

## INFORMATION TO USERS

This manuscript has been reproduced from the microfilm master. UMI films the text directly from the original or copy submitted. Thus, some thesis and dissertation copies are in typewriter face, while others may be from any type of computer printer.

**The quality of this reproduction is dependent upon the quality of the copy submitted.** Broken or indistinct print, colored or poor quality illustrations and photographs, print bleedthrough, substandard margins, and improper alignment can adversely affect reproduction.

In the unlikely event that the author did not send UMI a complete manuscript and there are missing pages, these will be noted. Also, if unauthorized copyright material had to be removed, a note will indicate the deletion.

Oversize materials (e.g., maps, drawings, charts) are reproduced by sectioning the original, beginning at the upper left-hand corner and continuing from left to right in equal sections with small overlaps. Each original is also photographed in one exposure and is included in reduced form at the back of the book.

Photographs included in the original manuscript have been reproduced xerographically in this copy. Higher quality 6" x 9" black and white photographic prints are available for any photographs or illustrations appearing in this copy for an additional charge. Contact UMI directly to order.

**UMI<sup>®</sup>**

Bell & Howell Information and Learning  
300 North Zeeb Road, Ann Arbor, MI 48106-1346 USA  
800-521-0600





Université d'Ottawa · University of Ottawa



# **The Question of Assumptions in Economics**

by

Craig Eschuk

A thesis

presented to the University of Ottawa

in fulfillment of the

thesis requirements for the degree of

Master of Arts

in

Economics

Ottawa, Ontario, Canada, 1998

©(Craig Eschuk) 1998



National Library  
of Canada

Acquisitions and  
Bibliographic Services

395 Wellington Street  
Ottawa ON K1A 0N4  
Canada

Bibliothèque nationale  
du Canada

Acquisitions et  
services bibliographiques

395, rue Wellington  
Ottawa ON K1A 0N4  
Canada

*Your file Votre référence*

*Our file Notre référence*

The author has granted a non-exclusive licence allowing the National Library of Canada to reproduce, loan, distribute or sell copies of this thesis in microform, paper or electronic formats.

The author retains ownership of the copyright in this thesis. Neither the thesis nor substantial extracts from it may be printed or otherwise reproduced without the author's permission.

L'auteur a accordé une licence non exclusive permettant à la Bibliothèque nationale du Canada de reproduire, prêter, distribuer ou vendre des copies de cette thèse sous la forme de microfiche/film, de reproduction sur papier ou sur format électronique.

L'auteur conserve la propriété du droit d'auteur qui protège cette thèse. Ni la thèse ni des extraits substantiels de celle-ci ne doivent être imprimés ou autrement reproduits sans son autorisation.

0-612-38745-3

Canada

## Abstract

The first chapter provides an overview of some of the main developments in the discussion of unrealistic assumptions in economics since Friedman's (1953) contribution. The subsequent chapters are devoted to clarifying the issue of unrealistic assumptions in economics. It is asserted that posing the issue in terms of the "realism of assumptions" is hampered by severe ambiguities. Rather, we should think of assumptions in terms of whether they are false or not, and ask how false assumptions pose problems to making true claims. After this, a discussion of the essential and often unacknowledged role that deductive arguments play in orthodox economics and in economics in general is given. It is argued that it is difficult to test theories by their empirical implications alone, as is often asserted, by orthodox economists, in defence of the use simplifying assumptions. The importance of deductive arguments points to the necessity of evaluating whether any false/simplifying assumptions used in deductive arguments falsify the conclusion. A false assumption is not necessarily problematic, but this must be evaluated to determine whether this is so. A chapter is spent discussing what is involved in making this evaluation. The thesis ends with a chapter discussing two other important reasons for the refusal of orthodox economists to evaluate consistently the impact of simplifying assumptions on conclusions. These are the ideas that optimizing behaviour is somehow equivalent to self-interested behaviour and the apparently greater predictive power of orthodox economics afforded by the use of certain assumptions.

## **Acknowledgements**

I would like to thank my thesis supervisor, Professor Marc Lavoie. He was most helpful throughout the execution of this thesis.

## Table of Contents

<b>1</b>	<b>INTRODUCTION.....</b>	<b>IV</b>
<b>2</b>	<b>DEVELOPMENTS IN THE DEBATE ON THE REALISM OF ASSUMPTIONS IN ECONOMICS SINCE FRIEDMAN WITH AN EMPHASIS ON RECENT CONTRIBUTIONS.....</b>	<b>6</b>
2.1	FRIEDMAN'S IRRELEVANCE-OF-ASSUMPTIONS THESIS .....	9
2.2	A METAPHYSICAL CORE OF ASSUMPTIONS.....	12
2.3	FURTHER DISTINCTIONS BETWEEN ASSUMPTION TYPES.....	15
2.4	SOME CRITICISMS OF THE IDEA THAT THEORIES ARE AND OUGHT TO BE TESTED BY THEIR EMPIRICAL IMPLICATIONS ALONE.....	19
2.5	HAUSMAN'S INEXACT AND SEPARATE SCIENCE OF ECONOMICS.....	22
2.6	LAWSON'S CRITICAL REALISM.....	25
2.7	MÄKI'S CLARIFICATIONS .....	28
<b>3</b>	<b>A REORIENTATION OF THE ASSUMPTIONS ISSUE .....</b>	<b>31</b>
3.1	THE IMPORTANCE OF TRUTH .....	31
3.2	HOW FALSE ASSUMPTIONS MIGHT MATTER.....	36
3.3	THE IMPORTANCE OF GENERATIVE ASSUMPTIONS FOR VERIFICATION.....	39
<b>4</b>	<b>A CRITICAL APPROACH TO FALSE/SIMPLIFYING ASSUMPTIONS .....</b>	<b>47</b>
4.1	SIMPLIFYING AND FALSE ASSUMPTIONS .....	47
4.2	THE DEMONSTRATION OF CLAIMS THROUGH DEDUCTIVE MODELLING.....	51
4.3	THE METHOD OF ISOLATION .....	54
4.4	THE REVISION OF CLAIMS TO TAKE INTO ACCOUNT FALSE ASSUMPTIONS .....	57
<b>5</b>	<b>SOME REMARKS ON THE METHOD OF ORTHODOX ECONOMICS IN RELATION TO THE QUESTION OF UNREALISTIC ASSUMPTIONS .....</b>	<b>63</b>
5.1	SELF-INTERESTED BEHAVIOUR IMPLIES THE METHOD OF CONSTRAINED OPTIMIZATION ..	64
5.2	SIMPLIFYING ASSUMPTIONS FACILITATE PREDICTION .....	72
<b>6</b>	<b>SUMMARY REMARKS .....</b>	<b>81</b>
<b>7</b>	<b>REFERENCES.....</b>	<b>84</b>

## **1 Introduction**

The aim of this thesis is to clarify the issue of realistic assumptions in economics. The central question here is whether unrealistic assumptions are problematic and, if so, in what way. This, I believe, is the central methodological issue in economics today, for it has a crucial and significant bearing on how we do economics. It divides schools of economic thought, in the sense that different attitudes towards the importance of realistic assumptions are in part constitutive of the kinds of theories and explanations offered by these schools. The post-Keynesian and Institutional schools of thought, for example, distinguish themselves from the neo-classical school by their explicit attempt to base their theories on more realistic assumptions. The difference lies most crucially at the level of assumptions about the decision-making processes of agents, coupled with assumptions about the sort of information available to agents. Standard neo-classical assumptions such as constrained optimization and knowledge of risk are regarded as unrealistic, and rejected because they are thought to lead to erroneous conclusions. In general, Post-Keynesians and Institutionalists pay more attention to whether simplifying assumptions lead to erroneous conclusions.

It is my hope that such an inquiry could serve as a basis for arbitrating between the different schools of thought, insofar as they are based on competing methodological approaches to assumptions. If one methodological approach to assumptions leads to a greater likelihood that economists will arrive at and agree upon truthful claims, then this is to be accepted and the other rejected. Such an inquiry can also yield answers about how we are to deal with the inevitable simplifying assumptions that enter into economic arguments. We must have some way to evaluate whether simplifying assumptions distort the conclusions that we have drawn from them. This is

especially important when the assumptions are used to support a conclusion, and not just instrumental in the derivation of that conclusion.

The second chapter presents an overview of, what I see as, the main contributions to the debate on assumptions since Friedman's (1953) advocacy of the relative unimportance of realistic assumptions in economics. There are three main themes in these contributions. First, what is the role of (realistic) assumptions in the justification of claims? This is the principal concern of Friedman's article. Second, what are the different types of assumptions, and what bearing do these distinctions have on this issue of realistic assumptions? For example, some thinkers want to put certain, fundamental assumptions beyond direct empirical evaluation. These assumptions are only to be rejected if the theory built upon them fails to meet certain criteria. This puts aside any question of them being unrealistic. Third, what is the role of assumptions in orthodox and heterodox economics? This theme overlaps with the first somewhat.

In the third chapter, I attempt to define how we should understand the issue of assumptions in economics. Conceptualizing the issue in terms of whether and in what way assumptions should be realistic has hampered the discussion about the realism of assumptions. Unfortunately, the term "unrealistic" is very ambiguous and has a number of senses. The issue of realistic assumptions should really be understood entirely in terms of whether and how false assumptions are problematic. Furthermore, I will make the argument that what we want to obtain are true claims. We want true theoretical statements. There is an argument that theories and models must necessarily be simplifications and hence false. I argue against this interpretation of the theories.

Another crucial issue is the role of assumptions in the justification of claims. Mainstream economists tend to push aside the problem of false assumptions in economics with the argument that these assumptions do not play a role in the justification of claims. Justification lies, in the final

instance, in empirical testing, and not in any kind of deductive logic. The implication is that deductive derivations are used only for the discovery of claims. I attempt to show why deductive arguments are an important means of justifying claims in mainstream economics. In particular, they are essential in allowing us to make assertions about causality. The empirical testing of claims often offers only a very weak basis for justifying a causal argument.

The fourth chapter discusses the method of evaluating assumptions. First, we need to be able to identify false or simplifying assumptions (these, I regard, as almost synonymous). This is most problematic in regard to assumptions about the decision-making processes of agents. Second, how can we use false assumptions in order to demonstrate deductively the empirical validity of a claim? Clearly, obtaining a warrant that the conclusion of the derivation is independent of the false assumption can only do this. We need to ask the counterfactual question: If we had substituted a true assumption for the false assumption, would we have arrived at the same conclusion? Third, the method of isolation is an important technique in economics to identify the effects of causal factors, and it is often invoked to justify the use of simplifying assumptions. However, we cannot just assume that the isolation has been successful. The supposed effect may only be a function of the simplifying assumption. The isolation may be a false one. This calls for an evaluation of whether the isolation is valid or not. Fourth, sometimes the simplifying assumptions do lead to a false conclusion. Then, the question becomes whether we can revise the conclusion to take into account these simplifying assumptions. This is related to the method of successive approximation. We make a simplifying assumption to derive a conclusion, then we relax the simplifying assumption. I review this method.

In orthodox economics, the assumption that agents are constrained optimizers is fundamental. Sometimes the assumption is relaxed, but the bulk of theoretical work makes this

assumption. Now, if it is true that agents do not actually make the sort of calculations required for optimization, then what justifies the use of this assumption? The fifth chapter takes up this question. One justification for using the method of constrained optimization is to argue that it is a technique for studying the implications of self-interested behaviour (which is seen to be the basis of economic action). Constrained optimization is understood to be implied by self-interest, in some way. There are a number of versions of this implication. One is a strict equivalence between self-interest and constrained optimization, that is, self-interested actors are constrained optimizers. A second, looser version is that self-interested behaviour leads to equilibrium outcomes that are defined by optimization on the part of agents. The idea is that choices that are not the optimizing ones are not stable. A third version is that optimization gives us a sort of central tendency. Actual outcomes are random deviations around this central tendency. After setting forth my argument concerning the importance of the supposed relationship between constrained optimization and self-interest to orthodox economics, I relate this to some of Daniel Hausman's insights into the nature of neo-classical economics.

In the second section of chapter five, I take up a second justification for using the assumption of constrained optimization. This is to argue that constrained optimization facilitates prediction. The alternatives appear, at least, to be not as predictively powerful. I argue that there is some basis for this, but that it can also be an illusion. It can create the illusion, because it can yield more determinate results at the level of theory than competing assumptions about the decision-making powers of agents. These more determinate results at the level of theory make it seem as though the assumption of constrained optimization offers greater predictive ability. However, in reality this is not true. I relate Tony Lawson's ideas on closed and open systems to this, since he, effectively, also articulates the idea that simplifying assumptions can create the illusion of

possessing predictive power.

## **2 Developments in the debate on the realism of assumptions in economics since Friedman with an emphasis on recent contributions.**

Before reviewing these developments it is helpful to overview briefly the dominant methodological perspectives prior to the 1940s and the changes that happened around that time in the methodological self-consciousness of orthodox economists.

Prior to the 1940s, the dominant perspective was, what can be labelled, *deductivism* (see Hausman 1989: 116-117 and Blaug 1992: 51). This perspective extends back to the writings of Nassau Senior (1836) and John Stuart Mill (1836), who, in some of the first extended writings on economic methodology, attempted to come to terms with and justify Ricardo's deductive and abstract method. Methodologists in this tradition stress the fact that it is essential to start with established premises, either known inductively or *a priori*. These initial premises — such as the law of diminishing returns or "people seek more wealth" — were intended to reflect the operation of fundamental causal factors (Hausman 1989: 116). The deductions then served to draw out the implications of these causal factors. It was understood that the basic premises represented tendencies that could be temporally disturbed by other causal factors, with the consequence that the implications of economic reasoning might not always be observed. The premises and the theories that were built upon them were seen as tendencies rather than as matters of fact. In other words, *ceteris paribus* clauses were regarded as present in most theories. This meant that the empirical verification of theories was a hazardous enterprise. Empirical disconfirmation did not necessarily mean that the theory was false, but only inapplicable in the situation because temporarily disturbing causes might be present. Empirical confirmation was seen, at best, to have a minor justificatory role — it could serve the role of pointing out errors in deduction or of determining whether significant

causes had been left out of the analysis (Hausman 1989: 116). In any case, disconfirmation of theory had no bearing on the basic premises and fundamental laws of economics. And confidence in economic theory derived for the most part from the confidence that the basic premises represented the fundamental causes of economic phenomena.

This is, briefly, the basic methodological perspective of Ricardian and subsequently neo-classical economists until about the 1940s, when positivistic philosophies of science started to make their way into the methodological consciousness of economists. There were, of course, alternative economic methodologies expressed prior to the shift, most notably the historico-inductive method of the German historical and American institutionalist schools. In fact, the chief methodological controversies in economics before this shift centred on the relative merits of the abstract-deductive and historico-inductive methodologies. For the institutionalists, the central question was, and still is, how economic action is shaped by purpose and circumstance, in its institutional detail. They were interested in delineating historical patterns and the evolution of the economy over time. Not surprisingly, they found the abstract-deductive method sparse and the conception of economic man, especially that associated with the neo-classicals, to be unrealistic and wanting (see Hodgson 1989).

Terrance Hutchison (1938) was one of the first positivist/Popperian critics of the deductivist methodology to catch the attention of the economics profession. His principal criticism pertained to the untestability of neo-classical economic theory. Untestability was generated when propositions were couched in *ceteris paribus* conditions and by an excessive reliance on abstract deductive reasoning based on simplistic assumptions. He thought that the fundamental assumptions of economics and the propositions of theory should be restricted to empirically testable propositions, i.e., these propositions should be falsifiable. In fact, there should be more reliance on the induction

of empirical generalizations, than on deductive abstract reasoning.

Few orthodox economists and writers of economic methodology accepted Hutchison's criticism that all the propositions of economic theory should be testable and by extension tested. However, they were increasingly ready to abandon the deductivist's scepticism of the possibility of empirically testing theories and to abandon the idea that confidence in theories stemmed from derivation from premises that captured the fundamental causes of economic phenomena. Deduction from basic premises would come to be viewed only as a method for the discovery of theory, rather than as means of lending support to theory. Paul Samuelson (1947) exemplified this shift. He advocated that theories be operationally meaningful. This meant "little more than the prescriptive statement that economists should proceed by deriving conceivably falsifiable hypotheses, which compares favourably with Hutchison's methodological invocations" (Caldwell 1982: 190). However, unlike Hutchison, Samuelson never advocated testing assumptions directly. Rather, it was only theories that were to be tested.

These changes were aided by the increasing importance of econometrics, which enlarged the scope for the empirical testing of hypotheses, and by the work of Hall and Hitch (1939) and Richard Lester (1946, 1947), who demonstrated through the use of survey research that firms did not set their prices according to marginal pricing rules. These critiques of marginalist pricing could be quite troublesome for neo-classical economics if confidence in theories depended upon confidence in the assumptions, as the deductivists maintained. All of these factors encouraged economists to abandon the idea that confidence in the conclusions of economic reasoning stemmed from the assumptions. Instead, it was only the empirical testing of hypotheses that in the final analysis could perform this function. This brings us to Milton Friedman's (1953) contribution, which, while emphasizing the central nature of empirical testing, added, as we shall see, a new

twist. Friedman's principal concern was not deductivism, which by the time he was writing was well on the wane, but the still nagging question raised by Hall and Hitch and Lester concerning the unrealism of marginalist pricing rules.

## 2.1 Friedman's irrelevance-of-assumptions thesis

Milton Friedman's (1953) essay *The Methodology of Positive Economics* has been one of the most influential and discussed essay in the history of methodological thought in economics. Even recently, in the 1990s, prominent writers in economic methodology have published a number of commentaries on this essay.<sup>1</sup> Although the bulk of commentaries over forty years since its publication have been critical of the essay, Friedman's views have been extremely influential amongst working economists.

