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Richard Lipsey

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**SOME CONTENTIOUS ISSUES IN THEORY AND POLICY
IN MEMORY OF MARK BLAUG**

by

Richard G. Lipsey

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Emeritus Professor of Economics at Simon Fraser University

1125 West 26th Street

North Vancouver, BC

V7R 1A4

Voice: 604 770-2791

Email: rlipsey@sfu.ca

URL: <http://www.sfu.ca/~rlipsey>

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ABSTRACT

This paper discusses a number of issues covering selected areas of both micro and macroeconomics. Included on the micro side are issues related to modelling the price system, assessing its efficiency, competing models of competition, modelling technological change and economic growth, dealing with risk and uncertainty, and policies related to R&D support, picking winners, and support of infant industries. Included on the macro side are issues related to the alleged refutation of old fashioned Keynesian economies by the stagflation of the 1970s, the use of the Dixit Stiglitz model of monopolistic competition in macro models, the current imperative that all the micro behaviour underlying macro relations be based on dynamic inter-temporal optimization, the downplaying of fluctuations in aggregate demand as a driver of cyclical fluctuations, and the interpretation of the economy's recent behaviour.

Key words: methodology, welfare economics, micro economic policy, uncertainty, R&D, Keynesian economics, New Classical economics

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SOME CONTENTIOUS ISSUES IN THEORY AND POLICY IN MEMORY OF MARK BLAUG¹

Mark Blaug's book *The Methodology of Economics* was one of the most important books on the nature of economic reasoning written in the 20th century. One of its major themes is that if a theory is to be applicable to the world in which we live, it must make statements – predictions – that are at least potentially refutable. In the process, Mark presented cogent critiques of the formalism and methodological permissiveness that increasingly dominate modern economics, two developments that he argued are closely related. In the book's second edition he reviewed many of the debates that were set off by the first edition, refuting, for example, the arguments that falsification of a theory is impossible; that we can have meaningful explanations of economic events without any testable predictions being implied; that the bad practices of some economists imply that it is useless to have good norms; and that because induction is logically impossible there can be no creative leap to new theories based on close observations – a process that he called “adduction”. I would make this book compulsory reading for every economics graduate student and undergraduate major.

In this paper, following in Mark's footsteps, I discuss a number of issues that arise from my survey of some key aspects of modern economics, many of which were also addressed by Mark. Some relate to methodology, some to theory and some to policy. In this necessarily brief survey, I cannot give an exhaustive discussion of any of these issues. Sometimes I merely state them, content to show that there are unresolved issues here; in other cases, I review some of the evidence that suggests a resolution; in yet other cases, I offer my own resolution, at least in broad outline.

I. GENERAL ISSUES

In this section I discuss some issues that are important to subsequent sections. These are still debated by many economists although for both Mark and I they have been settled.

Falsifiability

Not only do I wish that every graduate student in economics would study *The Methodology of Economics*, I also wish the same for journal editors! Indeed, there would be a great advance in the relevance of journal publications if editors would ask the author of each theoretical article: “Tell us what evidence would conflict with your theory or, if there is none, tell us why we should be interested in your theory.”

Issue 1: What can be rejected by a specific set of observations?

In the advice given just above, I make a weaker statement when I say “conflict with” than does Mark near the conclusion of *The Methodology* (1992: 248) when he says: “...the ultimate question we can and indeed must pose about any research program is the one made familiar by Popper: what events, if they materialized, would lead us to reject that program?” Mark's

¹ For their many comments and criticisms, I am greatly indebted to Roger Backhouse, Clifford Bekar, Laurence Boland, and William Scarth. The paper is forthcoming in *Mark Blaug: Rebel With Many Causes*, Marcel Boumans and Matthias Klaes (eds.) Cheltenham, Edward Elgar

injunction is stronger than mine for two reasons. First, Mark refers to a research program in Lakatos' sense of the term, which includes not just its "protective belt" but its inner "core," whereas I refer to an individual theory, which is likely to be only one small part of a larger research program. Second, Mark speaks of facts leading to the *rejection* of a program, whereas I speak of facts *conflicting with* a theory. Both Mark and I accept falsifiability – that a necessary condition for a theory to have relevance to the world in which we live, is that it rule out a non-empty set of empirical observations. But while a single theory can be rejected by the observation of a specific set of facts, whole research programs typically cannot. Instead:

“... when it finally does come, the replacement of the old [research] programme by a new competing programme is not the result of a single crucial test. Instead, in response to the accumulation of anomalies, a broad consensus slowly develops that some competing programme deals with these anomalies better than does the established one. Since judgement is involved, the timing of the replacement is partly a matter of psychology.” (Lipsey 2000:57-8)²

What to test?

Issue 2: Should a theory's assumptions be subject to testing or just its predictions?

If making falsifiable predictions is a necessary condition for a theory to have relevance, is it sufficient? I think Mark would have agreed with me that the answer to this question is “no”. After all, the simple assertion that there will be a recession next year is a testable prediction but not even a theory. Next consider a simple theory of a universal domino effect in human behaviour that predicts a recession the next time any one bank fails in the U.S. This is a testable prediction following from a theory. But it is not necessary to wait to see if there is a recession the next time a bank fails because the theory is driven by a behavioural assumption that is quite ridiculous and obviously counter-factual.

Mark was always critical of what he called (Blaug 1998: 20) “...the license that Friedman's 'methodology of positive economics' gave economists to make any and all unrealistic assumptions, provided only that their theories yielded verifiable implications.” Because a full discussion of this issue (as in Blaug 1992: 91-205) is beyond the scope of the present essay, I mention here just a few critical points that matter for subsequent discussions of assumptions. First, Friedman failed to note the important requirement that assumptions that are patently counter factual be robust in the sense that they can be relaxed without seriously altering the theory's predictions. Second, Friedman's discussion did not recognise the several different senses in which assumptions are used in economics and the different implicit ways in which they should be assessed. Third, since one of the key purposes of behavioural assumptions is to link a theory's predictions to observable behaviour, the view of the extreme irrelevance of assumptions reduces theory to mere operationalism. It then does not make sense to ask which of the assumptions is causing the trouble if a theory that seemed to accord with the facts no longer does so.

² I disused the statement quoted from *The Methodology* with Mark several times and I believe he came to accept that his was an over statement of what he actually believed, which was closer to the position I advocated to him and have repeated here.

But can we test behavioural assumptions? There is space for just one of the many possible examples. It is commonly assumed in micro economics that firms have positively sloped marginal cost curves. Statistically estimated cost curves show, however, that such curves are often horizontal. It makes sense to ask what caused this difference between the assumption and the empirical evidence. It is then found that the assumption is in fact derived from two prior ones: that the law of diminishing returns (or variable proportions) holds and that the fixed factor is subject to an equality constraint so that the firm must use all of it as output is varied over the short run. If so, the ratio of the fixed to the variable factor must vary as output varies, causing costs to vary according to the law. But evidence shows that in many (possibly most) firms the fixed factor is subject to an inequality constraint so that the firm can alter the use of its fixed factor up to its maximum capacity by leaving some it unemployed. In this case, the use of the fixed and variable factors can be altered as output varies, allowing the ratio of the two to be held constant at its optimal value and so avoiding the conditions under which the law holds.

Consider what we have done here. We started by investigating a commonly made assumption about the shape of a firm's cost curve, one that is often used in both micro and macro theorizing. We found that the evidence was in conflict with the assumption. When we sought the source of the problem, we found that this assumption was in fact a prediction derived from two prior assumptions and that one of these, that the fixed factor was subject to an equality constraint, was often not correct. Now we have learned to be cautious of any theory for which the shape of the firms' short run cost curve is a non-robust assumption.

Letting Mark have the last word on this subject (Blaug 1997b: 8):

“Economists are loath to examine their assumptions by the use of survey techniques, by simply asking agents what they believe or what they do, because Friedman's methodology gave economists the false impression that nothing can ever be learned by such means. Perhaps the real trouble is our age-old belief...that...we infer economic behaviour on the basis of some assumptions about motivations and some stylized facts about prevailing institutions, suppressing even the temptation to ask whether these are descriptively realistic assumptions and accurately chosen facts. It is high time economists re-examined their long-standing antipathy to induction, to fact-grubbing, to the gathering of data before and not after we sit down to theorise.”

