence of pre-trade comparative cost differences, as both Lovasy (1941) and Krugman (1979) showed

From a methodological point of view, the type of intra-industry trade which Grubel and Lloyd measured, matters considerably Some of their definitions were compatible with the principle of comparative advantage and, indeed, with a factor proportions explanation of that comparative advantage These types of intra-industry trade do not, therefore, warrant a new theory of mtra-industry trade They are fully compatible with the static theory of international trade as it stood before the publication of Krugman (1979) The third type of intra-industry trade identified by Grubel and Lloyd, that which corresponds to the Lovasy/Krugman definition, is *not* compatible with the principle of comparative advantage and the factor proportions theorem This third type of intra-mdustry does necessitate an alternative theory of trade

This distinction between different types of intra-industry trade, and their methodological significance did not go unnoticed by those theorists engaged in empirical research into mtra-mdustry trade In the same year as Grubel and Lloyd's textbook appeared, Finger published his critique of Grubel and Lloyd's (1971) paper Finger argued forcefully that, "this paper does not attempt to contribute to the intra-industry trade literature Rather it argues that this literature is valueless" (Finger 1975 581)

Finger argued that what Grubel and Lloyd were measuring, in their indices, was not intra-industry trade, but rather "trade overlap" (Finger 1975 581) 'Trade overlap' as defined by Finger corresponds to the Marshall/Taussig phenomenon of cross trade It is intra-industry trade which is compatible with the principle of comparative advantage and the factor proportions theorem, if the industry is reclassified at a lower level of aggregation It is trade which exists, merely as a result of statistical aggregation (For the sake of continuity, I shall continue to use the term cross trade to refer to intra-industry trade which is compatible with comparative advantage and factor proportions, and intra-industry trade to refer to trade which is not compatible with these propositions)

Finger's argument was that, while Grubel and Lloyd might have made the methodological distinction between cross trade and intra-industry trade in their general discussion of the concept, this distinction did not carry through to their statistical investigation of the phenomenon The argument centred on Grubel and Lloyd's (1971) contention that the 3-digit industrial classification corresponds most closely to the concept of industry as used in the static theory Finger argued that in this industrial classification, factor proportions can vary as much within each industry as between them (Finger 1975 586) Finger held, therefore, that the international trade which Grubel and Lloyd identified in their indices could be fully explained by the factor proportions theorem

Finger argued that, in order to show that the international trade which they identified was incompatible with a factor proportions explanation, Grubel and Lloyd "must show not only that trade overlap exists at a given level of aggregation, but also that input requirements do not vary substantially within commodity groups at that level of aggregation" (Finger 1975 584)

Finger argued for the necessity of a clear definition of intra-industry trade

> if the formulation and testing of a 'theory of international trade' is to add to our knowledge of the international economy, it must focus on separating those elements of economic theory which are consistent with intra-industry trade from those which are not, so that the extent of trade overlap constitutes empirical evidence applicable to separating valid propositions from invalid ones (Finger 1975 587)

The thrust of Finger's argument is that intra-industry trade, in the Lovasy/Krugman sense, had to be proven to exist as an empirical phenomenon, before it could be taken as an anomaly to the factor proportions theorem, and a warrant for a new theory of international trade

Despite the methodological significance of his argument, Finger's critique did not produce much of a rethink on the Grubel/Lloyd indices and the phenomenon they measured Trade theorists continued to use the Grubel/Lloyd indices to measure intra-industry trade Grubel and Lloyd, themselves, recognised the problem that aggregation poses, and investigated the extent to which the amount of intra-industry trade they calculated at the 3-digit level was reduced at lower levels of industrial aggregation (Grubel and Lloyd 1971 500) This was obviously not conclusive evidence, for Finger, that intra-industry trade exists The extent of aggregation, even at the 5-digit level, still did not, for Finger, preclude an explanation of intra-industry trade based on different factor requirements among the products in the 5-digit classification

By the late 1970s, empirical investigation had turned to an assessment of the extent to which cross trade, as identified by Grubel/Lloyd indices, was compatible with the predictions of the dynamic theory of international trade put forward by Linder, Posner, and Vernon Significant tests in this regard were presented at a conference on intra-industry trade held at Kiel University in 1978 Some of the papers presented raised the issues which Finger had made in respect of the Grubel/Lloyd indices

Gray distinguished between intra-industry trade and trade which occurs as a result of "categorical aggregation" (Gray 1978 87) This latter was international trade in products categorised in the same industry, but with different production functions (Gray 1978 88) Gray surveyed the statistical studies of intra-industry trade and his conclusions are similar to those of Finger none "can withstand the insistence of critics that the definition of an industry be limited to production units producing completely identical goods in different nations" (Gray 1978 92) Gray attempted to re-focus trade theorists on what he saw as the central issue "to attempt to confirm or refute the existence of intra-industry trade in quantities sufficient to warrant analytic concern with its causes" (Gray 1978 98)

