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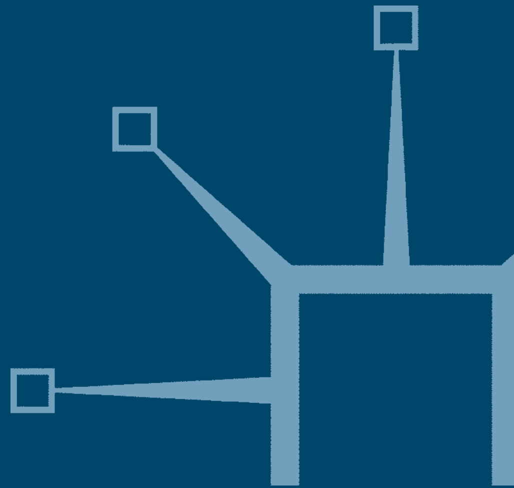

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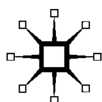

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First published 2009 by
PALGRAVE MACMILLAN

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Palgrave Macmillan in the US is a division of St Martin's Press LLC, 175 Fifth Avenue, New York, NY 10010.

Palgrave Macmillan is the global academic imprint of the above companies and has companies and representatives throughout the world.

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ISBN-13: 978–0–230–21725–6 hardback
ISBN-10: 0–230–21725–7 hardback

This book is printed on paper suitable for recycling and made from fully managed and sustained forest sources. Logging, pulping and manufacturing processes are expected to conform to the environmental regulations of the country of origin.

A catalogue record for this book is available from the British Library.

Library of Congress Cataloging-in-Publication Data

King, J. E. (John Edward)
Nicholas Kaldor / by John E. King.
p. cm.—(Great thinkers in economics series)
Includes bibliographical references and index.
ISBN-13: 978–0–230–21725–6
1. Kaldor, Nicholas, 1908–1986. 2. Economists – Great Britain – Biography. I. Title.

HB103.K36K56 2008
330.092—dc22 2008021221

10 9 8 7 6 5 4 3 2 1
18 17 16 15 14 13 12 11 10 09

Printed and bound in Great Britain by
CPI Antony Rowe, Chippenham and Eastbourne

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1
An Economist from Hungary

1.1 The Kaldor plan to save the world

It is May 1984, and the 75-year old Nicholas Kaldor is at the Bocconi University in Milan, delivering the fifth and last of his Mattioli Lectures on 'causes of growth and stagnation in the world economy'. He has already lectured on 'equilibrium theory and growth theory', 'alternative approaches to growth theory', 'the problems of intersectoral balance' and 'the effect of interregional and international competition'. Now it is time to specify the 'policy implications of the current world situation'.

That situation was not at all favourable. In 1984 the world was in its eleventh year of stagflation, an ugly new word that described an ugly state of affairs: high (and often rising) unemployment combined with high (and often rising) inflation. The long boom that the advanced capitalist world had enjoyed for almost three decades after 1945 ended in 1973, with a commodity price explosion that culminated in massive increases in the price of oil implemented by the producers' cartel, OPEC (Hobsbawm 1994, chapter 14). There was only a brief and faltering recovery from the ensuing global recession before a new downturn began at the end of the decade, made much worse by the new monetarist policies implemented in 1979–1982 by self-proclaimed 'free market' governments, first in Britain under Margaret Thatcher and then by the Reagan administration in the United States (Smithin 1996). Kaldor had long been critical of monetarism as an economic theory. After 1979 all his energies were devoted to attacking it as a political phenomenon.

In the Mattioli lecture Kaldor sets out four broad principles of macroeconomic management. 'The first is coordinated fiscal action including a set of consistent balance of payments targets and "full employment" budgets'. In the absence of an internationally coordinated

fiscal expansion, he argues, trade liberalisation will prove to be 'a serious obstacle to economic recovery' (Kaldor 1996, p. 87). Second, 'it is essential that interest rates should be brought down as rapidly as possible, and by as much as possible'. Third, it is important 'to prevent the great volatility of commodity prices' by means of international buffer stocks, financed by a new world currency. The European countries can initiate such a system, Kaldor suggests, using the existing Common Agricultural Policy and the proto-European currency, the ECU (forerunner of the Euro) (*ibid.*, pp. 87–8). Finally, it is vital to overcome 'the tendency to chronic inflation under full employment conditions, due to the system of settling wages by sectoral (or industrial) collective bargaining' (*ibid.*, p. 88). Monetary and fiscal restrictions on aggregate demand have simply generated unemployment, with little or no effect on cost inflation. The only solution, Kaldor concludes, is an incomes policy: 'a system of continuous consultation between the social partners – workers, management and the Government – in order to arrive at a social consensus concerning the distribution of the national income that is considered fair and which is consistent with the maintenance of economic growth, reasonably full employment and monetary stability' (*ibid.*, p. 90). Budget deficits, cheap money, commodity price stabilisation and incomes policy: Kaldor is evidently swimming against a very strong neoliberal tide.

The Raffaele Mattioli Lectures were established by the Banca Commerciale Italiana to honour the memory of its eponymous manager and chairman. Kaldor had been a personal friend of Mattioli since they met in Geneva, where he spent two years in the late 1940s working for the United Nations Economic Commission for Europe. His lectures summarised the work of an unusually long and productive academic life that began over half a century earlier with his first published work in economic theory. Kaldor's distinctive and unconventional views on economic policy were derived from a penetrating critique of mainstream economics, which he had dismissed in the earlier lectures as 'absurd' (*ibid.*, p. 7) but which was nevertheless

> expressed with a phoney kind of precision or 'scientism' of a most pretentious kind, using highly sophisticated, mathematical techniques for proving propositions which have no interpretative value of real-world phenomena, for the simple reason that they are based on *a priori* axioms which have no relation to the conditions which can be empirically observed. All this is aggravated, not helped, by the use of mathematics. (*ibid.*, p. 21)

He concluded that there had been since 1945 'a retrogression in comparison with the great innovation period of the 1930s' (p. 5), when John Maynard Keynes had begun to develop a genuinely scientific macroeconomics that was intended to illuminate the fundamental economic problems of the real world and to offer practical solutions to them.

One of Keynes's greatest – though by no means uncritical – disciples, Nicholas Kaldor died in Cambridge on 30 September 1986, a little more than two years after he delivered the Mattioli lectures. It took another decade for them to be published by Cambridge University Press, in a handsome volume where they were supplemented by A. P. Thirlwall's fine obituary and Ferdinando Targetti's comprehensive bibliography of his work. They form an impressive memorial to one of the twentieth century's most original, provocative and unorthodox economists.

1.2 Budapest, Berlin, London and Cambridge

Nicholas (Miklos) Kaldor was born in Budapest on 12 May 1908 into a prosperous middle-class Jewish family.[1] His father Julius was a lawyer, and the young Nicholas at first expected that he, too, would enter the profession, 'though I had a sneaking desire to become a writer'. He was the only boy in a prosperous household, two brothers having died in infancy before he was born. 'The deep mark of this privileged childhood', his pupil and close friend Luigi Pasinetti reported, 'has remained in Kaldor's attitude and demeanour during the whole of his life; it may help to explain his exuberant, egocentric, and undisciplined character' (Pasinetti 1983, p. 335). Kaldor himself recalled that growing up in Hungary during and immediately after the First World War,

> with its bewildering changes in social régimes from a monarchy to a liberal republic, then to a communist dictatorship lasting for four months, followed by a military dictatorship soon moderated by the need to conform to the institutional framework of a parliamentary system desired by the victorious Western powers, made me interested in the forces which govern the political evolution of society. (Kaldor 1986a, p. 11)

His interest in economics was further aroused by first-hand experience of the great German hyperinflation during a family holiday in the Bavarian Alps in the summer of 1923. By this time he was a student at the elite Model Gymnasium in Budapest, so called because it was the

state school where teachers were trained and syllabuses for other schools were worked out. Notable pupils from Kaldor's generation or slightly earlier included the physicists Eugene Wigner (born in 1902; Nobel laureate in 1963), Nicholas Kurti (born in 1908, the same year as Kaldor himself) and Edward Teller (also born in 1908); and the historian Michael Polanyi (born 1891). There was also Thomas Balogh (born in 1905, and so three years older than Kaldor) who also moved to England, enjoyed a distinguished career as an economist, and in the 1960s became a controversial public figure as adviser to the Labour government led by Harold Wilson.

In 1925 Kaldor persuaded his father to let him enrol as an economics student at the Humboldt University in Berlin. Here he attended lectures by Kurt Schumacher, the father of E. F. 'Fritz' Schumacher, with whom he would collaborate in 1943 as technical adviser to Sir William Beveridge on the establishment of full employment in postwar Britain,[2] and the former Marxist Werner Sombart, by this time a very conservative German nationalist. Kaldor was not particularly impressed by the quality of the teaching. 'To be at university in Germany', he told Cristina Marcuzzo half a century later, 'meant that you wandered from one lecture to another, without any discipline and without having a clear programme of courses to follow'. He spent much of his time in Berlin working as a journalist (using his fluency in German) to supplement his father's allowance:

> I was 18. I became an accredited foreign correspondent for a Hungarian newspaper and so I was able to go everywhere, in situations that wouldn't normally be accessible to a first-year student. Every Friday afternoon the Foreign Minister, who at that time was [Gustav] Stresemann, invited all accredited foreign journalists to tea with him at the Ministry. I remember also having an official pass for the Reichstag. I talked to politicians of all parties and conducted a lot of interviews, but I didn't do a lot of studying. (Kaldor 1986b, p. 29)

He soon realised that he needed the discipline of a systematic course of study if he was to make any progress with his economics. This was to be found in England, not Germany.

So, in the spring of 1927, Kaldor left for London, intending to spend one term at the London School of Economics (LSE). He decided to stay on, and in October 1927 he formally enrolled as an undergraduate; when he resigned, twenty years later, it was as Reader in Economics.[3] At the LSE he came under the influence first of the idiosyncratic American

Marshallian, Allyn A.Young, and then of Young's charismatic successor to the chair of Economics, Lionel Robbins.[4] Young, who had arrived from Harvard in 1926, taught at LSE for less than three years, succumbing unexpectedly to pneumonia in the winter of 1928–1929. 'Nonetheless', Kaldor recalled,

> his lectures and seminars left a lasting impression on my later development: 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 are intended to illuminate. (Kaldor 1980a, p. viii)

Young's most influential publication was his article on increasing returns and economic progress (Young 1928), which 'created a considerable stir on its publication, even though its main message was by no means fully understood at the time' (*ibid*., p. viii n2). Kaldor drew heavily on Young in one of his own earliest and most original papers (Kaldor 1934a),[5] and returned to the implications of increasing returns over and over again in the final two decades of his life. As he wrote in 1977, Young

> showed that the main function of markets is to transmit impulses to economic change, and thereby *create* more resources through enlarging the scope for specialisation and the division of labour – rather than to secure an optimum allocation of a *given* quantity of resources. And he also showed that with increasing returns continuing change is self-generated and 'propagates itself in a cumulative way'.[6] Hence no analysis which describes the forces operating on the economy as tending towards a state of equilibrium can capture the manner in which the development of markets make[s] for perpetual change. (Kaldor 1978c, p. xxv)

Kaldor's final, decisive break with equilibrium economics owed a great deal to the long-term influence of his first real teacher (see Chapter 8). What he made of Young at the time can be ascertained from the notes that he took at his 1928–1929 LSE lectures (Kaldor 1990).[7]

Young's most important lessons were lost on his successor, Lionel Robbins, who was a firm advocate of neoclassical economic theory. Robbins, though, was

> young, flamboyant and enthusiastic (he was only 30 at the time of his appointment) and extremely devoted both to teaching and to

> economics as a subject. He lavished his energies and vitality on his pupils and identified himself fully with their successes and their attainments. It was inevitable that those of us who were fortunate to have been among his first pupils – and there were a bare dozen of us then specialising at L.S.E. in the subject of 'analytical economics' – should fall completely under his spell. (Kaldor 1980a, pp. viii–ix)

Unlike Young, Robbins was a Walrasian rather than a Marshallian, having absorbed the Austrian variant of general equilibrium theory during his time in Vienna under Ludwig von Mises.[8] As first a student and then a junior colleague of Robbins, Kaldor also obtained a 'thorough grasp' of the marginal productivity theory of distribution 'in its generalised form, as expounded by Wicksell and Wicksteed'. Robbins believed in the theory 'with the fervency of a convert and propounded it with the zeal of a missionary', so that the young Kaldor absorbed it 'without being hampered by doubts and hesitations – which in other circumstances might have inhibited me (as it has inhibited other critics) from mounting the intellectual effort required for mastering its content' (*ibid.*, p. ix).

At the beginning of the 1930s Kaldor was almost midway through the third and final year of his degree. He was to pass with flying colours (except in Statistics and Scientific Method, which was his worst subject). Enrolled as a research student in 1930, he was appointed to an Assistant Lectureship in 1932 and, belatedly, promoted in 1938 to full Lecturer at the School.[9] The 'School Notes' section of *Economica* documents the progress of Kaldor's career. In autumn 1930 he was awarded a Research Studentship in Economics and Political Science to the value of £200 per year, plus fees. On 1 August 1931 he became 'Assistant' in Economics and two years later he and two colleagues were 'recognised as teachers of the University'. By this time he had come under the influence of another major economic theorist, the Austrian Friedrich von Hayek, who had been attracted to the LSE by Robbins in 1931. Politically, Hayek was an unreconstructed nineteenth-century liberal; in economics, he was at this time a convinced neoclassical of the Walras-Wicksell variety.[10] Briefly, in the early 1930s, his ideas had a profound impact on Kaldor, as we shall see in Chapter 2.

For some years Kaldor was very happy in London. He taught a wide range of theoretical and applied courses, often (but not always) connected to his research interests. Before 1939 they included economic theory, the theory of production, capital and interest, advanced problems of economic theory, the theory of tariff-making and public finance and

the trade cycle. When the LSE was evacuated to Cambridge during the War he also taught courses on international trade and foreign exchange, outlines of economic dynamics, problems of war economics, and (at the instigation of Keynes) the theory of distribution (Thirlwall 1987, pp. 32, 77). 'For students of the present generation', he wrote in 1979, 'it is difficult to convey the atmosphere of creative tension and excitement which prevailed at L.S.E. in the early years of the 1930s'. In addition to Robbins and Hayek there were several brilliant young graduates on the staff. Together they were trying to confront 'the intellectual challenge which the severity of the economic crisis (particularly in 1931–32) presented to all economists. It was a time of endless discussions, which went on at all hours of the day and night – during meals, during walks and during weekends' (Kaldor 1980a, p.xi). Among his closest friends at the time were Maurice Allen, then a socialist but later a very conservative Executive Director of the Bank of England, and the future Nobel laureate John Hicks. Other young colleagues included Thomas Balogh (from the Model Gymnasium in Budapest), the development economist Paul Rosenstein-Rodan and yet another brilliant Hungarian economist, Tibor Scitovsky.

On summer holidays in Budapest Kaldor began a friendship with John von Neumann, whom he remembered meeting for the first time over lunch in the terrace at the Ritz (Kaldor 1986b, p. 43). He introduced von Neumann to the economics of Wicksell, and often discussed economics with him, subsequently arranging for von Neumann's important paper on the theory of economic growth to be translated and published in the *Review of Economic Studies* (von Neumann 1945–1946). 'In retrospect', Kaldor thought, 'Johnny was more interested in asking questions than in answering them, but he was a very good raconteur and delighted in gossip' (Kaldor 1989b, p. vii). He had more serious conversations with Hicks, who introduced Kaldor to the Swedish school of monetary macroeconomics and 'first made me realise the shortcomings of the "monetarist" approach of the Austrian school of Mises and Hayek and made me such an easy convert to Keynes after the appearance of the *General Theory* three years later' (*ibid.*, p. xi). Kaldor's conversion to Keynes, and his more or less simultaneous return to the socialist beliefs of his youth, strained his relationships with both Hayek and Robbins to breaking-point, as will be described in Chapters 2 and 3.

The progress of his subsequent career will be discussed in later chapters. For the present, it is enough to highlight some of the principal landmarks. Kaldor resigned from LSE in 1947 to take up an appointment as Research Director of the Planning Department of the United

Nations Economic Commission for Europe under the great Swedish economist Gunnar Myrdal, in Geneva. This was where he met Raffaele Mattioli. With a team that included Tibor Barna, Robert Neild and P. J. Verdoorn, he was responsible for writing the Commission's surveys of the economic situation and prospects for Europe in 1947 and 1948, and also co-authored (and largely wrote) a report for the United Nations on national and international measures to promote full employment. Kaldor returned to academic life in the autumn of 1949 as Fellow of King's College, Cambridge, a position that he held until his death. At the same time he was appointed to a University lectureship; he was promoted to Reader in 1951 and (belatedly) awarded a personal chair in 1966. His teaching was rather more narrowly focused than it had been at the LSE. In his lectures Kaldor concentrated on the theory of value and distribution, economic dynamics and (beginning in 1952) the economics of growth. He also gave classes on economic theory and on current economic problems (Thirlwall 1987, p. 111).

He became a great traveller, visiting India, China, Japan, Chile and the United States on sabbatical leave in 1956, and later serving as a tax adviser to seven governments on three continents. Kaldor retired from his Cambridge chair in 1975, as in those days a 67-year old was required to do, but retained his college Fellowship. If anything he was even busier in retirement than he had been before, travelling ceaselessly, speaking at countless seminars and conferences, publishing five or six articles a year, writing new introductions to several volumes of his collected essays and (from 1979) relentlessly attacking the follies of Thatcherism from the Labour benches in the House of Lords.

1.3 Public figure and private citizen

Kaldor had never feared controversy. During his spell as advisor to the Wilson governments in the 1960s and 1970s he was constantly in the public eye, frequently being denounced by Conservative commentators for his supposedly dangerous and radical policy proposals. The business lobby group Aims of Industry devoted a pamphlet to attacking him and his fellow Hungarian, the acerbic Thomas Balogh. It denounced Kaldor's 'tax preoccupation', which had led him to recommend that the Indian government introduce a personal expenditure tax with a top marginal rate of '*three hundred* per cent' (George and Bewlay 1964, p. 3; original stress). Not surprisingly this proved highly unpopular, as did his advice to the 'socialist-minded governments' of Ceylon and Ghana 'on means of raising taxation to cover mounting deficits from heavy State spending'.

In both cases, 'riots over taxation ... followed in Mr. Kaldor's wake' (*ibid*., p. 5).[11] These were dangerous men, the pamphlet concluded, and their influence over Labour was pernicious. 'In a party largely noted for its doctrinaire approach the influence of the intellectual can be paramount. It is quite clear where the important influences of Messrs Kaldor and Balogh would lead: to punitive taxation; to nationalization; to a plethora of planning and licences and controls' (*ibid*., p. 8). This was a caricature, both of Kaldor and of the undoctrinaire and notoriously anti-intellectual British Labour Party, but in business circles it seems to have been widely believed. With the election of the Wilson Labour government in October 1964, Balogh and Kaldor became names to frighten the (Conservative) children with.

My former colleague Lazlo Csapo (himself from Budapest) used to tell a story about the 1986 Reykjavik summit conference, where the US President Ronald Reagan and the Soviet leader Mikhail Gorbachev got on so well that there was a real danger that the second Cold War would come to an end and peace break out. Earlier in the proceedings, however, there was a great deal of mutual suspicion, especially on the American side. Reagan was accompanied to every meeting by a team of stocky bodyguards in dark glasses, and one day Gorbachev was alarmed to see them carrying a huge and obviously very heavy suitcase. 'Mr. President', he asked, 'What *is* this?' Reagan looked at him rather sheepishly. 'Mr. Secretary', he said, 'I am obliged to inform you that my advisers fear for my safety at these talks. I must therefore warn you that this suitcase contains a Doomsday weapon. In the event of an attempt on my life it will be detonated, leading inexorably to the destruction of all life on the planet'.[12] Gorbachev sat silently for a minute and then made some mollifying remarks before the day's negotiations commenced. Next day it was Reagan's turn to be surprised. The KGB thugs who shadowed Gorbachev were now struggling under the weight of a truly massive suitcase of their own, much larger and heavier than the American one. 'Mikhail, Mikhail', Reagan began. The Soviet leader cut him short. 'My advisers are also concerned for my personal security', he told the President. 'It is my duty to inform you that inside this suitcase are two Hungarian economists ... '.

The twin harbingers of economic doom, satirised in *Private Eye* as Baldor and Kalogh, were also known as Buda and Pest. The origins of this long-standing joke are obscure. They probably date back to Kaldor's time at LSE, where he worked with the future Labour Chancellor of the Exchequer, Hugh Dalton. There is a 1950 reference to the pair of nicknames in Dalton's diary, and the memoirs of a later Chancellor, Dennis

Healey, include a mention of 'the terrible twins from Hungary, Nicky Kaldor and Tommy Balogh, whom Hugh Dalton used to describe with some justice as Buddha and Pest' (Healey 1989, p. 368). As Hayek recalled, 'nobody doubted who was who. Balogh was Pest. Kaldor was fat and seemed to look like Buddha, and Balogh was so universally disliked that he was Pest' (Kriesge and Wenar 1994, p. 87). Early photographs of Kaldor suggest that Hayek was exaggerating his girth, but in later life he enjoyed the pleasures of the table enough to put on a lot of weight. The Australian economist Peter Kriesler, a graduate student in Cambridge in the early 1980s (and himself the son of Hungarian parents), reports that Kaldor was known in student circles as 'the Hungarian gross domestic product'.

Even on the left, the terrible twins were not universally popular. In 1965 the Labour politician George Wigg went well beyond the mildly xenophobic in describing Kaldor (in a private conversation) as a Hungarian traitor.[13] Within the Labour Party Kaldor had the last laugh, returning to Whitehall in 1974 as adviser to the Wilson and (briefly) Callaghan governments, while Wigg sank into richly deserved obscurity. It was a different story in the wider world, where the egalitarian Keynesianism that Kaldor espoused was abandoned in favour of a return to a savage pre-Keynesian monetarism that he detested, especially when it was taken up by the Labour Chancellor, Healey. As in politics, so in academia: in the final decade of his life Kaldor moved effortlessly from insider to outsider, from mainstream to heterodoxy, from centre to left, simply by standing still. In this respect he reminds me of another dissident economist about whom I have written, the American, Sidney Weintraub (1914–1983), who began his career with a flurry of articles in all the best academic journals and ended it an outcast (King 2008). Kaldor was a much better theorist than Weintraub, but their professional lives took a course that was in some ways strikingly similar.

At the height of his public notoriety, just a year after the publication of the Aims of Industry exposé, a much more friendly account (only slightly spoiled by a sensational headline) came in a *Sunday Times* profile of 'Nicholas Kaldor, the tax iconoclast in the Budget team'. 'In Cambridge', the journalist Godfrey Smith reported, 'there is a group called the "two-car socialists" and the Kaldors are in it: he runs a Peugeot, she a Morris 1100'. In 1965 this was clear evidence of great wealth. 'The days of his long country walks with Cambridge economists Joan Robinson and Richard Kahn seem to have ended. Instead he does the Canadian Air Force exercises'. Neither Kaldor nor Thomas Balogh had been very effective in Whitehall, Smith concluded, and he ended

his profile by quoting Balogh on the 'reverse harlot phenomenon' – economic advisers had great responsibility but very little power (Smith 1965, p. 49). Kaldor had married Clarisse Goldschmidt in 1934, the same year in which he became a British citizen. The *Sunday Times* reported that Clarisse was a Labour councillor in Cambridge and a member of the Campaign for Nuclear Disarmament. This placed her somewhat to the left of her husband, who was solidly in the Labour mainstream until revulsion at Margaret Thatcher and the dangerous Cold War rhetoric of Ronald Reagan pushed him further to the left in the final years of his life.[14] The Kaldors had four daughters, two of whom had distinguished academic careers of their own, one (Frances Stewart) as a development economist and the other (Mary Kaldor) as an international relations specialist.

Kaldor's happy home life was not seriously impaired by his forgetfulness, which, like his driving, impressed so many of his friends. Dalton's diary reports a characteristic incident in March 1951:

> Dined in Hall in King's with Kaldor. Oh, but against that tremendous background of my old college, physical, spiritual and memorial, he seems an in-comer – a small, slightly displaced person! He had first muddled the weekend, and then, this Saturday morning I went to his room, high in Gibbs Building, full of memories, and he arrived late, sweating, breathless, untidy, only just in time for Hall. (Dalton 1986, p. 509)

John Kenneth Galbraith described Kaldor as 'not only one of the most noted but also one of the most absent-minded of British economists' (Galbraith 1981, p. 327). They met again in Calcutta in 1956:

> It was the time in China when the hundred flowers were about to bloom;[15] Nicky and Clarisse, to the openly expressed envy of everyone at the Institute, were on their way to Peking. Then he would return to India. After a bountiful dinner and warm farewells, the Kaldors departed one night for Dum Dum Airport and China. Two hours later they were back; Nicky had forgotten the passports. (*ibid.*, p. 328)

Earlier that year the two families had travelled together in the north of India. 'Nicky had become an enthusiastic photographer, but his work was impaired because, as his daughter explained, he never remembered to remove the cap from the lens' (*ibid.*, p. 335 n8).[16] Matters did not

improve as the Kaldors proceeded eastwards. My favourite document in the Kaldor Papers at King's College, Cambridge is a letter from the President and General Manager of the Imperial Hotel in Tokyo, dated 12 June 1956: 'Your letter of June 5th is before me and it is my great pleasure to advise you that we have found your dentures. We had them sent yesterday by air parcel and it is my every hope that you will receive them in good condition'.[17] There was, alas, to be no happy ending to this story. Two months later, with Kaldor now settled at Stanford University in California, the missing dentures had still not arrived.[18]

Four years earlier Nicholas and Clarisse had taken their newly married friends Tony and Hilary Crosland on a motoring tour of southern Spain. It was a strange way for the Croslands to spend their honeymoon, not least because of Kaldor's insistence on doing the driving:

> Nicky's driving unsettled everyone else's nerves. He gripped the steering wheel like Toad of Toad Hall,[19] but – unlike Toad – Nicky talked as the motor car plunged along, as often as not his eyes fixed on the face of the person beside him. Meanwhile his passengers stared ahead, mesmerised, wondering how they would avoid the next oncoming lorry. (Crosland 1982, p. 60)

Kaldor believed that his friendship with Crosland was never quite the same after this trip. His absentmindedness almost got him into more serious trouble just after the Labour Party's election victory in October 1964, as Susan Crosland reported: 'Nicky Kaldor got through on the telephone, his voice melancholy. He'd just been stitched up in hospital following a motor accident. His car had overturned. His mind had been on devaluation instead of the fact that he was about to drive across a one-way thoroughfare' (*ibid.*, p. 128). This time he did have an excuse, having been engaged in frantic lobbying of the new government whose decision not to devalue the pound, he believed, would have disastrous consequences.[20]

1.4 Sixty years of economics

The book deals with Kaldor's intellectual achievements in broadly, but not slavishly, chronological order. Chapter 2 outlines his work in the 1930s, when he progressed from undergraduate student to become one of the world's leading young economic theorists. In Chapter 3 his transformation during the Second World War into an applied economist and policy analyst is described, while his return to theory in the 1950s is the

subject of Chapter 4. By the early 1960s Kaldor had become one of the profession's great generalists, writing on a wide variety of theoretical, applied and policy issues in the context both of the advanced capitalist countries and the under-developed regions of what was then known as the Third World.[21] Strict chronological order becomes impossible at this point, and so I have chosen to discuss Kaldor's work as a policy adviser to the British Labour governments of Harold Wilson and James Callaghan in Chapter 5, which focuses on the period 1964–1979 but necessarily deals also with his writings on economic policy between 1945 and 1964. Chapter 6 is devoted to his work on development economics, which began in 1956 and continued until his death thirty years later. In this chapter I also assess Kaldor's 'North-South model' of global growth and stagnation, which contained his interpretation of the post-1973 stagflation. Then, in Chapter 7, I explore his critique of monetarism, beginning with his earliest views on monetary policy and concluding with his attack on the macroeconomics of Margaret Thatcher. Chapter 8 returns to more abstract themes, setting out the reasons for Kaldor's rejection of equilibrium theorising and the methodology that underpinned it. These two chapters rely heavily, but by no means exclusively, on his extensive published work in the 1980s. In Chapter 9, I explain why I think that Kaldor was important in his time, and remains important in ours.