Intuitive (appreciative) and formal reasoning

Issue 3: Should economic theories be restricted to those that can be formalised in mathematical models?

Richard Nelson distinguishes between 'formal reasoning', arguments that can be put down in mathematical expressions and manipulated formally and what he calls 'appreciative reasoning', arguments that are expressed in words and/or graphs, that follow the rules of logic, and often appeal to empirical evidence. Most mainline modern economics is based on formal reasoning while most economics up until the middle of the 20th century was based on appreciative reasoning, as is most historical and some philosophical analysis.

Much formal theorising in economics is devoid of empirical content as Mark argued in many places (see e.g., Blaug (1998) and as attested by the editor of the *Economic Journal*, when he wrote in his retiring essay (Hey 1997: 4):

“It often appears that the model has been constructed for no other purpose than to produce a result which is a stylised fact observed by the author. Simply producing a model that comes up with a desired result is a test of the cleverness of the author not a test of the relevance of the theory.”

There is insufficient space here to study in detail the issues relating to the contrast between these two approaches, so suffice it to make just a few points. (1) Many of the most important issues of understanding the behaviour of society are not conducive to being modelled in formal mathematical terms: e.g., what were the causes of the First and Second World Wars and of the First and Second Industrial Revolutions? (2) The beginnings of new ideas that can develop into formal theories are often not well enough understood to be modelled formally until they have been refined by rounds of appreciative theorising and appeals to empirical evidence. (3) The usual requirement of formal models is that their assumptions be necessary and sufficient to derive the model’s implications, predictions, or whatever one wishes to call them. But events in the real world are very often over determined in the sense that they are the consequence of a set of conditions no one of which is either necessary, in the sense that if it were not present the event would not have occurred, or sufficient, in the sense that any one of them could have caused the event in the absence of all the others. Say, in a not untypical example, that there were ten factors that contributed to event *A* and that any five of them would have been sufficient to cause the event. Much effort can be wasted when historians argue which of the factors was the ‘real cause’ of the event, while mathematical models that seek to explain the event by a set of equations that express the minimum number of causal factors that are together necessary and sufficient for the event miss the reality of the situation.

Two Types of Policy Implications

In what follows, I distinguish two related types of policy advice. The first, which I call “theory based,” can be formally derived from a well-specified theory; the second, which I call “intuition based,” is not formally derived but is in the spirit of the theory in question and seems reasonable to those who accept that theory.

Issue 4: How should one assess intuition-based policy advice when it follows in spirit, if not strict logic, either from a formal theory whose validity is generally accepted or one whose validity is much debated?

This issue is particularly important because a survey of economists’ advice shows that intuition-based policy advice is commonly and confidently given, although it is often based on theories that are heavily debated such as New Classical or New Keynesian economics.

II. MICRO-ECONOMIC AND GROWTH ISSUES

My reasons for treating micro and growth issues under the same heading will become apparent in what follows.

Two Views of the Economy

In conventional textbooks of economic theory, either a part or the whole of the economy is studied in equilibrium. This may be the static equilibrium of most micro texts or an equilibrium path as in most growth texts. In stark contrast, we see in our world of experience firms competing in a constantly changing world each seeking temporary advantages over their

competitors, often by innovating new products, new processes and new forms of organisation. These activities, particularly those involving technological change, occur under conditions of uncertainty that make it impossible to determine in advance which of two alternatives is the better one.

If we seek to deal with this kind of behaviour, we find that it undermines many of the foundations of Neoclassical economics. Here I state some examples that will be supported later in this Part, both conceptually and by noting important policy implications.

1. There is no unique set of choices that maximize a firm's expected profits. Only after the event might we know which was the better of two alternative R&D plans, each with contrasting implications for future resource allocations and growth experience.
2. Thus competing firms are more correctly visualized as groping into an uncertain future in a purposeful and profit-seeking manner instead of maximizing the present value of expected future profits.
3. Hence there can be neither a static nor a dynamic unique optimal allocation of the nation's resources.

In the following discussion, I use the term *Neoclassical* to refer to conventional economic theory that is largely expressed in formal models that make frequent use of a static equilibrium concept. I use the term *evolutionary* to refer to theories that stress the dynamic view of competition, the importance of endogenous technological change understood at the micro-economic level, and decisions taken under conditions of uncertainty as well as the importance of institutions.³ Although many evolutionary theories are stated in formal terms, many others use appreciative theorising and historical analysis, often because the processes they seek to analyse are too complex to be expressed by current mathematical techniques and often because they deal with complex changes over time. After a long critique of Neoclassical theory Mark wrote (Blaug 1998: 31)

“Among the most hopeful, and I believe most fruitful, developments in economics is the recent growth of evolutionary economics in books [such as Nelson and Winter, 1982 and Rosenberg, 1982] and a series of papers by Richard Lipsey, leading to a forthcoming major work on technical change and growth [Lipsey, Carlaw and Bekar 2005]. The style of all these works is less rigorous, less enamored of precise results, and less inclined to thought experiments employing logical deduction that we are accustomed to from reading mainstream economic literature. But they more than make up for that by their continuous reference to real-world questions in close touch with empirical evidence.”

In other words, they rely as much on appreciative as on formal theorizing, although they do use both.

³ Of course, there are many kinds of non-Neoclassical economics. I use the term 'evolutionary' to refer to those that more or less emphasise the characteristics mentioned in the text, whether or not they employ an explicit set of assumptions concerning the evolutionary process such as mutations and natural selection. For further discussion see the introductory section of Carlaw and Lipsey (2012).

Modelling and Assessing the Price System

One of the great insights of the early economists was that, given the appropriate institutional structure, an economy based on free-market transactions is self-organizing. The key to explaining this market behaviour is that agents respond to the same set of prices, which are determined in markets that tend to reflect the overall conditions of scarcity or plenty. The important caveat concerning the existence of appropriate institutions was de-emphasised over time and by the mid-1950s had virtually disappeared from most micro theory text books. This led many students to draw the inference that the 'miracle of the market' could do the whole job without any human assistance. That such an inference was wrong was forcibly illustrated by the disastrous consequences of the marketization of the former Soviet Union's economy in the absence of many of the needed institutions.

It was easy enough to demonstrate that each individual free market composed of price-taking buyers and sellers would have its own equilibrium and that, given some not unreasonable assumptions about behaviour, each equilibrium would be unique. Going further, however, economists had long assumed, although often just implicitly, that an economy composed of such markets would have a unique overall equilibrium. But they had not, in spite of the heroic efforts of Leon Walras, been able to prove that gut feeling satisfactorily until Kenneth Arrow and Gerard Debreu took up the problem. Using some mathematics hitherto unknown to most economists, they proved the existence of an equilibrium for a model of an entire competitive economy. The conditions that their general equilibrium model required were quite special and many were not found in the real world. As Mark wrote (Blaug 2009: 222) the bout of general equilibrium theorising that this work started off was "...extremely rigorous but, alas, totally irrelevant: it has no empirical content and is incapable of answering any practical question that an economist might want to pose." Nonetheless, it was shown that a general equilibrium could exist in a model that did mirror at least some aspects of real economies. One of the most basic of the many necessary conditions is that all firms be price takers operating in competitive markets, not price setters operating in oligopolistic markets. This was a characteristic of markets as Alfred Marshall modelled them in 1900 but not as Joseph Chandler's *Visible Hand* (1997) showed them to be by the mid-20th century.⁴

Issue 5: What reason do we have to accept predictions made with respect both to the reaction to shocks and the value of various economic policies when they are derived from models that assume the economy has a unique equilibrium that no one has even seriously attempted to prove for a model that displays the variety of market forms – competitive, oligopolistic and monopolistic – and behavioural modes that characterise any modern economy?

I answer that when we use the comparative static properties of an economy-wide competitive equilibrium to generate predictions and policy prescriptions concerning real market economies, we are merely taking shots in the dark since no one has any idea of the comparative static properties of a model that captures the realities of the market structures and behavioural modes that characterise any real economy. (For elaboration see Lipsey 2007.)

That market economies, buttressed by appropriate institutions, are self-organising is beyond question. So is the observation that they are more efficient than the alternative of running

⁴ Of course, the author of *Industry and Trade* knew a lot about non-perfect markets, even though he did develop the theory of perfectly competitive markets.

them solely according to the commands of bureaucrats – a question that was settled beyond doubt in the 20th century by the failed experiments in planned economies in the USSR and its satellite nations.⁵ But just how efficient are the results of that self-organisation is much debated (and, hence, so is the appropriate degree of mix between market determination and state intervention).