The most interesting part of Gray's paper, from a methodological point of view, is appendix B, in which Gray attempted "to set limits to an industry so that the concept becomes operational" (Gray 1978 107) The derivation of an 'operationally meaningful' and hence measurable definition of industry is central to Finger's argument Gray redefined industry in the following way "An industry comprises those goods which use generally applicable inputs in similar proportions in the absense of product-specific inputs" (Gray 1978 107)

$$Q_{ij} = f_{ij}\, s_{ij}\, (K_{ij}, H_{ij}, L_{ij}, PSI_1, \quad , PSI_n)$$

(Gray 1978 107)

In the above, j denotes subvarieties of the good i PSI are product specific inputs None of these product specific inputs are vital to the production of the j subvarieties The product could be made just using K, H and L S_{ij} denotes scale properties

At given factor prices and at a certain scale of production, "there will be a distribution of unit variable costs of production of the j subcategories around some mean" (Gray 1978 108) This gave Gray the following definition of an industry "an industry is defined as comprising those j^* sub-categories of the ith good which fall within some arbitrary range (say + 5 percent) around the mean of the distribution, when scale and the prices of generally applicable inputs are determined" (Gray 1978 108)

From this definition of industry, Gray defined intra-industry trade thus "when a nation simultaneously exports and imports some of the j^* subcategories of the ith industry" (Gray 1978 108) Thus, in Gray's definition, the production functions of these traded j subcategories are identical in respect of the general factors, K, H and L, where the product specific inputs do not have an 'overwhelming' impact on the cost of the jth subvariety, nor on the extent of its differentiation (Gray 1978 108) Gray's definition of intra-industry trade, under his definition of industry, precludes not only a factor proportions explanation of intra-industry trade, but also a comparative advantage explanation Within the j^* subcategory, the production function is taken to be identical between firms Any international trade of varieties included in the j^*

subcategory is intra-industry trade in the Lovasy/Krugman sense This trade occurs without the existence of any pre-trade comparative cost differences between countries

Gray's analysis would appear to be a positive step in attempting to derive tests which would show intra-industry trade rather than cross trade Yet his paper prompted the following comment from Hesse "I cannot understand in what way the question 'What is an industry' can be at all meaningful Our task as economists is not to develop a generally valid definition of an industry, but to explain reality" (Hesse 1978 112) Hesse argued that the definition of an industry is moot, where what is being considered is the relative impact of price differentials and consumer preferences on the pattern of international trade

In this case, Hesse argued, industries should be determined by consumer demand functions, rather than by production functions (Hesse 1978 112) This implies that the appropriate classification of industries is according to the elasticity of substitution in demand, not in production (In fact, a model of international trade based on elasticity of substitution in demand had already been postulated by Armington (1969) The Armington model was expanded on by Lloyd (1978), a paper presented at the same conference) This argument by Hesse sidestepped the main point raised by Gray, and earlier by Finger (1975), which was that existence of intra-industry trade cannot be taken as an anomaly to the factor proportions theorem, if the industrial classification used in the empirical test does not correspond to the concept of industry employed in the factor proportions theorem

Finger and DeRosa attempted to reinstate the factor proportions theorem through a series of regressions (Finger and DeRosa 1978) They took labour, physical capital, skill ratio, and the rate of product turnover as independent variables set against US cross trade as the dependent variable The inclusion of the skill ratio in their analysis was on the basis of the argument put forward in Larson (1978), that large-scale production techniques can be operated by relatively unskilled labour, whereas small-scale production techniques involve frequent adjustment as product varieties change thereby involving more skilled labour which is classified as capital

The prediction from this analysis was that labour intensive countries are likely to export the more standardised versions of products, whereas (human) capital intensive countries are more likely to export specialised versions of products Finger and DeRosa found only insignificant correlations between the extent of cross trade and their independent variables (Finger and DeRosa 1978 221) Because of this, they included cross trade as an independent variable, and used as a dependent variable "our measure of revealed comparative advantage the share of U S exports in the exports of fourteen major industrial countries to the world (less the U S)" (Fmger and DeRosa 1978 224)

Their results were significantly different to their first set of correlations They found the following "human capital has a consistently positive effect on U S exports basic labor services are consistently negatively correlated with U S export performance the Leontief paradox continues to hold for U S trade U S export advantage appears stronger for those manufactures which are characterized by greater product turnover [w]here a large proportion of trade is overlapped, the revealed comparative advantage of U S manufactures m international trade is also above average" (Finger and DeRosa 1978 224-225)

