

**STYLIZED FACTS AND THEORY BUILDING: ON SOME
METHODOLOGICAL ISSUES IN ECONOMIC THEORY WITH
REFERENCE TO THE WORKS OF NICHOLAS KALDOR.**

*Dissertation submitted to the Jawaharlal Nehru University
in partial fulfilment of the requirements
for the award of the Degree of*

MASTER OF PHILOSOPHY

Submitted by

Shipra Nigam



Centre for Economic Studies & Planning
School of Social Sciences
Jawaharlal Nehru University
New Delhi-110 067
India
2005



CENTRE FOR ECONOMIC STUDIES & PLANNING
SCHOOL OF SOCIAL SCIENCES
JAWAHARLAL NEHRU UNIVERSITY
NEW DELHI-110067, INDIA

Certificate

Certified that the dissertation entitled '**STYLIZED FACTS AND THEORY BUILDING: ON SOME METHODOLOGICAL ISSUES IN ECONOMIC THEORY WITH REFERENCE TO THE WORKS OF NICHOLAS KALDOR.**'

Submitted by **Shipra Nigam**, is in partial fulfilment of Master of Philosophy degree from the university. The work presented is original and has not been submitted in part or full for any other degree to this or any other university to the best of our knowledge.

We recommend that this dissertation be placed before the examiners for evaluation.

Krishmendu Ghosh Dastidar
for Chairperson

(Prof. Jayati Ghosh)

Prabhat Patnaik
Supervisor

Prof. Prabhat Patnaik

Acknowledgments

To Prof. Prabhat Patnaik for many long thought-provoking discussions, extremely perceptive comments and forthright criticisms, for perspective, consideration, unstinting support and patience, all of which, besides being indispensable in helping me evolve my arguments and resolve my ambiguities, also made writing this dissertation a personally stimulating experience. To Prof. Jayati Ghosh for all the warmth and affection and for all the personal help and encouragement which also made the past two years of my association with the department so pleasant and comfortable.

Dr. Vatsala Narsimham remains a source of strength and inspiration in all my expeditions, both academic and personal, besides always being there to provide all support, guidance and sane advice. To Dr. Omkarnath for opening up so many new vistas in economics; for his incredible enthusiasm, warmth and cheerfulness. To Dr. Anindita Mukhopadhyaya and Dr. Shasheej Hedge for numerous exhilarating conversations, discussions and debates, when in moments of shared camaraderie one crossed so many hierarchical and institutional barriers. To Prof. Neeladri Bhattacharya for stimulating lectures in historical methods where disciplinary boundaries got blurred in movements towards broader horizons. To Swarilipi for a careful and meticulous proofreading at a very short notice.

To the staff of all the libraries; the central library and the Exim Bank library in JNU, the Ratan Tata library and the library at the Institute of Economic growth.. To Raju for taking care of everything during the most crucial days and for demonstrating how dignity of labour and sensitivity towards others is maintained in face of such monotonous and limiting circumstances.

To all the comrades in arms from M.A days, where it all began : Bahni, Neelu, Neetu, Madhu, Paushali, Qurrat, Rose, Shal, Sharmila, Soumya, Soumya, Sukhi, Sunitha, Vasu. To Bochka for timely help in enabling a dismal scientist to finally make it through patient listening, conversations over coffee and assorted cd's. To Ankita, Ena, Janaki, Kakul, Kalyaan, Mona, Mrinalini, Padmini, Sanjukta, Sophia, Sreeti and Vivek for being there in their own ways, for making the lighter moments brighter and the dampening ones more bearable. To cherished friendships that have survived the distance of the proverbial seven seas: Apurva, Satya, Shairi, Shraavya and Sophie. To Mario, Ponni, Polumi and Uditi for all the love and care. To Parth for the same but also for shared music and for that combination of innocence, naiveté, and flashes of intuitive wisdom. To my brother Kunal for those cherished memories and for that unspoken bond which only comes with a shared childhood. To Geetika and Ravi, so near and so dear, for uncritical love, sharing and sustenance and for making everything worthwhile in the end. To My parents, beloved and indispensable, for letting me go and grow, for providing the secure foundations of my existence. It is to them that I dedicate this piece of writing.

Contents

	<i>Preface</i>	1
Chapter 1	<i>The aims and claims of Economics and questions of dominance and pluralism: Some debates in Contemporary Economic Analysis</i>	2-17
Chapter 2	<i>A Historical Survey: Contextualising Kaldor within the theoretical and methodological debates in Economics</i>	18-47
Chapter 3	<i>Abstractions, Realism and Theory Building on the basis of Stylised Facts: Reconstructing Kaldor's critique of Equilibrium Orthodoxy</i>	58-82
Chapter 4	<i>Long Run Growth, Development and Stylised Facts: A critical appraisal of Kaldor's methodological assertions</i>	83-108
Chapter 5	<i>Concluding Remarks: A brief overview of the Central Arguments</i>	109-113
	<i>Bibliography</i>	114-122

Preface

This dissertation is an attempt to explore some methodological issues in economic theorizing vis-à-vis the works of Nicholas Kaldor, arguably an original, provocative and influential economist of his generation, located within the tradition of economic heterodoxy. Given the background of contemporary debates within the discipline surrounding the questions of aim, validity and relevance of economic theorising and the more general debates on the questions of 'truth', 'rationality' and 'objectivity' in contemporary philosophy, it attempts to contextualise and assess specific aspects of the Kaldorian contributions of economic thought. Accordingly, it's a critical reconstruction and appraisal of his distinct methodological perspective informed by his conceptualization of and his teleological beliefs regarding the motivations, objectives and concerns of economics as a discipline. It also attempts to explore the relevance and usefulness of such a perspective for analysis of certain contemporary economic issues which have a parallel in Kaldorian contributions to aspects of economic theory (especially those on economic growth and development) and methodology. The emphasis throughout is on his later works where there is an underlying unity of purpose in his theoretical and applied analysis in terms of his consciously articulated methodology based on 'Stylized facts'.

Chapter One

The aims and claims of Economics and questions of dominance and pluralism: Some debates in Contemporary Economic Analysis

What are the aims of economics? What is the role and nature of scientific enquiry in the discipline? What constitutes a test of validity and significance of any given economic doctrine? Any methodological exercise is also an attempt to provide answers, maybe provisional and perhaps even partial, to some or all of these questions. Accordingly, it might be useful to begin by stating the relevant background of as well as the perspective with which one is embarking on such an exercise. Since any methodological discussion also proceeds on the basis of certain implicit or explicit assumptions regarding the objectives of the discipline in question, I shall begin with an explicit discussion of the issues involved therein.

Contemporary Economics: Aims, Claims and 'Reality'

Modern Economics situates itself within the realm of social sciences. As such the ultimate aim of most theorisation in economics in most broadly stated terms is to illuminate those aspects of social reality which concern the working of 'real' economies in historical time and space. It could be argued, with some justification, that economics occupies a unique position within social sciences given the emphasis on the role of practical relevance for policy questions in economics perse, evident in the manner in which the usefulness of the discipline is perceived and has been perceived in the past, both within and outside the economic community.

This is not to state that there are not other important motivations and concerns underlying the study of economics. As stated by Amartya Sen, the role of description and evaluation, amongst others, is equally important:

While description , prediction and evaluation maybe the main elementary exercises with which the subject is concerned, there are more complex (and in some ways, more derivative) problems too, such as using

economic arguments for political advocacy or seeing “ the rhetoric of economics” as an object of direct importance, in addition to its instrumental role in means of communication. (A. Sen 1989: p 301)

Though the point regarding the diversity of the subject matter is well taken, it is important to underline the emphasis (atleast in terms of explicit statements by those belonging to the discipline) on prediction and practical relevance in economics vis-à-vis other disciplines within social sciences, for this is crucially linked to the self conception of economics as a ‘science’ and its concern for the scientific method. Practical relevance involves the notions of the right or ‘true’ explanation and identification of the causes of various features of empirically observed phenomenon for prescriptive purposes. This makes evaluation amongst competing theories, which can account for observable phenomenon, a difficult task. Matters are further complicated by the controversies and debates surrounding the questions of ‘truth,’ ‘objectivity’ and ‘reality’ ever since the post linguistic turn in contemporary philosophy brought in its wake a fundamental critique of foundationalism and teleology.

For most of the history of mainstream political economy and economics, from Adam Smith to Keynes (and probably even more specially so before Smith) the traditional aim (atleast avowedly so) has been to discover or construct theories which would contribute, directly or indirectly, to less unsuccessful policy making. Even in the case of heterodox traditions such as Marxist economic analysis, despite the emphasis on normative analysis, prediction has been an integral part of the analysis not merely stemming from an attempt to base the analysis on credible and scientific grounds but also to diagnose economic problems of immediate relevance and urgency.

The last few decades however have witnessed a widely acknowledged ‘crisis’ in economics¹ stemming from , besides other things, the breakdown of the postwar Keynesian paradigm during the course of the 1970’s which seems to have brought with it ambiguities, even fundamental disagreements, over the basic aims of the discipline. To some extent this was an expected outcome of a perceived crisis within the discipline. No crisis, methodological or theoretical, needs to be held to exist merely because economists cannot provide appropriate political formulae to reconcile popular demands with existing,

¹ A detailed historical description of the origin and nature of such a crisis is attempted in the next chapter.

or inevitable institutional constraints. This leads one to conclude that somewhere in the acknowledgement of the crisis within the discipline is a perceived failure of the subject in meeting its objectives adequately within the realm of the possible.

As Rubenstein reflects,

Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question ‘where does it lead?’ (Rubenstein 1995: p.12)

As pointed out by Boylan and Gorman (1995), the relationship between the preoccupation with the general state of economics (which includes amongst other things, the public standing and the perceived success of the discipline; the methods of recruitment and training in the discipline; the allocation of resources which reflect the reward and control system within the discipline etc) and its methodological base is both a complex and subtle one. The interconnections between the aims and claims of economics and their implications for economic methodology shall be a recurring theme in this dissertation. It might be worthwhile then, to elaborate somewhat on the general state of contemporary economics and its implications.

Recent trends in Economic Theorising and Methodological appraisals: Questions of dominance and pluralism

Contemporary methodological criticism of the dominant mainstream paradigm, with its origins in neoclassical orthodoxy, has largely been directed at its *overemphasis* on formalistic, mathematical deductivist approach to theorising which has not been very successful in the explanation of observed socio economic reality. As pointed out in various quarters², the criticism is directed not at the use of formalistic methods *per se* but to the “manner in which they are everywhere imposed, to the insistence on their being almost universally wielded, irrespective of, and prior to, considerations of explanatory relevance, and in face of repeated failures.”(T. Lawson 2003: p.xix) This domination of economics by the mainstream project and its links (via an erosion of the general standing of the discipline) to the facts of declining enrolment in economics courses, receding

² For details see M. Blaug (1980, 1992), B. Ward (1972), S. J. Latsis (1976), L.E. Boland (1982), T.J. Hutchison (1992), T Lawson (2003).

percentage of college students choosing to major in economics in the west as well as to the severe dissatisfaction expressed by existing students regarding the lack of relevance and realism in the prescribed curricula of graduate courses has often been noted with concern by several economists.³

More significant, in this context, have been the growing complaints against the complete hegemonisation of such abstract deductivist methods *catering to the requirements* of the neoclassical theoretical framework within the academic echelons. As observed by Richard Lipsey,

To get an article published in most of today's top rank economic journals, you must provide a mathematical analysis, even if it adds nothing to your verbal analysis. (Lipsey 2001: p.184)

Or as David Worswick points out two decades earlier right at the beginning of the current crisis,

There now exist whole branches of abstract economic theory which have no links with concrete facts and are almost indistinguishable from pure mathematics. (qtd in Blaug 1992: p.237)

However as stated earlier, it's not the mere fact of domination per se but the inability of mainstream theorising to illuminate vital aspects of social reality that has been the subject of much despair, even among the mainstream theorists themselves.

Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilises mathematical tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those offered by the natural sciences, and the link between economic theory and practical problems... is tenuous at best. (Rubenstein 1995:p.12)

Or as Leontief pointed out two decades earlier,

Continued preoccupation with the imaginary, hypothetical, rather than with observable reality has gradually led to a distortion of the informal valuation scale used in our academic community to assess and rank the scientific performance of its members. Empirical analysis, according to this scale, gets a lower rating than formal mathematical reasoning. (qtd in Blaug 1992:p.237)

³ See for example Abelson 1996; Parker 1993; Krueger. *et al.* (1990); Fullbrook (2003).

The thrust of the critique, it must be emphasised over here, is not directed at the use of formalistic methods *per se* but at the victory of technique over substance and the forsaking of practical relevance for elegance.

... Graduate programs may be turning out a generation with too many *idiot savants* skilled in technique but innocent of real economic issues (Krueger *et al*, 1991, pp. 1044–5).

Perhaps a case can be made for the ‘intellectual pursuit’ of economics for ‘pleasure’ or for a ‘good game’ or for pursuit of ‘theory for its own sake’ if explicitly argued for by its practitioners. This could be justified not merely on grounds of ‘preserving academic freedom’ but also on the grounds of not a completely erroneous scepticism regarding the possibility of economic theorising to contribute to successful policy making given the nature of the subject and the political limitations on implementation of relevant policy conclusions arising out of advances, if any, in theorising. Moreover it could be argued that given the ‘fallibility’ of all knowledge, one never knows when overcautiousness might lead to elimination of theorising, however abstract or simplified, which might find real world applicability and policy relevance in the near future.

Whatever be the merits of such a view, it has hardly been forthcoming. As stated by T.J.Hutchison,

Far from any manifesto of the movement having appeared proclaiming the need for, or fruitfulness of, greater and more extensive abstraction, and the desirability of some kind of pure knowledge for its own sake, there has been nothing in the way of explanation or justification, even in the face of severe criticisms of excessive abstraction by eminent economists of different schools and views. (T.J. Hutchison 1992: p15)

The one exception seems to be R. Weintraub’s defence of General Equilibrium theory against charges of verificationism and falsificationism on postmodernist lines. According to him, there exists “no position apart from the doing of economics which can inform the consideration of the doing of economics” (Weintraub 1989: p.272). However he endorses the use of methodological conversations and argumentation amongst members who know about the theory and are part of the particular community engaged in the practice of that theory. A problem with such a methodological stance for justifying mainstream theorising today is that it leads one to the tension between its implications and the alleged

facts of domination of such theorising in the academia: for if each theory is to be understood only on its own terms, all forms of theorising are valid and its methodological pluralism rather than methodological domination which is warranted on the basis of such a relativist stance.

On the other hand if no such positive pronouncements are forthcoming from the majority of mainstream practitioners and implied acceptance of some kind of practical relevance as a fundamental objective in economics is assumed, there is a need to evaluate how far can the 'aims' of economics can be 'claimed' to have been met in 'reality' by the actual practice of economists. This leads us to the crucial question of verification and testing of economic theories and it is on this count that modern mainstream economic theorising has been the subject of severe criticism recently. There are several issues at stake here: Must all economic analysis be subject to verification and testing? Given the complexities of economics as a social science dealing with human behaviour, is unambiguous testing or theory confirmation even possible? What is it that is to be tested: the conceptual apparatus, the assumptions or the predictive implications of economic theories? Given the complexities involved in analysis of certain economic problems with predictive implications which may require detailed conceptual and analytical exploration, what should be the time span within which implications must be subjected to empirical scrutiny?

There are no easy answers and debates around these issues shall continue in the near future. However even after giving due allowance to the heterogeneity of the subject matter of economics, the problems of verification and testing and the multiplicity of research programs being pursued within the discipline, one cannot get away from the fact that there exist several relevant and significant criticisms regarding the practice of the dominant mainstream economic tradition. These involve the empirical content of economic theory, the concept of the rational economic man, the uses and abuses of equilibrium analysis, the interconnectedness of economics with other disciplines, laissez-faire policy conclusions and so on. All of these criticisms somewhere deal with the failure of mainstream project to carry out its vocation as a social science to ultimately, in the final analysis, illuminate aspects of socio-economic reality. It's not surprising then that the failure of mainstream theorising to meet the criteria of descriptive adequacy and

explanatory relevance has been severely criticised in various methodological pronouncements on the state of contemporary economics. Whether the root cause of such failure lies in merely the use of formalistic methods is debatable. It could be argued that the criticisms outlined above are directed at the core assumptions and goals of neoclassical theorising and the consequent structure and strategy that is adopted to meet their requirements. Formalism then becomes merely the symptom of a deeper malaise. I shall explore this proposition in greater detail while discussing Kaldor's works.

Meanwhile, several new developments within economics in the past few decades have been greeted with enthusiasm, for instance, as argued by B. Caldwell, they hold promise in that they "may ultimately have more impact on the practice of economics than will the writings of the philosophical authorities" (1988a: p.46). These open new avenues of escape from the orthodoxy: new reflections on the market, the firm, organization and rationality, and new attempts to construct approaches to economics with historical, social and ethical dimensions. They are reflected in the emergence and in some cases re-emergence of approaches which have their roots in economic heterodoxy: 'behavioural economics' dealing with effects of deviation from purely rational behaviour both theoretically and empirically, the 'neo-Austrian' approach with its emphasis on entirely subjective interpretations of individual economic agents facing institutional constraints, 'New institutional economics' dealing with the study of emergence of and changes within institutions and the effects of such changes, and so on. To what extent these attempts have been able to circumvent the perceived failures of the neoclassical orthodoxy remains debatable. It requires consideration of a host of other largely unsettled questions such as identification of root causes such failures and their causes⁴.

In this context, the role of heterodox traditions within economics assumes importance given their attempts to move towards a more relevant economics by defending perspectives on theory and method which seek to overcome what they see as perceived failures of received economic doctrines. Significant contributions to these traditions have been made by Marxist and radical political economics, Institutional economics and Post Keynesian economics. The present study assumes importance in this context by analysing

⁴ One such development in the form of 'endogenous growth theory' has been examined in this dissertation from the vantage point of Kaldorian critique of the neoclassical orthodoxy.

the methodological and theoretical contributions of an economist located within the latter tradition.

Methodological analysis in economics, in keeping with the emphasis on the role of economics as an empirical science, has largely been concerned with questions of empirical theory choice. For a long time the literature on methodology grappled with the demand of providing an 'algorithm for doing good economic science.' Not surprisingly then, economic methodology has been influenced by all the major developments in the philosophy of science and it is the evolution and interaction of these two areas of discourse which has provided the context within which most of the contributions towards economic methodology have been located till date. In keeping with contemporary debates in philosophy of science, there recently has been a trend towards 'a more pluralistic age' arising as a consequence of controversies relating to the epistemology of 'Truth,' based on the notion of 'fallibility' of all knowledge and a fundamental critique of foundationalism and teleology in general. Put in very simplistic terms, fallibility of all knowledge is based on the premise that one can never know if one has discovered the 'truth' or 'reality' as it is, for one can never know what such a truth or reality is with certainty. The critique of foundationalism argues that the existence of an objective, rational 'truth' or 'reality' independent of its perception is impossible, given that *all facts are theory laden*. The issues at stake here however are far more complex involving divergent conceptions of nature and existence of reality which are crucial to this analysis as shall be brought out as we proceed. In contemporary literature on economic methodology inspired by parallels in philosophy of science, these have come out in the three emerging trends: 'the sociology of scientific knowledge' framework originating from the works of Kuhn and Feyerabend⁵, the rhetoric of economics programme based on recent developments in literary theory⁶ and the programme for a realist philosophy of economics⁷ based on the revival of 'scientific realism' in contemporary philosophy of science. The last is particularly important for this study as I shall argue for its potential

⁵ For details see Coats (1984) and Maki (1992)

⁶ See D. N. McCloskey, 'The Rhetoric Of Economics,' *Journal Of Economic Literature*, vol.21(June, 1983)

⁷ See Maki (1992, 1997), Lawson (1989, 2003)

usefulness in laying down the epistemological grounds of Kaldorian analysis.⁸

It's also been argued in the literature moreover that the overemphasis on discovering various prescriptive methodologies based on an idealised version of practice within a natural science like physics has been largely counterproductive.⁹ Not only is their straightforward applicability in social sciences being subjected to growing scepticism, the validity and relevance of such prescriptive methods in the field of natural sciences itself is increasingly being called into question. A balanced approach is called for where any engagement with perspectives within the philosophy of science or even those within philosophy of social science¹⁰ or literary theory is sensitive to the specificity of the problems encountered within a discipline like economics. This becomes especially applicable in any appraisal of a research program located within heterodoxy. The general attitude of researchers within the mainstream, too busy doing substantive work in economics by applying the theoretical apparatus given to them during periods of 'normal science' in the Kuhnian sense, is to consider methodological debates largely peripheral and redundant. At the same time their criticism as well as dismissal of alternative approaches as 'bad science or quackery' is based on methodological grounds, implicitly asserting the supremacy of the dominant methodological practice. The role of any methodological appraisal in such instances is not merely to point out the inconsistency of such an approach and to assert that there is no given single practice which makes for 'good science' but also to demonstrate that reasonable and productive debate is still possible.

For as a social *science*, economics can hardly do away with the need for 'scientific discipline'. If the positivist criterion of testability, or the Popperian one of falsifiability or attempting to falsify are rejected on grounds of not only the latest developments within contemporary philosophy of science itself, but also on account of the problems associated with, and criticism of, their applicability in a social science like economics given its

⁸ Lawson (1989, 2003) is one of the earliest advocates of the relevance of realism for Post-Keynesian analysis and in particular for Kaldorian analysis.

⁹ In this context, the idea of an evolutionary economics based on insights from evolutionary biology is also gaining currency. See for example, Hodgson (1993, 2002), Laurent and Nightingale (2001) amongst numerous others. Kaldor himself favoured a more evolutionary approach with parallels in biology for the study of economic dynamics (1984, 1985).

¹⁰ The institutionalists' attempts are most notable in this regard.

behavioural, historical and institutional dimensions, then what should be the criterion of ensuring scientific discipline in theoretical and methodological appraisals ? As Hausman puts it,

Since philosophers of science have no gospel for scientific practice, economic methodologists have no prepared sermons. Cast among heathen, bereft of revealed truth, methodologists must face the bewildering task of attempting to understand and to assess the practices and products of economists. (1989: p.283)

Although judicious use of insights gained from a study of contemporary philosophy of science might still be useful, those interested in economic methodology are faced with the daunting task of exercising their judgement and knowledge of the practice of economics to defend rational standards for the practice of economics. Hence, application of any general standards or criteria of scientific practice in methodological appraisals requires caution and a thorough understanding of the actual problems and procedures of the research programme being studied. Accordingly, this has resulted in inevitably pluralistic strands in methodological appraisals which in contemporary debates has broadly taken shape in two emerging trends. On one hand it's reflected in the eclectic nature of current literature on economic methodology in diverse works which usually turn on the details of the specific practices of economists¹¹. On the other hand, it's reflected in explicitly pluralistic positions taken in methodological appraisals. Amongst economists known for using a more eclectic and empirical vein in methodological appraisals are Bruce Caldwell (1982) with his particular version of methodological pluralism described as critical pluralism and Donald McCloskey (1985) with his "rhetoric of economics." The latter argues for the application of tools of classical rhetoric and literary theory and a careful study of economic argumentation in understanding the practice of economists.

'*Critical pluralism*,' as advocated by Caldwell on the other hand begins with an explicit recognition at the outset that there is "no universally applicable, logically compelling method of theory appraisal" (1994: p 245). Given that the present dissertation adopts a broadly similar position in its appraisal of the Kaldorian research programme, a detailed description of this approach is now attempted which largely takes off from the

¹¹ For example see Neil deMarchi and A. Hirsch (1986), Rosenberg (1983).

account provided by Caldwell (1989, 1994).

A critical pluralist is a historian of ideas whose chosen subject is the methodological writings of economists, and whose task is the reconstruction and evaluation of the position contained in those writings. (Caldwell 1989: p. 43)

Accordingly, methodological pluralism is a meta-methodological position; it advocates the study of a range of methodologies (by means of rational reconstruction). Critical pluralism, in addition, involves the criticism of this range of methodologies by means of a range of criteria given the absence of a basis for deciding in favour of any single methodology. At the same time Caldwell is at pains to emphasise that “critical pluralism” does not imply a ‘methodology of free love’.

...when advocates of the new methodology say that a concern with questions of theory choice should no longer be at the top of the methodologist’s agenda, it does not mean that all standards disappear. (Caldwell 1982: p15)

This position differs from radically relativist positions like the rhetorical/hermeneutic approach or postmodernist approach; while the former accepts a plurality of understanding and plurality of method as a description of reality but refuses to make any normative judgement on the nature and extent of those pluralities, the latter positively argues in favour of a range plurality of understanding and methodology as reflecting a fundamentally fragmented reality. Unlike radical relativism, pluralism among methodologies here is not considered to be intrinsically desirable. However in absence of any given algorithm of appropriate scientific practice, it is judged to be instrumentally advisable to have a number of alternative methodologies to inspect. Critical assessment under such circumstances takes place through a variety of criteria implicit in which is the recognition of plurality: there are several ways to evaluate differing methodological and theoretical ventures. At the same time, the possibility of showing some methodological views to be better than the others through the process of criticism is not ruled out. Besides enhancing one’s understanding of the practice of economics, Caldwell argues that critical pluralism may possibly in future also lead to a broad consensus where certain approaches are viewed as broadly acceptable across a variety of different paradigms in economics (1989: p. 42-52).

In this context, the current dissertation attempts to contextualise and assess specific aspects of the Kaldorian contributions of economic thought given the above background of contemporary debates within the discipline surrounding the questions of aim, validity and relevance of economic theorising and the more general debates on the questions of 'truth', 'rationality' and 'objectivity' in contemporary philosophy. It negotiates the terrain between naturalism¹² and radical relativism by adopting a broadly critically pluralist position as laid down by Caldwell with certain qualifications. To begin with, it explicitly endorses the contention that the ultimate goal of economic enquiry is to improve our understanding of 'real economies' operating in historical time and space.¹³ Also, as pointed out by Sheila.C. Dow (1997 [2002]: p.143-145), it recognises that even methodological pluralism should entail an explicit clarification of the methodologist's own ontological and epistemic positions. Here I would go along with her to endorse an open system epistemology and ontology as the grounds of recognition of the inevitability of a range of methodologies in the absence of any gospel for scientific truth, thereby justifying a pluralistic approach in methodological appraisals. As the concept of open and closed systems is important in the present appraisal, I shall soon come to a detailed explanation of their nature and implications. However it suffices over here to state that an open system is one whose boundaries, the nature and range of its constituent variables and the structure of their interrelationships are not predetermined and maybe in a continuous state of evolution. If reality is understood as an open system, an evolutionary epistemology entailing the recognition of a range of methodologies becomes consistent with the advocacy of the employment of a particular range of methods as means of improving one's knowledge, where knowledge itself is understood as an open system.

Accordingly, it might be fruitful to begin by stating that the present study is an evaluation of a heterodox research program, involving a 'rational reconstruction' of the methodological content of the works of an individual economist from the vantage point of "*critical pluralism*.' It adopts a blend of broad prescriptions along with historical description which brings together both the objective and subjective elements of the appraisal. It also involves a critical discussion of both, the strengths as well as the

¹² Naturalism as a philosophy endorses, besides other things, a unitary understanding of reality and universal criteria for methodological and theoretical appraisals.

¹³ Here reality is conceived of as an open system.

limitations of the particular methodological approach being scrutinised. Instead of arguing in favour of any given methodological practice, it seeks to appraise such a program on its own terms as well as in terms of its comparison with other established programs within the discipline (more specifically the dominant mainstream neoclassical orthodoxy). The specific criterion for such an appraisal shall be the nature of reality advocated by the programme, its orientation in terms of the aspects of socio economic reality it focuses on (in this instance, specifically, the analysis of long run growth and development in modern capitalist economies) providing an alternative to the mainstream vision and its specific contributions to economic methodology and theory which follow consequently. In this process it shall probably raise more questions than the answers it shall provide.

It is important to emphasise over here that the real purpose of the present exercise is not one of a historical appraisal of Kaldor's works or even to merely retrieve the Kaldorian perspective on aims and methods of economics. Rather it is an attempt to explore some fundamental methodological issues in economic theory and their complex interaction with the basic aims and objectives of economics as a distinct social science vis-à-vis the manner in which certain questions of contemporary relevance are addressed in Kaldorian contributions to economic thought.