Issue 6 Can we learn anything about the efficiency of real-world market economies by studying the efficiency of Arrow-Debreu-style general equilibrium models?

Some economists hold that although this idealised model of the competitive economy bears little relation to the economy in which we live, it nonetheless helps us to understand the virtues of our actual market economy; others hold that this model is of no use in guiding our understanding of any actual market economy. When discussing this issue, Mark wrote (Blaug - 2007: 200): “How can anything that is so patently impractical be a useful reference point? Well, actually, it cannot...”. He went on to refer to economists' schizophrenia in accepting that, although the assumptions required by this model bear virtually no relation to reality, the model nevertheless is in some way useful in helping us to understand reality.⁶ The importance of the model lies, he argued, solely in its “ceremonial value”. I agree but also observe that the extreme importance of this model lies in its underpinnings of intuition-based policy prescriptions – intuition based on hours and hours of hard intellectual work spent in graduate school in proving the optimal properties of this model. “If I was asked to work so hard on understanding this model, surely it must be of some value” is a common reaction of students. I stress that such policy advice is intuition- not theory-based. Many, perhaps most, teachers would argue that they stress the limitations of GE theory in general and the two fundamental theorems of welfare economics in particular. But try as they may, it is hard to undo students’ intuition that the time spent on these theories must mean that they have some policy relevance to the real world – and the favourable comments cited in footnote 6 certainly help to reinforce such intuition.

General versus context-specific advice

Issue 8: Can we expect to have specific items of policy advice that apply virtually to all times and all places or is most, or even all, relevant advice likely to be highly context specific?

Economists spend much time teaching their students that Arrow and Debreu's idealisation of the market economy will not only be relatively efficient but will, in equilibrium, be *perfectly efficient* in the sense that it is then impossible to make anyone better off without simultaneously making someone else worse off. In the Arrow-Debreu, theory-based policy any interjection of market power, whether from governments or large firms, causes markets to work less well and lowers the society's welfare. The intuition-based non-intervention policy, which is commonly but not universally employed by neoclassically trained advisors, makes a critical jump, often

⁵ Another alternative is to run economies along lines set by traditional behaviour. The economic inefficiency of such forms of organisation has been shown beyond doubt by the failure of any traditionally based economy to achieve economic growth in modern times. (This says nothing about the degree of happiness that may be achieved by those living in traditional economies.)

⁶ In his article, Mark contrasts the judgment of Baumol and Akerlof on the real-world inapplicability of the theorems with statements such as Starr's (1997:151) that they provide “...a significant defense of the market's resource allocation mechanism” and Mas-Colell, Whinston and Green's (1995: 556) that they offer “...a strong conceptual affirmation of the use of competitive markets, even for dealing with distributional issues.”

implicitly, of assuming that what is welfare reducing in the imaginary GE economy will be welfare reducing in any real economy.

Piecemeal Neoclassical policy advice was for many years based on the belief that removing a ‘distortion’ that prevented the fulfilment of any one of the conditions needed to produce an optimum allocation of resources in an Arrow-Debreu GE model would raise welfare under all circumstances. This gave rise to the so-called one-size-fits-all policy advice: whenever a distortion is found, remove or reduce it if possible and so increase welfare. *The General Theory of Second Best* (Lipsey and Lancaster 1956) showed that this was not a valid conclusion. What happens to welfare when a specific distortion is removed or reduced depends on the state of everything else in the economy. In other words, the sign of the effect on welfare (or any other objective function) of altering one ‘distortion’ is context-specific.

Lipsey (2007) considers and rejects a number of attempts to avoid this second best proposition by establishing one-size-fits-all general rules for raising welfare piecemeal when a first best optimum cannot be achieved.⁷ For example, in a series of publications Ng (see especially (1977)) has argued that his theory of third best provides a non-context-specific welfare-increasing rule because the probable loss in welfare from a marginal reduction in an existing ‘distortion’ that moves it away from its second best value is less than the probable gains from a marginal move towards that value. However, Lipsey (2012) shows that Ng’s argument requires that the distortion in question be the only one preventing the achievement of a first best optimum. In spite of these general results, intuitive based theory often dominates practical policy in the form of one-size-fits-all policy advice, particularly that given to developing nations.⁸

This discussion illustrates that the refutation of a theory need not always be on empirical grounds. A refutation may instead show on purely theoretical grounds that (1) the theory has some internal contradictions (2) that, as in the present case, it depends on some very specific assumptions not likely to be found in the real world or (3) by producing a counter example that follows from assumptions no less unrealistic than those used in the original theory.

Competition

As suggested above, Neoclassical equilibrium theory concerns the end state that would occur if all change-causing forces were allowed to work themselves out. In a perfectly competitive equilibrium, all firms have access to the same technology, all charge the same price, all just earn the opportunity cost of their capital (often called ‘normal profit’), and there is no incentive either to exit or to enter the industry. In contrast, Mark embraced the Austrian or evolutionary conception in which competition is viewed as a process rather than as an equilibrium end-state, a view that goes at least as far back as Schumpeter:

“...firms jostle for advantage by price and non-price competition, undercutting and outbidding rivals in the market-place by advertising outlays and promotional expenses, launching new differentiated products, new technical processes, new

⁷ Here are some examples of propositions found in the literature that Lipsey(2007) shows to be incorrect : lowering the highest distortion must be welfare increasing; two small distortions are always better than one large one; the optimal level of a policy reduced distortion must fall short of correcting the distortion fully; a small dose of a policy that has some effect on the distorted margin is always better than no policy at all.

⁸ For further examples and criticism of actual one-size-fits-all policies, see Griffiths (2003)

methods of marketing and new organisational forms, and even new reward structures for their employees, all for the sake of head-start profits that they know will soon be eroded.” (Blaug, 1997a: 255)

It is clear that Mark thought that this conception was the more fruitful way to view real-world competition. He wrote:

“If competition really is a process and not just an end-state must we then abandon the concept of perfect competition, the theory of general equilibrium and the New Welfare Economics associated with the perfectly competitive general equilibrium? Yes, precisely. Perfect competition is a grossly misleading concept whose only value is to generate examination questions.” (Blaug 1997a: 255)

Issue 9: Granted that process competition is a better description of real world competition among firms than is end-state competition, is that sufficient to require the use of the former rather than the latter in our models?

The above observation may make one suspicious of results and policy advice that comes from end-state models but I think Mark would have agreed with me in answering that the main reason for preferring the process rather than the end-state formulation is that the former leads to predictions and policy advice that are closer to reality than does the latter, some of whose predictions are completely erroneous and, when applied to policy, seriously misleading.

Issue 10: What are the policy implications of the extensive process competition and the resulting market power that clearly exist in modern market economies?

In Neoclassical economics market power creates "market failure" because in equilibrium the price is too high and the output is too low to be optimal, while in perfect competition price and output are optimal. Textbooks presenting micro-economics typically have a section entitled the "deadweight loss of monopoly" (which in the circumstances means any firm with market power). The study of the theory of monopoly has many valid uses but problems arise when it is used to develop the theory-based policy of making the firms in all such industries behave as if they were in perfect competition – marginal cost pricing being an example. From that follows naturally, if not inevitably, the intuition-based policy to reduce market power wherever possible.

In evolutionary economics, as Schumpeter (1934) long ago observed, firms with market power (clearly the majority of all firms) compete to a great extent in technology using their profits to finance their R&D. For example, the agricultural innovations that allowed Western countries to move within half a century from around 50 per cent of the labour force on farms to less than 5 per cent came mainly from oligopolistic firms in the farm equipment industry, not from price-taking farmers. Chandler (1977 and 1990) provides numerous other examples, while examples of major innovations coming out of perfectly competitive, price-taking firms are hard to find. In this view, the challenge to economic policy is to accept the common existence of temporary market power that is both a cause and consequence of technological change and to keep oligopolistic firms actively competing rather than coalescing into de facto monopolies.