Finger and DeRosa considered some possible explanations for the high correlations between cross trade and the extent of U S revealed comparative advantage They considered Vernon's theory in the light of the high correlation between the rate of product turnover and revealed comparative advantage They argued that while the product life cycle theory can explain why the U S would have a comparative advantage in varieties of products which are non-standardised, the product life cycle theory would also predict a high correlation between cross trade and the rate of product turnover This second prediction was not borne out by their empirical results (Finger and DeRosa 1978 226-227)

For an alternative explanation, Fmger and DeRosa turned to Larson's theory described above As in the product life cycle theory, this theory predicts that large, human capital intensive countries will tend to export specialised varieties, and import standardised varieties from basic labour intensive countries Finger and DeRosa drew the following conclusion

> A large industrial country (e g the United States) will, producing only for the home market, achieve economies of scale in more varieties than will a smaller industrial country This suggests that in those industries with a relatively large number of 'fringe' product varieties, trade will be overlapped and production techniques will be skill intensive (Finger and DeRosa 1978 228)

This argument is supported by the significant correlation between cross trade and revealed comparative advantage, and also between cross trade and skill intensity which Finger and DeRosa found for the U S (Finger and DeRosa 1978 228) Finger and DeRosa's evidence showed that the factor proportions theorem was still a valid and important factor in the explanation of cross trade, and that the extent of product differentiation, while important in the explanation of U S comparative advantage, may not be that important as an explanation of why countries import and export within the same industrial classification Finger and DeRosa's findings in support of the factor proportions theorem did not deter trade theorists from attempting to derive an alternative theory of trade which would allow for the phenomenon of intra-industry trade

It was not Krugman's mtentıon to abandon the HOS theorıes altogether, although he dıd argue that theır sıgnıficance had been serıously undermıned "The evıdence on mtra-mdustry trade does more than downgrade conventıonal factor proportıons theory ıt provıdes consıderable posıtıve support to one partıcular alternatıve theory whıch combınes factor proportıons wıth economıes of scale and dıfferentıated products" (Krugman 1978 13) Krugman argued that "the emphasıs on factor proportıons ın theoretıcal trade lıterature ıs, of course, not the result of an empırıcal judgement It ıs, ınstead, a matter of followıng the lıne of least mathematıcal resıstence" (Krugman 1978 14) "Followıng the lıne of least mathematıcal resıstence" ıs exactly what trade theorısts were dırected to do by the neoclassıcal heurıstıcs domınatıng the ınternatıonal trade sub-dıscıplme It ıs ıronıc that Krugman should have chosen to crıtıcıse the work done on the factor proportıons theorm on these grounds, sınce, when Fınger's argument ıs taken ınto account, ıt becomes clear that the theory of mtra-mdustry trade as ıt was later defıned by Krugman (1979) was not based on empırıcal consıderatıons eıther

Thıs sectıon shows that the dıscussıons on the phenomenon of mtra-mdustry trade were not cohesıve There were fundamental dısagreements over the defınıtıon of mtra-mdustry trade, and how to measure the phenomenon There was debate as to whether the empırıcal evıdence for mtra-mdustry trade was anomalous to the factor proportıons theorem There was debate as to whether the empırıcal evıdence provıded corroborations of the dynamic theories of international trade Yet by the early 1980s, a new international trade sub-discipline had been established, which not only "downgraded" the HOS theories but also removed the Ricardian principle of comparative advantage as the sole explanation for patterns of international trade The question addressed in the final section of this chapter is the extent to which this new sub-discipline was actually borne out of the empirical investigations which preceded it

8 3 RATIONAL RECONSTRUCTIONS THE IMPACT OF EMPIRICAL EVIDENCE ON THE DEVELOPMENT OF A THEORY OF INTRA-INDUSTRY TRADE

The intra-industry trade identified first by Lovasy (1941) and later by Krugman (1979), cannot be explained by the factor proportions theorem Nor, indeed, can it be explained by the principle of comparative advantage, since Lovasy and Krugman showed that this type of international trade would arise where there are no pre-trade comparative cost advantages across countries When faced with empirical evidence for intra-industry trade, the appropriate falsificationist response would have been to reject the factor proportions theorem, and the demi-core principle of comparative advantage, and formulate an alternative conjecture to explain international trade patterns

Superficially at least, this is what trade theorists appear to have done During the 1970s, empirical evidence identifying intra-industry trade was accumulated, and in the late 1970s/early 1980s, an alternative model of international trade was proposed by Krugman (1979) The superficiality of this falsificationist rational reconstruction highlights a significant problem with the methodology of historiographic research programmes In using the history of a science in a quasi-empirical way in order to evaluate methodologies, MHRP presupposes that "a certain method M was used and was responsible for the success" that is identified by the scientific elite (Radnitzky