Friedman (1953) begins the essay by making the distinction between positive and normative economics. He states that the ultimate goal of the former is prediction (p. 7). Moreover, "the only relevant test of the *validity* of a hypothesis" is the comparison of a theory's predictions with experience (p. 8; his emphasis). "The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis): it is accepted if its predictions are not contradicted" (p. 9). Correspondingly, a hypothesis cannot be tested by the realism of its assumptions (p. 23). An unrealistic assumption cannot falsify a hypothesis. However, he does add two types of cases where an assumption can be used as an "indirect test" of the theory. This is where the assumptions themselves are also implications of the hypothesis and where the assumptions have been used to derive predictively successful hypotheses in other types of situations

---

<sup>1</sup> For example, Lawson (1992), Mäki (1992b), and Caldwell (1992). Boylan and O'Gorman (1995) also give extensive discussion to Friedman's essay and to commentaries by Lawson and Mäki on it.

(pp. 26-30). With the latter, he is thinking in particular of the assumption that "man seeks his own self-interest" (p. 29). I presume he sees this to be equivalent to the constrained optimization assumption.

Friedman is sometimes ambiguous about what is meant by the term "unrealistic." Sometimes he means that an assumption abstracts from reality. Other times he means that it is untrue. The former interpretation can probably account for his statement that "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality..." (p. 14).

Although Friedman does not use the term, his philosophy of science has been widely understood to be a form of instrumentalism. However, Friedman's brand of instrumentalism differs from what is the more usual form of this view, at least outside of economics, which is that theories cannot be legitimately characterized as true or false. This is the form of instrumentalism that the philosopher Ernst Nagel advocates and that Karl Popper attacks.<sup>2</sup> For Friedman, by contrast, while theories can be characterized as true or false, their truth or falsity does not matter (see Caldwell 1992). As with all forms of instrumentalism, only the predictive success of theories matters. His instrumentalism is reflected in his recommendation to construe hypotheses and assumptions as *as if* statements. If firms, for example, do not actually solve the mathematical problem required for profit maximization, they can be viewed *as if* they maximized profit, as long as their behaviour conforms to the predictions of profit maximization. It is clear by his examples that he means that theories only "explain" by predicting phenomena, not by rendering a causal account, the latter of which is the more usual notion. He is not interested in giving causal accounts of phenomena, but only in prediction.

---

<sup>2</sup> See Caldwell (1992: 119-20) and Pheby (1988: 81-82), respectively.

Most commentaries have been critical of Friedman's instrumentalism. One noteworthy exception is Lawrence Boland (1979).<sup>3</sup> His defence is that instrumentalism solves the Humean problem of induction. This is the problem of knowing *with certainty* that an empirical generalization is true. How, for example, is it possible to demonstrate that all swans are white? Any enumeration of swans always leaves open the possibility that the next one is not white. The problem of induction is the problem of generalizing from knowledge of particulars. Boland's point is that instrumentalism skirts the problem of whether these generalizations are true or not by denying that the truth or falsity of generalizations matters. Rather, for an instrumentalist all that matters is that a theory is useful, namely that it offers fruitful predictions.

Although the bulk of commentaries on the essay have been almost uniformly critical of his irrelevance-of-assumptions thesis, many commentators have found Friedman to have made some contribution to our understanding of the role of assumptions. A good example is D.V.T. Bear and Daniel Orr (1967). They argue that tests of assumptions are important tests of hypothesis. Nevertheless, they suggest that the difficulty of testing assumptions makes it legitimate to treat the assumptions *as if* they were true and get on with the process of testing the predictions of theory. In such cases we are to choose theories on the basis of the success of their predictions. In yet another twist, Alan Coddington (1979) finds reason to commend *as if* form of theorizing on the grounds that "it is forced on us as soon as we abandon...a 'replicative' view of theorising" (p. 3). He recommends that the subject matter should be viewed "not 'as' the model, but rather...'as if' the model could be used as a surrogate for it" (p. 3). He believes that abandonment of the *as if* principle poses problems for the value of theorizing that assumes, for example, no government, no trade, or

---

<sup>3</sup> Fritz Machlup also at times appears to take an instrumentalist position. However, Machlup is best characterized as an instrumentalist in the more usual sense that theories cannot be usefully characterized as true or false. See Mäki (1989), Caldwell (1992), and Machlup (1955).

only one commodity.

Friedman's essay helped to establish the view in the economics profession that the testing of a hypothesis by its assumptions is both misleading and unnecessary.<sup>4</sup> Despite his implicit instrumentalism, which denied the importance of true conclusions altogether, his essay did suggest the idea that a false assumption does not prevent a true conclusion.<sup>5</sup> thus making tests of hypotheses by their assumptions misleading. The essay more directly argued that "economic theories should be judged in the final analysis by their implications for the phenomena that they are designed to explain" (Blaug 1992: 110).

## 2.2 A metaphysical core of assumptions

Another approach that deflects away criticisms against theory for being based on unrealistic assumptions is to argue that there exists a core of assumptions that are insulated from direct empirical refutation. Instead, only indirect empirical refutation of these assumptions is possible by way of evaluating the research programme as a whole. This is the approach of Fritz Machlup, and more recently of Lawrence Boland. Imre Lakatos' *methodology of scientific research programmes*, which has received a significant amount of attention amongst writers of economic methodology, represents the most sophisticated development of this basic idea.<sup>6</sup>

For Machlup (1955: 10) fundamental assumptions ("that people act rationally, try to make the most of their opportunities, and are able to arrange their preferences in a consistent order; that

---

<sup>4</sup> Blaug (1992: 110) has makes this point, but he also gives Fritz Machlup (whom I will discuss below) some credit for establishing this view as well.

<sup>5</sup> Even though A implies B, the falseness of B cannot be inferred from the falseness of A. Rather, only the truth of B can be inferred from the truth of A.

<sup>6</sup> E. Roy Weintraub's (1985) study of general equilibrium theory is perhaps the most fully developed application of Lakatosian ideas to economics. For other applications see Latsis (1976).

entrepreneurs prefer more profit to less profit with equal risk") are "high level generalizations." These are to be distinguished from "*specific* (factual) assumptions, which are supposed to correspond to observed facts or conditions" (p. 9: his emphasis). For Machlup "fundamental assumptions are not directly testable and cannot be refuted by empirical investigation" (p. 11). This relates to the fact that all of his examples of fundamental assumption are mental states or processes. Machlup rejects interrogation as a legitimate way to test these assumptions, for "such a test would be gratuitous, if not misleading" (p. 11). However, this does not mean that they are beyond challenge and modification or refutation. Rather, the assumptions are only rejected "with the theoretical system of which they are a part, and only when a more satisfactory system is put in its place" (p. 11). He does not say how we are to judge the latter, but presumably it is on the basis of which theoretical system has greater predictive success.

Similarly, Lawrence Boland (1992) identifies the rationality assumption as part of the "metaphysics" of the neo-classical program. He claims that "today most people realize that every explanation has its metaphysics...a foundation of given behavioural and structural assumptions" (pp. 17-18) Furthermore, "metaphysical statements...are the assumptions of a research programme which are deliberately put beyond question" (Boland 1992: 17). Similar to Machlup's claim that the fundamental assumptions are untestable, he claims that we can never prove this rationality assumption to be false. This is not to say that it could be false, "but as a matter of logic we cannot expect ever to be able to prove that it is" (p. 16). His reason appears to be that there may always be some function that the agent maximizes; we can never know for sure.

Lakatos' (1978) methodology of scientific research programmes is yet another version of this idea that there exists a core of assumptions insulated from empirical verification. His theory attempts to be descriptive about the actual character of scientific research programmes and

prescriptive about whether such a research programme is *progressive*. Every research programme, according to Lakatos, possesses both a *negative heuristic* and a *positive heuristic*. The negative heuristic instructs researchers not to question the programme's *hard core*: this is its fundamental assumptions. For example, the claim that agents optimize could be construed as part of the hard core of the neo-classical research programme. Boland's notion of a metaphysical foundation echoes this Lakatosian notion of a hard core and negative heuristic. The positive heuristic is a set of rules that are used to apply the hard core. It "consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme" (p. 50). The "refutable variants" of the programme are what he calls the *protective belt*. It is best understood as a residual category, that part of the research programme that is not the hard core, positive, or negative heuristic. Unlike the hard core, the protective belt is continuously evolving.

A research programme is appraised according to whether it is progressive, which as the name suggests means that it is improving. A research programme is *theoretically progressive* if it is continually predicting novel facts, and *empirically progressive* if these novel facts are confirmed. A novel fact is unsuspected and comes to light only as an implication of the theory. When a research programme fails to realize both of these goals it is *degenerating*. If a research programme is degenerating, we are to abandon it in favour of some rival progressive programme or, I suppose, in favour of a less degenerative programme. Lakatos added a number of caveats to these appraisal criteria. New programmes should be sheltered for a while (perhaps decades) against more powerful rivals (pp. 70-71). Furthermore, degenerating programmes may come back to life again by a change in the positive heuristic (p. 113).

What clearly makes this a contribution to the debate over the realism of assumptions in economics is the immunization of the hard core from empirical criticism, except indirectly through

evaluations of the progressiveness of the research programme. Indeed, it has been claimed that part of the attraction of Lakatosian methodology is the existence of metaphysical hard cores in economics (Hands 1993: 68).<sup>7</sup> Furthermore, there is quite clearly here a method, even if it is not entirely clear how it is to be implemented (i.e., how the research program is to be evaluated), something to which Machlup did not give much attention.

### 2.3 Further distinctions between assumption types

One frequently acknowledged shortcoming of Friedman's essay is his failure to distinguish between different types of assumptions.<sup>8</sup> In this section, I provide some distinctions that various authors have made with the purpose of shedding some light on the issue of unrealistic assumptions in economics. Of course, the idea that there is a metaphysical core of assumptions not subject to direct empirical tests is a way of distinguishing between assumptions as well.

Directly responding to Friedman's essay, Jack Melitz (1965) distinguishes between auxiliary assumptions and generative assumptions. Generative assumptions are used to derive a hypothesis, whereas auxiliary assumptions "are used in conjunction with the hypothesis in order to deduce predictions" (p. 42). These auxiliary assumptions are, in effect, statements of initial conditions. Assumptions may be used in either capacity, but usually tend to take on one role or another. He gives the example of profit maximization as a generative assumption and *ceteris paribus* as an auxiliary assumption.

Melitz makes the point that the realism of auxiliary assumptions is relevant and important. The success of a predictive hypothesis in any given situation depends on whether the initial

---

<sup>7</sup> See also Rosenberg (1992: 87-88).

<sup>8</sup> See, for example, Blaug (1992: 94).

conditions, that allow for the regularity, hold. However, it is not as essential that generative assumptions be realistic, for the reason that "the falsehood of  $A_g$  [generative assumptions] does not preclude the truth of  $H$  [the hypothesis]," as long as the latter does not include any generative assumptions (p. 45). Nevertheless, Melitz maintains that the falsehood of the generative assumptions does raise the probability that the hypothesis is false. He states that

Broadly speaking, the more the evidence conforms to  $A_g$ , the greater the rational basis for confidence in  $H$ ; and vice versa...Furthermore, it may also be observable that  $H$  may derive an important measure of its existing support from the testimony acceptable to  $A_g$  (p. 46).

He goes on to state that, if the generative assumptions,  $A_g$ , are not sufficiently corroborated, they would not "likely be used" to develop hypotheses or hunches. The important point here is that Melitz is arguing that the generative assumptions are often used to lend support to the hypothesis.

This harkens back to the deductivist methodology. Despite the wholesale abandoning of the deductivist methodology, the notion that generative assumptions are required to lend support to claims in economics is a very important point that is not emphasized enough nor sufficiently developed in the discussion on assumptions in the methodology of economics. In the next chapter, I will develop this idea further.

In another contribution, Gibbard and Varian (1978), defending the mainstream economics, seek to distinguish between assumptions by their role in different types of models. They ask the question, "in what ways can a model help in understanding a situation in the world when its assumptions, as applied to that situation, are false?" (p. 665). Their concern is with what they call theoretical models, as opposed to econometric ones. Theoretical models are intended "to explain aspects of the world," i.e., give a causal account (p. 672). Furthermore, they distinguish between "ideal" and "descriptive" theoretical models, of which the latter is their concern. The former

describes an ideal case, while the latter describe economic reality (p. 665). Finally, within the class of descriptive models they distinguish between models as "approximations" and models as "caricatures." It is these two types of descriptive, theoretical models that are the focus of their analysis. They define the former as models that aim to give an approximate description of reality, while the latter "seek to 'give an impression' of some aspect of economic reality not by describing it directly, but rather by emphasizing — even to the point of distorting — certain selected aspects of the economic situation" (p. 665).

These two types of models, in turn, place different requirements on the nature of the assumptions that they employ. With models as approximations, the assumptions will be either approximations or "sufficiently close to the truth" such that the conclusions will be approximations (p. 670). With models as caricatures, by contrast, assumptions are chosen to "exaggerate and isolate some feature of reality" (p. 673). According to Gibbard and Varian, the exaggeration of a causal factor apparently sometimes allows for a "better explanation" of the role of that causal factor (p. 673). It appears that the conclusions of such models "roughly depict some feature of the situation" because the assumptions "caricature features of the situation, and the conclusions are robust under changes in the caricature" (p. 675). They say that such models are tested by formulating models with "disparate caricatures of the same complex aspect of reality," to determine if the same conclusion is obtained.

Gibbard and Varian have identified two important purposes of theoretical models. The first, approximation, is relatively straightforward. The second, the notion of models as caricatures, is in effect the method of isolation. The method of isolation is simply investigating the effects of some factor by sealing off the effects of other factors. Gibbard and Varian, I believe, see this as exaggerating reality, because the method implies focusing on one or a set of causal factors, while

leaving aside others. However, to characterize the method of isolation as involving exaggeration is not helpful and potentially confusing because it is not clear what sort of exaggerations are prohibited. There is no limit to our potential to exaggerate reality. Furthermore, it adds nothing to our understanding of the method of isolation. Both approximation and isolation are important techniques, but it is always necessary to evaluate whether one has truly approximated or isolated some feature of reality. Further analysis is always required. This is generally not emphasized enough. The advice given by Gibbard and Varian on how to evaluate the validity of the approximation and isolation does not seem to me to be adequate. The advice, instead, should be to consider the effects of relaxing any simplifying assumptions on the conclusions. I will discuss this further in a subsequent chapter.

Alan Musgrave (1981) makes an important and frequently cited contribution to the literature on assumption in economics. Musgrave attempts to evaluate Friedman's irrelevance-of-assumptions thesis in light of a threefold distinction between assumptions. *Negligibility assumptions* are defined as assumptions about a factor having a negligible impact on some phenomenon of interest. For example, this could be the assumption that air resistance is of negligible importance to the acceleration of a compact ball falling a short distance. These types of assumptions are a subset of assumptions that are used to generate approximate conclusions, as discussed in regard to Gibbard and Varian. *Domain assumptions*, by contrast, define the domain of applicability of the theory, i.e., the cases where the theory applies. The domain assumption must be true of some situation to make the theory applicable. Finally, there are assumptions that are made only to facilitate the development of the theory, and which are eventually relaxed. Musgrave calls these *heuristic assumptions*: they are first made, then relaxed. "In the first stage he [the scientist] takes no account of factor F, or 'assumes' it is negligible; in the second stage he takes account of it

and says what difference it makes to his results" (Musgrave 1991: 383).<sup>9</sup> Musgrave claims that the truth of negligibility and domain assumptions is essential. With the former, we are able to discount a factor because it is correct to assume it is negligible. With the latter, the truth of the domain assumption tells us whether the theory is applicable in that situation. Musgrave also asserts that it is important that the heuristic assumption not be wildly unrealistic, although he does not defend this point.

Finally, it is also worth mentioning Thomas Mayer's (1995: 61) distinction between necessary and fat assumptions, as I will make use of the former concept. Necessary assumptions are as the label suggests necessary for the empirical validity of the claim upon which they rest. Fat assumptions, by contrast, contain "excess content." He gives the example of the assumption that "all prices are flexible in the long run" in the quantity theory of money. (pp. 61-62). The idea is that this assumption is more than is required for the quantity theory to hold. Only the assumption that "a large proportion of the prices, not all prices, are flexible" is needed (p. 62). For Mayer, realistic assumptions are essential only if the assumption is necessary, and it is only legitimate to test a hypothesis by an assumption if it is necessary. The realism of fat assumptions is only important with respect to any necessary component of the fat assumption.

#### 2.4 Some criticisms of the idea that theories are and ought to be tested by their empirical implications alone.

Charles Wilber and Robert Harrison (1978) criticize the contention that the truth of theories are, in fact, verified by direct empirical testing, and in particular by testing the theory's "predictions against experience" (p. 66). They argue, for one, that by the very nature of the subject matter,

---

<sup>9</sup> This is well known as the *method of successive approximation*. In fact, Musgrave refers to the term.

successful prediction is difficult in economics. They emphasize the instability and ever changing quality of behavioural processes and the open nature of social systems. Successful prediction requires a certain determinateness, i.e., absence of disturbing factors, which is not in the nature of social systems and not available to economists due to the limited opportunities offered for experimental control (pp. 67-68). They also argue that economists routinely ignore instances of the failure of predictions. They suggest that this is in part due to the open nature of economic systems. Economists use the "*ceteris paribus* technique" in order to control for factors that might disturb a relationship, so that when predictions fail they are able to blame the conditions embodied in the *ceteris paribus* clause for not holding (p. 68). The tendency to ignoring of predictive failures also stems from the "difficulty of constructing clear-cut tests of an hypothesis in economics. Most of the traditional statistical tests...are very weak ones which a large number of different theories are capable of passing" (p. 69). Furthermore, the quality of the data can be blamed. This includes the fact that "typically, economic data are statistically constructed and are not conceptually the same as the corresponding variables in the theory" (p. 69). Consequently,

positive economics thus becomes perfectly insulated from refutation. It cannot be harmed by demonstrating that the assumptions and laws of the formal model are abstract and unrealistic, and the model is not rejected when its predictions fail to fit the facts...the theory collapses into an *a priori* formal model that compels assent by logic, not by its conformity with empirical reality" (p. 69: their emphasis).