Importance of technological change

A major weapon used by firms engaged in process competition is the introduction of new technologies, such as the computer that replaced the mechanical desk calculator; new processes, such as the modern robotised factory that replaced the Fordist assembly line; and new forms of

organisation, such as the replacement of the old pyramidal form of business organisation by the looser more lateral form that is common today. All of this implies that technology is in fact an endogenous variable that is being continually changed by firms searching for temporary advantages over their competitors. Voluminous evidence shows that these changes occur not randomly but in a path dependent trajectory where what happens today depends critically on what has happened yesterday.⁹

Although most of today's economists would agree that technological change is endogenous to the system, much economic analysis takes place in, and much policy advice is derived from, Neoclassical microeconomic theories in which technology is unchanging. Since such end-state equilibrium analysis requires the absence of forces causing change, consumption preferences and production technologies are typically assumed to be exogenously determined constants.

Issue 11: What are the pitfalls in treating technology as constant in end-state analysis?

One such pitfall is found in the common advice that a developing nation should seek to develop industries in which the country has an existing comparative advantage. The 19th century Scottish born, Canadian economist John Rae (1905) argued that since technology is endogenous, the policy advice of building on one's current comparative advantages, which is an argument for staying with the status quo, is not obviously the best advice. A country's competitive advantages can be changed by public policy.

A second related pitfall is found in the standard Neoclassical infant industry argument for tariff protection. It is designed to assist a new industry in a developing country that is subject to substantial economies of scale when such industries are already established elsewhere. According to this argument, because capital markets are imperfect, the industry needs assistance to grow large enough to move to the bottom of an extended, negatively-sloped, long-run cost curve (i.e., to fully exploit existing economies of scale). Once the industry reaches efficient scale, the protection can be withdrawn.

Since, as recognized in evolutionary economics, technology is subject to continuous, endogenously generated change, effective infant industry protection cannot be seen as merely a matter of moving along a static long run cost curve. Instead it is necessary to establish a dynamic industry that can hold its own in fierce international competition where technological change is one of the main weapons.

The dramatic vindication both of Rae's critique of advice based on existing comparative advantage and of the standard reasons for justifying infant industry protection came not long after the end of the Second World War. Three poor, undeveloped countries, South Korea, Taiwan and Singapore, created the basis for the Asian growth miracle by setting out to create their own comparative advantages in manufacturing industries in which they had no obvious current advantage and in which scale of operations was not the most important barrier to entry.

Their firms, having no initial experience in foreign markets, had to climb steep learning curves concerning such things as product quality, reliable delivery times, marketing techniques and after-sales servicing. The public policies for helping them create viable export-oriented

⁹ Many students of technology have noted the path dependent nature of technological change. See for example Nelson & Winter (1982), Arthur (1994) and Lipsey, Carlaw and Bekar (2005).

industries were complex but they amounted to creating new competitive advantages through rewarding success with many types of continued government assistance while penalising failure by withdrawing that assistance. The Taiwanese government, for example, virtually created its electronics industry in which it became a world leader. In a very few decades, the living standards of these once-poor countries approached those of the established industrialised nations – a miracle of government-managed endogenous technological change.¹⁰

Risk and Uncertainty

In this section I discuss the distinction, well-known but not always remembered, between something being *risky* (i.e., we can calculate the odds on it happening) and *uncertain* (i.e., we cannot calculate the odds). It is a characteristic of risky situations that two equally well-informed maximising agents, possessed of the same knowledge will make the same choice – the one that maximises the expected value of the outcome. It is a characteristic of uncertain situations that the same two agents may make different choices and there is no way, until the results of both choices are known, of deciding who made the better choice.

Research into technological change (see especially Rosenberg 1982, 1994 and 1996) establishes that uncertainty is always present and often pervasive in the search for new technological knowledge. One cannot even enumerate the possible outcomes of various lines of R&D devoted to inventing and innovating some new technology. Large sums are sometimes spent with no positive results, while trivial expenditures sometimes produce results of great value. Furthermore, the search for one objective often produces results relevant to different objectives. Uncertainty is involved in more than just making some technological breakthrough. There is uncertainty with respect to the range of applications that some new technology may have. The steam engine, electricity, the laser, the electronic computer, the Internet and fibre optics are examples of technologies that were initially thought to have very limited potential. Commercialization is another important part of the innovative process that involves uncertainty. Many marvellous technological advances were commercial flops.

Although the term uncertainty is often mentioned in Neoclassical, end-state theories, and occasionally in standard growth models, the models are almost always based on rational calculation to reach maximising positions – something that can be accomplished under conditions of risk but not under uncertainty.¹¹

Issue 12: What are the limitations of formal models based on decisions taken with perfect knowledge or in risky but not uncertain situations?

A major reason for assuming risk and excluding uncertainty is the desire to obtain determinate comparative static (or comparative dynamic) results. Models containing uncertainty are typically non-ergodic without closed form solutions. (See Davidson (1991) for discussion of the importance of ergodicity in equilibrium models.) Instead, they must usually be expressed as simulation models in which two paths that start from the same initial conditions will typically

¹⁰ For further discussion see Rodrik (1993 and 1994), Westphal (1990) and Lipsey, Carlaw and Bekar Chapter 16.

¹¹ As in most of the topics discussed in this paper, there is a massive literature on this issue. For one excellent example see Davidson (1991). Although most of his discussion is related to macro models, it is equally relevant to innovation where, as observed in the text, although agents can and do form some idea of the outcomes of their R&D, they are often surprised in both small and large ways.

develop differently. The theorist's life is certainly more difficult when dealing with non-ergodic models than ergodic ones, but if the former are better models of the world in which we live, it may be misleading to pretend otherwise. (See Carlaw and Lipsey (2011) for an example of a formal growth model that incorporates a substantial element of uncertainty.) Of course, there are circumstances in which ergodic models will predict well while in other circumstances they will not. This raises the next issue.

Issue 13: If many processes in the world in which we live are non-ergodic, in what circumstances can one use ergodic theories successfully?

This of course is a big question that I can do no more than raise here. Note, however, that as mentioned at the outset of in Section II, pervasive uncertainty makes it impossible to determine a unique, most efficient allocation of the nation's resources. When we consider two alternative resource allocations, if we cannot know in advance the set of possible outcomes of each, nor put probabilities on those that we can foresee, then we cannot say which of the two is the better choice. This undermines the whole of Neoclassical theory of an economy-wide, efficient allocation of resources based on equilibrium analysis of competitive economies. I stress economy-wide since if we have a well-defined objective function for specific policies, such as an X per cent reduction in smoke pollution, we can use welfare analysis to study alternative methods for achieving that result. (See Lipsey (2007) for further discussion of when welfare analysis can be used successfully in spite of the problems that arise from uncertainty.)

Modelling Growth

The technological change we have discussed above is the main driver of long-term economic growth. As has been accepted since the early classical economists, growth based on a mere accumulation of capital with constant technology would slow and eventually come to a halt, yielding what they called the stationary state. Because it is driven by technological change, real growth does not just produce more of the same. We have ten times as much real income as our Victorian ancestors, but we do not spend it on more horse drawn carriages and steam train rides to the local seashore. Instead, we spend it on new products, produced with new, more efficient types of capital equipment and new forms of organisation. Most of the products and production processes that we take for granted today would have seemed like miracles to our grandparents and inconceivable to theirs.

The fact that technological change is a major tool of process competition and a major driver of economic growth explains why the two need to be considered together and not as separate aspects of two distinct compartments of economics: price theory and growth theory.

Issue 14: What are the disadvantages of treating the results of technological change and the results of competition as distinct issues when in fact they are interrelated?

Understanding technological change, seen at the micro economic level – something Joseph Schumpeter (1934) thought important and the kind of material that Nathan Rosenberg analyses in his great book *Inside the Black Box* – is the key to understanding long-term growth and growth policy. In contrast, up until very recently standard models of economic growth typically treated technology, whether modelled as exogenous or endogenous, as a single variable or parameter in an aggregate production function. Indeed, far more can be learned about the technologies that drive long term growth – their basic characteristics, how they are developed, how they evolve over time and policies to influence them – from the appreciative theorising in

books such as Rosenberg's than from any formal growth model that treats technology in a typically simplistic fashion.¹²

Issue 15: Is it not very limited, if not downright misleading to allow students to study economic growth without learning anything about the anatomy of its driving forces: the structures of technology and technological change?