1976 517, see chapter 3 2 for a review of the debate among philosophers as to the validity of MHRP as a meta-methodological framework) In other words, MHRP assumes that if the rational reconstruction accurately predicts actual historical events, then the theory of scientific rationality underlying that rational reconstruction gives methodological rules which are followed by scientists in practice However, the above argument in respect of a falsificationist reconstruction of the development of international trade theory in the late 1970s, shows that a rational reconstruction may accurately predict actual historical events without explaining the development of those same historical events This outcome shows the danger in employing MHRP as a way of choosing between methodologies This is the danger which Koertge discussed, the possibility that history would become distorted so that it would fit any theory of scientific rationality (Koertge 1978 361) A closer examination of the historical events highlights the extent to which a falsificationist rational reconstruction fails to explain these developments in international trade theory

An investigation reveals that the factor proportions theorem was not in fact rejected by trade theorists, rather the theorem was relegated to an explanation of only some international trade patterns Moreover, the empirical evidence on intra-industry trade was inconclusive The previous section shows that trade theorists were not measuring intra-industry trade, as identified by Lovasy and Krugman, but rather cross trade Cross trade *is* compatible with the principle of comparative advantage and with the factor proportions theorem

How, then, can a falsificationist methodology explain the development of an alternative theory of international trade? One could argue that trade theorists believed that the tests they developed during the 1970s, *did* provide empirical corroboration for intra-industry trade Given this belief, the development of an alternative theory of international trade is warranted by the falsificationist methodology However, the papers of Finger (1975), Finger and DeRosa (1978), and Gray (1979) were prominently published, and serve as evidence that trade theorists were aware of the methodological problems with their data on intra-industry trade Trade theorists knew their tests were inconclusive This suggests that it cannot be considered Popperian rational for trade theorists to have developed an alternative theory of international trade on the basis of their empirical evidence on intra-industry trade In terms of a falsificationist theory of scientific rationality, it would not have been rational for trade theorists to base a theory of intra-industry trade on this empirical evidence, because this empirical evidence did not falsify the propositions of the static theory of international trade based on factor proportions While a falsificationist rational reconstruction *can predict* the development of the theory of intra-industry trade, it *cannot provide a causal explanation* for this development It provides only a rather spurious correlation with the historical facts

Does a Lakatosian rational reconstruction provide an adequate causal explanation for the development of an alternative theory of international trade? Under MSRP, the new theories of international trade should have been born out of a depletion of novel predictions from the older static theory of international trade This depletion would warrant either a creative shift in the static theory, or the generation of a whole new theory which would entail the predictions of the older, static theory as a special case Chapter 6 3 argues that the incorporation of the new theories of international trade into the sub-discipline resulted in the generation of an alternative demi-core to that of the older, static theory of international trade In Lakatosian terms, when a change occurs in the hard (demi-) core, a whole new programme (sub-discipline) is created In order to constitute a progressive move in the development of the science, the new programme should generate, not only the empirically corroborated predictions of the older programme, but also a whole plethora of *new* empirically corroborated predictions

The problem here is that neither the novel predictions of the older static theory of international trade based on factor proportions, nor the novel predictions of the new theories of international trade based on the Lovasy/Krugman definition of intra-industry trade, have been empirically corroborated The new theories *appear* to be more concerned with the prediction of actual trade patterns, but they are really an attempt to account *in a theoretical way* for disturbing causes to the predictions of the static theory of international trade There has been a certain amount of theoretical progress in the shift from the factor proportions research programme to the programme based upon the new theories of international trade, but in MSRP, theoretical progress (ie the identification of theoretical, but not empirically corroborated novel facts) is not sufficient to explain why one programme should take precedence over another (Lakatos 1970 118)

De Marchi (1976) showed how the neoclassical heuristics of the older factor proportions theory of international trade did not direct trade theorists to empirical analysis The incorporation of the new theories of international trade does not break the link between the neoclassical economics programme and the static theory of international trade From a positivist point of view, the impetus for the generation of the new theories cannot rationally have come from the empirical work of the 1970s, since this empirical work did not highlight the existence of intra-industry trade as defined by Krugman (1979) in the first of the new theories' models The empirical work of the 1970s cannot be considered a crucial experiment, an experiment which allowed trade theorists to make an empirically based choice between the older static theory of international trade, and the new theories