So why Kaldor? There are several reasons behind this choice as shall become obvious as we proceed. For one, Kaldor was one of the founding members of the Post-Keynesian stream in economics belonging to the Cambridge school, a heterodox tradition purportedly working towards establishing a more relevant economics by putting forward various criticisms of the mainstream tradition and defending perspectives on theory and method consistent with an allegedly richer and broader view on economic reality than the revealed presuppositions of the modern mainstream. Despite important methodological implications of the Cambridge critique in general and Kaldor's contribution in particular, not much has been written within the literature¹⁴ to provide a coherent framework to interpret and assess the methodological basis which informed this critique of mainstream economics. Kaldor's critique of equilibrium orthodoxy also has resonances with various

¹⁴ There are a few exceptions such as Lawson's (1989) critical realist interpretation of Kaldor's methodology.

contemporary debates regarding problems with mainstream theorising, some of which have been discussed above. In particular, Kaldor's specific contributions lay in his articulation of a distinct methodological perspective based on the notion of *Stylized facts* and his own alternative formulations on growth. His intellectual commitment to increasing returns led him to reject formal macroeconomic modelling and equilibrium notions as inappropriate and inadequate in explaining the dynamics of growth in modern capitalist economies. Accordingly, in the last phase of his career he attempted to develop a model of disequilibrium growth based on the principle of cumulative causation, described by Eatwell (1982) as the progeny of a 'marriage between Allyn Young's view on increasing returns and Keynes principle of effective demand'. It is arguable that Kaldor's contribution lay not merely in his critique of the equilibrium orthodoxy which could be seen as largely derivative, but in his proposition that economics should proceed by identifying and explaining *Stylized facts*. According to him research in social sciences should proceed inductively by identifying 'empirical regularities' which are however historically contingent, changing over time with transformations in the economic system. Hence, it is not necessarily the goal of economics to develop a grand unified theory accounting for all empirical regularities; separate theories maybe required for each Stylized fact. The implications of Kaldorian contributions to economic thought accordingly provide interesting perspectives to explore some of the questions raised in the broader discussion carried out above, which are explored throughout this dissertation from a largely methodological vantage point. A brief overview of the structure of the remaining chapters now follows.

In the second chapter, following a kind of a social constructivist approach, an attempt is made to historically contextualise Kaldor's evolution as an economist relative to main axes and lines of development of economic thought, thereby placing the present work in context by identifying arguments, techniques and connections to other works in the subject area. Also side by side, a parallel discussion on the evolution of theoretical and methodological debates within the discipline is attempted to contextualise the arguments of the present study.

In the third chapter, an attempt is made to understand, appreciate and assess the specificities of Kaldorian analysis and contributions to economic thought through a rational reconstruction of Kaldorian economic analysis from a methodological perspective, which also involves an assessment of his critique of the neoclassical orthodoxy and his own distinctive methodological stance. It begins with a brief discussion of the structure and strategy of neoclassical theorising to provide the background for Kaldor's critique of equilibrium orthodoxy followed by a discussion of the ontological and epistemological issues involved therein which in turn leads to a detailed analysis of his methodological stance of theory building on the basis of Stylized facts. It is argued that an open system organicist ontology and epistemology sets the grounds for Post-Keynesian analysis in general and Kaldorian analysis in particular. In this context, Lawson's critical realist reading of Kaldor is analysed in detail. While it is suggested that ontological presuppositions of critical realism could be shown to be consistent, not merely with Kaldorian analysis, but also a range of heterodox traditions in economics, it is also argued that Lawson's conceptualisation of Stylized facts in terms of the method of abduction contains several ambiguities and is not very convincingly established. The chapter concludes by suggesting that a more comprehensive understanding of Kaldor's methodological contribution perhaps also requires a consideration of the more specific aspects of his analysis which are then explored in detail.

An attempt is made to appraise and place Kaldor's contribution within the methodological debates from a critical perspective by picking up the threads of various arguments in the fourth chapter, the background for which has already been laid down in the earlier chapters. Since his account of growth forms the basis of his methodological assertions and provides the context for most of his theorising in other areas, a brief overview of the important theoretical insights in his own alternative formulations on growth and distribution is carried out. Also as important applications of his methodological assertions, they become especially important in facilitating comparisons and in providing specific instances for critiquing such assertions from a methodological vantage point. This is followed by a very brief overview of contemporary growth analysis carried out by 'endogenous growth theories' to contextualise Kaldor's critique of

neoclassical growth theory. Towards the end, a discussion of a short exchange between Kaldor and Paul Baran over the latter thesis on the Marxist theory of underconsumptionism alongwith a review of certain criticisms of Kaldor's own formulations on growth, is attempted, in order to highlight certain features of and point out certain inconsistencies and deficiencies in Kaldor's methodological stance of theory building on the basis of Stylized facts. The discussion is followed by concluding remarks where an overview of the present study is attempted.

Chapter Two

A Historical Survey: Contextualising Kaldor within the theoretical and methodological debates in Economics

In the introductory chapter, I had indicated the manner in which I propose to reconstruct and evaluate the Kaldorian research programme both internally, on its own terms, and externally on the basis of how it compares with alternative programs, given certain criterion involving perceptions on the nature of reality and the aspects of socio economic world the programme is oriented towards and its specific contribution to theory and methodology vis-à-vis such criterion. I'd also mentioned that this appraisal is from the vantage point of methodological pluralism, which involves a combination of broad prescriptions with historical descriptions. The need for historical descriptions or reconstructions is obvious, given that any comprehensive understanding of a research programme requires a perspective embracing a context transcending the immediacy of the problem which the analysis addresses. Such a context is rooted in the past and provides linkages with the present. This brings us to the crucial question of how such a historical description, keeping in mind the objectives of such an appraisal, is proposed to be carried out.

History or methodology: the positive normative dispute in methodological appraisals

Any methodology of a scientific practice assumes a historicity of context, whether it be the 'context of discovery' or the 'context of justification'.¹⁵ While the former allows for a 'historical reconstruction,' (an attempt to analyse the past on its own terms), the latter usually leads to a 'rational reconstruction'¹⁶ (an attempt to look at the past from a given lens in order to justify a perspective or to trace the lineage of a particular viewpoint).

In contemporary philosophy of history, historical reconstructions have been shown to be

¹⁵ Context of discovery implies the actual historical process by which scientists arrive at a given theory. Context of justification on the other hand refers to the means by which scientists try to justify their theories once they are there-which includes testing the theory, searching for corroborating evidence and so on.

¹⁶ The terminology 'rational reconstruction' and historical reconstruction' is borrowed from Blaug (1997).

virtually impossible, while rational reconstructions are invariably anachronistic. To believe it is possible to write a history of economics without one's political and philosophical preconceptions being involved in the preanalytical act of selection of what constitutes relevant economic history, is to commit the inductive fallacy in history writing. On the other hand rational reconstructions usually degenerate into an uninteresting exercise in omniscience. An awareness of the pitfalls in either form of writing keeping the distinction between them firmly in mind is the only way to carry out the exercise with some degree of credibility. However in practice, it is usually impossible to separate the two in general and in such an attempt as the present one in particular.

In contemporary philosophy of science the closest we come to discovering an appropriate framework for approaching a historical description of the kind attempted here could possibly be found in the '*Structure of Scientific Revolutions*' by Thomas Kuhn (2nd edition, 1970) or in the Lakatosian framework (Lakatos 1970) as outlined in the *methodology of scientific research programmes* (MSRP). At this point, it might be useful to briefly outline the two given frequent mentions of the terminology used and the methods espoused in both the frameworks in the present study.

Kuhn in his revolutionary work, regards 'normal science' i.e. problem solving activity in the context of an orthodox theoretical framework as the rule, and the overthrow of one framework (paradigm¹⁷) in consequence of repeated refutations and mounting anomalies, the exception in history of science. The practitioners of 'normal science' form an invisible club in the sense that they are in agreement among themselves both on the problems that require solution and on the general form that the solution should take; moreover only the judgement of colleagues is regarded as relevant in defining problems and solutions, in consequence of which normal science is a self sustaining, cumulative process of puzzle solving within the context of a common analytical framework. Breakdown of normal science, when it does occur is heralded by the proliferation of theories and the appearance of methodological controversy; the new paradigm offers a decisive solution to hitherto neglected puzzles, and this solution turns out in retrospect to have long been recognized but previously ignored; there is a paradigmatic

¹⁷ Despite certain modifications in his definition and use of the term paradigm, the focus remained on the meaning of the term to bring out "the entire constellation of beliefs, values, techniques and so on shared by members" which made normal science possible (Kuhn 1970).

incommensurability as the old and the new generations talk past each other with the unsolved puzzles in the old paradigm becoming corroborating examples in the new; since there is always a loss of content as well as gain, conversion to the new approach takes on the nature of a religious experience, involving a 'Gestalt' switch; and as the new paradigm conquers, it becomes in turn the normal science of the next generation(Blaug 1992: p. 27-30). The most controversial aspect of Kuhn's description regarding periods of 'revolutionary science' was his thesis of incommensurability which undercut any objective, rational basis for science and the notion of linear progression in science with better (more objective or logically more compelling) theories replacing older ones. Competing paradigms provide different world views, dictate meanings for terms and even determine selection of data for testing (i.e. *facts are theory laden*). Hence comparison merely on the basis of some objective, rational criterion does not provide conclusive grounds for choice and the establishment of a new paradigm involved amongst other things, an act of faith, with persuasion and values of a scientific community playing an important role in the paradigmatic shift. As regards the criterion for theory choice, Kuhn was of the view that though 'standard criteria' were in the nature of norms and values of a scientific research community, their application depended on the subjective interpretations of individual members of the community (given that such criterion are usually imprecisely defined; competing theories may meet different criterion; the weights attached to different criteria by different individuals may differ).

Lakatos's MSRP provided a compromise between the normative prescriptions of philosophers of science and descriptive accounts of historians of science, claiming to be a theory which was both descriptively coherent and prescriptively adequate (Lakatos 1970). Lakatos introduced the concepts of hard core and protective belts to describe the structure of scientific disciplines. Each scientific discipline consists of a dynamic system comprising of a range of research traditions, each with a sequence of theories which are subject to change over time. The hard core of a research programme contains a number of fundamental assumptions, usually deemed to be irrefutable and therefore not the object of interrogation by those working within the programme. The encounter with empirical data takes place in the protective belt, where the empirical implications of the programme are scrutinised and eventually modified if deemed necessary. This maybe a protracted

TH-13211

process and is reflected in a series of 'problem shifts.' The prescriptivist foundation of Lakatosian methodology is contained in the criterion for evaluating the progressive or degenerative nature of the problem shifts. A research programme is deemed to be progressive if every new theory, or problem shift, brings into light some previously unanticipated or novel facts which are then corroborated. Failure to come up with novel facts and their corroboration is sufficient grounds to judge a research programme to be degenerative. However Lakatos provides no satisfactory rule regarding the acceptable time horizon within which the status of a particular research programme is to be judged or within which it is rational to abandon a degenerative research programme. It is possible for a programme to be initially progressive, then to stagnate or even degenerate and then become progressive again. Lakatos also claimed that his framework provided an adequate description of the actual 'internal history of science' which was a history of 'rational preferences' of scientists for progressive over degenerative SRP's, for content gain always exceeded any content loss in such cases. However, aware of the fact that not all science fits so conveniently into a notion of steady cumulative scientific progress in which older theories are constantly superseded by newer, more general ones, he allowed for an 'external history' of science. Such a history included not only all the normal pressures of a social and political environment, but also failures of scientists to act in accordance with MSRP; such as to accept a degenerating SRP in preference to a progressive one on grounds of greater perceived elegance and sophistication of the former (a phenomenon whose existence has been proclaimed by several economists including Kaldor in the contemporary dominance of neoclassical orthodoxy).



Has the history of economic thought been a progressive one? Is there any objective basis for judging such progress? Has there been any commensurability and comparability between competing paradigms in economic history? Has the history of economics been one of periods of 'normal science' alternating with those of 'revolutionary science' or has it been one of gradual evolution of competing paradigms involving progressive 'problem shifts'? The questions are largely unsettled ones and any attempts at their resolution would require expertise far greater than that of a student of economics. However a few general comments related to the exercise I'm about to embark on might be useful over here. Kuhn's analysis is generally sought to be superior for describing cataclysmic

330 DISS
N56 St
TH13211

changes and theoretical monism while Lakatos's model could prove to be more appropriate for describing gradual evolution within any single research tradition and the coexistence of competing theoretical traditions over time. Not only could the two models be fruitfully used to complement analysis but there also might be more suitable frameworks of analysis. It's important however to remember that all facts, including historical ones are theory laden, and hence one's subjectivities play an important role in any historical reconstruction. Probably the best way to begin is with an explicit statement of purpose.

The contemporary relevance of the present study within this context, I would contend, lies both in the analytical content and insights of Kaldorian economic analysis as well as the political and philosophical preconceptions of his times wherein lay the seeds of some of the current debates over the aims and methodological practices of economics and economists. Hence, though this is a methodological appraisal rather than a historical account, I shall begin with a historical contextualization, as this assessment is based on studying the specificity of Kaldorian economic worldview and methodological perspective relative to main axes and lines of development of economic thought, thereby placing the work in context by identifying arguments, practices and connections to other works in the subject area¹⁸.

One of the most important aspects of such a contextualization is to bring out Kaldor's subjective position in terms of being an active participant of the 'Keynesian revolution' in the discipline in the Kuhnian sense, where a paradigmatic shift from the established neoclassical orthodoxy occurred. More significantly, he was one of those participants of the Keynesian revolution and contributors to the so called 'post Keynesian paradigm' (Eichner and Kregel, 1975) who lived to see the reestablishment of the neoclassical orthodoxy or the liberal resurgence in mainstream economics within the academia with an emphasis on mathematical deductivist modelling in its rejuvenated form. The latter was largely a consequence of the growing axiomatisation, formalization and mathematisation of the discipline- a quiet but steady change occurring in the aftermath of the 'general theory' whose effects were not fully felt until much later. His career as an

¹⁸ Accordingly the historical survey attempted is circumscribed by its necessity to contextualise Kaldor's subjective position within the discipline. In this process a survey of literature dealing with a wide range of other, equally rich developments has not been touched upon.

economist spanned an era (roughly mid 1930's to mid 1980's) rich in developments and debates which brought about marked transformations in the landscape of economic theory. It was not only characterised by diversity of schools of thought, but also by convergence, overlap and shifts- sometimes incomplete, sometimes temporary- which made the borders nebulous or mobile. This was also reflected in the complex evolution of the thought processes of the individual economists themselves: many followed their uniquely chartered paths, some not belonging to any school, others following paths which led them successively to a variety of schools. As for those whose tendencies had linked them to a single school, their place on the economic scene and the manner in which they were perceived also changed. Kaldor's biographical evolution as an economist brings out many of these aspects. These had significant implications for the last and perhaps the most interesting phase (from late sixties to early eighties) of his career which is central to the present analysis.

Accordingly this attempt at a 'rational reconstruction' of such an evolution is carried out through its demarcation into three distinct phases (1930-1945, mid forties to mid sixties and late sixties to early eighties) for expository convenience with only a partial exposition of the last phase (analysed in greater detail in the next chapter). The demarcation, as I hope will become obvious as I proceed, serves the useful purpose of underlining important continuities and breaks not merely in Kaldor's evolution as an economist but also in the evolution of theoretical and methodological debates within the discipline. A parallel discussion of the history of debates on economic methodology in the literature is also attempted to contextualise the arguments of the present study.

The early phase – the Keynesian revolution (1930-1945)

The *General theory of Employment, Interest and Money* by J.M.Keynes appeared in 1936, a little over a century and a half after Adam Smith's *Wealth of Nations*. The latter constituted a point of departure in the course of political economy which later came to be known as Classical Political Economy on its transformation by the marginalist revolution, marking the advent of the neoclassical orthodoxy which was to subsequently dominate economic thought. This transformation was in part necessitated by acutely significant historical and institutional changes (in particular the rise of Marxism and its influence

over the European Workers' organisation) which called into question long standing inadequacies of the theory, methods and policy doctrines of the classical theory. As stated by Hutchison,

The subject's simple message of laissez-faire was increasingly being called into question by new political forces.....In particular; institutional changes were calling in question the significance of both the natural wage theory and the wages fund doctrine. (1994: p.175)

Though fundamentally different in certain aspects from the classical theory, the marginalist theory further developed Smith's parable of invisible hand, giving it a mathematical formulation and upheld Say's law of markets with Walras's law bringing out the dichotomy between real data and monetary data. Its theory of value and distribution based on the notion of marginal productivity was to later come under severe attack by the post Keynesian school to which Kaldor belonged.

The year 1936 was almost halfway through a period of almost continuous violent turmoil between 1914 and 1945, where the First world war, the agricultural crisis post war, the Great Depression, Second World war followed each other in quick succession with only brief interludes in between. Another important development was the initiation of the process of decolonisation of the third world which was to have a significant impact decades later in terms of the historical and institutional changes it brought in its wake. Throughout these various developments, reality never stopped contradicting the vision shared by several classical and neoclassical economists that the freeplay of markets is enough to ensure full employment of resources and their optimal allocation. However a broad range of critiques were already being heard defying the liberal orthodoxy with its advocacy of increased monetary rigour and price flexibility as the only remedy in face of growing instability and unemployment. The importance of the *General Theory* inscribed in this broad intellectual transformation taking place during the 1930s and 1940s, lay both in its claim as a theoretical construction replacing the existing orthodoxy and in its theoretical justification of interventionism (Beade and Dostaler 1995 : p.18-43).

In this broad background, began the career of Nicholas Kaldor who after graduating from London School of Economics, (the citadel of neoclassical orthodoxy through which both the Walras Pareto school and the Austrian school entered the English speaking world in those times) taught there from 1932 to 1947. Here he made several contributions

to mainstream theory on diverse topics like the theory of the firm, capital theory, trade cycle theory, welfare economics and monetary theory. In these early years his chief mentors were Robbins and Hayek, but he gradually broke away from their narrow approach to economics and their libertarian philosophy to become a swift convert to Keynes's ideas. The historical context, alongwith discussions with friends John Hicks (who was instrumental in his becoming acquainted with the monetary readings of the Swedish school, in particular those of G. Myrdal) and Abba Lerner played an important role in this early conversion.

Another important influence, especially in terms of its relevance to current analysis, during that time was that of Allyn Young , who came from Harvard to LSE in 1926 to succeed Edwin Cannan, but his stint there was unexpectedly cut short due to his premature death in the winter of 1928-1929. One of his papers in particular, '*Increasing Returns and Economic Progress*' (Young 1928) left a lasting impression on Kaldor. In spite of its serious implications for the entire structure of neoclassical economics, it was described by Lionel Robbins as 'one of the lasting and most important contributions to pure economic analysis of that part of the century'(L. Robbins, *Autobiography of an Economist*, London: Macmillan,1971) The paper revived the Smithian idea of economic progress as a cumulative process, in which through increasing returns as a macroeconomic phenomenon , the division of labour expands the market and expansion of market leads to further division of labour. These insights formed part of a mature Kaldor's¹⁹ challenge to equilibrium theory as well as his own works on growth and applied economics. As he himself states,

It was to him that I owe a basic distrust of abstract systems *per se* and an awareness of the need to adapt the tools of theoretical analysis to the practical problems which they intend to illuminate. Economics, in Allyn Young's view, is best defined by the particular interests which have prompted its founders –not by its subject matter as such. (1986 [1989a]: p 14)

¹⁹ Interestingly G.M. Hodgson points out an early link here between Kaldor and the institutionalists: "Young, a friend of Mitchell and admirer of Veblen' (Dorfman, 1964) taught Kaldor, and hence there is an -hitherto unexplored -link between the Veblenian and Kaldorian schools of thought. This complements the acknowledged influence of Myrdal on Kaldor" (Hodgson, 1993).

Another significant development in economic theory during the inter war period was the emergence of theory of monopolistic competition. Initiated by the cost controversy that originated with Pierro Sraffa's famous paper 'The laws of returns under competitive conditions' (Sraffa, 1926), the analysis had its origins in the work of Chamberlin, Harrod, Kahn and Joan Robinson who attempted in the 1920's and 1930's to reconcile orthodox theory with the existence of monopolies. Kaldor also made some early theoretical contributions to the debate.²⁰ Though the developments were important in terms of their implications for 'micro theoretic' basis of Keynesian macro analysis, later on Joan Robinson (severely critical of her early work) and more generally the post Keynesian theorists (especially Kaldor) would shift towards distinctly Kaleckian positions, endorsing the concepts of markup pricing and an income distribution based on class conflicts.

From Deductivism to Positivism: A Shift in Methodological Perspectives

In such times of cataclysmic changes, methodological perspectives were bound to undergo transformations too. The relationship between the general standing of the discipline and its methodological base has over time been both complex and subtle. At the same time, as pointed out earlier, the preoccupation of the subject with the scientific method ensured the significant exogenous influence of major developments in the philosophy of science on almost all methodological debates within the discipline.

Upto the 1930's the dominant methodological perspective could be best described as 'abstract deductivism'. In the view of J. S. Mill, scientific enquiry in a subject like 'political economy', given its complexities, called for the application of the deductive method. Here induction was to be primarily used to establish the basic psychological and technological laws and their economic implications were to be deduced given the specifications of relevant circumstances. The role of empirical confirmation was to verify the applicability of such implications, the correctness of the deductions made and to ensure that important causal factors were not left out. However such testing was not to lead to any questioning of the basic laws which had already been arrived at through

²⁰ For details, see 'Market Imperfections and Excess Capacity' by N. Kaldor, *Economica*, Feb 1935. However interestingly as early as that, his analysis focussed on the incompatibility of the notion of optimality implied in equilibrium analysis with the existence of increasing returns.

introspection or experimentation. These basic laws were not in the nature of universal laws but were statements of tendencies, subject to counteracting influences which could not be specified in advance necessitating the use of vague *ceteris paribus* clauses. Economics was accordingly an 'inexact science' (Hausman 1989, 1992) which explored the consequences of these basic, but inexact laws, the implications deduced from which might not always obtain.

As noted by Mark Blaug while commenting on the role of 'verification' in the views of classicists (Senior, Mills, Cairnes) and early neoclassicists (like J.N.Keynes),

...one verifies in order to discover whether 'disturbing causes' can account for the discrepancies between stubborn facts and theoretically valid reasons; if they don't, the theory has been wrongly applied but the theory itself is still true. The question of whether there is any way of showing a theory to be false is never even contemplated. (1980: p.81)

The transition from classical to neoclassical economics brought with it certain shifts in methodological perspective as well given the latter's emphasis on individual preferences and decision making. However despite such differences, early neoclassical economists in principle agreed with Mill that the basic premises of economics were justifiable and that empirical failures did not call them into question. This continuation in the line of methodological thought was articulated in Lionel Robbins famous monograph, '*An Essay on the Nature and Significance of Economic Science*', (1940) with Mill as the principal figure to whom Robbins acknowledges his intellectual debts. In certain respects, strong echoes of such views linger in contemporary debates on theory appraisal and methodology. Infact, Hausman goes so far as to state that "if one updates the language and the economic theory, one has the view to which, I suggest, most orthodox economists (regardless of what they may say in methodological discussion) still subscribe." (1989: p 117)

However this entrenched methodological stance of 'Subjectivism, methodological individualism and the self evident nature of the basic postulates of economic theory' was challenged in the 1930's with the publication of T.J. Hutchison's '*The significance and basic postulates of Economic Theory*' in 1938. Though not the first criticism of deductivism *per se* (the German Historical School amongst others had launched a sustained attack on political economy's deductive methodology in the nineteenth century),

it caught the profession's interest not least because it exposed economics to some of the central ideas of logical positivism, which had become by the beginning of twentieth century, 'the modern scientific worldview'.

The early logical positivists²¹ had a high regard for the achievements of natural sciences, including mathematics and logic. They believed in the rationality and objectivity of science and held scientific knowledge based on empirical testing, logic and mathematics to be the only valid form of knowledge which led to the discovery of 'truth' that there is. They also believed in the unity of the scientific method (the methods of obtaining valid scientific knowledge are the same for all fields of enquiry). While logical positivism, along with Karl Popper's early work (Popper 1934) advocating the criterion of falsifiability instead of verifiability for scientific inquiry, informed Hutchison's critique, it specifically focussed on several aspects of economics which he found to be fundamentally flawed. He was extremely critical of the standard and what he termed as 'inadmissible' use of *ceteris paribus* clauses in economic analysis while describing what Mills and Robbins called 'tendencies' or 'inexact laws'. Since the content of *ceteris paribus* clauses was never specified, these statements were found to lack empirical content and hence 'unfalsifiable'. Either things were as claimed by the tendency, or were influenced by 'counteracting tendencies'. No outcomes were prohibited, and no evidence seemed to justify a revision of the 'basic laws' or 'tendencies'. However in his criticism, Hutchison did make allowance for legitimising conditions when the *ceteris paribus* clause might be fruitfully employed in conjunction with empirical regularities. He also denounced the use of 'hypothetical' or 'isolating' method of reducing complex situations to simplified cases with a view to providing an understanding of actual situations through 'successive approximations'.²²

Hutchison's critique was an effective representation of the disconcerting logical positivist attack on the profession. The initial hostile response came from F. Knight(1940) who argued that the positivists underestimated the complexities involved in testing of scientific theories in general and those relating to social sciences in particular, given the

²¹ A loosely knit group of philosophers who met in Vienna in the early 1920's and early 1930's under M. Schlick and later fled to the states to escape persecution by the Nazis.

²² As shall be seen later, there exist distinct echoes of such a methodological stance in Kaldor's own critique of 'equilibrium economics'.

fact that the latter dealt with human motivations, values and goal directed behaviour and reasoning. There was a short but inconclusive interchange between Hutchison and Knight, which as stated by Boylan and Gorman (1995: p 14), served as a paradigmatic example of Kuhnian incommensurability. However attempts at positive transformations of theory, such as Samuelson's "operationalism" (1949), were already beginning to appear. His revealed preference theory in particular provided a behaviourist recasting of preferences and utility in terms of observable claims about human actions. The call for liberating economics from 'abstract, tautologous and empirically vacuous' modes of theorising in favour of reformulations in form of empirical laws " began the first and only major change in economists' official position on the appraisal of microeconomics "(Hausman 1989) in particular, and economics in general .

The age of interventionism: the Keynesian Revolution (mid forties to late sixties)

The *General Theory*, within the context of the large movement of ideas of which it was part, produced several developments which broadly came under the rubric of Keynesianism that marked the postwar period, as much in the field of economic policy as in that of applied economics and theory. Karl Polanyi (qtd in Bead and Dostaler 1995:p.33) coined the phrase 'the great transformation' for the collapse between 1900 and 1940 of an international system which had taken its roots in the nineteenth century, based on four institutions: the balance of powers, the international gold standard, the self regulating market and the liberal state. Given the historical context, the political climate was conducive to interventionism. The aftermath of the Great Depression, exigencies of post war Europe, the rise of the Soviet Union and the emergence of erstwhile colonies as newly independent nations amongst other things made growth, rehabilitation and development everybody's primary concerns. The crisis of capitalism (that arose as a consequence of the historical developments outlined above) had made economic policy necessary; and the management of aggregate demand and its principal elements was to be the primary instrument: 'Planning for full employment and maximum production involves, among other things, planning for stability' (Hansen 1947: p.3). Full employment as a priority objective gained widespread acceptance throughout the developed world. One saw a victory for Keynesianism made concrete by renewal of

approaches, tools of analysis and economic policies, although it was interventionism which basically triumphed.

Stimulated by Keynes and the exigencies of war, Kaldor's involvement with practical matters concerning economic policy which was to continue throughout his long career began around this time. He began writing regular reviews of the National Income White Papers (on war finance) as they appeared. He also worked out the financial implications of the influential Beveridge Report on Social Insurance (1942) and wrote the Appendix B to the Beveridge report on *Full Employment in a Free Society* (1944) wherein he worked out the quantitative aspects of full employment. He established a reputation for himself as an meticulous and insightful applied economist able to combine 'theory with close factual analysis' (Kaldor 1986) This led to several invitations during the postwar period which took him from France to Hungary to Germany to United States. There followed, at the invitation of Gunnar Myrdal, his appointment as Director of the Research and Planning division of the newly established Economic Commission for Europe in Geneva originally created to administer Marshall Aid. Here he was responsible for initiating, and writing, the annual Economic Surveys of Europe acclaimed for their penetrating analysis of the economies of both Eastern and Western Europe. Though subsequent annual Surveys improved greatly in quantity and quality of their information, the basic framework- 'the comparative treatment of the rates of progress of different countries and the conclusion drawn from the commodity analysis of international trade has remained the same' (Kaldor 1986: p 25).