Some of the problems raised by the previous four issues can be illustrated by contrasting some of the policy prescriptions derived from Neoclassical and evolutionary theories of growth.

First consider policies for growth in general. According to the most extreme Neoclassical view, governments should ensure that the economy functions well by having a good financial system and effective laws governing such things as the copyrights, the security of property, and bankruptcy. After that, the 'miracle of the market' can be left to do the job. Any direct intervention into how the market allocates resources is to be avoided as causing 'distortions'. This applies to R&D as well as everything else.

Critics, including evolutionary economists, agree that providing these background arrangements is a necessary condition for ensuring sustained growth. But, as we observe below, they argue that doing so is by no means sufficient, more proactive policies are also helpful, even if not absolutely necessary. They also argue that voluminous evidence shows that their view is closer to the truth than the Neoclassical view. At the most general level, evidence shows that not one of the currently advanced countries industrialized under completely free trade. Even Britain, the country that came closest to establishing purely market driven sustained growth, protected its textile industry in the 18th century by banning the importation of Indian cotton goods, restricting manufacturing activities in its colonies, and forcing colonial exports destined for any European country to pass through English ports. (Ormrod (2003) shows that Britain's success owed much to its mercantilist policies.) Germany, France, the United States, and the former British Dominions used tariffs to protect their emerging manufacturing industries. These were reduced and sometimes eliminated *after* the industries had developed over long periods of time. More detailed evidence is cited below.

Many policy disagreements concern two distinct issues: the absolute amount of support, if any, that should be given to R&D and the relative amounts to different types of activity.

Issue 16: How can we determine if the total amount of R&D expenditure is too much, too little, or about right?

Kenneth Arrow (1962) settled this issue to most economists' satisfaction long ago.¹³ His argument, also accepted by evolutionary economists, was that the free market would allocate too

¹² From the original Solow growth model in 1956 until very recently, the strictures in the text would have applied to virtually all textbooks on economic growth. They still apply fairly generally to intermediate macro books such as Mankiw (2010) and to many more advanced books. But beginning probably with Aghion and Howitt (1997) Schumpeterian growth theory has been entering the textbooks of growth theory, although these are still a long way from conveying the sort of empirical knowledge found in the writings of Rosenberg.

¹³ O'Brien (1998) disputes Arrow's result. In so far as Arrow's argument depended on the advantage of being first mover, O'Brien's criticisms are well founded. Voluminous evidence, much of which O'Brien cites, attests that neither first nor subsequent movers in a technological race have an inevitable advantage. Who has the advantage is highly content specific. However, Arrow (1962:319) gave three reasons for under-allocation of resources to R&D: uncertainty and risk, limited appropriability, and increasing returns to use. From an evolutionary point of view, the main reason for agreeing with Arrow is related to the vast externalities and spillovers that a new technology creates.

few resources to R&D. For one reason, those who invent and innovate major new technologies do not reap anything like the full value of the social benefits that they create. For example, major new technologies enable the profitable development of myriad derivative technologies by many firms that were not involved in developing the original technology. So there is a case for using subsidies and tax relief on R&D expenditures to encourage more R&D than the unaided market would provide.

Issue 17: How should we evaluate the efficiency of the allocation of total R&D among its various possible uses?

On this issue the two sides disagree. Because in most Neoclassical treatments R&D is subject only to risk, the probability of success in each particular line of enquiry can be calculated. This allows profit-oriented private firms to allocate R&D money so as to maximise the expected value of all resources devoted to R&D. Any state interference to alter this allocation is a 'distortion' that will lower the expected value of the total effort. Hence non-distorting assistance should be provided in the form of generally available R&D subsidies or tax relief.

Two objections follow from evolutionary theory, the first based on the risk-uncertainty distinction and the second based on a deeper understanding of how technologies change than is found in Neoclassical growth models. First, because voluminous evidence shows that the development of new technologies is subject to major uncertainties, there is no unique discernible maximising allocation of R&D, departures from which are 'distortions'. Second, evidence shows that the degree to which agents can appropriate the value of new inventions varies greatly. For example, firms in industries such as pharmaceuticals, where patents are effective, are able to internalize much of the value that they create – enough to provide strong incentives to innovate. Because their profits are already protected by patents, these industries gain doubly from R&D support. Also as argued in more detail in Lipsey and Carlaw (1996), the inability to keep the results of pre-commercial research secret may lead to too little of it, while ability to keep it secret may lead to an excess of overly duplicative R&D. An R&D subsidy in sectors where firms are hoarding and thus duplicating pre-commercial R&D efforts only aggravates what is often wasteful behaviour. So a generalised form of R&D support is neither efficient nor neutral, since it encourages those who do not need it as much as those who do.

Another policy difference is in the vexing issue of picking winners. By and large Neoclassical economists have been hostile to selecting specific firms, industries or technologies for public support on the above-mentioned theory-based grounds that any non-general support for R&D is a 'distortion'. The intuitive-based argument is that even if it were desirable in principle, governments are particularly bad at it and will mostly make mistakes in directing their support. "Governments cannot pick winners" is a commonly heard statement in this respect.

Evolutionary economists argue that the assumptions of Neoclassical theory are too abstracted from the details of technology to even adequately formulate the issues involved in this debate –an example of non-robust assumptions as discussed in Part I. Indeed, detailed microeconomic research suggests that purely private incentives are insufficient to get many radically new technologies past their early stages of development. Consider some American

For a discussion of the distinction between externalities and spillovers and their importance in economic growth and technical change see Carlaw and Carlaw (2002).

examples.¹⁴ Early on, the US aircraft industry received substantial assistance from a government body that, among other things, pioneered the development of large wind tunnels, and demonstrated the superiority of the retractable landing gear. The airframe for the Boeing 707 and the engines for the 747 were both developed in publicly funded military versions before being transferred to successful civilian aircraft. Electronic computers and atomic energy were largely created in response to military needs and military funding. Early support for the US semiconductor industry came mainly from military procurement whose rigid standards and quality controls helped to standardise practices and to diffuse technical knowledge. The US government's heavy involvement in the early stages of the US software industry produced two major spin-offs to the commercial sector. One was an infrastructure of academic experts, built largely with government funding; the other was the establishment of high and uniform industry standards. The space industry with its myriad industrial spinoffs, that is currently just beginning to be taken over by private firms, was fully created by public funds, in particular those associated with the race to the moon in the 1960s.¹⁵ In his study of the development of American technologies, Vernon Ruttan (2001:vii) shows that "...the public sector had played an important role in the research and technology development for almost every industry in which the United States was, in the late twentieth century, globally competitive."

What these examples show is that American public bodies have had many successes in picking winners. A more difficult question is: Did this backing make a large, possibly crucial, contribution to success or did it merely provide rents for private sector agents who would have succeeded in any case? Detailed case studies such as are found Lipsey and Carlaw (1996), Mowery and Nelson (1999) and Ruttan (2001) make a strong case that compared with leaving the developments solely in the hands of private sector agents (i) in some cases, the development was advanced by decades over what it would have been (ii) in other cases, it might not have occurred within any foreseeable time horizon and (iii) in yet other cases it gave a U.S. industry a critical advantage over competing industries in other countries where they received less support.¹⁶

A similar list covering the successful picking of winners can be compiled for many other countries. I hasten to add, however, that although governments clearly can and do pick winners, it is also clear that they have picked a large number of massive failures. So the real policy issue is to establish the conditions under which government interventions to encourage particular technological developments are likely to succeed or to fail.

Issue 18: Given that both propositions, (i) governments can almost never pick winners and (ii) governments can almost always pick winners, are refuted by voluminous evidence, how

¹⁴ Most of these examples are drawn from Lipsey & Carlaw (1996).

¹⁵ Since most of these U.S. support activities did not take the form of direct subsidies listed in the government accounts, Americans are free to believe the myth that the miracle of the unaided market accounts of their 20th century industrial predominance when in fact it was a judicious combination of public support and private initiative in both the 19th and 20th centuries. Sadly for Americans, all too many of today's US Republicans, bent on minimizing government, do not understand that this public-private cooperation was what lay behind so much of their past successes.

¹⁶ For just one possible example of each: (i) atomic energy, (ii) space technologies (iii) the success of Boeing 707 over the British VC 10, which in some ways was a technically superior aircraft but with a small but significant disadvantage in operating costs.

can one judge the potential for success of new policies whose goal is picking some specific winner, either a firm, or a particular new technology or a more general direction for R&D?