Lakatos did argue that "*there are no such things as crucial experiments,* at least not if these are meant to be experiments which can *instantly* overthrow a research programme" (Lakatos 1970 173) However, he went on to say that "scientists, of course, do not always judge heuristic situations correctly" (Lakatos 1970 173) Lakatos cited several cases where what appeared at the time to be crucial experiments were later withdrawn For Lakatos, these examples serve as proof that "instant rationality" such as that suggested by Popperian falsificationism does not exist (Lakatos 1970·174)

One could argue that the apparent interpretion by trade theorists of the empirical investigation into intra-industry trade as a crucial experiment which refuted the older, static theory and justified the development of an alternative, was simply a methodological mistake If so, it is a mistake which shows that "rationality works much slower than most people tend to think, and, even then, fallibly" (Lakatos 1970 174) Lakatos suggested that incorrect judgements as to the nature of empirical investigations are made only by a few members of the scientific community "A rash scientist may *claim* that his experiment defeated a programme, and parts of the scientific community may even, rashly, accept his claim" (Lakatos 1970 173) Yet, surely, for an incorrect judgement to make a difference to the development of a programme or sub-discipline, it must be accepted by the majority of the scientific community? An incorrect judgement that is ignored cannot delay rational decision-making in the way suggested by Lakatos

As was argued above in respect of the Popperian rational reconstruction of the development of the new theories of international trade, the methodological flaws in the empirical analysis of intra-industry trade were publicly exposed by Finger (1975) and were well-known Surely it would be permitting of too much irrationality for Lakatosian standards, to argue that trade theorists *knew* that they were making a methodological mistake and accepted it? It appears, therefore, that, even with the acceptance that methodological mistakes can be made, MSRP cannot provide an adequate explanation for the development of the new theories of international trade Worrall extended Lakatos' arguments on the criteria for choice between research programmes, and argued that, where two research programmes are empirically equivalent, a choice might be made between them on the grounds of their internal consistency (Worrall 1978a 63, see chapter 3 1) The research programme whose hypotheses are derived in a way that is consistent with the positive heuristics of that programme is the preferred programme (Worrall 1978a 63) In this case, however, both research programmes are equally internally consistent, and Worrall's appraoch does not offer a "clear rationale" (Worrall 1978a 63) for the preference of one of the programmes over the other

Both falsificationism and MSRP argue that it is empirical warrants which drive science forward The new theories of international trade are generally accepted as an example of progress in international trade theory by international trade theorists themselves However, this identification of the new theories as progressive cannot be explained by theories of scientific rationality which employ empirical warrants as the criteria for theory/research programme choice As is argued at the end of chapter 3 2, a failure of positivist rational reconstructions forces an examination of what it is that economists do in practice What, then, are the criteria that international trade theorists use, which prompted them to view the new theories of international trade as progressive?

The incorporation of the new theories of international trade into the static theory generated both a heuristic and a demi-core change in the static theory The principle of comparative advantage was relegated to the explanation of only some patterns of international trade Economies of scale and product differentiation were incorporated into the demi-core, in order to explain the patterns of international trade which the principle of comparative advantage could not The change in the heuristics of the static theory was also significant It allowed for the generation of non-*ad hoc* novel predictions from *partial* equilibrium models of international trade However, this heuristic change did not mean that the link between the static theory of international trade and the greater neoclassical programme was broken The new theories of international trade were not *ad hoc* to the neoclassical programme This is because the heuristic change in the static theory of international trade was in response to previous changes in the heuristics of the neoclassical programme While the shift generated by the new theories of international trade may in Lakatosian terms have resulted in a whole new theory, in purely heuristic terms the changes between the older, static theory of international trade and the new theories were slight The new theories of international trade can be interpreted as a creative shift which allowed for the continuity of the neoclassical explanation for international trade by incorporating in a theoretical way, more disturbing causes

But what of the indications that an empirical warrant developed within the international trade sub-discipline during the 1970s? It was argued in chapter 7 3 that the dynamic theory of international trade developed under an empirical heuristic Trade theorists, in the dynamic theory, were instructed to empirically test their hypotheses The empirical investigations of intra-industry trade were not concerned with the *theoretical* concept of intra-industry trade found in the new theories These studies tended to focus on the novel predictions of the dynamic theory as opposed to the novel predictions of the static theory of international trade It could be argued that this empirical work was in fulfilment of the heuristic requirement of the dynamic theory, but was *not* indicative of a shift within the international trade sub-discipline as a whole to evaluate theories on empirical grounds