In 1951, as a dissenting member of the Royal Commission on the Taxation of profits and Income, he wrote his own minority report, the *Memorandum of Dissent* (1955), where he crystallised his own views on the structure of an equitable and just tax system. His later book, *An Expenditure Tax* (1955), outlined the case for substituting expenditure for Income as a basis for taxation. From here began his role as a policy advisor on matters of taxation, fiscal and monetary policy and economic development which took him to several corners of the globe.²³

²³ Interestingly as pointed out by L.L.Passinetti this never included any invitation from any official institution, whether progressive or conservative, in the United States. He traces the reasons to Kaldor's methodological critique of both the neoclassical orthodoxy as well as Marxist economic analysis. (Nicholas Kaldor: *a few personal notes*, in Symposium on Kaldor's Growth Laws, Journal of-

Coming back to the larger historical context, the 1950's and 1960's turned out to be the golden age of interventionism. The economies of nearly all developed nations enjoyed a period of considerable success: very high levels of employment were accompanied with only mild inflation, while some of the more important countries experienced unprecedentedly high levels of growth. This also brought about an intellectual boom of confidence in the subject.

“Economists could plausibly convince themselves that they were on their way to solving the basic economic problems confronting mankind – unemployment and economic instability.”(Hutchison 1994: p178)

As Prof. C. Goodwin states,

“(After the war), economists were credited with helping to avoid the return of depression and with constructing a new international economic order.”
([1989] qtd in Hutchison 1992: p.13)

The broad consensus concerning economic policy was reinforced by theoretical developments in the field of applied economics : a fresh and simple reading of national economies , with large macroeconomic aggregates and the functional relationships which linked them provided with a coherent structure and empirical data through the development of a comprehensive system of national accounts. Their design, conception and use in turn was facilitated by the conceptual framework offered by Keynesian analysis. During the entire postwar period, there was mutual interaction and inspiration which guided the progress of national accounting frameworks, macroeconomic analysis and econometrics (Beade and Dostaler: 1995).

Optimism at the prescriptive level was complemented by theoretical and methodological buoyancy. The transition from the ‘dismal science’ to ‘positive economics’ brought with it pluralism of method and revival as well as growth of economic heterodoxies. However parallel to the mutations brought about by the Keynesian revolution, a more fundamental change was occurring: the growing formalisation and mathematisation of economics within the academic echelons propelled by the growth of econometrics and techniques of mathematical analysis alongwith the

reformulation of general equilibrium framework. This led to rejuvenation of the neoclassical orthodoxy, located in some respects in classical economics but largely within the theoretical edifice laid down by the marginal revolution, further strengthened in terms of its theoretical coherence and logical consistency, its foundations rooted in academic tradition. It also led to a remoulding of Keynesian macroeconomics in terms of the equilibrium through the 'neoclassical synthesis'. However far from bringing together the more penetrating insights of classical and Keynesian analysis, it was a syncretic rapprochement of compatible elements and their insertion in formalised frameworks, theoretical and econometric models (ibid). The rewriting of Keynes in terms of simple functional relations between macroeconomic magnitudes was carried out at the expense of certain essential Keynesian insights and hypotheses. However it was this synthesis which became the new dogma and dominated economic thought for a long time during the postwar period.

The broad consensus nevertheless did not lead to unanimity. The era was also marked by the establishment of Post Keynesian heterodoxy, the evolution of institutionalism (enriched by the contributions of original thinkers such as G. Myrdal, Perroux, J.K Galbraith, R. Coase and H. Simons) and the revival of Marxism (significant contributors included Paul Baran, Maurice Dobb, Paul Sweezy, C. Bettelheim, Ernst Mandel and Oscar Lange).

A significant parallel development was the emergence of Development Economics as a new analytical perspective focussing on a body of data and of associated problems where the centre of attention was twofold: firstly the causes of relative poverty of underdeveloped countries, and secondly the potential way forward for these countries; the specification of route to progress in these largely pre-industrial regions. A distinct body of literature here which represented Marxist political economy had within its corpus a dynamic theory in Marx's own work, a subsequent body of analysis of economic imperialism and its implications for colonial development, and another body of analysis debating the route to growth for an independent but initially largely agricultural socialist economy: the Soviet Union.

However In the 1940's and 1950's the early analysis of the two dominant perspectives: one in Western Europe and North America, and one, initially atleast, largely in Latin

America (the structuralist paradigm) despite its differences had certain methodological similarities. There was a rejection of the neoclassical paradigm as the appropriate basis for understanding the problems of developing economies. All emphasised the lack of realism in a theoretical edifice which assumed that 'every disturbance provokes a reaction within the system directed towards restoring a new state of equilibrium' (Myrdal 1958). Some also pointed to the failure of the static theory of comparative advantage as a guide to optimal long run resource allocation strategies in primary exporting economies. This was complemented by a critique of assumptions of general equilibrium theory as an empirically valid basis for individual investment decisions. It was widely criticised for failing to reflect a world characterised by indivisibilities, externalities, market failures and imperfections. The policy conclusions reached, in keeping with those sweeping the developed world, largely advocated public intervention to accelerate the pace of growth.

From flexible programming to Soviet planning, to the multiple efforts to establish straightforward and robust macroeconomic models (especially those of J. Tinbergen and G. Myrdal), planning was the undisputed tool of industrialisation and modernisation for most of the third world (inspired largely on the lines of the Soviet model). There was an emphasis on the dynamic aspect of the growth process as brought out in the analytical terms used in various conceptualisations: big push, vicious circles, low level equilibrium traps, circular and cumulative causation, forward and backward linkages and so on. Though the extent and the nature of Keynesian influence on development economics has been debated upon in the literature²⁴, suggested areas where it might have manifested itself include the adaptation to analysis of developing economies of Harrod Domar growth model, the structuralist concern with the role of aggregate demand as the engine of growth and the propensity of early development economists to think in macro economic terms. All these developments are also significant for our present study given Kaldor's role as a policy advisor to various developing countries specifically in the field of taxation as well as the later shift in his theoretical perspectives on explanation of differing industrial development paths of different countries. In particular the influence of Gunnar Myrdal's (1957) analysis of underdevelopment in terms of cumulative causation on the last phase of Kaldor's formulations on growth has been widely acknowledged.

²⁴ See for example Lewis (1954), Lal (1983), Johnson (1978).

Kaldor's explanation of the geographic concentration of economic activity, for instance, is very similar to Myrdal's work on the causes of regional specialisation of activity and income inequality (Kaldor 1970, 1983). Similarly Kaldor's critique of Heckscher-Ohlin-Samuelson trade theory and his emphasis on the applicability of cumulative causation in both advanced and developing economies has parallels in Myrdal's writings on the subject.

Logical Positivism, Empirical Testing and Friedman's Predictionism

Meanwhile the profession's response to the logical positivist critique during the 1940's was characterised by intense empirical research that attempted to test fundamental propositions of neoclassical economics, particularly the neoclassical theory of the firm, such as that of Hall and Hitch (1939) in U.K and Lester (1946, 1947) in the United States.

The results which questioned the acceptability of key propositions of neoclassical theory sparked off several responses. Fritz Machlup in particular, developed a sophisticated response (Machlup, 1946, 1947) to the emerging positivist critique based on the later work of logical positivists or logical empiricists as they had now become. He argued that acceptance of theories where direct testing of basic assumptions (given the difficulty in carrying out controlled experiments in a social science like economics) was not possible should be based on their capacity to generate correct empirical observations. He drew parallels with acceptance by the logical empiricists of the role of theories in physics which posited the existence of unobservable entities yet generated correct empirical predictions. Despite various ambiguities and subtle shifts in his positions, he was able to deflect what he considered to be the most damaging aspects of the 'ultra empiricist' attack on the discipline.

However the "centrepiece of postwar economic methodology" (Blaug 1980: p 103) and "probably the best known piece of methodological writing in economics" (Caldwell 1982: p173) was Friedman's *The Methodology of Positive Economics* (1953). Nevertheless it hardly had any admirers amongst the writers on economic methodology. M. Blaug on retrospection was struck by 'the lack of methodological sophistication' the entire debate surrounding the essay displayed, arguing that the unsophisticated positions adopted could "only be understood as a reaction to a century of critical bombardment of orthodox

theory.” (Blaug 1980a: p.120) It remains, till date, a puzzling piece of methodological writing where a dogmatic insistence on the correct criterion of theory testing is combined with subtle qualifications, ambiguities and a novel absence of any explicit references to the literature on philosophy of science.

Predictive capacity rather than explanatory power was Friedman’s declared aim for a positive science such as economics in the essay. He argued that the entire thrust of the empirical critique of the 1940’s was redundant as it was based on an empirical assessment of the assumptions, rather than the implications or predictions of the neoclassical theory. However as noted by Hausman, it was an ‘odd instrumentalism’ which suggested that *unrealism* or *proven falsity* of assumptions does not matter unless it detracts from a theory’s performance in predicting the phenomenon in which one is interested. By stating that a “ theory is to be judged by its predictive power for the class of phenomenon which it is intending to explain”, Friedman’s not merely stating that one should use a theory if it ‘works’, but that all one wants of science are theories that work for particular purposes. His ‘instrumentalist’ defence of neoclassical theory attempted to undermine not merely contemporary criticism but a critical tradition which had its roots in the German Historical school as well as in the American institutionalists. These traditions had questioned the value of abstract theorising as well as the ‘unrealistic’ and ‘unreasonable’ assumptions of neoclassical theory. I shall come back to a more detailed discussion of the instrumentalist position *per se* in the third chapter.

At the moment however, it’s interesting to note a point made by various surveys and analytical writings on economic methodology²⁵ regarding the happy coexistence of two mutually incompatible philosophies of science: the aprioristic and deductivist perspective of Robbins with its epistemological commitment to the correctness of the basic postulates of economic theory and Friedman’s instrumentalist opposition to realism of assumptions, on which, the methodological foundations of neoclassical orthodoxy were supposed to rest. As Koopmans perceptively notes,

After more than a century of intensive activity in scientific economics, two economists who have made outstanding contribution to our science, and whose positions on questions of economic policy are moreover not far apart, seek the ultimate basis of economic knowledge in considerations

²⁵ See Koopmans (1957) and Boylan and Gorman (1995)

which (a) contradict each other and (b) are each subject to strong objections. One is led to conclude that economics as a scientific discipline is somewhat hanging in the air...the positions which our two authors so strongly (but contradictorily) embrace have in common that, in so far as either is adopted, its effect is a conservative one...in either case the argument surrounds and shields received economic theory with an appearance of invulnerability which is neither fully justified nor at all needed. (1957: p 141-2)

The Emergence of the Post Keynesian Paradigm

A postwar development significant for our present analysis was the emergence of the post Keynesian paradigm: a diversified and heterogeneous current in economic thought, with a particular lineage of thought running from Keynes, through Kalecki and for many to a return to some aspects of the works of the classicists, in particular, Ricardo, also Smith and, for some, to Marx. It sought to extend some of the central Keynesian insights which had brought out a rupture in traditional economic thought: As Paul Davidson claims (Davidson, 1981, p 171),

For members of the Post Keynesian schools the notions discussed above-historical time, uncertainty, expectations, political and historical institutions (especially money and forward contracts)-represent fundamental characteristics of the world we inhabit- *the real world*.

Amongst others, it was founded by a small group of Cambridge (U.K) economists, in particular Joan Robinson, Richard Kahn, Pierro Sraffa and Nicholas Kaldor alongwith important contributions from outsiders like Roy Harrod and J.Meade. It sought to extend Keynesian analysis, in particular the principle of effective demand, from income determination in the short run to determination of rate of growth of income and of income distribution in the long run. It was through a wide selection of diverse works that this body of analysis was progressively set up.

Ever since the marginalist revolution of the late nineteenth century, marginal productivity theory was the generally accepted theory of distribution. In the field of growth, the neoclassical orthodoxy was challenged in 1939 by Roy Harrod's seminal essay (Harrod 1939) on dynamic economics. Here instability in the growth process of capitalist economies was sought to be demonstrated by an extension in a dynamic setting of the theory of effective demand in an analysis combining the multiplier and the

accelerator and using the concepts of warranted, natural and actual growth rates. The neoclassical response to Harrod's *instability* problem, based on the notion of an aggregate production function with a flexible capital coefficient depending on relative remuneration of factors, was fast becoming the orthodoxy by the 1950's.

The early post Keynesian attack on the prevalent orthodoxy led to an alternative theory of growth and distribution, generating the famous 'capital controversy' between the two Cambridges (Cambridge U.K and Cambridge Massachusetts) and led to the establishment of distinct post Keynesian positions on the theory of value and distribution. As mentioned earlier, Kalecki's early formulation of a theory of distribution and pricing in his analysis of the instability of capitalist economies based on notions of mark up pricing and an income distribution based on the balance of power between the capitalists and the workers, heavily influenced the Cambridge theorists, especially Joan Robinson and Kaldor.

Joan Robinson's approach however differed from Kaldor's in several respects, especially in her insistence that long run steady growth with unemployment was possible, while the assumption of full employment was retained in most of Kaldor's early growth models. Kaldor's original contributions to the theory of growth and distribution are numerous, found in a long series of papers and in his three different versions of a model of economic growth (Kaldor: 1957, 1961, 1962).²⁶ However one of his most remarkable contributions to the theory of distribution, which also constituted the basis of his analysis on growth, was a theory of income distribution on Keynesian lines outlined in the last section of his paper '*Alternative Theories of Distribution*' (Kaldor, 1956). The theory made distribution of income between profits and wages the mechanism for equilibrating savings with the predetermined level of investments required to maintain full employment in an economic system. Based on differentiated propensities to save out of profits and wages and reversing the chain of causation in Ricardo's theory of distribution, it made wages residual while profits were seen as exogenously determined. This had far reaching implications both for theoretical analysis (as a critique of marginal productivity

²⁶ Some of the principal ideas developed in Kaldor's later analysis of growth were found in one of his earliest and most remarkable papers on business cycles (Kaldor, 1940): taking into account income distribution to explain economic dynamics and the importance of expected profit on which depends the level of changes in income distribution.

theory of distribution) as well as for policy decisions (especially taxation policy).

Changes in the distribution of income also served as a mechanism for equilibrating Harrod's warranted and natural rates of growth. However at this stage, the rate of growth was still exogenously given and hence the model was more of an analysis of distribution in the context of growth than of growth *per se*.

His distribution theory was an integral part of his more rigorous formulations on growth, with his models of growth designed to explain what he considered to be the '*Stylized facts*' of growth in modern capitalistic economies : a steady trend rate of growth of labour productivity; a steady increase in amount of capital per worker; a steady rate of profit on capital; the relative constancy of the capital output ratio; a fairly constant share of wages and profit in national income, and wide differences in rate of growth of output and labour productivity between countries (related to different levels of investment).

In addition to his distribution theory, he was probably amongst the earliest writers on growth theory to introduce the concept of endogeneity of technical progress functions. The notion of a 'technical progress functions' was used to overcome the distinction between movements along a production function (as a result of capital deepening) and shifts in the function (as a result of technical progress). In his model, capital accumulation and technical progress function are interconnected by the technical progress function which directly relates the rate of growth of output per worker to the rate of growth of capital per worker. However, the rate at which technical progress is introduced falls with increased accumulation. Given an investment function in relation to expected profit, rate of growth of output per worker becomes equal to the rate of growth of labour productivity. The flexibility of distributive shares supplies the necessary saving that the economy needs to grow at that rate, which represents the long run equilibrium growth rate. As the rate of growth of output per worker and labour productivity grow at the same rate, the capital output ratio is constant in long run equilibrium. Given the constancy of capital output ratio and a constant share of profits, the model then demonstrates that the long run rate of profit is uniquely determined by the rate of growth of output (which is determined by parameters of the technical progress functions) and inversely related to the propensity to save out of profits. Hence, given the natural rate of growth G , in the case

where workers do not save, the rate of profit r is given by the 'Cambridge Equation': $r = s.G$, where s is the saving propensity of profit earners. If one further assumes that profit earners save all their income, one obtains the equation of Von Neumann's model: $r=G$. Later Luigi Passinetti demonstrated that the 'Cambridge equation' held even when workers saved, provided their propensity to save is less than the share of investment in income.

Another paper in this period written with J. Mirlees (Kaldor, Mirlees 1961-2) is one of the earliest examples of a vintage model in which only new capital goods bring about a higher productivity of labour. In a stance different from other Cambridge economists involved in the 'capital theory debate,' new capital goods were taken to be different from older vintages with competition equalising the rate of growth of only new capital goods. His last article in this area (Kaldor 1966) suggested various areas for further research and clarified the hypothesis that the propensity to save of the capitalists is higher than that of workers: this propensity was attributed to managerial firms which hold back a part of their profits to meet the need for financing their investment needs.

This marked the end of the second major theoretical phase in Kaldor's career where he attempted to lay the foundations of an alternative analysis of growth and distribution explaining the dynamics of the capitalist system in the Keynesian tradition. It is important to note that the need for explaining '*Stylized facts*' of modern capitalist economies operating in historical time and space informed the perspective with which he embarked on this venture. However it was his own empirical research as well as practical experience which made him deeply disenchanted with the excessively aggregated nature of such formal macroeconomic models. Their failure to account for some crucial '*Stylized facts*' (sectoral complementarities, absence of full employment, spatial patterns of growth etc) of growth in contemporary economies led him to move towards a new phase in his analysis of growth. It is here that most of his important theoretical and empirical insights found culmination on the basis of the formulation of a matured and clearly articulated methodological stance. Before moving on to this last and perhaps the most interesting phase of his career, which is also central to our analysis, I shall make a few concluding remarks winding up both an attempt to map Kaldor's historical evolution and the broader historical contextualisation attempted over here.

As far as Kaldor is concerned, the trajectory followed by him during a highly productive career as an economist led him from an early training in neoclassicism to becoming an important proponent of Keynesianism to being actively involved in the establishment of the Post-Keynesian heterodoxy. His continuous evolution as an economic theoretician was marked by a very eclectic approach to economic analysis especially adopted by him in the last phase of his career. Here a wide range of influences, varying from the classical to Keynesian to the institutionalist schools were manifest. His role as an influential policy advisor in Britain and beyond to many developing countries and the wide range of his interests played a significant part in the combination of the theoretical and the empirical; the rejection of theoretical pursuits for their own sake; powerful intuitions and insights as well as concentration on topics of public concern. Though not a revolutionary, he was a radical thinker and a reformer within the established market institutions. He remained convinced that the operation of capitalist system, if left to itself, led to glaring inequalities and international disorders. Accordingly he was a passionate advocate of enlightened state intervention and international cooperation. Probably this contributed to his ability to completely break away from the neoclassical modes of thought where theoretical approaches were often crucially determined by liberal doctrines of *laissez-faire*. In particular, he remained severely critical of the employment of tools of static microeconomic production theory to understand the inherently inequalizing and dynamic forces which led to growth in modern capitalistic economies.

The crisis of the 70's and the liberal resurgence

The euphoria surrounding the 'age of interventionism' both at the level of policy making as well as theoretical analysis within the discipline began to disappear towards the end of 60's as significant changes affecting employment conditions and the power of trade unions began to appear along with a rise of inflationary expectations. In advanced capitalist countries, a new era began characterised by breaks in productivity growth, rising unemployment, inflationary tendencies and dysfunctions of the international monetary system. Moreover, the emergence of *stagflation*, a term coined for the

coexistence of high levels of unemployment with increasing inflation, phenomenon till then thought to be mutually incompatible, led to a questioning of the certainties associated with Phillips curve and the so called Keynesian policy prescriptions.

As far as the developing world was concerned, the insuperable difficulties faced by their economies also led to a questioning of existing perspectives on development both by the radical left and from the tradition of neoclassical economics. It was argued that while the intervening post war period with its emphasis on capital accumulation and import substitution had led to further growth and prosperity for the developed world, the conditions of the masses in developing nations had only worsened, with some also explicitly stating the latter to be, partly atleast, a consequence of the former. Others found the roots of the problems associated with prescribed policy interventions in inefficiencies of resource allocation.

The gradual dissolution of broad consensus on the 'Keynesianism' in particular, and interventionism in general led to a resurgence of alternative theories and policy prescriptions which had their roots in liberalism. In particular monetarism, of which Milton Friedman was the most influential and prominent spokesperson, asserted itself in the liberal counter-offensive, as much on the political as on the theoretical level based on a rehabilitation of the quantity theory of money and the concept of 'a natural rate of unemployment.'²⁷ Supply side economics²⁸ and diverse other liberal currents also came up with alternative contributions. 'New Classical' macroeconomics also emerged, preceded by several types of macroeconomics of Keynesian inspiration, itself challenged by disequilibrium theories and new Keynesian economics.

However the most important development, as described earlier (p 8) was the gradual reestablishment of the neoclassical orthodoxy with its emphasis on mathematical deductivist modelling (in keeping with the growing axiomatisation and formalisation of the discipline) within the mainstream academia, which continues, with some modifications and improvisations, till date.

²⁷ Kaldor in particular soon became one of the most vociferous critics of monetarism both at the policy and at the theoretical level. Theoretically he criticized the 'unscientific foundations' of the doctrine on grounds of his analysis of endogeneity of money supply as determined by demand in capitalist credit-money economies. At policy level, he launched his offensive thru numerous correspondences with newspapers, pamphlets and speeches in the House of Lords. For details see: *The scourge of monetarism*, (OUP, 1982), *The Economic consequences of Mrs Thatcher*, (London, Duckworth, 1983b).

²⁸ Identified as 'Reagonomics', the contributions of this current can be found in Laffer and Seymour's *The Economics of the Tax Revolt* Laffer and Seymour (1979). New York, Harcourt Brace Jovanovich.

The Demise of Positivism and Emergence of Methodological Pluralism

The past few decades have witnessed an explosion in the literature on economic methodology which has taken the form of various monographs, anthologies, general introductory surveys and even the emergence of new journals devoted to the field. The contemporary resurgence in methodological debates has roots in a complex set of interacting influences both internal and external to the discipline. Internally, the 'crisis' in economics is reflected in the breakdown of the postwar Keynesian paradigm during the course of the 1970's (as mentioned earlier: p3) which led to a long drawn out period of theoretical reassessment bringing with it a protracted methodological appraisal of the discipline as its natural corollary. This was reflected in a number of authoritative, vigorous criticisms which came from within the discipline itself.

In 1971, the president of the Royal Economic Society, W. Leontief in his presidential address, voiced his dissatisfaction with the "continued preoccupation with the imaginary, hypothetical, rather than with observable reality" stating that in "no other field has so massive and sophisticated statistical machinery been used with such indifferent results." R. Frisch, another Nobel laureate was equally scathing in his attack on use of abstract techniques in economic analysis: "We should not mobilize an army of people to produce queer assumptions...and to deduce consequences from these assumptions...Such exercises maybe an entertaining intellectual game...But it might be a dangerous game both socially and scientifically" (qtd in Hutchison 1992: p 18). H.P. Brown (qtd in M. Blaug 1992: p237) complained that assumptions regarding human behaviour were literally "plucked from the air," blaming such empty theorising on lack of adequate training in history of mainstream economists. Similar sentiments were heard from various quarters,²⁹ with B. Ward coming up with his book *What's Wrong with Economics* (1972) in which he claimed that a 'revolution' had taken place which might be called "the formalist revolution" and which though more profound than the Keynesian revolution "was essentially methodological rather than substantive...there are just not any substantive changes of direction brought about." He concluded that,

The lesson from economics is that it is not always enough that, for

²⁹ Worswick (1972), Colander and Klamer (1990), Leontief(1971) amongst others.

example, practitioners are in substantial agreement as to the properties of acceptable puzzles and their solutions to insure that a science is seriously engaged in the attempt to understand the relevant phenomenon. (p255)

As pointed out by Hutchison (1992) most of the criticism was directed at the excessive and arbitrary abstraction from reality, emphasising the break from past norms of practical policy relevance and at the unsuitability of most university education in economics for work in government and business. Kaldor's methodological critique of equilibrium economics, beginning in his altercation with Samuelson Modigliani in 1966 and a series of articles which culminated in the Okun memorial lectures of 1983 and the Mattioli lectures of 1984 and crucial in our present analysis, was in line with this deep discontent with the general state of economics perse.

Externally, the 'crisis' in contemporary philosophy of science arising out of a sustained critique of logical positivism and its more muted variant logical empiricism which held sway during the 1950's led to the emergence of an expanding array of alternative paradigms involved in a discussion on the role, status and evolution of scientific knowledge in general and contributions of scientific theories in particular. Given the preoccupation of economics with its status as a scientific discipline, this was bound to have spillover effects in discussions on economic methodology.

Given the widely acknowledged role of Karl Popper's works on the importance of demarcation and 'falsificationism' in scientific enquiry in bringing about the demise of logical positivism, they were approached with some enthusiasm by economic methodologists and historians of economic thought. Principle amongst them is T.J. Hutchison (1938, 1992, 1994) who throughout has maintained his commitment to the Popperian programme and who was later joined by Blaug (1980), Klant (1984) and Boland (1985). Influenced by the 'growth in knowledge tradition' in philosophy of sciences, all major schools of economics were subjected to a stringent Popperian critique based on the criterion of falsifiability. However problems soon emerged in applying a prescriptionist Popperian programme with even its most committed advocates acknowledging the difficulties associated with adopting the 'falsificationist criterion' of theory appraisal in a social science like economics. As brought out by Caldwell (1991), within economics there seemed to be good reasons for rejecting the Popper's arguments

against 'immunizing stratagems.'³⁰ Popper himself advocated the method of 'situational logic' for social sciences where explanations of social behaviour were sought in the situation in which individuals found themselves. Given the objective situation there will be a unique response which follows from the logic of the situation which is then put forward as the 'rational' or 'logical' response given situational constraints. With the exception of Latsis (1972) and Hands (1985), this aspect of Popper's work has not received much attention within the literature on economic methodology.

However the role played by Kuhn's '*Structure Of Scientific Revolutions*' (1970), was perhaps the most fundamental one in bringing about the dissolution of positivist ideals of a rational, objective science moving in a linear progression towards the discovery of 'objective truth'. It brought in the importance of subjective beliefs, values and norms effectively critiquing foundationalist notions of historical evolutions of scientific disciplines and teleology. It also heralded the advent of a more pluralistic age in economics given the failure of Popperian interlude to provide workable solutions to economists grappling with the unique methodological problems facing their discipline. Lakatos's prescriptive cum descriptive methodological approach MSRP (1970), discussed earlier, which held promise as a compromise between the positivist notions of 'instant rationality' and the Kuhn's subjective quagmire of 'social psychology,' was initially taken up with some enthusiasm by economic methodologists. However the Lakatosian contribution to economic methodology over time has been subjected to intensive critical assessment³¹ within economics with economic methodologists pointing out the absence of progressive research programmes, in the Lakatosian sense, and the impossibility of reconstructing most important episode in history of economic thought on Lakatosian lines.

Coming back to contemporary times, with its more pluralistic environment, dealing simultaneously with questions of dominance and pluralism in methodology, theory and practice, a few concluding remarks regarding the survey undertaken in this chapter are called for.

³⁰ According to Popper, this term referred to various adjustments made to hypotheses in order to immunise them from falsification in instances where unambiguous testing was in any case not possible.

³¹ The most extensive re-evaluation of the applicability of the Lakatosian framework to economics till date was purportedly carried out in the Latsis foundation symposium at Capri in 1989, details of which are summarised in De Marchi and Blaug (1991).

Again there is the important question of progress over time which is related to the question of judging explanation in social sciences as opposed to natural sciences. For as has been brought out in our analysis, increases simply in the range and refinement of techniques do not necessarily lead to the progress in explanation of the actual workings of 'real economies' in historical time and space. Not only are their significant positive historical and institutional changes which alter the behavioural foundations of economic subjects, these changes also lead to changes in valuations regarding the weightage to be given to different economic problems, which derive mainly from shifts in policy perspectives. Accordingly previously adequate explanations might be rendered insufficient and even obsolete. This brings us back to the question of 'aims,' 'claims,' and reality.

However a point worth making here is that the progress in economics as a scientific discipline concerned with changes in historical time and space needs to be differentiated from progress as perceived by the 'social prestige' enjoyed by the subject in response to its ability to provide convincing policy answers to the main policy problems of the day. As noted by M. Blaug (1997),

The history of economics is not so much the chronicle of a continuous accumulation of theoretical achievements as the story of exaggerated intellectual revolutions in which truths already known are discarded in favour of new revelations. Indeed, sometimes it seems as if economics has been propelled forward by a sense of symmetry which demands that every new theory should always be the exact reverse of the old.