As far as possible I have tried to let Kaldor speak for himself, quoting extensively from his prolific writings. He was a natural linguist, blessed with the rare ability to write perfect prose in his second and third languages (he was effectively trilingual in Hungarian, German and English, and seems to have had at least a good reading knowledge of French and Italian). His written English was clear, concise and accurate, if not always particularly elegant (Dorfman 1961, p. 495). The blemishes – the repeated words, the occasional clumsiness – reflected a failure of self-editing, understandable with someone as constantly busy as Kaldor must have been. His obituarist Geoff Harcourt sees this as symptomatic of a deeper failing: 'after the war Kaldor himself became less painstaking and careful in his arguments and presentations, preferring to sketch outlines and let others fill in the details and provide rigorous coherent arguments. (Before the war he was much more careful, revising many times before he submitted papers for publication)' (Harcourt 1988, p. 160). In compensation, his later writing was more forthright, vigorous and occasionally very funny. I have drawn heavily on the introductions that Kaldor wrote to the eight volumes of the *Collected Essays* that were published in his lifetime.[22] As a sort of

compensation for the treatise that he never felt able to attempt (Kaldor 1978a, p. xxix), he wrote some 150 pages of reflection, retrospection and sometimes harsh self-criticism that I have found invaluable as a guide to his large and complex body of work.

A Reader's Guide to Kaldor would commence with *The Essential Kaldor*, a selection of his best papers chosen by his first and second biographers (Targetti and Thirlwall 1989). The eight volumes of his *Collected Essays* that were published in his lifetime are indispensable, in particular (as I have already indicated) the extensive introductions that he wrote to each of them, which contain a lot of autobiographical material in addition to Kaldor's reflections on theoretical and policy issues. His (brief) reminiscences were published just before his death (Kaldor 1986a, 1988a). Much more detail is provided in Cristina Marcuzzo's extended interviews, which were unfortunately published only in an Italian translation (Kaldor 1986b). As already noted, a copy of the original English transcript is in the Kaldor papers at King's College, Cambridge.[23] The two early biographies by Thirlwall (1987) and Targetti (1992) are both much longer than the present volume, and offer a great deal of more information about many aspects of his life and work than I have been able to provide here. Targetti's also contains a comprehensive Kaldor bibliography, complete at least so far as academic books and articles are concerned (only the long-delayed Mattioli lectures are missing from it).[24] Turner (1993) is a rather less satisfactory biography. Finally, there are the two bulky *Festschrift* volumes that appeared shortly after Kaldor's death, containing a wide range of tributes and critical appraisals of his work (Lawson, Palma and Sender 1989; Nell and Semmler 1991); the former was published simultaneously as a special issue of the *Cambridge Journal of Economics*.

Although I had read several of Kaldor's best articles as an undergraduate, beginning with the classic 'Alternative Theories of Distribution' (Kaldor 1956a), it was not until the 1980s that I first began to think seriously about Post Keynesian economics more generally. This was at the prompting of Mike Howard, who persuaded me that Michał Kalecki formed an essential bridge between Marx and Keynes. In 1992–1993 I interviewed Post Keynesians in several countries, supported by a grant from the Australian Research Council, and several of them spoke at length about Kaldor and his work (King 1995a). I am especially grateful to Geoff Harcourt, Peter Kriesler, Kurt Rothschild and (in particular) Tony Thirlwall for sharing their knowledge of Kaldor with me. I am still benefiting from the research assistance provided by Rosemary Moore and Julie Rowe, who were paid a pittance out of the ARC grant. Edward

Elgar encouraged me to put together an edited volume of articles on Kaldorian growth themes (King 1994), which made me do a lot more reading.

Others whose assistance I must acknowledge include Philip Arestis, Paul Davidson, Mary Kaldor, John McCombie, Michael McLure, Basil Moore, Luigi Pasinetti and Malcolm Sawyer, none of whom, of course, is to be held responsible for any errors or opinions in this book. Librarians at La Trobe University, the University of Leeds, the University of Lancaster and the University of Waterloo, together with archivists at King's College, Cambridge and the LSE, have also been of great assistance. An early version of Chapter 2 was presented at the 19th conference of the History of Economic Thought Society of Australia in Ballarat in July 2006, and subsequently published in the Society's journal, *History of Economics Review* (No. 46, Summer 2007, pp. 39–61). An early version of Chapter 3 was presented at the December 2006 conference of the Society of Heterodox Economists at the University of New South Wales, and in March 2007 at a graduate seminar at Monash University; it was published as Discussion Paper No. 25/07 by the Department of Economics at Monash.

Study leave from La Trobe University allowed me to finish the book while a guest of the University of Graz, and I must conclude by thanking Christian Gehrke and Heinz Kurz for their hospitality in that most beautiful of Austrian cities.

2
Not the Devil's Decade

2.1 Macroeconomics, capital and prices

The Anglo-Irish journalist Claud Cockburn gave his memoir of the 1930s the title of *The Devil's Decade* (Cockburn 1973). As a summary description of the state of the world in the 1930s this is hard to gainsay, but it cannot be applied to Nicholas Kaldor, who began the decade as an undergraduate student and ended it as one of the world's leading young economic theorists. He was always a prolific writer. In Berlin, at the tender age of 19, he had worked as a stringer for the Budapest press, a practice he continued for a while after his move to London.[1] By 1931 he was addressing dense three-page missives to John Maynard Keynes querying details of the argument in the *Treatise on Money* and eliciting a courteous, if rather frustrated, reply (Keynes 1987, pp. 238–42). Kaldor was also co-translator of Friedrich von Hayek's extended critique of underconsumption theory, which was published in the LSE house journal, *Economica*, while still a research student (Hayek 1931). His own professional publications began in 1932 with a 12-page article on the Austrian economic crisis that Keynes had rejected for the *Economic Journal* but Kaldor managed to place in the *Harvard Business Review* (1932c). Over the next eight years he published 22 journal articles, 8 substantial book reviews and a translated book (again by Hayek).[2] His interests covered the entire gamut of economic theory and – to a more limited extent, in the 1930s – of economic philosophy and policy. Four of Kaldor's articles from this first decade of his academic career stand out from the rest, and I shall devote a section to each of them. These are his 1934 paper on the nature of equilibrium theorising; two pieces from 1939, on the compensation principle in welfare economics and on money, finance and the consequences of speculative behaviour;

and the 1940 paper setting out his Keynesian model of the trade cycle.

First, though, something must be said about his other writings in the 1930s. The Hayek translations are important if only because they gave Kaldor the opportunity to familiarise himself with the Austrian's ideas. Hayek had been appointed in 1931 to the Tooke chair at the LSE, partly on Kaldor's initiative (Targetti 1992, p. 3), and together with Robbins he was to dominate the teaching of economics there for almost twenty years. Although Kaldor was later to claim that the work of translation also awakened him to many of Hayek's theoretical errors (Kaldor 1986a, p. 30 n7), there is no indication of this in any of his own publications in the early 1930s, which are essentially liberal, Austrian and (by implication) pre-Keynesian in character. He had in fact fallen, briefly, under Hayek's spell. Thus he criticised the German socialist Emil Lederer for claiming that technical progress was responsible for a permanent increase in unemployment, arguing that this is possible only if money wages are assumed to be rigid downwards. Lederer's case, he concluded, 'sounds very strange to people who were brought up on the marginal productivity analysis' (Kaldor 1932a, p. 190). Unemployment was due not to technical change but to 'monopolistic interference with the price system', in this case – presumably – by trade unions (*ibid*., p. 195).[3]

Reviewing Carl Landauer's *Planned Economy and Market Economy*, Kaldor invoked Ludwig von Mises's criticism of socialist economics:

> Even if we assume that a 'free market' for consumption goods can be preserved, the methods of producing these goods will have to be decided arbitrarily; as the Socialist producer cannot, even if he tried to, find out the true displacement [i.e., opportunity] costs of the factors of production. This problem, which emerged as soon as the conception of 'real costs' was abandoned, has so far proved insoluble. (Kaldor 1932b, p. 279)

It was his reading of the *General Theory*, and discussions with young LSE colleagues like Maurice Allen, Abba Lerner and his then very close friend John Hicks, that finally convinced Kaldor of the defects of Austrian macroeconomics. At about the same time, in the mid-1930s, he returned to the moderate Fabian socialist beliefs that he was to hold for the rest of his life.[4]

In later life Kaldor read little and rarely reviewed books, but in the 1930s he was a very active reviewer, in particular for *Economica*. His knowledge of German made him an obvious choice to review books in

that language, and this allowed him to assimilate the latest developments in economic thought in both Germany (before Hitler came to power in May 1933) and Austria (where academic freedom survived largely intact until the *Anschluss* in March 1938). In addition to his reviews of Landauer and Lederer, already noted, he also reviewed Erich Schneider's *Theory of Production*, which failed to convince him of the merits of a mathematical approach to economic analysis,[5] and H. von Stackelberg's *Market Form and Equilibrium*, which impressed him greatly (Kaldor 1936a, 1936b).

There was another important Austrian connection. Kaldor's original topic for his PhD dissertation was 'Commercial Policy of the Danubian States after the War'.[6] Although he never submitted a thesis he did publish a substantial article on the economic crisis in Austria, as already noted. Drawing a very long bow it is just possible to detect in this paper an early version of Hyman Minsky's theory of financial instability (Minsky 1986), in which the operating losses of bank-owned industrial enterprises in Austria forced them into a 'vicious circle' of Ponzi finance, borrowing repeatedly to cover their commitments from previous loans.[7] For the most part, however, Kaldor's interpretation followed the conventional 'sound finance' critique of Austrian financial policy: both the government and the population were living beyond their means, and the day of reckoning had eventually arrived.

In 1935 Kaldor was awarded a Rockefeller Scholarship. He was in very good company, since Oskar Lange and Nicholas Georgescu-Roegen were the other European recipients that year. This permitted him to travel widely in the United States in 1935–1936. He visited Columbia, Harvard, Chicago and the University of California, presenting a paper on wage subsidies as a remedy for unemployment at the 1935 meeting of the Econometric Society in New York (Kaldor 1936c, p. 271 n1), and also attending the 1936 meeting of the Society in Colorado Springs. Never again would Kaldor spend so much time in the United States, or feel so close to prominent American economists. One product of his Scholarship was a commission to write the 1937 'Annual Survey of Economic Theory' for the Society's journal, *Econometrica* (Kaldor 1937b). For the most part this was an intense, respectful and rather uninteresting account of the analytical differences between Hayek and the Chicago capital theorist Frank Knight,[8] but there was a sting in the tail. In the final two pages of the survey Kaldor slid his stiletto into Austrian capital theory, silently and without warning but with fatal effect:

> The purpose of the 'investment period' approach is to reduce the production function to two variables, substituting 'waiting' for the

> services of all produced (or variable) factors, with interest as the price of 'waiting'. In this way – and only in this way – can *capital as capital* be treated as a factor of production, commensurate with 'labour'. (*ibid.*, p. 232; original stress)

This, however, could be done only if both the fixed factors and the final output were themselves homogeneous.[9] In general, Kaldor concluded, the 'degree of roundaboutness' is not a useful concept, even under static conditions. Once capital accumulation came into consideration, the Austrian theory was in even deeper trouble: 'There can be no doubt that for an analysis of dynamic problems – and especially of the *par excellence* dynamic problem of the trade cycle – the investment-period concept could hardly be of any use' (*ibid.*, p. 233; cf. 1939a, 1940a). Twenty-five years later, irritated by Joan Robinson's apparent belief that criticism of neoclassical capital theory had originated in Cambridge, Kaldor would claim that it was, on the contrary, born at the LSE (King 1998b, p. 428).

His early contacts with Cambridge economists, and their influence over his thinking on macroeconomics, will be touched on in Sections 2.3 and 2.4. In the 1930s he also wrote frequently on microeconomics and in particular on another topic close to Cambridge hearts, the theory of the firm. His extended review of Joan Robinson's *Economics of Imperfect Competition*, written before he had met Robinson,[10] was both complimentary and critical. She had neglected oligopolistic interdependence between producers, Kaldor complained, and had little to say about selling costs. 'Under the circumstances it is not surprising that her book...inevitably becomes a treatise on monopoly' (Kaldor 1934c, p. 335). Although he doubted whether monopsonistic exploitation of labour was still of practical significance, given the strength of the British trade unions, he praised Robinson's treatment of the labour market: 'Of all Mrs. Robinson's results, unquestionably the most valuable are to be found in Books VII–IX, which deal with the extension of the marginal-productivity theory of distribution to monopoloid situations' (*ibid.*, p. 337). When she returned to *Imperfect Competition*, writing a new introduction to the second edition in 1969, Robinson and Kaldor were barely on speaking terms, and she did not refer to his review, but the principal weaknesses that she now acknowledged were precisely those that had been identified by Kaldor thirty years earlier. She now admitted that her treatment of oligopoly involved 'a shameless fudge', while she remained proud of her analysis of wages under imperfect competition, 'what for me was the main point' (Robinson 1969, pp. vi, xii).

Kaldor also took on Robinson's great rival, the Harvard theorist of 'monopolistic competition', Edward H. Chamberlin, in articles on the

equilibrium of the firm (Kaldor 1934b) and the relationship between market imperfection and excess capacity (Kaldor 1935), and in a protracted reply to Chamberlin's criticisms (Kaldor 1938b). The American's considered response can be found in his *Towards a More General Theory of Value* (Chamberlin 1957, pp. 45–6, 73–9, 177–9, 187–8). Again there is an almost Talmudic flavour to these pieces,[11] and they have little abiding interest for non-specialists.[12] But this was the cutting edge of microeconomic theory in the 1930s, and Kaldor had shown himself to be a force to reckon with in this area, too.

2.2 Equilibrium (1934)

One by-product of his interest in the theory of the firm did prove to be of lasting significance. This was 'A Classificatory Note on the Determinateness of Equilibrium', published in the very first issue of the *Review of Economic Studies*[13] in February 1934, the very same month in which the Dollfuss putsch destroyed democracy in Austria. Kaldor's theme was 'the conditions necessary to make equilibrium "determinate": the conditions under which we can give a scientifically precise description of the actual course of economic phenomena' (Kaldor 1934a, p. 122). He identified three grounds for concern; in effect they are the now-familiar questions of existence, uniqueness and stability. Kaldor's treatment of the existence problem was both original and idiosyncratic.[14] He was concerned not with the properties of mathematical systems – his discussion was entirely non-mathematical – but rather with the problem of path-dependency:

> For the mere fact that there is, in any given situation, at least one system of prices, which, if established, would secure equilibrium, does not imply that this particular set of prices will also be put into operation immediately; and if any other set of prices is established, not only will further price-changes become necessary, but the equilibrium system of prices (i.e., that particular set of prices which does *not* necessitate further changes in prices) will itself be a different one. It is not possible, therefore, to determine the position of equilibrium from a given system of data, since every successive step taken in order to reach equilibrium will alter the conditions of equilibrium (the set of prices capable of bringing it about) and thus change the final position – unless the conditions are such that either (1) an equilibrium system of prices *will* be established immediately, or (2) the set of prices actually established leaves the conditions of equilibrium

> unaffected (in which case the final position will be independent of the route followed). (*ibid*., p. 124; original stress)

Path-dependency was always a possibility, Kaldor noted, unless any move from one position of equilibrium to another was 'immediate' (*ibid*., p. 126). Where such a move required time,

> it is only by means of a 'theory of the path' (a theory showing what determines the actual path followed) that a causal-genetic approach can arrive at generalisations concerning the nature of equilibrium – and such a theory has not hitherto been forthcoming, although the necessity for it has frequently been emphasised by writers of the Austrian School. (*ibid*., p. 128)

Here Kaldor cited a book by the Austrian theorist Hans Mayer and Appendix F of Marshall's *Principles* (*ibid*., p. 128 n1, n2), together with classic texts by Jevons, Walras and Edgeworth and papers by his friend John Hicks in both English- and German-language journals.[15] Avoiding path-dependence, he continued, required 'that tastes and obstacles [that is, technology], on each day, for everybody, should be unaffected by the events of the previous day' (*ibid*., p. 128). Thus for equilibria to be 'determinate', or path-independent, there must be no significant learning effects.[16]

Kaldor's discussion of multiple equilibria included the relatively familiar cases of 'backward-rising' supply curves and 'backward-falling' demand curves that had been recognised by Walras, Wicksell, Marshall and Hicks (*ibid*., pp. 129–30). Significantly, Kaldor pointed out that non-uniqueness was a sufficient condition – though not a necessary condition – for path-dependence:

> multiple equilibrium will always be present whenever there are stages of 'increasing returns' to single industries, i.e. whenever there are stages of diminishing technical marginal rates of substitution. In these cases, therefore ... the final situation will be 'indeterminate' in the sense that it will depend upon the direction which happens to be adopted initially; though equilibrium may still be determinate on our definition of the term, since all possible equilibrium positions may still be deduced from the data of the initial situation. (*ibid*., pp. 131–2)

The first and second problems of equilibrium theorising were thus intimately connected.

The third problem was 'whether in any given case equilibrium will be "definite" or "indefinite" (i.e., whether it will be approximated to or not)', and this 'appears to depend on the velocities of adjustment of the factors operating in the system' (*ibid.*, p. 133). Here Kaldor drew upon published work (all in German) by his LSE colleague Paul Rosenstein-Rodan, by Henry Schultz and by Umberto Ricci, and also cited an Italian paper by Fasiani. He distinguished between 'completely continuous' and 'completely discontinuous' adjustments, the latter case being much easier to deal with. Here 'stability (or "definiteness") of equilibrium will depend on the relative elasticities of demand and supply; according to what may be called "the cobweb theorem" of Professor Henry Schultz and Professor U. Ricci' (*ibid.*, p. 134). Kaldor illustrated the theorem in two diagrams (*ibid.*, p. 134, figures 2 and 3). He claimed to have originated the term 'cobweb theorem',[17] and this is confirmed by Pashigan (1987). Today the conclusions are known to all students of intermediate microeconomics, but in 1934 they were stated in English for the very first time:

> (i) If demand is elastic relatively to supply, the cobweb will be contracting; equilibrium will be 'definite';
> (ii) If supply is elastic relatively to demand, the cobweb will be expanding; equilibrium will be 'indefinite';
> (iii) If the elasticity of supply and demand are the same, there will be a constant range of fluctuations. (Kaldor 1934a, p. 135)[18]

In the more difficult case of continuous adjustment, Kaldor concluded, stability was governed by the relative speeds of adjustment of supply and demand rather than by the relative elasticities.

Kaldor was always proud of this paper, and justifiably so (1960a, p. 4). Its most important contribution, however, was not the elucidation of the cobweb theorem but the discussion of path-dependence. Again, this was not Kaldor's discovery; it can be traced back (like so much in the history of economic thought) to Alfred Marshall and his doubts concerning the reversibility of the long-run supply curve, a point which had been emphasised by Allyn Young in his LSE lectures (Blitch 1990; Sandilands 1990). But Kaldor brought out its microeconomic significance with exceptional clarity, even if he failed to recognise just how subversive of neoclassical economic theory it really is (see Kaldor 1972a for a belated acknowledgement of this point; cf. Harcourt 1988). He did not, in 1934, indicate any awareness of the implications of path-dependence for macroeconomic theory. Decades later he would, however,

invoke precisely this principle in his attack on the neoclassical approach to economic growth (1975a, 1985a; cf. Setterfield 1998). By this time similar arguments had come to figure prominently in Joan Robinson's critique of mainstream theorising (Robinson 1974) and, under the fancy title of *hysteresis*, were being used both by Post Keynesians and more orthodox macroeconomists to attack monetarist claims about the so-called 'natural' rate of unemployment (Arestis and Sawyer 2004; cf. Cross 1987), while path-dependence features prominently in the influential work of scholars like Paul David (1975) and W. Brian Arthur (1994). All this was implicit in the work of the 25-year old Nicholas Kaldor.

2.3 Welfare (1939)

His second major contribution came in his short paper 'Welfare Propositions of Economics and Interpersonal Comparisons of Utility', published in the September 1939 issue of the *Economic Journal* (1939d). Some background on the development of welfare economics in the inter-war years may help to put this brief note in context. For conservative economists, utilitarianism had always seemed to carry a dangerously egalitarian message. Since everyone accepted the law of diminishing marginal utility, 'maximising utility' for society as a whole appeared to require the complete elimination of inequality in income and wealth, subject only to qualifications about the need to avoid seriously damaging the incentives to work, save and take risks.[19] Since an extra dollar is worth so much more to a poor person than to a rich one, the case for taking from the latter and giving to the former was hard to deny ('Do you really need 50 cents more than he does?', as the twenty-first century charity advertisements would ask, under photographs of young African famine victims). Even an old-fashioned liberal like A. C. Pigou was driven a long way towards socialism by the force of this argument (Pigou 1937, chapter 2).

One way in which a utilitarian could escape from egalitarianism was by means of a dogmatic denial of the very possibility that the utility levels of different individuals could be compared in the first place. This defence of privilege required an unconvincingly solipsistic approach to the problem of comparing the states of mind of different individuals, but it was advocated with great energy and considerable eloquence by Lionel Robbins, Kaldor's boss at the LSE (Robbins 1932, pp. 138–41), and it caused some unhappiness among his less conservative colleagues. The Fabian socialist Barbara Wootton, for example, made Robbins's

argument a centrepiece of her bitter *Lament for Economics*, published in 1938. It was not just a question of rich and poor, Wootton complained. The inability to compare the utilities of different individuals destroyed the economist's ability to make *any* welfare judgements about resource allocation. '*Nothing* can get us past the difficulty that where one man works, and another uses the product of his work, it is impossible to say with certainty whether or not the product was really worth the making' (Wootton 1938 pp. 14–15; original stress). It was the inability to justify inter-personal comparisons of utility, more than any of the other difficulties that she identified in contemporary economic theory, which led Wootton to abandon the discipline in favour of sociology, which seemed to offer a more coherent approach to social policy-making (King 2004b). Hers was an extreme case, but she was by no means the only left-leaning economist to be worried by Robbins's position. Roy Harrod, though dismissive of her 'jeremiad' (Harrod 1938, p. 384), agreed with her on this point: 'If the incomparability of utility to different individuals is strictly pressed, not only are the prescriptions of the welfare school [Pigou and other advocates of cardinal utility] ruled out, but all prescriptions whatever. The economist as an adviser is completely stultified' (*ibid.*, p. 397).

Half a century later John Harsanyi dismissed Robbins's position as essentially fallacious, except where it was applied to artistic and cultural judgements:

> It seems to me that economists and philosophers influenced by *logical positivism* have greatly exaggerated the difficulties that we face in making interpersonal utility comparisons with respect to the utilities and disutilities that people derive from ordinary commodities and, more generally, from the ordinary pleasures and calamities of human life. (Harsanyi 1987, p. 957)[20]

Those who agreed with Robbins, however, needed an alternative foundation for their policy prescriptions. A solution was, in fact, at hand. In 1894 and 1896–1897 Vilfredo Pareto, writing in Italian, had defined an optimal allocation of resources as one in which it was not possible to make any one individual better off without simultaneously making one or more other individuals worse off.[21] This principle of *Pareto optimality* (to use modern terminology) did not require inter-personal comparisons of utility or, indeed, any notion of cardinal utility at all; it relied upon people's ordinal judgements as to whether a change from one state of the world to another would make them better off or worse off, or leave them

indifferent. A *Pareto improvement* is thus a change that makes one or more individuals better off without making someone else worse off. Again, all this is known today by every intermediate student of micro-economics. In 1939, however, Pareto's work was yet to be translated into English, though it was available in French (and, of course, in Italian).

Enter Nicholas Kaldor, by now a Fabian socialist, who may not have read Wootton's *Lament* but was certainly familiar with the arguments.[22] He was in 'entire agreement' with Robbins, Kaldor confirmed, that inter-personal comparisons of utility were in principle impossible, but this did not entail that 'economics as a science' could say nothing about policy. Take a classic example: the repeal of the Corn Laws, which benefited capitalists and workers at the expense of the landlords.[23] This was evidently not a Pareto improvement, but it was a good thing none the less, since the British government could have compensated the landlords for their loss of income while leaving everyone else better off than before. Kaldor's statement of the principle involved, and its implications for the assessment of economic policy, is so lucid that I shall cite it at some length:

> In this way, everybody is left as well off as before in his capacity as an income recipient; while everybody is better off than before in his capacity as a consumer. For there still remains the benefit of lower corn prices as a result of the repeal of the duty.
>
> In all cases, therefore, where a certain policy leads to an increase in physical productivity, and thus of aggregate real income, the economist's case for the policy is quite unaffected by the question of the comparability of individual satisfactions; since in all cases it is *possible* to make everybody better off than before, or at any rate to make some people better off without making anybody worse off. There is no need for the economist to prove – as indeed he never could prove – that as a result of the adoption of a certain measure nobody in the community is going to suffer. In order to establish his case, it is quite sufficient for him to show that even if all those who suffer as a result are fully compensated for their loss, the rest of the community will still be better off than before. (Kaldor 1939d, p. 550; original stress)

Thus Pigou had been correct to propose that welfare economics be divided into two parts:

> The first, and far the most important part, should include all those propositions for increasing social welfare which relate to the increase

> in aggregate production; all questions concerning the stimulation of employment, the equalisation of social net products, and the equalisation of prices with marginal costs, would fall under this heading. Here the economist is on sure ground; the scientific status of his prescriptions is unquestionable, provided that the basic postulate of economics, that each individual prefers more to less, a greater satisfaction to a lesser one, is granted. In the second part, concerning distribution, the economist should not be concerned with 'prescriptions' at all, but with the relative advantages of different ways of carrying out certain political ends. For it is quite impossible to decide on economic grounds what particular pattern of income-distribution maximises social welfare. (*ibid.*, p. 551)

The economist is not qualified, as an economist, to make judgements about the distribution of income, or to proclaim for or against a lesser or greater degree of inequality. 'All that economics can, and should, do in this field, is to show, given the pattern of income-distribution desired, which is the most convenient way of bringing it about' (*ibid.*, p. 552).

Kaldor did not invent the compensation principle, which must have been familiar to lawyers for centuries, and in political discourse already had a long, if chequered, past. In the 1790s, for example, the radical activist Thomas Spence asserted the right of all citizens to a Basic Income as 'fair compensation' for the losses they had incurred as a result of the private appropriation of the land (King and Marangos 2006). Surveying the economic literature, Chipman (1987) traces the compensation principle back to Dupuit (in 1844) and Marshall (in 1890), the latter making use in his *Principles* of what would later become known as Pareto optimality. Pareto himself was the first to apply the principle to the analysis of competitive general equilibrium, and his follower Enrico Barone elaborated upon it in an important paper (again in Italian) on the economics of socialism in 1908. The compensation principle was at the very least implicit in their work. The concepts of Pareto optimality and of compensation were rediscovered, apparently independently, by Kaldor's friend (and at the time LSE colleague) Abba Lerner in a 1934 paper on the measurement of monopoly power. They were used four years later by Harold Hotelling (1938) to discuss the relative merits of income taxes and excise taxes.

But, leaving Marshall aside,[24] Kaldor was the first economist writing in English to identify compensation as a general principle for judgements

concerning social welfare.[25] His paper proved extremely influential, as he later noted with a mixture of pride and exasperation:

> This idea was subsequently taken up and developed by Professor Hicks and, following him, the proposition was subjected to a searching examination by a whole host of economists (including Scitovsky, Baumol, Little, Samuelson, Arrow, Graaf, Reder, Dobb and many others), until it became a veritable *cause célèbre* under the flattering title of the 'New Welfare Economics'. On re-reading the original note in the light of all this subsequent work (some of which, I must confess, I found too tedious to read and some of which was plainly beyond my comprehension), I still feel unrepentant in rejecting Professor Robbins' proposition that the impossibility of making inter-personal comparisons of utility puts an effective bar to 'economics as a science saying anything by way of prescription'. (Kaldor 1960a, p. 5)[26]

By the end of the twentieth century the compensation principle was widely used by moral philosophers like Robert Nozick (Lacey 2001), apparently without any recognition of Kaldor's contribution. At all events, his short note must be counted as one of the most influential three-page articles in the history of economic theory.