Since empirical tests fail to discriminate adequately between theories, economists end up assessing "theories on the basis of desirable logical qualities such as simplicity and generality, all qualities inherent in formal models" (p. 69).<sup>10</sup>

---

<sup>10</sup> They advocate instead of deductive modelling what they call *pattern modelling*. This is to

A similar point is made also by Alfred Eichner (1986: 178). He attests to the still prevalent use of logical coherence, i.e. that a conclusion follows logically from the assumptions as a means of justifying propositions. "Economists, especially those esteemed by their colleagues as theorists, by and large still believe this test to be sufficient" (p. 178). In fact, he says that neo-classical economics only adequately passes this test, and "has consistently failed to meet any...empirical tests" (p. 181).

While Wilber and Harrison emphasize the *difficulty* of evaluating hypotheses by their empirical predictions, Bart Nooteboom (1986) stresses the *inadequacy* of such an evaluation. He argues that economists should employ considerations of *plausibility*. "To be plausible, a proposition should be 'well connected' or 'coherent'" (p. 208). This simply means, "it is consistent with supported facts and well-supported theories or their implications" (p. 207). This well-connectedness "admits of various degrees," depending on the relative weight of knowledge consistent with the proposition to knowledge in conflict with it. Considerations of plausibility include not only the standard empirical tests of theories, but empirical support for assumptions. Another consideration of plausibility is support for elements of the theory — assumptions, propositions, and implications — from other "corroborated, successful, and fruitful theories" (p. 210).<sup>11</sup> This includes a test of whether one's proposed theory shares an assumption, proposition, or implication with the well-

---

explain phenomenon by characterizing the relationships in which they are embedded. The researcher is supposed to identify patterns in which a particular type of concrete phenomena is embedded: "an event or action is explained by identifying its place in a pattern that characterizes the ongoing processes of change in the whole system (p. 73). Such a method clearly favours the use of realistic assumptions.

<sup>11</sup> Sometimes he speaks as though considerations of plausibility exclude empirical tests of the implications of the theory. However, this is not particularly important. There is nothing in his definition of plausibility that would imply this exclusion.

supported one. It would also include a test of whether an element of the proposed theory is implied by the well-supported one or if the proposed theory itself implies an element of the well-supported one. He states that it is irrational for economists to limit themselves to testing the empirical validity of implications. "because we would forego opportunities of tests against other funds of experience... We need all the evidence we can get" (p. 221).

Nooteboom considers plausibility to be important for at least four reasons: the selection of explanatory principles in the early stages of theory development; confidence in forecasts and policy evaluations (which requires an assessment of the stability and projectability of causal relationships); an assessment of the relevance and falsificatory force of an empirical anomaly; and guidance in deciding which part of the theory is to be replaced or modified in the advent of empirical disconfirmations (pp. 221-22). He also gives another reason. "the resolution of conflicts concerning the meanings of terms and interpretation of phenomena that may arise from different theoretical perspectives..." (p. 221-22). But it is not clear how this is related to the assessment of plausibility as he has defined it.

## 2.5 Hausman's inexact and separate science of economics

The root of Daniel Hausman's idea of economics as an inexact and separate science lies in the deductive methodology of J.S. Mill. Hausman attempts to update J.S. Mill's methodology of economics with the primary aim of describing the method of neo-classical economics. However, as we shall see, his exposition does have a prescriptive side. Hausman's focus is only on neo-classical economics, to which he refers to as economics for the sake of simplicity. First, let me explain his notion of economics as an inexact science.

According to Hausman (1992: 133), "economic laws are qualified with *ceteris paribus* clauses": economic laws are qualified with the condition of "other things being equal." A familiar

example of such a law is. "*ceteris paribus* a fall in the price of widgets will increase the demand for widgets." He also gives a somewhat unusual example. "*ceteris paribus* people's preferences are transitive." Presumably, in this case the only cause of a person's preferences not being transitive would be changes in other factors, such as a change in a person's tastes (see p. 136). He also calls these *ceteris paribus* laws inexact laws. As in Mill's formulation, the idea here is that these laws are only supposed to take into account the important cause(s) of a phenomenon.

From here he attempts to "formulate a schema sketching a "deductive" method of theory appraisal that is both justifiable and consistent with existing theoretical practice in economics, insofar as that practice aims to appraise theories empirically" (p. 221). It consists of the following steps (see p. 222). First, formulate *ceteris paribus* laws and generalizations "concerning the operation of relevant causal factors." Second, deduce predictions from these generalizations and from "statements of initial conditions, simplifications etc." Third, test the predictions. Fourth, "if predictions are correct, then regard the whole amalgam as confirmed." As Hausman notes, this is little more than the hypothetico-deductive method of hypothesis appraisal (see pp. 222, 304), a cousin of the familiar deductive-nomological method of scientific explanation. The main difference is that disconfirmations can be ignored as either failures of the *ceteris paribus* condition or of other simplifications required to deduce the prediction.

He says that "it is not unacceptably dogmatic to refuse to find disconfirmation of economic 'laws' in typical failures of their market predictions" (p. 207). The idea is that, given the problems that arise when testing basic laws, "it is rational to remain committed to them in the face of apparent disconfirmations" (p. 253).<sup>12</sup> However, it is only rational, of course, when the basic laws have a certain initial credibility. Furthermore, the laws may possess "pragmatic virtues," making the

---

<sup>12</sup> Cf. Lakatos (1978).

theory "mathematically tractable, consistent, and determinate" (p. 210). Hausman seems to be saying that the confirmation of these inexact laws stems both from testing the deduced implications of these laws, and from the axioms by which these laws are deduced in the first place. However, the latter is the most important, given problems with testing.

Hausman does find concern for dogmatism in economics. However, this stems from the commitment of many neo-classical economists to economics as a separate science, rather than from the inexact method just described (p. 223). "Economics as a separate science" is a label he applies to describe a connected set of ideas that characterize the theoretical mission of economics. He lists three properties: "economics is defined in terms of certain causal factors" rather than in terms of a domain of human activity; there is a "distinct domain, in which these causal factors predominate" (i.e., the economy); and the laws of these "predominating causal factors are already reasonably well-known" (pp. 90-91). The chief causal factor here is "rational greed" (p. 95). This substantive vision of economics implies a further fourth property: the "economic theory, which employs these laws, provides a unified, complete, but inexact account of its domain" (p. 91). According to Hausman, this is how economists view economics.

These views associated with the idea of economics as a separate science leads to an unwillingness to accept theory change, even in the face of disconfirming evidence, suggesting dogmatism. The distinctive theoretical mission of economics gives economists the sense that *there is no better alternative*. The alternative it seems must have a "compact and parsimonious theoretical core" that is as "comprehensive in scope," i.e., capable of covering the same range of phenomena (p. 235). However, Hausman believes that this demand is misplaced, since such criteria, although valuable, must take second place to empirical appraisal. In effect empirical relevance demands that economists must "part with the grand vision that a single theory could provide one with a basic

grasp of the subject matter" (p. 225).

Hausman has a number of recommendations. He thinks that economists should give up their vision of economics as a separate science, and be more willing to embrace alternatives to neo-classical economics, as credible research projects. In particular he thinks that economists should pay attention to the work of other social scientists and pay attention to different styles of theorizing, such as exemplified by the institutionalists (p. 254). His plea is for greater diversity in economics. Corresponding to the need to consider other theoretical strategies, economists need to spend significantly more resources gathering data to test the basic postulates of neo-classical economics and to serve the development of alternatives (pp. 253-54).

## 2.6 Lawson's Critical Realism

Tony Lawson's (1992) critical realism also makes a contribution to the debate on the realism of assumptions in economics. His argument is, in essence, that the realism of assumptions matters because of the necessity of understanding causal mechanisms. Unrealistic assumptions have the potential to distort our understanding of causal relationships.

He argues that the Friedmanite idea that theories are to be evaluated by their predictive adequacy runs into difficulty once we recognize the essential openness of economic systems. Lawson distinguishes between *closed* and *open* systems. A closed system is one where a constant conjunction of events, i.e., a regularity of the form, whenever event x occurs, then event y occurs (Lawson 1997: 19; 1992: 150). He calls this regularity determinism, event x determines event y.<sup>13</sup> For a regularity to occur, it must be insulated from disturbing factors. But that is the problem:

---

<sup>13</sup> He also defines regularity stochasticism, where events x and y are "regularly conjoined under some (set of) 'well-behaved' probabilistic formulation(s)" (1997: 76). This is mainly used in econometrics.

disturbing factors sometimes or often occur. This is to say that actual systems are open. There are regularities, but they are not constant. He speaks of *demi-regularities*, a regularity that sometimes does not hold up (1997: 204).

Now, predictive success requires a regularity of events, i.e., the absence of any disturbances. However, he claims that disturbances can and do occur: actual systems are open. For this reason, an understanding of underlying causal factors is required both to evaluate the predictive adequacy of hypotheses and apply predictive hypotheses. In regard to the former, we face the problem of knowing the circumstances appropriate for the test of its predictive success (1992: 161). Certain circumstances will be inappropriate due to the existence of factors disturbing the regularity of events. The appropriate set of circumstances is defined by the assumptions: these are in Musgrave's usage domain assumptions. Lawson claims that we must be able to distinguish between the case when it is that the domain assumptions are false, i.e., when the circumstances are inappropriate for the test and when the "the hypothesis as a whole" is false (or, at least, whether the disconfirming evidence should count against the predictive success of the hypothesis) (p. 162).<sup>14</sup> The domain assumptions identify those situations where the regularity is not subject to disturbing factors. It is a question of whether the hypothesis is inapplicable in the situation or inadequate as a whole. To determine this, we must grasp the nature of causal mechanisms affecting the supposed regularity, as this will reveal the domain assumptions that delimit the circumstances in which the regularity will hold.

This argument also extends to the application of predictive hypotheses. Regularity is required for prediction. Hence prediction requires knowing when the regularity breaks down. This

---

<sup>14</sup> Recall that Friedman (1953: 9) recommends that a "hypothesis is [to be] rejected if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted."

means understanding the causal structures impinging on the outcome.

Lawson believes economics and indeed all scientific activity is better construed as a search for causal mechanisms rather than a search for event regularities. For Lawson, this follows from the open system nature of social and natural processes which render continuous event regularities rare phenomena. Because disturbances are probable, it is more fruitful to search for the causal mechanisms underlying phenomena, rather than to attempt to identify event regularities. Lawson argues that what motivates orthodox economics is its search for event regularities.

In fact, it is clear from his discussion that he sees this search for event regularities as an important cause of the prevalent use of unrealistic assumptions in orthodox economics. He argues that strict event regularities require closure of a twofold nature, an *intrinsic constancy* and an *extrinsic closure* condition. The former means "that the internal, or intrinsic, structure of any (delineated state of any) individual of analysis be constant" (Lawson 1997: 98). The latter means non-interference from outside disturbances (p. 99). For Lawson, the objective of obtaining closure leads to and reinforces the use of unrealistic assumptions. He cites as an example of intrinsic constancy, the characterization of economic agents as optimizers (p. 100). The extrinsic closure condition amounts to "treating each agent as acting within an isolated and typically highly limited fixed set of conditions" (p. 101). The search for event regularities (i.e., system closure) encourages the use of unrealistic assumptions.<sup>15</sup>

---

<sup>15</sup> This relates to Lawson's point that orthodox economics misconceives the nature of abstraction: "Any explicit reference to the term is taken almost exclusively to denote the (typically illegitimate) activity, or result, of *excluding* something real, of assuming it away entirely...the term abstraction stands in as rhetoric for the pretence that economic phenomena are, after all, generated under conditions equivalent to those achieved through experimental control" (1997: 234-35; his emphasis). The problem here, of course, is that the factors assumed away may have an important causal bearing on the phenomena of interest. In effect, Lawson is saying that it is illegitimate to appeal to the notion of abstraction to justify the use of unrealistic assumptions.

Lawson's emphasis on the notion of causality is an important contribution to the debate on assumptions in economics. If prediction is the only goal of economics, then conceivably the realism of assumptions do not matter. However, once we acknowledge that understanding causal mechanisms is important, we must pay attention to the realism of assumptions. A point perhaps appreciated by Friedman, given his instrumentalism with its assertion that theories are only instruments for prediction. But, as Lawson attempts to show, even if prediction is the only goal, causal understanding is usually important to successful prediction. In this he is right.

## 2.7 Mäki's clarifications

Uskali Mäki has in a number of papers expended considerable effort in an attempt to clarify the issue of the realism of assumptions in economics. Below I discuss two of his more successful clarifications.

Mäki contends that the debate about realism in economics can be significantly clarified if we understand the different senses in which the term realism and its cognates can be and is used. He contends that the terminology of realistic, realism, and all of its other cognates is often "highly misleading and, together with underlying conceptual obscurities, productive of stagnation in the discussion" (Mäki 1989: 176). He contends that writers on economic methodology should be more careful about the use and interpretation of these terms.<sup>16</sup> Mäki lists a number of ways a representation can be considered unrealistic. A representation can be said to be unrealistic if the

---

<sup>16</sup> Mäki (1989) also separates the issue of realisticness from philosophical issues of realism, of which he sets forth a number of categories. Since his categories of philosophical realism do not add to our understanding of the issue of the realism of assumptions, I will not discuss them. However, he does say that talk of the realism of theories and assumptions easily misleads economists "to think that those who favour more such 'realism' in theories are advocates of realism as a philosophical doctrine, while those who are content with unrealistic assumptions are non-realists" (Mäki 1994: 247).

objects to which it refers are not real or not observable. An example of the former is references to the Walrasian auctioneer. Alternatively a representation may be unrealistic if it is not true. Furthermore, theoretical representations can also be said to be unrealistic if it is not about essentials. In their methodological discussions, economists have also meant unrealisticness to mean partiality — for example, Friedman (1953: 32) uses this meaning of unrealistic when he refers to a theory of the wheat market that leaves out the eye colour of the traders as being unrealistic. Partiality is often thought of as abstractness. Instead, Mäki uses the term "abstractness" to mean, for example, references to a ball as opposed to a specific ball. According to Mäki, this is another way in which a representation can be said to be unrealistic. Unplausibility is yet another form of unrealisticness.<sup>17</sup>

In another attempt to clarify the issue of assumptions Mäki (1994) also argues that the issue of the realism of assumptions is misconstrued when it is put in general and abstract terms. This is when it becomes a question

over whether, descriptively, the assumptions of a given economic theory are realistic or unrealistic, or whether, as a general normative principle, theories in economics should involve realistic assumptions, or whether, given the current situation, theories and assumptions should be 'more' realistic (p. 239).

Instead, he argues that the issue should be "one over which specific assumptions are and should be realistic or unrealistic:" for, we cannot resolve the issue in the abstract because "all theories are unrealistic in a number of ways" (p. 239). He leaves the definition of unrealistic open here, but presumably all theories can be unrealistic at least in the sense of being partial, as defined above. He

---

<sup>17</sup> For discussions of the different possible ways the term unrealistic can be construed see Mäki (1989: 179) and (1994: 241-243).

appeals for what he calls a more concrete approach. This means appreciating the function of unrealisticness in the context of specific theories. He seems to see two functions for unrealistic assumptions: to pick out "what is believed or hypothesized to be essential" and to allow for formal tractability (p. 253). Obviously, in regard to the former the task is actually to ensure that the essential is picked out and inessential left behind. In regard to the latter, he gives no guidelines. He calls on economists and economic methodologists "to develop principles that could be used for assessing and choosing between rival claims to realisticness based on theories that involve unrealistic ingredients" (p. 253).

### **3 A reorientation of the assumptions issue**

In order to clarify the issue of unrealistic assumptions, we must determine how unrealistic assumptions matter. This involves determining what role assumptions play in economics, and how their status affects this role. The central question is whether assumptions are only used for discovery of claims, as is often maintained by defenders of orthodox economics, or whether they have a justificatory role as well. But before discussing this issue, I will first attempt to clarify the role of truth in regard to assumptions and theories.

#### **3.1 The importance of truth**

As Mäki and others have stated, demands for more or less realism in economics, although certainly meaningful, are hampered by ambiguity. Such demands can easily be misconstrued, especially by those predisposed against one's position, i.e., those who are satisfied with the status quo. One important step out of this ambiguity is to recognize that we only need concern ourselves with the truth of assumptions. As long as an assumption is true, its use poses no problem. Only its falsity is potentially problematic. This is easy to see. It is problematic for any argument based upon the false assumption. For how can we justify a claim based on a false assumption? It is problematic for any deductive derivations of claims, because it can lead to false conclusions. As Melitz has stated, the falsity of assumptions does reduce the probability that we will derive true conclusions from it. Furthermore, an assumption cannot be false and be one of Mayer's necessary assumption without falsifying the claim that rests upon.

Abstractness and unobservability simply do not matter: falsity matters. Furthermore, it is essential that the representations refer to real things. That the subject of the proposition refers to

something real seems to be a precondition of whether we can meaningfully say whether it is true or false. Finally, we should not regard the question of unrealistic assumptions as a question of whether they are implausible or not. Like the term "unrealistic," the term "plausible" is ambiguous. Certain claims, like the identification of optimal choice behaviour with self-interested behaviour, are superficially plausible. As a consequence, such an identification may be deemed, in minds of some, to pass the test of plausibility. The real question is whether the assumption is true, and what warrant there is for this.

The next question is the importance of truth in theories, models, and claims. One view I want to address here is the idea that due to the very complexity of economic reality, models and theories must be simplifications and hence violate the truth. This is the view that models are necessary simplifications and hence false, since economic behaviour is sufficiently complex. Such a view may stem, as Lawson (1997: 238) suggests, from regarding theories and models as "smaller, scaled down, or otherwise simplified, versions or mappings of the original." Advocates of this view may be thinking of models and theories expressed in precise mathematical form. Clearly, real relationships between variables are often not quite as simple that expressed by such models or theories. Such a view is appropriate for many econometric models. They are simplifications. However, such models are not the concern of this essay. My interest is in theory and theoretical models: statements about how the economy works. In fact, there are two categories here, deductive models and theory. A deductive model is a model where a set of conclusions is deductively derived from a set of assumptions. Commonplace examples in economics include the Solow growth model and the model of a firm in perfect competitive market.