The conclusion of these arguments is that the new theories of international trade did not stem from the empirical investigations into intra-industry trade which took place in the 1970s The new theories and the empirical investigations were essentially concerned with different phenomena The argument that trade theorists did not recognise this difference is not accepted, given the well-documented evidence to the contrary If trade theorists recognised this difference between the theoretical and the empirical investigations, then it cannot be argued that the new theories of international trade were an empirically rational progression from the empirical investigations of the 1970s Yet, both a falsificationist and a MSRP rational reconstruction arrive at this same conclusion It has been argued above, that the compatibility of these rational reconstructions with the actual historical events is merely a correlation, as opposed to a causal explanation for those events An explanation, it is argued, is to be found in the methodological hypothesis that trade theorists denote progress as inherent in those theories which allow more known facts to be incorporated into the neoclassical framework

Under a rational reconstruction which emphasizes the use of this theoretical criterion, it is argued that the new theories of international trade are simply a creative shift borne out of previous heuristic changes in the neoclassical programme Interpreted as such, the new theories of international trade allowed for the continuity of a neoclassical explanation of patterns of international trade The empirical investigations, on the other hand, were a response to the heuristics of the *dynamic* theory of international trade The division between the static, neoclassical theory of international trade and the dynamic theory, persisted into the early 1980s This argument refutes the methodological hypothesis that the new theories of international trade facilitated a convergence between the static and the dynamic theories of international trade

CHAPTER NINE

CONCLUSION

Part one of this dissertation has been concerned with the influence of three twentieth century philosophies of science, Popperian falsificationism, Kuhnian scientific revolutions, and Lakatosian methodology of scientific research programmes, both on the actual practice of economics and on economic methodology

Popperian falsificationism appeared to provide a set of defined, obective criteria which would ensure progress in economics Economic methodologists examined the development of economics, and in particular econometrics, in order to see whether it progressed in the pattern of conjecture and refutation suggested by Popper This analysis has shown that falsificationism is a difficult procedure for economists to adopt This difficulty is due in part to a lack of consensus among economists as to what constitute adequate tests of their theories There were also practical difficulties in empirical testing arising out of the fact that economic theories tend to be probabilistic hypotheses These problems show that falsificationism has not been applied to economics in any general way, but they do not show that falsificationist criteria, if applied, would fail to generate progress in economics However, there are certain flaws in Popper's theory of scientific progress, principally with the concept of verisimilitude, which suggest that falsificationist criteria might not generate scientific progress

Despite these problems, Popperian falsificationism has continued to exert a considerable influence on economic methodologists, and to a lesser extent on economists in general This preoccupation with Popper has lasted much longer among economic methodologists, than among philosophers of science In the 1960s, philosophers of science began to question the whole notion that an objective set of criteria could be derived which would guarantee progress in science This backlash against orthodox philosophy of science culminated in the publication of Kuhn's *The Structure of Scientific Revolutions* in 1962 Kuhn's theory of science provided a framework whereby the history of a science could be analysed in order to discover the grounds upon which scientists themselves chose between scientific theories Kuhn's philosophy was not an alternative theory of scientific rationality to Popper's Rather, Kuhn's philosophy was based on the notion that "rationality ought to be defined by what is found in the history and practice of science rather than set out formally in advance and imposed upon history" (McMullin 1978 239) A Kuhnian philosophy, therefore, advocates a sociological examination of the history of science However, economic methodologists have tended to apply Kuhnian philosophy to the history of economics as though it were an alternative theory of scientific rationality In other words, they have been principally concerned with identifying paradigms, normal science and scientific revolutions in the history of economics, rather than with reconstructing this history as a sociological study

Because of their 'objectivist' application of Kuhn's philosophy to economics, the failure to identify scientific revolutions in the history of economics has led to a conclusion that economics is not progressive in Kuhnian terms Not surprisingly, then, economic methodologists turned back to positivist philosophies of science, and in particular, Lakatos' MSRP (1970) As with their use of Kuhnian and Popperian philosophies, the concern of economic methodologists has been with the identification of Lakatosian concepts in the history of economics Thus, the present preoccupation in economic methodology is with identifying research programmes, heuristics and hard cores, and most importantly, Lakatosian novel facts, m the history of economics

Economic methodologists have had problems in identifying Lakatosian novel facts in the history of economics What Lakatos defined as novel does not appear to correspond to what economists themselves define as novel In other words, Lakatosian rational reconstructions do not tend to correspond with the reconstructions of economists, just as Popperian rational reconstructions do not tend to correspond with the reconstructions of economists

The method of rational reconstruction has brought economic methodologyonto the meta-methodological level of analysis Both Popper and Lakatos suggested meta-methodological criteria whereby one methodology could be deemed preferable to another Lakatos' methodology of historiographic research programmes (MHRP) involves the comparison of a rational reconstruction of the history of a science with the actual history of a science The assumption is that the 'closer the fit' between the rational reconstruction and the actual history, the greater the likelihood that scientists actually employed the criteria expounded by the particular theory of scientific rationality underlying the rational reconstruction MHRP has been much criticised by philosophers of science who argue that a closeness of fit cannot be assumed to reflect a causal explanation