2.4 Speculation (1939)

Kaldor's article 'Speculation and Economic Stability', published in the *Review of Economic Studies* in the month that the Second World War began, is easily the longest, most densely written and most complex of his four seminal papers from the 1930s, and also arguably the best. It has by far the most convoluted publishing history. I shall cite the version that Kaldor included in his 1960 collected essays rather than the original, since that was his preferred text, but readers should be warned that it differs significantly from the original.[27]

The intention of the paper was clearly expressed in the title. Kaldor defined speculation as

> the purchase (or sale) of goods with a view to re-sale (re-purchase) at a later date, where the motive behind such action is the expectation of a change in the relevant prices relatively to the ruling price and not a gain accruing through their use, or any kind of transformation effected in them or their transfer between different markets. (1980b, p. 17)

The term 'goods' was slightly misleading, as it soon became apparent that financial securities played a major part in Kaldor's story. In fact the 1939 paper constituted an important – if generally unheralded – contribution to the theory of finance. It is very much in the heterodox tradition,[28] especially since, by 1940, Kaldor had been convinced by his critics, Christopher Dow (1940) and Ralph Hawtrey (1940), that expectations could not successfully be modelled in terms of a single representative agent and that analysis of the forward market required at the very least a distinction between bulls and bears (Kaldor 1940d, p. 200).[29] It was also a critique of the *General Theory*, not only taking in Keynes's chapter 17 discussion of own-rates of interest but also extending to the theory of liquidity preference and the multiplier. Finally, Kaldor touched on the theory of money and the analysis of monetary policy, using arguments that have sometimes (contentiously) been interpreted as foreshadowing his post-1970 Post Keynesian views on endogenous money.

Objects of speculation, Kaldor noted, must be standardised, durable and valuable in proportion to bulk (1980b, p. 20). Only two classes of assets satisfy these conditions: certain industrial raw materials and financial assets (bonds and shares) (*ibid.*, p. 22). He set out the necessary relationships between expected price, current price and future price in a series of equations involving the rate of interest, the yield, the carrying cost and the risk premium (*ibid.*, pp. 23–5). This drew heavily on Keynes's treatment of these questions in his *Treatise on Money*, and Kaldor was later to suggest that his own article 'provides a missing link – or rather, one of the missing links – between Keynes's ideas in the *Treatise* and in the *General Theory*' (Kaldor 1960b, p. 4). There followed a lengthy and involved discussion of the influence of expectations on the volatility of prices, for both raw materials and securities, and an equally long and demanding analysis of the effects of speculation on the level of economic activity (Kaldor 1980b, pp. 31–54).

Writing in 1980, Kaldor described his article as an attempt

> to generalise Keynes' theory of the multiplier by demonstrating that it results from the stabilising influence of speculative expectations on prices which applies in all cases in which the elasticity of speculative stocks is high ... [and] to show that Keynes' theory of interest contains two separate propositions. The first regards interest as the price to be paid for parting with liquidity, and it arises on account of the *uncertainty* of the future prices of non-liquid assets. The second concerns the dependence of the current rate of interest on the interest

> rates expected in the future. While the first proposition provides an explanation of why long-dated bonds should normally command a higher yield than short-term paper, it is the second which explains why the traditional theory of the working of the capital market was inappropriate – why, in other words, savings and investment are brought into equality by movements in the level of incomes, far more than by movements in interest rates. And this second effect will be the more powerful the *less* is the uncertainty concerning the future, or the greater the firmness with which the idea of 'a normal price' is embedded in the minds of professional speculators and dealers. (1980a, p. xvii; original stress)

When he met Keynes at a Cambridge tea party a few weeks after the article appeared, Kaldor was pleased to find that he had already read it, 'and said that I might well be right that it is the price stabilising influence of the policies of dealers and speculators, rather than the premium which the public requires for parting with liquidity, which explains why an increase in the propensity to save is not itself capable of generating more investment' (*ibid.*, p. xvii n2).

If Kaldor's argument cast doubt on the theory of liquidity preference, it also had significant implications for the operation of monetary policy. These were brought out in section 4 of the article, which concluded that 'we must expect the bank rate mechanism, as an instrument of economic policy, to become increasingly ineffectual'. While it was 'still available for dealing with an occasional boom', bank rate 'becomes more and more ineffective as a safeguard against the ravages of deflation' (1980b, p. 58). This passage, and the earlier discussion in an extended footnote and the associated diagram, has often been seen as evidence that Kaldor was already in 1939 an advocate of endogenous money. In a footnote he drew a money market diagram with a highly elastic money supply curve, though not a horizontal one (*ibid.*, p. 32 n2), and commented that:

> The elasticity of the supply of money in a modern banking system is ensured partly by the open market operations of the central bank, partly by the commercial banks not holding to a strict reserve ratio in the face of fluctuation in the demand for loans, and partly it is the consequence of the fact that under present banking practices a switch-over from current deposits to savings deposits automatically reduces the amount of deposit money in existence, and vice versa. (*ibid.*, p. 39 n2)

This has something in common with both the accommodationist and the structuralist versions of the subsequent Post Keynesian theory of endogenous money.[30]

To do full justice to 'Speculation and Economic Stability' another full-length paper would be required (see Sardoni 2006). As Hicks told Kaldor, after the publication of the 1960 version, it was 'the culmination of the Keynesian revolution in theory. You ought to have had more credit for it' (cited by Targetti and Thirlwall 1989b, p. 4). Kaldor remained justifiably proud of the paper, which convincingly analysed real-world capitalist markets where stocks dominate flows and expectations about the future course of prices are crucial in modelling both supply and demand.[31] But he never really followed it up, failing to develop the alternative macroeconomic model, faithful to the spirit of Keynes but incorporating all his criticisms, which it seemed to demand. His 1940 trade cycle model, for example, which is described in the following section, owes absolutely nothing to his analysis of speculation and economic stability and barely mentions money or finance.

2.5 Cycles (1940)

There is, however, a real sense in which Kaldor (1940c) can be said to contain the first truly Keynesian model of the trade cycle, after an abortive first attempt two years earlier (1938d). Keynes himself had provided only 'Notes on the Trade Cycle', a chapter heading that accurately described the contents. He attributed the cycle very largely to fluctuations in the marginal efficiency of capital, themselves explained by changes in the profit expectations of business people (sometimes reinforced by the effects of sharp changes in stock market prices). The discussion is informal and discursive throughout (Keynes 1936, chapter 22).

Kaldor set out to formalise the argument, and in the process eliminated much of its subtlety. In the Keynesian system the level of income is determined by the relationship between saving and investment. If planned or intended saving and investment differ, the equilibrium level of income will change, and so, Kaldor suggested, it should be possible to construct a model of cyclical fluctuations in terms of regular and systematic changes in the savings–investment relationship. Kaldor's own contribution was to recognise that cycles could be generated by requiring both saving and investment to be non-linear functions of income. The investment function is non-linear because, at low levels of income, excess capacity is a strong deterrent to new investment and, at high levels of income, construction costs rise rapidly and the difficulty

of borrowing increases. The saving function is non-linear because the average propensity to save rises with income and, at very low levels of income, saving almost disappears (*ibid*., pp. 81–2). There are two ways in which this differs from contemporary multiplier-accelerator models of the trade cycle (e.g., Harrod 1936; Steindl 1937; Samuelson 1939). First, since Kaldor's savings function is non-linear the marginal propensity to save, and thus also the multiplier, varies over the cycle. Second, in Kaldor's model investment depends not on the rate of change of income (or consumption) but on its level; there is no explicit accelerator mechanism. A third difference from the Harrod model is that there is no trend; in 1940 (though not in subsequent work) Kaldor was happy to model the cycle in the context of a static and not a growing economy.

The mechanism of Kaldor's six-stage cycle is quite straightforward, although his diagrams are not especially easy to follow (Kaldor 1940c, pp. 83–4, figures 5 and 6; see also Targetti 1992, p. 81, figure 3). To generate cycles, Kaldor makes the S and I functions shift over time as a result of changes in the capital stock and in the level of income itself. He identifies three necessary conditions (which together are sufficient) for recurrent cyclical fluctuations. First, the I function must be steeper than the S function at normal levels of activity. Second, the I function must be less steep than the S function at extreme levels of activity (i.e., in both boom and slump conditions). Third, the level of investment at the upper turning-point must be large enough for the I function to fall, relatively to the S function; and the converse must be true at the lower turning-point (*ibid*., pp. 85–6). If the first condition is not met, the economy will be stable, and there will be no trade cycle. If the second condition is not met, the economy will be unstable downwards (and capitalism will collapse). Kaldor concluded that the forces acting to bring a slump to an end were less reliable than those that end a boom: 'the danger of chronic stagnation is greater than the danger of a chronic boom' (*ibid*., p. 87).

All this was set out without any mathematics. When the model was eventually subjected to formal analysis it proved to have serious weaknesses. The necessary and sufficient conditions for cyclical fluctuations that are specified by Kaldor are defective, and the persistence of cycles depends crucially on parameter values, the speed of adjustment, initial disturbances and the exact position of the S and I functions (Chang and Smyth 1971). In his reply to his critics, Kaldor acknowledged that a fourth condition was required, and had been implicit in his own paper: movements along the I and S curves must proceed faster

than shifts in the curves due to capital accumulation. If this condition did not hold, no Keynesian equilibrium was possible (Kaldor 1971c, p. 45). He did not, however, acknowledge the crucial methodological point, that any coherent model of the trade cycle must be expressed in mathematical terms. There was an additional problem with the model, which Kaldor again neglected. Apart from one brief reference to the difficulty of obtaining finance as a constraint on investment in a boom there was no monetary or financial component. It is as if 'Speculation and Economic Stability' had been written by someone else altogether.[32]

What were the prospects for controlling the cycle through macroeconomic policy? Keynes had expressed strong doubts concerning the effectiveness of monetary policy, since fluctuations in the marginal efficiency of capital were likely to swamp any feasible changes in interest rates. 'I conclude that the duty of ordering the current volume of investment cannot safely be left in private hands' (Keynes 1936, p. 320). Kaldor took this for granted, arguing that public investment should be undertaken early in the downturn, to prevent the movement from Stage II to Stage III. Once Stage III is reached, a sharp contraction is unavoidable, he suggested, and the volume of public investment required to move the system from Stage IV to Stage VI is much larger than that needed to move it from V to VI. 'Thus just when the depression is at its worst the difficulty of overcoming it is the greatest' (*ibid.*, p. 88). On balance Kaldor was pessimistic:

> The chances of 'evening out' fluctuations by 'anti-cyclical' public investment appear to be remote. For if the policy is successful in preventing the downward cumulative movement, it will also succeed in keeping the level of private investment high; and for this very reason the forces making for a down-turn will continue to accumulate, thus making the need for continued public investment greater. (*ibid.*, p. 88)

There is a strong hint here of Michał Kalecki's celebrated reference to 'the tragedy of investment', which is 'that it causes crisis because it is useful' (Kalecki 1939 [1990], p. 318). In an appendix Kaldor does compare his model with Kalecki's:

> The drawback of such explanations is that the existence of an undamped cycle can be shown only as a result of a happy coincidence,

> of a particular constellation of the various time-lags and parameters assumed ...
>
> Moreover, with the theories of the Tinbergen-Kalecki type, the amplitude of the cycle depends on the size of the initial shock. Here the amplitude is determined by endogenous factors and the assumption of 'initial shocks' is itself unnecessary. (Kaldor 1940c, pp. 91–2)

These features of the 1940 Kaldor model were important. They opened up an important new area of trade cycle research, in which regular endogenous fluctuations could be generated without the need to rely on time-lags or erratic shocks (Matthews 1959).

However, the article received little critical attention on publication, appearing as it did only months before the end of the 'phoney war' and the start of the Battle of Britain. Ten years later Kaldor's old friend Hicks referred briefly to it in his own book on the trade cycle, claiming in a footnote that

> Mr. Kaldor's model is not based on the acceleration principle (investment depending on changes in output); it is based on an assumed connexion between investment and the absolute level of output, a relation which seems to me to be much less defensible. Nevertheless, it must be said that the Kaldor theory is better than the assumptions on which it is formally based; for a certain amount of what is really 'acceleration' is allowed to go on, as it were, behind the scenes. (Hicks 1950, p. 9 n2)

Kaldor did not stress his objections to the accelerator in the 1940 paper, but when he published an extended review of Hicks in 1951 it was at the centre of his critique: 'The real weakness of Mr. Hicks' model consists in his use of the "acceleration principle", which is a crude and highly unsuitable tool for analysis – and also an obsolete one, that an economist of Mr. Hicks' subtlety should have long ago discarded' (Kaldor 1951b, p. 837). The capital–output ratio was not a technical constant, Kaldor now maintained, but an economic variable, which depended on the financial resources of firms and on entrepreneurial expectations (*ibid.*, pp. 839–41).[33] Thus a non-linear relationship between investment and income, as in the 1940 Kaldor model, was much more plausible than the constant accelerator coefficient used by Hicks.

The second half of Kaldor's review was devoted to the connection between the trend and the cycle (*ibid.*, pp. 841–7). He returned to this

question in a later article, noting that Keynesian macroeconomics tends to preclude anything other than a static model of the cycle:

> Indeed, the development of trade-cycle theories that followed Keynes's *General Theory* has proved to be positively inimical to the idea that cycle and dynamic growth are inherently connected.... For it has been repeatedly (and in my view conclusively) shown that a few simple additions to Keynes's own model of a general equilibrium of production in the economy will produce the result that this 'equilibrium' will take the form, not of a simple steady rate of production in time, but of a rhythmical movement of constant amplitude and period – in other words, a perpetual oscillation around a stationary equilibrium position. (Kaldor 1954, p. 54)

Keynes-inspired theories of growth, on the other hand, were not able to incorporate cyclical fluctuations with any degree of success. They neglected the crucial factor: 'human attitudes to risk-taking and money-making' (*ibid*., p. 67). 'The same forces therefore which produce violent booms and slumps', Kaldor concludes, 'will also tend to produce a high trend-rate of progress; though the connection between the two is far too complex to be reducible (at present) to a simple mechanical model' (*ibid*., p. 70).[34]

This was, perhaps, a rather veiled piece of self-criticism. Kaldor's final reflections on the 1940 model were more favourable. In the introduction to his collected *Essays on Economic Stability and Growth* (Kaldor 1960b, pp. 9–10), he noted that a non-linear investment function had been used by both Hicks (1950) and Richard Goodwin (1951) in their own cycle models. Kaldor reasserted his objections to the accelerator principle and acknowledged that Goodwin, at least, had later reformulated his analysis in a more acceptable fashion (Goodwin 1955). When Kaldor returned to questions of macroeconomic instability to explain the stagflation crisis of the 1970s he developed an entirely different method of analysis from that in the 1940 paper (Kaldor 1976), and at the end of the decade he claimed of his original model only that it had 'not...been rendered obsolete by subsequent work' (Kaldor 1980a, p. xvi). This seems an entirely reasonable assessment.

The outbreak of war marked the beginning of a new phase in Kaldor's career. As a British citizen he was not liable to internment but, as a recent ex-Hungarian, he could not expect to play anything other than a menial role in the war effort. And so he chose to remain in academia, moving to Cambridge with the residue of the LSE and turning away

from theory to concentrate on applied economics and issues of policy. The shift was already apparent in his short but perceptive analysis of war finance, published in August 1939 in *The Banker* (1939c), which will be discussed in the next chapter. One can only speculate on what Kaldor might have achieved in economic theory in the 1940s; his record in the devil's decade suggests that he was capable of great things. But the war intervened, and his attention shifted away from pure theory to much more practical questions of economic policy.

3
Kaldor's War

3.1 From London to Cambridge

As with so many lives, Kaldor's was turned around by the war. This was not the result of enemy action, and he himself was not called to arms. While he had become a British citizen in 1934, and made enquiries about joining the Civil Service as an economic adviser, Kaldor was told that his Hungarian origins would disqualify him from anything other than menial duties in Whitehall. He therefore decided to stay in academia, and relocated to Cambridge with his remaining LSE colleagues in September 1939, when the Ministry of Works took over the School's Aldwych site in central London.[1]

Now based at Peterhouse, Kaldor was able to deepen old friendships and develop new ones. A 'war circus' of economists began to operate, named by analogy with the 'Cambridge circus' of young theorists who had interrogated Keynes in 1930–1931 after the publication of the *Treatise on Money* and helped to focus his mind on the revolutionary breakthrough of the *General Theory* (Moggridge 1995, pp. 531–2). In addition to Kaldor the war circus included Joan Robinson, Piero Sraffa and (when he could escape from official duties in London) Richard Kahn. Kaldor was particularly fond of Sraffa, though the secretive Italian would never let him (or anyone else) into the details of his very long-term project for the rehabilitation of classical economics. He also became a close friend of Robinson, who took him on long walks in the Cambridgeshire countryside during which they discussed economic theory with an intensity that once nearly got them into serious trouble:

> Quite inadvertently we walked into an ammunition dump for the R. A. F. We were arrested and brought before a very fine old English

> major. He was much too old for fighting in the war, and like many other retired officers, was put in charge of routine duties, like looking after an ammunition dump. He took our telephone numbers and addresses and wanted to check our identity by telephone. I gave my wife's number and Joan Robinson gave her husband's number. Then he said, 'Excuse me, I hope I'm not creating any family problems by ringing up your husband?' He was a fine gentleman. (Kaldor 1986b, p. 68)[2]

While he must have discussed theoretical questions on these long country walks, Kaldor's thinking had already shifted, fundamentally, away from questions of pure theory and towards the policy issues thrown up by the war. Lacking any public responsibilities and free from the constraints imposed by official secrecy, he was able to publish a series of articles on the principles of war finance and the prospects for postwar reconstruction. By 1945 Kaldor was no longer merely a theorist. He had emerged as an applied economist of great talent and energy, and had acquired a taste for providing policy advice that he would retain for the rest of his life.

3.2 How to pay for the war

In November 1939 Keynes wrote a series of articles for the London *Evening Standard* that was subsequently published as a best-selling booklet. In *How To Pay For The War* Keynes demonstrated that his new macroeconomic theory was far from being restricted to the 'economics of depression', as even sympathetic critics like J. R. Hicks (1937) had once claimed. Precisely the same principles of public finance that Keynes had prescribed for the achievement of full employment in the mid-1930s were now invoked to explain how total war could be waged successfully without disastrously accelerating inflation or inflicting gross injustice on any section of the civilian population. Keynes's pamphlet confirmed his status as Britain's greatest living economist, and the analytical framework that he set out in it was used to great effect by the financial planners of the Second World War (Moggridge 1995, pp. 629–34; Skidelsky 2000, pp. 67–8).

It is not widely known that Nicholas Kaldor had (almost) got there first. In a short, incisive article in *The Banker* magazine in August 1939 he demonstrated that Keynesian macroeconomics could be adapted to a fully employed (in fact, over-employed) economy at war. The two 'principles of emergency finance', Kaldor maintained, were 'first, to

ensure that the *aggregate real burden* falling on the community should be as small as possible; and second, that the *distribution* of this burden should be equitable' (Kaldor 1939c, p. 149; original stress). The first principle required that total output should be as large as possible. An increase in government expenditure would raise aggregate demand and stimulate output. This was true even if the extra government spending was matched by increased taxation, Kaldor suggested, since part of the extra taxes would be 'paid by a reduction of savings, not by a reduction in consumption', so that aggregate demand would rise (*ibid.*, p. 150).

Here he was very close to enunciating the balanced budget multiplier theorem, according to which a one-dollar increase in government spending, accompanied by a one-dollar increase in lump-sum taxation, is not neutral in its effect on demand but in fact raises aggregate demand by one dollar. This theorem was not published in English until 1945.[3] Although Kaldor was a little vague as to the details he was crystal clear on the underlying mechanism: 'as most economists now agree, it is quite possible for the aggregate real burden of additional public expenditure to be negative, rather than positive' (*ibid.*, p. 149) – crowding *in*, to use more familiar language, instead of crowding out. Everything hinged on the extent to which output could be increased, and this was largely a question of the available reserves of labour-power, which in turn depended on the numbers of men still involuntarily unemployed, the willingness of workers to put in longer hours, and the possibility of increasing the employment of women. 'The limits of "safe" borrowing cannot therefore be determined without knowledge of Government policy in regard to labour.... Taxation during the war should be screwed up only to the extent necessary to avoid an inflationary spiral' (*ibid.*, pp. 150–1). This was the first principle of war finance.

Many people believed, Kaldor continued, that there was an upper limit to the size of the National Debt. But this was 'a pure bogy and it is high time that this bogy should be laid' (*ibid.*, p. 151). Doubling the debt would also double the interest burden (assuming the rate of interest to remain unchanged), and would therefore double the taxes required for that purpose. At the same time, however, it would double the amount of interest received by the owners of the newly created government securities. The effect would be a redistribution of income and wealth, to those who ended up owning the additional securities, from those who did not. Kaldor thought that entrepreneurs would be the principal beneficiaries and fixed-income recipients (salary-earners and rentiers) would be the losers; wage earners would be largely unaffected. His

argument here was rather opaque. It seems to rest on a theory of income distribution that was not clearly articulated, in which profits (mainly) and wages (to a lesser extent) rise as total output increases, while salaries and interest payments do not. These distributional changes would presumably accelerate if inflation occurred along with the growth in real output. Entrepreneurs would tend to save a significant proportion of their increased incomes, while wage-earners would spend almost all of theirs, and so the additional government securities would come to be owned by the entrepreneurs. Similar effects on the distribution of income and wealth would occur if the extra government spending was financed by taxation (*ibid*., pp. 152–4).

After his move to Cambridge Kaldor taught courses in both income distribution and public finance, and he soon began to clarify his thinking on these questions (Targetti 1992, pp. 8–9). As he later recalled, he was 'primarily stimulated by listening to the lectures on the problems of war finance given by Keynes, and to the discussions to which they gave rise'. His thinking was influenced not only by Keynes but also by 'the long debates with one of my earliest pupils, Erwin Rothbarth', who was working as Keynes's research assistant on *How to Pay for the War* (Kaldor 1980a, p. xviii). With the exception of some rather minor criticisms of Hayekian and Pigovian macroeconomics (Kaldor 1941b, 1942c; cf. Hayek 1942, Pigou 1942), the only theoretical questions that seriously engaged Kaldor during the war concerned taxation. As we have seen, there was a very good reason for this. To win the war, the British government had to assume control of a very much larger proportion of the country's economy than it had in peacetime.

There were three ways in which this could be done. First, the state could commandeer resources, effectively suppressing the price system, the market and the use of money for the duration of hostilities. In Australia the expatriate Keynesian economist Colin Clark, now working for the Queensland government, proposed precisely this immediately after the fall of Singapore, when a Japanese invasion seemed imminent. Michal Kalecki's widely discussed plan for 'general rationing' of all consumer goods in Britain was a less extreme version (King 1998a). Second, the state could buy all the commodities, labour and raw materials that it needed, using its ability to issue securities and print money without limit to outbid private customers. This, however, would generate a high, rising and inherently unpredictable inflation rate, which would be grossly unfair to those on fixed incomes and would almost certainly provoke bitter and costly industrial conflict. Third, the state could try to prevent demand inflation by increasing taxation to reduce the sum

of private consumption and investment expenditure by an amount corresponding to the increase in government spending. This, in essence, was the question that Kaldor had been addressing in his 1939 article on the principles of emergency finance.

Almost everyone agreed that higher taxation was, in principle, superior to both a command economy and uncontrolled inflation as a means of mobilising resources to fight the war. An economic theory of taxation was essential if this was to be done effectively, and would also be crucial for postwar reconstruction, when aggregate demand might be either excessive or seriously deficient. In any case, something would have to be done about the excess profits that would have been made, and the unearned fortunes that would have been accumulated, by the 'hard-faced men who had done well out of the war'. In August 1939 Kaldor still had some way to go in working out his ideas on the principles of optimal taxation. This did not stop him from making some very forthright policy proposals. First, in order to minimise the distributional impact of increased borrowing, interest rates should be kept as low as possible. Second, and for the same reason, high marginal rates of taxation should be imposed on the excess incomes generated by the war, especially (but not exclusively) on profits. Third, a wealth tax should be introduced after the war, the proceeds being used to reduce the National Debt. These measures would help to counter the 'twin phenomena of the "*nouveau riche*" and the "*impoverished bourgeoisie*", which so often appear after periods of war, [and] never fail to be a cause of great social unrest' (Kaldor 1939c, p. 154). As he noted three years later, 'The main argument in favour of a post-war capital levy for repaying the National Debt is distributional: it makes it possible to fix the burden of the Debt on a particular group of taxpayers, the capitalists, rather than diffuse the burden among income tax-payers in general' (Kaldor 1942a, p. 140 n1).

3.3 Accounting for macroeconomists

If the theory of war finance was still a work in progress, the same was very much true of the data. In 1941, 1942 and 1943 Kaldor published three papers in the *Economic Journal* examining in painstaking detail the new official statistics on national income and expenditure. Since the overall planning of the war effort demanded reliable information on all the relevant magnitudes, the definition and measurement of aggregate output was no longer a purely academic question, and considerable resources were devoted to it in Whitehall. Kaldor's analysis of the

three White Papers never lost sight of the big issues that lay behind the rather arid technical distinctions drawn by the statisticians.

It is worth noting at this point that there were two distinct national income accounting projects under way in the late 1930s and early 1940s: distinct, but not separate, for they shared a great deal of common ground, but distinct none the less. Project one involved the measurement of economic welfare, sometimes referred to as the 'national dividend', so that questions of the following type could be answered. Are we better off on average now than our grandparents were and, if so, by roughly how much? Are we better off on average than people in other parts of the world, or worse off, and by roughly how much? Project two was all about macroeconomics, and was originally inspired by the need to deal with the mass unemployment of the 1930s. It attempted to answer a different set of questions. What is the maximum potential output of the entire economy, if all its productive resources – especially labour – are fully employed? How far short of this potential is the economy at the present time, and how can the gap between actual and potential output be eliminated so that full employment can be achieved? Project one had its roots in classical economics; project two was the product of Keynes's *General Theory*.

We now know that these were indeed distinct projects, though it was much less obvious then. They were not, to repeat, separate: they relied heavily on the same sorts of data (on total output, income and expenditure, valued at market prices), and they gave rise to monetary aggregates, for there was no other way of adding them up. But they were distinct. The techniques for measuring Gross National Product (GNP) and Gross Domestic Product (GDP) that were generated by project two are indispensable for coherent macroeconomic policy-making, but they are likely to be seriously misleading if carried over uncritically into project one. Today every student of economics understands that per capita GNP is not a satisfactory index of economic welfare for many (in hindsight very obvious) reasons. GNP fails to take account of environmental degradation, the value of leisure, the non-market work performed very largely by women .... the list is a long one, and it has led critics of national income accounting to advocate other and supposedly superior indices of welfare, from the United Nations' Genuine Progress Indicator to the King of Bhutan's concept of Gross National Happiness (Stilwell 2002, pp. 41–2). But these alternative measures are themselves unsuitable for solving the problems of macroeconomic management that were at the heart of project two. And it was project two which, in 1939, suddenly became a matter of great urgency,

as Kaldor's early and imperfect musings about 'emergency finance' revealed.

His interest in Britain's first published national accounts was not, therefore, a purely academic one:

> It is impossible to judge intelligently the system of taxation, or the scale of public expenditures, without a quantitative record of the total economic activity of the nation, which forms the background. This is perhaps even more important in war-time, when the Government controls so much larger a part of the national income; but it is vital in peace-time as well. If a statement of this kind had been presented year by year, simultaneously with the Budget, many financial mistakes of past Governments might have been avoided.
>
> Moreover, the regular publication of this document would stimulate both Government and Parliament to look upon the level and the stability of the National Income, rather than the conventional and narrowly financial standards, as the true criterion of budgetary policy; to regard the movements of the national expenditure, and not merely of the expenditures of public departments, as within their province. It is on the assumption of this wider responsibility that our best hope lies for the post-war world. (Kaldor 1941a, p. 181)

This criterion would soon be restated, by Kaldor's old LSE friend Abba Lerner (1943), as the principle of 'functional finance': 'Government expenditure and revenue should be determined so that total expenditure in an economy is at the rate which will produce full employment without inflation. This is to be done without any concern about whether the resulting budget is in surplus or deficit' (Nevile 2003, p. 149). The principle of functional finance is in sharp contrast with the traditional principle of 'sound finance', which requires that the budget always be balanced, either annually or over the course of the trade cycle. Only in exceptional circumstances, Lerner argued, would the precepts of functional finance and sound finance coincide. When they were in conflict, functional finance should prevail.