Deductive models that are meant to be a contribution to positive theory have two immediate

purposes.<sup>18</sup> One of the chief ones is to deduce true conclusions. This is how neo-classical economists generally construe the purpose of the Solow growth model, the efficiency wage model, and the model of perfect competition, for example. It is not that the model as a whole, including its assumptions, is true, but rather that only certain conclusions are true. It is these conclusions that are of interest. Of course, due to the existence of simplifying assumptions, such models may contain some information, besides what is in the assumptions, which is neither true nor important to the economist.

The other immediate purpose of deductive models is to tell us what would happen under a certain set of simplifying assumptions. It is to know what would happen if such and such were the case. Here the assumptions are usually regarded as idealizing assumptions. Capital structure theory in its initial presentation by Modigliani and Miller fits here (see Cools et al. 1994). The model of perfect competition may fit here as well, depending on whether its results are regarded as applicable to some situation or not. These models tell us what happens in "ideal" conditions, despite the fact that these ideal conditions may never hold. These are what Mayer (1995: 61) calls formal models, as opposed to an empirical model. Formal theory "develops the implications of certain assumptions" (p. 61). Empirical theory, by contrast, "describes actual occurrences with sufficient accuracy for the theory's predictions and explanations to be useful" (p. 61). We should ask, what use is formal theory, if it is not a game, a contribution to normative theory, a pedagogical device, or a counter-argument. It must be a contribution to positive theory. How else can such theory be warranted? This means that it must ultimately lead to a valid claim about economic reality.

So, how can such theory fulfil this function when it does not have any interesting

---

<sup>18</sup> Other possible purposes include: a contribution to normative theory, a pedagogical device, and as a counter-argument. These are of no concern here.

conclusions that are true of any situation? Firstly, the theory could serve the purpose of being the preliminary stages in the successive relaxation of the simplifying assumptions. This is the method of successive approximation. In the first instance, due to the complexity of the matter, a number of simplifying assumptions are made with the aim of eventually relaxing them. We familiarize ourselves with a more simplified model, before adding more complex features. But the ultimate purpose is to come up with valid claims about economic reality. Secondly, the conclusions of the model may be true as limit conditions. A limit condition is a condition that is approached as the idealizing assumptions are approached. The best example of this is Boyle's law in physics<sup>19</sup> or what is referred to as the ideal gas law. No gas is an ideal gas, so Boyle's law does not hold in any situation, but is approached as the underlying idealizing conditions are approached. This is actually an empirical phenomenon. Boyle's law is truly a limit case. Whether the same could be said of something like general equilibrium theory is doubtful at least within the universe of modern capitalist economies. We would first have to make sense of this notion of "approach" in the context of general equilibrium theory. The idea of general equilibrium theory as a limit condition is only meaningful if we can approximate the idealizing assumptions underlying the theory so that its conclusions will be approximations as well. Finally, this leads to a third and related purpose, approximation. Some of these idealizing models may only approximate reality under some conditions, and not be a limit condition. If the conclusions of the model were to describe a limit condition, this state of affairs could arbitrarily be approached, as the idealizing conditions underlying the model are approached. If that is not true, the conclusions of the model could be true as approximations only. The conclusions of the model are true as approximations; that is, it is true that the relationship expressed is approximate.

---

<sup>19</sup>  $PV = rT$ , where P is pressure, V is volume, T is temperature, and r is a constant.

This, I think, covers all the ways in which these formal models can make a contribution to positive theory. Ultimately, the purpose of such models must be to make true claims about economic reality, if they are not a contribution to normative theory.

Besides deductive models, what is left is just a set of one or more related propositions. We call this a theory, although the notion of a theory is flexible enough to include deductive models. The purchasing power parity theorem and the law of one price are examples. I cite these two examples because these are two theories that in their usual stated form are always false. Nevertheless, some would say that they are still empirically relevant. This is true. But they are only relevant insofar as we can actually formulate related claims that are valid and contain some essential idea contained in these theories. Similar to deductive models they could be construed as limit cases: that is, as a case that is approached as the idealizing assumptions underlying them are approached. This would have to be shown to be an empirical phenomenon. Correspondingly, these theories may make claims that are approximately true. On a slightly different, but related note, these two laws do capture a similar idea: the equalizing effects of arbitrage on prices. Arbitrage has a certain causal effect on prices in the direction of the equalization of prices. It is true statements like this that make these theories interesting, although they may not themselves be true or even approximately true in any situation of interest.

Instrumentalism is another issue on this theme of whether we should seek truth in theories. I doubt that most economists would truly embrace instrumentalism. According to Friedmanite instrumentalism, the truth of theories does not matter, only their predictive success matters. Instrumentalism forsakes any role whatsoever for explanations. Since explanations are causal accounts of phenomena or, in other words, accounts of a phenomena's existence, they are only possible with valid claims. Either the causal account is true or false. I doubt also that economists

would want to give up the notion of causality.

Furthermore, it seems that the instrumentalist is forced to think about causal mechanisms. It is not clear where a theorist would start if he or she wanted to develop a theory that might have good predictive success without thinking about potential causal mechanisms. This means thinking about the truth or falsity of potential causal mechanisms. The theorist must also, as Lawson points out, think about the factors that might disturb this mechanism, in order to develop domain assumptions for when the theory will be predictively successful. Being forced to think about causal mechanisms does not prohibit the theorist from regarding the theory as an instrument for prediction. But it does put him/her in the odd position of having to think about the truth of causal mechanisms and denying importance of truth in theories.

### 3.2 How false assumptions might matter

First, let us consider how we justify (give reasoned support to) claims in economics. One method is a deductive argument.<sup>20</sup> The idea behind a deductive argument is that if all the premises are true, it is impossible for the conclusion to be false. All arguments that involve model building are deductive. Deductive arguments often proceed on the basis of knowledge about how economic actors make decisions and the type of environment they face, leading to a deduction about how they will respond to changes in the environment.<sup>21</sup> The strength of a deductive argument lies in the strength of the premises. The second method of justifying a claim is to seek empirical evidence that

---

<sup>20</sup> A deductive argument is not, as is often characterized, to be exclusively associated with an argument from general to particular, although it can never go from the particular to the general. Neither should a deductive argument be exclusively associated with an argument whose ultimate foundation is a set of axioms.

<sup>21</sup> Here is a very simple example. If we assume that entrepreneurs set prices as a mark-up on normal costs and assume that entrepreneurs desire to maintain a certain mark-up, we can conclude that an increase in perceived normal costs will cause entrepreneurs to desire to increase prices.

is consistent with or supportive of a claim. This evidence varies in its ability to support the claim. Let us call this an inductive argument. I mean inductive in the broad sense — any kind of ampliative inference. These are inferences where the concluding claim goes beyond the claim jointly made by the assumptions. Supporting a theoretical claim with statistical evidence is an inductive argument. Both deductive and inductive arguments are equally capable of demonstrating the empirical validity of a claim.<sup>22</sup>

Now we can consider the different ways false assumptions can matter. Firstly, it is a legitimate criticism of any chain of reasoning, whether this is a deductive or an inductive argument, that it is based upon a false assumption. A true conclusion cannot be supported with a false assumption. Secondly, it is also a legitimate criticism of any given claim that its validity depends upon the validity of another assumption that is false. A claim is not true if its supposed truth depends on a false assumption.<sup>23</sup> These are Mayer's necessary assumptions. Notice the difference between the two cases. In the first, the conclusion does not necessarily depend on the objectionable assumption for its validity (although in some instances it might), and as a consequence the conclusion is not invalidated by its falsity. Nonetheless, a fatal objection is made: the reasoning cannot support the conclusion, so if the conclusion is to be upheld, other support must be found. In the second, the claim is a necessary assumption (we often say implicit assumption), and its

---

<sup>22</sup> Tony Lawson (1997: 24) speaks of what he calls *retroduction or abduction*, in contrast to deduction and induction. "It consists in the movement, on the basis of analogy and metaphor amongst other things, from a conception of some phenomenon of interest to a conception of some totally different type of thing, mechanism, structure or condition that, at least in part, is responsible for the given phenomenon." Retroduction may be an important exploratory device, but only deduction and consistency with empirical phenomena can ground a claim. This appears to be his understanding, although he does not make this clear (see p. 310).

<sup>23</sup> For example, Keynes criticized the proposition that supply creates its own demand on the basis that the implicit assumption that "an act of individual savings inevitably leads to a parallel act of investment" (Keynes 1936: 21) is false.

invalidity necessarily falsifies the claim. However, insofar as the false implicit assumption invalidates the claim itself, it invalidates any reasoning taken to support the claim.

There is also a third scenario. This is where we merely derive a conclusion from a false assumption, but do not rely on this derivation to establish the validity of the claim. The assumption is used only for discovery, and not verification. This has been how many defenders of "unrealistic" assumptions have presented the issue, as if assumptions were only used to derive claims and had no role in the justification of claims. It is certainly possible to derive true conclusions from false assumptions. However, the false assumptions must be chosen judiciously for this to happen. Furthermore, as I will argue below, it is difficult not to rely somewhat on the derivation (i.e., the deductive argument) to give support to a conclusion. It is often very difficult to demonstrate a claim using econometrics alone, especially those claims that are often the products of such derivations.

In what follows I will only be concerned with necessary assumptions and, following Melitz's (1965) usage, *generative assumptions*, assumptions that are used to arrive deductively at a conclusion. Even though the term "generative" suggests that the assumption is used only for discovery and not justification of the conclusion, I will use it to refer to assumptions that are used for either purpose. *Auxiliary assumptions* are also important, but they concern only inductive arguments or deductive arguments that attempt to falsify a claim. I will not discuss auxiliary assumptions, because economists have generally recognized the importance of their truth in the testing of theories.

Another important category of assumptions are simplifying assumptions. We may distinguish between simplifying assumptions that are explicitly made and those that are implicitly made. When a simplifying assumption is explicitly made, it is necessarily a false assumption. It would not be a simplifying assumption otherwise. We call them "simplifying assumptions" because

the term "false assumptions" gives rise to an unwanted connotation: the assumption should not have been made in the first place. Simplifying assumptions that are implicitly made have a more ambiguous relation to the notion of a false assumption. Consider, for instance, the case where we construct a simple Keynesian model with aggregate income, consumption, and government expenditure and use this to derive the multiplier. We can regard the absence of a government sector as an implicit simplifying assumption. But it is only a false assumption, if the conclusion somehow depends on it, either necessarily or in the context of the other assumptions. If we leave out of consideration some factor in a deductive argument, and the conclusion does not depend on having done this, then our leaving it out of consideration cannot be considered a false assumption. Perhaps it should not even be called an assumption. But this is just a matter of semantics. In any case, we must take heed of the fact that all implicit simplifying assumptions are potentially false assumptions insofar as they may affect a conclusion.<sup>24</sup> In summary then, we should treat simplifying assumptions as false assumptions. They are one and the same.

### 3.3 The importance of generative assumptions for verification.

If positivism, Popper's falsificationism, and Friedman's instrumentalism have one thing in common it is the idea that theories are tested by their empirical implications. The adoption of this idea, while in some respects an advance over the old deductivist methodology, has ended up creating a substantial gap between methodological practice and understanding, which is still very much with us today. Economists have been led to misconstrue the real nature of how claims get confirmed in economics. The reality is that while empirical testing is important, it is not often the

---

<sup>24</sup> Let us note that an explicit simplifying assumption may actually not be simplifying. But it depends on your point of view. Any false assumption explicitly introduced, I shall call a simplifying assumption.

only reason or even the primary reason economists view a particular claim to be empirically valid. Empirical evidence is not usually compelling and can often admit of a host of alternative explanations. Instead, economists rely on what Melitz called the generative assumptions, assumptions used to derive a hypothesis, to give credibility to a theory. Below I show why it is that we often must allow theories to gain a measure of support from the generative assumptions. In particular it is assertions of causality that require the support of such assumptions. This is also in keeping with the Nooteboom's notion of plausibility and with Sheila Dow's (1985: 14-17) appeal to a notion of verification she calls Babylonian. Dow expresses the idea that theories are mutually supported or justified with different arguments, both deductive and inductive.

In economics deductive arguments provide a crucial basis for the confirmation of many claims. Consider how it is that many economists have confidence in the following claims: free trade enhances economic welfare: the introduction of a minimum wage will increase unemployment: there is no lasting trade-off between inflation and unemployment: the introduction of free trade will tend to equalize factor prices across trading countries: in the long-run money is neutral: there is an inverse relationship between the rate of profit and the capital-labour ratio: if the central bank sets interest rates too low it will spur inflation: government deficits crowd out private investment: in the long-run the economy converges to a balanced growth path: the rate of capital accumulation is limited by the amount of savings available: the payment of efficiency wages is a cause of unemployment: the bond-financing of deficits causes macro-economic instability.<sup>25</sup> It is difficult to assemble enough direct empirical evidence that can warrant the acceptance of these claims.

---

<sup>25</sup> Some schools of thought are more reliant on deductive arguments than others to confirm their claims. Neo-classical economics is much more reliant on deductive arguments than say institutional economics (of the so-called old kind), but, paradoxically, the institutionalists seem to take the problem of false/simplifying assumptions more seriously.

Inevitably we rely on the support of a deductive argument coupled with any empirical evidence that we can obtain.

Predictive success cannot displace deductive arguments as a method of justifying claims. Consider an example where a model might indeed achieve predictive success, the Solow growth model. Like most theories in economics, the Solow growth model does not quantify relationships: it only gives us the sign of a particular relation. The model tells us that there is convergence in per capita income between countries, but tells us nothing about the rate at which it will happen. Now suppose that we did observe convergence in per capita income between countries. Could we thereby conclude that this convergence was caused by the fact that capital-rich countries had lower rates of return to capital as the Solow growth model claims? No. This convergence could be caused by catch-up, i.e., absorbing a backlog of technology. Distinct from catch-up, convergence may also be caused by economies being in a different stage of economic development, i.e., differences in productivity growth rates between agriculture, industry, and services. Unless all possible alternative explanations have been discounted, we cannot give any credence to the fact that diminishing returns to capital cause convergence. If convergence is to be explained by diminishing rates of return, we must evaluate the simplifying assumptions underlying the Solow growth model and this includes the assumption of diminishing returns to capital. The importance of deductive arguments suggests that it is questionable whether the economists who advocate this notion that theoretical claims are only to be evaluated by their empirical implications follow this methodological rule.

To illustrate these points further consider the claim that non-frictional unemployment (ignoring any so-called structural unemployment) is caused by real wages above equilibrium. Many economists would accept this claim as true. The reason is not because of any compelling direct empirical evidence, but because of a deductive argument that ultimately rests on certain

assumptions about the decision-making behaviour of firms and individuals, along with certain assumptions about technology. The argument demonstrates that there is only one real wage that clears the market, so that non-frictional unemployment is caused by a real wage that is too high. Consider what sort of direct empirical evidence we would have to assemble to support the claim. Evidence that real wages and unemployment are higher in Europe than in the U.S. is clearly very weak unless we could exclude all other possible explanations for the difference in unemployment rates. In that unlikely event, we would have to gather statistical evidence on the real wage and unemployment as well as on other possible factors that might affect unemployment besides the real wage, and then do some statistical analysis. Supporting the claim in this way is a very difficult affair, as we would have to show that increases (decreases) in non-frictional unemployment are *caused by* increases (decreases) in the real wage, and cannot be accounted for by other factors. The trouble is that it is very difficult to demonstrate causality with econometrics.

As this example suggests, deductive reasoning is an important means to substantiate claims about causal relationships among variables such as unemployment, inflation, aggregate demand, exports, and other abstract entities. Our understanding of causal relations among such abstract entities tends to be built up largely from an understanding of the powers and decision-making procedures of agents and institutions. This should not be surprising because causal relationships only exist among such entities in virtue of the way agents and institutions act. Causal relationships exist among these more abstract entities because changes in them represent changes in the environment of agents, which in turn alters their behaviour, and this is manifested again in changes in these abstract entities. Such relationships among these abstract entities are deduced from what we know about agents and institutions.

Moreover, information about the powers and decision-making procedures of agents and

institutions also gives us important information about the causal mechanism linking variables. Suppose that, through tests of Granger-causality, we have well founded evidence that an increase in demand causes an increase in the rate of inflation. However, there is still a question about the sort of causal mechanism that links these two variables. Perhaps, firms face diminishing returns. Alternatively, increases in demand could be putting upward pressure on wages or on the cost of raw materials. Upward pressure on wages, in turn, may be either caused by union agitation or by firms bidding up the price of labour that is in short supply. There are a number of causal routes available. Unlike in the natural sciences, theories in economics tend not to offer precise predictions. At best, the predictions offered are of a qualitative comparative static nature where it is either specified that a variable increases, decreases, or is unaffected. This opens the door to a number of potential causal mechanisms linking variables. If these alternative causal mechanisms offered precise predictions, it would be easier to discriminate among them on the basis of empirical tests.

Donald McCloskey (1985) is one critic of the idea that empirical evidence is the exclusive criteria in selecting theories. He provides the following example:

The Keynesian insights were not formulated as statistical propositions until the early 1950s, fifteen years after the bulk of younger economists had become persuaded they were true. By the early 1960s the Keynesian notions of liquidity traps and accelerator models of investment, despite repeated failures in their statistical implementations, were taught to students of economics as matters of scientific routine (pp. 17-18).