In the kind of exercise advocated by Mark in the quotation given at the end of the “What to Test?” section, Lipsey and Carlaw (1996, 1998a and 1998b) have studied selected government successes and failures in attempts to pick winners and used these as a basis for a series of generalisations concerning the design features of programs and projects that seem to favour success and those that seem to favour failure. Mowery and Nelson (1999) also study both successes and failures in an attempt as they say to make the policy debate “more nuanced and less polarized” (Mowery and Nelson: 15).

Some readers of an earlier version of this paper have argued that if decisions are taken under general uncertainty, all bets are off and there is no way to improve performance beyond random behaviour. This is not so. Just because firms are operating under uncertainty does not imply that they are acting randomly. They make assessments of various outcomes based on their subjective probabilities, past experiences, local circumstances, and the incentive structures that they face. Thus government assistance can design incentives to encourage certain types of behaviour, which raises the question: Are there types of behaviour that make success more or less likely? For example, one of the many examples of design features that Lipsey and Carlaw consider concerns capture. Some designs make capture of a project by producers or consumers more likely than others, Their case studies show that when a project becomes subject to capture, failure is often the direct result. This work leads to an interesting issue:

Issue 19: Are there other better ways than generalising from case studies to learn how to design successful interventions and avoid unsuccessful ones?

III. MACRO-ECONOMIC ISSUES

Keynesian versus New Classical Economics

The prototype of old Keynesianism is the IS-LM model and one of its major novel aspects was to provide a theory of aggregate demand and its fluctuations. In it there was no market-clearing, full-employment equilibrium. Later, the model was closed with a Phillips curve, first simple and then expectations-augmented. Hardly noticed at the time, this Phillips curve closure had the paradigm-changing consequence of introducing into the Keynesian model a full employment equilibrium with the price level changing to remove inflationary gaps (quickly) and deflationary gaps (slowly); in both cases propelling the economy back to its *assumed* unique, full-employment equilibrium¹⁷.

The OPEC oil shock of the early 1970s caused a stagflation with simultaneous increases in inflation and unemployment, a conjunction not seen before in modern times. About that time the New Classical economists attacked Keynesian economics on two grounds

- They asserted that Keynesian economics was decisively refuted by stagflation. Lucas and Sargent (1979) spoke of “the spectacular failure of the Keynesian models in the 1970s” and asked what could be salvaged from the “wreckage”.
- They accused it of having inadequate micro underpinnings.

¹⁷ See Lipsey and Scarth (2011) for a fuller treatment of this issue.

But instead of accepting this “decisive refutation,” Keynesians argued that there was no such refutation. (Indeed, it was even argued that stagflation showed that the assumption of continuous market clearing was demonstrably false.) They then responded to stagflation by extending their model to accommodate it. The IS-LM part was compressed into an aggregate demand curve and an upward-sloping, short-run aggregate supply curve was added.¹⁸ Now exogenous increases in costs such as were caused by the OPEC oil shock, shifted the AS curve upwards, pushing the economy along its AD curve causing prices to rise while output fell. This extension of the model first entered an intermediate text authored by Dornbush and Fischer in the late 1970 and elementary textbooks by Baumol and Blinder and Lipsey and Steiner in the early 1980s. It remains today in most first year principles texts.

Beginning early in the 1980s, at the start of every new edition of our UK and Canadian texts, (of which the latest editions are Lipsey and Chrystal (2011) and Ragan and Lipsey (2011)) my co-authors and I ask teachers if they want something of the New Classical macro model added but we have always been told that the old Keynesian model is good enough for first years.

Issue 20: Is it not strange that a theory that had been “decisively refuted” decades ago is still taught to beginners, many of whom will study no more economics?

My answer to this question is that stagflation did not refute the Keynesian model but only required that it be strengthened on the supply side. This would explain why some teachers want to use IS-LM as at least a first approximation to what they regard as an acceptable Keynesian explanation of macro behaviour.

In the place of the Keynesian micro underpinnings¹⁹, the New Classical economists focussed on a representative consumer who maximized utility over the infinite horizon and a representative producer who was a price taker operating in a perfectly competitive, flexible-price market and later a version of monopolistic competition that was mathematically tractable but which had been abandoned by Chamberlin himself after a debate with Nicholas Kaldor. Both producers and consumers had rational expectations about all future events. Cyclical fluctuations occurred around an equilibrium growth path and were caused by random fluctuations in preferences and technology.²⁰ Since all agents were maximizing under rational expectations, it followed that the cyclical behaviour of both output and employment were optimal; policy intervention could only worsen matters. As Mark pointed out (Blaug 1998: 20-21) the assumptions used here are certainly not robust ones.

¹⁸ For further discussion, see Lipsey (2010).

¹⁹ As outlined in detail in Lipsey (2000), these underpinnings had been extensively studied and empirically tested during the decades that followed the publication of *The General Theory*. They included oligopolistic price setters who had horizontal short run marginal cost curves (because their fixed-factor constraint was a weak rather than a strong inequality) and who therefore adjusted to short term demand fluctuations by altering quantities rather than prices; some consumers who responded to income changes as predicted by life cycle theory and others who were credit constrained and thus responded as the original consumption function theory predicted; investors who were subject to bouts of 'animal spirits' and hence occasional 'irrational' bandwagon behaviour; and labour markets in which wages responded to long term forces rather than short term, day-to-day fluctuations in demand and supply..

²⁰ See Carlaw and Lipsey (2011) for an alternative model in which endogenous technological change drives the economy along a non-stationary growth path that displays unpredictable fluctuations due to the many uncertainties faced by its agents.

The above paragraph raises a number of important issues. To see the first issue, note that in order to provide micro foundations for models in which markets do not clear, economists often use a mathematically tractable model of monopolistic competition developed many years ago by Dixit and Stiglitz (1977). However, it is based on Chamberlin's symmetry assumption that all differentiated versions of a generic product are equal substitutes for each other – e.g., small cars, medium sized cars and large luxury cars. Eaton and Lipsey (1989) showed that monopolistically competitive markets behave very differently when this assumption is replaced by the empirically relevant assumption that differentiated goods stand in a chain of various degrees of substitutability. However, behaviour in such markets cannot be aggregated into a mathematically tractable demand function for the whole industry as it can in the Dixit-Stiglitz version. One model cannot be regarded as simplification of, or an approximation to, the other since they produce very different types of predicted behaviour.

Issue 21: What is the significance of the common use in many models of an empirically refuted version the monopolistically competitive market form whose behaviour differs markedly from a model that conforms to the empirical characteristics of monopolistically competitive goods?

Although it is possible to compare the predictions from these two models when applied to a single monopolistically competitive market, no one has tried to compare the differences that arise when they are embedded in a more embracing multi-market model. So we do not even know what difference it makes to use one model or the other in these more general settings. This is another example of economists preferring a model that is mathematically tractable over one that is not, even though the latter captures major elements of observed behaviour – indeed elements that distinguish the market form under consideration from other market types.²¹

To raise what are probably the most important methodological issues that are implicit in the above discussion of the New Classical model, note that because the New Classical economists, whose work dominated discussion of macroeconomics after the 1970s, assumed (albeit incorrectly) that Keynesian economics had been decisively refuted by stagflation, a debate about the relation between micro and macro variables never took place. Most macro economists simply assumed that macroeconomic models needed micro foundations and that these should be inhabited by completely rational agents. Here are some of the issues that should have been debated at length.

Issue 22: Why is it necessary that all the micro behaviour that is assumed to underlay macro relations be based on dynamic optimization over the infinite time horizon when there are compelling theoretical arguments backed by solid empirical evidence showing that such behaviour is impossible given the uncertainty under which most growth-driving technological change is produced and adapted to?

Instead of debating this issue which surely concerns non-robust assumptions, we have the result recently stated by Lipsey and Scarth (2011: xxvii) in their review article on the Phillips curve: "In modern macroeconomics, no analysis is deemed to have pedigree if it is not based on explicit dynamic optimization."

²¹ This does not provide an argument for reverting to perfect competition, as Friedman might have argued, but for avoiding models that build on non-robust assumptions that are demonstrably empirically false.