In this spirit Kaldor took his readers through the details of the White Paper on National Income and Expenditure, explaining with great care the way in which it was proposed to finance the British war effort. There was increased government spending of £2,500 million on one side of the balance sheet and five items on the other side: a cut in consumption (£350 million), increased output (£450 million), reduced private fixed

investment (£500 million), depletion of inventories (£250 million) and an increase in the overseas deficit (£950 million) (Kaldor 1941a, p. 189; all magnitudes are in 1938 prices). These figures, he concluded, were 'encouraging in showing how much more we can still do before our war effort really reaches its maximum'. Consumption might feasibly be cut by an additional 15 per cent, and output could be increased further. 'Not until real output is at least 115 per cent of the 1938 level, and the Government sector takes up at least 50 per cent of this larger total (as compared with the 37 per cent in the last quarter of 1940), can we feel that our economy has been fully geared to the war' (*ibid.*, p. 190).

In a sense these were vital military secrets, as Kaldor acknowledged:

> The Government are to be congratulated on their wisdom and courage in revealing the 'secrets of War Finance' in the middle of the war. Their frankness – in significant contrast to German methods, which conceal even the budgetary figures – makes possible intelligent discussion and criticism of our methods of War Finance, and provides a solid foundation for the confidence of the nation in its own tremendous strength. (*ibid.*, p. 191)

When he travelled to Germany at the end of the war the Nazi archives revealed their own financial secrets. They told a surprising story:

> the picture of the German war effort which dominated Allied imagination was very largely a false one. Germany did not fight a 'total war'; despite all the propaganda talk, she made no serious attempt to exploit her own war potential fully, except perhaps for a brief period in August and September, 1944, when it was too late to be of any consequence. Whatever the ruthlessness she may have shown towards vanquished enemies, there is no evidence of ruthless sacrifices having been imposed upon her own people for the sake of victory; in terms of the thoroughness of the war effort, Germany lagged well behind not only Britain or Russia in the present war, but also behind her own showing in the first World War. Whatever else may be said about the German war economy it certainly was not 'totalitarian'. (Kaldor 1945–1946, p. 33; cf. Milward 1965; Sereny 1995, chapter XII)

Throughout the war the British government continued to publish its own estimates of the national accounts. Reviewing the 1941 White Paper, Kaldor commented on the huge increase in personal savings that

had taken place since 1938 (Kaldor 1942b, p. 213). By this stage

> about one-quarter of our increased Government expenditure since the war was furnished by increased production, and one-quarter by reduced consumption, while the remaining half came from sources which involve a 'burden on the future' – i.e., which make the amount of resources at the disposal of the post-War generation less than they would have been if there had been no war. In 1940, just under two-thirds of the war expenditure came from such sources; so that, as between these two years [1940 and 1941], the proportion furnished by sources which involve no burden on the future has increased. (*ibid.*, p. 221)

There was still scope for improvement: the government could at a pinch increase its command over resources by another 14 per cent, 'but this rate may be approached in 1942' (*ibid.*, p. 222).

Kaldor's third and final excursion into national income accounting, written with his Hungarian colleague Tibor Barna, drew even more optimistic conclusions. By 1942 reduced consumption made up roughly 25 per cent of the 'real sources of war finance', about the same as in 1940; increased output now constituted 43 per cent, up from 17 per cent in 1940; and the contribution made by reduced investment, at home and abroad, had fallen from 60 per cent to 32 per cent (Kaldor and Barna 1943, p. 272). 'In all these respects', they concluded,

> the performance greatly exceeded the promise; there are few economists (if any) who would have dared to predict in 1939 that the war-time increase in the national income could become so large, or that the war-time capital consumption or the degree of price inflation could be kept so small. The latent reserves of our peace-time economic system have proved to be greater than even the most optimistically (or pessimistically?) minded observer could have expected. (*ibid.*, p. 263)

There were important implications for Britain's postwar economic prospects. 'If the war ended tomorrow and hours were reduced to the 1938 level, and if only half of the five millions additionally occupied could be retained in industrial employment, home-produced output would still show an increase of some 18 per cent over 1938' (*ibid.*, p. 273). Added to this, productivity would almost certainly continue to rise, so that, on the assumption that full employment was maintained, 'post-war

home-produced output can be expected to be about a quarter above the pre-war level; and this expectation should provide the framework in which plans for post-war reconstruction are to be fitted' (*ibid*., p. 274). Here project one and project two came together.

3.4 Unless we plan now ...

Kaldor now began to ponder more general questions about the organisation of the economy, and the broader society, after the war. Again the context is important. Public opinion in Britain swung sharply to the left between 1939 and 1942. George Orwell believed, as late as February 1941, that a socialist revolution was both imminent and necessary for victory over the Nazis, since only radical political change would give the working class sufficient motivation to continue the fight (Orwell 1941). This was an extreme view that soon proved to be false, but there was an almost universal conviction that there could be no return to the mass unemployment, poverty and intolerable inequality of the 1930s (Addison 1975). Once the danger of a German invasion had passed there was an intense and sustained debate on how to create a better future, which attracted students to thousands of adult education classes (both civilian and military) and spilled over into the popular press and the BBC. Never before (or since) had popular interest in economics been so widespread or so intense.

Kaldor's first contribution to these debates came in a pamphlet on the special problems of transition from a wartime to a peacetime economy written with his old friend Peggy Joseph. Published by the Association for Education in Citizenship, it was intended for use by adult classes (and ended with a list of questions for class discussion). The tone of the pamphlet was consistently optimistic. The problems that Britain faced were less serious than commonly supposed, Joseph and Kaldor argued, and the solutions would prove painless and effective. There was a general feeling in the country that in many ways things had got better since the start of the war, but also a sense of foreboding and a vague but powerful belief that it was all too good to last:

> Most people, if asked about conditions in war-time, would answer that although life is undoubtedly harder than it was before the war, it is not nearly so bad as they feared it would be when war broke out. Hours of work are longer, food is simpler, amusements are fewer, life is more monotonous – but there are compensating advantages. There is a greater sense of comradeship among people of all classes; there is

> more social equality. There is also much less economic insecurity; people are much less afraid of losing their jobs and of suddenly ceasing to earn their daily livelihood. (Joseph and Kaldor 1942, p. 2)

All this was due to rationing, which ensured fair distribution, and to full employment. However, many people had the impression 'that all this war-time spending must be at the cost of the future; if things are not so bad now – because people are earning decent wages and there is work for all and enough food for all – this only means that conditions will be all the worse afterwards' (*ibid.*, p. 2).

This unease was given some credence by memories of the unhappy experience at the end of the First World War, when a brief but furious boom was followed by a severe depression. To 'avoid the mistakes of the last demobilisation and to set up an economic system which will enable us to enjoy the full benefits of our economic wealth' (*ibid.*, p. 7), sensible policies would be necessary in both the immediate aftermath of the war and in the longer term. Planned demobilisation would be essential in the short term, with the retention of wartime controls for as long as was necessary to transfer labour and other resources to peacetime use. The 'bonfire of controls' that had been staged in 1919 could not be repeated. In the longer term, policy should be directed towards the 'three outstanding features of the pre-war economic system which everyone would like to get rid of…unemployment, poverty and inefficiency' (*ibid.*, p. 14). Full employment could be maintained through government spending, which would win general support 'once it is understood that increased State expenditure for the purpose of employing idle resources is not wasteful but is actually a way of avoiding waste' (*ibid.*, p. 16). But full employment would bring new problems, the most important of which was wage and price inflation:

> There is a great danger…that with the present system of sectional wage-bargaining, in a state of full employment a tug of war will ensue between the workers of different industries for larger slices of the national cake, in the course of which wages and prices will continually rise…. A policy of full employment will require, therefore, that the present system of wage-bargaining by trade unions and employers' federations in *individual industries* should be replaced by a system of wage determination on a national basis'. (*ibid.*, p. 18; original stress)

For the rest of his life Kaldor was to be a consistent advocate of national incomes policy as the principal anti-inflationary instrument, superior in every way (be believed) to more orthodox policies of demand deflation.[4]

The problem of poverty, Joseph and Kaldor continued, could be solved by implementing the newly published Beveridge proposals for a comprehensive welfare state (see Section 3.5 below). The question of inefficiency arose from the lack of genuine price competition in many industries. As a result,

> prices in most industries are much higher than the costs of production with the most efficient methods and are generally high enough to allow many inefficient firms to exist side by side with efficient ones. And so it comes about that only a fraction of each industry's output is produced by the most efficient methods. (*ibid.*, p. 21)

Since 'competition does not fulfil its function of eliminating the unfit' (*ibid.*, p. 22) the state should step in, 'forcing industries to sell at truly competitive prices – and at prices at which only the efficient, and not the inefficient firms can survive'. This method, they concluded, 'is the only one by which capitalism could be made to work' Discussion of 'the much wider question of Socialism versus Capitalism' was deferred to a later date (*ibid.*, p. 23).

Kaldor and Joseph were in no doubt, though, that a suitably reformed British capitalism *would* work. The war had not greatly reduced the nation's capital stock, and would probably improve the skill and technical ability of the working population. Given a reasonable degree of international economic cooperation, the postwar prospects for British exports were bright. The loss of foreign assets would reduce investment income from overseas, but only by 2–3 per cent of prewar national income (*ibid.*, p. 5). There was no reason to worry about the (domestically owned) War Debt, 'for the simple reason that the members of the nation are the creditors as well as the debtors' (*ibid.*, p. 6). There would have to be a transfer of income to the owners of government debt, but in all likelihood this would be quite modest: 'The total increase in interest-burden owing on all the debt incurred in the first two years of the war is only about £50 million per annum – or about half the average annual pre-war cost of unemployment benefits' (*ibid.*, p. 6 n).

3.5 Beveridge I: The welfare state

In Sir William Beveridge's first wartime report, *Social Security and Allied Services*, the Director of the LSE proposed a comprehensive system of social security to eliminate poverty (Beveridge 1942). Joseph and Kaldor gave Beveridge their endorsement in a brief section of their pamphlet (*ibid.*, pp. 18–20). Kaldor became an enthusiastic propagandist for the

plan, publishing a scholarly appraisal of its financial implications in the *Economic Journal* (Kaldor 1943a) and a popular version in another pamphlet published by the Social Security League (Kaldor 1944b). This pamphlet was the transcript of a BBC broadcast in October 1943, in the series 'The World We Want: What Must We Give to Get It?' Two fundamental principles were involved, Kaldor told his radio audience:

> First, the idea of *universality*; everyone is brought in, irrespective of social status or income level. Everyone would pay the same rate of contribution and everyone, the millionaire as well as the navvy, would be entitled to the same benefits. Under the present scheme, only *employees* are covered by social insurance, and only up to a certain income level. (*ibid.*, p. 4; original stress)

The second new principle was 'even more important: it is the idea of a '*minimum standard'*: the scales of benefit are worked out on the basis of the cost of purchasing the basic needs of life' (*ibid.*, p. 5; original stress). All this was expected to cost £265 million per year, with £125 million coming in contributions from insured persons, £86 million from the government and £54 million from employers' contributions. The latter was effectively a tax on wages, which would fall, 'like any indirect tax, on the consumer, who pays it in the form of higher prices' (*ibid.*, p. 10); it would add only about 1 per cent to the cost of consumer goods. The Beveridge proposals as a whole were financially very modest, Kaldor concluded: 'The whole burden of the social security plan is not much more than the *increase* in the national income in a single year which is due to the normal rate of progress of society in peace-time' (*ibid.*, p. 9; original stress). It was a price well worth paying, and even this might prove to be exaggerated, since the improved health and efficiency of the population would raise aggregate output. At the very least, this would far outweigh any loss from increased malingering (*ibid.*, p. 9).

Kaldor made the same point in his *Economic Journal* assessment of the social security proposals:

> the cost of Beveridge to the taxpayer will be a '1*d*. on beer and 6*d*. on the income tax' – a very moderate sacrifice, indeed, for the abolition of want. It is less than 1.2 per cent of the average incomes of all classes of the community; less than 1.6 per cent of the average 'disposable incomes' – the incomes remaining after all other taxes and compulsory levies have been paid. (Kaldor 1943a, p. 18)

Assuming that full employment could be maintained after the war, the cost to the taxpayer would be more than met by savings on unemployment relief. The employee contribution could not be regarded as a significant burden, since 'insurance against sickness, old age, unemployment, etc. .... [would provide] tangible benefits for which the insured would be quite ready to pay voluntarily, if the opportunity were offered to him' (*ibid.*, p. 24).[5]

Kaldor did criticise the reasoning behind the employer contribution:

> If it is intended that its incidence should fall on the employees, it would be much better to charge it to the employees openly, and to abolish the employers' contribution altogether. If it is intended, on the other hand, that it should be a charge on the employers – that it should fall on profits, and not on wages – it is no use levying a tax which enters into prime costs; it should be raised as a tax on profits, and not in the form of a tax on employment. (*ibid.*, pp. 26–7)

As he argued in his contribution to the National Peace Council's 'Peace Aims Pamphlet', *Planning for Abundance*, the Beveridge plan involved 'practically no re-distribution of income from rich to poor. That does not mean that the Plan is not a good thing, but it should not be regarded in any way as a measure towards socialism – a measure which will make the income distribution of this country more equal' (Kaldor 1943b, p. 26). Overall, Beveridge offered very large social benefits in exchange for very small costs. 'There is really little cause to be afraid' (Kaldor 1943a, p. 24).

3.6 Beveridge II: Full employment in peacetime

Kaldor had concluded his radio broadcast with the following words:

> But we must also remember that social security is not everything. To get the 'world we want' it must be part of a larger pattern of reform – which includes better housing, better education, and the provision of full employment.
>
> You can think of other things for yourself. All I would like to say in conclusion is that this last reform – full employment – is perhaps the most important of all. For it would not only remove a great evil; it would also make us much more prosperous and able to afford more easily all the other things that we want. (Kaldor 1944b, p. 12)

Thus his final contribution to the debate on postwar reconstruction was to extend the macroeconomic principles of war finance to the long-term problem of maintaining full employment, without undue inflation, after the war. This was the subject of the second Beveridge Report, *Full Employment in a Free Society*, which was published in 1944 and for which Kaldor wrote a lengthy technical appendix (Beveridge 1944; Kaldor 1944a). Along with Fritz Schumacher, in fact, he was Beveridge's chief economic adviser on the full employment project (Wood 1984, p. 162).

Kaldor's starting point was, once again, the principle of functional finance. He rejected what he described in an article that he wrote for *The Times* in March 1943 as the 'blind fetish worship of the "balanced budget"' (Kaldor 1943c, p. 5).[6] Instead, fiscal policy must be 'so regulated as to secure adequate total outlay for the community as a whole' (1944a, p. 345). There were four ways in which this could be done, on the assumption that there was no government interference with private business investment decisions:

> The first is by increased public expenditure covered by loans; the second is by increased public expenditure covered by taxation; the third is by increased private spending brought about through remission of taxation, and the fourth is by increased private spending brought about through changing the incidence of taxation or imposing a combined system of taxes and subsidies. The first two methods imply that idle resources are primarily absorbed for purposes that are determined by, or are under the control of, the State; the last two that they are absorbed in uses determined by private citizens. (*ibid.*, p. 345)

These were the four routes to full employment.

His first contribution to the Beveridge Report was to make precise calculations as to what they would each have implied for the achievement of full employment in 1938, in an economy where national output was thereby increased by 11 per cent (from £4675 million to £5175 million). This would have required a corresponding increase of £500 million in aggregate expenditure, from public and/or private sources as specified by the four routes previously mapped out. Route I held tax rates constant and raised government spending, 'to the extent necessary to secure adequate total outlay', and involved a budget deficit. Route II raised government spending and taxation in equal amounts, retaining a balanced budget. Route III held government expenditure

constant and relied exclusively on tax cuts; again the budget would be in deficit. Route IV kept the budget in balance, but changed the structure of taxes in such a way as to increase consumption spending, and reduce taxation, by the necessary amount (*ibid*., p. 361). Kaldor added two variants: route IIa, in which only direct taxation was increased, and route IIIa, in which tax cuts were restricted to indirect taxation. Route IV could then be disregarded, as it was 'a virtual combination of Routes IIa and IIIa' (*ibid*., p. 362).

To make the necessary calculations Kaldor had to consider the effect of increased government spending, and various types of changes in taxation, on aggregate consumption expenditure, and therefore on saving; he had also to estimate the impact of higher incomes on spending on imports. Although he did not (quite) use the terminology, what he was doing here was to estimate the marginal propensities to consume and to save out of wages and profits (*ibid*., p. 357), and the marginal propensity to import (*ibid*., pp. 358–9).[7] He had also to guess at the increase in private investment spending that might be associated with an 11 per cent increase in total output (Kaldor 1944a, pp. 360–1). He could then begin his assessment of the various routes to full employment. The actual level of government spending in 1938 was £800 million, of which £725 million had been covered by tax revenues and £75 million (the budget deficit) had been financed by loans. Full employment could have been achieved in 1938 by any one of the five routes (I, II, IIa, III and IIIa). The fiscal implications of each of them are shown in Table 3.1.[8] It can be seen from the Table 3.1 that the five routes to full employment had very different consequences for the government's finances. Three of them (I, III, IIIa) required budget deficits, and the two that did not (II, IIa) involved extremely large increases in government spending.

Table 3.1 Five routes to full employment in 1938

	Route I	**Route II**	**Route IIa**	**Route III**	**Route IIIa**
Govt. spending	1090	1710	1435	800	800
Taxation	860	1710	1435	460	515
Deficit	230	0	0	340	280
Govt. spending	290	910	635	0	0
Taxation	135	985	710	−265	210
Deficit	155	−75	−75	265	210

Source: Derived from Kaldor 1944a, p. 363, table 46. All figures are in £million.

So much for 1938. The situation would be quite different after the war, when the needs of private industry,

> together with the higher ratio of exports to imports, are likely to set up, for a number of years, a demand for labour that will be much more closely related to the available supply than was the case before the war. This might enable a full employment policy, for a time, to be consistent with budgetary surpluses, rather than public borrowing. But taking a longer view, there appears to be no reason why the employment problem should not again present itself in much the same aspects as in the 1930s; and once this stage is reached, the practical methods of maintaining full employment will again be the creation of loan expenditure, either by increasing public outlay, or by lowering taxation. (*ibid.*, p. 348)

The second and third parts of Kaldor's contribution to the Beveridge Report thus dealt with the elimination of the 'inflationary gap' (*ibid.*, p. 368) that was at first to be expected after the war, and with the long-run consequences of the public borrowing that would be needed to eliminate the subsequent deflationary gaps.

Supposing that the transition to a peacetime economy had been completed by 1948, Kaldor repeated his calculations on the assumptions that both the distribution of income between wages and profits and the UK's terms of trade were unchanged from their 1938 levels. He expected labour productivity to be 13 per cent higher, and income from foreign investments 40 per cent lower, than before the war (*ibid.*, p. 369). One very important institutional change was assumed to occur:

> Our hypothesis is that the Government, through a National Investment Board, will so regulate the rate of capital expenditure (by fitting together the investments undertaken by public authorities and by private industry into a common national plan) as to ensure stability and adequacy in the national outlay as a whole. (*ibid.*, p. 388)

Kaldor now calculated the implications of three alternative investment plans, each of them designed to achieve full employment without inflation. In plan I, total net investment (public and private) would be £765 million in 1948, compared with the 1938 level of £610 million; in plan II, it would be £1000 million; and in plan III, £1333 million (all at 1948 prices, which were expected to be one-third higher than in 1938). Plans II and III would allow a much more rapid programme of house-building

and much higher levels of business fixed investment than would be possible under plan I. On Kaldor's estimates, 'each of these plans is consistent with a higher level of private real consumption than obtained in 1938, and would thus leave the community better off, in terms of current standard of living, than they were before the war', by 19 per cent, 14 per cent and 7 per cent respectively (*ibid.*, p. 391). Plans II and III would require large budget surpluses and correspondingly steep increases in tax rates (by 20% and 49%):

> But in the case of Plan III at any rate, the required increase in taxation is so stiff – it implies an income tax of 8s. 8d. [43.3%], instead of 5s. 10d. in the £ [29.2%], if all Central Government taxes were raised proportionately – that it might be preferable, in this case, to secure the required reduction in consumption (at least in part) by other means of control, such as rationing. (*ibid.*, pp. 391–2)

Finally, Kaldor turned to the problem of the long run. Real incomes would rise over time, due to the combined effects of continued capital accumulation and technical progress. Measures would then be necessary to ensure that consumption grew at the appropriate rate. This might well involve 'more radical methods of income redistribution', since 'it will no longer be possible to afford the degree of inequality of incomes that can be sustained during the period of relatively high investment'. In all likelihood this would entail deficit financing, and the final section of Kaldor's Appendix was thus devoted to analysing 'the effects of a policy of continuous borrowing under peace-time conditions' (*ibid.*, p. 393). It has a remarkably modern ring.

What, Kaldor asked, was the 'real burden' of a growing national debt? His answer was rather more cautious than it had been in 1939 or 1942, since he now recognised the possibility of disincentive effects.[9] On the (admittedly highly unrealistic) assumption that the national debt was equally distributed among the population at large, a growing ratio of debt to income would mean that the composition of each citizen's income would change, with a growing proportion coming as 'rent' (that is, interest payments from the government) and a declining proportion from productive effort. A case could therefore be made for stabilising the ratio of debt to income to avoid discouraging effort. What would this mean for the British economy over the 25 years after 1948? Kaldor's analysis was numerical rather than algebraic, but it effectively established the twenty-first-century rules for 'fiscal sustainability' (Burger 2003). The critical relationship, he maintained, was that between the

rate of growth of output and the real rate of interest. Kaldor assumed the latter to be fixed at 2 per cent, with a nominal interest rate of 2 per cent and a stable price level established by suitable 'monetary and wages policy' (*ibid.*, p. 398). The rate of growth of output would depend on changes in three factors: the working population, average hours of work and output per hour. Kaldor expected that the first two factors would work to reduce output, since low fertility would cause the working population to fall (shades of our own allegedly looming 'ageing crisis'!) and hours of work would continue to decline, perhaps by 10 per cent over the quarter-century under consideration. Productivity, however, would grow, in particular because 'the past tendency towards an exorbitant number of people entering the field of distribution might be arrested'.[10] On balance, output might increase by 1 per cent per annum.

Now come the numerical estimates. In 1948 GDP would be £8450 million, at 1948 prices, while interest payments on the national debt were expected to amount to 6 per cent of GDP, or £500 million. (With the rate of interest set at 2%, this implied a national debt of £25 billion or approximately three time annual output). Output would grow by approximately £90 million each year. To keep the ratio of interest payments to GDP constant, the debt could grow by £250 million per annum (since 0.02 × £250 million = £5 million, and 5/90 = roughly 6%). If the working population began to grow again after 1970, as demographers predicted, this figure would rise. At all events,

> the contention that a policy of increasing the National Debt in peace time involves a steadily increasing potential burden on the taxpayer is very far from the truth. This could only be the case with a rate of borrowing that is far in excess of anything that might be necessary under peace time conditions in order to sustain a full employment policy. (*ibid.*, p. 400)

Kaldor might also have emphasised the important role played by cheap money in these calculations.[11]

Looking back on his 1944 forecasts in 1964, in the introduction to volume 1 of his collected *Essays on Economic Policy*, he noted contemporary criticisms by the econometricians Richard Stone and A. G. Hart (Hart 1945; Stone and Jackson 1946), and acknowledged 'important errors ... which I can only attribute now to youthful exuberance' (Kaldor 1964d, p. xi). He had underestimated the negative effect of wartime dislocation on labour productivity, Kaldor admitted, and had taken an overly optimistic view of Britain's postwar terms of trade. 'These two

factors explain the discrepancy between the 20-per cent rise in real national income that was forecast and the 4-per cent increase which actually occurred, and this in turn was responsible for the erroneous conclusions on taxation' (*ibid.*, p. xii).

3.7 After the war

Evidently Kaldor's wartime socialism was cautious and moderate. He seems to have been uninterested in any significant extension of public ownership or in the introduction of comprehensive microeconomic planning. Even in 1942–1943, when wartime radicalism was at its peak, he advocated limited reforms to a largely unchanged capitalist economy, reforms that (he implied) would be agreed upon by all reasonable and intelligent citizens. There is no trace, in Kaldor's wartime writings, of the political tensions that dominated the thinking of contemporaries like Joan Robinson and Michal Kalecki. Robinson detected alarming echoes of Fascism in the growth of business support for economic planning, while Kalecki famously predicted the rise of capitalist opposition to full employment, which would come to be viewed as a threat to 'discipline in the factories' (Robinson 1942; Kalecki 1943; cf. King 2004a).

At a more technical level, Kalecki reported that his own independent statistical assessment of the prospects for full employment in postwar Britain was 'not much divergent' from Kaldor's (Kalecki 1944b, p. 285 n1). The Polish economist was less inclined to believe that that stimulating private investment offered a viable route to full employment, since the effects 'depend…on the reaction of entrepreneurs, and it is quite possible that when they are in a very pessimistic mood they may not respond even to considerable inducements. This may happen, for instance, if they do not feel confidence in the political situation' (Kalecki 1944a, p. 53). Kalecki therefore favoured the two alternative solutions: deficit-financed public investment and public consumption, and the redistribution of income from rich to poor. There is none of this in Kaldor's wartime writings. Oxford and Cambridge seem not to have communicated on these important issues; at any event, there is no record of Kaldor having responded to Kalecki's arguments. On the contrary, the tone of his own wartime writings was consistently (indeed, remarkably) optimistic.

His personal circumstances were less cheerful. Relations with Hayek had long been strained, as can be inferred from the ill-tempered exchanges between the two men in *Economica*, where Kaldor attacked the Austrian's almost unintelligible writings on the trade cycle and

Hayek complained of systematic and apparently wilful misunderstanding (Hayek 1942; Kaldor 1942c). This was not the way for a young lecturer to advance his career at the LSE. The atmosphere in Cambridge was much more to his liking. Indeed, Kaldor had been unsettled at LSE for some time:

> My later years at L.S.E. in the 1930s were not altogether happy. Though the place never lacked intellectual stimulus – and there was plenty of opportunity to expound one's views in Lionel Robbins' weekly seminars – I felt out on a limb as an early and enthusiastic supporter of Keynes, and out of sympathy with the rigid neo-classicism of Robbins, Hayek and most of the senior members of the economics department. Though L.S.E. was always regarded as 'left-wing' by outsiders, this was an image largely created by the 'media'. During the period while I was there, 'left-wing' views were confined to Harold Laski and a few lecturers in law and sociology. The economics department was dominated by those who held orthodox views both on money and the functioning of a free market system – an ideology which I embraced for a brief period, but abandoned well before the appearance of Keynes' *General Theory*. (Kaldor 1980a, p. xxi n2)

His difficulties with Hayek continued during the war. The Kaldor papers include a remarkable exchange of letters in 1942–1944, which contain what is probably the only instance in recorded history of an academic demanding to be given *more* examination scripts to mark. In January 1942 Kaldor complained to his friend and patron, Harold Laski, that Hayek had excluded him from examining duties. This provoked Hayek to tell Laski that 'Kaldor thinks himself too good for the work he is asked to do', neglecting his teaching commitments and failing to report on the students he was supposed to be supervising. Kaldor complained to the Director of the LSE, Alexander Carr-Saunders, who managed to smooth things over,[12] but the grievance rankled enough for him to engage in a public row with Hayek two years later; this time Lionel Robbins intervened, on Hayek's side. Kaldor now placed a sinister construction on the behaviour of his two adversaries: the reappointment of the same examiners, year in, year out, 'inevitably compelled students to concentrate their work on a particular [Austrian?] approach to economics, and excluded others' [Keynesians?].[13]

Things could not go on like this for much longer. Significantly, Kaldor did not move back to London when the LSE returned there at the end of the war. Instead he remained in Cambridge and commuted to carry

out his teaching duties. Official responsibilities took up more and more of his time. In 1945 he was seconded to the United States Strategic Bombing Survey, where he worked with a talented team of economists that included John Kenneth Galbraith (the Survey's director), Paul Baran, Edward Denison, Fritz Schumacher and Tibor Scitovsky (Galbraith 1979, pp. 219–20; 1981, p. 210). In the following year Kaldor worked briefly at the Ministry of Defence as adviser to the British Bombing Survey, and also advised the Hungarian government on postwar reconstruction. In 1947, after providing the French government with a report on the reform of taxation, he was approached by Gunnar Myrdal to join the United Nations Economic Commission for Europe in Geneva. Kaldor was refused leave of absence from LSE and so resigned, after an association that had lasted exactly 20 years.[14] While in Geneva in 1947–1949 he largely wrote two of the Commission's annual 'Economic Surveys of Europe', served as adviser to the United Nations Technical Committee on Berlin Currency and Trade, and wrote much of the 'Report on National and International Measures for Full Employment' for another United Nations expert committee.[15]

Much though he enjoyed having access to the corridors of power, Kaldor was not cut out for a career in the public service, national or international; he valued his freedom too much for that. So he sought an early return to academic life, preferably in Cambridge. As early as 1943 Keynes had proposed him, along with Richard Stone, as a Fellow of King's (Skidelsky 2000, p. 160). Two years later a proposal to appoint Kaldor to a Cambridge lectureship was rejected by 4 votes to 2, Piero Sraffa and the philosopher Richard Braithwaite voting in favour and Dennis Robertson, Gerald Shove, C. R. Fay and J. W. F. Rowe opposing him (Marcuzzo 2004, p. 16 n35). Kaldor seems to have been seriously attracted by the prospect of becoming the first professor in the newly established economics department at the Australian National University in Canberra, but eventually decided against the move (Cornish 2007a). Finally, in 1949, he was appointed to a Fellowship at King's, at the instigation of Richard Kahn and Joan Robinson, and also to a University lectureship. In October 1949 Kaldor returned to Cambridge, to academic life – and to economic theory.