Younger economists selected Keynes' theories in part because they were convinced of his deductive arguments and of his critique that key orthodox arguments were false because they relied on false assumptions.<sup>26</sup> This is not to say that the sense that his conclusions were consistent with the real

---

<sup>26</sup> "For if orthodox economics is at fault, the error is to be found not in the superstructure, which

workings of the economy was not important. It was. It offered an explanation for the Great Depression and the subsequent booming wartime economy. But the deductive argument was essential. It is extraordinarily difficult to rely on empirical confirmation alone in supporting Keynesian-type or neo-classical type theoretical claims.<sup>27</sup>

Another fact suggesting that economists tend to take seriously the verification of claims through deductive reasoning is the widespread acceptance by the profession, and especially of leading members of the profession, of the idea that macro-economic theories should be consistent with micro-economics, and in particular the neo-classical rationality assumption. If, in the final analysis, theories are to be tested by their empirical implications, it is difficult to see why concepts like wage and price stickiness must be derived from the assumption of constrained optimization. Those economists who accept this sort of call for microfoundations are implicitly accepting the idea that theories are to be tested by their assumptions. In other words, as long as assumptions are only used for discovery and not verification, it does not matter what assumptions underlie a theory as long as the theory is empirically confirmed. Thus, there is a contradiction in claiming that theories are in the final analysis only to be tested by their empirical implications and demanding that they rest on a proper microfoundations. Of course, we may be able to improve upon the theory by deriving it from the assumption of constrained optimization, but in the final analysis the respective

---

has been erected with great care for logical consistency, but in a lack of clearness and of generality in the premises" (Keynes 1936: v). This is to say that he felt that some of the conclusions were invalidated by false premises.

<sup>27</sup> Another fact worth considering is how claims such as these are demonstrated to students. Most textbooks are devoted to the presentation of deductive arguments with relatively little space given to the assessment of empirical evidence. Microeconomic textbooks, in particular, contain virtually no references to econometric studies. Insofar as such textbooks propose to assert valid claims that are not directly observable, the only reasoning given is deductive arguments. Furthermore, class time is largely taken up with the presentation of deductive arguments.

theories would be evaluated in terms of their empirical implications.<sup>28</sup>

Up to this point I have been speaking about the problems of relying on statistical tests alone, for the confirmation of theoretical type claims. Problems also arise with relying on disconfirming tests to weed out false claims.<sup>29</sup> Consider, for instance, these remarks by Gregory Mankiw (1990: 1648) explaining the breakdown of the so-called consensus that prevailed in macroeconomics until the 1970s. He suggests that empirical failures was not enough to reject the models:

Suppose the macroeconometric models had failed to explain the events of the 1970s, but macroeconomists had felt confident in the theoretical underpinning of these models. Undoubtedly the events could have been explained away. As defenders of the consensus view often assert, much of the stagflationary 1970s can be attributed to the OPEC supply shocks. The remainder could always have been attributed to a few large residuals. Heteroskedasticity has never been a reason to throw out an otherwise good model.

Mankiw argues that it was also necessary that new classical economists be able to criticize persuasively the consensus view for having inadequate microfoundations. This was its theoretical

---

<sup>28</sup> Mayer (1995) raises a similar point. He suggests that the demand for micro-foundations may be rooted in the idea that "what validates economics is its derivation from self-evident propositions" (p. 27). He explicitly refers to this as the continuing hold of the old deductivist methodology. But he goes on to say that this methodological position is illegitimate.

<sup>29</sup> Popperians will claim that we should not attempt to justify or verify a claim, but only falsify it. However, serious objections can be raised against the practicality of falsification. Hausman (pp. 172-191) and Caldwell (pp. 231-243) also raise arguments about why we should not attempt to practice falsificationism. The lack of practicality of falsification is perhaps best demonstrated by the fact that it is not practiced to any significant extent in economics, despite the fact that Popperian ideas have been around in economics since Hutchison (1938). "The historical evidence suggests that falsificationism has never been practiced to any significant extent in economics, despite forty years of advocacy by proponents, and despite the entrance of falsificatist precepts into the methodological rhetoric of the discipline" (Caldwell 1982: 236). Moreover, "in actual practice in both science and everyday life, people make estimates of how well-established and plausible various claims are, and how they are likely to lead one astray" (Hausman 1992: 186).

failure. It is easy to dismiss contrary evidence for one reason or another. Note also that change in economics is to a large extent propelled by successful attacks on the underlying assumptions of accepted theories. This was true of the significant changes also wrought by Keynes, as noted above.

The Duhemian problem presents significant problem for testing in economics. The Duhemian problem is the idea the theories are never tested alone, but always in conjunction with auxiliary hypothesis. The auxiliary assumptions are necessary to perform the test. If the test contradicts the implications of the theory, then it may be due either to the failure of the auxiliary hypothesis, the theory, or both. There may be no way to tell unless it is possible to verify independently the truth of the auxiliary assumptions. The *ceteris paribus* imposes auxiliary conditions concerning the absence of disturbing factors: this is a form of initial conditions. There are other kinds of auxiliary assumptions. Testing requires the auxiliary assumption that one's data is reliable.<sup>30</sup> That the statistical test be appropriate is another auxiliary assumption. Another consideration is the fact that testing a theory often requires the construction of models. This necessarily involves making simplifying assumptions, as well as assumptions whose truth we are uncertain. Such auxiliary assumptions are often required, for example, to specify in mathematical form, the relationships between variables. This includes the specification of an error term. Furthermore, it will be necessary to impose constraints on the error term concerning its distribution (see Kim: 1991).<sup>31</sup>

---

<sup>30</sup> The data that we have may not even be suitable to our theoretical construct. For example, actual price indices calculated by statistical agencies may not be suitable for testing the purchasing power parity theorem.

<sup>31</sup> For further discussion of the Duhemian problem, see Hands (1993: 63-64).

## 4 A critical approach to false/simplifying assumptions

If we depend on generative assumptions for the justification of claims, as the previous section suggested, then we are compelled to evaluate whether the falsehood of any generative assumptions has a bearing on the conclusions. In this section I want to discuss what is involved here. Also of concern are necessary assumptions, as these always falsify a claim.

### 4.1 Simplifying and false assumptions

As I stated above, an explicit simplifying assumption is equivalent to a false assumption. In evaluating assumptions, we also must be cognizant of any implicit simplifying assumptions, as these may affect the conclusion as well. If an implicit simplifying assumption affects the conclusion, then the conclusion is false.

The most obvious examples of simplifying assumptions include perfect competition, perfect knowledge, perfect substitution between labour and capital, rational expectations, transitive preferences, given tastes and technology, the non-existence of government, the non-existence of a foreign sector, the free and costless mobility of capital and labour, costless exchange, the full employment of capital and labour, the flexibility of prices, the ceteris paribus clause, and the absence of externalities.

In addition, implicit simplifying assumptions, often unaware, enter into the analysis when attempting to analyze some causal mechanism, resulting in a misspecification of the causal mechanism. Implicit simplifying assumptions amount to leaving out of consideration certain chains of causation, which, if had they been considered, would have altered the conclusion. For example, on a number of occasions in the *General Theory of Employment, Interest and Money*, Keynes attacked orthodox theory on the basis that implicit simplifying assumptions were being made which affected the nature of the conclusions drawn. By relaxing these simplifying assumptions he found

that the causal relationship asserted by orthodox theory no longer held up. Keynes argued that the orthodox loanable funds approach to the theory of interest depends on the assumption that the supply and demand curves for loanable funds are independent of one another. He attacked this assumption for depending upon the implicit simplifying assumption that income is constant when there is a shift in the demand curve for loanable funds. Keynes (1936: 179-80) asserted that changes in investment (a shift in the demand curve) will lead to a change in income, which in turn will lead to a change in savings (a shift in the supply of loanable funds). The interdependence of these two curves undermines the causal mechanism asserted by the loanable funds approach, whereby the interest rate is caused by the tendency for the supply and demand of loanable funds to be in equilibrium.

To determine whether assumptions about decision-making procedures are false, they have to be compared to the actual decision-making procedure used. It is the actual decision-making procedure that determines the actual decision made. Typically, the profit maximization rule,  $mc=mr$ , is a false assumption. Only infrequently do firms set prices and output according to a profit maximizing criteria. Frederic Lee (1994: 327) reports that over 80 per cent of 4200 enterprises covered in 64 studies used some kind of cost-plus pricing procedure, rather than marginal cost pricing.

It is sometimes stated that even though firms may not use profit-maximizing formula, it underlies the decisions of firms, with the consequence that the outcome is the same or approximately the same as the cost-plus pricing rule.<sup>32</sup> But it must be demonstrated that this is a

---

<sup>32</sup> This is, in fact, how Lee (1984: 235-36) describes the primary response of neo-classical economists in the U.S. to the cost-plus pricing controversy of the 1940s: "In the case of costs, it was argued that if average direct costs of production were constant with respect to different flow rates of output, it would coincide with marginal costs. As for the mark-up, it was argued that if it included demand considerations and was sufficiently flexible, it would stand as a proxy for the price

good assumption. It must be shown that with the function one is using, the assumption of profit maximization either leads to an outcome that is exactly the same, in which case it is true that agents make decisions according to the profit-maximizing decision, or it leads to an outcome that is approximate and leads to approximate conclusions. I discuss the latter scenario in more detail below. When we represent the decision-making procedure of agents not as they actually make decisions, but by some apparent underlying procedure, then we must be very careful that this alternative representation actually captures the outcome of the real procedure. This cannot simply be assumed.

Another related justification for using constrained optimization assumptions more generally is the idea that all decision-makers maximize something.<sup>33</sup> This may be true. Every decision may be decomposable into the optimization of some function given a set of constraints, despite the fact that the decision-maker does not utilize this or any optimizing procedure. The problem here, however, lies in identifying this underlying function. For any given decision, there are likely many functions that when maximized yield that decision. However, it is not clear whether we can find one function that could be used over a range of decisions. For example, for any given choice of price and quantity, we may find some function that when optimized yields that price and quantity. But it is questionable whether we could use the same function for different choices of price and quantities. The fact that every decision can be represented as the maximization of certain function given a set of constraints, says nothing about whether this function can be used to represent another decision, let alone a range of decisions, for that agent. In other words, it is not clear whether there is a stable

---

elasticity of demand. Therefore, if the two conditions were fulfilled, the businessman would in fact be equating marginal cost and revenue when using his full-cost pricing procedure to set his price."

<sup>33</sup> See Boland (1992: 15-16) for example.

function which, if optimized, will yield the choices of an agent. Even if there is a stable function, then it may be very complicated or just about impossible to find. All and all, it may be simpler just to assume the actual decision-making procedure, in which case we can be sure that our conclusions are not distorted.

We also need to be careful about assumptions about knowledge. Assumptions about the fact that economic agents know or even have some fairly precise subjective view of the probability of an outcome of a certain event are also in most cases false. This is in large part because these probabilities are virtually unknowable, and perhaps non-existent. There is almost always some degree of uncertainty about the likelihood of a particular event. How economic agents form their expectations will vary depending on the type of situation, but is almost certain that they are heavily influenced by the state of expectations.

Finally, we must also be careful about domain assumptions, i.e., assumptions that define the domain of applicability of a theory. One of the most important in economics is the assumption about the returns to scale in the area where the firm produces. There has been a pronounced tendency amongst economists to assume diminishing returns — i.e., the tendency to draw upward sloping supply curves. However, there seems to be substantial evidence that this is a false assumption in many cases, particularly in manufacturing. According to a popular microeconomic textbook,

one of the most interesting conclusions of the empirical studies is that the long-run average cost function in most industries seems to be L-shaped, not U-shaped. That is, there is no evidence that it turns upward, rather than remaining horizontal, at high output levels... Another interesting conclusion of the empirical studies is that marginal cost in the short run tends to be constant in the relevant output range (Mansfield 1985: 223).

Critics have suggested that these studies are biased in one way or another, but the fact remains that there is little evidence to suggest that cost curves are U-shaped in the relevant range of production.

#### 4.2 The demonstration of claims through deductive modelling

Modelling by its very definition involves the use of simplifying assumptions. This makes any effort to demonstrate claims through deductive modelling problematic. There is always the danger that one's conclusions depend on the simplifying assumptions. This calls for analysis. For this we need to ask whether any of the simplifying assumptions affect the conclusion? In other words, by substituting true assumptions for simplifying assumptions, could we have deduced the same conclusion? We need to obtain some warrant for believing that the conclusion is independent of the simplifying assumptions.

Conclusions are usually only independent of simplifying assumptions when the restrictions on variables are *not* defined by equalities. Such an exact restriction is usually influenced by the simplifying assumptions.<sup>34</sup> Conclusions that are potentially independent typically do not impose precise restrictions on variables. Restrictions, instead, are defined by the direction in which a variable will change when, say, another variable changes. The Keynesian multiplier illustrates this. Suppose we have a simple economy with only consumption (C) and investment (I) and a constant, positive, less than unity marginal propensity to consume (b) out of gross domestic product (Y). In this case, it can be shown that an injection of one dollar of investment increases output by  $1/(1-b)$ . This implies, in turn, a second conclusion that an injection of one dollar of investment will increase output by more than one dollar. Here the inexact relationship is concluded from a more exact version that is dependent on the simplifying assumptions in the argument. Although the first does

---

<sup>34</sup> Exact restrictions can be true as approximations. I shall discuss such a scenario latter.

not. the second conclusion clearly still holds even if the marginal propensity to consume is not constant. This is clear when we consider the multiplier as a dynamic process. as increasing expenditure by 1.  $b$ .  $b^2$ .  $b^3$ .  $b^4$ . and so on in each successive stage.

Deductive models. if they are to produce valid conclusions. should refer to real causal mechanisms. The causal factors are the domain assumptions and the exogenous variables. The latter are the independent variables in some postulated causal relationship — for example, the marginal propensity to consume and investment in the multiplier model above. Here the conclusion itself is a causal relationship. But whether the conclusion is or is not a causal relationship. domain assumptions will always refer to causal factors. Domain assumptions are a precondition for the existence of phenomena expressed by the conclusion. For example, the output multiplier effect assumes some excess capacity.

We identify simplifying assumptions that could affect the conclusion and that call for further analysis by focusing on those explicit and implicit simplifying assumptions that could affect the underlying causal mechanism. We can ignore all those simplifying assumptions that clearly have no bearing on the causal mechanism. For example, we can derive a basic Kaleckian growth model using a number of simplifying assumptions such as one-sector, simple mark-up pricing, overhead labour, no savings by workers, no change in productivity. From this basic model we arrive at a number of causal mechanisms. We derive some conclusions about the relationships between changes in costs and savings and changes in capacity utilization, the rate of investment and profit. Underlying these relationships is a causal mechanism. The question then becomes whether any of these causal mechanisms depend on the simplifying assumptions. If it is not clear whether and how a simplifying assumption might affect the causal mechanism, we can try to relax it, partially or wholly, and attempt to rederive the conclusion. Even the partial relaxation of the one-

sector assumption to a two-sector model may be a big step, and tell us whether the conclusion is dependent on only having assumed a limited number of sectors. A two-sector model will help reveal the effects of interaction between sectors on the initial conclusion. It may not be necessary to build a three-sector model, if it is clear that more sectors will not affect the underlying causal mechanism.<sup>35</sup>

Formal rederivation, like the formal initial derivation, has some advantages over a purely verbal line of argument supporting the independence of the conclusion from the simplifying assumptions. Formalizing the argument with mathematical symbolism could prove to be an important check of its validity. Such formalized arguments are also a good way of discovering domain assumptions. In fact, we may find that the conclusion is only independent of the simplifying assumption under certain domain assumptions. These domain assumptions will be in addition to those already given in the initial derivation.

Finally, sometimes we may be more concerned with showing that a conclusion built upon simplifying assumptions is false. This is how we can understand one of the goals of the Cambridge, England side in the Cambridge debates. Their strategy was to relax some of simplifying assumptions of the standard neo-classical models purporting to demonstrate that in competitive conditions the marginal product of capital determines the rate of profit and that there is an inverse relationship between the quantity of capital employed and the rate of profit, amongst other relationships.<sup>36</sup> The Cantabridgians argued that models that purported to demonstrate these claims were dependent upon simplifying assumptions, mainly assumption of a single capital good, and

---

<sup>35</sup> For a discussion of the implications of relaxing some of the simplifying assumptions in the Kaleckian growth model, see Lavoie (1992: 282-371). For an analysis of whether and how the two-sector model changes the conclusions, see Lavoie and Ramirez-Gastón (1997).

<sup>36</sup> See Harcourt (1972).

that, if these special assumptions were relaxed, the neo-classical conclusions would not hold. They did this by constructing models which dropped some simplifying assumptions, but kept others. What these slightly more complex models showed is that neo-classical conclusions did not hold up due to reversals of capital intensity and reswitching. Although the models that purported to demonstrate these paradoxical phenomena were based on simplifying assumptions, they could be taken as valid counter-arguments against the simpler neo-classical models which purported to demonstrate the existence of the above relationships.

### 4.3 The method of isolation

This method allows us to isolate the effects of one or a set of causal factors<sup>37</sup> by discounting the potentially disturbing effects of other causal factors.<sup>38</sup> The exclusion of potentially disturbing factors amounts to making simplifying assumptions. The idea is to isolate a real causal mechanism. If the isolation is legitimate, then disturbing factors, if present, would change the way in which the causal mechanism is observed, but would not negate the operation of the mechanism. In other words, the causal mechanism should still be operative with the disturbing factors present if the isolation is a true one. The method of isolation allows us to analyze the effects on a phenomenon of a set of causal factors, to the exclusion of other causal factors that affect the phenomenon

---

<sup>37</sup> We could also potentially isolate non-causal relationships, but this is not important in economics. A scale model can be considered a set of isolated non-causal relationships.

<sup>38</sup> Mäki (1992a: 321) defines it as follows: "In an *isolation*, something, a set of X of entities, is 'sealed off' from the involvement or influence of everything else, a set Y of entities; together X and Y comprise the universe." For Mäki, the method involves the mental act of sealing off any factor whatsoever, i.e. the leaving out of consideration any factor (He actually calls this theoretical isolation, in opposition to material isolation, which is a physical act). This implies that any analysis is an isolation. In my definition, the method of isolation amounts to sealing off only potentially disturbing factors. He defines the method too broadly. The act of isolation should only be defined as the putting out to play of known or potentially disturbing factors which might affect the nature of the conclusion drawn. This is much more useful and, I think, faithful to the term's typical usage.

separately.