The point is well illustrated by the nature of the Keynesian boom where the intellectual euphoria in response to its initial success sometimes led to an overlooking of serious limitations of original theoretical claims leading to mounting anomalies and the ultimate crisis brought in by historical and institutional changes over time. It would seem that validity of large parts of Kuhnian analysis (especially with reference to periods of cataclysmic changes and revolution) regarding the nature of paradigmatic shifts in economics is confirmed by its history. However as far as incremental changes are concerned, the analysis seems to be less universal. To be sure there are some instances where new discoveries and anomalies lead to changes that affect only a small group of specialists working in that area (for example as in the case of the 'capital controversy'

between the two Cambridges) which . The Lakatosian analysis of coexistence of competing research programmes, where auxiliary changes and modifications in hypotheses in the protective belt within a single research tradition are made to substantiate the assumptions implicit in the hard core maybe more promising to analyse the gradual evolution of competing approaches in themselves (such as the growth of the mainstream paradigm and other more permanent heterodoxies over time).The continued domination of the mainstream paradigm might also become more explicable given an important point common in both Kuhnian and Lakatosian analysis: however serious a methodological and theoretical crisis in a discipline, the dominant paradigm or the degenerating research programme is never abandoned by any scientific community unless a viable alternative in the form of a new paradigm or a more progressive research programme is available. Of course as emphasised earlier, in a social science like economics with a historical dimension bringing about constant changes in the nature of reality, the problem arises as to determining which programme is more progressive and the Lakatosian ideal becomes difficult to implement. Besides, it remains very difficult to provide a formula for the necessary ingredients that constitute a new paradigm. Kuhn himself gave no unambiguous answers with his analysis being more descriptive than prescriptive. The contemporary era leads one to wide open chasms with its recognition of the pluralistic and relativist implications of all human knowledge.

Where does all this leave one on questions of theory appraisal and choice? Also is there complete incommensurability between competing paradigms to make any kind of comparison relevant? Our analysis indicates some commensurability and comparability might be possible when the focus of analysis is the same, atleast on the basis of the ontological and epistemological assumptions employed (say as in Keynesian and classical approaches to nature of unemployment or in the post Keynesian and neoclassical approaches to long run growth and distribution). However in some cases incommensurability does exist such as has been pointed out, in the case of the capital controversy between the two Cambridges where differences in methods and focus led to differences in the meaning given to terms such as 'capital' and 'production function' with the two sides at times talking across each other.

There are other important questions of contemporary relevance over here: if we accept

that there is no objective rational basis on which a progressive new paradigm emerges, then what constitutes the basis for emergence of a new paradigm? Is pluralism or domination called for to bring about progress? More specifically, does epistemic relativism arising out of theory ladenness of facts necessarily imply that there exist no criterion of theory appraisal? Is progress to be assessed according to the Kuhnian suggestion that evolution in the 'state of knowledge' rather than some sort of teleology may provide a better answer in analysing the progression of a discipline's thought? How acceptable is such a notion given the emphasis on prediction and descriptive adequacy in economics as a social science concerned with the actual workings of real economies evolving in historical time and space? What is the nature of this economic reality? What constitutes scientific theory or methodology under such circumstances? Can we overcome the problem of 'fallibility' of all knowledge to arrive at some understanding on the right way to proceed in carrying out economic analysis? Or is all methodological practice just based on heuristics? Is there any objective basis for empirical testing of theories? How crucial is historical evidence on observed economic phenomenon in determining the acceptability of different theories and hypotheses? It is of course impossible over here to provide any concrete responses to most of these questions. However I'll try and address some of these issues in the analysis to come, while exploring the answers to some of these questions in Kaldor's contributions to economic thought in the last, most interesting phase in his career.

Chapter Three

Abstractions, Realism and Theory Building on the basis of Stylized Facts: Reconstructing Kaldor's critique of Equilibrium Orthodoxy

The last chapter was an attempt to historically contextualise the subjective position and the evolution of Nicholas Kaldor as a heterodox economist within the discipline. It was part of the larger attempt towards a rational reconstruction of his mature works from the vantage point of critical pluralism. Such an approach entails both the reconstruction in a coherent manner of the particular research programme under consideration and a critical evaluation of its strengths and weaknesses by employing a range of criteria. It seeks to appraise the programme on its own terms. As pointed out earlier, the epistemic justification of critical pluralism as a criterion of theory appraisal is based on certain truisms in contemporary philosophy; namely that *all facts are theory laden*. One would argue further that *all methodologies are theory laden* and sometimes *theories are methodology laden*. This partly arises, as I've indicated earlier, due to the very intricate relationship and tension that exists between the specific and perceived goals of any given field of social sciences and the need to legitimise its practice on scientific grounds.

Under such circumstances, how is one to understand methodology and the role of methodological appraisals? Methodology in a narrow sense implies techniques of scientific work and their application (which tools to use for what purpose, within a particular methodological approach). However in its broader sense it could be seen as the framework within which particular methods are chosen and the manner in which methods of scientific enquiry are applied in theorising about real world economies. Methodological appraisals explore the methods by which economics arrives at its posited truths about the world and critically explores the alleged rationale for these methods. Issues concerning the sense in which theories are accepted, the nature of confirmation relation between evidence and hypothesis, the degree to which theoretic claims can be falsified by observational data and the like are typically the concerns of methodological appraisals. There are other issues to be explored also; such as the interrelationship between the worldview, goals, conceptual apparatus and values (whether explicit or implicit) of a particular research programme and its methodological content.

The Kaldorian research programme in this context is one of the rare ones which brings out many of these issues and in which a practising economist explicitly articulates his methodological position, albeit incoherently, as well as identifies the approach (the equilibrium orthodoxy) in opposition to which it stands, both methodologically as well as theoretically.

As brought out earlier, Kaldor has been widely recognized as one of the founders of the Post-Keynesian school in economics, a school marked by diversity of theoretical contributions along with certain underlying similarities which could be said to constitute its hard core in the Lakatosian sense. They include: a persistent opposition to the mainstream neoclassical orthodoxy³²; an emphasis on philosophical and methodological reasoning;³³ a continued emphasis on the role of fundamental uncertainty, expectations, historical processes, irreversibility of time, institutions and real human choice; a particular lineage of thought running from Keynes but also taking in Marx and some of the classicists such as Smith and Ricardo. Kaldor's analysis in particular had an *eclectic* base to it, influenced (as has been indicated in the last chapter) by a wide variety of approaches including a very predominant institutionalist influence (especially that of Allyn Young and G. Myrdal). This was in keeping with his overall methodological approach towards theorising as shall be argued over here.

Any rational reconstruction of a research programme attempts to express its underlying methodological position in as coherent a manner as possible. The need for it arises, as pointed out by Caldwell (1989: p.44), due to an inevitable lack of coherence in the original analysis which may occur due to several reasons. Methodological pronouncements of practising economists are usually roughly thought out (though Kaldor himself was far more explicit in this regard than many other economists) and often contain random citations of sometimes incompatible philosophical positions. These are influenced by the subjective position of the individual economist within the discipline and are often made with a specific opponent in mind (in this case explicitly identified by Kaldor himself). Hence the last chapter was an attempt to bring out these subjectivities

³² See Kaldor (1972, 1977, 1983, 1984, 1986) and Robinson (1974, 1977).

³³ See Dow (1998, 2002), Lawson (2003), Harcourt (2001).

through a historical contextualisation. In this chapter the task however is to understand, appreciate and critically assess the specificities of Kaldorian critique of mainstream theorising and his own alternative methodological stance of theorising on the basis of Stylized facts. In this process, a somewhat abstract discussion concerned with a specification of the ontological and epistemological grounds on which the Kaldorian research programme is based and the manner in which they inform its methodology, has also been incorporated.

It is important to emphasise over here that it is *not* the aim of the present study to critically analyse the postulates of *mainstream theory* as regards to its merits or shortcomings, but to bring out the specificities and finer points of Kaldor's analysis and to contextualise *his* critique of the equilibrium orthodoxy and his own contributions to economic thought. The following discussion of structure and strategy of neoclassical theorising is accordingly circumscribed by the necessity to provide the background for Kaldor's critique and his own alternative vision of the purpose and method of economic enquiry.

Structure and strategy of neoclassical equilibrium economics

As it is impossible here to provide here a comprehensive and non controversial statement of what mainstream economic theory consists of, only a very broad account will be attempted. Theoretical work in neoclassical economics takes the form of formulating models where several simplifying assumptions are made to explore the various properties of postulated entities and phenomena. To a large extent these conceptual explorations are carried out by using tools of mathematical and logical reasoning. Applications in terms of explanation and prediction are then made by specification of initial conditions which may include institutional, epistemic or physical conditions. The general approach to theorising is hence deductive and as pointed out by J.S. Mill several years ago, a kind of a hypotheco-deductive approach is employed. According to him, the basic laws of economics are either introspectively established psychological claims such as "agents attempt to maximise utility" or empirically confirmed technical claims such as the law of diminishing returns. Using these basic laws, testable predictions regarding various hypotheses pertaining to economic phenomenon

are made in light of which the formulated hypotheses are tested. Most of the usual positivist criticism against neoclassical economics has been directed at the failure to successfully implement the last stage, i.e. at the failure to empirically test, verify or falsify the implications of the various hypotheses due to the use of vague *ceteris paribus* clauses and simplifying assumptions and employment of econometric and statistical techniques of questionable validity³⁴. However, as has been often pointed out in literature, not only have positivist criteria of theory appraisal come under severe attack in general, conclusive confirmation or even falsification becomes especially problematic in case of economics which deals with the actual functioning of complex, evolving real economies where there is an absence of laboratory controlled conditions for empirical testing and verification. The question that then arises is how to establish the credibility of economic theory on scientific grounds in absence of some given objective positivist criterion?

The earliest justification and an overall vision of the purpose of economic enquiry was laid down by Mill and has recently been argued for by Hausman (1992) as the most appropriate and coherent way to look at the structure and strategies of neoclassical economic theorising. As discussed in the preceding chapter, according to Mill the basic laws of economics were not in nature of universal laws but were statements of tendencies, subject to counteracting influences which could not be specified in advance thereby necessitating the use of vague *ceteris paribus* clauses. Now this vision combined with a methodological commitment to equilibrium theory then gave the earliest and to a large extent still persisting and coherent account of neoclassical theorising. Economic phenomena then become the consequences of rational choices predominantly governed by some variant of consumerism and profit maximisation. The explanatory task of neoclassical theory was accordingly done once economic phenomena had been traced to the fundamental causal laws of economics which are assumed to predominate in the economic domain. To the extent that causal factors not considered in economic theory were left out of analysis, economic theory was inexact and complete only at a high level of abstraction or approximation. It is important to note that the deductive justification is ultimately inductive: the evidence (basic causal factors) that supports (inductively) the

³⁴ See amongst others B. Ward (1972), T.J. Hutchison (1938, 1992), M. Blaug (1980, 1992) for a detailed account of such criticisms.

premises of a deductive argument is the (inductive) basis in the arguments conclusions. Rational choice of optimising agents, the notion of equilibrium and the universal scope of economics become central causal features of all neoclassical economic analysis. Equilibrium here was a fundamental organising concept defined as a state in which the intended actions of rational optimising agents were mutually consistent and hence optimal, leading to a harmonious settlement of economic processes. Accordingly growth is analysed in terms of 'comparative statics' i.e. in terms of comparison of different equilibrium positions given different specifications of initial conditions. Comparative static predictions and explanations are causally explained in terms of changes in initial conditions. Hence many of the derived generalisations, such as law of diminishing returns are in the form of *actually obtaining event regularities* of a causal sequence sort.

Now problem arises in evaluation of such theorising and this is where most of neoclassical theorising runs into problems. Under such circumstances how do we assess the worth of such theorising? On what grounds can the reliability, exclusivity and law likeness of fundamental laws of equilibrium theory be established? Are they really well established? What is the credibility of abstractions employed to deduce conclusions from basic propositions? To what extent are these conclusions justified? For given the existence of numerous interferences, there is little basis for increased confidence in an hypothesis when things are as predicted, and little basis for rejecting hypothetical claims when not confirmed by evidence.

Econometric modelling usually cited as the more empirical and applied dimension of economic theory, also happens to be one of the most controversial and arguably flawed aspects of neoclassical theorising. A possible justification behind the employment of econometric techniques could be provided by an instrumentalist philosophy. Instrumentalism holds that scientific theories are merely instruments of prediction and it is predictive efficiency than descriptive adequacy which constitutes the test of validity of any theory. In general instrumentalists hold that truth or falsity of theories does not matter as long as they can be shown to be consistent with the given set of data in question.³⁵ Accordingly econometric testing of theories in the NCM tradition is carried

³⁵ In this case it is important to note our earlier discussion on the peculiar form of instrumentalism which underlay Friedman's widely discussed positivist views on economic theorising where all that was required of economics are theories which lead to accurate predictions and falsity of assumptions or postulated entities did not matter.

out to statistically test formal economic models. The construction of such econometric models is characterised by the presence of a priors which are reflected in the way the model is constructed. Accordingly not only do models postulate event regularities but the results are largely predetermined by underlying theoretical constructs, the manner in which the model is specified, in the identification of variables of interest and selection of data. The distinction between *application and verification and testing* is blurred in the process. For instance, the applied dimension of neoclassical growth theory of the 1950's and 1960's , initially led to a move from vague policy laden notions of applied economics to a more quantitative and empirical approach towards the application of growth theories. The early Solow Swan models gave rise to a theoretical framework in which the stability of growth equilibrium (in one sector models) was guaranteed through a simple adjustment of factor substitution, which made possible the use of statistical techniques. It was in this context that the concept of applying theories became very closely associated first with measurement and then with using econometrics, in order to report (*estimating, not testing*) empirical regularities and provide predictions for policy making.

What's important here is that not only did the neoclassical model influence the theorist's ideas on growth, but also their conception of applied economics. Broadly speaking, application here entailed the quantification of certain concepts and variables *previously identified and contextualised by theories*. The operational definitions are very dependent on theoretical guidelines, and the task of applied economics consists of gathering and analysing predetermined empirical evidence (Comim 2000: p.156-157).

From a methodological point of view, the practices and activities involved in this conception of applied economics privilege a mathematical style of argument postulating the presence of event regularities. Ironically enough, even these prespecified 'growth accounting' exercises found that the bulk of the increase in long run national output is due to reasons other than increase in quantity of factor supplies which were deemed to be largely exogenous to the system.

The above discussion provides the background necessary to bring out the radical nature of Kaldor's methodological critique, his methodological stance and his alternative theoretical formulations. It is important to emphasise that Kaldor's critique of neoclassical equilibrium orthodoxy, though not novel and arguably even largely derivative, had its own distinct features. For as will be brought out, his critique was not

merely empiricist aimed at failure of neoclassical theorising in meeting positivist criterion of testability one but was directed at the basic structure and strategy of neoclassical theorising *per se* and the manner in which it precluded real scientific progress by being of little use in explaining observed economic phenomenon and the *actual processes of growth in historical time and space*. It was also aimed at the purpose and the primary vision of such theorising which led to the employment of a mechanical and logical notion of equilibrium to analyse allocation of economic resources with the purpose of demonstrating the optimality of market mechanism. It needs to be seen in the background of his theoretical commitment to the prevalence of increasing returns and a process of cumulative causation as fundamental to any explanation of observed historical growth patterns. As far as theory assessment was concerned, it rejected the instrumentalist criterion of judging a theory on the basis of its merely being shown to be consistent with any give set of data through application of econometric techniques.

Critique of Equilibrium Orthodoxy

Kaldor's fundamental critique of equilibrium orthodoxy lay in its failure to explain 'the critical aspects of how things work' in modern capitalist economies functioning in historical time and space. It is a layered critique where the theoretical and methodological arguments often complement each other in locating the reasons behind the inability of mainstream theorising in achieving this goal. His assault on equilibrium theory gathered momentum during the 1970's³⁶ in provocative essays with suggestive titles, '*The Irrelevance of Equilibrium Theory*'(1972) and '*What is Wrong with Economic Theory*'(1975) and culminated in the Okun Memorial Lectures '*Economics without Equilibrium*'(1985) and the Mattioli lectures '*Causes of Growth and Stagnation in the World Economy*' (1984) which encapsulated his major criticisms of equilibrium economics while at the same time outlining a vivid and lucid picture of how market economies function in reality. The notion of equilibrium he had in mind was 'the general economic equilibrium originally formulated by Walras, and developed with ever

³⁶ This was in line with numerous other criticisms of neoclassical orthodoxy which, as outlined in the last chapter, came around at the same time from various quarters.

increasing elegance, exactness and logical precision by the mathematical economists of our own generation' (Kaldor 1972).

However the basic intuition behind his critique could be seen in terms of an opposition to the *ahistorical* character of the neoclassical approach to theorising where universal applicability of its basic laws was postulated. Fundamental to his critique was the treatment of *time in the notion of equilibrium* as conceived by the neoclassical orthodoxy which failed to consider some of the most crucial aspects of modern capitalist growth where *basic uncertainty, continuous growth in knowledge and the presence of increasing returns* made the process of accumulation a non deterministic one. For him these deficiencies arose partly out of elementary methodological fallacies (especially in case of general equilibrium theory which was the main target of his methodological critique) which were related to and which reinforced the neoclassical approach towards economic theorising. This approach, he would argue, completely overlooked the fact that actual functioning of economies had far more in common in analogies drawn from biology than mechanics.

As he outlines in his theoretical critique in the Mattioli lectures, economic theory since its conception has been preoccupied (whether explicitly or implicitly) with two kinds of basic enquiries. One deals with the problem of allocation in a de-centralised unplanned market economy while the other deals with exploring the determinants of and critical factors which lead to long run economic growth and distribution. On both counts, his criticism was directed on the failure of equilibrium theory to illuminate crucial aspects of the working of actual economies with regards to what they set out to do. In the first instance the dichotomy itself was misconceived for the problem of optimal allocation of resources loses its relevance in economies characterised by increasing returns where the position of the production possibility curve itself depends on allocation. Resource constraints lose their bite under such conditions, production generates its own resources and the path to equilibrium itself becomes indeterminate. The existence of increasing returns, endogenous technical change and factor creation rendered the analysis of the allocative functions of the market secondary to that of the creative functions of the

market and of complementarities based on demand for products, activities and factors in general.

According to him the failure to incorporate these facts arose chiefly because of the *a-priori logico-deductive* modelling employed by the mainstream theorists which had initially been designed to obtain the assumptions required for proving the existence, uniqueness and stability of a general equilibrium. He interprets the model to have been initially set at a high level of abstraction as a first approximation with the original intention of the authors being to remove this scaffolding, as it were, through successive approximations directed towards the construction of a *descriptively more adequate* and permanent structure. However, as he notes,

...since Walras first laid down his system of equations over a hundred years ago, progress has definitely been backwards not forwards in the sense that the present set of axioms are far more restrictive than those of the original Walrasian model. The ship is no nearer to the shore but considerably farther off, though in a logical mathematical sense the present system of derived tautologies is enormously superior to Walras's original effort. (1985: p.13)

Apart from his technical critique of neoclassical theory - strong on proving the existence, stability and uniqueness of general equilibrium and weak on an explanation of 'dynamics' of movements from one equilibrium to another- his more basic critique was related to the employment of more and more restrictive assumptions by mainstream theorists in order to develop with a high degree of sophistication the logical and mathematical properties (uniqueness, existence, stability) of economic equilibrium. Here *explanation* was intended in a purely logical and not necessarily in a scientific sense where scientific enquiry is deemed to be primarily concerned with an understanding of observed phenomenon rather than merely the construction of a grand logically consistent theoretical edifice ([1986] 1989a: p. 14-17). He recognises that a basic reason (besides the widely held conception that use of sophisticated mathematical modelling *per se* is scientific) because of which general equilibrium theorising holds such fascination for economists lay is in its core intuition that the price mechanism guides human action in a competitive market mechanism.

...the intuitive belief...the Walrasian model and its most up to date successor may both be highly artificial abstractions from the real world

but the truth that the theory conveys- that prices provide the guide to all economic action- must be fundamentally true, and its main implications that free markets secure the best results must also be true.... (1985: p.13)

He laments the fact that this fascination has led to the adoption of increasingly abstract theoretical constructs which impede the employment of a more realistic approach towards economic theorising.

The fascination exerted by the Neo –Walrasian system on academic community created the opposite kind of movement: the economic theorists view of reality became increasingly distorted, so as to become closer to the theoretical image rather than the other way round...So neoclassical theorists increasingly claim to believe that markets *are* continuously market clearing, and everyone behaves as if one has the right answer to every question, except for stochastic misperceptions. (1985: p.60)

Accordingly he might be construed as suggesting that the search for conditions under which the price mechanism ensures optimal coordination amongst economic agents in an unplanned undirected market economy was facilitated by the adoption of a methodology where event regularities are actually obtained. The postulating of optimising atomistic individuals functioning under isolationist *ceteris paribus* clauses which facilitate mathematical deductivist modelling becomes a compelling way to achieve in a clear cut manner the desired objectives *that market mechanism ensures optimal allocation*. Here the focus shifts to the ability of the methods employed and theoretical constructs used to achieve desired results (the optimality of the market mechanism), apart from the usual questions regarding the scientificity of the methods adopted and methodology becomes theory laden.

However the fundamental problem according to him was not with the use of abstraction *per se* but with the “wrong kind of abstraction”. The critique was methodological in that it considered such abstraction, including the employment of unrealistic assumptions, as synonymous with a method of enquiry which was unscientific. His conception of economics as an empirical science is defined as “a body of fundamental theorems based on assumptions that are empirically derived (from observations) and which embody hypotheses that are capable of verification both in regard to the assumptions and the predictions” (Kaldor 1972: p.1237) . Hence he considered the neoclassical approach to theorising to be obstructing the path to real scientific progress with progress being

assessed in terms of ability of methods employed to secure a better understanding of observed socio-economic reality. As he notes again and again,

My basic objection to the theory of General Equilibrium is not that its abstract – all theory is abstract and must necessarily be so since there can be no analysis without abstraction –but that it starts with the wrong kind of abstraction, and therefore gives a misleading ‘paradigm’...of the world as it is: it gives a misleading impression of the nature and manner of operation of economic forces. (Kaldor 1975, 1978: p.202)

Hence the problem with which Kaldor is concerned is not a simplistic one which recognises that assumptions of models and theories must necessarily be based on abstractions but a more difficult one of being careful to choose a type of abstraction that is appropriate to the characteristic features of the economic process as recorded by experience (Kaldor 1963). He is extremely critical of the fact that unlike any *scientific* theory, where basic assumptions are chosen on the basis of their ability to explain observed phenomenon, the behaviour of which forms the subject matter of the theory, most of these assumptions were chosen for the purpose of ensuring mathematical tractability. The requirement for tractable mathematically determinate solutions necessitated assumptions such as perfect competition, perfect knowledge, homogeneous linear production functions, constant returns to scale, perfect divisibility of factors etc.

They were either ‘demonstrably untrue’ (such as existence of perfect information, absence of material and monetary inventories etc) or unfalsifiable (such as agents are guided solely by the criterion of optimisation). Then there are others axioms which “contain assertions about the real world that can be refuted and without which the main conclusions of the theory would not hold, as for example, those relating to the laws of production.” (1985: p.11).Kaldor was particularly critical of these assumptions for precluding any possibility of understanding of observed growth processes characterised by oligopolistic and monopolistic market structures, uncertainty, expectations, indivisibilities in production and consumption, institutional constraints and increasing returns.

Though he himself, as pointed out by Lawson (1989: p.68), is not very explicit about what is the ‘appropriate’ kind of abstraction that can be legitimately employed, it can be construed from his various writings on the subject that what he found singularly

problematic about the equilibrium orthodoxy was that any attempt at introducing additional assumptions representative of the real world – historical time, increasing returns, imperfect competition, unforeseen technical progress, the importance of quantity signals in coordinating individual action - led to the collapse of the entire structure of the neoclassical edifice. The *methodological* critique hence was directed at the mode of and purposes for which mathematical deductive reasoning was employed which led to the

Setting up a logically watertight system, with its precise number of necessary axioms that formed “a mathematical crystal”... “some rigid thing, which maybe correct or incorrect but without an intermediate use.” (1985: p.60)

The point to note is that it is the *structure of explanation* which is being considered now. As Kaldor points out, contrary results are explained away by simply noting that the assumptions of the model did not account for changes in such things as knowledge or just assumed away uncertainty and technical progress. For Kaldor, such method of explaining away discrepancies between results of theoretical models and the observed facts of the world has *little interpretative value*. This could also be seen in context of his criticism of the developments with a high degree of sophistication the logical properties of general equilibrium which “made the theory a *less usable tool* (italics mine) than it was thought to have been in its early and crude state before the full implications of general equilibrium had been so thoroughly explored” (Kaldor [1974] 1989b: p.399). He advocated an alternative method which proceeds by identifying empirical regularities in historically observed phenomena as ‘stylized facts’.

Stylized Facts and Theory Building

There is a frequent reference to notion of *Stylized facts* and tendencies in Kaldor’s numerous methodological asides and assertions with the following statements being typical of his methodological remarks,

Any theory must necessarily be based on abstractions; but the type of abstraction chosen cannot be decided in a vacuum: it must be appropriate to the characteristic features of the economic process as recorded by experience. Hence the theorist, in choosing a particular theoretical approach, ought to start off with a summary of the most which he regards as relevant to his problem. Since facts, as recorded by statisticians are

always subject to numerous snags and qualifications, and for that reason are incapable of being accurately summarised, the theorist, in my view should be free to start off with a Stylized view of the facts – i.e. concentrate on the broad tendencies, ignoring individual detail, and proceed on the ‘as if’ method, i.e. construct a hypothesis that could account for these ‘Stylized facts’ without necessarily committing himself to the historical accuracy, or sufficiency, of the facts or tendencies thus summarised. (Kaldor 1961, 1978: p.2)

These and numerous other such methodological asides provide suggestions of an alternative methodological and philosophical position to complement his methodological critique of equilibrium economics. Now the emphasis on *Stylized fact* as a starting point of analysis (as opposed to perception, intuition or direct experience alone) is fundamental to the entire range of Kaldor’s theoretical analysis as well as his major theoretical and methodological critiques of various contributions to economic theorising. It, along with his emphasis on induction, is the basis for popular perceptions of his works and methodological assertions as ‘empiricist³⁷’, ‘positivist,’ or ‘verificationist.’ For example, as Kaldor states at times,

In other words contrary to the prevailing trend, one should subordinate induction to deduction, and discover the empirical regularities first, whether through a study of statistics or through special inquiries.... (Kaldor 1985: p.8)

The point however is that the emphasis on induction perhaps needs to be seen in context of his criticism of ‘*prevailing trends*,’ i.e. in terms of underlying the failure of the equilibrium orthodoxy to “embody hypotheses that are capable of verification both in regard to the assumptions and the predictions (ibid).” It is the insistence on the fact that basic assumptions be consistent with the observed behaviour of phenomenon under consideration which led him to emphasise the realism of assumptions. Theorising on the basis of stylized facts however, in itself, involves abstractions both at the level of identification of empirical regularities as well as in constructing hypotheses on the basis of the ‘as if’ method. However, his use of an ‘as if’ method is different from the neoclassical use of an ‘as if’ method. Unlike neoclassical economists who employ simplistic assumptions ‘as if’ they are true, Kaldor would have us explain ‘Stylized’ facts

³⁷ Sir Douglas Wass for instance compared his approach to Baconian Empiricism (Thirlwall 1987.)

as if they truly represented the reality we want to explain. Further, he wished to focus attention on the difficult problem of choosing the appropriate abstraction for the economic world we live in while avoiding unproductive debate over historical accuracy and hence his conceptualisation of Stylized facts as broad empirical regularities “ignoring individual detail (ibid).”