*Issue 23: Why is it acceptable to criticise old Keynesian economics for being based on ad hoc assumptions while basing the driving force of cyclical fluctuations in the real business cycle theory of New Classical economics on the ad hoc assumption of random variations in preferences and technology?*²²

There is no substantial empirical evidence to support the assumption concerning preferences and there is clear empirical evidence establishing the path dependent, non-random nature of changes in technology making the random fluctuations hypothesis a non-factual non-robust assumption.²³

Issue 24: Why is everything that has been established in Industrial Economics about the behaviour of firms in small group competition deemed irrelevant to macro behaviour, which is an implicit assumption when a single representative firm is used to stand for the production side of the New Classical economy?

One specific example, was mentioned in Part I. Empirical evidence shows that many firms have short run marginal cost curves that are horizontal. This has two empirically correct implications: first that they will respond to short run fluctuations in demand mainly by altering outputs rather than prices and second, that until they are operating at full capacity, they would typically be willing to supply more at the existing price if the demand were forthcoming. Yet the standard assumption of market clearing is that no firm would be willing to supply more at the existing prices.

More generally, under the desire to adapt to empirical observations, the newer Keynesian models have been evolving to contain aspects of the original Keynesian theory that were totally rejected by the New Classical theorists. As Mankiw (1992) pointed out some time ago, the New Keynesian synthesis is a reincarnation of old Keynesian views in new theoretical clothing rather than a complete resurrection of the original. Some of this new clothing seems to me to be an improvement, although two major parts do not: the acceptance of a long run unique equilibrium, deviations from which set up equilibrating forces;²⁴ and the introduction of inter-temporally maximising firms, either in total or as a leading group, to replace oligopolistic firms who face horizontal marginal costs curves and hence react to cyclical fluctuations in demand with changes in quantities rather than prices.

The de-emphasis of demand side fluctuations.

The replacement of the Keynesian by the New Classical as the prevailing macro-economic theory had one particularly important implication: it transferred emphasis from fluctuations in aggregate demand as the main driver of cyclical fluctuations in income and employment to the supply side, which produced equilibrium at all times with demand only

²² Although there are many others, I stress this particular ad hoc assumption here because it is the driving force of the real business cycle and because it is so at odds with what we know about technological change, which behaviour would radically alter the model's results if it were introduced.

²³ Of course it can be argued that the relevance of these assumptions lies not in their empirical validity but in their ability to generate predictions that pass empirical test. Here I would argue, as is done in detail in Carlaw and Lipsey (2012), that the tests applied to real business cycle theory are too weak in discriminatory power to establish a presumption of the validity of that theory.

²⁴ See issue 3 above, and for an alternative to the long run unique equilibrium view and a test that rejects it see Carlaw and Lipsey (2012) especially section III.

determining the price level. This was done by making obligatory the assumption of rational dynamic maximization by all agents and making freely fluctuating prices rather than fluctuating quantities the main adjuster of the economy's short run behaviour, thus replacing the Keynesian prediction that the markets often contained unsatisfied agents with the assumption of continuous market clearing.

Issue 25: What are we to make of the fact that New Classical economics gives little or no place to demand fluctuations as a cause of fluctuations in employment and unemployment while much (possibly most) popular discussions and policy analyses related to the current recession continues to emphasize demand as a major cause?

Can we really assume that market clearing models inhabited by far-seeing rational agents are relevant to understanding the recession that began in 2007 and the depressed conditions that continued into 2013? Some agreed facts of this recent experience are that starting in the mid-2000s there was a boom in new housing construction financed by credit creation through mortgages and new derivatives. As housing prices rose, existing owners were able to re-mortgage their homes and spend the proceeds. High consumer spending combined with high incomes in the housing industry caused a high level of output and employment. These were partly sustained by the unrealistic expectations of homeowners that their prices would go on rising indefinitely and overly zealous and sometimes dishonest sales persons. Many of the mortgages had low or non-existent payments for several initial years, followed by relatively high ones. Many were sold to persons who had little possibility of servicing them once the initial low payment period was over. By then the sellers had repackaged the mortgages and, with the aid of rating agencies, classified them as low risk and sold them both domestically and internationally. When the higher interest payments kicked in, defaults rose dramatically and the building boom came to an abrupt halt. Many lost their homes while others, even though they could sustain their mortgages, had to cut other spending. The resulting collapse in demand was followed by a major recession.

Two explanations of these events are emerging in the literature, a mainly demand side explanation by Keynesian-inclined economists and a mainly supply side explanation by newclassically inclined economists. The conflicting views are being fought out in the journals and a pessimist, which I am in this case, would suspect the result will be two internally self-consistent explanations that will co-exist and which will never be resolved to the satisfaction of the potential loser, at least during the lifetimes of the current protagonists. To avoid specific attribution in an on-going debate, I raise a few of the issues as a set of assertions from two imaginary economists.

1. *Demand sider:* As shown above, the recession was caused by a collapse of demand following the collapse of the housing boom, a boom that had been caused partly by animal spirits of buyers and over eagerness and downright deception on the part of sellers and their financiers. *Supply sider:* The boom, and then the collapse, was caused by inappropriate government intervention, operating party through Fannie Mae and Freddie Mac.

2. *Demand sider:* The rise in unemployment was caused by a fall in demand for housing, leading to a more general fall in the demand for goods and services, leading to a fall in the demand for labour services. *Supply sider:* The cause was a rise in frictional and structural unemployment, i.e., something emanating from the supply side.

3. *Demand sider*: Those who lost their homes and those who had increased their mortgages to sustain consumption spending did not have rational expectations about the future housing prices nor about their ability to handle the mortgage payments when the higher rates kicked in. *Supply sider*: buyers made mistakes but these were consistent with errors being random. (After all a sequence of random coin tosses can sometimes produce a long series of tails.) They were also faced with perverse incentives coming from the government and they responded rationally to these.

4. *Demand sider*: The market for subprime mortgages and the resulting derivatives were not efficient markets. Fraud, pervasive ignorance and band-wagon effects played an important role in them. *Supply sider*: In so far as there was market inefficiency, it was caused by inappropriate government policy.

5. *Demand sider*: the micro evidence from the actual behaviour of the participants strongly suggest that my interpretation is correct and there is nothing in what we see that cannot be explained by my demand-side interpretation and nothing that would have surprised an old fashioned Keynesian, given only the knowledge of the institutional changes since his/her time. *Supply sider*: My explanation can be made consistent with the data.

6. *Demand sider*: Modern macro models need a much fuller treatment of the financial sector than they typically now have. For example, we should add elements of Minsky's model to Keynesian models. *Supply sider*: I agree with the need and am building models of a financial sector where policy distortions can lead to excessive and/or distortional borrowing and lending.

Issue 26: Are there observations of this recession and slow recovery that could be used to choose between these two explanations. Will comparative tests be attempted rather than just showing that the evidence is capable of being interpreted either way? Will an effort be made to develop models that go beyond two oversimplified versions of the event?

IV. CONCLUSION

In conclusion, I can only express my sadness that I can no longer meet with Mark to continue our past discussions of many of the issues raised here. Will the prejudice against methodology wane, so that economists will come to understand that leaving their students totally ignorant of methodological issues is to leave them unable to assess in a constructive manner many of the issues they face? Will economists become wary of models that make no statements that could possibly be contradicted by observations? Will the criterion that an acceptable model is one that can track existing data – at least when it has been suitably filtered – be amended to make tests of predictions the main criterion? Will people seek to make comparative tests to judge the balance between two competing models? Will the obsession with formal models to the exclusion of appreciative theorising soften at least a bit? Will economists become more interested in the historical facts of the things they seek to model and the technical details of their explanatory variables? Will economists become more willing to alter their theories to accommodate the growing body of evidence about how people actually behave that comes from behavioural economics as well as the new observations showing that humans (as well as chimpanzees and possibly other primates) have an innate sense of morality that influences their

actions?²⁵ Will economists become more interested in the empirical relevance of the non-robust assumptions than many of them now are? If the past is any guide, these issues will still be around long after both Mark and I have been forgotten.

²⁵ See Wilkinson and Klaes (2012) on the first issue and Hodgson (2013) on the second.