This will become clearer with an example using the *ceteris paribus* clause. *Ceteris paribus* clauses are one of the chief ways economists carry out the method of isolation. For example, when we say that a decrease in trade tariffs will increase imports *ceteris paribus*, we can be said to be isolating the effect of a decrease in trade tariffs on imports. This is similar to most legitimate isolations in economics. The causal factor (lower trade tariffs) encourages economic agents to alter their behaviour in a certain way (increase their purchases of imports); however, other factors (such as whether the economy is expanding or contracting) will alter the nature of this effect, perhaps even overwhelming it entirely and leading to a reduction of imports. A decrease in the tariff rate will encourage economic agents to purchase more imports, even though we might actually observe a drop in imports after the tariff reduction for other reasons. The causal mechanism is not dependent on any simplifying assumption associated with *ceteris paribus* clause. Were the relaxation of the *ceteris paribus* clause to make lower tariffs actually tend to cause a decrease in imports, then the isolation would not be legitimate. But we could try to exclude this scenario with a domain assumption. Note that the existence of the causal mechanism does depend on the truth of the domain assumption that the money spent on Giffen goods in the imported bundle be not too large. In any case, this was not included in the restrictions imposed by the *ceteris paribus* clause.

In other cases, invoking a *ceteris paribus* clause may produce erroneous results. It is illegitimate to analyze the effects of a change in real wages or in the savings rate on either employment or investment by only taking into account the impact of these factors on the supply side alone by invoking a *ceteris paribus* clause to hold demand side factors constant. Changes in real wages and the rate of savings have important demand side effects that need to be taken into account when analyzing their effects on either employment or investment. By taking into account

the supply side only, we may be inclined to say that there is a causal mechanism between increases (decreases) in the real wage and decreases (increases) in employment. However, the demand side effects of the change in real wage may destroy this causal mechanism entirely. The idea is that to know whether a real causal mechanism has been isolated, we must ask whether it still holds up despite the simplifying assumptions. We must go through this process of asking whether the conclusion is affected by the simplifying assumption.<sup>39</sup> It amounts to thinking about how the relaxation of the simplifying assumptions affects the conclusion.<sup>40</sup>

The difference between successful and unsuccessful isolations is the difference between whether the isolated cause combines, to use J.S. Mill's (1843) terms, "mechanically" or "chemically" with the factors left out of consideration. When the isolated cause combines mechanically, its effects can be "added up", so to speak, with the effects other causes left out of consideration. The final effect can be taken as the sum of the effects of each cause taken singly. "On the other hand, when causes are combined 'chemically,' some qualitatively novel, emergent

---

<sup>39</sup> We cannot simply assume a false assumption will generate a successful isolation. Consider for example, Frank Hahn's comments on the rational expectations assumption: "What a rational expectations theory provides is an understanding of an imagined economy which satisfies the assumption. As such it may be of great use. For instance, it allows us to study pathologies which cannot be traced to expectational mistakes. Or again it leads to an understanding of how market variables by revealing information to the uninformed reduce (or negate) the benefits of special information. Or yet again it allows us to grasp the informational disturbances introduced by an unknown monetary policy. It is a vulgar misunderstanding of theory and its aims to dismiss Rational Expectations theorising because of its assumptions. I repeat again that I do not hold this on 'as if' grounds" (Hahn 1985: 11-12). In effect, his claim is that using rational expectations allows us to isolate the effects of certain variables. But this cannot simply be assumed. Individuals certainly do not form their expectations according to rational expectations as it is commonly understood. This means following the procedure described above: grasp the nature of how expectations are really formed and then ask how this difference affects the conclusions drawn. In so far as this is not done, there should be little confidence in the validity of the conclusions of such theorizing, unless strongly validated by direct empirical tests.

<sup>40</sup> Cf. Gibbard and Varian's advice concerning the testing of models discussed in the first chapter.

outcomes ensue" (Mäki 1992a: 349). Of course, in economics we cannot usually specify relations so precisely that we can figure out the exact outcome by adding up their effects. But the point is the same.

#### 4.4 The revision of claims to take into account false assumptions

When simplifying assumptions do affect a conclusion or when a false assumption is a necessary assumption for a claim, we can try to revise the conclusion or claim to take the false assumption into account.

One way to revise a claim is by adding a proviso that states that the relationship expressed by the conclusion is an approximation. Normally when we say of a conclusion that it is an approximation, we do so because an assumption is also an approximation. A good example is Galileo's law of falling bodies. The law states that  $s = \frac{1}{2}gt^2$ , where  $s$  is distance travelled by the body,  $t$  is the time,  $g$  is the gravitational constant. This claim depends for its validity on a number of simplifying assumptions: the air pressure is zero, all other gravitational forces are nil, all magnetic forces are zero, amongst other assumptions (Mäki 1994: 240). Despite the fact that Galileo's law depends upon these simplifying assumptions, there is good reason to believe that the law is an approximation if air resistance is small enough and there are no powerful magnetic forces in the vicinity. The distance travelled will be approximately equal to that given by the law. In other words, adding the proviso that it is an approximation under certain condition can make Galileo's law a valid claim: " $s = \frac{1}{2}gt^2$  approximately, under certain conditions". The conditions are a low air resistance and low magnetic force. This is convincing because we know that as air resistance and magnetic forces approach zero, Galileo's law will give a very close approximation of the distance travelled. There is some vagueness in the proviso "approximation, under certain conditions," but not so much as to not make it meaningful. It is possible, given a certain air resistance and magnetic

force. to give more precise boundaries on the deviation between the value given the theory and the true value.<sup>41</sup>

It is sometimes said that the profit maximizing condition will yield approximate results. To determine this we must ask ourselves whether the profit maximizing condition will generate conclusions that are approximate compared to what the real-decision-making procedure yields. We must analyze how the difference between the simplifying assumption and the true state of affairs corresponding to it, affects how close the conclusions drawn with the simplifying assumption come to reality. This is what is going on when we call Galileo's law an approximation. Unfortunately, I suspect that the assertion that profit maximization will yield approximate results is *not* rooted in this type of analysis. It is rooted in the fact that it supposedly approximates the causal effects of self-interested behaviour. This is a very questionable assumption, as I will discuss further in the next chapter.

Another example of how to go about revising a claim is provided by the absolute version of the purchasing power parity theorem (PPP). Let us begin with a statement of the theorem. The exchange rate ( $e$ ) between two countries is in its long- or medium-term equilibrium when it is equal to the ratio of the price levels in the two countries:

$$e = P/P^*$$

where  $P$  is the domestic price level,  $P^*$  is the price level in the foreign market.  $P$  and  $P^*$  use exactly the same weights and each includes all and only goods that are traded between both countries, and  $e$

---

<sup>41</sup> I use a physics example because I am hard pressed to come up with a good example from economics. Certainly, many econometric relationships are approximations. But this is known. I believe, only through comparison with the data; the error is sufficiently small. It is not a matter of thinking about how assumptions by being approximate may affect the difference between how the real relationship is observed and how it is expressed by the conclusion. It could be that approximations are relatively rare with theoretical or deductive models.

is the price of one unit of foreign currency. This theorem rests on one false or idealizing assumption: Arbitrage is costless among and within all countries that have economic relations between the two countries (i.e., there are no trade restrictions, transportation costs are zero, there are zero costs to information, sales taxes are identical, and so on). This is a necessary assumption for  $P/P^*$  to be a unique long- or medium-term equilibrium rate of exchange. It is not a short-term equilibrium, because we have a concept for the short-term equilibrium already. This is what balances the demand and supply of the currencies under a free-floating exchange rate at any given time. Furthermore, even if arbitrage costs are zero, it takes time for the exchange rate to come to equality with  $P/P^*$ . For convenience, I will just call this a long-term equilibrium.

If arbitrage costs are zero, then any differential in the exchange adjusted price between the two countries will, in time, disappear due to price competition. Consequently, in equilibrium, the ratios of the price of each good in the foreign market to its price in the domestic market will all be the same and equal to the exchange rate.  $P/P^*$  is not fixed, and it is not assumed that prices are constant in either country and unaffected by the exchange rate. Rather, prices and the exchange rate adjust until  $e = P/P^*$ .

Now consider the implications of relaxing the necessary assumption of zero arbitrage costs. The question is whether we can still say that, when the exchange rate equals  $P/P^*$ , it is in a long-run equilibrium. To do this we must take into account other factors that have an effect on the exchange rate besides changes in the prices of tradable commodities. These include capital flows, service payments, and autonomous changes in the demand for imports. All of these factors may bring the exchange rate away from  $P/P^*$  for persistent periods of time. There is an argument that if these factors have a neutral effect over time, then we could still regard  $P/P^*$  as a long-term equilibrium. In fact, Officer (1982: 124), in his book length study of the purchasing power parity theory, defends

$P/P^*$  as the primary determinant of the long-term equilibrium value of the exchange rate on the grounds that these other factors are cyclical and will not persist in one direction for extended periods of time. In effect, they are neutral, over time, with regard to the long-term equilibrium value of the exchange rate.

Even if this were true, however, there would be no tendency for the exchange rate to equal  $P/P^*$ . Arbitrage costs are not zero; hence, there will be a number of stable outcomes for the exchange rate in regard to price competition. The exchange rate can come to rest well away from  $P/P^*$  because non-zero arbitrage costs means that price competition is no longer a force that will push it to that value. The force of price competition cannot define a long-term equilibrium value for the exchange rate if arbitrage costs are not zero. Now without  $P/P^*$  defining the long-run equilibrium value, there appears to be no other plausible candidate for such a centre of gravitation. We would need to identify a value towards which the exchange rate has a tendency to gravitate to over time. The idea is that there are forces that will tend to push the exchange rate to this value, since all disturbing forces are temporary.

Officer (1982: 15) defines the long-run equilibrium rate of exchange as "the fixed rate that yields balance-of-payments equilibrium over a certain time period" (He means beyond the very short term, since, of course, there is a fixed rate that yields a balance-of-payments equilibrium in the very short run). But this is not a good definition. Suppose that we assume that this fixed exchange rate has not affected the demand and supply of currency, then under a floating exchange regime this long-run equilibrium rate would be the exchange rate at the end of some arbitrary period, assuming no intervention by the central bank. However, this so-called equilibrium rate is no more than what the myriad of forces affecting the balance-of-payments have produced at the end of this arbitrary period. The next day, or even in the next hour, this so-called equilibrium value will be

different. To identify a long-run equilibrium rate we must identify a force that makes this long-run rate a centre of gravitation. We must have some reason to believe that it is a centre of gravitation, like the concept of the normal rate of profit.

If, however, the notion of a long-run equilibrium value were a meaningful concept in regard to the exchange rate, then it is safe to say that  $P/P^*$  would be an important determinant of this value. The long-run equilibrium value is a function of  $P/P^*$ . Similarly,  $P/P^*$  is a significant determinant of the short-run equilibrium value of the exchange. This is how we can revise the PPP theorem to make it an empirically valid claim. In fact, this is how Officer (pp. 16-17) makes sense of the absolute PPP theorem, as a determinant of both the long- and short-run equilibrium value of the exchange rate. However, he even goes farther and says that these equilibrium values are principally determined by the absolute PPP (p. 17). The absolute PPP theorem does not on its own define either the long- or short-run equilibrium value for the exchange rate.

We must not forget that the exchange rate has an important effect on prices so that  $P/P^*$  is not exogenous with respect to the exchange rate. Hence, the idea that the short-run equilibrium value of the exchange is a function of  $P/P^*$  is nothing more than saying that relative prices of tradable commodities have an important impact on the equilibrium value of short-run exchange rate, and vice versa. To this theory, we can also attach the claim that these effects will be greater when comparing countries that have strong trading relations, i.e., when trade between the two countries is a significant part of either countries GDP (e.g., Canada and the United States).<sup>42</sup>

---

<sup>42</sup> Once arbitrage is no longer costless, we face the problem of what weights and what commodities to use in calculating the price levels in each country. I defined the weights as equal, as this was a necessary condition for  $P/P^*$  to be an equilibrium value for the exchange rate. Other than this, the weights did not matter. But now that arbitrage is not costless and  $P/P^*$  is not an equilibrium value for the exchange rate it no longer matters that the weights are identical in both countries. In fact, the best scheme seems to be for the weights in each country's index to express the share of each tradable commodity in that country's combined exports and imports. The task is to construct a

When researchers state that the absolute PPP theorem holds when say deviations from the exchange rate are not more than ten or twenty per cent. all they are asking is whether the short-run equilibrium rate is predominantly a function of  $P/P^*$ . In my interpretation the absolute PPP theorem is false: it is not a question of whether it holds. It does not hold. However, it is true that the exchange rate is a function of  $P/P^*$ . The important question is how strong is this relationship. This is to be answered empirically, and that is all researchers are doing when they are engaging in this type of empirical work on the purchasing power parity theorem. They are not testing it, because the theorem is false. It is to avoid such confusions and silly debates over whether the theorem holds or not that such an evaluation of the simplifying assumptions underlying a claim can yield. It tells us how we can formulate a true claim out of the PPP about the relationship between the exchange rate and prices. It also, I think, yielded the valuable insight that there does not exist a long-term equilibrium value for the exchange rate. It is only a meaningful concept when arbitrage costs are zero.

---

price index that would have the greatest impact on the equilibrium value of the exchange rate. It might even be worthwhile to include certain service items. In any case, the construction of such indices only really matters if we want to empirically evaluate the strength of the relationship between the exchange rate and  $P/P^*$ .

## **5 Some remarks on the method of orthodox economics in relation to the question of unrealistic assumptions**

I have already discussed the dominant methodological view that theories are tested by their empirical implications and the fact that this becomes a justification for not taking seriously the problem of unrealistic assumptions in economics. And, despite this official methodology, the practice of deductively supporting claims is also prevalent in orthodox economics, so that there is a gap between rhetoric and practice. But if deductive derivations are not a means of justifying claims, then what is the purpose of it? There are two reasons that could be given. Consistency with certain assumptions could be argued to be important. A second reason is that a certain assumption may be seen to be an effective technique for the discovery of claims. Both of these depend on there being a set of foundational or fundamental assumptions. In neo-classical economics the principal assumption is constrained optimization. Another principal assumption is that in the long-run there is full employment of resources. However, even this one is thought to follow from the assumption of constrained optimization. Consistency and effective technique are alternative justifications for making the assumption of constrained optimization in regard to a particular theory. The demand for consistency bars alternative theories based on other assumptions. The idea that making a certain assumption is useful (i.e., an effective technique) is not so strong: it does not bar alternatives. But the effect is much the same. Only derivations based on the useful assumption are made.

The question arises, then, as to why the assumption of constrained optimization is thought to be so important or useful. The principal reason is that it is believed that constrained optimization is in some way equivalent to self-interested behaviour. In this way, theories that are not consistent with constrained optimization are not consistent with self-interested behaviour. Alternatively, the

method of constrained optimization can be seen to be a useful technique for studying the implications of self-interested behaviour.

There is another justification for the method of constrained optimization as a useful technique. This is that it facilitates prediction. This is supplementary to the first.

### 5.1 Self-interested behaviour implies the method of constrained optimization

There is a widespread belief amongst orthodox economists that economics is studying the consequences of self-interest, and that self-interested action implies the use of the method of constrained optimization. The first part of this is not all that controversial: it is the belief that economic action, by and large, is rooted in self-interest. The controversial part is the second part: it is the idea that self-interested agents are constrained optimizers (this is the cruder version of the relation between self-interest and constrained optimization, which is widely held: I will discuss a more sophisticated version below). Any action not in accordance with constrained optimization, according to this perspective, is not in an agent's self-interest. After all, what is an agent's self-interest but to maximize his/her utility and wealth? When the agent is not maximizing as such, he or she is acting irrationally. Quite frequently economists call non-optimizing decision-making procedures, such as cost-plus pricing, "irrational". When they do this, they are implicitly making this link.<sup>43</sup> Not optimizing can only be irrational if it is not in an agent's self-interest. Irrational behaviour is always, by definition, not in one's self-interest.

---

<sup>43</sup> Even if the term "irrational" were used in a purely neutral manner to describe non-optimizing behaviour, it would still leave the question of why this term is chosen in particular for this task when clearly the notion of irrational behaviour always implies or suggests behaviour that is not in one's self-interest. Why not call it instead "bounded rationality" or just "non-optimizing behaviour"? Consider the following quote by Austin Robinson (1984: 231): "We were, it is true, shocked at being told that rational behaviour, as exemplified by balancing marginal cost against marginal revenue, was less desirable than irrational behaviour."

In this methodological perspective, models that have assumed optimizing behaviour are superior to models that assume cost-plus pricing or bounded rationality generally, because these latter assumptions presume irrational behaviour, i.e., behaviour that does not flow from self-interested agents. According to this perspective, the first step to approaching any phenomenon is to create a model that assumes optimizing behaviour. The second step is to evaluate whether its predictions are confirmed or not. Since the theory was derived with the assumption of optimizing behaviour, successful prediction is able to confirm the theory and its assertion to causality. The implicit justification is that the assumption of optimizing behaviour reflects real causal factors, namely self-interest. Confidence in the theory stems in part from the fact that it was derived from the assumption of constrained optimization. If there are two competing theories derived from optimizing behaviour, it becomes more complicated. Empirical tests that can discriminate between the theories will be devised. Other criteria may be used as well, especially when empirical tests are controversial. A theory may be rejected for being based on simplistic assumptions, as new Keynesians tend to reject new classical theory.

If the predictions are contradicted by the data, there could be a number of responses. It could be that the data or some simplification used in constructing the empirical test could be deemed to be at fault. Or the problem could be a failure in a *ceteris paribus* clause. If the problem seems to point to the theory itself, and the theory is sufficiently complex and not regarded as fundamental, then it can be faulted and either modified or discarded. The discarding of a theory is facilitated if there is an alternative that assumes optimizing behaviour or, at most, small departures from optimization.