Another important point to note over here is that unlike the neoclassical approach, there is a distinction between the question of verification and testing of theories and their policy application. The role of statistical analysis is limited to an input into the general framework of the application of a theory, where the main objective is to provide concrete guidelines for historically determined situations. For example as Comim (2000: p.158) suggests in his analysis of applied aspects of Cambridge growth theories, Stylized facts then could be seen as providing new referential units for an empirical assessment of theories that was meant to avoid the quantitative bias present in neoclassical applied economics. In his papers dealing with more applied issues such as the one on the case for regional policies (1970) he begins by establishing the basic Stylized facts from empirical evidence, develops the theoretical background that explains those facts, and concludes with suggestions on policy implications. Kaldor argues that the principle of cumulative causation privileges a theoretical mechanism (endogenous factors resulting from the process of historical development) that explains Stylized facts better than the neoclassical argument based on exogenous differences in resource allocation. It might be then suggested that Kaldor’s applied economics was shaped in a form that allowed him to feel more comfortable with the use of economics in real and historical systems where the quantification strategy itself did not lay down the uses of economic models he was aiming for³⁸ (Comim 2000: p.159). As he states,

... But the particular issues of economic policy, and the manner in which economists tackle them, are even more ephemeral than the theoretical ideas that form their background. Moreover their consideration often involves a detailed analysis of the economic situation of some particular country at some particular time. (Kaldor 1964: 1 p.vii)

Application for Kaldor then was closely related to the historical and institutional uses of

³⁸ This is to be seen with reference to our earlier discussion on the neoclassical quantification strategy (p....).

growth models and to a style of argument – the opposite of Solow Swan model- which allowed the discussion of particular and ephemeral issues. Accordingly he was scathing in his criticism of neoclassical economist to

take off his hat as a theorist and put on his hat as a policy advisor or as an interpreter of current events...When it come to judging the effects of policy measures-whether it relates to unemployment, foreign trade, the incidence of taxation, exchange rates etc – he applies conclusions derived from the theory of general equilibrium to the real world without any hesitation: that is to say without investigating how far his results are dependent on implied or explicit assumptions that are manifestly contrary to experience. ([1986] 1989a: p.16)

A related criticism was directed at the nature of testing and empirical research being carried out which would hardly improve the economists knowledge of functioning of modern market economies. Acquiring such knowledge required “new methods of research that would make greater use of knowledge gained through personal contact and on-the-job investigations, and less on testing of formal models through statistics and econometrics. There is an enormous amount of empirical research going on but it is stifled by operating *within* the framework of established theory” (Kaldor 1985: p.54)

Ontology, epistemology and methodology

Since there is a continuous reference towards the need for explicating economic phenomena as observed in reality, it might be useful to explore the underlying conceptions of the socio-economic reality which characterise Kaldor’s numerous methodological and theoretical assertions. Hence a brief diversion is now undertaken to explore how the ontological, epistemological and methodological basis of both his critique of equilibrium orthodoxy as well as his own alternative analysis could indicate the manner in which a more coherent and explicit reconstruction of his methodological position can be achieved. Here the concept of open and closed systems becomes important. A closed system is one where the boundaries are predetermined and so are the full range of constituent variables and the structure of their interrelationships. This does not exclude possibilities of stochastic variation. An open system on the other hand is one whose boundaries, the nature and range of its constituent variables and the structure of their interrelationships, are not predetermined. This is not a matter of stochastic variation.

Closed systems are the province of classical logic and lend themselves to a Cartesian Euclidean mode of thought³⁹ built on axioms which are self evident or true by definition in abstract-deductive, closed systems. Open systems on the other hand lend themselves to ordinary logic and can be thought to be consistent with a mode of thought in the Babylonian tradition based on different approaches using several strands of arguments which reinforce each other using a variety of methods.⁴⁰ An understanding of reality as an open system allows for notions of human creativity, agency and freedom of choice. A closed system understanding of reality may lead one to the notion of a grand, mechanically functioning, predetermined structure.

Now an important point to note over here, to which I shall come back later in analysing Kaldorian critique of equilibrium orthodoxy, is that an open system ontology does not necessarily entail an open system epistemology. It can be argued that even if one understands reality as an open system, knowledge of it can only be acquired by proceeding as if reality were a closed system.

As has been pointed out by Lawson (2003) and Dow (1997) amongst others, that an open system *organicist*⁴¹ ontology sets the ground (though implicitly⁴²) for Post-Keynesian analysis. Such an open system ontology differs from radically relativist positions in that it believes in the existence of regularities in nature which any science, including economics, should aim to identify but posits them to be regularities of *processes* rather than of *events* which cannot be isolated from evolutionary or other irregularities. The economy like knowledge is best understood as an open system. Mainstream theorising on the other hand has traditionally seen its scope as being defined by universal regularities which can be dualistically separated from irregularities and are best understood within a closed theoretical system⁴³.

³⁹ A mode of thought is broader concept than methodology. It pertains to the manner in which arguments are constructed, presented and proved (tested).

⁴⁰ This has been argued for, as a characteristic feature of several different strands of theorising (such as pragmatism, structuralism, Marxian dialectics, non-axiomatic mathematical reasoning and Babylonian Talmud as well as Keynesian and Post-Keynesian schools) by Dow (1996: p.13)

⁴¹ The term organic is used to emphasise the mutually reinforcing relationship that characterises the operation of human agency in structured social systems.

⁴² Again what is important over here is not whether Post-Keynesianism explicitly proclaims such an ontology but the fact that such an ontology necessarily characterises any coherent understanding of the theoretical premises of the analysis.

⁴³ That Kaldor himself was aware of the basic problem with drawing significant implications from

A closed system epistemology is implicit in the use of formalistic methods employed in mainstream theorising. Formal systems are necessarily closed, since it is necessary to give variables fixed meanings, and to specify structural relationships and the exogenous variables. General equilibrium theorising is a good illustration of a closed system theoretical structure. The boundaries of the system are well defined clearly differentiating variables as endogenous or exogenous. As pointed out by Dow

The aim is to reach an agreement on the best representation of the structural relationships between variables, for universal application. This entails conformity of representation through formalism. The appraisal criterion of conformity to the principles of classical logic reflects a closed system epistemology; where the additional criterion is applied of goodness of fit in econometric testing, closed system ontology is evident. ([1997] 2002: p.137)

On the other hand various heterodox traditions (such as Post-Keynesianism or institutionalist-evolutionary approaches) adhering to an organicist ontology *implicitly* presuppose an open system epistemology which allows for a range of understandings and theorising. Given that any human understanding of an open organic reality is bound to be limited (since knowledge is based on imperfect knowledge), it is inevitable that there will be a range of understandings of reality among agents. However, again, this epistemology differs from absolute pluralism in that it believes that there are regularities in the knowledge generation process of agents which limit the range of rational beliefs. The choice of belief then becomes a matter of rational debate (Dow [1997] 2002: p. 136-140).

In the last phase of Kaldorian analysis, the alternative approach to theorising outlined by him which finds its culmination in Okun (1985) and Mattioli memorial lectures (1984), looks “ upon the economy as a continually evolving system whose path cannot be predicted any more than the evolution of an ecological system in biology”(Kaldor 1985: p 12). As would be brought out, this worldview is consistent with his analysis which brings out reality as an open, intrinsically dynamic and human agency dependent system which is organic by virtue of being highly internally related and structured (it does not

abstract deductivism to an open reality is also evident in his argument that “ most abstract economic models postulate a ‘closed system’ but they *apply the conclusion reached to open systems*, without being fully aware of the inconsistencies involved in this procedure” ([1977] 1989: p.426-427; italics mine)

reduce to human practice and actualities but includes underlying structures and processes with their own powers and tendencies). These implicit ontological and epistemological presuppositions regarding the nature and existence of economic reality and its knowledge in Kaldorian analysis, common to the Post-Keynesian school and analysed extensively in methodological literature,⁴⁴ were crucial not only in determining his own methodological stance but also in laying grounds for his methodological and conceptual critique of the neoclassical orthodoxy in all its formulations.

Though not many attempts have been made towards a reconstruction of these premises in Kaldor's works within economic literature, Tony Lawson's (1989) compelling philosophical interpretation of Kaldor's methodological insights within the framework of critical realism stands out as a stimulating and challenging contribution. It is important to remember over here however that the question involved is not a historical one, i.e. whether Kaldor was a critical realist. Rather it is whether critical realism provides an appropriate basis for understanding Kaldorian methodological stance and his critique of equilibrium orthodoxy. Accordingly an analysis of Lawson's critical realist reading of Kaldor is carried out. This is followed by a contextualisation of Kaldor's critique of equilibrium orthodoxy and his own methodological stance in terms of the underlying conceptions regarding the nature of economic reality and the manner in which the theorisation of such reality should be attempted.

Critical Realism

Contemporary philosophy of science has recently witnessed a revival of scientific realism, a philosophy of science which contains a specific thesis regarding the aims of science and scientific theorising. Though there exist several strands of realism with their own subtle and perhaps crucial variations on central themes, I shall merely discuss some basic common features relevant to our discussion here.

Scientific realism is a doctrine which states in opposition to radical relativism, that there exists a material and social world which exists independently of our knowledge of it. Further true theories of real existing entities can be obtained and in contrast to the

⁴⁴ See Lawson (2003), Dow (1997), Carbelli (1988) amongst others.

instrumentalist views regarding the goals of science, the aim of scientific theorising is to discover such truths about the subject matter of real world as well as serve practical interests by enabling human beings to anticipate and control relevant phenomenon. Now Lawson bases his interpretation on a specific form of realism (critical realism) advocated by him as relevant for economic theorising which is inspired by Bhaskar's 'transcendental realism' (Bhaskar 1978). Bhaskar's version of realism makes some interesting observations which also relate to our earlier discussion on event regularities. For Lawson, Bhaskar's contribution lies in bringing out the importance of two commonly observed features of experimentation in science. The first feature is that most of the constant conjunctions of events that constitute important results in science occur *only* in laboratory controlled closed systems. These *closed systems* usually do not occur outside experimental situations. Secondly, the laws supported by experimental activity are however frequently successfully applied outside experimental situations. If laws are to be understood as necessitating event regularities of the causal sequence sort, then the problem arises that such laws do not necessarily hold outside experimental situations. Specifying that they hold only under certain conditions obtained under experimentally controlled conditions leads to the problem of explaining what governs events outside such situations.

Besides there is the related problem that occurrence of event regularities (denoted by such laws) depends on human agency. Yet, one cannot get away from the fact that experimentally established conditions are often successfully employed outside experimental situations. These apparent contradictions can be avoided, according to Bhaskar, by invoking ontology of generative structures and causal mechanisms which lie behind and govern the flux of observable phenomenon. Laws then become statements of *tendencies* regarding the workings of generative structures. They generally do not lead to observable event regularities due to the operation of countervailing tendencies (Lawson 1989: p.61-65).

Now, as Lawson points out, this view is perfectly consistent with J. S. Mill's methodological contention, outlined earlier, that basic laws of economics were not in nature of universal laws but were statements of tendencies, subject to counteracting influences. As he notes,

... [A person's] error generally consists ... in making the wrong kind of assertion: he predicated an actual result, when he should only have predicated a tendency to that result – power acting with a certain tendency in that direction. (qtd in Lawson 1989: p.63)

In economics, the question arises as to whether any causal powers and tendencies exist independent of human agency in social reality. One view is that because everything that happens in social world is a result of human intervention, all causal forces are essentially reducible to human agency. However according to Lawson, though it is true that social structures exist by virtue of human activity, the latter presupposes the aprior existence of social structures. As he states,

The point is, ofcourse that ; although dependent on human activity in general, social structures, relations, practices , conventions etc. exist prior to any individual act (at a given point in time and space) and govern it (that is, make a difference to it) by providing limiting and enabling conditions (that are necessary for action to take place). In this sense social structures have causal powers... Ofcourse the contribution of individual agency to the reproduction and transformation of these structures will often (perhaps usually) be unintended, while the bearing that these structures have on individual agency will perhaps be only tacitly and incompletely understood. (Lawson 1989: p.65)

Ofcourse such underlying mechanisms are likely to be less enduring in the social realm by virtue of their dependence on human agency. Given such a critical realist perspective, the aim of scientific enquiry in economics becomes to identify the powers, mechanisms, structures and tendencies which generate observable phenomenon. In this manner the union of economic theory and explanation is inevitably linked to the notion of underlying causes.

However the problems arises in bringing out an alternative and viable methodological approach which is more suitable for understanding an open, organic, dynamic and structured social reality. It is here that Kaldor's own methodological contribution involving the notion of Stylized facts becomes important in terms of outlining *one* such approach. Lawson's critical realist reading of Kaldor's methodological approach based on Stylized facts then provides an interesting perspective. To begin with he identifies an alternative approach in Kaldor's assertions, consistent with a critical realist worldview, which he breaks into three steps: identifying empirical regularities, forming causal

hypotheses that can account for them, and then subjecting entities postulated at the second stage to further/continuous scrutiny. For an instrumentalist, the first two steps are adequate as a theory is acceptable merely if the phenomenon in question is 'as if' it had been generated by the hypotheses constructed in the theory. However the search for theories which identify and understand real structures and mechanisms that govern real observed phenomenon leads the realist to subject the both the empirically identified phenomenon and the reality of hypotheses constructed and causal mechanisms identified to further examination and scrutiny. The third step then becomes essential and it primarily involves discriminating among the competing causal hypotheses that are consistent with the identified regularities. Here reasoning termed as 'abduction' or 'retroduction' is employed in selection where criteria such as ability of the constructed hypotheses to illuminate a range of empirical findings are used. Moreover the 'reality' of postulated entities is itself subjected to further scrutiny in terms of the structures, causal mechanisms etc underlying it thereby giving rise to layers of reasoning. This mode of inference is central to realist analysis for it incorporates the last two steps suggested by Lawson in his alternative approach.

Now coming to Kaldor's specific methodological assertions, Lawson suggests that the notion of *Stylized facts* could be interpreted as the empirical counterparts of tendencies, the latter being conceptualised as the non empirical powers or features of causal structures, which because of the existence of numerous countervailing mechanisms, maybe operative at the same moment and place, but maybe exercised without being manifest in actual economic outcomes. Even if the effects of the tendencies of an economic agent, entity or structure will frequently be modified or hidden by the effects of countervailing mechanisms and hence uniform regularities be rarely observed, all might not be lost. Despite the absence of laboratory controlled situations in economics, some degree of uniformity, persistency or generality may and does "shine through": total chaos need not reign at the level of observed phenomenon. The presence of some degree of uniformity or persistence provides a prima facie case for postulating some enduring generative economic mechanisms (ibid: p.67-68). Conceptualisations of these partial regularities which in Kaldor's words ignore 'individual detail' are then put forward as an interpretation of Stylized facts.

To complete Lawson's (1989) realist reading of Kaldor's works, it is suggested that the best way to move from the conceptualisation of some manifest phenomenon as Stylized fact to possible mechanisms which give rise to it, "is one neither of induction nor deduction", and is captured by Kaldor in his reference to "as if method" involving the construction of 'a hypothesis that account for these "Stylized facts" ...' (ibid: p.68).

According to Lawson, this perfectly fits in with the method of abduction or retroduction, a kind of inference to the best explanation as explained above. As he points out, though the notion of abstraction is important in Kaldor's works, he gives no explicit formulations of what are the appropriate kind of abstractions that could be employed. Lawson's suggests two guiding principles which are consistent with Kaldor's critique of equilibrium orthodoxy as well as his alternative theoretical formulations. In the first place, an abstract conception must be concerned with the real rather than some ideal convenient fiction. A realist aims at discovering real causal mechanisms and hence should avoid such formulations. This principle rules out idealized notions such as universal perfect competition, rational expectations and perfect foresight which are conceded to be unrealistic by the mainstream theorists themselves. The second principle states that the correct kind of abstraction "must be concerned with the essential rather than merely the most general". The point of the contrast is to focus attention on the prevalent practice among a substantial body of orthodox economists of formulating economic axioms as relatively contentless generalisations (such as agents have preferences) which consequently have little explanatory power, in that they fail to explore the essential natures of the economic mechanisms and structures which govern the observable Stylized facts (ibid: p.68-73). Hence it's actually the powerful and often unrealistic assumptions designed to achieve mathematical tractability which when combined with basic axioms do the real work. This has resonances with Popper's advocacy that science should proceed through subjecting conjectures with a high empirical content to the criterion of falsifiability. Kaldor's critique of the use of unrealistic and tautologous assumptions by the equilibrium orthodoxy, the emphasis on analysing observed features (such as endogeneity of change, imperfect competition, increasing returns etc) in terms of fundamental underlying phenomenon such as circular and cumulative causation, can all be put in perspective over here.

Critical Realism, Post-Keynesian ontology and Stylized Facts

How does one read Lawson's critical realist reading of Kaldor? I propose to analyse it at two levels. Firstly, in terms of the ontological and epistemological presuppositions it carries and the manner in which they alongwith our earlier discussion of such premises in Post-Keynesian analysis can be used to contextualise Kaldorian methodological assertions as discussed above. Secondly, in terms of critically evaluating the manner in which Lawson specifically lays down his critical realist interpretation of the methodology of Stylized facts.

As far as the doctrine of realism is concerned, the central contentions, shared by all variants of realism are that a world exists beyond our perceptions of it and the goal of scientific enquiry is to attempt to address and understand aspects of this real world rather than the pursuit of self-contained logical games. There is a shared realist imperative: to understand the real world and there exists no room for a philosophy of science in which 'anything goes'. These propositions to a large extent are endorsed by theorists belonging to divergent traditions in economics, arguably even including sections of mainstream theorists. The problem then arises in evaluation of competing claims regarding the ability of the methods employed and theoretical constructs used by alternative streams in achieving this aim. As outlined above, Kaldor's critique of equilibrium orthodoxy was largely directed towards this very failure of mainstream theorising in meeting this basic aim of all economic theorising.

As critical realism is concerned, Lawson in addition argues for the existence of a structured social reality which depends on human agency while at the same time laying down various enabling and disabling structures within which such agency operates. This notion of a complex social reality can be shown to be consistent with the ontological presuppositions of a wide range of other heterodox traditions such as Institutionalism, Austrianism and even Marxism.⁴⁵ Infact these ontological premises would perfectly fit in

⁴⁵ Lawson himself acknowledges this in his most recent work (Lawson 2003) and goes on to suggest that differing heterodox traditions consistent with a critical realist ontology could perhaps then be distinguished on the basis of aspects of socio economic reality they focus on. He also suggests that such criteria of differentiation may also help to explain why such groups inevitably persist (especially if one accepts the proposition that many issues that separate such groups cannot be settled empirically). The distinctiveness of Post-Keynesian analysis in general, and Kaldorian analysis in particular could then be seen in terms of the specific aspects of socio economic reality it focuses on such as in its emphasis on the role of fundamental uncertainty of expectations that guide human behaviour (2003).

with our discussion of an open organicist ontology which characterises Post-Keynesian analysis. From this perspective, the aim of economic enquiry becomes an attempt to provide true scientific explanations of the actual functioning of an open and dynamic economic reality in terms of its underlying structures, mechanisms and entities.

In terms of ontological and epistemological presuppositions, the implicitly realist perspective stands in opposition to two diametrically opposing views in the philosophy of economics, namely, naturalism and anti naturalism. Linked to both these views is the question whether a philosophy borrowed primarily from natural sciences especially the physical sciences, can be correctly applied to economics. The naturalist tradition which has dominated most orthodox substantive positions in economics, especially econometrics, answers in the affirmative and is identified with Humean positivism which focuses on a regularity of or on constant conjunction of events. The instrumentalist approach to testing of theories through application of econometric techniques adheres to the implicit assumption that obtainable knowledge of any science is constituted by *empirical regularities between discrete events*. Experience of these atomistic events and their conjunctions exhaust our knowledge of nature. Deductive mathematical modelling of the kind employed by mainstream theorists in itself posits the presence of event regularities. The epistemic justification of this approach usually put forward is that our knowledge of reality is necessarily limited to the observable with the notion of causality being reduced to humean regularities in terms of constant conjunction of events. Any such epistemic critique of realist conceptions of causal generative structures underlying observable phenomenon however loses most of its bite in view of contemporary developments in the philosophy of science where the observable unobservable distinction loses force given the often stated truism that all known facts are theory laden. Besides critical realism, as outlined earlier, justifies an ontology positing process regularities of the kind advocated by Post-Keynesians among others, by arguing, that the phenomenal success of scientific activity would be incomprehensible in the absence such structured generative mechanisms which lead to the operation of certain causal tendencies (though they might not always be manifest in observable phenomenon) in nature.

On the other hand anti naturalism asserts that it is impossible to make any statement regarding the truth value of theories and hence to choose amongst competing theories. Here it is important to differentiate between the rhetorical/hermeneutic approach and the postmodernist approach both of which could be seen as adhering to such a view but on

different grounds. While the former chooses to remain silent over the question of the existence of an independent reality, the latter argues that reality itself is fragmented and somehow created by the various theories and perspectives on it. In its pure form, it implies ontological pluralism which denies the existence of unifying forces in nature. Both approaches entail a plurality of understandings of reality on grounds that there exists no known way of establishing what constitutes knowledge. A defence of general equilibrium theorising on such lines has been put forward by Prof E.R. Weintraub (1989), where an explicitly pluralist position has been taken, denying any scope for theory appraisal on grounds that there exists a plurality of understandings of reality. Accordingly, he dismisses Kaldor's critique of general equilibrium on the grounds that any scope for methodological argumentation could only be thought of in terms of the interaction among the select community of General equilibrium theorists:

It is our interpretive activity as members of a community given to seeking an understanding of the 'texts' community members produce...They [*opponents of general equilibrium analysis like Kaldor*] claim to discuss practice from a privileged position outside practice. They claim to have a perspective apart from, and thus neutral with respect to, the analyses they discuss. (Weintraub [1989]: p.267)

Such claims are rejected on grounds that given all facts are theory laden; there exists "no position apart from the doing of economics which can inform the consideration of the doing of economics (ibid: p.272)." Ironically enough he seems to argue for a context specific approach towards theory appraisal to justify general equilibrium theorising with its assumptions of universality of the scope of analysis carried out therein. Besides surely if an economist's understanding of reality is fragmented then so must be that of economic agent's (Dow 1997)?

A possible way could be to interpret Weintraub as arguing for an appraisal of general equilibrium theorising on its own terms, with regard to its aims and the manner in which it sets out to achieve them. I shall soon come to a discussion of issues involved therein .

while critically assessing Kaldor's critique of equilibrium theorising. Weintraub's dismissal of Kaldor's methodological critique however needs to be judged in terms of the differences in the implicit understanding of reality involved therein. As described earlier, Post-Keynesian or Kaldorian analysis does not stand in opposition to epistemic pluralism but it justifies it on grounds of 'fallibility' of all knowledge in understanding a complex open reality. Unlike radical relativism, it does not deny the absence of unifying forces in nature but posits the presence of partial regularities of processes rather than events which cannot be isolated from evolutionary or other irregularities. So Kaldor's critique from a realist perspective could be seen as implicitly more fundamental, denying the very notions of reality on which general equilibrium theory could be legitimised philosophically, whether it be the positing of event regularities in nature or a radical relativist position claiming that all theories could be justified on their own terms given a plurality of understandings of reality.

However, a very different case for formalistic techniques employed in the kind of closed system theorising which characterises General equilibrium analysis can be carried out in terms of their scientific use for heuristic purposes and for developing internal critiques. As far as the use of formalism in the development of an internal critique is concerned, the impact of an effective internal critique as pointed out by Hodgson (2004) is generally negative rather than positive; it shows the limits of an existing theory rather than building a new one. Such critiques do not themselves provide new theories, although they may suggest some appropriate measures and establish some relevant pointers. By their nature, internal critiques are not claims to map the real world. Instead, they are attempts to show where theories are inadequate or overly restrictive in regard to the kind of world to which they relate (ibid). However, it can still be argued, with some justification, that even for such purposes, mainstream theorising's emphasis on formalism has been excessive by any standards, given the amount of resources and skill expended in developing the sophisticated mathematical analysis of the past four decades. As Kaldor points out,

Professor Hahn turned the argument around and suggested,...,that the importance of general equilibrium theory lies precisely in showing how stringent the conditions must be for "free markets" to secure results in terms of welfare that are naively attributed to them. This may well be true, but if so, it is truth bought at a very high cost. (Kaldor 1985: p.14)

This brings us to the other use of formalism in economic theory; in terms of its heuristic value. In general, as pointed out by Hodgson (2004), no mainstream economist would deny that the world is open, and no adequate presentation of a formal model would omit to mention that other (omitted) causal mechanisms exist. Hence, it is not essential that a particular model involves explicit or implicit assumptions about the ontology of the social world. As discussed above, perhaps a section of mainstream theorists would even endorse a realist ontology. The heuristic purposes of mainstream theorising then could be seen in terms of its attempts to identify possible causal mechanisms that form part of a more complex and inevitably open system. Heuristics can be useful without necessarily making adequate predictions or closely matching existing data. Their purpose is to establish a plausible segment of a causal story, without necessarily giving an adequate or complete explanation of the phenomena to which they relate (Hodgson 2004). Though there haven't been many explicit statements by general equilibrium theorists clarifying the aims of their analysis, the views expressed by K. Arrow and Hahn among others could be seen as some of the possible exceptions in this regard. Hahn's views in particular are important for not only do they provide an informative and explicit account of the sort of reasoning involved, but some of them were also made in response to Kaldor's critique of general equilibrium theorising.⁴⁶ Though he concedes that general equilibrium theories have little directly to say about real economies, Hahn argues that they however answer important theoretical questions. Economic theory attempts "to gain an understanding of the particular by reference to generalising insights and in light of certain abstract unifying principles."(Hahn 1985: p.3)

According to Hahn, the basic axioms of general equilibrium theory as distinguished from assumptions are the ones that contain claims about the real world "so widely agreed as to make further arguments unnecessary" (ibid p.5) such as 'agents have preferences'. Ofcourse, as pointed out earlier while considering Kaldor's critique of equilibrium economics, these axioms are in themselves "relatively contentless abstractions" (Lawson 1989: p.72) with most of the analytical content being derived from powerful assumptions

⁴⁶ In particular see 'On the notion of equilibrium in economics' (1975) and the Jevon's memorial lecture 'In praise of economic theory.' (1985).

used to 'idealise and strengthen' the axioms, chosen more for mathematical tractability than for their ability to mirror real world phenomenon. For Hahn, the aim of equilibrium theory is not realism but to "understand where it is that the axioms and assumptions that are made logically lead" (ibid p.13). In addition, as Hausman has recently pointed out, theoretical justification of such analysis could also be seen in terms of its heuristics value in providing a conceptual apparatus or 'a bag of tools' to dip in as and when required. It can further be argued that formalistic reasoning is usefully employed in developing the internal coherence and logical structure of a theory. One such possible application argued for has been the "theoretical reassurance" provided by the existence proofs in General Equilibrium theory (Hausman 1992) to the central contention of neoclassical economics that 'equilibrium analysis' is useful in explaining how economies work (In particular, on its usefulness in explaining how self interested individual action within particular institutional constraints leads to coherent economic order). Now the use of theory for such conceptual explorations is perfectly justified but its relevance needs to be assessed in terms of the goals and motivations underlying such analysis. Excessive formalism then becomes the symptom of a deeper malaise: the neglect of developing interpretative contexts which take into account the complexities of a dynamic, evolving and open world. For, the question that then arises is that how does one assess the merit of the conceptual apparatus itself unless it is applied in understanding of any aspect of economic reality? Accordingly, analysing the usefulness of the interpretative framework of General Equilibrium theorising then becomes important in appraising the problem of excessive or misplaced formalism in economics. The problem then is not with the use of formalism *per se* but in an approach to theorising which leads to the inadequacy and underdevelopment of the interpretative context in which it is placed. As Hodgson suggests,

An adequate interpretative framework would depend on the discussion of the genesis, meaning and methodological significance of key concepts that are involved in the model or its interpretation. Yet in modern economics such interpretative and conceptual matters are often marginalized and underdeveloped. (Hodgson 2004)

It might be then suggested that perhaps Kaldor's more vague pronouncements on the use of formalism *in itself* was not 'spot on,' his repeated diatribe against the inability of

contemporary mainstream theorising to be useful for practical purposes and in dealing with real world contexts, hit the nail on its head. For instance as Harcourt suggests in his analysis of Kaldor's Mattioli lectures (1985),

He [*Kaldor*] sometimes made mistakes on details – for example, in the present volume i suspect he has been too harsh on the limitations of stability analysis in general equilibrium theory- but his criticisms of the cores of the theories and his intuitions are almost invariably spot on.(2001: p.241)

Accordingly, if a commitment to some kind of realism is accepted, our earlier discussion of Kaldor's critique of the mode of and purposes for which formalistic techniques are employed in General Equilibrium analysis, which lead to its being of little interpretative value, then continues to carry weight and can be put into perspective over here. For instance, as suggested in Lawson's critical realist reading of Kaldor

The methods of economic theory in general and General Equilibrium Theory in particular may have their own rationale even if, as Hahn critically notes, many of their proponents do not carry them through coherently. If a commitment to realism is accepted, however, the whole basic approach seems misconceived... Thus in starting from that which is merely most general, that which is most essential is lost. And by incorporating a layer of assumptions designed merely to achieve mathematical tractability, that which is real is emasculated. From this perspective, then, the resulting conceptions, as 'abstractions,' can indeed be seen, following Kaldor, as 'inappropriate,' 'artificial' or the 'wrong kind'. (Lawson 1989: p.76)

Also, Kaldor's advocacy of Stylized facts as a starting point in theoretical analysis can then be seen as an attempt to provide the basis on which the usefulness of competing explanatory abstractions could be brought into a clear and decisive focus. The aims of economic theorising under such circumstances, becomes the explanation of the functioning of a modern capitalist economy and its economic problems rather than merely an exercise in logic. As Kaldor states while explicating his critique of neoclassical economics as inhibiting the progress of useful knowledge:

By the term "useful" I do not just mean that it did not help the 'decision makers,' whether in the public or the private field, in arriving at the right decisions. Though the ultimate justification of scientific enquiry whether in the natural or the social field, is to improve our power over the environment, additions to knowledge are useful even if they do not have any immediate application, so long as they enable one to construct

improved models that highlight the critical aspects of how things work.(1985: p.57)

At the same time, there is a clear emphasis on a problem solving approach where the main motive is not

...the pursuit of economic theory for its own sake- the construction of more advanced theoretical models-but the severely practical motive of discovering methods or policies to improve the performance of the economy in terms of the twin objectives of efficiency and equality.(1985: p.8)

Given these ontological and epistemological presuppositions of Kaldorian analysis and his views regarding the practical and explanatory goals of economics as a social science, his methodological and theoretical critique of neoclassical economics and the rationale for his theorising based on *Stylized facts* naturally follows. For instance, in the construction of any theoretical model of economic growth and capital accumulation, Kaldor suggested a set of six Stylized facts⁴⁷ which could serve as a useful starting point. He further claimed that none of his 'Stylized facts' could plausibly be explained by the the existing apparatus of the neoclassical model. This validity and significance of this claim will be analysed in detail while discussing the more applied aspects of his works in the next chapter. However it is important to note over here that at the core of his critique then was not merely the perceived failure of equilibrium theorising to illuminate vital aspects of economic reality at present but also the inherent incapacity of such methods to *ever do so* given the methods employed by it which led to "the pursuit of economic theory for its own sake." According to him, the axiomatic abstract nature of equilibrium theorising (which presupposes a closed, static and atomistic world) has led to its becoming

...barren and irrelevant as an apparatus of thought to deal with the manner of operation of economic forces, or as an instrument for non trivial predictions concerning the effects of economic changes. ([1972] 1989b: p.1237)

⁴⁷ These were: a steady trend rate of growth of labour productivity; a steady increase in amount of capital per worker; a steady rate of profit on capital; the relative constancy of the capital output ratio; a fairly constant share of wages and profit in national income, and wide differences in rate of growth of output and labour productivity between countries (related to different levels of investment).