Have economic methodologists been using this flawed meta-methodological framework to choose between theories of scientific rationality? They have certainly devoted a substamtal amount of time to searching for Popperian, Kuhnian and Lakatosian concepts m economics Has their acceptance of MSRP been because it provides a better closeness of fit than the other two philosophies? It appears not Both Blaug (1976) and Hands (1990) have argued that in several important cases, a Lakatosian rational reconstruction has failed to highlight the examples of progress which economists themselves give However, they do not conclude that the effort in trying to fit Lakatosian rational reconstructions has been a waste of time Rather, they argue that the analysis of the discrepancies between Lakatosian rational reconstructions and the actual history of economics has led to a greater understanding of what it is that economists do It is my contention that due to a failure of rational reconstructions based on positivist philosophies of science to identify what economists themselves hold to be examples of progress in economics, economic methodology has been forced into a Kuhnian type of analysis, an analysis of what economists actually do and of the criteria of progress economists themselves use in order to identify progressive theories

In order to show that this use of the method of rational reconstruction is fruitful, the development of international trade theory is considered in the light of the theories of scientific rationality discussed in part one of the dissertation

Part two begins with an identification of a rational reconstruction of the history of international trade theory by trade theorists themselves This fits in with the Kuhnian argument that the scientists of the present paradigm will present a history of their science which implies that the scientists of past paradigms had the same scientific concerns as those of the present paradigm (Kuhn 1970a 138) This rational reconstruction of the history of international trade theory is found in all textbooks of international trade theory and is in fact the Pure Theory of International Trade In this dissertation, it is refered to as the neoclassical rational reconstruction This is not to suggest that neoclassicism is an alternative theory of scientific rationality akin to falsificationism and MSRP Rather, it is to stress the close link between the Pure Theory and the general equilibrium programme in economics

Part two is essentially an exercise in the comparison of three sets of rational reconstruction of the development of international trade theory those of the positivist philosophies of science, that of the historian, and the rational reconstruction put forward by trade theorists themselves Kuhn argued that each of these rational reconstructions will differ, because the philosopher, the historian and the scientists will identify a different set of "essentials" to form the basis of his rational reconstruction (Kuhn 1977 15) It is argued that a comparison of these distinct sets of rational reconstructions leads to a greater understanding of how progress is defined in international trade theory

Chapters 5 to 8 traced the parallel development of the static theory and the dynamic theory of international trade It is argued that both theories belong to the same sub-discipline because they share a demi-core That demi-core includes the principle of comparative advantage, the proposition that there are mutual gains from international trade, and that free trade increases those gains However, these theories have developed very different methodologies While the static theory of international trade has adhered to a neoclassical heuristic, the dynamic theory has been driven by empirical heuristics Not surprisingly, the neoclassical rational reconstruction commonly found in international trade textbooks focuses on the development of the static theory The neoclassical reconstruction depicts a history of international trade theory which involves only the development of the principle of comparative advantage in a 2x2x1 model of trade, by Ricardo, Mill and Marshall The implication from this reconstruction is that the overriding concerns of 19th century trade theorists in general, were neoclassical concerns A MSRP rational reconstruction, on the other hand, identifies the dominance of the dynamic theory of international trade as the principal mode of analysis during the 19th century, and indeed, until the 1930s There were very few attempts to amalgamate the two theories of trade, perhaps because they were not alternatives, but rather complementary explanations of the impact of international trade

From the 1930s, the static theory of international trade developed under the 'Samuelson shift' to become the dominant mode of analysis This dominance lasted until the late 1950s, and it is during this period that the sub-discipline exhibited signs of what Blaug diagnosed as the "disease of formalism" (Blaug 1992 190) Blaug implied, however, that international trade theory continued to be dominated by excessive formalism until more recently Such a conclusion would be suggested by a neoclassical rational reconstruction which ignores the extent of empirical investigations conducted within the dynamic theory of international trade from the early 1960s A MSRP reconstruction points out the empirically corroborated novel predictions of Linder (1961) and Posner (1961) A MSRP reconstruction would argue that the dynamic theory was progressing during the 1960s, while the static theory of international trade was bereft of empirically corroborated novel facts

Why, then, was the static theory retained? It is argued in chapter 7, that a rational reconstruction which focuses on the extent of internal consistency within the static and dynamic theories of international trade, shows that the static theory did not have the derivation of empirically corroborated novel facts as its heuristic goal It remained consistent with the neoclassical aim of formalising the theory of international trade within its general equilibrium framework Novelty, according to the neoclassical heuristics of the static theory, was not to be found in the derivation of empirically corroborated predictions, but rather in the ability to incorporate more extraneous phenomena into the general equilibrium framework