On the other hand, if the theory is a fundamental one, then some attempt will be made to salvage the theory, if only to say that there is no better alternative. A fundamental theory is one that

is almost universally accepted within the orthodoxy and which is an implication of widely accepted assumptions. Hausman (1992: 227-44) provides the case of preference reversals: this phenomenon contradicts the predictions of utility theory. He traces the unsuccessful attempts to explain preference reversals away, and finds a couple of leading participants in the debate not rejecting utility theory because they believe that there is no better alternative.<sup>44</sup>

The connection between self-interest and optimizing behaviour is crucial. It gives users of the method of constrained optimization the conviction that they are dealing with real causes and confidence that their theory will yield good predictions.

Orthodox economists, of course, will admit that in the real world departures from optimal choices actually occur. They admit that agents make mistakes and that agents do not use the optimization procedures to make decisions. However, there is a belief that insofar as self-interest lies behind economic action, these departures from optimal outcomes will over time either tend to disappear or be white noise. These respectively refer to the notion of the optimal choice being an equilibrium concept and central tendency. With the former, outcomes that are not the product of optimal choices are not stable: they are not in equilibrium. We can see this in regard to the standard justification of rational expectations: agents will not persist in expectations that are systematically disappointed. It is *not* in their self-interest. Rational expectations will eventually be realized. Seen in this way, the notion of rational expectations is an equilibrium or steady state concept. The latter, optimal choice as a central tendency, is illustrated by Gibbard and Varian (1978: 670):

"If deviations are random, or more precisely, are not systematic, there might be good reason to have some faith in the conclusions of the model even though the assumptions, strictly

---

<sup>44</sup> See also Keuzenkamp (1994) for a discussion of attempts to explain away disconfirming tests of the homogeneity assumption in consumer theory.

interpreted, are implausible. Perhaps a case in point is the economist's assumption of perfect optimizing behaviour."

The association between self-interest and optimization that I am making here is a more sophisticated version than the strict identification between acting in one's self-interest and optimizing behaviour first given above. It is subtler. It admits that people acting in their self-interest will make mistakes, and may not be always capable of choosing the optimal action.

Orthodox economists tend to be suspicious of models that assume less than full optimizing behaviour, thinking that there may be an explanation in terms of optimizing behaviour. Recall Boland's defence of the constrained optimization assumption: there may be something that the agent is optimizing. There is a tendency to think that an apparent irrational behaviour (anything less than that is not optimization) could really be rational. Nevertheless, small departures from full optimization are sometimes seen to be acceptable. It depends on the predisposition of the economist. Some, like the new classicals, seem absolute in their insistence on the optimizing assumption. Others less so. But, in general, the rule is, "only accept a non-optimizing explanation if a plausible optimizing one cannot be found" (Mayer 1995: 144).<sup>45</sup>

The identification between self-interest and optimizing behaviour explains why it is that users of this method typically believe that they are dealing with real causes in their theories. To put it in another way, it accounts for the fact that users of constrained optimization see it as a way of

---

<sup>45</sup> "A reasonable procedure would be to look first for a thin theory explanation and to use a thick theory only if that fails. This would allow us to cut through the ideological fog and elucidate the underlying economic motive where it does exist" (Mayer 1995: 144). For Mayer, a thin theory appears to be one based on the optimizing assumption along with *only* other standard assumptions of neo-classical economics. But Mayer, being a methodologist, exhibits a more sophisticated understanding than, I suspect, most neo-classical economists: "Admittedly, since there is prestige to be gained, at least among economists, by finding an underlying economic motive, there is a danger that such a motive will be found, even where it does *not* exist, or when it only plays a minor role" (p. 144).

*explaining* phenomena. Explanations are causal accounts. There are a vast number of possible explanations of an agent's conscious behaviour, besides the procedure he or she used to choose that behaviour. Normally, the consequence of an agent's decision is not explained by postulating some mechanism, which, if operative, *could have* produced the phenomenon. Instead, it is to understand the real decision-making behind the phenomenon. However, an exception is made for constrained optimization, since this is thought to be synonymous with acting in one's self-interest. If the method of constrained optimization can be used to predict a phenomenon, it explains it, as long as the simplifications used to construct the theory are satisfactory (however, this depends on the taste of the theorist).

This association between self-interest and optimizing behaviour also explains why orthodox economists, to a great extent, have bought into the new classical insistence that macroeconomic theories should be rooted in microeconomic principles. The key microeconomic principle here is the optimization assumption. To the insistence that these microeconomic principles should include perfectly flexible prices, the new Keynesians have attempted to show that rigidity in prices can be grounded in the optimization assumption. Namely, rigid prices can be shown to be in the self-interest of agents. If constrained optimization were really just a tool for the discovery of theories, then this demand for microfoundations would not have had the same force. The idea that assumptions such as mark-up pricing and inflexible prices are *ad hoc*, as the new classical maintain, is based in the idea that they are irrational. They are not grounded in self-interested behaviour.

This (partial) account of the methodology of orthodox economics is very similar to Hausman's exposition of the methodology of neo-classical economics reviewed in the first chapter. I include here both his specification of economics as an inexact science and as a separate science.

but not his specification of an inexact law as a *ceteris paribus* law. The idea of an inexact law does fit somewhat with my account. Insofar as economists do admit to some irrationality, i.e. selection of non-optimal choices, on the part of agents, and they assume rationality, then their laws will be inexact. Although Hausman does not want to view it this way, economists, I think, tend to see inexactness as approximation.<sup>46</sup> Nonetheless, the important point of Hausman's contribution is his emphasis on the deductive method supplemented by prediction as a test of theories. This describes neo-classical economics very well.

However, his discussion does not adequately emphasize the importance of the idea that self-interest implies constrained optimization has in orthodox economics. For Hausman (1992: 95), "[neo-classical] economics studies the consequences of rational greed." This is not quite accurate. There is something more basic than this: economics studies self-interested agents. This in turn implies optimizing behaviour. But partisans of the method of constrained optimization will at times admit explanations based on non-optimizing behaviour. In the more sophisticated version, there is no strict association between self-interested behaviour and optimization. In this way, explanations based on non-optimizing behaviour could be disequilibrium accounts. Secondly, an orthodox economist might be willing to admit that in some circumstances optimizing behaviour is prohibitively difficult or that agents are prohibited from acting on their optimizing choices due to, for example, the existence of unions. The attraction of a non-optimizing account is enhanced when an adequate optimizing account is not at hand. Thus, Hausman overemphasizes the centrality of the notion of rationality. Sometimes a phenomenon may be modelled on the assumption that the underlying behaviour is irrational, i.e., the behaviour is not optimizing. Orthodox economists are

---

<sup>46</sup> In defining an inexact law as a *ceteris paribus* law, Hausman is trying to justify at least one way in which a law can be an approximation. His purpose is prescriptive, not descriptive.

more willing sometimes to relax the rationality postulate than his characterization suggests. If economists see themselves at root as studying the consequences of self-interested behaviour and not just rational behaviour, then they will allow, as they sometimes do, for non-optimizing behaviour.

The crude and strict association between self-interested and optimizing behaviour is easy to rebut.<sup>47</sup> The only way acting on non-optimal decisions implies not acting in one's self-interest is if the agent is capable of identifying the optimal action. In other words, acting in one's self-interest only implies optimization where there is the ability to identify the optimal solution. However, this condition is rarely fulfilled. Agents face two constraints: they can *lack the ability* to identify the optimal action, and they can *lack the information* necessary for choosing the optimal solution (See Simon 1967: 241-56). It is no good to argue in response to the latter constraint that individuals will go about gathering information until the marginal expected benefits equal the marginal expected costs. As is well known, there is problem of infinite regress here. We need to collect information about the expected costs and benefits of gathering more information. Then again, we will have to collect this information until the marginal expected costs and benefits are equated, and so on. Furthermore, the whole procedure is clouded over by the uncertainty that results from having to use incomplete knowledge to calculate the expected marginal benefits and costs. This uncertainty will often, and justifiably, give the sense that this procedure of optimization is useless.

Once we recognize that agents are unable to identify the optimal solution, acting in one's self-interest is compatible with the notion of bounded rationality. Acting in one's self-interest means that one chooses the course of action that *one thinks* that is in one's self-interest. If we are unable to identify the optimal action, then what we think is in our self-interest may clearly not be the optimal

---

<sup>47</sup> We could also rebut the strict association between economic action and self-interested behaviour.

action. Bounded rationality is not irrational. as neo-classical economists imply when they call anything less than non-optimizing choices irrational choices. That is why it is not called "bounded irrationality."

This is only to rebut the idea that acting in one's self-interest is synonymous with successful optimization. There is the other, more subtle idea, which admits that agents will not always be successful optimizers, but nonetheless sees optimization as an equilibrium concept or central tendency. It is difficult to see why this would be true if agents cannot identify the optimal choice. What would make actual choices tend toward the optimal choice or be somewhat evenly distributed around the optimal choice? If agents cannot identify the optimal choice, they would be incapable of correcting their errors. If no deliberate account is taken of the optimal choice, agents will be unlikely to tend to choose the optimal choice as a mean. It would only happen by chance. The notion of the optimal choice being an equilibrium concept or central tendency is superficially attractive, but does not seem to be justifiable without direct empirical demonstration. I should add that even if actual choices were to equal the mean of optimal solutions over time, it is not clear whether the assumption of constrained optimization would still not distort the conclusions, especially if the analysis were short-run, whereas the time it takes for this equality to come about is in the long-run.

There is yet another way of seeing a relationship between self-interest and optimizing behaviour: self-interested behaviour will be *approximately* the same as the optimal solution. However, again, why would we assume this if the agent does not pay attention to the optimal solution? Furthermore, this should be demonstrated and not, as is often the case, just assumed.

Finally, this association between self-interest and the constrained optimization assumption sets the stage for the uncritical use of other simplifying assumptions. Certain simplifying

assumptions ensure the applicability of the constrained optimization assumption. Chief among these are simplifying assumptions concerning knowledge, such as the assumption of full knowledge or the assumption that agents know the probabilities associated with outcomes of alternative courses of action. Other simplifying assumptions, like perfectly flexible prices or in the long run there is full employment of resources, are sometimes thought to be implied by the optimization assumption. Of course, the new Keynesians have shown that rigid prices are entirely compatible with the optimization assumption.

## 5.2 Simplifying assumptions facilitate prediction

Simplifying assumptions are an important device for generating predictions. Not to rely on a particular simplifying assumption in generating a prediction would require that we fill in the gap left by this simplifying assumption with actual empirical data. Collecting this can be quite difficult and burdensome. It is easier to use a simplifying assumption. As long as this simplifying assumption does not distort reality too much, the resulting prediction will be close to what is actually observed. Simplifying assumptions liberate the researcher from having to accumulate situation specific knowledge in order to generate predictions in regard to those situations. Deriving a prediction requires significantly more understanding of the situation than revealing the existence of a causal mechanism or of making some judgement of the importance of a causal mechanisms in a given type of situation. Simplifying assumptions, in effect, help to solve the principal problem faced when generating a prediction: lack of information. Of course, there is always the problem that one's simplifying assumptions might distort reality so much that they lead to poor or useless predictions.

The introduction of a simplifying assumption can not only facilitate the generation of a prediction, it can also, conceivably, help yield a more precise prediction. This sounds paradoxical.

but it is not. It is the consequence of insufficient information, so that if we had not relied upon a simplifying assumption, we would be limited in our ability to generate a more precise prediction. Using a simplifying assumption, after knowing about the true state of affairs corresponding to that assumption, does not yield a more precise prediction.

Predictions can be more precise in two senses. A prediction of the sort "if  $x$  then  $y$ " is more precise than the prediction, "if  $x$  then  $y$  only if there are no disturbances." The imprecision of this latter type of prediction comes from the fact that we do not know very well how disturbances may obstruct the regularity. In effect, the problem is that we cannot say with certainty whether  $y$  will happen if  $x$  occurs. Yet, we may have some idea of the likelihood of  $y$  given  $x$ . The stronger this likelihood, the more precise the prediction. The second sense in which a prediction can be more precise is where the restriction it imposes on a variable is sharp, i.e., defined by an equality. The contrasting case is a prediction that defines whether a variable will increase or decrease when another one changes in a certain way.

Once we reach a certain level of understanding of empirical reality, and incorporate this into our theories to make a prediction, then the superior predictive precision offered by the corresponding simplifying assumptions becomes an illusion. Of course, the simplifying assumption may not have offered anything we could legitimately call predictive precision in the first place. But if it did, then once our understanding of the corresponding true state of affairs reaches some point, the simplifying assumption would cease to offer superior predictive precision. To illustrate this, consider the example of profit maximization. Suppose we have no understanding about how a firm actually makes their pricing decisions. Clearly, using the assumption of profit maximization is better than no knowledge at all about how firms set prices. We may actually get some predictive success by using the assumption. Nevertheless, once we accumulate enough knowledge about how

a firm actually makes pricing decisions (usually cost-plus pricing), we will be able to improve on the predictive success offered by the simplifying assumption of profit maximization. This assumption may *appear* to give more precise predictions compared to cost-plus pricing. However, this may be only apparent, and not real. The theory based on the simplifying assumption may be stated as though it is more precise than the theory based on the more realistic assumption, but the latter may actually give the more precise predictions in either of the two senses defined above.

It is the use of simplifying assumptions that makes neo-classical economics *seem* more capable of generating predictions and, moreover, precise predictions, compared to alternatives such as Post-Keynesian and institutional economics. Since these schools of thought are generally unwilling to use simplifying assumptions to the same degree as the neo-classicals, being concerned that this will distort the nature of conclusions drawn, they do not have the same *prima facie* predictive power. This *prima facie* predictive power, in fact, is one of the attractions of using simplifying assumptions, and becomes a justification for not taking a more critical attitude toward simplifying assumptions. *Prima facie* predictive power does not necessarily translate into real predictive power.

One important aspect of the situational independence that simplifying assumptions garner in neo-classical economics is the uniform treatment of agents. The assumption of constrained optimization assumes away differences among agents, and makes the model less dependent on the agent's particular choices. This is the difference between constrained optimization and bounded rationality. With the knowledge of agents taken as given (the usual assumption is full knowledge; in any case, simplifying assumptions are typically used here even if full knowledge is not assumed), then we only need to know the goal of the agent. The goal is usually to maximize profit, utility, or some other variable. With bounded rationality, by contrast, we need to understand what decision-

making procedure the agent actually uses. This depends on the agent and is variable among agents.

Hence, constrained optimization can potentially help us yield a prediction or a more precise prediction, if not enough is known about the agent's actual decision-making procedure. However, it can also give the illusion of generating a more precise prediction, because in contrast with bounded rationality, it may give a *prima facie* more determinate result. With cost-plus pricing, for example, it may not be clear whether and when a price change will ensue from changes in costs. With profit maximization, by contrast, the change in price is instantaneous, or at least, there is always some precise point where it becomes profitable to change the price. The point is that profit maximization tells us precisely when and how prices will change when costs change. Cost-plus pricing is much more ambiguous about when a price change will occur. However, this supposed superior predictive ability of profit maximization is not real. In reality, price changes do not occur as soon as costs change or when the revenue lost from not changing the price exceeds the cost of changing it. It might be better to use cost-plus pricing as a means to predict the frequency of price changes to costs, perhaps with a bit of extra information about how firms respond to changes in costs. As a final point, this uniform treatment of agents also gives the analysis an air of certainty, because we do not have to be concerned that the decision-making procedure that we assume about agents may change or be concerned that it is applicable in regard to a certain set of agents.<sup>48</sup>

Lawson (1997) also articulates this idea that simplifying assumptions facilitate prediction with his critical realism. He expresses this in terms of the notions of closed and open systems. To repeat from the first chapter, closed-system theorizing is the positing of deterministic and stochastic

---

<sup>48</sup> "If we adopt a thick theory anchored in the latest findings of psychology, we may have to abandon it if subsequently psychologists change their minds. By using — if perhaps on the 'as-if' principle — a theory so thin that it has very little psychological content we avoid this problem" (Mayer 1995: 135).

regularities, that is, closed systems. The alternative is open system theorizing. This means elucidating causal mechanisms. And if given enough information about these causal mechanisms, we can obtain weak conditional prediction. This is prediction of the form, "if x then y if no disturbances," in contrast to "if x then y," which is strong conditional prediction (Lawson 1997: 286). Strong conditional prediction or, what is the same thing, regularity determinism or closed system is, according to Lawson, a rare occurrence.

For Lawson, closed system theorizing underlies the method of neo-classical economics, and open system theorizing underlies, for the most part, post-Keynesian, institutional, and Marxist economics (p. 247). Now Lawson spends considerable effort in attempting to show that the problem with orthodox economics is its devotion to close-system accounts, i.e., accounts in terms of regularities of events. As he sees it, the problem with orthodox economics is that it is focused on the generation of knowledge in the form of deterministic or stochastic regularities. This has a profound and distorting effect on the method of orthodox economics and, in particular, encourages the uncritical use of simplifying assumptions. Simplifying assumptions garner intrinsic and extrinsic closure. Simplifying assumptions need to be used because regularities are a rare occurrence: closed systems are rare. Rather, open systems are the norm, and the method of economics has to be tailored to it. If economics is oriented to closed system accounts, it ends up with a distorted understanding of the economy, largely through the use of unrealistic assumptions (see p. 109).

This is similar to what I said above about simplifying assumptions creating the illusion of predictive precision. Simplifying assumptions help yield event regularities or closed systems. Hence simplifying assumptions facilitate the generation of predictions and give the theory a certain *prima facie* predictive power. But this is not real predictive power, since in reality closed systems

are rare. In fact open system theorizing, when it allows for prediction, will offer more precise predictions, because the underlying causality is not distorted, as when closed system theorizing is attempted. Nevertheless, there is a problem with Lawson's use of the notion of closed system and hence with his critique of orthodox economics.