The question that remains however is that how does the critical realist perspective in general and Kaldorian analysis based on Stylized facts in particular, deal with the problem of 'fallibility' of knowledge and theory ladenness of facts in theoretical appraisals given that it identifies the true aim of scientific enquiry to be to provide accurate explanations of the actual workings of an independent and open reality? The problem of theory assessment becomes more severe on taking into account the fact that explanation is sought in terms of process regularities manifested in structures, mechanisms and entities underlying observable phenomenon which might not actually be discernable in actual events. Under such circumstances how does one discriminate among competing hypotheses put forward by alternative streams?

This leads us to the second level of analysis of Lawson's critical realist reading of Kaldor's methodology of Stylized facts. Lawson advocates the adoption of the method of abduction or retroduction to discriminate amongst competing hypotheses capable of explaining observable phenomena. It is here that his critical realist interpretation of Kaldor's methodology based on Stylized facts is perhaps the weakest.⁴⁸ Not only is it far from clear that Stylized facts adhere to this principle, there also exist ambiguities his delineation of the method of abduction in itself and the manner in which it can be employed in actual theorising. At times it almost seems that his description of Kaldorian methodology based on Stylized facts is coloured by his attempts to concretise and outline the principles underlying abduction itself rather than to demonstrate how such a methodology fits in with an explicitly laid out concept of the abductive method.

However his analysis of Stylized facts *per se* does have certain interesting insights. For instance, he lays down how the Kaldorian emphasis on starting with observed empirical regularities in any theoretical analysis could be reconciled with realist conceptions of causal generative structures and processes underlying observable phenomenon by conceptualising this regularities or Stylized facts as 'empirical counterparts' of tendencies or non empirical powers or features of such causal structures. His comments also suggest how Kaldor's own approach could be seen as *one* way in which the dilemmas arising due

⁴⁸ . As Lawson himself points out, Kaldor's emphasis on "subordination of induction to deduction" distracts somewhat from the type of inference that is central to the realist analysis, that of abduction. He also notes that Kaldor does conflate the notion of a stylised fact and a tendency at times while in his realist reading of Kaldor the two notions are related but not conflated (1989: p.65).

to epistemic relativism are sought to be addressed in a manner consistent with basic propositions of a realist perspective. For as he points out, as far as Stylized facts are concerned, though obviously preconceptualised, they might be seen as useful entry points to begin the analysis. A transformational view of social activity is implicit in Kaldor's reliance upon an existing heritage- in an exposition of what could be considered as truly exogenous at a given point of time.

The only truly exogenous factor is *whatever exists at a given moment of time*, as a heritage of the past. This includes all material things, whether the products of nature, or man, or a combination of them,...All these in existence at the present moment, the heritage of all past history, determine what can be produced or created in the immediate future, say in the next day, and that, together with what exists now, determines the range of alternatives for the day after, and so on. (Kaldor 1985: p.61)

Such a transformational view is relevant, according to Lawson, not merely because it mirrors a manifest social reality but also because of its importance for scientific activity given the implicit acknowledgement of epistemic relativism in the recognition that all analysis necessarily starts somewhere (with a given stock of theories, methods, hypotheses and facts etc). It also counters foundationalist notions that it is possible "to obtain some immutable foundations for knowledge through some thing like perception, intuition or direct experience alone." (Lawson 1989: p.67) Hence according to Kaldor, Stylized facts are so called

Because in social sciences, unlike the natural sciences, it is impossible to establish facts that are suggestive and intriguing in their implications, and that admit of no exceptions. (1985: p.8)

Accordingly Stylized facts become a starting point for further analysis:

When we say, for example, that in course of economic growth the profit rate or the capital/output ratio tends to remain constant, or that productivity varies procyclically whereas the variation in real wages is neither procyclical nor anticyclical, we do not imply that any of these 'facts' are invariably true in every conceivable instance but they are true in the broad majority of observed cases-in a sufficient number of cases to call for an explanation that would account for them. (ibid: p.9)

Infact an implicit acceptance of fallibility of knowledge is implicit in Kaldor's articulation of his own methodological stance as it leaves scope for continuous reformulation and reconceptualisation at all levels. Not only are the implications of

hypotheses constructed subjected to further scrutiny, identified empirical regularities are themselves historically contingent (unlike basic postulates of equilibrium theory) and hence subject to continuous revision in view of an open, dynamic evolving reality:

Such hypotheses relate to particular *aspects* of the economy and maybe suggestive of others. They maybe discarded if they prove inconsistent with other observed features and then be replaced by something else. (ibid: p.8-9)

This stands in radical opposition to the foundationalist notions implicit in the neoclassical approach to theorising, as discussed earlier, where explanatory task of neoclassical theory is done when economic phenomenon have been traced to the fundamental economic causal factors. Now it is important to note that again both views are in a way consistent with Mill's philosophical contention that laws in economics need to be seen as statements of tendencies which are operative in an economy but may not materialise due to presence of countervailing tendencies. However their methodological approaches differ in terms of the implicit beliefs regarding the nature and properties of regularities that could be conceived as laws. *Event regularities and the concept of fundamental psychological laws derived from introspection, intuition and observation characterise neoclassical theorising. In contrast process regularities and laws based on empirical regularities which are however subject to continuous revision in keeping with an evolutionary perspective towards reality* are essential features of Post-Keynesian analysis in general and Kaldorian analysis in particular. Within this perspective the role of statistical analysis is limited to an input into the general framework of the application of a theory, where the main objective is to provide concrete guidelines for historically determined situation.

The exposition of the implications underlying Kaldorian methodological stance attempted here would be incomplete without explicitly bringing out the notion of piecemeal engineering which characterises theory building on the basis of Stylized facts. As Kaldor points out, Stylized facts entail a more problem determined approach to theorising where one advances 'bit' by 'bit' nearer to a more comprehensive understanding of reality.

In comparison with the high sounding principles of the great systematizers, this kind of inductive deductive theorizing may appear pedestrian. But it is

far more likely to lead to a better understanding of how capitalist economies work than the all embracing principles of great system builders who, in the field of economics at any rate, are more likely to obstruct the progress of knowledge than to promote it. (Kaldor 1985: p.9)

It also facilitates

“Argument in the Babylonian style ... conditioned by the problem at hand, employs(*employing*) a range of methods suited to the problem and these methods cannot be combined into one formal deductive argument without drastically changing their nature” (Dow 1996 : p. 13)

What is at stake here is the entire structure of justification, and not deduction *per se*, for all analysis requires a kind of abstract deductive analysis in the first instance, but the Kaldorian analysis stands in opposition to a comprehensive treatise which can account for all essential features of modern capitalist economies.⁴⁹ Again to what extent does this kind of piecemeal engineering actually “lead to a better understanding of how capitalist economies work” is debatable and a discussion of the issues involved is carried out in the following chapter while critically assessing Kaldor’s alternative formulations analysing the processes of growth and development in modern capitalist economies.

To sum up, the rational reconstruction of Kaldorian economic analysis from the perspective of his methodological critique of the neoclassical orthodoxy and his own distinctive methodological stance, attempted here, brings out various ontological, epistemological and methodological issues. It has been argued that an open system organicist ontology and epistemology sets the grounds for Post-Keynesian analysis in general and Kaldorian analysis in particular. As far as Lawson’s critical realist reading of Kaldor goes, its ontological presuppositions were shown to be consistent not merely with Kaldorian analysis but also a range of heterodox traditions in economics. Though alternative philosophical positions which stand in opposition to realism and which could possibly be used to legitimise mainstream theorising were examined, it was also argued that as far as the central doctrine of realism *per se* is concerned, the realist understanding of economic reality and goals of scientific theorising, in itself, was not necessarily in contradiction with explicitly laid out aims of mainstream theorising. Towards this end a

⁴⁹ Kaldor says as much in justifying why he never attempted a systematic or comprehensive statement of his views, “I have never felt that one’s understanding of economic processes has reached a stage where it is no longer liable to radical revision and development in the light of new experience.” (Kaldor 1978)

justification of the employment of a closed system theoretical structure by mainstream theorising in general and General Equilibrium analysis in particular for heuristic purposes and for the development of internal critiques was explored. It was then argued that the Kaldorian critique of equilibrium orthodoxy here could still be contextualised in terms of the latter's inability to address realist concerns of improving our understanding of the actual functioning of modern capitalist economies operating in historical time and space. Accordingly, he was construed to be suggesting that the kind of abstract deductivist methods employed by mainstream theorising and the kind of abstractions they necessitated were inappropriate and of the 'wrong kind' with little interpretative value. He identifies an alternative approach to economic theorising which proceeds on the basis of identification of empirical regularities as Stylized facts. It was argued that Lawson's conceptualisation of this approach in terms of the method of abduction was not very convincing and had several ambiguities which^{are} left unaddressed. However, it does bring in certain interesting insights regarding the manner in which Stylized facts *per se* could be perceived such as their conceptualisation as 'empirical counterparts' of tendencies underlying observed phenomenon which serve as useful entry points for further analysis. The specific features of the Kaldorian methodology based on Stylized facts and the manner in which they contrasted with mainstream approach were then highlighted which include: a more empirical and informal basis for theorising proceeding on the basis of identification of process regularities; the emphasis on induction; an eclectic approach characterised by a kind of piecemeal engineering; the advocacy of an evolutionary approach which draws its parallels from the biological rather than the physical sciences.

Chapter Four

Long Run Growth, Development and Stylized Facts: A critical appraisal of Kaldor's methodological assertions

In the last chapter, an attempt was made to reconstruct Kaldor's critique of equilibrium economics and his own methodological approach towards economic theorising in a rational and coherent manner. Now, as brought out in the reconstruction attempted in the last chapter, the implicit ontological stance in the Kaldorian research programme is one postulating an open, organic and dynamic socio-economic world which exists independently of our knowledge of it. It was also argued that given Kaldor's numerous methodological assertions, a critical realist perspective was consistent with the epistemic aims of Kaldorian analysis. From this perspective, the aim of economic enquiry becomes an attempt to provide true scientific explanations of the actual functioning of an open and dynamic economic reality in terms of its underlying structures, mechanisms and entities. Kaldor's own contribution lies in his distinct methodological approach of theory building on the basis of Stylized facts in order to achieve this aim

Any overview of various criticisms that could be or have been directed at the Kaldorian research programme from different quarters could be located at multiple levels. It could be directed at its ontological and epistemological presuppositions, at the critical realist reading of Kaldor, at the critical realist approach to theorising itself, to the methodology of Stylized facts and also towards its specific application in Kaldor's distinct approach to growth. Some of these issues (such as ontological and epistemological questions, critical realist reading of Kaldor etc) have already been critically discussed at different points in the analysis carried out till now. I shall try and touch upon most of them over here but the emphasis shall be on the last two aspects which though related to the rest, also constitute the basis of Kaldor's own contribution to economic analysis from a methodological perspective.

A more explicit and critical discussion of the important methodological issues in theory building through Stylized facts needs to be explored through Kaldor's own formulations on growth as well as through certain questions raised by alternative contemporary analysis in the field. Kaldor's application of his distinctive methodological approach in

framing a pioneering alternative analysis of long run growth and distribution stands out as his distinctive contribution. Alongwith his emphasis on increasing returns, factor creation and endogenous technical progress as essential features of modern capitalist growth, it forms the basis of his critique of equilibrium economics and his own alternative vision of the actual processes of growth in modern capitalist economies. Accordingly, as an important application of his methodological assertions, it becomes important in providing specific instances for critiquing such assertions from a methodological vantage point. Hence, in what follows, I begin with a brief overview of some of Kaldor's more relevant theoretical contributions around the themes of growth, accumulation and development. The relevance of Kaldor's critique of neoclassical approach towards long run growth and distribution⁵⁰ is then further explored with regards to contemporary growth analysis in endogenous growth theories. This is followed by a critical exposition of important methodological issues underlying the methodology of Stylized facts in order to bring out its inherent ambiguities and inconsistencies given the context provided by Kaldorian own analysis on growth.

As brought out earlier, an important feature of the methodology of Stylized facts is its 'piecemeal approach.' For Kaldor, it is not necessarily the aim of economics to develop a grand unified theory accounting for all empirical regularities; separate theories maybe required for each *Stylized fact*. He was as good as his word. The last period of his work from 1966, which focussed on the growth process in industrial economies "never developed into anything approaching that level of synthesis" which characterised his earlier work in the 1950s on income distribution and technical change (Thirlwall 1991: p.33). In this last phase, he also gave up formal model-building employed in his earlier work; mathematics was employed more as a language than a tool or was largely restricted to elementary regression analysis (as in Verdoon Law).

I'll be chiefly drawing upon the last phase (mid sixties to mid eighties) of his analysis⁵⁰ where all the features of the methodological approach outlined above are present in a

⁵⁰ As has been emphasised earlier, prior to mid sixties, he himself was significantly involved with an approach which implicitly emphasised mainly deductive reasoning based on highly abstract macroeconomic formulations.

greater or lesser degree. It is important to remember that Kaldor's most basic critique of equilibrium orthodoxy arose out of the manner in which long run growth was analysed through 'comparative statics' which completely overlooked essential features of observed growth processes such as endogeneity of forces bringing about change and the operation of increasing returns. Since his account of growth forms the basis of his methodological assertions and provides the context for most of his theorising in other areas, it might be fruitful to begin there and slowly build up a relatively complete Kaldorian economic worldview from his numerous theoretical articulations.

Kaldorian analysis of Long Run Growth and Development in modern Capitalist Economics

As has been indicated earlier, his own empirical research and practical experience made Kaldor deeply dissatisfied with the excessively aggregated formal nature of macroeconomic growth models. Their microeconomic underpinnings also seemed inadequate. As he reflects back,

In my subsequent work I followed a different method. I tried to find what kind of regularities can be detected in empirically observed phenomenon and then tried to discover what particular testable hypotheses would be capable of explaining the association...It is an approach which in one sense is more modest in scope (in not searching for explanations that derive from a more comprehensive model of the system) and also more ambitious in that it directly aims at discovering solutions (or remedies) for real problems. (Kaldor 1978: p. xvii).

This launched the second major phase of Kaldor's work on growth, in which several fundamentally new ideas displaced or modified some of the principles of the earlier phase in which some of his earlier theoretical insights were reintroduced. It was based on analysis incorporating some widely prevalent phenomenon such as the existence of sectoral complementarities, spatial patterns of development, absence of full employment etc. He began with an attempt to explain some Stylized facts of economic growth derived from the conceptual schema of statistics and observed historical patterns. These were among others: a steady trend rate of growth of labour productivity; a steady increase in amount of capital per worker; a steady rate of profit on capital; the relative constancy of the capital output ratio; a fairly constant share of wages and profit in national income, and

wide differences in rate of growth of output and labour productivity between countries (related to different levels of investment).

Accordingly he constructed various hypotheses explaining the actual process of growth in modern capitalist economies in terms of the various underlying phenomena (captured by the role of uncertainty, expectations and demand, the presence of increasing returns, the endogeneity of technical progress, the operation of cumulative causation etc) which could account for these observed Stylized facts. Kaldor's analysis at this stage was particularly eclectic- the sources were varied and a kind of synthesis of various strands of theorising was attempted-in particular, the phrase – "*the marriage of Young and Keynes*"(Eatwell 1982) - was employed to characterise his analysis of increasing returns, circular and cumulative causation and long run growth and distribution.

As pointed out by Boylan and Gorman (1995), Kaldor's intellectual commitment to increasing returns can be linked within the broader framework of his ideas to a critique of time as embodied in the notion of equilibrium. *Time* for Kaldor was a '*continuing and irreversible process.*' Hence it was to be analysed in terms of its historicity necessitating the need to engage the influence of specificity of context and circumstances. He further argued that it was impossible to assume the constancy of anything for all periods of time. The only real exogenous factor was whatever existed at a given moment of time, the legacy of an unchangeable past. This historical legacy would determine future events in a manner that varied inversely with the distance of the future from the present. Therefore the capacity to predict an uncertain future becomes progressively less with respect to the more distant, as compared to the more immediate, future.

For Kaldor, the concept of the equilibrium and, in particular, the notion of long run equilibrium as used by the neoclassical orthodoxy utterly failed in dealing with this line of analysis. It was not surprising that it was unable to explain the dynamics of human society given the employment of the methods, laws, concepts and theoretical tools belonging to the field of social statics. Given its methodology, equilibrium orthodoxy requires that the operation of economic forces be explained by the interaction of a given set of exogenous variables that determined the endogenous variables. Hence the concept of time underlying equilibrium treats exogenous variables as "independent of history in their most important characteristics" (Kaldor 1985: p.62). Kaldor argued that continuous

growth can only be thought in this framework as a steady state where everything changes at proportionate rates, “though what the proportions are, or what the growth rate is, is itself the outcome of economic forces” (ibid: p. 62). The failure to recognize the real dynamic form of growing societies led to imposition of an analytical shape on their growth models (such as the basic Solow-Swan model) which required convergence to either the ‘stationery state’ or the ‘steady state’. The main methodological characteristic of the neoclassical growth model then was that rather than asking what real world economic dynamics looks like and how it can be modelled (as proposed by methodology of Stylized facts), the neoclassical growth theorist wants to know how the existing corpus of production theory could be used to generate an outcome that can meet a technical definition of growth. As a result the analysis was incapable of dealing with two most important phenomenon features of capitalist growth: continuous change in knowledge and non-linearities in production i.e. the presence of increasing returns. He accordingly set out alternative formulations where the interface between growth, increasing returns and technical progress feature prominently.

Increasing Returns, Endogenous Technical Progress and Factor Creation

According to Kaldor, manufacturing is the *engine of growth* in modern capitalist economies as it lends itself easily to the operation of increasing returns. In industry, the growth of output per worker arises principally because of static and dynamic economies of scale, whose realisation depends on (but also contributes to) expansion of markets for industrial products. In Kaldor’s view, Increasing returns, noted by Smith but subsequently emphasised by only a few economists such as Marx, Marshall and Allyn Young, are a multidimensional, pervasive feature of Industrial production. They often exist at plant and firm level and are also to be found at the industry level: larger scale permits greater internal specialisation of production among different firms. Finally increasing returns operate at a macroeconomic level, partly because different industries stimulate each others development through demand and supply linkages especially due to spatial concentration of manufacturing activity, partly because all of them benefit from a common labour market large enough to justify the development of many highly specialised skills. Hence Kaldor adopted a very broad view of increasing returns placing

no *a priori* limits (unlike Young, Hirschman and other theorists in the cumulative causation tradition) on the growth of internal economies and consequently regarding oligopolistic industrial structures to be a typical feature of advanced economies

Increasing returns and technical progress are mutually reinforcing. This is because the construction and operation of large scale plants, the finer subdivision of production processes, and the emergence of more specialised skills all require the development and application of new knowledge. Taking an evolutionary perspective, Kaldor emphasises that Increasing returns are not simply a static function of the scale of production, but also of the cumulative amount of production over time. Each revolutionary stage of realisation of scale economies is arrived at by a sort of 'learning by doing' process. Kaldor's conception of 'learning by doing' encompasses both incremental improvements in efficiency flowing from the repetition of a given manufacturing process and the generation of new technologies. Technical progress facilitates industrial growth but does not drive it.

For Kaldor, factor supplies are largely endogenous to such system. Even labour constraints were not operative given availability of labour supply as a result of international and national migration; the latter mainly involving transfer of workers from low productivity agriculture or services to high productivity manufacturing. Also for Kaldor, given his concern for growth and dynamics as opposed to allocation of fixed resources, complementarity in production and consumption is far more pervasive and significant than the neoclassical principle of substitution. It arose due to the presence of indivisibilities and fixed factor coefficients. In addition, given endogenous factor creation and increasing returns, and excluding the very short run, the supply of one product does not have to be at the expense of another. (Kaldor 1985: p.61-62). Hence the assumption of factor substitutability was part of Kaldor's fundamental critique of equilibrium growth theory not merely due to problems regarding meaning and measurement of capital as a factor of production but also because the focus on substitution

...ignores the essential complementarity between different factors of production (such as capital and labour) or different types of activities (such as between primary, secondary and tertiary sectors) which is far more important for an understanding of the laws of change and development of the economy than the substitution aspect.... Indeed, it is, I think the concentration on substitution which makes "pure" equilibrium theory so lifeless and motionless. (Kaldor 1974[1989].: p.348)

Effective Demand and Constraints to Growth-the marriage of Young and Kaldor

Kaldor was critical of Young's classically inspired growth model for ignoring the implications of effective demand. While he agreed with Young that demand generates its own supply, he rejected Young's view that supply automatically generates its own demand. To explain the effect of an increase in aggregate supply or increased productivity on aggregate demand Kaldor used three central Keynesian insights: these are the role of money, entrepreneurial expectations and the notion of induced investment. In particular the process of endogenous self sustained growth for him required both "an elasticity of expectations concerning the volume of sales (in regard to manufactures) and inelasticity of expectations concerning prices (in respect of primary products)" (Kaldor 1972: p.1250). Though he never attempted a very detailed synthesis of Young and Keynes, he used a modified version of the conceptual apparatus available therein to formulate his own ideas on growth. He was critical of Keynes' failure to break away with neoclassical economics especially in the latter's acceptance of diminishing marginal productivity, marginalist theory of price formation and failure to unambiguously reject the quantity theory of money. In addition he also differed from Keynes in endorsing Harrod's claim that exports are a principle source of autonomous demand. While Young argued that manufacturing was subject to self sustained growth (subject only to the constraints that increasing returns and high income elasticities of demand for manufactured goods are not exhausted), Kaldor uses the Keynesian concepts of endogenous and exogenous demand to explain how the long run rate of growth of manufacturing output is determined from outside the sector. Endogenous demand 'reflects (i.e. is automatically generated by) production' though in a money economy demand can be a function of supply as an exogenous component (Kaldor 1983: p.49). While Keynes emphasised the role of domestic investment as the source of exogenous demand, Kaldor gave special importance to exports in determining the constraints to long run growth in open economies.

Limits to growth and sectoral complementarities: Kaldor's two sector-two stage disequilibrium growth model

In Kaldor's view technical progress as well as growth in labour productivity in

agriculture and mining was largely exogenous and less responsive to growth of output than industry, which implied that there was a relatively inflexible upper limit on the rate of growth of primary production. He used a two sector- two stage model of development. The two 'stages' respectively refer to a closed economy at an early phase of industrialisation or in a broader sense the world economy as a whole and an open economy at a later stage of industrialisation.

In a closed system, say the world economy as a whole,⁵¹ this technically determined upper limit on the rate of primary production growth is the main long term constraint on the growth of industrial production, and hence on the growth of the whole economy. One reason for this is that expansion of industrial production requires increased amounts of wage goods for industrial workers and of raw materials for processing. Relatedly, growth of primary production and incomes is a vital source of growth in demand for the products of industry. According to Kaldor the growth of demand from the primary sectors in a closed economy actually determines the long term growth rate of industrial production. This is because there is no enduring limit to growth within the industry itself: the supply of industrial capital, labour, knowledge and skills will generally respond to whatever happens to be the rate of growth of overall demand for industrial products. It is also because expansion of demand for industrial products from within the industrial sector is in the long term passively determined by expansion of industrial production.⁵² The presence of relatively inflexible primary-industry terms of trade becomes important for the analysis to hold, since otherwise any primary sector output constraint on industrial growth might be overcome by an increase in the prices of agricultural and mining products relative to industrial products, which could make a larger volume of primary output profitable, increase the purchasing power of primary producers over industrial goods, and switch some industrial purchasing power from primary to industrial products. This does not happen, in Kaldor's view because industrial wages-and hence industrial prices-are inflexible downwards in terms of their purchasing power over primary

⁵¹ The analysis is also applicable to largely closed economies like India where foreign trade constitutes a relatively insignificant share of the total economic activity.

⁵² Kaldor considers his analysis as an extension in a long run setting of the Keynesian principle that output is determined by effective demand, combined with Harrod's concept of foreign trade multiplier and Hick's concept of supermultiplier.

products. As a consequence primary- industry terms of trade usually do not improve enough to prevent primary sector production from constraining the long term pace of industrial growth. Consequently it is the rate of technical progress in land based activities which determines the rate of growth of manufacturing output.

In an open economy (at a later stage of development), export demand for industrial goods grew more rapidly than domestic demand from the agriculture chiefly because of a higher income elasticity of demand for manufactured products (Thirlwall 1987: p.220). It is the growth of exports, through the foreign trade multiplier, which determines the rate of manufacturing output growth for a particular country. The rate of growth of manufacturing output for a particular country accordingly depends on the rate of growth of world income and the share of world demand captured by the country's exports. Kaldor therefore advocated a development strategy based on industrial specialisation and export expansion. However he did acknowledge a role for import substitution and infant industry arguments, though under strict conditions. This two sector-two stage model is critically examined from a methodological perspective in the next chapter.

In keeping with his stance of discarding hypotheses found to be inconsistent with observed phenomena, he shifted from his earlier position that *shortage of labour* constituted a constraint to Industrial growth to arguing that expansion of industrial output is normally not constrained by availability of labour. This was largely because the primary sector in developing and the tertiary or services sector in developed nations acted as an industrial employment reservoir, since they contained a considerable proportion of underemployed workers earning lesser wages than industrial workers available and ready to fill the vacancies created in the Industrial sector. Also for Kaldor service sector expansion was the consequence, rather than an active ingredient of growth of output and employment in other sectors, particularly industry.

Kaldor's Growth Laws

Kaldor attempted to provide strong empirical support to his disequilibrium growth analysis through his three growth laws regarding development of industrial economies the basis for which was laid down by the view that ' manufacturing is the engine of growth' and which were derived from regressions carried out using a cross section of data

on twelve developed nations. The regression results accordingly formed his three growth laws : the existence of a strong positive relationship between the rate of growth of gross domestic product and the rate of growth of manufacturing output ; a strong positive relationship between the rate of growth of productivity in manufacturing industry and the growth of manufacturing output (Verdoon Law⁵³) ; a strong positive relationship between the overall growth in productivity and the rate of growth of output and employment in manufacturing.