The final issue dealt with is the status of the new theories of international trade and their relation to the empirical work on intra-industry trade conducted during the 1970s The new theories of international trade are frequently cited as the rejuvenators of international trade theory, as facilitating a convergence of the static and dynamic theories of international trade (An exception would be Baldwin 1992) It is argued in chapter 8 3 that the new theories of international trade, while they focus on intra-industry trade, were not born out of the empirical work carried out in the 1970s, but rather were born out of developments in the neoclassical programme, the heuristics of which drive the static theory of international trade These new theories of international trade do constitute a positive development in the static theory of international trade In fact, they have resulted in the generation of a whole new static theory of international trade They have allowed for more extraneous phenomena to be incorporated into a general equilibrium model of international trade But they do not, in themselves, constitute a convergence of the static and dynamic theories of international trade

Has the international trade sub-discipline been a progressive one? The answer to this question depends on the definition of progress In terms of the Popperian and Lakatosian theories of scientific rationality, the time spent on the theoretical development of the static theory of international trade could have been more fruitfully employed in empirically-oriented investigation However, Koertge argued that, over the life of a research programme, a paradigm, or a sub-discipline, there will be occasions when theoretical considerations take precedence

> Scientists are looking for theoretical systems which are both interesting (i e deep, explanatory, informative, simple) and true But in the course of their search they are sometimes temporarily forced to trade off interest for truth and *vice versa* In a balanced research programme neither factor will be overriding in all situations (Koertge 1978 267)

Hausman has criticised economic methodologists for their failure to see that a large proportion of economic theorising is "a conceptual exploration" (Hausman 1989 115) It is pointless, therefore, to assess them "in terms of some philosophical model of confirmation or falsification" (Hausman 1989 115) It has been argued in this dissertation that the conceptual exploration which occurred within theinternational trade sub-discipline was progressive, *within the terms of its own neoclassical heuristics* A neoclassical rational reconstruction does suggest, however, that the international trade sub-discipline has been imbalanced, that there has been a trade off of empirical considerations for theoretical considerations The comparison of this neoclassical rational reconstruction with a Lakatosian rational reconstruction shows the history of international trade theory to be much more balanced as between empirical and theoretical considerations than the neoclassical reconstruction suggests Blaug's argument that international trade theory has not paid due attention to empirical considerations is mistaken (Blaug 1992 190) International trade theory contains elements which are progressive from the neoclassical point of view, but also contains elements which can be defined as progressive from the objective, positivist perspective

Negishi has argued that "modern [trade] theory has failed to learn from classical theory" (Negishi 1992 229) This failure is not surprising if one considers the amount of the actual history of international trade that is omitted from its neoclassical rational reconstruction The neoclassical rational reconstruction of international trade theory identifies essentials which cause it to ignore or omit part of the history of international trade theory An historian's rational reconstruction includes those elements of international trade theory omitted from the neoclassical rational reconstruction, but without making any judgments as to why international trade theory developed in a particular way A comparison of these rational reconstructions with the reconstructions of positivist philosophies focuses the analysis on definitions of progress used by trade theorists themselves If one accepts Hausman's argument that economists are largely concerned with "conceptual exploration," then it is not surprising that the definitions of progress inherent in the neoclassical reconstruction of international trade theory are very different from those inherent in positivist rational reconstructions of the same theory What is perhaps surprising is that a proportion of international trade theory *is* in fact progressive in terms of MSRP In other words, the definition of progress employed in *certain* theories of international trade is an empirical definition

The next stage in this analysis would be to reconstruct international trade theory from a sociological and a rhetorical point of view, in order to further explore the reasons behind the dominance of neoclassical considerations over empirical considerations in the development of international trade theory from the 1940s until the late 1970s The aim would be to provide a complete history of international trade theory formed from the combination of a number of reconstructions, each identifying a different set of 'essentials ' This history would, it is argued, enable economists to learn much more from the development of international trade theory, than a rational reconstruction based on the current definition of progress in use in international trade theory would do This is because, as is evident from the post-1980 developments in international trade theory, the methodological objectives of economists change over time These changes should have a greater and more fruitful impact on the history of economic thought, than simply another rewriting of the history of economic thought which portrays earlier trade theorists as "having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution m scientific theory and method has made seem scientific" (Kuhn 1970a 138) Similarly, a history which is solely a rational reconstruction based upon positivist philosophies of science is of limited use to the practical economist, given that so much of economics is, as Hausman pointed out, to do with ‘conceptual theorising ’

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