The problem is that closed systems are a rare occurrence. The term "closed system" prejudices the issue: it is better to think of a deterministic regularity. Deterministic regularities abound. For example, we can actually define the conditions under which a ball will fall when released in mid-air or we can define the conditions that allow the multiplier effect to take place when an increase in government expenditure occurs. Once we define these conditions, we obtain a deterministic regularity. And as we know, these regularities occur frequently. They are not rare. The problem lies in obtaining the knowledge to specify the conditions. This is often not easy. But in the case of the multiplier it is: the propensity to consume domestic goods or services must be greater than zero, and the economy must be capable of expanding production. We could do the same for the ball released in mid-air — the sum of the force of upward air drafts, magnetic forces, gravitational forces, and so forth would have to be less than the sum of the downward forces. We can elucidate regularities, and they not rare. The trouble is that regularities cannot often be expressed at a high level of generality, since many conditions have to be attached to annul potentially disturbing factors.

Regularities are not rare, just often difficult to define or definable only at a prohibitively low level of generality. It sometimes takes a substantial amount of knowledge to define a regularity, especially one that predicts a sharp restriction on a variable (i.e., a restriction defined by an equality). One alternative is to define, what Lawson calls, a demi-regularity, along with some idea as to the likelihood that we will observe the regularity in a given instance. This enables weak

conditional prediction. We can see this in neo-classical economics with the use of *ceteris paribus* clauses. The clause denotes both the unknown and known conditions that are necessary for a regularity to hold. In fact, this use of *ceteris paribus* clauses suggests that neo-classical economists do recognize that there may be disturbances in the postulated regularities. i.e., they do engage in a form of open system theorizing. And this suggests that it is either ambiguous or erroneous to associate the neo-classicals with closed system theorizing, as Lawson does in his critique of neo-classical economics. Consider also the association of post-Keynesians with open-system theorizing. A deterministic or stochastic regularity seems to be a quite desirable form of knowledge that is sometimes available. I find it hard to believe that the positing of regularities is not an aim of post-Keynesians. This is not to say that it is necessarily the only aim given the difficulty of defining these regularities. But then it does not seem to be the only aim of neo-classical economics either. In sum, the association of closed-system theorizing with neo-classical economics and open-system-theorizing with post-Keynesian economics does not seem to be quite true. This is an additional problem to Lawson's critique of the orthodox economics in terms of the notion of closed-systems.

The association of orthodox economics with closed-system theorizing and post-Keynesian economics with open-system theorizing has an element of truth in it. It is this. Post-Keynesians are more willing to accept a theory that only elucidates the causes behind a phenomenon, and that does not have a great deal of *prima facie* predictive power. The neo-classicals find the *prima facie* predictive power of post-Keynesian economics unacceptable. They believe that they have a method that better allows them to generate real predictions and more precise predictions. i.e., the method of constrained optimization. In fact, the focus of the neo-classicals is to obtain predictive ability, the stronger the better. And they are willing to deploy simplifying assumptions to get it, without, I believe, due attention being paid to how these assumptions might affect the conclusions. The Post-

Keynesians, by contrast, want predictive ability, but they are more careful about simplifying assumptions. They recognize the difficulty of generating regularities. They are more focused on the real causes of phenomena. The more information about causes that can be gathered, the stronger the prediction available. With the focus on prediction, the neo-classicals do not pay due attention to the causes. But this is simply a product of believing that their method does capture the causes of phenomena: it is based in the fact that economic action is rooted in self-interest and that the method of constrained optimization is the proper way of investigating this. To the Post-Keynesians, the method of the neo-classicals is inadequate to understand the causes behind phenomena. The method of constrained optimization is not usually a good way of studying self-interested agents.

It is not that closed-system theorizing is the essence of orthodox economics as such. Rather, it is the willingness to deploy certain simplifying assumptions to facilitate the generation of predictions. Furthermore, there is the belief that the method does properly capture the underlying causal relationships, in virtue of the connection between self-interest and optimizing behaviour. Some economists may forsake causality, but they are few. Critics of orthodox economics, like the Post-Keynesians, are often unwilling to make the same simplifying assumptions for fear of distorting the conclusions. They also recognize the difficulty of generating predictions with the precision suggested by orthodox theory. They reject the connection between optimizing behaviour and self-interest and they reject instrumentalism.

Nevertheless, even if closed-system theorizing cannot be identified as the essence of neo-classical economics as such, we can say that neo-classicals focus excessively on the generation of event regularities. The first step in theorizing should be to elucidate causal mechanisms. If we know enough about the causal mechanisms operative in a given type of situation, we can obtain weak conditional predictions or, what are the same, partial event regularities. Sometimes and only

sometimes, we will have enough knowledge to describe a deterministic event regularity.

## 6 Summary Remarks

In discussing the issue of unrealistic assumptions it is essential that the issue be put in terms of the notion of truth. For the goal of positive economics is to obtain true claims. The question of unrealistic assumptions should always be understood in terms of the question of how false assumptions can yield true claims. True assumptions are never a problem. False assumptions are problematic when they are necessary assumptions and when they are used to justify claims, either inductively or deductively.

Mainstream methodology does not adequately capture the real nature of how claims are justified in orthodox economics, as well as in other types of economics. The confirmation of claims is primarily understood as inductive argument. Deductions are seen not to offer empirical justification, but only seen primarily as a means for the discovery of claims. This is reflected in the idea that the method of constrained optimization provides a useful or effective technique for understanding economic phenomena. Besides useful technique, deductions are also seen to ensure logical consistency with fundamental principles. However, this is always regarded to be distinct from empirical justification: internal versus external consistency. Both of these ways of construing the purpose of deductions hide their underlying empirical justificatory purpose. They are used to establish the empirical validity of a claim. Inductive arguments (especially, testing the empirical implications of a claim) are also crucial. However, these alone are often not enough to have confidence in the assertion of causality and consequently confidence in the fact that the theory will predict successfully in the future.

The use of false or simplifying assumptions in deductive arguments means that it is necessary to evaluate critically whether these false assumptions affect the conclusions. In defence of the use of simplifying assumptions, it is often commented that false assumptions (but usually

referred to as unrealistic assumptions) are frequently used in the natural sciences, with Newton's and Kepler's laws and Galileo's and Boyle's law all serving as common examples.<sup>49</sup> It is true that these laws make implicit or for that matter explicit idealizing, i.e., false assumptions. However, it is also true that scientists have some grasp of possible true state of affairs that correspond to these false assumptions. They also have an understanding of what this difference between the possible true states of affairs and a simplified state of affairs implies for how the observed phenomena will depart from the phenomena as specified by the law or theory. When experiments are run, they can be done with an eye to how the laws interact with the idealizing assumptions: conclusions can be made as to how the simplifying assumptions distort the conclusions of the theory. It is understood that differences between observed relationships and those specified by the law result from the fact that the theory involves implicit false assumptions. Economists, on the other hand, do not appear to have quite the same understanding in regard to false assumptions. Successful attempts to refine the above laws, by Newton in the case of Galileo's law and Kepler's laws, Einstein in the case of Newton's laws, and Vanderwall in the case of Boyle's law, suggest that natural scientists appreciated the fact that simplifying assumptions underlying the above laws affected them. It is now understood that Newton's law and the others listed above only give approximate results when the conditions sufficiently approximate the idealizing or simplifying assumptions underlying the theory. It is necessary for the economist to have the same sort of appreciation of the effects of simplifying assumptions on conclusions as the natural scientist exhibits.

Taking a more critical attitude towards simplifying assumptions will require not only that

---

<sup>49</sup> Alexander Rosenberg (1992: 58) states that, "Surely, the 'unrealism,' the idealized character of assumptions throughout the most successful theories of physical science, has long ago settled the question of whether unrealistic assumptions are permissible in predictively successful scientific theorizing."

economists appreciate the importance of the deductive method of justification, but that they abandon the idea that the assumption of constrained optimization is implied by the fact that agents attempt to act according to their self-interest. The idea that optimizing and self-interested behaviour are somehow equivalent is erroneous. Taking a more critical attitude towards simplifying assumptions will also require appreciating the fact that the heavy use of simplifying assumptions can create the illusion of predictive power, but be quite inferior in predictive power to the alternative which relies less on simplifying assumptions.

## 7 References

- Bear, D.V.T. and Orr, D. (1967) "Logic and Expediency in Economic Theorizing" *Journal of Political Economy* 75(February-December): 188-96 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar, 1993: 129-37).
- Blaug, M. (1992) *The Methodology of Economics: Or How Economists Explain*. 2nd edition. Cambridge: Cambridge University Press.
- Boland, L.A. (1979) "A Critique of Friedman's Critics" *Journal of Economic Literature* 17: 503-22.
- (1992) *The Principles of economics: Some lies my teachers told me*. London: Routledge.
- Boylan, T.A. and O'Gorman, P.F. (1995) *Beyond Rhetoric and Realism and Realism in Economics: Toward a Reformulation of Economic Methodology*. London: Routledge.
- Caldwell, B.J. (1982) *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: George Allen & Unwin.
- (1992) "Friedman's Predictivist Instrumentalism—a Modification" *Research in the History of Economic Thought and Methodology* 10: 119-28.
- Coddington, A. (1979) "Friedman's Contribution to Methodological Controversy" *British Review of Economic Issues* 2(4): 1-13 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar, 1993: 152-64).
- Cools, K., Hamminga, B., Kuipers, T.A.F. (1994) "Truth Approximation by Concretization in Capital Structure Theory." *Poznan Studies in the Philosophy of Sciences and the Humanities* 38: 205-28.
- Dow, S.C. (1985) *Macroeconomic Thought: A Methodological Approach*. Oxford: Basil Blackwell.
- Eichner, A.S. (1986) *Toward a New Economics: Essays in Post-Keynesian and Institutional Theory*. London: Macmillan.
- Friedman, M. (1953) *Essays in Positive Economics*. Chicago: University of Chicago Press.
- Gibbard, A. and Varian, H.R. (1978) "Economic Models" *The Journal of Philosophy* 75(11): 664-77.
- Gordon, R.A. (1976) "Rigor and Relevance in a Changing Institutional Setting" *American Economic Review* 66(1): 1-14 (reprinted in Samuels, W.J. (ed) *Institutional Economics II*. Aldershot: Edward Elgar, 1988: 124-37).
- Hahn, F. (1985) "In Praise of Economic Theory." *The 1984 Jevons Memorial Fund Lecture*. London: University College.
- Hall, R.L. and Hitch, C.J. (1939) "Price Theory and Business Behaviour" *Oxford Economic Papers*, 2(May): 12-45.
- Hands, D.W. (1993) "Popper and Lakatos in Economic Methodology." in U. Mäki, B. Gustafsson, and C. Knudsen (eds) *Rationality, Institutions and Economic Methodology*. London: Routledge: 61-75.
- Harcourt, G.C. (1972) *Some Cambridge Controversies in the Theory of Capital*. Cambridge: Cambridge University Press.
- Hausman, D.M. (1989) "Economic Methodology in a Nutshell" *Journal of Economic Perspectives* 3(2): 115-27 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar, 1993: 275-87).
- (1992) *The Inexact and Separate Science of Economics*. Cambridge U.K.: Cambridge University Press.

- Hodgson, G. (1989) "Institutionalism and Economic Theory: the Old versus the New" *Review of Political Economy* 1(3).
- Hutchison, T. (1938) *The Significance and Basic Postulates of Economic Theory*. Reprint with a new Preface. New York : A. M. Kelley, 1960.
- Keuzenkamp, H.G. (1994) "What if an Idealization is Problematic? The Case of the Homogeneity Condition in Consumer Demand" *Poznan Studies in the Philosophy of Sciences and the Humanities* 38: 243-54.
- Keynes, J. M. (1936) *The General Theory of Employment Interest and Money*. London: Macmillan.
- Kim, J. (1991) "Testing in Modern Economics: the Case of Job Search Theory." in N. de Marchi and M. Blaug (eds) *Appraising Economic Theories*. Aldershot: Edward Elgar: 105-31.
- Lakatos, I. (1978) *The Methodology of Scientific Research Programmes I*. J. Worrall and G. Currie (eds) Cambridge: Cambridge University Press.
- Langlois, R. N. and Csontos, L. (1993) "Optimization, Rule-Following, and the Methodology of Situational Analysis." In U. Mäki et al. (eds) *Rationality, Institutions and Economic Methodology*. London: Routledge: 113-32.
- Latsis, S.J. (ed) (1976) *Method and Appraisal in Economics*. Cambridge: Cambridge University Press.
- Lawson, T. (1992) "Realism, Closed Systems, and Friedman" *Research in the History of Economic Thought and Methodology* 10: 149-69.
- (1997) *Economics & Reality*. London: Routledge.
- Lavoie, M. (1992) *Foundations of Post-Keynesian Economic Analysis*. Aldershot: Edward Elgar.
- Lavoie and Ramirez-Gastón (1997) "Traverse in a Two-Sector Kaleckian Model of Growth with Target-Return Pricing" *The Manchester School of Economic and Social Studies* LXV(2): 145-69.
- Leamer, E. (1996) "Questions, Theory and Data." in S.G. Medema and W.J. Samuels (eds) *Foundations of Research in Economics: How do economists do Economics?* Aldershot: Edward Elgar.
- Lee, F. (1984) "Whatever Happened to the Full-Cost Principle (USA)?" in P. Wiles and G. Routh (eds) *Economics in Disarray*. Oxford: Basil Blackwell: 233-39.
- (1994) "From post-Keynesian to Historical Price Theory. Part 1: Facts, Theory and Empirically Grounded Pricing Model." *Review of Political Economy* 6(3): 303-36.
- Lester, R. (1946) "Shortcomings of Marginal Analysis for Wage-Employment Problems" *American Economic Review* 36(March): 62-82.
- (1947) "Marginalism, Minimum Wages, and Labor Markets" *American Economic Review* 37: 135-48.
- McCloskey, D. N. (1985) *The Rhetoric of Economics*. Madison: University of Wisconsin Press.
- Machlup, F. (1955) "The Problem of Verification in Economics" *Southern Journal of Economics*. 22: 1-21 (reprinted in Machlup, F. *Methodology of Economics and Other Social Sciences*. New York: Academic Press, 1978: 137-57).
- (1960) "Operational Concepts and Mental Constructs in Model and Theory Formation" *Giornale degli Economisti* 19: 553-59 (reprinted in Machlup, F. *Methodology of Economics and Other Social Sciences*. New York: Academic Press, 1978: 159-88).
- Mäki, U. (1989) "On the Problem of Realism in Economics." *Ricerche Economiche* XLIII(1-2): 176-98 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar, 1993: 193-215).
- (1992a) "On the Method of Isolation in Economics." *Poznan Studies in the Philosophy of*

- Sciences and the Humanities* 26: 319-54.
- (1992b) "Friedman and Realism" *Research in the History of Economic Thought and Methodology* 10: 171-95.
- (1994) "Reorientating the Assumptions Issue." in Backhouse, R.E. (ed) *New Directions in Economic Methodology*. London: Routledge: 236-56.
- Mankiw, N. G. (1990) *A Quick Refresher Course in Macroeconomics*. *Journal of Economic Literature* XXVIII (December): 1645-60.
- Mansfield, E. (1985) *Microeconomics: Theory/Applications*. 5th edition. New York: W. W. Norton & company.
- Mayer, T. (1995) *Doing Economic Research: Essays on the Applied Methodology of Economics*. Aldershot: Edward Elgar.
- Melitz, J. (1965) "Friedman and Machlup on Testing Economic Assumptions" *Journal of Political Economy*. 75(February-December): 37-60 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar. 1993: 105-28).
- Mill, J.S. (1836) "On the Definition of Political Economy and the Method of Investigation Proper to it." in *Collected Works of John Stuart Mill*, vol. 4. Toronto: University of Toronto Press. 1967.
- (1843) *A System of Logic*. London: Longman, Green & Co., 1949 (Reprinted in *Collected Works of John Stuart Mill*, vol. 7-8. Toronto: University of Toronto Press. 1967).
- Musgrave, A. (1981) "'Unreal Assumptions' in Economic Theory: The F-twist Untwisted." *Kyklos* 34(3): 377-87.
- Nootboom, B. (1986) "Plausibility in Economics" *Economics and Philosophy* 2(2): 197-224 (reprinted in Caldwell, B. (ed) *The Philosophy and Methodology of Economics I*. Aldershot: Edward Elgar. 1993: 165-92).
- Officer, L.H. (1982) *Purchasing Power Parity and Exchange Rates: Theory, Evidence and Relevance*. Greenwich Connecticut: JAI Press.
- Pheby, J. (1988) *Methodology and Economics: A Critical Introduction*. London: MacMillan Press.
- Pope, D. and Pope, R. (1972) "In Defence of Predictionism" *Australian Economic Papers* 11: 232-38.
- Robinson, A. (1984) "Comment." in P. Wiles and G. Routh (eds) *Economics in Disarray*. Oxford: Basil Blackwell: 222-32.
- Rosenberg, A. (1992) *Economics—Mathematical Politics or Science of Diminishing Returns?* Chicago: University of Chicago Press.
- Samuelson, P. (1947) *Foundations of Economic Analysis* Cambridge MA: Harvard University Press.
- Senior, N. (1836) *Outline of the Science of Political Economy*, reprinted New York: A.M. Kelley. 1965.
- Simon, H.A. (1967) *Models of Man: Social and Rational*, New York: John Wiley & Sons Inc.
- Weintraub, E.R. (1985) *General Equilibrium Analysis: Studies in Appraisal*. Cambridge: Cambridge University Press.
- Wilber, C.K., and Harrison, R.S. (1978) "The Methodological Basis of Institutional Economics: Pattern Model, Storytelling, and Holism." *Journal of Economic Issues* 12(1): 61-89 (reprinted in W. J. Samuels (ed) *Institutional Economics II*. Aldershot: Edward Elgar, 1988: 95-123).