REFERENCES

- Aghion, Philippe and Peter Howitt (1997), *Endogenous Growth Theory*, (Cambridge:MIT Press)
- Arrow, Kenneth J. (1962), "Economic Welfare and the Allocation of Resources for Innovation," in *The Rate and Direction of Inventive Activity: Economic and Social Factors*, (NBER Conference series), 609-626.
- Arthur, W. Brian (1994), *Increasing Returns and Path Dependence in the Economy*, (Ann Arbor: University of Michigan Press).
- Blaug, Mark (1992), *The Methodology of Economics* (2nd ed.), (Cambridge, Cambridge University Press).
- _____ (1997a), "Competition as an End-State and Competition as a Process" in *Trade Technology and Economics: Essays in Honour of Richard G. Lipsey*, (Cheltenham UK: Edward Elgar), 241-262.
- _____ (1997b), "Ugly Currents in Modern Economics," *Policy Options*, September: 3-8.
- _____ (1998), "Disturbing Currents in Modern Economics," *Challenge* **41(3)**, 11-34
- _____ (2007), "The Fundamental Theorems of Modern Welfare Economics, Historically Contemplated," *History of Political Economy*, **3(2)** 185-207.
- _____ (2009). "The Trade Off between Rigour and Relevance: Sraffian Economics as a Case in Point," *History of Political Economy*, **41(2)**, 219-247.
- Carlaw, Kenneth and Richard G. Lipsey (2002), "Externalities, Technological Complementarities and Sustained Economic Growth," *Research Policy*, **31(1)**, 305-1315.
- _____ (2011), "Sustained Endogenous Growth Driven by Structured and Evolving General Purpose Technologies," *Journal of Evolutionary Economics*, **21(4)**, 563-593.
- _____ (2012) "Does history matter?: Empirical analysis of evolutionary versus stationary equilibrium views of the economy," *Journal of Evolutionary Economics*, **22(4)**, 735-766
- Chandler Alfred, D. (1977), *The Visible Hand: The Managerial Revolution In American Business*, (Cambridge, Mass: Harvard University Press).
- _____ (1990), *Scale and Scope : The Dynamics of Industrial Capitalism* (Cambridge, Mass: Belknap Press).
- Davidson, Paul (1991), "Is Probability Theory Relevant for Uncertainty? A Post Keynesian Perspective" *Journal of Economic Perspectives*, **3(1)**, 129-143
- Dixit A. K. and J. E. Stiglitz (1977), "Monopolistic Competition and Optimal Product Differentiation," *American Economic Review*, **67**, 297-308
- Eaton, B. C. and Richard G. Lipsey (1989), "Product Differentiation" in *Handbook of Industrial Organization*, R. Schmalensee and R. Willig, (eds), (Amsterdam: North Holland), 725-768.
- Griffiths, Peter. (2003), *The Economist's Tale: A Consultant Encounters Hunger and the World Bank*, New York: ZED Books.

- Hey, J. (1997), "The Economic Journal: Report of the Managing Editor," *Royal Economics Society News Letter*, 3-5.
- Hodgson, Geoffrey M. (2013), "*From Pleasures Machines to Moral Communities: An Evolutionary Economics without Homo economicus*," (Chicago: University of Chicago Press).
- Lipsey, Richard G. (2000), "IS-LM, Keynesianism and the New Classicism," in *Macroeconomics and the Real World, Volume 2: Keynesian Economics, Unemployment, and Policy*, Roger E. Backhouse and Andrea Salanti (eds), (Oxford: Oxford University Press), 57-82.
- _____ (2007), "Reflections on the General Theory of Second Best at its Golden Jubilee," *International Tax Public Finance*, **14**, 349-364.
- _____ (2010) "The Aggregate Demand Aggregate Supply Diagram", in *Famous Figures and Diagrams in Economics*, (Mark Blaug and Peter Lloyd, eds.), (Cheltenham: Edward Elgar), 365-376.
- _____ (2012), "A critique of Ng's third-best theory," Department of Economics, Simon Fraser University Study paper, DP12-2.
- _____ and Kelvin Lancaster (1956), "The General Theory of Second Best", *The Review of Economic Studies*, 1956, **24**, 11-32.
- _____ and Kenneth Carlaw (1996), "A Structuralist View of Innovation Policy", in *The Implications of Knowledge Based Growth*, Peter Howitt (ed.), (Calgary: University of Calgary Press), 255-333.
- _____ (1998a), "Technology Policies in Neoclassical and Structuralist-Evolutionary Models," *OECD Science, Technology and Industry Review*, **22**, Special Issue, 31-73.
- _____ (1998b), *Structural Assessment of Technology Policies: Taking Schumpeter Seriously on Policy*, (Ottawa: Industry Canada).
- _____ and Clifford Bekar (2005), *Economic Transformations: General Purpose Technologies and Long-Term Economic Growth*, (Oxford: Oxford University Press).
- _____ and Alec Chrystal (2011), *Economics* 12th edn., (Cambridge: Cambridge University Press) Originally published in 1963 as *An Introduction to Positive Economics* by Richard G. Lipsey.
- _____ and William Scarth (2011) "The History, Significance and Policy Context of The Phillips Curve," Introductory essay to *Inflation and Unemployment: The Evolution of the Phillips Curve* (3 Vols.) (Richard Lipsey and William Scarth, eds.) (Cheltenham: Edward Elgar), xii-xxxvii.
- Lucas, R. E., Jr., & Sargent, T. J. (1979), "After Keynesian Macroeconomics," *Federal Reserve Bank of Minneapolis Quarterly Review*, **3**, 1-6.
- Mankiw, Gregory (1992), "The Reincarnation of Keynesian Economics" *The European Economic Review*, **36**(2-3), 559-565.
- _____ (2010), *Macroeconomics*, (Worth Publishers).

- Mas-Colell A., M. D. Whinston and R. J. Green. (1995), *Microeconomic Theory* (New York: Oxford University Press).
- Mowery, David C and Richard R. Nelson (1999), *Sources of Industrial Leadership: Studies of Seven Industries*, (Cambridge: Cambridge University Press).
- Nelson, R., and S. Winter (1982), *An Evolutionary Theory of Economic Change*, (Cambridge: Harvard University Press).
- Ng, Yew-Kwang (1977). "Towards A Theory of Third-Best." *Public Finance* **32**(1), 1-13.
- O'Brien D. P. (1998), "Four detours," *Journal of Economic Methodology*, **5**(1) 23-41.
- Ormrod, David (2003), *The Rise of Commercial Empires: England and the Netherlands in the Age of Mercantilism, 1650-1770*, (New York: Cambridge University Press).
- Rae, John (1905), *The Sociological Theory of Capital*, (New York: Macmillan) first published in 1834 as *Statement of Some New Principles on the Subject of Political Economy Exposing the Fallacies of the System of Free trade and of Some Other Doctrines Maintained in the Wealth of Nations*.
- Ragan, Christopher and Richard G. Lipsey (2011), *Economics* 13th Canadian edn, Toronto Pearson Press.
- Rodrik, Dani (1993), "Taking Trade Policy Seriously: Export Subsidization as a Case Study in Policy Effectiveness," *NBER Working Paper* **4567**.
- _____ (1994), "Getting Interventions Right: How South Korea and Taiwan Grew Rich," *NBER Working Paper* **4964**.
- Rosenberg Nathan (1982), *Inside The Black Box: Technology and Economics*, (Cambridge: Cambridge University Press).
- _____ (1994), *Exploring The Black Box: Technology, Economics and History*, (Cambridge: Cambridge University Press)
- _____ (1996) "Uncertainty and Technological Progress" in *The Mosaic of Economic Growth*, R. Landau, T. Taylor and G. Wright (eds), (Stanford: Stanford University Press).
- Ruttan, V. W. (2001), *Technology, Growth, and Development: An Induced Innovation Perspective*, (New York: Oxford University Press).
- Schumpeter, Joseph (1934), *The Theory of Economic Development, English Translation*, (Cambridge: Harvard University Press) (First published in German 1912).
- Starr, R.M. (1997). *General Equilibrium Theory: An Introduction*, (Cambridge: Cambridge University Press).
- Westphal, Larry E. (1990), "Industrial Policy in an Export Propelled Economy: Lessons from South Korea's Experience," *Journal of Economic Perspectives*, **4**(3), 41-59.
- Wilkinson, Nick and Matthias Klaes (2012), *An Introduction to Behavioral Economics* 2nd ed., (Basingstoke: Palgrave Macmillan)