4
A Return to Theory

4.1 Generalising the *General Theory*

Back in Cambridge, Kaldor soon resumed old friendships. 'For many years', Ferdinando Targetti records, 'the Kaldors, Sraffa, [Joan] Robinson and Kahn spent their summer holidays together in the Alps and the Scandinavian mountains: with long walks for the Kaldors and adventurous climbs for the others' (Targetti 1992, p. 13). Inevitably economics was discussed, abroad and at home on the regular Sunday walks from Cambridge to Grantchester. It would be a slight exaggeration to say that when Kaldor took up his fellowship at King's he had been away from economic theory for almost a decade. For one thing, he had continued to lecture on the theory of employment, and also on value and distribution theory, on which he applied the 'history of thought' perspective that was common then. Hugh Dalton was told that his lectures were 'a Box Office draw', comparable to those of the celebrated Noel Annan (Dalton 1986, p. 576).

One product of his teaching responsibilities was a *Chambers' Encyclopaedia* entry on 'Income Distribution'. He approached the problem historically, outlining the theories of Smith and Ricardo before discussing the subsistence theory of wages, the work of Karl Marx and the contributions of the marginal productivity theorists. Kaldor concluded by emphasising the weaknesses of neoclassical distribution theory. 'Serious and [as] yet unresolved difficulties however confront any attempt to find a quantitative measure of "capital" in terms of the production period, whose marginal productivity, on the suppositions of the theory, determines the rate of interest'. Moreover, the new mark-up pricing models had important implications for the relative shares of wages and profits, which had yet to be worked out. 'Thus the problem

posed by Ricardo, the discovery of "the laws which regulate distribution", still eludes the grasp of economists' (Kaldor 1950, p. 556).

For Kaldor this was an unusually negative conclusion, perhaps reflecting the depth as much as the length of his wartime shift away from economic theory. His membership of the Cambridge 'war circus' had, of course, exposed him to the theoretical musings of Joan Robinson, Richard Kahn and Piero Sraffa (though the secretive Italian gave very little away about his own long-term efforts to rehabilitate Ricardian economics). Kaldor's main interests throughout the 1940s were, however, in applied economics and policy issues. By the beginning of the 1950s he had a lot of theoretical work to catch up on.

The most pressing analytical problem in macroeconomics was what Robinson (1952) termed 'the generalization of the *General Theory*'. Keynes had deliberately confined his analysis to the short period, in which investment was allowed to increase aggregate demand but not to add to productive capacity. He had been criticised for this at the time (Pigou 1936). It was a simplifying assumption, made in order to keep the argument manageable in much the same way that Keynes had restricted the *General Theory* to the unrealistic but much more tractable case of a closed economy.

The first attempt to extend the analysis to the long period came not from Cambridge but from Oxford, where Roy Harrod had tried repeatedly to incorporate capital accumulation into a Keynesian model of the business cycle (Harrod 1936, 1939, 1948).[1] Harrod distinguished the actual rate of growth from the maximum or 'natural' rate (given by population growth and technical progress), and both the actual and natural rates from what he termed the 'warranted rate' (that rate of growth at which entrepreneurs were satisfied with the outcome of their investment decisions). There was no obvious reason why these three rates of growth should be equal, and this posed potentially serious problems both for the capitalist economy and for economic theorists. While Harrod intuitively grasped what Michał Kalecki described as 'the tragedy of investment',[2] he proved unable to construct a satisfactory model of cyclical growth (Besomi 1999). He did, however, identify a possibly very dangerous source of instability. If the warranted growth rate were to diverge from the natural rate, the logic of Harrod's argument suggested that the economy would move further and further away from it. Harrod himself disliked the description of this situation as a 'knife-edge', but the graphic metaphor stuck.[3]

The difficulty of course was, as Keynes himself had put it, that capitalism was not normally violently unstable. The Great Depression

of the 1930s had been the exception, not the rule. As E. J. Mishan subsequently put it, reviewing the second volume of Kaldor's *Economic Essays*: 'Harrod's knife-edge equilibrium growth can therefore be regarded either as an interesting science-fiction *motif* or else as a starting point to the question which should leap at once to the mind: what is the nature of the interacting mechanism that in fact prevents the economy from doing the ridiculous things that such simple equations would have it do?' (Mishan 1962, p. 88). So what was it that prevented the economy from contracting – or expanding – without limit? In the early 1950s many of the best minds in economics all around the world were focused on this question. Two quite distinct answers emerged, one orthodox or neoclassical and the other heterodox or 'Post Keynesian'.[4] The neoclassical solution was developed independently by the American, Robert Solow, and the Australian, Trevor Swan. It relied upon capital-labour substitution in response to changes in relative factor prices. In Harrod's growth equation, $g = s/v$, where g is the rate of growth, s the savings ratio and v the capital-output ratio, and both s and v are assumed to be constant. In the neoclassical growth model v becomes a variable, while in the Post Keynesian solution to Harrod's problem it is s that varies, not v. Capitalists have a much higher propensity to save than workers, so that a redistribution of income from wages to profits, which might be expected to occur in a strong boom, will raise the average propensity to save.

4.2 Trend and cycle revisited

All this emerged only in the mid-1950s, with the publication of the Swan and Solow models and of Joan Robinson's *magnum opus*, *The Accumulation of Capital* (Robinson 1956). To begin with, at least, Kaldor's theoretical interests were slightly different. His first substantial theoretical contribution after his return to Cambridge was a review article on J. R. Hicks's book, *The Trade Cycle* (Hicks 1950). Central to his criticism of the book was the relationship between cycles and growth: 'Mr. Hicks rightly stresses at the beginning that since historically cyclical fluctuations took place against the background of a rising trend of output, a theory of the cycle ought to be built around a dynamic theory of economic development rather than in a static framework' (Kaldor 1951b, p. 841). This was also Harrod's position. Both Hicks and Harrod stressed the importance of entrepreneurial expectations, 'which suggests that an economy is likely to grow at the rate at which its business men expect it to grow' (*ibid.*, p. 842). This was evidently true, but it was not very

enlightening. 'For the time being, however', Kaldor concluded, 'it cannot be said that the problems of a theory of dynamic development embracing both trend and fluctuations has [*sic*] as yet been solved, though both Mr. Harrod and Mr. Hicks have made important contributions to the mechanics of such a theory' (*ibid.*, p. 846).

Kaldor returned to these questions in 1954, again focusing on entrepreneurial expectations. This now led him to reverse the procedure adopted by Hicks, Harrod and many other theorists of inserting a trend rate of growth into an otherwise static or trendless model of the cycle. On the contrary,

> so far from the trend rate of growth determining the strength or duration of booms, it is the strength and duration of booms which shapes the trend rate of growth. It is the economy in which business-men are reckless and speculative, where expectations are highly volatile, but with an underlying bias towards optimism, where high and growing profits are projected into the future and lead to the hasty adoption of 'unsound' projects involving over-expansion, which is likely to show a higher rate of progress over longer periods; while it is an economy of sound and cautious business-men, who are slow at reacting to current events, which is likely to grow at a slow rate. (Kaldor 1954, pp. 68–9)

The article concluded on an uncharacteristically pessimistic note. Differences in the rate of growth across time and space, Kaldor suggested, were due very largely to differences in 'human attitudes to risk-taking and money-making'. In any attempt to apply economics to these great issues the theorist unavoidably 'trespasses on the fields of sociology and social history; and the most that an economist can say is that there is nothing in economic analysis as such which would dispute the important connection, emphasised by economic historians and sociologists, between the rise of Protestant ethic and the rise of Capitalism' (*ibid.*, p. 67). Even in the most recent phase of capitalist development, 'both the trade cycle and economic growth are the resultant of particular attitudes of entrepreneurs – more precisely, of the volatility of entrepreneurial expectations' (*ibid.*, p. 70). This significantly limited the power of formal economic analysis. 'The same forces therefore which produce violent booms and slumps will also tend to produce a high trend-rate of progress; though the connection between the two is far too complex to be reducible (at present) to a simple mechanical model' (*ibid.*, p. 70).

4.3 Alternative theories of income distribution

These reservations were not, in the final analysis, strong enough to deter Kaldor from attempting to model the process of economic growth (he would certainly have denied that his own models were either simple or mechanical). First, though, he needed a formal theory of income distribution. This duly appeared in his celebrated 1956 paper in the *Review of Economic Studies*, but there had been hints of it earlier (Kaldor 1951b, pp. 838 n2, 845). The underlying argument was very lucidly explained in a paragraph that Kaldor added to the 1966 version of his *Chambers's Encyclopaedia* entry. It was

> based on the idea (which is the crucial innovation of the Keynesian system of economics) according to which it is the expenditure decisions of those whose power to spend is not confined by current receipts (because they either possess financial assets or unexhausted borrowing power that is large in relation to their expenditure in a given period) which has [*sic*] a critical role to play in the determination of the aggregate demand for goods and services in the economy.... [Hence] the share of profits, given the propensities to save and spend of the entrepreneurial classes and of the wage and salary earners, will tend to be such as to generate sufficient 'savings' to finance the 'investment' which entrepreneurs have decided to undertake. Since the proportion of profits which is currently saved is normally about ten times as high as the proportion of savings in wages and salaries (the former is of the order of two-thirds, whereas the latter only of six to seven per cent) the proportion of savings in the national income is highly responsive to changes in the share of profits in income. (Kaldor 1966b, p. 561)

In the short period the share of investment in national income could be taken as fixed; in the long period, however, it was governed by the rate of economic growth:

> On this theory, therefore, it is the factors which determine the trend rates of economic growth (technical progress and population growth) which mainly determine the distribution of income between profits and wages, or between property and work. The theory thus serves to explain the long-observed fact (which puzzled several generations of economists) that distributive shares are constant over long periods whilst they fluctuate over shorter periods (in other words, in the long

> run real wages rise at much the same rate as productivity per worker) as well as the fact that in fast-growing economies the share of profits is generally appreciably greater than in economies which grow at a relatively slow rate. (*ibid.*, p. 561)

Set out like this, the 'Cambridge theory of distribution' was by no means new. The process involved when investment expenditure rose and income was redistributed towards profits in order to provide the necessary increase in saving was known to mainstream theorists in the 1920s and early 1930s, from Wicksell to Hayek. It was somewhat misleadingly described as 'forced saving' (Hansson 1987). Another Cambridge economist, the Marxist Maurice Dobb, used a similar line of argument to criticise marginal productivity theory in an unjustly neglected early article that Keynes had published in the *Economic Journal* (Dobb 1929). Keynes's own discussion of the 'widow's cruse' parable in the *Treatise on Money* made a very similar point (Keynes 1930, Volume I, p. 129), and the same principle had also been enunciated by Michał Kalecki (1939 [1990], pp. 258–61; 1942). Although Kaldor was not close to Dobb, who was a loyal lifelong member of the Communist Party of Great Britain, he was of course familiar with Keynes, Kalecki, Wicksell and (especially) Hayek. But it took him a long time to make the connection between distribution and growth.[5]

Kaldor presented the first draft of his paper in Cambridge at a meeting of the so-called 'secret seminar'[6] in October 1955, just before he left on an extended world tour that would take him to India, China, Japan, Chile and the United States. Precisely the same argument appeared in Joan Robinson's book (1956, pp. 405–6), though it was not a particularly prominent part of her broader analysis. She herself was unimpressed by Kaldor's seminar presentation. As she told Richard Kahn,

> We had Nicky at [*sic*] Tuesday. His aim was to get his own theory clear before reading mine. It was quite a good evening but he was a bit disappointed that he has not got any further. He set out to find a theory of the constant relative shares and after going after Ricardo, Walras etc. (this was quite fun) came down for the Widow's Cruse theory that given p-to-consume out of wages and profits each separately the shares are determined by the rate of investment. But when about 11.15 I asked him how he got the share of investment constant he did not seem to have any views. I fear it will be a long time yet before he has 'figured out' enough for it to be possible to argue about the fine points. Meanwhile I amuse myself explaining it to the

> research students....The general view seems to be that Nicky hadn't much to say that wasn't obvious. (Robinson to Kahn, 26 October 1955, cited in King 1998b, p. 418)

However, Kaldor persevered. The final paper was very widely read; over the next twenty years it was reprinted, in full or in part, in half a dozen volumes of collected readings in economic theory.[7] After an extended critical discussion of the classical, Marxian, neoclassical and Kaleckian or 'degree of monopoly' theories of distribution, Kaldor set out his own, 'Keynesian' theory in simple algebra. The model describes a capitalist economy in which total income (Y) is distributed between wages (W) and profits (P); investment (I) is equal to saving (S), which is the sum of saving out of profits (S_p) and out of wages (S_w). Kaldor thus wrote three identities: $Y \equiv W + P$, $I \equiv S$ and $S \equiv S_p + S_w$. With s_p and s_w as the propensities to save out of profits and wages respectively (both assumed to be constant), it followed that

$$I = s_p \cdot P + s_w \cdot W = s_p \cdot P + s_w \cdot (Y - P) = (s_p - s_w)\, P + s_w \cdot Y, \text{ from which}$$
$$I/Y = (s_p - s_w)\, P/Y + s_w, \text{ and}$$
$$P/Y = 1/(s_p - s_w) \cdot I/Y - s_w/(s_p - s_w).$$

'Thus', Kaldor concluded, 'given the wage-earners' and the capitalists' propensities to save, the share of profits in income depends simply on the ratio of investment to output' (Kaldor 1956a, p. 95). As he noted, the model worked only if $s_p > s_w$. In the special case where $s_w = 0$, the profit share depends only on the savings propensity of the capitalists and the ratio of investment to income. In this case, $P/Y = 1/\, s_p \cdot I/Y$. 'The critical assumption', Kaldor continued, 'is that the investment/output ratio is an independent variable' (p. 96). He provided a simple numerical example. If I/Y is 20%, $s_w = 0$ and $s_p = 50\%$, it follows that $P/Y = 40\%$; an increase in I/Y to 21% will thus increase P/Y to 42% (*ibid.*, p. 96 n2).

Kaldor briefly discussed the complications that arose if there was a minimum level below which the real wage could not fall; a minimum rate of profit on capital; and a minimum rate of profit on turnover (*ibid.*, pp. 97–8). He concluded by setting out the implications of his model for the theory of economic growth. The investment–output ratio now became a variable, given by the relationship between the rate of growth of output capacity (G) and the capital–output ratio (v). Since $v = K/Y$ and $G = I/K$, $I/Y = Gv$. This was Harrod's first equation. Kaldor rewrote Harrod's second equation, $s = I/Y$, in terms of his own theory of distribution, that is, as $I/Y = (s_p - s_w) \cdot P/Y + s_w$. 'Hence the "warranted" and the "natural" rates of growth are not independent of one

another; if profit margins are flexible, the former will adjust itself to the latter through a consequential change in P/Y' (*ibid*., p. 97). This did not mean that steady growth was inevitable. On the contrary, 'the process of growth' might break down, in which case 'the economy will relapse into a state of stagnation'. This, Kaldor argued, might occur for several reasons. Entrepreneurs might be too pessimistic; an excessive degree of liquidity preference might put too high a floor under the rate of profit on capital, which (owing to uncertainty) must always exceed the rate of interest; and inadequate competition might lead to 'over-saving' because of excessive profit margins. If none of these difficulties arose, 'there will be an inherent tendency to growth and an inherent tendency to full employment. Indeed the two are closely linked to each other' (*ibid*., p. 99).

This last point was to prove extremely contentious, in what was supposed to be a 'Keynesian' model of distribution and growth. However, Kaldor did not dwell upon it. Instead, his final sentence took a shot at neoclassical distribution theory. 'I am not sure where "marginal productivity" comes in in all this', he wrote, 'except that in so far as it has any importance it does through an extreme sensitivity of v to changes in P/Y' (*ibid*., p. 100). The defenders of neoclassical orthodoxy could not ignore this calculated insult. A torrent of criticism soon flowed Kaldor's way, including James Tobin's sardonic proposal for a '*general* Kaldorian theory of distribution' in which the original two-class model was replaced by '*n* mutually exclusive classes.... Actors, Bird-watchers, Conservative peers' and so on, each with a different savings propensity (Tobin 1960, p. 120). More serious objections came from Franco Modigliani and Paul Samuelson (1966), who defended the marginal productivity theory of distribution and objected that Kaldor's results were valid only under particular (and unrealistic) values of his parameters.[8] Kaldor's response to these criticisms was a simple one:

> A capitalist system can only function so long as the receipts of entrepreneurs exceed their outlays; in a closed system, and ignoring Government loan expenditure, this will only be the case if entrepreneurial expenditure exceeds workers' savings. Unless one treats the consumption expenditure of entrepreneurs as an exogenous variable, given independently of profits, it is only the 'Kaldor-Pasinetti inequality' (i.e. the excess of business investment over non-business savings) which can ensure the existence of profits. (Kaldor 1978c, p. xvi)

Kaldor's neoclassical critics never seemed to grasp this essential, and rather obvious, truth.[9]

There was also friendly fire, which came from an Italian economist with strong Cambridge connections. Luigi Pasinetti (1962) pointed out that there was a logical flaw in Kaldor's analysis. If workers saved they acquired assets, and presumably earned a return on them. This meant that they had both labour income and property income, so that the dichotomy between the two classes broke down, and with it the elegant simplicity of the model. Pasinetti demonstrated, however, that this problem could be overcome, restoring Kaldor's original conclusion that the rate of profit depended on the capitalists' savings propensity alone, even when workers did save. His paper spawned a huge literature, much of it by fellow Italians, including Kaldor's own 'neo-Pasinetti theorem' that allowed for workers' saving through pension funds and for the issue of securities by corporations (Kaldor 1966c, pp. 316–19). The original model was eventually further extended to include an overseas sector, government expenditure and taxation, and a rate of interest lower than the rate of profit (Panico and Salvadori 1993), and also accommodated the phenomenon of retained business earnings (O'Connell 1995). It constituted a coherent alternative to the unsatisfactory neoclassical analysis of distribution, which invoked the nebulous concept of a 'well-behaved', twice-differentiable aggregate production function whose parameters alone determined the relative shares of labour and capital. The Cambridge (UK) critics demonstrated that such neoclassical models were likely to generate paradoxical results – 'reswitching' and 'capital reversal' – that could be avoided only if they were assumed away. By 1966 the neoclassicals appeared to have conceded defeat (Samuelson 1966), but this did not prevent them from continuing to defend the marginal productivity theory of distribution or from employing aggregate production functions in their later models of economic growth.[10]

4.4 Kaldor on growth: Mark I

By 1957 Kaldor had overcome his earlier reservations about the possibility of a precise, formal model of the growth process, and was now ready to produce his own.

It contained elements from his 1954 and 1956 articles, but went far beyond them in several ways. He began by asserting that any acceptable theory of growth must be able to explain 'the remarkable historical constancies revealed by recent empirical investigations' (Kaldor 1957b, p. 591).[11] In the very long run, in a number of advanced capitalist economies, the wage and profit shares in national income, the capital–output

ratio and (in consequence)[12] also the rate of profit on capital all seemed to be roughly constant. Existing (neoclassical) theory could not account for these constancies, except as the result of a series of implausible historical accidents.

The first and most controversial of the 'basic properties' of Kaldor's model was the assumption of full employment. As in 1956, he asserted baldly that 'an equilibrium of steady growth is inconsistent with an under-employment equilibrium' (*ibid.*, p. 594). This apparent endorsement of Say's Law in the long period led to Samuelson's unkind but understandable quip about 'Jean Baptiste Kaldor' (Samuelson 1964, p. 345). Kaldor continued to defend this position, arguing as follows in his introduction to the second volume of his *Collected Economic Papers*, in which the 1957 article was reprinted:

> Fruitful as it has proved in the analysis of short-period fluctuations, the Keynesian under-employment hypothesis which assumes that output at any one time is limited by demand and not by supply factors, is obviously inappropriate to the analysis of conditions in a prolonged boom. Such a boom may be taken as a first approximation to the conditions one is seeking to isolate in studying growth; and I now believe it is more promising to regard the cyclical character of growth in capitalist economies as caused by interruptions to the growth process, which must be explained in a second approximation, than to root the study of growth in the mechanism of the cycle. (Kaldor 1960b, pp. 12–13)

This notion of growth as 'a prolonged boom' remained central to his thinking for several years.

The second basic property of the model was that Kaldor now 'eschews any distinction between changes in techniques (and in productivity) which are induced by changes in the supply of capital relative to labour and those induced by technical invention or innovation'. More capital per worker, he argued, almost inevitably involved improved technology, while technical progress generally had to be embodied in new capital equipment. Thus the orthodox distinction between movements along a given production function, and a shift in the function due to technical progress, was 'arbitrary and artificial' (Kaldor 1957b, p. 596). In Kaldor's model the static production function was therefore replaced by a new Technical Progress Function, which related the rate of growth of output per worker to the rate of growth of capital per worker. Here capital was measured in tons of steel. Kaldor would soon abandon this

'convention' (*ibid*., p. 599), which was not at all satisfactory. 'How many tons, e.g., is a railway tunnel?', Piero Sraffa had asked Joan Robinson back in 1936 (Sraffa to Robinson, 27 October 1936, cited in King 2002, p. 81). It was not really central to his argument. The critical point came with Kaldor's claim that 'the system will always tend towards the point where the growth in capital and the growth in productivity are equal' (Kaldor 1957b, pp. 597–8). This gave one of the historical constancies, or 'stylised facts', with which he began: output per head and capital per head grow at the same rate, so that the capital–output ratio does not change. Kaldor's investment function had investment determined by the rate of growth of output and the rate of profit, and the wage and profit shares were given by the two propensities to save, as in the 1956 model. With constant savings propensities, both the wage and profit shares and the rate of profit were therefore constant. The profit rate itself 'depends only on the rate of economic growth and the division of capitalists' income between consumption and saving, and is independent of everything else' (*ibid*., p. 613). This was the 'Cambridge equation': $r = g/s_p$.[13]

Kaldor presented his completed model for two cases, first with population constant and then with a growing population; these corresponded approximately to the situation of the developed and the developing countries. In the first case, the rate of growth depended only on the parameters of the Technical Progress Function, and in particular was not affected by the capitalists' and workers' savings propensities. There was an unexpected echo here of the neoclassical, Solow–Swan model, in which an increase in thriftiness increased the level of output per head but not its rate of growth. In the second case, Kaldor's analysis was broadly Malthusian: growth was restricted by the existence of diminishing returns in agriculture, which limited the production of the food surplus that was needed to move an increasing proportion of the working population into industry. He devoted some effort to establishing the stability conditions of the model, since they were crucial to the ability of under-developed countries to escape from the 'low-level equilibrium trap' that had been identified by contemporary development economists, in which low incomes and high birth rates were mutually reinforcing.

The analysis was given a historical twist. What were now the developed countries had been in the Malthusian stage until the middle of the nineteenth century, Kaldor suggested. With the demographic transition there had begun a 'second and more cheerful stage of capitalism in which production and employment continue to grow, and real wages

are steadily rising with the growth of production'. This continuous improvement in working-class living standards had been 'quite unforeseen by Marx' (*ibid.*, p. 621). As Kaldor maintained, in an extended review of Paul Baran's *Political Economy of Growth*, attempts by modern Marxists to show that this was *not* true of the latest stage of 'monopoly capitalism' had foundered on the 'stylized fact' of constant wage and profit shares in total output (Kaldor 1958a). He had argued a very similar case in 1956 in a lecture in Beijing (Kaldor 1957a). Kaldor's views on economic development are discussed in much greater detail in Chapter 6.

4.5 Kaldor on growth: Mark II

As Frank Hahn and Robin Matthews observed in an important survey article, Kaldor's analysis of growth was itself unstable: 'Kaldor's views have undergone a number of changes, and there is reason to believe that they have not yet attained their steady state' (Hahn and Matthews 1964, p. 797). Two years after the Mark I model appeared, he gave a series of lectures at the LSE on growth and inflation, and took the opportunity to speculate on its microeconomic implications. Kaldor's treatment was informal, even by his own standards, and his efforts to demonstrate the existence of multiple equilibria between aggregate demand and aggregate supply through the device of a Marshallian 'representative firm' were unconvincing (Kaldor 1959, pp. 214–20; cf. Harcourt 2006, p. 118). It may have been this resounding analytical failure that led him to look for serious technical assistance. In his 1957 article he had acknowledged the assistance of his Cambridge colleague David Champernowne, but their collaboration did not go as far as to involve joint authorship.[14] Five years later he recruited the brilliant young theorist (and future Nobel Laureate) James Mirrlees to produce a rare co-authored paper that set out his Mark II model of economic growth, with a much greater degree of mathematical sophistication than in previous articles. The analytical sophistication came entirely from Mirrlees. As Kaldor recalled, proudly, in the final year of his life: 'I never had time to learn mathematics' (Kaldor 1986c, pp. 87–8).

The new model retained one central feature of the Mark I analysis: 'growth-equilibrium necessarily carries with it a state of continuous full employment' (Kaldor and Mirrlees 1962, p. 175). No distinction was now drawn in this context between advanced and developing countries, or between early and later stages of capitalist development. The full employment principle did not, however, entail full capacity utilisation, since markets were assumed to be imperfectly competitive and

each entrepreneur 'prefers to maintain an appreciable amount of excess capacity so as to be able to exploit any chance increase in his selling power either by increasing his share of the market or by invading other markets' (*ibid.*, p. 175). Thus full employment of labour did not mean full employment of capital.

There was one further, and much more radical, change. The new model 'avoids the notion of a quantity of capital, and its corollary, the rate of capital accumulation, as variables of the system; it operates solely with the value of current gross investment (gross (fixed) capital expenditure per unit of time) and its rate of change in time' (*ibid.*, p. 174). Hence the Technical Progress Function was redefined. On the vertical axis Kaldor measured 'the annual rate of growth of productivity per worker *operating on new equipment*' (*ibid.*, p. 176; original stress), while the horizontal axis measured the rate of growth of investment per worker (*not* the rate of growth of capital per worker, as in the 1957 model). It was still effectively a one-sector model, however, since the rate of technical progress was assumed to be the same in all sectors.

Even more than in the Mark I model, in Mark II technical progress was

> the main engine of economic growth…determining not only the rate of growth of productivity but – together with other parameters – also the rate of obsolescence, the average lifetime of equipment, the share of investment in income, the share of profits, and the relationship between investment and potential output (i.e., the 'capital/output ratio' on new capital). (*ibid.*, p. 188)

The model was Keynesian in the important sense that 'entrepreneurial expenditure decisions are primary; incomes, etc., are secondary'. It was

> severely *non*-neo-classical in that technological factors (marginal productivities or marginal substitution ratios) play no role in the determination of wages and profits. A 'production function' in the sense of a single-valued relationship between *some* measure of capital, K_t, the labour force N_t and of output Y_t (all at time t) clearly does not exist. (*ibid.*, p. 188)

At this point Kaldor and Mirrlees reverted to a theme that Kaldor had emphasised back in 1934 and then allowed to fade from view

> *Everything depends on past history*, on how the collection of equipment goods which comprises K_t has been built up. Thus Y_t will be greater

> for a given K_t (as measured by historical cost) if a greater part of the existing capital stock is of more recent creation; this would be the case, for example, if the rate of growth of population has been accelerating. (*ibid*., p. 188; stress added)

This phenomenon of path-dependence would later form an essential part of Kaldor's attack on what he called 'the irrelevance of equilibrium economics' (see Chapter 8).