Increasing Returns and Stylized Facts of Industrial Economies

In addition to the empirical support for his disequilibrium growth model provided by his growth laws, Kaldor argued that the model received strong support for and provided explanations for several key features or *Stylized facts* of mature industrial economies. These facts were important not only because they contradict the predictions of equilibrium theory: they also can only be explained by the presence of external economies and increasing returns.

Cumulative Causation and Spatial Patterns in Trade and Development: Industrial Agglomeration and Regional income disparities

Kaldor used the analysis of his disequilibrium growth model to provide a related theoretical explanation for the differing growth paths of different regions of the world economy alongwith a complementary account of determinants and consequences of trade between these regions based on the underlying phenomenon of circular and cumulative causation. In opposition to the orthodox theory of international trade, he held that free trade did not necessarily benefit all participants. He was extremely critical of the use of standard assumptions such as constant returns to scale and identical production functions in standard Heckscher-Ohlin-Samuelson trade models which led to such conclusions.

According to Kaldor, the root cause of the concentration of Industrial development in particular regions was increasing returns, which led to self perpetuation of forces making for success or failure. As a result any country or locality which obtains an initial

⁵³ The empirical relation was first identified by P.J. Verdoon (1949) and Kaldor drew on Verdoon's work in the formulation of his second growth law.

advantage in setting up an industrial centre achieves higher labour productivity than other smaller industrial sectors, which with fairly uniform wages, implies lower unit costs. This results in competitive advantages such as ability to charge lower prices or incur increased research and development expenditure leading to an expansion in market share and production which further lead to higher productivity and so on with migration of workers from declining industrial centres overcoming any labour shortages in the expanding industrial centres.

Kaldor emphasised a subtly different version of cumulative causation based on Verdoon relationship between growth of industrial output and industrial labour productivity, coupled with the assumption that the relative growth rates of exports from different localities depend on relative growth rates – rather than relative levels – of unit costs which made the difference in industrial growth rates self-perpetuating. For, just as in a closed economy the long term rate of industrial growth is determined by growth of demand from primary producers, for a particular locality the necessary external determinant of its industrial growth is the growth rate of industrial exports. Thus cumulative causation was important in explaining the differing industrial development paths of different countries. Rapid industrial growth, rapid labour productivity growth and rapid export growth constitute a virtuous circle for some countries, with a corresponding vicious circle of low growth for others. The underlying mechanism is not exactly the same as for different localities within the same country, because restrictions on international migration mean that wages are not uniform across countries. Moreover, empirical observations seemed to confirm what Kaldor's own theory of distribution implied, that variation in level and growth of real wages across different countries (basically developed nations) is closely related to level and growth of labour productivity which clearly reduces the competitive advantages in international markets which would be otherwise enjoyed by countries with higher or faster growth rates of productivity. This initially led Kaldor to advocate a policy of dual exchange rates for developing countries to enable them to exploit the competitive advantage of lower levels of real wages. However experience with floating exchange rates after 1971 led him to the conclusion that neither exchange rate adjustments nor linkages between productivity and real wages are in reality adequate to neutralise cumulative causation. A possible reason behind this,

suggested by Kaldor, was greater innovations in product quality through increased research and development which led to greater competitiveness and hence made for faster export growth and so on.

However, in terms of policy conclusions, the emphasis on increasing returns led him to have mixed feelings about trade. The world as a whole benefited from trade due to expansion of markets needed for realisation of increasing returns and the most advanced nations from benefits of increased industrial specialisation and exchange. However given the destructive and disequalising effects of trade for industrially backward countries, the larger (and unsolved) question remained that how to secure these collective benefits without aggravating their difficulties. His two sector-two stage model seemed to prescribe a narrowly based industrial structure as the optimum strategy to exploit scale economies for developing countries. I shall come to a critical evaluation of its implications in a while.

Microtheoretic Foundation: Oligopolistic market structure, Role of Prices and Price Formation

Kaldor's analysis of the actual functioning of markets in capitalist economies both stemmed from and contributed to his macroeconomic view of growth. The continued preoccupation with perfect competition, diminishing returns, pure price system of market clearing etc was fundamental to his critique of failure of economics to adopt what he considered to be more scientific methods. As he states,

If economics had been a 'science' in the strict sense of the word, the empirical observation that most firm operate in imperfect markets would have forced economists to scrap their existing theories and start thinking on entirely new lines. (1986: p.19)

He was critical of Keynes failure to break away from traditional modes of thought, especially those of competitive market mechanisms which were incompatible with Keynesian macroeconomic analysis of the existence of unemployment and regretted the fact that an attempt to build a new integrated theory based on Keynesian macroeconomics with microeconomics built on the foundation of imperfect competition and oligopoly had not been undertaken seriously. He sketches various areas and lines of analysis, pursuing which might prove fruitful. These include a theory of how prices are determined in the

oligopolistic conditions prevailing in industry and in perfectly competitive conditions prevailing in agriculture and most types of mineral extraction (where the producers are *price-takers* not *price-makers*) and finally under conditions approximating the 'pure' imperfect competition of 'polypoly' (small scale businesses combined with free entry, with each seller facing a limited market) which prevails over much of the tertiary sector, such as retail distribution or miscellaneous market services. For manufacturing industry in particular, he emphasised the indeterminacy of pricing and forms of competition as Stylized facts of modern industrial economies. He was of the view that prices are set by producers as a mark up on normal costs of production assuming normal capacity utilisation. He also ventured to suggest that the mark up was determined so as to maximise "the attainable rate of growth of profits" given the two conflicting objectives of maintaining an increasing market share to exploit economies of scale on one hand and risk reduction through reducing reliance on external sources of finance by expansion through retained earnings.

He was also of the opinion that price stability was a pervasive feature of industrial sector where quantity adjustments were a better indication of changes in market demand. Also such stability was desired as it aided corporate planning and helped in maintenance of customer goodwill.

I've just briefly sketched Kaldor's numerous theoretical contributions around the theme of growth and distribution in modern capitalist economies with the purpose of bringing out their consistency with his overall methodological stance based on the notion of Stylized facts. They also brings out a picture of modern capitalist economies evolving in historical time and space characterised by uncertainty, expectations, group behaviour where the neoclassical fallacy of composition⁵⁴ in explaining the behaviour of economic agents is avoided. The analysis is both organic and structured in terms of bringing out the mutual interactions and interrelationships between agents, entities and structures underlying observed phenomenon. At the same time the emphasis on building the analysis on empirical foundations does not lead to a purely inductive analysis. Abstraction and deduction play an important role at the level of analysis where a wide

⁵⁴ The fallacy arises due to failure to recognize the fact that the whole is not necessarily the sum of its parts.

variety of conceptual tools, chosen for their appropriateness given the specificities of the context, are employed.

The significance of his analysis and his critique of neoclassical approach towards long run growth and distribution is now looked at through the lens of contemporary growth analysis in the form of endogenous growth theories which attempts to overcome one of the fundamental criticisms directed against the neoclassical growth model by Kaldor, namely in its inability to endogenize technical progress.

Endogenous Growth Theory and Stylized Facts

To begin with, there has been a current revival of interest on the question of increasing returns and externalities in mainstream literature on growth represented by what is popularly known as 'endogenous growth theory' which has emerged as an extension of Solow- Swan model. A principal implication of the neoclassical growth model was that the underlying rate of output growth is the sum of exogenous labour force increase and exogenous technical change. This gave rise to a central paradox in neoclassical growth theory whereby it demonstrated that "technological change was of fundamental importance for economic growth... but denied the possibility that economic analysis could have anything to say about this process" (Romer 1991: p.85). As pointed out earlier this theoretical paradox was reinforced by neoclassical growth accounting exercises which found the bulk of the increase in long run GDP's to be due to reasons other than increase in the quantity of factor supplies.

Like the neoclassical theory, endogenous growth theory identifies technological innovation as the principal source of increase in per capita output, but locates the source of technological change within the system by invoking some externality (primarily knowledge) that offsets any propensity to diminishing marginal productivity arising from capital deepening *per se*. In most models of endogenous growth, knowledge is generated as a product or by-product of economic activity with its specific sources identified as investments in R&D, human capital, physical capital and learning by doing. Knowledge is conceived as a unique economic good, since to varying degrees it is non-rivalrous and non-excludable. As a result there are no opportunity costs in the use of non rival inputs (Romer 1994). Also diminishing returns to a factor do not apply, as improvements in the

quality or productivity of factors (especially capital) offset such tendencies. Accordingly knowledge and cumulative expansion of knowledge 'sustain both capital accumulation and growth.'

Now the basic problem with the introduction of technological change in earlier models on growth was that it violated the assumption of perfect competition, owing to the generation of extraordinary profits by innovators, and requires the introduction of a more complex production function as ways of achieving equilibrium solutions (Snooks 1998: p.42). Realism was accordingly constrained by the nature of existing microeconomic theory⁵⁵.

In the endogenous growth theory the attempts to endogenize technical change have passed through two main stages (Snooks 1998, Barro and Sala-i-Martin 1995). The earlier stage involved an attempt to introduce technical change as a form of 'learning by doing,' (inspired by Arrow's 1962 model) which arose as an unintended consequence of investment. Thus, technical discoveries immediately spill over to the entire economy through an unrealistic instantaneous diffusion process. The advantage of this assumption of course was that monopoly profits did not arise allowing the competitive framework to be retained. However, as was also brought out in our analysis of Kaldor's growth models, the assumption is highly unrealistic for not only does innovation require intentional actions by economic agents in the form of R&D investments but also, in reality, technological diffusion takes place gradually over long periods of time.

The second stage of in development of endogenous growth models began by the introduction of theories of R&D and imperfect competition by Romer (1987, 1990). Here technological change was the outcome of deliberate investment in R&D to secure monopoly profits. The outcome is a positive long run growth for as long as such investment continues. Owing to generation of monopoly profits, these models are usually accompanied by policy prescriptions for improving welfare outcomes through taxes and subsidies.

Whilst endogenous growth theory has developed some fundamental criticisms of neoclassical theory which take into account the Kaldorian critique on the endogeneity of

⁵⁵ While highly critical of earlier growth models on this account, none of the new growth theorists seem to be aware Kaldor had already developed a model that endogenized technological change using a technical progress function as far back as 1957.

factors which make for change, it retains most of the flaws which formed the basis of *his* methodological critique of neoclassical growth theory in explicating the features of dynamic, open economies characterised by fundamental uncertainty. From a realist methodological perspective in general and Kaldorian perspective in particular, its fundamental deficiency, lies in its emergence from the corpus of static neoclassical production theory which results in distorted models of societal dynamics. The focus remains on *events or outcomes rather than processes*; on supply rather than demand (effective demand not consumer demand); and on the mechanics of production than on uncertainty and expectations⁵⁶.

Ironically, enough most of these flaws arise out of a preoccupation with achieving mathematical tractability, which also happens to be one of the reasons behind Romer's dismissal of Cambridge growth models. As Romer comments,

Nicholas Kaldor and Joan Robinson waged a form of guerrilla warfare against the neoclassical model, and they invoked Young's name among others, in support of their cause. But they offered no tractable alternative model. (Romer 1991: p.89)

The thrust of Kaldor's critique, as would be obvious by now, was on the other hand directed at this overarching concern with formalistic modelling at the cost of real world applicability⁵⁷. Moreover this attempt at achieving realism through incorporating endogeneity of change in mathematically tractable models has led to a loss of generality as well as an increased arbitrariness of the theoretical constructs. For instance, as Snooks (*ibid*) points out, in Romer's growth model (1990), the production function assumes a specific functional form which is required in order to *achieve the desired outcome*: the absence of diminishing returns and the presence of endogenous long run growth. Romer chooses to endogenize technical change through an increase in product variety as a proxy for innovation. Firms invest in R&D in order to discover new intermediate products in expectation of monopoly profits. The production function adopted as a basis for the model specifies diminishing returns for each input and constant (rather than increasing) returns to scale for all inputs put together. The production function for the *i*th firm is then

⁵⁶ Ofcourse Kaldor's earlier, more formal growth models also had similar deficiencies and were part of the reason for his abandonment of the same in his later informal phase of theorising on growth which has been outlined in detail above.

⁵⁷ Perhaps this could also be seen as another example of Kuhnian paradigmatic incommensurability.

$$Y_i = A.L_i^{1-\alpha} \cdot \sum_{j=1}^N (X_{ij})^\alpha$$

Where $0 < \alpha < 1$, Y_i is output, L_i is labour input, and X_{ij} is the employment of j th type of specialised intermediate good. Technological change is introduced in the form of expansions in intermediate good N , and is expressed as

$$Y_i = A.L_i^{1-\alpha} \cdot N \cdot X_i^\alpha = A.L_i^{1-\alpha} \cdot (N X_i) \cdot N^{1-\alpha}$$

Accordingly if the increase in NX_i takes the form of a rise in N for given X_i , diminishing returns do not arise and endogenous long run growth occurs. Hence endogenous growth arises from this particular property or assumption of the production function (Snooks 1989: p.43-45).

Technological change is an arbitrary assumption to the growth models as they lack any behavioural explanation regarding the motivations underlying investments in risky R&D activities and the manner in which they are undertaken in face of dynamic uncertainties. In addition, recent attempts by new growth theorists to incorporate ideas of development and historical economics to explain conditional convergence as evidenced by global growth rates through ad hoc assumptions has led them to a departure from the method of rigorously deriving results from existing body of mainstream economic theory. Thus, they begin to lose the very theoretical robustness and coherence which was arguably an important reason behind the adoption of the a priori deductive methodology in the first place. As Snooks comments,

At the end of all this endogenizing we have a complex model that can only be grasped intuitively in its details and only through the computer in its overall implications. This is always the problem with models that cannot see the ultimate for the proximate causes – the wood for the trees. (Snooks 1998: p.45)

Accordingly, Kaldor's criticisms regarding the structure and strategy of the a priori formal deductivist methodology of equilibrium economics, which resulted in its becoming a "logically watertight thing" of little interpretative value, inherently unable to incorporate assumptions representative of real world economies in a logical and coherent manner becomes significant.

As regards realisticness, not only have the optimistic implications of the new growth models predicting an indefinite growth process come under criticism, development

economists express serious reservations regarding their ability to explain recent growth processes. They seem to endorse a kind of Kaldorian methodology of observing real world growth paths of nations and groups of nations, and to explain them in statistical terms⁵⁸. Further, the new growth theories seem to recognize only technological externalities as opposed to the very broad view on increasing returns endorsed by Kaldor, which incorporated static and dynamic economies of scale involving both pecuniary as well as technological externalities existing at plant, firm, industry and also at an economy wide level. The exclusive focus on technological externalities seems to be driven by the need to achieve compatibility with the mathematically tractable equilibrium approach as effects of pecuniary externalities usually exhibit marked disequilibrium properties.

Pecuniary externalities in contrast play a significant role in Kaldor's analysis based on Stylized facts of observed growth processes. According to Scitovsky they arise when the profits of a firm depend on its own output and factor utilisation as well as on the output and factor utilisation of other firms or industries (1954: p.300). They can be seen as special features of investment in modern growth economies (in particular of the industrialisation process of developing economies) which arise due to 'interdependence through the market mechanism', although they also entail direct interdependence. Also, investment projects occur in real historical time, with such projects changing future demand and supply conditions. The combination of imperfect information about future market conditions, indivisibilities⁵⁹ and the existence of adjustment lags in the volume and industry composition of investment give rise to disequilibria and pecuniary externalities. As Joan Robinson states,

The uncertainty that surrounds expectations of the outcome of a plan of investment, of the course of technical progress, of the behaviour of future prices, not to mention the effects of natural and political cataclysms, cannot be reduced to 'calculated risk' by applying theorems of mathematical probability... As soon as the uncertainty of the expectations that guide economic behaviour is admitted, equilibrium drops out of the argument and history takes its place. (Robinson 1977: p 48).

Accordingly, if a commitment to realism is accepted, the "Guerrilla warfare" so scathingly attributed to Kaldor and Joan Robinson by Romer, was then perhaps not

⁵⁸ See for instance Angus Maddison (1995) and Steve Dowrick (1989)

⁵⁹ As a result of which more or less output will be produced than that will equate marginal costs and price.

without cause. For in Kaldor's view, it was highly improbable that a "tractable alternative model" could be employed to bring out the processes underlying observed features of actual growth (such as effects of pecuniary externalities), especially if tractability implies (as it does in Romer's own analysis) compatibility with equilibrium approach. The methodological priority given to creation of tractable equilibrium models and the corollary of admitting technological externalities as the only source of increasing returns leads to the retention of several features of neoclassical orthodoxy which impede their realism in economic analysis. At the same time by embracing ad hoc methods of theorising in order to somehow try and accommodate historically observed phenomenon within established theoretical corpus, the theoretical rigour and forcefulness of the models is diluted, reducing their appeal for neoclassical purists. Hence, the example set by these theories reinforces Kaldor's critique of the inability of the tools employed in mainstream theorising to successfully illuminate significant aspects of socio-economic reality without substantially compromising its theoretical edifice.

Stylized facts and growth in modern capitalist economies: some illustrative features

The methodology of Stylized facts itself, however, adopts a kind of inductive deductive approach which makes it susceptible to criticisms from both the advocates of empiricism as well as those who uphold hold theoretical rigour and logical coherence. These criticisms need not necessarily come from mainstream theorists or economic methodologists alone. As was pointed out earlier, a critical realist ontology is consistent with a wide range of other heterodox traditions such as Institutionalism and Marxist economic analysis. It is not possible to go into a detailed analysis of their distinctive theoretical and methodological approaches over here. However in general, it could be safely assumed in a prima-facie manner that they would go along with the contention that the basic task of economic theory is to provide an explanation of the structures, mechanisms and entities which underlie observed economic phenomenon. However they would differ in terms of the appropriate approach towards achieving this objective and it is here that they might differ from the methodology of Stylized facts on methodological and theoretical grounds. In this context, a brief exchange that occurred between Kaldor and Baran over the latter's thesis on underconsumptionism in the 1950's (during Kaldor's initial phase of more formal theorising on growth) might be illustrative.

Underconsumptionism refers to the view that a shift in the distribution of social income away from the workers to the capitalists, produces, through shrinking demand, a tendency towards stagnation under capitalism. The standard objections to "underconsumptionism" were based on its empirical foundations and were two-fold: first, there was no perceived tendency towards secular stagnation in the capitalist world. True, the inter-war years had witnessed the "Great Depression" which had persisted until the start of re-armament (in fascist countries earlier, and in liberal capitalist countries under the fascist threat), but this did not amount to a secular tendency, since post-war capitalism had experienced remarkable growth rates. Secondly, there was not even any statistical evidence to show that the share of profits in output was rising in the advanced capitalist countries as predicted by the underconsumptionist argument. Kaldor made the latter point when he dismissed the argument that as a result of increasing monopoly, "the share of profits would go on rising beyond the point where it covers investment needs and the consumption of capitalists" and therefore, that "the system would fail to generate sufficient purchasing power to keep the mechanism of growth in operation" with the statement: "the plain answer to this is so far, at any rate this has not happened."(1957 a: p.621)

A realist perspective is clearly underlined in Baran's response (1969), which as pointed recently by Prabhat Patnaik (2004), brought out underconsumption as an ex-ante tendency in advanced capitalist economies. The ex ante tendency towards underconsumption, which underlies the new situation, is not directly visible: it has called forth and is therefore camouflaged by State intervention. This, Baran argued, is exactly what was happening in post-war capitalism, where State intervention, taking the form of larger military expenditure, had prevented the realization of the ex ante tendency towards underconsumption (Patnaik 2004). As Baran states

... in a theoretical analysis of the generation of "sufficient purchasing power to keep the mechanism of growth in operation," little is gained by registering whatever volume of purchasing power happened to enter the market resulting in such a level of income and employment (and unemployment) as happened to prevail, if no effort is made to pierce the obvious to comprehend the forces which give rise to that volume of purchasing power and which determined the nature and the rate of growth of output related thereto. (Baran 1969: p.188)

Again as Patnaik points out, Baran's analysis had the following implications: first, since advanced capitalism had succeeded to a large extent in manipulating its internal contradictions, the main resistance to it could come only from the "outlying regions" of the third world where its military might was being put to use for imposing a new capitalist order; secondly, it is its oppressiveness and irrationality, as opposed to any internal politico-economic crisis arising from its unworkability on account of the playing out of its immanent laws, that constituted the real flaw of contemporary capitalism. The system in other words was not one that got bogged down in crises and stagnation, but one that worked by wasting huge amounts of resources on maintaining massive military expenditures.

Accordingly Baran could be seen as highly critical of the inductive implications of Kaldor's critique. As he states while dismissing Kaldor's empirical critique, "It should be obvious, however, that this argument, far from disposing of the problem, fails even to reach the theoretical level on which it rises" (Baran 1969: p.188). The empirical merits of his claims apart, it is hardly possible to dismiss Baran's analysis here on grounds of absence of a realist outlook, for it also claims to bring out certain mechanisms inherent in modern capitalist system which lie behind observable economic phenomenon. Also, it concentrates on processes rather than outcomes, and is not dictated by the requirements of mathematical tractability and formalistic modelling.

For Kaldor on the other hand, a constancy of the share of profits in national income was an important Stylized fact and a phenomenon calling forth hypotheses to explain it.⁶⁰ This brings us back to the discussion carried out in the previous chapter regarding Kaldor's predisposition in sometimes conflating the notion of a 'Stylized fact' with a tendency. Besides the methodology of Stylized facts in itself fundamentally differs from certain other 'realist' approaches in economic theory in that the movement to and from abstraction always involves empirical study, for not only are empirical regularities chosen as the starting point of analysis, the different abstractions are combined and synthesised

⁶⁰ It is important to note that in his later analysis on growth, apart from some cursory remarks on price formation in oligopolistic structures, Kaldor did not put forward any explicit account of distribution and the variety of complex transmission mechanisms 'between changes in productivity and changes in demand and income distribution. However his analysis on the existence of mark up pricing could be seen as a possible explanation behind the observed constancy of the share of accounting profits in total industrial output.

in knowledge of contingent relations and conditions. It could be argued over here then that even if one accepts a realist ontology, theory building on the basis of Stylized facts might preclude an understanding of certain hidden generative mechanisms due to an overly inductive emphasis at times.

Ironically enough, as Toner (1999) brings out in his analysis of theories in the cumulative causation tradition, another range of critique that has been levelled against Kaldor's application of his methodological stance in his two sector-two stage model is based on its not being inductive enough. For instance, the limits to growth identified by Kaldor in both his 'open' as well as 'closed' sector model have come under severe attack on empirical grounds. In case of closed economy model, Kaldor's contention, that presence of diminishing returns in agriculture and mining made productivity growth in land based activities the ultimate determinant of the long run rate of growth of manufacturing output, is criticised on the grounds that this phenomenon is scarcely evident on the basis of available statistical evidence. In particular, Kaldor's own regression results do not support the claim that the primary sector is subject to diminishing returns. If anything, they indicate that both agriculture and mining are subject to greater increasing returns than manufacturing. For example, they reveal that a one percent increase in output was associated with a 0.5 percent increase in manufacturing productivity, while in agriculture and mining it was associated with a 0.7 percent increase in productivity. Besides Kaldor's constraint to growth is held to be inconsistent with the *Stylized fact* that 'long run real global commodity prices (excluding oil) have declined or at best remained stable, at the same time as their output has increased dramatically' (Spraos 1980). Interestingly, Kaldor here supports his analysis by invoking the presence of diminishing returns as an ex-ante tendency,

...the classical contention that [agriculture and mining] ...are diminishing return industries: the fact that this is overlaid by technological progress or the adoption of more capital intensive methods may statistically conceal this, but it does not eliminate its significance. (Kaldor 1966: p.16-17)

Similarly, Kaldor's proposition that the long run rate of manufacturing output in an open economy is determined by the rate of growth of manufacturing exports and the consequent policy implication of specialisation through narrowly based industrial sector that exploit economies of scale has also come under criticism. While not denying the

beneficial role played by exports and the validity of balance of payments constraint to growth, it has been pointed out that import substitution and export growth are not necessarily competing strategies and it is not at all clear that an import substitution and domestic demand led growth strategy is not sustainable on the basis of observed historical patterns of growth. As Eatwell, citing results of a study on growth of production for domestic consumption and export in manufacturing industries in Japan⁶¹ points out,

The Japanese case suggests that the traditional dichotomy between import substitution and export led growth is invalid. Whilst Japanese industry was developed within a rapidly growing and protected home market, that growth proved to be a springboard for expansion into world markets. Exports were domestic growth led. (Eatwell 1987: p.738)

Accordingly, “the case against Kaldor is that he did not follow his own inductive precepts, but imposed an *abstract* two sector-two stage model on the study of industrialisation. He thereby ignored the great diversity in historical patterns of development and denied the scope of endogenous self sustained growth through import substitution and domestic output growth” (Toner 1999: p.156-157, *italics mine*).

The empirical merits of the above cited instances critiquing Kaldor’s methodological assertions and theoretical formulations apart, what could be their implications for the methodology of Stylized facts? Besides pointing out the inconsistencies that were present in Kaldor’s critique of alternative analysis and his own theoretical contributions vis-à-vis his methodological stance, they also raise other, more complex, questions at a broader level.

As far as the methodology of Stylized facts is concerned, there seems to exist at times, a tension between the acceptable levels of abstraction and the need to meet the criterion of theoretical rigour. The problem with an rigid adherence to the tenet that observed empirical regularities which form the ‘Stylized facts’ of modern capitalistic economies, be the starting point of all analysis, is that the operation of numerous countervailing tendencies lead to an unmanageable flux of observed events deciphering which (even by

⁶¹ The choice is significant for as Cornwall points out “growth in exports seldom exceeds growth in home market, even in a country[Japan] considered to be most oriented towards realising and exploiting foreign markets...growth would have to be characterised as homespun. (Cornwall 1977: p.193)

identifying partial regularities) might invariably lead one to theoretical inconsistencies in any broad based analysis. Kaldor was perhaps aware of this problem as a result of which he advocated a bit by bit problem solving approach, insisting that “one should seek the most reasonable explanation” capable of accounting for each separate “Stylized fact,” independently of whether it fits into the general framework of received theory or not” (1985 : p.8-9). He also indicated that observed historical patterns might be the ultimate indicators of the validity of all analysis.

The practical problems with such an approach are evident in his own analysis on growth where he attempts to explain his own ‘Stylized facts’ of modern market economies. Selection of any particular set of Stylized facts over others for explanation in itself is open to charges of ad-hocery in absence of clearly laid down criterion for determining which ‘Stylized facts’ are acceptable. Not only can one point out problems with the choice of particular facts for which an explanation was attempted, it is also arguable, as brought out by our discussion, that there exist various inconsistencies in Kaldor’s own theoretical formulations vis-à-vis observed patterns of growth.

For all his assertions on not attempting to move towards any grand theory on the functioning of modern market economies, he himself comes very near to providing such a complete structural explanation of long run growth processes. Also the two sector-two stage model outlined earlier raises questions regarding whether the kind of piecemeal engineering advocated by Kaldor precludes the tendency of a theorist to arrive at sweeping conclusions on the structural workings of the entire system. This is brought out in his analysis of overall ‘limits to growth’ in modern economies, which, as was pointed out above, is subjected to considerable debate in the literature. Infact the problem might be sometimes more acute in such analysis as compared to grand theories purportedly working towards a comprehensive understanding of the entire system. This may occur due to the fact that unlike the latter, in a piecemeal approach the theorist is under no obligation to explicitly account for all relevant factors and might hence escape accountability commensurate with the theoretical implications of his analysis.

Ofcourse one might argue, with some justification, that Kaldor’s analysis on growth is just one application of theory building on the basis of Stylized facts and cannot be used to discredit entirely the methodological implications of this approach to economic theorising.

An appraisal based on such applications still remains important, given the fact that Kaldor's theoretical contributions, in his later phase of theorising on growth, are probably the only applications that attempt to rigidly adhere to the guidelines prescribed by his methodological stance.

However, its significance also needs to be judged in terms of its pioneering approach towards a more informal analysis on growth where formal model building was consciously renounced. It, alongwith an emphasis on realism and a need to make theoretical constructs more amenable to interpretative structures representing the real world within which they can be placed, underlie his more eclectic methodological approach. Accordingly, they provide an interesting perspective, as a point of contrast with neoclassical theorising where an emphasis on analytical rigour often comes at the cost of real world applicability.