The 1962 growth model carried clear implications for economic policy. Entrepreneurs must be encouraged to speed up the retirement of old equipment, for example by the introduction of a tax on obsolete plant (or, Kaldor and Mirrlees might have added, accelerated depreciation allowances and other types of subsidy for the introduction of new equipment). This would move the economy to the right along the Technical Progress Function and increase the rate of growth of output per worker, but only temporarily:

> A more permanent cure, however, requires stimulating the technical dynamism of the economy (*raising* the technical progress function) which is not only (or perhaps mainly) a matter of more scientific education and more expenditure on research, but of higher quality business management which is more alert in searching for technical improvements and less resistant to their introduction. (*ibid*., p. 190; original stress)

There was just a hint in this sentence, with which the paper concluded, that Kaldor was beginning to ponder the reasons for Britain's relatively slow rate of growth.

4.6 Kaldor on growth: Mark III

The 1962 model was set at a very high level of abstraction, in an idealised one-commodity, one-country world where no distinction was made between the agricultural, manufacturing and service sectors and there were no balance of payments problems. It seems likely that Kaldor had become dissatisfied with it almost before it was published. His doubts intensified after 1964, when Harold Wilson led the Labour Party to a narrow election victory after 'thirteen wasted years' of Conservative rule and Kaldor became special adviser to the Chancellor of the Exchequer (see Chapter 5, Section 4). In Whitehall Kaldor was forced to reflect upon the real problems of the British economy – slow growth,

the 'stop-go cycle', chronic balance of payments problems and an over-valued currency – which seemed to have only the loosest connection with either his Mark I or Mark II models of growth.[15]

In 1966 Kaldor was finally appointed to a personal chair at Cambridge and, as was the custom in those days, was required to deliver an inaugural public lecture. He chose to talk about 'Causes of the Slow Rate of Growth in the United Kingdom'. His analysis was so greatly different from anything he had previously published that it deserves the title of a Mark III model (though Kaldor himself did not use the term). He began by noting that between 1950 and 1965 output in the United Kingdom had grown much more slowly than in most other advanced capitalist economies. Interestingly, he refrained from making any longer-term comparisons, although by the mid-1960s economic historians were already beginning to debate the causes of Britain's (relative) economic decline, which they dated to the 1890s or even to the 1870s. Kaldor may have been aware of the literature on this question, even though he did not cite it. At one point in the lecture he did seem to summarise it:

> There has been no shortage of explanations. Some put the blame on the inefficiency of our business management; some on the nature of our education giving too little emphasis to science and technology, and too much to the humanities; some on the general social milieu which deprecates aggressive competitiveness and looks down on mere money-making as a career; some on over-manning and other restrictive practices of trade unions; some on the alleged national dislike of hard work; some on the insufficiency of investment, or of the right kind of investment; some on the economic policies of successive governments, being either too inflationary, or too deflationary, or both; and no doubt one could cite many other such 'explanations'.
>
> There may be truth in some, if not all, of these contentions. The difficulty about them is that with one or two possible exceptions, they are not capable of being tested, and there is no way in which their individual role could in any way be quantified. (Kaldor 1966a, p. 2)

What *could* be quantified, however, was the 'maturity' of the British economy, which Kaldor defined as 'a state of affairs where real income per head has reached broadly the same level in the different sectors of the economy' (*ibid.*, p. 3). This was significant because, almost alone among the industrialised countries, the United Kingdom had no reserves

of surplus labour in low-productivity agriculture that could be transferred to the manufacturing sector. There was, Kaldor argued, a strong positive relationship between the rate of growth of total output and the rate of growth of output in manufacturing. This reflected the importance of increasing returns to scale, which had been stressed by Adam Smith, by Alfred Marshall and above all by Kaldor's old LSE professor, Allyn Young. For Young, increasing returns were dynamic rather than static in nature; they were related to the *growth* of output, not the *level* of output. They were connected with learning, which was itself the product of experience, and they were a 'macro-phenomenon', since each industry benefited from the expansion not just of its own output but of output as a whole (*ibid.,* p. 9). Crucially, Kaldor maintained, increasing returns were associated with 'the so-called "secondary activities" – with industrial production, including public utilities, construction, as well as manufacturing – rather than with the primary or tertiary sectors of the economy' (*ibid.*, p. 11).

He now introduced the Verdoorn Law, discovered by his former United Nations colleague, the Dutch economist P. J. Verdoorn, as early as 1949: productivity growth is a function of output growth.[16] Regressing the rate of growth of labour productivity in manufacturing on the rate of growth of manufacturing output in twelve countries over the period 1953–1954 to 1963–1964 revealed that

> the rate of growth of output must have played a major role in the determination of productivity growth rates … apart from an 'autonomous' rate of productivity growth of around 1 per cent per year, the latter is a function of the growth in total output: each percentage addition to the growth of output requires a 0.5 per cent increase in the growth of employment in terms of manhours, and is associated with a 0.5 per cent increase in the growth of productivity. These coefficients are very close to those found by Verdoorn and other investigators. (*ibid.*, p. 11)

The regressions actually placed Britain's productivity performance in a relatively favourable light. 'If we award, as we must on this test, β to the strictly average performers and $\beta+$ to the moderately good performers, Britain, I think, must be rated $\beta+?$' (*ibid.*, p. 15).[17] Thus it transpired that the economic historians' explanations could be tested after all, and they turned out to be false. It was the slow growth of manufacturing output that was primarily responsible for Britain's slow productivity growth rate.

Kaldor insisted that Verdoorn's Law applied *only* to manufacturing, and not to primary production (which was characterised by diminishing rather than increasing returns) nor to the service sector, where 'economies of scale are not nearly so prominent and are exhausted more quickly' (*ibid*., p. 17). What was it, then, that determined the rate of growth of output in manufacturing, on which so much depended? Or, as Kaldor himself put it, what constrained manufacturing output growth? He emphasised supply rather than demand, and distinguished two types of supply constraint: commodities and labour. For any individual country, commodity supply problems tended to take the form of a balance of payments constraint, since otherwise the necessary commodities could simply be imported. Many commentators argued it was precisely the balance of payments that had inhibited British manufacturing growth after 1945. In each boom imports had increased more rapidly than exports, and governments were thus forced to take deflationary measures to protect the currency, bringing the boom to an untimely end.

Kaldor disagreed. Even in the absence of balance of payments difficulties, the labour constraint would have been binding. 'In postwar Britain', he claimed, 'periods of faster growth in manufacturing industry invariably led to severe labour shortages which slowed down the growth of output and which continued for some time after production reached its cyclical peak' (*ibid*., p. 25). This, in turn, was a reflection of the country's economic maturity:

> Britain, having started the process of industrialisation earlier than any other country, has reached 'maturity' much earlier – in the sense that it has attained a distribution of the labour force between the primary, secondary and tertiary sectors at which industry can no longer attract the labour it needs by drawing on the labour reserves of other sectors'. (*ibid*., p. 31)

It was not entirely clear what this implied for economic policy. Robert Bacon and Walter Eltis (1976) would subsequently use the 'labour shortage' explanation of Britain's economic woes to advocate the proto-Thatcherite programme of cutting public spending, thereby expelling workers from unproductive government jobs into more useful employment in the private sector.[18] Kaldor never endorsed this proposal, but what he had to say about policy in his inaugural lecture was not at all convincing. His definition of economic maturity as a situation in which labour productivity was roughly equal in all sectors is not easy to

reconcile with his call 'to concentrate our efforts on a more rational use of manpower in *all* fields, and to limit the absorption of labour into those sectors in which – if I may use a Pigovian phrase – the marginal social product is likely to be appreciably below the marginal private product' (Kaldor 1966a, p. 31). And his argument for 'concentrating our resources in fewer fields and abandoning others ... increasing the degree of interdependence of British industry with the industries of other countries' (*ibid.*, p. 32) sat uneasily with Allyn Young's conception of increasing returns as a 'macro-phenomenon'.

Kaldor's Mark III growth model was ignored by the economic historians and won little support from his fellow economists. Wolfe (1968) argued that Kaldor's regression equations were wrongly specified and his interpretation of them questionable, while Katz (1968) demonstrated that the Verdoorn relationship was something of a Trojan horse, since it could be deduced from impeccably neoclassical theoretical foundations. Further criticisms came from his young Cambridge colleague Bob Rowthorn (1975) and in the symposium devoted to 'Kaldor's growth laws' in the spring 1983 issue of the *Journal of Post Keynesian Economics*.[19] Critical appraisals of his inaugural lecture continued well into the 1980s (King 1994, chapters 24–41). Typically, Kaldor himself soon abandoned the labour shortage explanation of Britain's slow rate of growth in favour of an analysis that emphasised poor export performance.[20] His fascination with increasing returns, however, remained.

4.7 Theory, history and policy

Almost in spite of himself Kaldor began to take an interest in British economic history. He had no training in the discipline, and does not seem to have been very well read in it, but he could no longer avoid the question of when, why and how the country's economic performance had become so unsatisfactory. There are many historical references scattered throughout his writings after 1966, but the only systematic discussion came in a 1972 paper that was unpublished until 1977, when he came under some pressure to contribute to the newly established *Cambridge Journal of Economics*.[21] By this time there was already a large literature on these questions, and it would soon grow substantially;[22] however, Kaldor cited only one article by an economic historian (Aldcroft 1964).[23] He preceded his historical explanation of the causes of Britain's relative economic decline with some theoretical considerations, based on his understanding of the lessons of global development

over the previous two centuries: 'both the level and the rate of growth of output of the capitalist sector are dependent on the level, or rate of growth, of the effective demand for its products coming from *outside* the capitalist sector' (Kaldor 1977a, p. 198; original stress).[24] This led him to another fundamental proposition, namely 'the doctrine of the "foreign trade multiplier", according to which the production of a country will be determined by the *external* demand for its products and will tend to be that multiple of such demand which is represented by the reciprocal of the proportion of *internal* incomes spent on imports' (*ibid.*, p. 199). This, Kaldor continued, pointed to a demand-side theory of growth:

> This doctrine asserts the very opposite of Say's Law: the level of production will not be confined by the availability of capital and labour; on the contrary, the amount of capital accumulated, and the amount of labour effectively employed at any one time, will be the result of the growth of external demand over a long series of past periods, which permitted the capital accumulation to take place that was required to enable the amount of labour to be employed and the level of output to be reached which were (or could be) attained in the current period. (*ibid.*, p. 199)

It would be tempting, but probably misguided, to describe this as Kaldor's Mark IV model of economic growth. It would be tempting, because it was substantially different from the Mark III model of his inaugural lecture: the emphasis on increasing returns in manufacturing and diminishing returns in agriculture remained, but labour shortage had entirely disappeared, along with supply-side constraints on growth. It would be misguided, however, because Kaldor did not really offer a model at all. Although he stated it in words, he did not even write the formula for the foreign trade multiplier, which Harrod had done in 1933: $Y = 1/m$, where Y is the level of output and m is the propensity to import (Harrod 1933, p. 106–7).[25] Still less did he provide any formal analysis of the determinants of m.

To be fair to Kaldor, this was not the purpose of the paper, which was instead to offer a new interpretation of British economic history in which the nation's 'industrial growth was "export-led" from a very early date. This is clearly shown by the timing of fluctuations in industrial output and investment which, both in the 18th century and (since the early railway boom at any rate) in the 19th century), were regularly preceded by fluctuations in the volume of exports' (Kaldor 1977a,

p. 200). It followed that economic historians like Aldcroft had been wrong to attribute Britain's economic decline to 'poor industrial management', or to 'the failure to keep up with the growth of modern technology, in comparison to countries such as Germany and the United States which had a more advanced network of educational institutions in industrial technology' (*ibid.*, p. 201). These were perhaps contributory factors, Kaldor maintained:[26]

> But so long as other countries were able to expand their industries under the umbrella of protective tariffs and thereby reduce the market for British goods through the process of 'import-substitution' and through exports to third markets, Britain was bound to operate under a strong handicap, as the growth of both her industrial investment and her labour productivity lagged increasingly behind the others, steadily weakening her competitive strength. (*ibid.*, pp. 201–2)

In the postwar period, he continued, 'the growth of all fast-growing countries appears to have been "export-led"' (*ibid.*, p. 202), as was shown by the examples of Germany, Italy and Japan. Britain had again lagged behind because, as he had argued in September 1970 in his Presidential address to Section F of the British Association, it had replaced export-led growth by consumption-led growth (*ibid.*, pp. 202–3, citing Kaldor 1971a).[27] (He did concede, however, that this had been better than the interwar policy of no demand management at all).

Kaldor and the economic historians seem largely to have avoided each other. There is, for example, no reference to Kaldor either in the text of Michael Dintenfass's *The Decline of Industrial Britain 1879–1980* or in his 12-page bibliography (Dintenfass 1992), while the only relevant work cited by Kaldor in his inaugural lecture was to a rather obscure article in the *National Institute Economic Review* (Paige 1961); in his earlier work he had acknowledged the empirical work of Henry Phelps Brown and Simon Kuznets (Kaldor 1957b, p. 592, nn1, 2 and 4). Kaldor's new interpretation of the 'lessons from Britain's experience' was not shared by the majority of economic historians. Thus Nicholas Crafts (1991, pp. 270–8) rejected the Kaldorian hypothesis that growth was constrained by the balance of payments, as set out in a more elaborate form by A. P. Thirlwall, in whose formula $y_B = \varepsilon z/\pi = x/\pi$. Here y_B is the rate of growth of income consistent with balance of payments equilibrium; ε is the world income-elasticity of demand for exports; π is the domestic income-elasticity of demand for imports; z is the growth of world income; and x is the growth of exports (McCombie 2003, p. 16).[28]

There was very little evidence, Crafts argued, that the two elasticities were less favourable for Britain than for most of her principal competitors, and that real wage resistance was especially strong in the United Kingdom, thus making a real depreciation of the currency more difficult to achieve. Crafts claimed that Verdoorn's Law applied to Britain only for the period 1950–1965, and not thereafter. Even then the law could be interpreted in a variety of ways, for example as simple evidence that British industry had been catching up very rapidly with US productivity levels. The evidence was in any case 'contaminated' by Okun's Law,[29] since the correlation between output growth and labour productivity growth must have reflected the short-run or cyclical effects of changes in capital utilisation in addition to any long-run impact of increasing returns to scale (*ibid.*, p. 278).

Crafts attributed the country's slow growth after 1945 overwhelmingly to supply-side factors, including poor industrial relations, low and misdirected spending on research and development, poor technical education and poor management. Similar factors accounted for the poor growth record of the United Kingdom in the much longer term, from 1870 to 1950,[30] with prime responsibility again attached to deficiencies in education, training and research, poor management (especially with respect to industrial relations), and weaknesses in the capital market that made it difficult for poorly run firms to be taken over by superior managers (*ibid.*, pp. 279–81). The relative success of the British economy in the 1980s, Crafts concluded, was also inconsistent with the 'Kaldor–Thirlwall hypotheses', and could again be explained in terms of supply-side factors, above all increased competition and industrial relations that had improved in a climate of fear (*ibid.*, pp. 283–90).

It must be said that Kaldor was inconsistent on this question, as on so many others. In several of his speeches in the House of Lords in the early 1980s, attacking Margaret Thatcher and her government, he castigated the reactionary and ignorant social forces that supported her, and above all 'the poor quality, the indifference and the amateurishness of leading figures in British industry' (Kaldor 1983a, p. 15). This had created 'a gap in know-how' between Britain and her competitors, especially Germany and Japan, which reflected 'the failure of our system of higher education; and, even more, our system of selection, which makes our ablest and best trained people wish to avoid like anything a career in industry' (*ibid.*, p. 23). Thus 'the British entrepreneur, even in the 1880s, was outstanding as a cause of poor performance, due to his prejudices against education, against anyone who had a university degree and against the introduction of new methods, new techniques,

or entry into new fields' (*ibid.*, p. 32). But these were precisely Crafts's 'supply-side factors'. It must be said, too, that Kaldor never satisfactorily responded to the objection, raised by Crafts and many other critics, that the elasticities in Thirlwall's formula were themselves determined by these same 'supply-side factors'. The sceptics claimed that the formula concealed more than it revealed, and provided no firm support either for 'export-led growth' or for the notion that (for any individual nation or region) exports are the *only* exogenous element in effective demand. Kaldor seems to have been guilty here of a rather elementary fallacy. To say that investment, for example, depends on the level of income (as in the 1940 trade cycle model) or on the rate of growth of income (as in his subsequent acceptance of the accelerator principle), is not to say that it depends *only* on income and must therefore be treated as *entirely* endogenous. A similar fallacy lay behind Milton Friedman's rejection of the Keynesian consumption function and with it of the investment multiplier (see Bunting 2003), and had he thought about it Kaldor would certainly have repudiated that.[31]

4.8 The end of high theory

It is significant that Kaldor introduced his ideas on export-led growth to a British audience in a paper on economic policy (Kaldor 1971a), though precisely what the policy implications were remained to be established.[32] The 1966 inaugural lecture had, in fact, marked the end of Kaldor's involvement in high theory. His Mark III and Mark IV growth analysis was descriptive, intuitive and empirically based. Kaldor had already come to see the formal growth models (Mark I and Mark II) as a dead end, though he continued to insist on the merits of his macroeconomic distribution theory, and remained dismissive of the marginal productivity approach. Somewhat ironically, his technical progress function anticipated subsequent developments in 'new growth theory', with its emphasis on endogenous technical progress and repudiation of the law of diminishing returns as applied to capital (Thirlwall 2000). New growth theory was still in its infancy in 1986, when Kaldor died. It is unlikely that he would have been greatly impressed by it. His doubts concerning neoclassical capital theory went back to his early critique of Hayek and the Austrians (Kaldor 1937b, pp. 232–3; 1942c, pp. 376–7), and he maintained his unyielding opposition to the mainstream position on capital, distribution and growth throughout the Cambridge capital controversies of the later 1950s and early 1960s. Kaldor's friendship with Joan Robinson and Richard Kahn came under increasing

strain in this period (King 1998b), but they found themselves on the same – losing – side in one of the most important theoretical battles in twentieth-century economics (Bliss, Cohen and Harcourt 2005).

Once again, Kaldor had already moved on. Between 1964 and 1970, and again in 1974–1976, he spent much of his time in Whitehall, where he was one of the most influential economic advisers to the Wilson (and, briefly, Callaghan) Labour governments. His views on economic policy are the subject of Chapter 5 (which deals with the pre-Thatcher era) and Chapter 7 (on the great monetarist experiment). Theoretically, Kaldor was now beginning to think about integrating his ideas on the specific problems of the less-developed countries with his continually evolving views of the policy dilemmas that faced the developed world. Discussion of the resulting 'North-South' model of global economic growth – which could almost be described as Kaldor Mark V – is deferred to Chapter 6, and his broader methodological attack on the foundations of mainstream economic theory is assessed in Chapter 8.

5
The British Economic Disaster, 1964–1979

5.1 In the beginning (1945–1951)

From Harold Wilson's narrow election victory in October 1964 until James Callaghan's defeat by Margaret Thatcher in May 1979, the Labour Party was in power in Britain almost continuously, interrupted only by the ill-fated Conservative government that held office under Edward Heath from June 1970 to February 1974. For much of this period Kaldor was an official policy adviser, retaining his Cambridge appointment (until his compulsory retirement in 1975) but spending most of his time – and energy – in Whitehall. He enjoyed himself greatly, becoming a household word and exerting considerable influence over Labour's economic policies, especially on taxation. But he also found it a frustrating experience, as his ingenious and constantly creative mind came up against the hard realities of parliamentary politics and, above all, of the country's apparently insoluble economic problems.

In this chapter I take a broadly chronological approach. I begin in Section 5.1 by recapping Kaldor's early thinking on policy questions and by summarising his (rather small) contribution to policy-making in the Attlee years (1945–1951). In Section 5.2 I discuss the nature of his socialist ideas, and in Section 5.3 I outline his writings on taxation and related issues during the 'thirteen wasted years' of Conservative rule between 1951 and 1964. The next three sections are devoted to Kaldor's work for the first two Wilson governments of 1964–1970 (Section 5.4), to the way in which his theoretical ideas influenced his policy advice, and vice versa (Section 5.5), and finally to Kaldor and economic policy in the 1970s (Section 5.6). Only passing reference will be made to his writings on economic development and the post-1973 crisis in the world economy, which are explored in detail in Chapter 6. Relatively little

attention is paid here to questions of monetary policy, since his sustained attack on monetarism is the subject of Chapter 7. Kaldor never ceased to think and write on economic theory and method, as will become evident in Chapter 8, but for the last two decades of his life he was above all a policy economist.

By the end of the Second World War Kaldor had developed very clear views on the appropriate targets, and instruments, of macroeconomic policy. To monetary policy he allocated a relatively minor role. Like Keynes, Kaldor was a cheap money man, but he advocated low interest rates more to keep the cost of government borrowing down and advantage the average taxpayer at the expense of the wealthy rentier than for any stimulus that they might provide to business investment spending. Full employment was to be maintained in peacetime, he proposed, by the skilful use of fiscal policy, with taxation and government expenditure varied to achieve the required budget deficit, or surplus, without any regard for the traditional criteria of 'sound finance'. The correct use of fiscal policy would eliminate any tendency to demand inflation, he believed, and cost inflation could be averted through an incomes policy, which would prevent excessive growth of money wages. International cooperation would be needed to stabilise commodity prices, which would otherwise be a second potential source of cost inflation. In any postwar return to a fixed exchange rate regime, Kaldor acknowledged, the balance of payments would pose additional difficulties, in particular for Britain with its severely weakened financial position after the war. In the longer term this problem could only be solved by international cooperation to ensure that countries with large and continuing surpluses were required to increase their spending on imports from the deficit countries, which would otherwise be forced to implement deflationary measures to restore payments equilibrium. In the short term, however, controls over both imports of goods and services and over the export of capital would undoubtedly be necessary.

In his contribution to the Beveridge Report Kaldor had displayed great optimism concerning the prospects of the postwar British economy – too much optimism, as he later conceded (see Chapter 3, Section 6). Like many of the first generation of Keynesian economists, Kaldor had exaggerated the narrowly economic benefits that could be expected from full employment and had neglected the socio-political costs. When, after 1945, labour was in chronically short supply, there proved to be inexorable upward pressure on money wages and also unanticipated constraints on labour productivity, through formal and informal restrictions on the supply of effort at work.[1] Above all, the international

environment proved to be much less hospitable than Kaldor had hoped. The 'employment approach' to the international monetary system that was advocated by European and Australian economists (Cornish 2007b; Turnell 2007) failed to convince the Roosevelt and Truman administrations in the United States, which favoured sound finance over functional finance at the international level. The outcome was a chronic dollar shortage, which was reflected in severe constraints on the ability of the rest of the world to import goods from the United States. Twenty years later Kaldor would formalise the longer-term aspects of this problem in his model of balance-of-payments-constrained growth. In the Britain of the late 1940s it was the short-period problem that counted. Neither private consumption, nor business investment, nor government spending on essential social services, could be allowed to reach the levels anticipated in Kaldor's 1944 projections. The inevitable consequence was higher taxation, stricter rationing and tighter and more comprehensive administrative controls than he and his Beveridge colleagues had proposed. This economic straitjacket made the Attlee government increasingly unpopular, and was the fundamental cause of its 1951 election defeat (Worswick and Ady 1952; Kynaston 2007).

Kaldor himself was not at the centre of economic policy-making during the six years of postwar Labour rule; he was certainly not the consummate insider that he yearned to become. Probably he was in 1945 too young, in a Labour Party still dominated by elderly men, and he was also perhaps too foreign. The fact that Harold Laski was his mentor and protector at the LSE could not have helped, since Laski soon fell out with Attlee in public and was famously rebuked for his pains (Newman 1993, p. 268). After 1947 Kaldor was simply too far away. But his work in Geneva for the United Nations Economic Commission for Europe (UNECE) was directly relevant to the policy dilemmas facing the British government, and he maintained his links with the younger generation of Labour intellectuals, including Hugh Gaitskell, Anthony Crosland and Douglas Jay. In July 1949, as the government dithered over what in retrospect was an overdue and utterly unavoidable devaluation, Kaldor took the drastic (and expensive) step of flying back to London especially to add his voice to those who were urging a lower value for the pound. He had lunch with Gaitskell and Jay (the Economic Secretary to the Treasury), who 'had in fact both come to much the same conclusion a few days earlier' (Gaitskell 1983, p. 129).

By the following year, back in England for good and with his friend Gaitskell now Chancellor of the Exchequer, Kaldor began at last to exercise some influence on Labour policy. He focused on the two most

serious short-term problems facing the government, the balance of payments and wage inflation. In 1951 he published a brief (seven-page) but densely argued paper on 'employment policies and the problem of international balance', drawing on his work at UNECE and to some extent foreshadowing his writings on international economics in the 1970s and 1980s. Kaldor distinguished the structural and cyclical aspects of the problem. *Structural* difficulties would arise if one large country chose to maintain lower levels of employment (and thus higher unemployment) than the others. This would mean lower imports, and hence reduced exports from the other countries, whose exchange rate would therefore fall, and the terms of trade would deteriorate. A case could be made, Kaldor suggested, for the introduction of discriminatory import controls by high-employment countries to protect themselves from the adverse consequences of low-employment policies overseas. This would allow them to keep the balance of payments in equilibrium, and would leave the overall volume of international trade unchanged. It should not be confused with the undesirable practice of 'exporting unemployment' by running large and persistent trade surpluses. 'It would be desirable, however', Kaldor admitted, 'if more satisfactory criteria could be evolved for separating cases in which the imposition of import restrictions does not run counter to the economic interests of other countries, from other such cases where the imposition of restrictions cannot be justified on such grounds' (Kaldor 1951a, p. 43), in particular where the need for import controls arose from the other country's failure to maintain full employment. Under the prewar Gold Standard, exchange rates had been fixed and balance of payments equilibrium was restored by reductions in output and employment, but this was inconsistent with a serious commitment to full employment. It followed, Kaldor concluded, that exchange rate fluctuations would have to be much more frequent than they had been in the past.

The *cyclical* aspect of the problem involved the impact of a recession in one major country on the balance of payments and the employment situation in other countries. In these circumstances there was a clear danger of an international economic downturn, since 'the attempt to re-establish equilibrium may lead to an indefinite multiplication of deflationary measures in the course of which all countries become engulfed in an ever-deepening depression without a new international equilibrium being attained at any definite point' (*ibid*., p. 45). This could in principle be avoided through the imposition of general discriminatory exchange controls against the country in depression, or through a general devaluation against its currency. Neither solution was

ideal. The former, in particular, was objectionable since it would lead to a contraction in international trade: 'even if the volume of production and employment is not actually reduced, the economic welfare generated at the given level of economic activity will certainly be less, as a result of the lower degree of international specialisation and exchange' (*ibid.*, p. 46).

In 1951 Kaldor was still a defender of the classical theory of comparative advantage (he would later become much less confident about the universal benefits of international trade, as we shall see in Chapters 6 and 8). 'For these reasons', he concluded, 'a more adequate solution of the problem would pre-suppose new international arrangements whereby each country would undertake to maintain the normal supply of its own currency to the rest of the world, irrespective of fluctuations in its internal level of activity' (*ibid.*, p. 48). Kaldor did not name names, but in the early 1950s the United States was the only potential culprit, in either structural or cyclical terms. In subsequent decades West Germany and Japan were often accused of exporting unemployment through building up huge and continuing payments surpluses. With delicious irony, by 2007 the same complaint was being made against China and the principal complainant was the United States (Palley 2007). The international agreement that Kaldor called for has never been attained.

The international payments problem was, of course, closely related to the control of domestic inflation. In June 1950 Kaldor wrote a brilliant memorandum on incomes policy that was published only in 1964. It raised broader issues of such great importance that it is worth considering it at some length. After the 1949 devaluation a freeze on wage and dividend increases had been introduced. These emergency measures had prevented any significant increase in the inflation rate, but they could not be maintained indefinitely. There was, however, a long-term problem:

> So long as full employment is maintained, and labour is generally scarce, the pressure for higher wages, in the absence of restraint, is bound to lead to wage increases out of proportion to the rise in productivity. On the other side, under full employment conditions, such wage increases are bound to lead to price increases of a character that will cause the wage-inflation to become augmented by a profit-inflation, thus carrying further wage increases in their trail. Hence the need for *some* restraint in both wages and profits is not a temporary need in a full employment economy, but a permanent need. (Kaldor 1964e, pp. 112–13; original stress)

Kaldor distinguished two aspects of wages policy. First, to avoid inflation wages must rise on average at the same rate as labour productivity. The tacit assumption here is that the existing shares of labour and capital in GNP are not to be altered; redistribution is a matter for tax and welfare expenditure policies, not for wages policy. 'The second main aspect of wages policy', Kaldor continued, 'is to ensure that the changes in the wage structure are such as to facilitate, rather than hinder, the necessary structural adjustments in the economy' (*ibid.*, p. 113). This question had been neglected, because in an economy with high unemployment it was unimportant: '[w]hen jobs in general are hard to find, the new entrants to industry will necessarily go to the trades where they can find them' (*ibid.*, p. 113). Under full employment it would be necessary to gear wages much more closely to the net advantages of different occupations. This did not mean, though, that wages should be linked to productivity growth in individual industries. On the contrary, as a general rule the benefits of technical progress should be passed on to the community as a whole in the form of lower prices. Neither was he in favour of linking pay to the performance of individual firms. As he told the House of Lords in 1979,

> There is no doctrine which is more fallacious than the idea that wages should be fashioned enterprise by enterprise, firm by firm according to the value of output per worker. All it means is that inefficient enterprises are artificially sustained by their being able to pass on their inefficiency to their workers who get lower wages. (Kaldor 1979b, p. 3)

At all events, if inflation were to be avoided prices had to fall in some sectors of the economy to offset the inevitable price increases in others.

To administer wages policy Kaldor proposed the establishment of a Wages Board, with government and trade union (but not employer) representatives, to determine whether or not wage increases in particular occupations or industries should be permitted immediately or postponed: 'The new Wages Board would not "fix" wages, nor would it supersede existing negotiating machinery in any way. It would merely fix the places of individual categories of workers in a queue' (Waldon 1964e, p. 114). Once workers realised that wages policy was the necessary price to be paid for full employment, Kaldor believed, they would readily accept it.

Long-term controls over dividend payments were an essential corollary of a permanent wages policy, but the existing system linking current

dividends to past levels could not be sustained. In a predominantly capitalist economy, 'any lasting policy with regard to dividends must necessarily permit the rewards of success to be reaped as well as the penalties of failure' (*ibid.*, p. 118). This could be achieved by restricting dividend payments to some given percentage of current profits. The objection that this would be 'unfair' to shareholders in unprofitable enterprises could not be taken seriously:

> The simple answer to this is that exactly the same kind of thing would happen if dividend limitation were abolished altogether. The social justification for dividend payments is as the reward for risk-bearing; it is in the essence of this reward that it should be apportioned by the criterion of success, rather than by some criterion of 'fairness' or 'equity'. (*ibid.*, p. 120)

The important thing was to preserve economic incentives, so that both labour and capital were employed where they were most urgently needed.

With the defeat of Labour in the 1951 general election wages and dividend policy disappeared from the political agenda, but the problem of inflation remained, and there were intermittent but unsuccessful attempts by Conservative governments to control wage increases over the next thirteen years (Crouch 1979, pp. 29–49). In October 1964 the incoming Wilson government was fully committed to a comprehensive incomes policy, and the publication earlier that year of Kaldor's paper was therefore timely. He had restated the fundamental principles in a 1963 report to the Reserve Bank of Australia (Kaldor 1964f) and he never repudiated them, though the tempestuous relationship between Labour and the unions between 1964 and 1970 made him much more pessimistic about their practicality. He was pleased to see that even Conservatives agreed on the need for an incomes policy, he told the British Association for the Advancement of Science in September 1970:

> though I must confess, I am no more clear than others by what kinds of institutional arrangements such an instrument could be effectively operated. Can it be based on anything less than the creation of a social consensus on what constitutes a fair system of pay differentials on a national scale, and if so, is there a way of bringing this about? (Kaldor 1971a, p. 4)

This marked an important shift in his thinking on the nature of incomes policy, even if he had no answers to the problem that he had

raised. Earlier, he seems not to have recognised the importance of the problem. Kaldor was unimpressed by Australian attempts to use wages policy as a means of improving the distribution of income. There is no suggestion in either the 1950 memorandum or his pre-1970 writings on the subject that social justice was a relevant consideration, or that low-paid workers (especially women) might benefit from above-average pay rises. Redistribution, to repeat, was a matter to be dealt with through the taxation and welfare systems. It can be argued that Kaldor worried too much about efficiency and not enough about equity. Considerations of fairness were much more important in determining trade union wage demands than he ever admitted (Wootton 1955), and he seems also to have exaggerated the importance of wage differentials in the allocation of labour. It was soon noted that the existence of job vacancies was normally itself sufficient, even under full employment, to attract workers to expanding industries, without any increase in relative earnings (Reddaway 1959).

There were problems, too, with the dividend policy that he advocated. Profits that could not be distributed to shareholders would be reinvested in the company, increasing both its assets and its share price and thereby swelling the contents of 'the rich man's piggy bank', as the union leader Clive Jenkins famously observed.[2] Again, Kaldor's solution to this difficulty involved the tax system. He advocated a steeply progressive tax on expenditure to penalise dis-saving by the wealthy, and the taxation of capital gains as income (see Section 5.3). Given his justified concern about the overall performance of the British economy in the longer run, Kaldor was surprisingly reluctant to propose government interference with the operation of the capital market. He never returned to the question of the National Investment Board that featured in his wartime appendix to the Beveridge Report, and he was content for undistributed profits to be reinvested inside the company rather than allocated through the market for capital. This position would have satisfied neither socialist advocates of investment planning nor the liberal supporters of free capital markets, who would have preferred all profits to be distributed to shareholders and then reallocated through the stock exchange. The tension between Kaldor's evident belief in the market, on the one hand, and his growing doubts about Britain's economic performance and about the relevance of equilibrium economics, on the other hand, will be explored further in Chapter 8.

Looking back in 1955, Kaldor concluded that the crucial political change in the postwar period had been the acceptance by both parties of the principle of full employment, with the consequent 'gain in social

contentment and the lessening of social tensions in a society where everyone can find a job relatively easily, and where the fear of being deprived of one's livelihood through general economic causes has more or less disappeared' (Kaldor 1955a, p. 98). The first four years of Conservative rule had not greatly changed the course of macroeconomic policy, although the Tories had (not surprisingly) tended to favour the rich at the expense of the poor and to make more use of monetary policy at the expense of fiscal policy. An additional (and largely unexpected) consequence of a tight labour market was 'the greatly accelerated growth in productivity from year to year under full employment conditions' (*ibid.*, p. 98). This was due partly to the increased investment that was induced by full capacity operation, but it had also resulted from

> the beneficial effects of a general scarcity of labour on the adoption of labour-saving innovations of all kinds, some of which involve changes in factory layout, or in organization, or in design, rather than the installation of more automatic machinery. Where labour is difficult to obtain it becomes the effective bottleneck that limits the growth of profits and turnover of individual businesses. Hence entrepreneurs are led to concentrate on finding ways of saving labour, just as they concentrate on the invention and use of substitute materials when any particular raw material becomes short in supply. (*ibid.*, p. 99)

As we saw in Chapter 4, Kaldor returned to this theme eleven years later in his inaugural Cambridge lecture, in which he identified labour shortage as the fundamental source of Britain's economic difficulties. In 1955 he simply welcomed it as the greatest achievement of British socialism.

5.2 Kaldor's socialism

What sort of socialist, then, was Nicholas Kaldor? Apart from a brief dalliance with Hayekian liberalism in the early 1930s, he was a lifelong social democrat – someone who, in the twenty-first century, might have described himself as 'Old Labour'. He was already a socialist in his late teens. Having chosen to study at the LSE after reading the *Fabian Essays*, he took tea with Beatrice Webb on his first Sunday in England.[3] In the mid-1930s the combined influence of Keynes's *General Theory* and of his wife Clarisse reinforced his commitment to democratic socialism. Somewhat surprisingly Kaldor was much more consistent in his politics

than in his views on economic theory. He was never attracted by Marxism either as philosophy – and in any case he took little or no interest in philosophical questions – or as economics, or as political practice. Kaldor attended Edward S. Mason's seminar on Marxian economics while visiting Harvard in 1935, and took part in the LSE research students' 'counter-seminar' on Marxism when he returned to Britain. He remained unimpressed: 'It seemed to me that they were all caught up in stupid discussions about the appropriate use of words' (Kaldor 1986b, p. 64). After 1945 the sufferings of his family in Communist Hungary further strengthened his commitment to political (but not economic) liberalism.[4] He always thought that the Attlee Labour Party had got the balance between state and market just about right. A substantial degree of government intervention was needed to maintain full employment and to promote microeconomic efficiency and social justice, but this left considerable scope for private enterprise, competitive markets and the price mechanism, engineered where appropriate by the judicious use of taxes and subsidies.

Kaldor's fascination with the economics of taxation led his friend Ian Little to note that his proposed expenditure tax was 'designed to make consumption more equal, and capitalism both more socially acceptable and more efficient'. Kaldor, he suggested rather mischievously, was in fact 'a very progressive Conservative' (Little 1956, p. 120). This, of course, was written with tongue in cheek. Many years later the American economist Carl Shoup, who in 1961 had undertaken a lengthy appraisal of the British tax system, was quite serious when he wrote, in similar vein, that Kaldor had left his mark on British politics:

> As late as 1957 the Labour Party was so intent on nationalization of key industries that it was overlooking a far more powerful engine for redistribution of income, the tax system. All that changed, of course, a few years later, and at least part of the credit, or responsibility, must go to Professor Kaldor and his two colleagues. (Shoup 1981, p. 1618)[5]

This is something of an exaggeration. Only on the left of the Labour Party was further nationalisation still being advocated in the late 1950s, and political texts like *The Future of Socialism*, written by Kaldor's old friend Tony Crosland (1956), were much more influential than anything produced by academic economists. Kaldor himself kept out of the controversy between traditionalists and revisionists over further nationalisation. He did advocate the public ownership of the land (Brocklebank *et al.* 1974), but this was a position that he shared with many classical

liberals, going back at least as far as John Stuart Mill; there was nothing essentially socialist about it.

Late in life, perhaps anticipating the privatisation mania of the 1980s, Kaldor defended the public sector and even advocated its extension, citing postwar Austria as an example of a country which combined a very substantial group of nationalised industries with very satisfactory overall economic performance (Kaldor 1980d, p. 3; cf. Kaldor 1983a, pp. 70–5). At the very beginning of the Thatcher era, he could not anticipate the full extent of the impending neoliberal revolution in economic policy,[6] but he was already taking a rather more critical position on the merits of capitalism than he had previously displayed:

> The concentration on material welfare of the successful modern consumer societies in the West is not only highly wasteful in terms of exhaustible resources, but creates a socially restless and basically frustrated competitive society which fosters a scale of values that moralists and religions throughout human history have regarded as reprehensible. Moreover it also leads, through the process of industrial concentration ... to a concentration of power in the hands of the owners and managers of giant enterprises which is no less distasteful than state power: indeed in some ways it is more so, since it is power without responsibility.... It is the power conferred by the possession of wealth, far more than the inequalities of living standards occasioned by it, which makes modern capitalism so unsatisfactory as a method of organisation of human societies.... For that reason, it is at most inconceivable that it should continue to exist indefinitely. Sooner or later it is inevitable that it should give way to, or be replaced by, or develop into, something else. But in the light of the history of the present century it is far less clear than it appeared at the end of the nineteenth century what that something else is going to be. (Kaldor 1980d, p. 10)

The economic record of the socialist countries, Kaldor concluded, even relatively successful ones like Hungary, suggested that they offered no viable alternative.

This was a brief and informal (if also entirely accurate) assessment of 'actually existing socialism'. For someone who had helped to create the modern economics of welfare, in fact, Kaldor made remarkably little use of it in his work on economic policy. He contributed nothing to the extensive literature on the economic theory of market socialism, seems to have been entirely unimpressed by the arguments for economic

planning, and apart from a few brief remarks (*ibid.*, pp. 10–12) he took no interest in the analysis of worker cooperatives, self-managed enterprises and other forms of social ownership to which his Cambridge colleague James Meade made an important contribution. Kaldor's own socialism, then, was of the postwar British variety, with its five central pillars: full employment, a mixed economy, a comprehensive welfare state, a unionised labour market and redistributive taxation (King 2003).

His faith in the egalitarian potential of the British tax system was, however, shaken by subsequent experience. In 1980 he drew a sharp distinction between the theory and practice of fiscal redistribution:

> while I have not changed my views on the analytical plane, I have become far more sceptical of the possibilities of improving the distribution of income and wealth through taxation or of introducing effective reforms when these are perceived, in anticipation, as affecting adversely the interests of the property-owning classes. (Kaldor 1980g, p. xxiii)

He believed the difficulties to be especially severe in developing economies, as we shall see in Chapter 6. But the problem was also encountered in Britain, where neither the introduction of inheritance tax and surtax in the 1890s and 1900s, nor the later reforms of the Attlee and Wilson governments, had done very much to reduce inequality:

> Society has clearly become far more equal – or far less unequal – than it was in the nineteenth century; but this was more the result of the sustained rise in real wages (accompanied by the rise in the collective provision of welfare services) than of any restriction of the spending power of the rich brought about by taxation. (*ibid.*, p. xxiii)

Kaldor's argument here is not entirely satisfactory. One of his 'stylised facts' was the long-term growth of real wages at a rate roughly equal to the growth of labour productivity (see Chapter 4), which would have kept the labour's share in total output constant rather than increasing it. And it is unclear why the rich, if they were indeed able to resist more egalitarian taxation, could not also obstruct 'the collective provision of welfare services' (or at least ensure that they were paid for by the working class, as Kaldor's own assessment of the first Beveridge Report had indicated that they would be – see Chapter 3). Observers even more sceptical than Kaldor drew the conclusion that 'Parliamentary

socialism' had in fact achieved very little by way of redistributing income and wealth (Miliband 1961), the only significant exceptions coming in and immediately after the two World Wars, when the wealthy felt compelled to buy the loyalty of the poor and were aided in this by the inability of rentiers to protect themselves against the wartime inflation. Characteristically, Kaldor never attempted a systematic assessment of the historical record, nor a confrontation with Labour's critics on the New Left.

Just three years before his death, his friend Luigi Pasinetti aptly summarised Kaldor's politics:

> He is not a revolutionary; but he is a passionate advocate of reforms. His overall view of the working of a market economy is basically an optimistic one, but not in the traditional sense of believing that the market will automatically bring about the best of all positions. Kaldor is convinced that capitalist systems exhibit glaring injustices on income distribution and inherent inefficiencies, leading to involuntary unemployment and international disorder. But he is also convinced that these defects can be corrected by enlightened action of governments and by international cooperation. He belongs to that generation of Gaitskellite British Socialists, deeply imbued with the Fabian tradition, who are idealists at heart. (Pasinetti 1983, p. 334)

Kaldor's socialism, then, was of the British rather than the Continental European variety: sincere, ethical, pragmatic and stubbornly untheoretical.

5.3 'Thirteen wasted years' (1951–1964)

The Conservative election victory in October 1951 eliminated any possibility that Kaldor might continue as an adviser in Whitehall. But it did not stop him from continuing to think, and to write, on policy issues. In the previous year his friend Hugh Gaitskell, then Chancellor of the Exchequer, had appointed him as a member the Royal Commission on the Taxation of Profits and Income, which continued its work under the new government. Kaldor was dissatisfied with the approach taken by the majority of the Commission, and wrote a highly critical memorandum of dissent[7] that was also signed by the trade unionists George Woodcock and H. L. Bullock. The experience led him to reflect more broadly on the British tax system, and in 1955 he published a 242-page book advocating the replacement of the existing income tax with a

progressive expenditure tax. The adjective 'progressive' is all-important. Although Kaldor was later a rather lukewarm supporter of the European Union's (proportional) value-added tax, in preference to the haphazard system of indirect taxation then in operation in the United Kingdom (Kaldor 1964h), in *An Expenditure Tax* he was advocating something quite different.

He began by noting that the case for taxing expenditure rather than income had a long and honourable history, beginning with Thomas Hobbes and taking in Mill, Marshall, Pigou, Luigi Einaudi and Irving Fisher, whose paper at the 1936 meeting of the Econometric Society in 1936 had first set Kaldor himself thinking about tax theory and policy (Kaldor 1955c, p. 12). Earlier proponents of an expenditure tax had claimed that income taxation was unfair and inefficient because it involved 'double taxation' of saving. Kaldor's fundamental argument was different. The taxation of income, he maintained, failed to tax 'spending power', since *dis*-saving (expenditure financed by running down capital) was not liable to taxation. Income taxation was thus inherently inequitable:

> Since these non-taxable sources of spending power are not distributed at random, but are closely linked with the ownership of capital, taxation according to 'income' introduces a bias in favour of property owners whose taxable capacity is understated relatively to those who derive their income from work. (*ibid.*, p. 14)

Income taxation was also inefficient, while '[a] tax assessed on expenditure does not discriminate against either saving or risk bearing; and it alleviates, even if it does not remove, the disincentive effects of progressive taxation on work' (*ibid.*, p. 14). This was so because income taxation encouraged speculation rather than productive enterprise (*ibid.*, p. 122), and also discriminated against short-term variations in effort that generated fluctuating incomes (*ibid.*, pp. 131–40). Replacing income taxation with an expenditure tax would promote both higher output and a faster rate of growth. It would also constitute a more effective instrument of macroeconomic stabilisation, since Keynesian fiscal policy aimed to regulate the volume of spending, and it was obvious that a tax on individuals' expenditure was the most direct and effective means of doing so (*ibid.*, pp. 14–15). Income taxation, by contrast, was 'blunt, cumbrous and ineffective' as an instrument of macroeconomic control (*ibid.*, p. 177).[8]

Kaldor concluded that there would be no conflict between efficiency and equity under a progressive expenditure tax, as there was with a

progressive income tax. Changing the tax base would make it 'possible to advance towards an egalitarian society whilst improving the efficiency of operation and rate of progress of the economy' (*ibid*., p. 15). It was precisely the egalitarianism of the proposal, he thought, that might provoke opposition (*ibid*., p. 17). Hence the book ended with 'a plea for moderation' (*ibid*., pp. 238–42). Since there were admittedly real administrative difficulties with the taxation of expenditure, the new tax should be introduced gradually, beginning with the very rich, but with marginal rates of taxation low enough to prevent widespread tax evasion:

> Taxation can be a powerful instrument of social progress but it cannot be made into an engine of social revolution. The noble experiment of gradually building a society that is both free and just through progressive taxation is bound to fail unless we recognize that fact. (*ibid*., p. 242)

This did not prevent Kaldor from advocating a marginal tax rate of 300 per cent for the very rich, defined as those individuals with an annual expenditure of £5,000 or more (*ibid*., p. 241).[9]

The book was widely reviewed and met with considerable critical acclaim. Richard Musgrave, the doyen of American public finance theorists, wrote an extended review in which he praised its 'high idea-to-page ratio' and 'high-voltage shock treatment to established thinking' (Musgrave 1957, pp. 201, 205). Arnold Harberger, the epitome of Chicago conservatism, described it as 'one of the best books of the decade in public finance', ranking with the work of Edgeworth and Pigou and displaying 'economic analysis of high order, with good theory being wedded to sensitive observations about the real world' (Harberger 1958, p. 84). When Kaldor's 'brilliant' memorandum of dissent to the 1955 Royal Commission was reissued in 1980, another influential specialist, John Kay from the Institute of Fiscal Studies, emphasised its historical significance:

> It was this memorandum which established Kaldor's position – nationally and internationally – as the outstanding exponent of comprehensive tax reform. In a great British tradition which almost justifies the institution of Royal Commissions, Kaldor, like Beatrice Webb on the Poor Law Commission or Keynes and McKenna on the Macmillan Committee, provided a dissentient analysis whose intellectual sparkle dazzled the dull conservatism of the majority and in

> the long run proved far more profoundly influential on thought and on policy. (Kay 1981, p. 580, referring to Kaldor 1955d)

The reception of the memorandum and, even more, of *An Expenditure Tax* showed that in 1955 Kaldor was still very much within the mainstream of contemporary economics. The long period of Conservative rule denied him the opportunity to implement any of his ideas in Britain before 1964, but he never stopped thinking about tax reform and was never slow to offer advice to overseas governments (see Chapter 6). Meanwhile he maintained his contacts with leading Labour politicians like Gaitskell and Crosland and continued to write extensively on policy questions, in particular on monetary policy (see Chapter 7). Just before the 1959 election Gaitskell had invited him to advise an incoming Labour government on comprehensive tax reform. In the event Labour suffered a crushing defeat, but after Gaitskell's death the offer was renewed by James Callaghan, the Shadow Treasurer (Kaldor 1980a, p. xi).

5.4 The first Wilson Government (1964–1970)

With the Labour Party's narrow election victory in October 1964 Kaldor was appointed Special Adviser to Callaghan, attached to the Treasury but seconded to the Inland Revenue; his old friend Robert Neild from his UNECE days was also taken on as a policy adviser. Their first priority in October 1964 was not tax reform but rather to argue the case for an immediate devaluation of sterling. There were strong similarities with 1949. The balance of payments was in serious and rapidly increasing deficit, and the only alternative to a lower exchange rate was deflationary fiscal (and perhaps also monetary) policy, which would frustrate the incoming government's ambitious economic and social agenda. This time, alas, Kaldor found that he was not pushing at an open door. Although Crosland favoured immediate devaluation (Crosland 1982, pp. 120–1, 126–8), Harold Wilson was strongly opposed to devaluation and Kaldor failed to win over even a close friend like the Housing Minister, Richard Crossman. As Callaghan later recalled, 'theirs was a minority view which I resisted both for political and economic reasons. The Conservatives would have crucified us' (Callaghan 1987, p. 159). Kaldor continued to press the case, in the summer of 1965 joining Neild and Thomas Balogh to suggest that the pound be allowed to float. This brought violent opposition from the egregious George Wigg, a member of Wilson's 'kitchen cabinet', who, Crossman recorded in his diary, 'blew

up and said I had been talking to Hungarian traitors'. Wigg was 'dismissing everyone with a different point of view as a dangerous Hungarian or a tool of the Hungarians' (Crossman 1975, pp. 294, 305).[10]

These diary entries give some idea of the poisonous political environment in which Kaldor now found himself. Those who have grown up in the post-1973 world of floating exchange rates must find the depth of feeling about the value of sterling in these years very difficult to understand. All other prices are subject to change; why should the exchange rate be an exception? Why *was* the Labour Party so reluctant to devalue? Kaldor probably overstated his case, warning Crossman of impending 'savage deflation and mass unemployment' (*ibid*., p. 212),[11] but there is no doubt that the Labour government made life much more difficult for itself by clinging so stubbornly to an overvalued currency for its first three years in office.[12] Wigg's bizarre reference to treason tells part of the story. In the 1960s the pound was viewed as an emblem of national pride, a sort of second Union Jack, which could not be lowered without deep collective humiliation. This reinforced a vague but powerful belief that devaluation would entail a breach of faith with Britain's overseas creditors, as if the interests of wealthy foreigners should always be put ahead of those of Labour's own working-class constituency.

More defensible was Callaghan's view, which was widely shared, that the Conservative opposition would exploit these irrational sentiments to discredit the Labour Party's credentials as an economic manager. There may also have been some scepticism about the economic benefits that devaluation might bring. If the relevant demand and supply elasticities took the wrong values, a lower value for sterling would not greatly relieve the balance of payments and (in extreme cases) might actually make things worse. There was also the possibility that strong and stubborn trade unions might resist the reduction in real wages that was necessary to divert resources from consumption to imports, pushing for higher money wages in response to increased import prices and thereby sparking a price-wage-price-devaluation spiral with hyperinflation as the end result. This 'banana republic' scenario had already been observed in some Latin American countries. Kaldor himself later came to share some of these misgivings, but in 1964–1965 he was a strong and consistent advocate of devaluation. Almost certainly he was right. Inside the Labour Party the debate continued into the summer of 1966, with Kaldor, Balogh and Neild urging that the pound be allowed to float. They won the support of the Minister of Economic Affairs, George Brown, but were again opposed by Callaghan and the majority of his Cabinet colleagues (Crossman 1975, p. 572; Crosland 1982, pp. 172–4).

On more mundane questions of taxation policy he had much greater political success, though not before he had almost been driven out of the Treasury by an exasperated Callaghan, who allegedly told Wilson that he was sick of Kaldor and would gladly see him transferred to the Ministry of Housing (Crossman 1975, pp. 342, 345). Kaldor remained proud of his achievements as a tax reformer,[13] which included the introduction of a long-term capital gains tax, the reform of corporation tax and (in the May 1966 budget) a new Selective Employment Tax designed to encourage the growth of employment in manufacturing at the expense of the service sector:

> The main idea behind the scheme was that a combination of taxes and subsidies on employment (or payrolls) applied in different sectors and regions of the economy would make it possible to effect a considerable improvement in the performance of the economy, and to raise its overall rate of productivity growth. The tax on employment in the service trades was to be combined with a general subsidy on employment in manufacturing (with a differentially high rate in the high-unemployment 'development' areas) which would have the effect of improving the rate of growth of the manufacturing sector, its competitiveness in foreign trade and thereby make it possible to sustain a higher rate of economic growth. (Kaldor 1980g, p. xxi)

As a later commentator noted, 'probably never in the history of taxation policy has a single tax raising major revenue been so clearly associated with a single individual' (Kay 1981, p. 580).

In the 1967 Budget a subsidy on manufacturing employment in regions with high unemployment, the Regional Employment Premium (REP), was introduced to complement the Selective Employment Tax (SET). As Kaldor noted, this was equivalent to a devaluation confined to the depressed areas of the country. Like other Kaldorian tax initiatives, these were highly controversial (Bewlay 1966), and did not long survive. Selective Employment Tax was abolished by the Conservatives in 1972, while REP was '"sacrificed" when the first wave of "expenditure cuts" came "on stream" in 1976, under the influence of the new monetarism which swept the City, the media and important parts of the Establishment' at the time, including the upper echelons of the Labour Party (*ibid.*, p. xxi). Kaldor remained convinced that SET had been a good idea, even if there was some evidence 'here, as elsewhere, that that the response of the economy to price and cost incentives proved to be less than economists were apt to assume on the basis of

econometric studies' (*ibid.*, p. xxii; cf. Thirlwall 1987, pp. 241–6). Others were more critical: 'it worked so well', Denis Healey commented sardonically, 'that over-manning became a crippling burden on manufacturing for many years' (Healey 1989, p. 368).

Although Kaldor gave up his formal position in the Treasury in 1968 to work for Richard Crossman at the Department of Health and Social Security, he never ceased to argue for tax reform. Indeed, if Kaldor had got his way the pre-election Budget in 1970 would have been quite different, and Edward Heath's election victory might not have occurred. He lobbied vigorously for income tax cuts and large increases in Supplementary Benefit,[14] designed to win back the working-class vote for Labour and financed by an increase in SET. But the 'Kaldor budget' was strongly opposed by the Chancellor, Roy Jenkins and rejected by Cabinet, much to Kaldor's deep distress (Crossman 1975, pp. 848, 850).

5.5 Theory and practice

As we saw in Chapter 4, there was a gradual but cumulatively very important shift in Kaldor's thinking about economic growth that began in the late 1950s and culminated in the mid-1960s in a more or less complete renunciation of the formal one-sector models that he had begun with. This was partly the product of his reflection on the disappointing growth performance of the British economy, and in turn it influenced his thinking on the appropriate policies to stimulate growth. Theory and policy came together in an influential paper on the case for regional policies, first presented early in 1970 as a public lecture in Aberdeen (a city as yet unaffected by the North Sea oil bonanza) and subsequently published in the *Scottish Journal of Political Economy*.