A very prominent feature of the above discussion is also its implications in terms of re-enforcing the value ladenness of all theoretical analysis. One could argue that ultimately what is more important is not the particular methodology adopted but the particular theoretical framework within which one is located and its explicit or implicit ideological leanings. For the particular facts chosen for explanation by the theorists and the conceptual apparatus with which he's working are crucial in determining the shape and the contents of the subsequent analysis. And the criterion behind these choices invariably depends on the theorist's value judgements. So Baran, in keeping with his Marxist ideological leanings, chose to focus on bringing out the exploitative or irrational aspects of modern capitalist economies employing a distinctively Marxist terminology and conceptual apparatus (as brought in analysis of 'secular stagnation', 'underconsumption', tendency towards a falling rate of profit etc) . Similarly, the neoclassical theorists, atleast those subscribing to the liberal doctrine of laissez-faire, concentrate on theoretical abstractions essential to lay grounds for proving the optimality of the free market mechanism within a framework laid down by general equilibrium analysis. Kaldor, a passionate reformer within the established market tradition, on the other hand chose to concentrate on manifest aspects of modern capitalist economies, especially where they led to glaring inequalities and inefficiencies, to facilitate policy analysis aimed at discovering methods or policies to improve the performance of the

economy in terms of the twin objectives of efficiency and equality , that is, how to minimise the cost in terms of economic inequality at higher productivity or efficiency. (Kaldor 1985: p.8)

The implication that political and philosophical conceptions of a theorist invariably influence even the most genuinely objective attempts at 'pure' economic analysis by determining the vision of the theorist and its manifestation in the preanalytical act of selection of certain feature of reality for explanation brings us back to the question of 'theory ladenness of all facts'. However, in opposition to radical relativism, the purpose behind the kind of methodological appraisal attempted throughout in this study has primarily been, among other things, to bring out the manner in which a critical assessment of the analysis under scrutiny could still be attempted. As Mark Blaug so succinctly puts it,

The problem is not denying the presence of propaganda but that of separating the scientific ideas from the ideology in which they are invariably embedded and to submit these ideas to the scientific test of validity. Moreover, propaganda is not the same thing as lying: to say that Karl Marx wanted to discredit capitalism and began with preconceptions about its defects is not to imply that his analysis is for that reason worthless. Political prejudices may even assist scientific analysis: a critic of capitalism is likely to pay more attention to the real blemishes of the system and it is surely no accident, for example, that Marx's comments on business cycles were fifty years ahead of his time. (Blaug 1996: p.5)

Accordingly an attempt to bring out the specificities, nuances and finer points of Kaldorian contributions to economic analysis as well as their critical assessment by employing a range of criteria has largely been the preoccupation of this dissertation. Probably its time for winding up the analysis carried out through the concluding comments.

Chapter Five

Concluding Remarks: A brief overview of the Central Arguments

I shall just review some of the central themes explored, the arguments made and the questions raised by this study over here. As I had warned at the outset, the study perhaps ends up with more questions than answers. Its purpose has been chiefly to explore the complex interface between the methodological presuppositions of a particular research programme and its worldview, goals, conceptual apparatus and values (whether explicit or implicit).

Towards this end a reconstruction of the Kaldorian research programme from the vantage point of methodological pluralism was carried out vis-à-vis its ontological and epistemological foundations, the aspects of socio economic reality it focuses on in given applications and the methodological and theoretical implications which follow. One of the questions the study began with was the extent to which the 'aims' of economics could be 'claimed' to have been met in 'reality' and attempted to search for the possible answers that could be found in an appraisal of the methodological premises of a given heterodox research programme and its critique of the mainstream theory.

As the analysis has brought out, the question in itself is a layered one, opening up a virtual Pandora's box by bringing out a plethora of complex issues around the problems of theory appraisal which continue to escape any satisfactory resolution. The purpose of this analysis has throughout been to explore these issues critically from the Kaldorian perspective. To begin with, there seem to exist certain ambiguities regarding the relative importance of competing claims such as prediction, explanation and description within the discipline given the diversity and heterogeneity of the subject matter under consideration. Still a broad consensus seems to exist around the contention that the primary aim of economics is to illuminate vital aspects of socio economic reality of real economies operating in historical time and space.

This raises important questions regarding the relevant criterion of assessment of progress within the discipline which in turn devolve on the notion of nature of economic 'reality' in itself as well as the perception of such a reality. Radical relativism arguing for

a plurality of understandings of reality, is often used to validate the claim that the only basis for appraisal lies in the inner evolution of any research programme and can as such be carried out only through the interpretative acts of the participants of such a programme itself. Accordingly, if progress is to be judged in terms of internal development which takes the form of a greater sophistication in tools of analysis, then the modern mainstream has indeed progressed, as witnessed by the range and refinement of analytical and mathematical techniques and abstract models that have emerged in the past few decades. However if the historical and institutional dimensions of a continuously evolving and open economic reality are accepted, then it could be argued, with a lot of justification, that enlargement in the array of tools and techniques at the disposal of mainstream theorists has not been accompanied by any commensurate improvement in the predictive and explanatory powers of the discipline given the actually observed processes of the historical evolution of modern capitalist economies. Kaldor's strongest objections to mainstream theorising, discussed and evaluated at length in this dissertation, arose primarily out of such concerns over the significance of large parts of mainstream economic analysis which precluded any useful explanation of 'the critical aspects of how things work in modern market economies.' It was argued that his critique was a fundamental one, aimed at the structure and strategy of mainstream theorising which had little interpretative value and informed by his conceptualization of economic reality and his teleological beliefs regarding the motivations, objectives and concerns of economics as a discipline. A cursory examination of contemporary analysis on long run growth and distribution in the form of endogenous growth theory was also attempted to bring out the relevance of his critique. This was brought in the inherent inability of mainstream economic theory to incorporate certain pervasively observed economic phenomenon (such as endogeneity of change and increasing returns) in a logically compelling manner within its received theoretical framework. Not only has the preoccupation with meeting the requirements of formalistic and mathematically tractable modelling in the 'new growth theories' led to a retention of several features which impede the realisticness of the models, the integration of arbitrary assumptions to include historically observed features of economic reality has at the same time compromised their theoretical and analytical rigour.

However, as was brought out in the historical contextualisation attempting to locate the Kaldorian contributions to economic thought within the discipline, the history of economics as well as of debates within the contemporary philosophy of science amply demonstrate how any easy answers regarding the precise steps or methods by which the primary aims of economic analysis can validly be met still elude us. Given the notion of fallibility of all analysis and the contemporary truism that all facts are theory laden, it seems to be virtually impossible to discover any algorithm of doing good economic science. Nevertheless, it has been argued over here that this need not necessarily imply radical relativism where either a plurality of hypotheses is considered to be good in itself or where it is stated that no claims at all can be made regarding the status of competing theories. Accordingly, one of the aims of this analysis has been to explore how recognition of inevitability of some degree of methodological and theoretical pluralism (given the absence of known, universally acceptable criteria of theory appraisal), need not necessarily imply radical relativism⁶² which precludes the employment of any standards for a critical assessment of competing paradigms. Plurality which arises as a corollary of an evolutionary understanding of reality and the knowledge of it as an open system does not rule out the possibility of any critical evaluation. This premise was explored at two levels in this analysis. Firstly, through employment of a critically pluralistic position to assess the claims of Kaldorian economic analysis using a range of criteria involving an evaluation of the ontological and epistemological basis of the analysis, its ability to provide answers to questions of epistemic relativism, its subjectivities in a wider historical context, its particular stance on the right way forward in comparison to the mainstream approach towards theorising and an assessment of the specific applications of its explicit methodological stance in Kaldor's alternative formulations on growth. Secondly, in exploring the ability of Kaldorian analysis in particular and Post-Keynesian analysis in general to deal with questions of epistemic relativism. It was argued that this problem is dealt with through an implicit open system organicist ontology which underlie the theoretical premises of such analysis. It differs from radically relativist positions in

⁶² As discussed earlier, this entails either complete *skepticism* (rhetorical/hermeneutical approach) or *anarchism* (following the postmodernist approach) regarding the nature and knowledge of reality in itself.

that it believes in the existence of regularities in nature which any science, including economics, should aim to identify but posits them to be regularities of *processes* rather than of *events* which cannot be isolated from evolutionary or other irregularities. The grounds for these premises were then explored in methodology of Stylized facts and its application in Kaldorian growth analysis.

In summary, a more nuanced approach towards arriving at a deeper understanding of Kaldorian contributions to economic thought, than is brought out through application of simplistic or reductionist categories such as empiricism, positivism or verificationism or through a dismissal of his analysis on purely ideological grounds, has been argued for. In this context, Tony Lawson's realist reading of Kaldor was brought out to demonstrate its significance in bringing out a more coherent and cogent articulation of Kaldor's contributions and in laying down their ontological and epistemological foundations. At the same time, it was also pointed out that a more comprehensive and critical approach needs to look at certain specific features of the Kaldorian contributions to economic theory and methodology *per se* in order to bring out its distinctiveness vis-à-vis other heterodox and arguably realist traditions in the discipline. Hence his methodological assertions regarding theory building on the basis of Stylized facts were analysed in detail with regards to its special features such as: a more empirical basis for theorising proceeding on the basis of identification of process regularities; the emphasis on induction; an eclectic approach characterised by a kind of piecemeal engineering; the advocacy of an evolutionary approach which draws its parallels from the biological rather than the physical sciences

Lastly an analysis of various criticisms of Kaldor's own formulations on growth alongwith an illustrative exchange between Kaldor and Paul.A.Baran (another heterodox economist within the Marxist tradition) on the question of underconsumptionism in modern capitalist economies was carried out with a view of bringing out certain contentious propositions and inconsistencies in theory building on the basis of Stylized facts. It was argued here that there exist certain ambiguities, inherent in the kind of inductive deductive approach and piecemeal engineering advocated by Kaldor, which leave the approach vulnerable to criticisms on grounds of both empirical validity and theoretical rigour.

However, despite such problems, the Kaldorian analysis provides an interesting perspective on contemporary debates in economics by providing an alternative approach to proceed with methodological and theoretical analysis. Event regularities and the concept of fundamental psychological laws derived from introspection, intuition and observation characterise neoclassical theorising. In contrast process regularities and laws based on empirical regularities which are however subject to continuous revision in keeping with an evolutionary perspective towards reality are essential features of Post-Keynesian analysis in general and Kaldorian analysis in particular. As has been brought out over here, the approach assumes significance in highlighting some problems of contemporary economic analysis which needs to grapple with the problem of understanding an open, rich and continuously evolving economic reality in all its dimensions and complexities.

Bibliography

Abelson, Peter. 1996. 'Declining Enrolments in Economics: The Australian experience.' *Royal Economic Society Newsletter*, no. 95, 19-20.

Agarwala, A.N. and Singh S.P. 1973. *The Economics of Underdevelopment*, Oxford University Press, New Delhi.

Arrow, K. J. 1962. 'The economic implications of learning by doing.' In W.N. Parker (ed.), *Economic History and the Modern Economist*. Oxford: Blackwell. pp. 13-20.

Backhouse, R.E. & Biddle Jeff ed. 2000. *Toward a History of Applied Economics*. Durham & London: Duke University Press.

Baran. P. A. 1970. *The Longer View: Essays Towards a Critique of Political Economy*, John O' Neill, New York, Monthly Review Press.

Barro, R.J. and X. Sala-i-Martin. 1995. *Economic Growth*. New York: McGraw Hill.

Boylan, T. A. & Gorman, P. F.O. 1995. *Beyond Rhetoric and Realism in Economics: Towards a Reformulation of Economic Methodology*. New York: Routledge.

Bead, M and G. Dostaler. 1995. *Economic Thought Since Keynes*. England: Edward Elgar.

Bhaskar, Roy. 1978. *A Realist Theory of Science*. Hemel Hempstead: Harvester Press.

Blaug, M. and Neil de Marchi. 1991. *Appraising Economic Theories: Studies in the Methodology of Research Programs*.

Blaug, M. 1980,(2nd edition 1992). *The Methodology of Economics*. Cambridge: Cambridge University Press.

_____.1997.(5th edition). *Economic Theory in Retrospect*. Cambridge: Cambridge University Press.

Boland, L. 1982. *The Foundations of Economic Method*. London: George Allen & Unwin.

_____. 1987. 'Stylized Facts.' Entry in Eatwell, J. Milgate, M., and Newman, P. (eds.)

Caldwell, B.J. 1982 (Revised ed.1994). *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: George Allen & Unwin.

_____. 1988a. 'Developments in Economic Methodology with Implications for Political science.' *Politics* 8(2): 43-8.

_____. 1988b. 'The Case for Pluralism.' In N. de Marchi ed. *The Popperian Legacy in Economics*. Cambridge: Cambridge University Press.

_____. 1989. 'Post-Keynesian methodology: an assessment.' *Review of Political Economy*. Vol 1. pp. 43-69.

Carabelli, A. 1988. *On Keynes's Method*. London: Macmillan.

Coats, A.W. 1984. 'The Sociology of Knowledge and the History of Economics.' In *Research in the History of Economic Thought and Methodology*. 2: 211-34.

Comim, F. 'On the Concept of Applied Economics: Lessons from Cambridge Economics and the History of Growth Theories.' In Backhouse, R.E. & Biddle Jeff ed. 2000. *Toward a History of Applied Economics*. Durham & London: Duke University Press.

Cornwall J. (1977), *Modern Capitalism: Its Growth and Transformation*, Martin Robertson, Oxford.

Davidson, P. 1981. 'Post- Keynesian Economics', in D. Bell and I. Kristol (eds.) *The Crisis in Economic Theory*. pp. 151ff.

DeMarchi, Neil. and A. Hirsch. 1986. 'Making a case when Theory is Unfalsifiable: Friedman's Monetary History.' *Economics and Philosophy*. April , 2: 1-22.

Dorwick, S. and D.T. Nguyen.1989. 'OECD comparative economic growth 1950-85: catch up and convergence..' *American Economic Review*. 79 (5): 1010-30.

Dow, Sheila C. 1990. 'Post-Keynesianism as Political Economy: A Methodological Discussion.' *Review of Political Economy*. 2-3, 345-58.

_____. 1992. 'Post Keynesian School.' In D. Mair and A. Miller ed. *A Modern Guide to Economic Thought: An Introduction to Comparative Schools of Thought in Economics*. Aldershot: Edward Elgar.

_____. 1997. 'Methodological Pluralism and Pluralism of Method.' In Andrea Salanti and E. Screpanti ed. *Pluralism in Economics: New Perspectives in History and Methodology*. Aldershot: Edward Elgar. pp.89-99. rep. in Hodgson(ed.) 2002. *A Modern Reader in Institutional and Evolutionary Economics Key Concepts*. Cheltenham U.K: Edward Elgar. pp. 136-145.

_____. 2002. 'History of Economic Thought in the Post-Keynesian Tradition.' *History of Political Economy*. Annual Supplement to Vol. 34. pp. 319-336

- Eatwell, J.1982. *Whatever Happened to Britain? The Economics of Decline*, Duckworth, London.
- _____. 1987. "Import substitution and export-led growth", in Eatwell et. al. (eds.) Vol. 2, pp.737-738.
- Eatwell, J. Milgate, M., and Newman, P. (eds.) .1987 *The New Palgrave Economics Dictionary*, 4 Volumes, Macmillan Press Ltd. : London.
- Eichner. A . and J. Kregel, 1975. 'An Essay on Post-Keynesian Theory: A New Paradigm in Economics', *Journal of Economic Literature*. vol.13, 1293-1314.
- Friedman, M. 1953. 'The Methodology of Positive Economics.' In *Essays in Positive Economics*. Chicago: University of Chicago Press.
- Fullbrook, Edward. 2003. *The Crisis in Economics: Teaching, Practise and Ethics*. New York: Routledge.
- Hall , R. L. and Hitch , C.J. 1939. 'price theory and Business Behaviour.' *Oxford Economic Papers*.2: 12-45.
- Hahn, F. 1985. *In Praise of Economic Theory*. The 1984 Jevons Memorial Lecture: University College London.
- Hamilton, D. *Evolutionary Economics*. London: Transaction Publishers.
- Hands. D. W. 1985. ' Karl Popper and Economic Methodology.' *Economics and Philosophy*. 1: 83-99.
- Hansen. A. 1947. *Economic Policy and Full Employment*. New York: Mc Graw Hill.
- Harcourt, G.C. 2001. *50 Years a Keynesian and Other Essays*. New York: Palgrave.
- Harrod. R. F. 1939. ' An Essay in Dynamic Theory.' *Economic Journal*, vol.49. pp. 14-33.
- Hausman, Daniel M. 1989 . ' *Economic Methodology in a Nutshell*. ' *Journal of Economic Perspectives*. ' Vol 3. No. 2. pp. 115-127 . rep. in In B.J. Caldwell (ed.) *The Philosophy and Methodology of Economics I*. pp. 275-284.
- _____.1992. *The Inexact and Separate Science of Economics*. Cambridge:Cambridge University Press.
- _____. 1998. 'Problems with realism in Economics.' *Economics and Philosophy*. Vol. 14, no. 2, October, 185-213.
- Hodgson, G.M. 1993. *Economics and Evolution*. London: Polity Press.

____.(ed.) 2002. *A Modern Reader in Institutional and Evolutionary Economics Key Concepts*. Cheltenham U.K: Edward Elgar.

____.2004. "On the Problem of Formalism in Economics." in Symposium on Reorienting Economics, *Post Autistic Economics Review*. Issue No. 28, 25 October.

Hunt. D. 1994. *Economic Theories of Development: An analysis of competing paradigms*. New York: Harvester Wheatsheaf.

Hutchison, T. J. 1938.*The significance and Basic Postulates of Economic Theory*. London: Macmillan.

____.1992. 'Changing Aims in Economics.' In *Lectures in Economics* Oxford: Blackwell.

____1994. *The Uses and Abuses of Economics: contentious essays on history and method*. New York: Routledge.

Johnson, E. and H. Johnson. 1978. *The Shadow of Keynes: Understanding Keynes. Cambridge and Keynesian Economics*. Blackwell.

Kaldor, N. 1935. 'Market Imperfection and Excess Capacity.' *Economica*. February.

____.1939. 'Speculation and Economic Stability.' *Review of Economic Studies*. October.

____.1943. 'Beveridge Report II. The Financial Burden.' *Economic Journal*. April.

____.1944. 'The Quantitative Aspects of Full Employment Problem in Britain, in W Beveridge. *Full Employment in a Free Society*. London: George Allen and Unwin

____.1955. *Memorandum of Dissent to the Final Report of the Royal Commission on the Taxation of Profits and Income*. Cmnd 9474, HMSO, London. June

____.1955. *An Expenditure Tax*. London: George Allen and Unwin.

____.1956. 'Alternative Theories of Distribution.' *Review of Economic Studies*. Vol. XXIII, No. 2.

____.1957. 'A Model of Economic Growth.' *Economic Journal*. December.

____.1961. 'Increasing Returns and Economic Progress: A Comment on Prof. Hicks.' *Oxford Economic Papers*. February.

____.1962. 'A New Model of Economic Growth (with J. Mirrlees). *Review of Economic Studies*. Vol.XXIX, No. 3.

____.1966. *Causes of the Slow Rate of Economic Growth of the United Kingdom*.

Cambridge: Cambridge University Press.

_____.1966 . *Strategic Factors in Economic Development*. Ithaca: Cornell University Press.

_____.1970. 'The Case for Regional Policies.' *Scottish Journal of Political Economy*. November.

_____.1972. 'The Irrelevance of Equilibrium Theory.' rep In F.Targetti and A.P. Thirlwall ed. *The Essential Kaldor*. London: Gerald Duckworth.

_____.1975. 'What is wrong with Economic Theory?' *Quarterly Journal of Economics*. August.

_____.1978 (a). *Further Essays on Economic Theory*. London: Duckworth.

_____.1978 (b). *Further Essays on Applied Economics*. London : Duckworth.

_____. [REDACTED]

_____.1984. *Causes of Growth and Stagnation in the World Economy*. Mattioli Lectures : Milan. Pub. In 1996, Cambridge: Cambridge University Press.

_____.1985.*Economics Without Equilibrium*. Okun Lectures, Yale University: University College Cardiff Press.

_____.1989 a. *Further Essays on Economic Policy and Theory and Policy* (Collected Economic Essays, vol.9) , London, Gerald Duckworth; New York, Holmes & Meier.

_____. 1989 b.Targetti. F. and A.P. Thirlwall (ed.). *The Essential Kaldor*. London: Duckworth.

Klant, J. 1984. *The Rules of the Game: The Logical Structure of Economic Theories*. Trans. I. Swart. Cambridge: Cambridge University Press.

Koopmans, T. 1957. *Three Essays on the State of Economic Science*. New York: Mc Graw Hill.

Krueger, Anne O. *et al.* (1991) 'Report on the Commission on Graduate Education in Economics.' *Journal of Economic Literature*, **29**(3), September, pp. 1035-53.

Kuhn, T. S. 1962,(2nd edition 1970). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Lakatos, I. 1970. 'Falsification and the Methodology of Scientific Research Programmes.' In I. Lakatos and A. Musgrave eds. *Criticism and the Growth of knowledge*. Cambridge: Cambridge University Press.

- Lal, D. 1983. *The Poverty of Development Economics*. (Institute of Economic Affairs. London.).
- Latsis, Spiro J. 1976. 'A Research Programme in Economics.' In Spiro J. Latsis ed. *Method and Appraisal in Economics*. Cambridge: Cambridge University Press.
- Laurent, J. and Nightingale, J. (ed.).2001.*Darwinism and Evolutionary Economics*. Cheltenham UK: Edward Elgar.
- Lawson, T., Gabriel Palma and John Sender (eds.) 1989. *Kaldor's Political Economy*. London And San Diego: Academic Press
- Lawson, Tony.1989. 'Abstraction, Tendencies and Stylized Facts: A Realist Approach to Economic Analysis.' *Cambridge Journal of Economics*. Vol. 13.no.1 March, 59-78. Also reprinted in Tony Lawson, Gabriel Palma and John Sender eds. 1989. *Kaldor's Political Economy*. London And San Diego: Academic Press.
- _____.2003.*Reorienting Economics* (London and New York: Routledge).
- Leontief, W. 1971. 'Theoretical Assumptions and non observed Facts.' *American Economic Review*, 61, 1-7.
- Lester. R. 1946. ' Shortcomings of Marginal Analysis for Wage –Employment problems.' *American Economic Review*.36: 62-82.
- Lewis. W. A. 1954. ' Economic Development and Unlimited Supplies of Labor.' *Manchester School*. Rep. in Agarwala and Singh (ed.)*The Economics of Underdevelopment*. Oxford . 1958.
- Lipsey, Richard, G. 2001. 'Successes and Failures in the Transformation of Economics.' *Journal of Economic Methodology*. Vol. 8, no. 2, June, 169-201.
- Maddison, A. 1995. *Monitoring the World Economy, 1820-1992*.Paris: Development Centre of the Organisation for Economic Cooperation and Development.
- Mäki, U. 1992. 'Friedman and Realism.' In *Research in the History of Economic Thought and Methodology*. 10:1-36.
- _____. 1997. ' The One World and Many Theories.' In Andrea Salanti and E. Screpanti ed. *Pluralism in Economics: New Perspectives in History and Methodology*. Aldershot: Edward Elgar. pp.89-99. rep. in Hodgson .(ed.) 2002. *A Modern Reader in Institutional and Evolutionary Economics Key Concepts*. Cheltenham U.K: Edward Elgar.
- McCloskey, D.N. 1983. 'The Rhetoric of Economics.' *Journal of Economic Literature*.

21: 481-517.

Mill, John Stuart. 1900. *Principles Of Political Economy with Some of Their Applications to Social Philosophy*. London: George Routledge & sons.

Myrdal, G. 1958. *Economic Theory and Underdeveloped Regions*. Methuen and Company: London.

.Parker, Richard.1993. 'Can Economists Save Economics?' *The American Prospect*. Vol. 4, no. 13, March 21.

Passinetti, L.G. 1986. ' Nicholas Kaldor: An Appreciation', *Cambridge Journal of Economics*, vol. 10, 301-3.

Patnaik. P. 2000 *A Saint and A Sage: Paul Marlor Sweezy (1910 - 2004)*. A tribute in www.networkideas.org. March 16

Popper, Karl. 1934. transl. as *The Logic of Scientific Discovery*. New York: Basic Books, 1959: Harper Torchbooks, 1968.

_____. 1976. 'The Logic Of Social sciences.' In T. Adorno et al. ed. *The Positivist Dispute in German Sociology*. New York: Harper and Row.

Robinson, J. 1974. 'History versus equilibrium.' *Thames Papers in Political Economy*. Reprinted in *Collected Economic Papers*, Vol. 5 (1979). Oxford: Basil Blackwell.

_____. 1977. 'What are the questions?' in *Journal of Economic Literature* 15(4): 1318-39. Reprinted in *Collected Economic Papers*, Vol. 5 (1979). Oxford: Basil Blackwell.

Robbins, Lionel. 1940. *An Essay on the Nature and Significance of Economic Science*. London: Macmillan.

Romer, P.M. 1986. ' Increasing Returns And Long-run growth.' *Journal of Political Economy*. 94: 1002-37.

_____.1987. ' Growth based on Increasing Returns due to Specialisation.' *American Economic Review*. 77(2): 56-62.

_____. 1990. ' Endogenous Technical Change.' *Journal of Political Economy*.98 (5) part ii: S 71- S 102.

_____. 1991. "Increasing returns and New Developments in the Theory of Growth", in Barnett, W.A., Cornet, B., D'Aspremont, C.,Gabszewicz J., Mas-Colell A. (eds.), *Equilibrium Theory and Applications, Proceedings of the Sixth International Symposium in Economic Theory and Econometrics*. Cambridge University Press.pp.83-110.

_____.1994. "The Origins of Endogenous Growth", *Journal of Economic Perspectives*, vol. 8. No.1, Winter pp.3-22.

Rosenberg. A.1983. "If Economics isn't Science, What is it ?" *ThePhilosophical Forum*. vol. XIV. Nos 3-4.

Rubenstein, Ariel (1991). 'Comments on the Interpretation of Game Theory.'*Econometrica*. Vol. 59,no. 4, 909-24.

Samuelson, P. 1948.'Consumption Theory in Terms of Revealed Preference.'*Economica*, Vol.15 : 243-53.

_____.1949. "International Factor Price Equalisation OnceAgain", *The Economic Journal* June pp.181-197 rep. in J.D. Bhagwati *Selected Readings in International Trade* The MIT Press, Cambridge Massachusetts Second Edition 1987, pp.5-20.

Scitovsky, T. (1954), "Two Concepts of External Economies", *Journal of Political Economy*, Vol. 62, April in Agarwala and Singh (eds.) 1973. pp.295-308.

Snooks, G. D. 1998. *LONGRUN DYNAMICS A General Economic and Political Theory*. New York : St. Martins Press.

Sraffa, P. 1926. 'The Laws of Return under Competitive Conditions.'*Economic Journal*. Vol. 36, 535-50.

_____.1960. *Production of Commodities by Means of Commodities: Prelude to A Critique of Economic Theory*. Cambridge : Cambridge University Press.

Spraos, J. 1980. The Statistical Debate on the Net Barter Terms of Trade between Primary Commodities and Manufactures. *Economic Journal* 90:107-28.

Thirlwall, A.P. 1983. "A Plain Man's Guide to Kaldor's Growth Laws", *Journal of Post-Keynesian Economics*, Vol.4 No.3, Spring, pp.345-358.

_____. 1987. *Nicholas Kaldor*. Grand Master Series in Economics. 1987. Great Britain: Wheatsheaf Books Ltd.

Toner, P.A. (1999), *Main Currents in the Theory of Circular and Cumulative Causation: The Dynamics of Growth and Development*, Macmillan Press: U.K.

Verdoorn, P.J. (1949), "Factors that Determine the Growth of Labour Productivity", *L'industria*.

Ward, Benjamin (1972) *What's Wrong With Economics?* (London: Macmillan).

Weintraub, E.R. 1985. *General Equilibrium Analysis: Structures in Appraisal*.

Cambridge: Cambridge University Press.

_____. 1989. 'Methodology Doesn't Matter, But the History of Thought Might.' *Scandinavian Journal of Economics*. Vol. 91. no.2, 477-93.

Worswick, G. D. N. 1972. 'Is progress in Economic Science possible?' *Economic Journal*. 82, 73-86.

Wood. A. 1987. ' Nicholas Kaldor.' Entry in Eatwell, J. Milgate, M., and Newman, P. (eds.)

Young, A. 1928. 'Increasing Returns and Economic Progress.' *Economic Journal*. VolXXXVIII: 527-542.