Kaldor began by noting that the huge discrepancy in growth rates between rich and poor nations since 1750 could be regarded as a regional issue, albeit on a global scale. It could not, however, be explained in terms of different resource endowments. A growing capital stock, in particular, was as much the result of rapid economic development as its cause. Kaldor again invoked Gunnar Myrdal's principle of 'circular and cumulative causation' (Myrdal 1957), according to which any initial advantage that one region might possess, relative to other regions, tended to increase when trade was opened up between them, rather than diminishing, as orthodox theory would lead one to expect:

> Whereas in the classical case – which abstracts from increasing returns – the opening of trade between two regions will necessarily

> be beneficial to both (even though the gains may not be equally divided between them) and specialisation through trade will necessarily serve to reduce the differences in comparative costs in the two areas, in the case of the 'opening of trade' in industrial products the differences in comparative costs may be enlarged, and not reduced, as a result of trade; and the trade may injure one region to the greater benefit of the other. (Kaldor 1970b, pp. 340–1)

Hence there was a need for regional policies to induce convergence (instead of divergence) between advantaged and disadvantaged regions.

Part of Kaldor's argument here was familiar, while part was quite new. He again invoked Verdoorn's Law, but with a new twist. The growth of productivity in manufacturing was positively related to the rate of growth of manufacturing output, which in turn – and this was the novel aspect – depended solely on the rate of growth of exports:

> From the point of view of any particular region, the 'autonomous component of demand' is the demand emanating from *outside* the region; and Hicks's notion of the 'super-multiplier' can be applied so as to express the doctrine of the foreign trade multiplier in a dynamic setting. So expressed, the doctrine asserts that the rate of economic development of a region is fundamentally governed by the rate of growth of its exports. For the growth of exports, via the 'accelerator', will govern the rate of growth of industrial capacity, as well as the rate of growth of consumption; it will also serve to adjust (again under rather severe simplifying assumptions) both the level, and the rate of growth, of imports to that of exports. (*ibid.*, p. 342; original stress)

On the assumption that money wages tend to rise at roughly the same rate in both prosperous and depressed regions, unit labour costs[15] 'will tend to fall in regions (and in the particular industries of regions) where productivity rises faster than average. It is for this reason that relatively fast growing areas tend to acquire a cumulative competitive advantage over a relatively slow growing area' (*ibid.*, p. 343). Kaldor conceded that the powerful forces creating regional divergence were offset, to some extent, by forces operating in the opposite direction, such as higher housing prices and congestion costs. 'But as is well known, many of these dis-economies are external to the individual producer and may not therefore be adequately reflected in the movement of money costs and prices' (*ibid.*, p. 344).

The case for regional policies followed directly from this:

> There is some presumption therefore for supposing that, if left to market processes alone, tendencies to regional concentration of industrial activities will proceed farther than they would have done if 'private costs' were equal to 'social cost' (in the Pigovian sense) and all economies and dis-economies of production were adequately reflected in the movement of money costs and prices. (*ibid*., p. 344)

This was the principal justification for the Regional Employment Premium, which had given the depressed regions the benefits of devaluation against the more prosperous areas (and also against the rest of the world).

The same broad principles governed Britain's economic relations with Western Europe. Kaldor was notorious for changing his mind on policy issues, but on the question of the Common Market[16] his position was consistent from the very beginning. He was a convinced internationalist and a firm supporter of European union, but he was always opposed to British membership of the economic entity established by the Treaty of Rome in 1957 on the grounds that it was against the nation's economic interests. He warned of the dangers of Common Market membership when the Macmillan government first applied to join in 1963 and repeated his objections when Heath made a new (and this time successful) application in 1971. He campaigned against British membership, writing articles for the *New Statesman* (at that time a very influential social democratic weekly) and a series of letters to *The Times*. Kaldor was distressed when, in the mid-1970s, the Labour government reversed the Party's long-standing opposition to British membership and urged a 'yes' vote in the 1975 referendum. Due to his official position as Special Adviser to the Chancellor of the Exchequer he was unable to speak out in public. Although he voted against British membership, Kaldor came to accept it as a *fait accompli* (Thirlwall 1987, chapter 10).

His case was set out very clearly both in scholarly form (Kaldor 1978d) and in writings designed for a popular audience. In a pamphlet published in 1972 by Trade Unions Against the Common Market[17] Kaldor displayed his considerable talents as a propagandist. He set out six reasons for his opposition. First, the case for British membership was being promoted by secret, well-financed business lobby groups, who almost certainly did not have the interests of the working population at heart. Second, the terms that Heath had negotiated were a very poor

bargain, with Britain being required to make a much larger contribution to the Community's expenditure than its share in European national income should have dictated. Third, and most important,[18] the forces of cumulative causation would seriously disadvantage Britain, to the benefit of more favoured regions of the Western Europe. Kaldor reiterated the argument of his *Scottish Journal* paper:

> Owing to the economies of regional concentration, free trade in industrial goods is likely to lead to the concentration of industrial development in 'central' areas, at the expense of peripheral areas.... The wonderful long-term 'dynamic advantages' of belonging to a home market of 250 million – which the Government and the 'pro-marketeers' dangle in front of our eyes – may, because Britain is a 'peripheral' area of the enlarged Common Market, turn out to be negative rather than positive, and compound the disadvantages of membership instead of offsetting them. (Kaldor 1972b, p. 6)

In all probability Britain's growth rate would decline if she joined the Common Market, Kaldor concluded. 'If Ireland suffered from being an offshore island of Britain in the 19th century, Britain is likely to suffer from being an offshore island of Europe' (*ibid.*, p. 14).[19]

The fourth part of Kaldor's case was political. 'Britain is in some ways a more socialist country than these other European countries' (*ibid.*, p. 6), with a progressive tax system and a generous welfare state that delivered a substantial 'social wage' to its working-class citizens. This was now threatened by Common Market membership; the European insistence that Britain introduce a flat-rate Value Added Tax would be a particularly regressive move. The fifth objection concerned the much higher price of food that would be entailed by Britain's adherence to the Common Agricultural Policy, which would bear especially hard on working people. Kaldor's sixth argument was a gloss on the third, but with a political dimension:

> There is finally the question of the freedom of capital movements in the Community. This will enable international companies to play off the workers of one country or region against another.... Capital will go where profits are the highest. Profits are the highest where industries are most efficient. And the trouble with modern industries, for any one or any group of them, is that success breeds success and failure brings failure. There are strong cumulative forces in operation. (*ibid.*, p. 10)

Kaldor concluded by asserting the need for Britain to retain national sovereignty, above all in the matter of the currency and the exchange rate (*ibid*., pp. 16–17). Rather surprisingly, in view of his increasing scepticism about the theory of comparative advantage, he came down very strongly in favour of free trade in industrial products, and declared himself a firm supporter of continued membership of the principal alternative to the Common Market, the European Free Trade Area known as EFTA (*ibid*., p. 16).

Kaldor's was not in any way an extreme or eccentric position. In the 1960s and early 1970s academic economists were more or less evenly divided on the merits of British membership of the Common Market, with the opponents sharing many of Kaldor's own misgivings (Worswick 1960; Khan and Johnson 1972; Middleton 1998, pp. 26–8). The subsequent refusal of Britain (and also Denmark and Sweden) to give up their national currencies for the Euro owed much to the arguments that he (and many others) had made. After Kaldor's death it was, ironically, free-market economists who campaigned for withdrawal from the now expanded and much-strengthened European Union, offering a revived EFTA as part of their alternative vision for Britain (Minford, Mahambare and Nowell 2005; cf. Miliband 1992; Milne 2004). Kaldor would no doubt have been gratified to find allies among the Thatcherites. But his fears that the British economy would be seriously damaged by Common Market membership proved to be ill-founded, and his designation of 'West Germany, the Ruhr, part of Holland, part of Belgium and North-East France' as the Community's most prosperous 'centre of gravity' (Kaldor 1972b, pp. 10–11) was quite simply wrong; these are precisely the areas that have since become the rust belt of Western Europe.

In general, however, the forces of convergence turned out to be much stronger than he had expected, and the striking success of the 'Celtic tiger' after 1973 demonstrated that the handicap of being an 'offshore island' was by no means insuperable.[20] Kaldor was, however, right to be concerned, and not only about the anti-socialist nature of Continental politics. Within the European Union convergence has not been a smooth and uniform process, as the simpler versions of neoclassical growth theory would have predicted. While the poorer countries have tended on average to catch up with the richer ones, there has also been a tendency for 'backward areas' *within* each country to fall further behind the more favoured regions, especially the capital cities (Carrington 2003; Martin 2005; Frenken and Hoekman 2006).[21] Thus the case for regional policies remains compelling, since the free market always produces losers along with the winners.

There were much broader implications of the Common Market debates, as Kaldor was quick to recognise. In his presidential address to Section F of the British Association for the Advancement of Science in September 1970 he attacked the foundations of postwar economic policy, which had 'treated the problem of full employment and (implicitly) of growth as one of internal demand management, and not [as] one of exports and of international competitiveness' (Kaldor 1971a, p. 5). The early Keynesians, including Kaldor himself, had worked from a closed economy model that was not appropriate for an open economy like Britain.[22] The result of this 'second-best' policy was that Britain's economic growth had been consumption-led rather than export-led, and this, Kaldor concluded, had been the main reason for the country's poor growth performance.

Some original – and contentious - macroeconomic reasoning underpinned this analysis. In his 'regional policies' lecture in February 1970 Kaldor stated the following, in a footnote: 'The necessary assumptions are that all other sources of demand except exports are endogenous, rather than exogenous – i.e., that both Government expenditure and business investment play a passive role, the former being confined by revenue from taxation, and the latter by savings out of business profits' (Kaldor 1970b, p. 318 n1). This is a profoundly anti-Keynesian statement, which reasserts both the doctrine of sound finance ('no budget deficits') and the determination of investment by saving, rather than vice versa. Kaldor must have thought better of it,[23] since in the September lecture he offered a quite different account of the determinants of investment expenditure:

> Though the intention of the policy has been to give priority to the growth of exports and investment, the very fact that the policy aimed at regulating the pressure of demand by internal measures – by stimulating or restraining personal consumption – meant, in the circumstances of the United Kingdom, that personal consumption expenditure took over the role of the 'prime mover' of the economy. This is because in a capitalist economy business investment is largely demand-induced: it responds little to direct incentives (such as interest rates, tax incentives or subsidies); it is far more effectively influenced through the control of final demand. Hence both fiscal measures and monetary measures (credit control) operated on the economy primarily by controlling the *rate of change* of consumer expenditure. (Kaldor 1971a, pp. 11–12)

This, Kaldor continued, had a number of disadvantages. First, government policy was inevitably constrained by the fear that excessive consumer spending would increase the trade deficit. Second, the resulting low level of consumption also weakened the incentive to invest, leading to a low investment–income ratio. Third, since it was determined by consumer preferences, the pattern of output was skewed towards services and away from manufacturing, with the consequent loss of the dynamic economies of scale that would have resulted from a more rapid expansion of the industrial sector. 'For both of these latter reasons – a high investment ratio and a high share of manufacturing in the growth of output – export-led growth generates a higher "underlying growth rate" than consumption-led growth' (*ibid.*, p. 13). The 1967 devaluation had been a step in the right direction, increasing exports from 21 per cent to 25 per cent of GNP in only two years, but much more would have to be done. There were serious problems at the national level, Kaldor concluded, in achieving the necessary increase in investment at the expense of consumption. Internationally, Britain would benefit from a system of 'managed floating rates' that would allow a further reduction in the value of sterling and improve the country's competitiveness.

This line of argument would soon lead Kaldor to formulate a new model of economic growth, drawing on the export multiplier devised by Roy Harrod in the early 1930s but not seriously applied to long-period issues (Harrod 1933, pp. 106–7). Kaldor's balance-of-payments-constrained growth model is especially relevant to developing countries, and will be discussed in detail in Chapter 6. As a theoretical generalisation from the experience of the British economy after 1945 the story that he told is a convincing one. To what extent it can be regarded as a Keynesian story is another matter. In his new enthusiasm for the accelerator mechanism Kaldor left no room for business expectations, uncertainty or financial instability as factors affecting the level of investment spending, which he now treated as endogenously determined.[24] When the world economy moved into deep recession in 1973–1974 a combination of a wage-induced profit squeeze and a collapse in business confidence was largely responsible; neither possibility formed part of Kaldor's 1970 analysis. It could also be objected that his emphasis on international competitiveness opened the door to supply-side themes which, in the hands of monetarists and New Classical economists, would soon eliminate aggregate demand from macroeconomic theory altogether. These were questions that Kaldor himself would soon have to confront (see Chapter 8).

5.6 The Wilson and Callaghan Governments (1974–1979)

Between June 1970 and February 1974 Labour was again in opposition. The Heath government faced the same problems that had defeated Wilson, and had even less success in dealing with them. Unemployment rose, reaching a postwar record of 3.7 per cent in 1972, while output and employment stagnated. Heath's initial commitment to deregulation and financial austerity soon gave way to a more conventional Keynesian policy package of fiscal expansion backed by statutory control of wage and price increases. Although inflation fell slightly (from 9.4% in 1971 to 7.1% in the following year), the restrictions on wage rises were increasingly unpopular, and inflation soon accelerated, reaching an annual rate of 16.0 per cent in 1974, the year in which industrial unrest finally brought down the Conservative government. Labour returned to office to confront a parlous economic situation in which both unemployment and inflation were increasing; a new term, 'stagflation', was now in widespread use to describe this unprecedented problem. The incoming Wilson government also inherited yet another payments crisis, which had been made very much worse by the large increases in the price of oil implemented by OPEC, the producers' cartel. From a small surplus in 1972 the current account had moved into substantial deficit two years later (Caves and Krause 1980, pp. 5–6, tables 4–6).

Kaldor returned to Whitehall as Special Adviser to the Chancellor, Denis Healey. This time he lasted less than two years, resigning in August 1976 when it became clear that his advice had failed to convince either Healey or the new Prime Minister, James Callaghan. He opposed the deflationary measures introduced by the government in the previous month to protect the currency, and was distressed by Labour's apparent conversion to monetarism. Four years later Kaldor described this as a fatal decision, worse in its long-run consequences than the return to the Gold Standard in 1925 (Kaldor 1980f). It condemned Britain to many years of high unemployment and slow economic growth.[25]

Although he always regarded monetarism as a dangerous and delusory 'pre-Keynesian' doctrine, Kaldor was not entirely consistent in his attitude towards Keynesian macroeconomics. In 1974 he identified the balance of payments as the root of Britain's economic problems, noted how the resulting 'stop-go' policy regime had created a 'political business cycle'[26] and concluded that the Keynesian diagnosis was faulty,

since unemployment had been caused not by excessive saving but by inadequate demand for British exports. Thus Keynesian stimulation of effective demand had turned a chronic unemployment problem into a chronic balance of payments crisis (Kaldor 1974a). The experience of the 1974–1979 Labour government, however, made him less inclined to criticise Keynesian policy:

> There is only one important matter on which the events of the 1970s caused me to change my mind. This concerns the relative importance of price (or cost?) competition, as against other 'non-price' factors, such as superiority of design or quality, length and reliability of delivery dates, after-sales service, etc. Exchange rate adjustments operate *mainly* on costs and prices; and despite vast changes in relative exchange rates – in real, and not just in nominal terms – there has been little effect on the pattern of trade in manufacturing. (Kaldor 1980a, p. xxxi)

By this time he had become a convinced and persistent advocate of import controls as the only effective remedy for the chronic balance of payments problem:

> The lesson of the 1970s, to my mind, contradicts the current intellectual trends which seek salvation through a return to a free market system. It shows that instruments which operate through market forces (such as devaluation or floating rates) are much too slow and much too weak in their effects to avoid unnecessary (and in the long run, intolerable) hardship caused by reliance on them. If the mainly private-enterprise market economy is to survive (as it must, if even less palatable alternatives are to be avoided) the world needs more planning and more regulation in the matter of income-distribution as well as in the field of international or inter-regional trade, and not less. (*ibid.*, p. xxxi)

This was by now the position of a well-established 'Cambridge Group', which included Francis Cripps and Wynne Godley in addition to Kaldor. It was a controversial stance. Liberal critics not only objected to the efficiency costs of import controls but also doubted their effectiveness. 'Most important', two prominent American macroeconomists complained, 'it is not apparent why reducing real wages through protectionism does not affect workers in the same way as reducing real wages through depreciation' (Dornbusch and Fischer 1980, p. 67). Margaret

Thatcher would soon smash through this 'real wage resistance' problem, but at a massive economic and social cost (see Chapter 7). For their part Callaghan and Healey prevaricated.

5.7 The verdict

This marked the end of Kaldor's Whitehall career. Labour politicians had mixed views on his contributions as a policy adviser. Richard Crossman was prepared to forgive him anything, including his tendency to fall asleep – on the sofa at home while talking to Crossman and even at High Table in the middle of a King's College Feast (Crossman 1977, p. 201). He was, Crossman confided to his diary in July 1969, popular with civil servants and ministerial advisers:

> Nicky has a much better reputation [than Thomas Balogh] in Whitehall and, on the whole, he is liked in the Treasury and the Inland Revenue. He is not thought of as a malicious Whitehall-manoeuvring politician, constantly fermenting ideas and launching attacks. Nicky doesn't bother about Whitehall, he really likes tax law, knows his friends and he is frightfully adroit and ingenious about taxes and contributions and all that kind of thing. (*ibid.*, p. 568)

But, Crossman reported, Harold Wilson and Roy Jenkins were less enthusiastic. There is no mention of Kaldor in Wilson's massive memoir of his first term in government (Wilson 1971), and only an inconsequential footnote in Jenkins's political testament (Jenkins 1991, p. 293 n). Denis Healey certainly found Kaldor hard to work with:

> His political judgement was bizarre, and his economic judgement was too often distorted by his changing theories ... he was typically insensitive to the political and social implications of his proposals; his advice to governments in the Third World was notorious for provoking revolution. Like all good academics, he was always ready to get back to the old drawing board when one of his theories failed; the ministerial victims of his advice usually found that their drawing boards were taken away, and sometimes their heads as well. (Healey 1989, pp. 391, 368)

Kaldor also clashed with academics-turned-mandarins like Alec Cairncross and Kenneth Berrill. Cairncross found him long-winded and excessively reliant on logic, without any firm grasp of human behaviour

(Cairncross 1998, pp. 242–3), while he got on so badly with Berrill that the latter had to be moved from the Treasury by an exasperated Chancellor. 'Odium academicum is quite as virulent a disease as odium theologicum', Healey concluded; 'political rivalries pale in comparison'. In the end he was not sorry to be rid of Kaldor. 'I did not discourage him from going back to academic life. I still regard Kaldor as the most brilliant economist of his generation in Europe. But government was not his *métier*' (Healey 1989, p. 391).[27] Since Callaghan and Healey were responsible for importing monetarism into Whitehall, three years before the arrival of Margaret Thatcher, Kaldor might reasonably have replied that macroeconomic policy was not their *métier*.

6
Kaldor and the Third World

6.1 Problems of underdevelopment

As a separate sub-discipline, development economics lasted only four decades, from its origins in the 1940s until it was submerged in the neoliberal Washington Consensus forty years later (Hirschman 1981; Mitchell 2004; Tignor 2006). Kaldor was not quite in at the birth, which owed something to contemporary research in economics and (especially) economic statistics and a great deal to the broader political context of the time. Economists had always recognised the existence of a gap between rich and poor countries, but the sheer size of the gulf between 'advanced' and 'backward' areas, or 'developed' and 'underdeveloped' regions, only became apparent with the refinement of national income accounting techniques that was discussed in Chapter 3 and the massive international data collection by Simon Kuznets and others that began in the 1930s. By 1956 it was almost a commonplace to acknowledge that 'the real income per head of the "developed" countries, making allowance for shortcomings in statistical measurement, is 10, 20 or perhaps even 30 times as large as the real income per head of Asia, Latin America or South Eastern Europe' (Kaldor 1956b, p. 20). There was a growing acceptance that the economic theory that appeared to work for the rich countries could not be applied without very substantial modification to poor regions, where agriculture was very much more important relative to manufacturing, and also very much less productive. A different set of policy prescriptions was also called for:

> The virtues of stable and uniform exchange rates, convertibility, bound tariffs, non-discrimination, the prohibition of export subsidies, etc., may be genuine enough when applied to the conduct of

> the industrialised countries in their trading relations with each other. They are far from evident when applied to the under-developed countries. (Kaldor 1964g, p. x)

The need for a new and distinctive economics of development was especially obvious in questions of macroeconomics. The unemployment problem in backward areas was enormous. It was also chronic and structural rather than cyclical, and almost certainly not susceptible to rapid reduction through Keynesian policies of aggregate demand management. Similarly, one-sector growth models of both the Harrod-Domar and Solow varieties, which ignored the distinction between primary and secondary activities, were of very doubtful relevance to the problems of the poorest countries. Again Kaldor was stating the (almost) obvious when he asserted that 'the key to an accelerated growth of the under-developed areas of the world lies in bringing about fundamental changes in both the mental outlook and the technical knowledge and skill of their peasant populations'. The Industrial Revolution had been preceded by an agricultural revolution, first in Britain and then in the other Western European countries. 'It is no accident that all the countries which succeeded in developing manufacturing industry on a large scale (such as Sweden, Germany, Belgium, Switzerland, apart from Britain and the US) possess a highly efficient and largely "commercialized" agriculture, with both high yields per acre and high productivity per man' (*ibid.*, p. 23). No body of theory that ignored this problem, Kaldor concluded, could claim to be useful in the analysis of development.

The specialism of development economics would not have emerged so quickly, however, had it not been for a number of important political changes in the 1940s, all of them connected to the expanding power of the Soviet Union and the growing influence of its Communist ideology. The Soviet defeat of Nazi Germany in the Second World War greatly increased the credibility of the Stalinist model of economic development. Even democratic socialists became much more sympathetic to economic planning and (at least by implication) less enthusiastic about the magic of the market. The extensive and very rapid decolonisation after 1945 created a new set of nation-states whose ruling elites were now free to choose between the capitalist and socialist models of development. Inevitably economic arguments became prominent in the ideological battles that largely replaced military conflict during the Cold War, which was waged between the Soviet Union and its allies (which after 1949 included China) and the United States and its supporters. As Kaldor warned his American readers in 1958, the US

government had become 'dangerously conservative in its attitude towards questions of political and social reform in backward countries. I am sure that this is a short-sighted policy for it prevents the transformation of these countries into economically progressive industrial societies, and in the long-run industrialisation under capitalism is the only effective antidote to Communism' (Kaldor 1958b, pp. 35–6). He would have been the first to admit that there was nothing original in this argument, which went back to the late 1940s and the introduction of Marshall Aid (Walker 1994).

Kaldor's first direct involvement with questions of economic development was in 1947–1949, when he was based in Geneva as Research Director of the United Nations Economic Commission for Europe, a continent that included the Balkan states and the Iberian peninsula, both then classified as backward areas. Some of the earliest work in development economics was in fact carried out on the agrarian problems of South Eastern Europe by Kaldor's former LSE colleague Paul Rosenstein-Rodan (1943). Through his Fabian Society connections Kaldor would also have been aware of the pioneering work of the West Indian economist W. Arthur Lewis and the many Indian intellectuals who moved in Labour Party circles, some of them passing through Cambridge in the early stages of their academic careers. He would certainly have discussed India's economic problems with Joan Robinson, a lifelong student of the subcontinent who had herself lived there for two years in the late 1920s. He himself first visited India in 1955 when he, the Robinsons and other Cambridge economists gave a refresher course for university teachers of economics in Poona, under the auspices of the University of Bombay (Thirlwall 1987, p. 132).

Kaldor's ambition to serve as economic adviser to the British government was thwarted by the thirteen long years of Conservative rule that began in 1951, but the Third World (as it soon came to be known) offered an almost unlimited market for his professional services. It cannot have been idle curiosity that took him to India, China, Japan and Chile en route to the United States in his 1956 sabbatical world tour. This was the year in which he began to write on development questions, and he never stopped. One of his last papers was on Indian exchange rate policy, and his North-South model of global growth and instability featured prominently in the 1985 Mattioli lectures (Kaldor 1984; 1996). The last of the eight volumes of his *Collected Economic Essays* was devoted entirely to development issues, which also took up about half of volumes 3 and 4 and perhaps one-fifth of volume 6 and of the posthumously published ninth volume. Between 1956 and 1962 he

took advantage of the rapidly falling costs of international air travel, in both time and money, visiting India, Ceylon (now Sri Lanka), Mexico, Ghana, British Guiana (now Guyana) and Turkey as an adviser to their respective governments on questions of taxation. Kaldor learned a lot from his experiences in the developing countries, which influenced his later thinking both on economic theory and on policy issues in rich countries like Britain.

6.2 The development economist

Kaldor's policy recommendations were always informed by his understanding of economic theory, and this was also the case with his work on the economics of development. He was very clear on the type of development economics that he did *not* endorse. As we saw in Chapter 5, Kaldor never had much time for Marxism. He regarded it as a nineteenth-century creed that had been rendered obsolete, at least in the advanced capitalist countries, by the long established tendency for real wages to rise at roughly the same rate as labour productivity without a reduction in the rate of profit. The reform of capitalism in the wake of the Keynesian revolution had destroyed any residual relevance that Marxist ideas might still have had for the Western world (Kaldor 1957a).

He was less critical of the Marxian approach to underdevelopment, as set out in Paul Baran's very influential book, *The Political Economy of Growth* (Baran 1957). Baran argued that poor countries were not really stuck in a 'low-level equilibrium trap' of an essentially Malthusian kind, as mainstream development theorists maintained. On the contrary, the ability of landlords to extract a large proportion of the peasants' crops in the form of rent demonstrated that there was in fact a significant surplus product in even the least developed areas. The key to successful development, Baran urged, was to ensure that this surplus was used productively to expand industrial capacity, and not frittered away on luxury consumption by corrupt and parasitic elites. The ruling classes of the advanced capitalist countries (above all the United States) benefited from their ability to extract surplus from the underdeveloped world, and it was therefore in their interest to keep the poor countries poor and to support local rulers who would also be threatened by genuine economic development.

In later life Kaldor could rarely be bothered to review books, and it is significant that he not only made an exception for Baran but also wrote an unusually long (six-page) review. He was severely critical of

the American Marxist's analysis of 'monopoly capitalism' in the rich countries. 'By comparison the analysis in the second part of the book on the roots and morphology of backwardness is far more interesting, and it is here that Baran poses really important questions' (Kaldor 1958a, p. 167). None the less, Kaldor continued, his argument was unconvincing:

> He pays too little attention (in my view) to the importance of a progressive agriculture which alone can provide the growing food surpluses necessary for industrialization; and he under-estimates the extent to which the reactionary political regimes which hinder social and economic development draw their strength from a native feudal landowning class, rather than from foreign economic interests. Though in some countries Western economic interests are undoubtedly instrumental in maintaining 'comprador regimes', it is, to say the least, a gross exaggeration to suggest that the economic development of the underdeveloped countries is 'profoundly inimical' to the dominant interests of Western capitalism. (*ibid.*, pp. 168–9)

On the contrary, Kaldor concluded, 'the real income of the United States and Western Europe would be considerably enhanced, and not jeopardized, through the economic development of the peoples of Asia, Africa and Latin America'. Evidence for this was provided by 'the record of far-reaching social and political reform of the U.S. administration in Japan (under the leadership of that notorious radical General MacArthur)'. Kaldor argued that the impressive reform of Japanese society carried out under US occupation was 'itself a sufficient refutation of Baran's thesis that the countries of Western capitalism are invariably the supporters of feudal and reactionary forces' (*ibid.*, p. 169).

Why, then, had Western Europe and North America pulled away from the rest of the world so dramatically after 1750? What was responsible for the enormous differences between rich and poor countries in real income per head? Kaldor denied that the answer lay in excessive population growth, lagging technical innovation or inadequate rates of saving and capital accumulation. These were all *consequences* of slow economic growth, and not the fundamental cause. Again he parted company with the Marxists on this issue:

> In my view the greatly accelerated economic development of the last 200 years – the rise of modern capitalism – can only be explained in

> terms of changing human attitudes to risk-taking and profit-making.... The emergence of the business enterprise characteristic of modern capitalism was thus the cause, rather than the result, of the change in the modes of production; and it was the product of social forces that cannot in turn be accounted for by economic or technical factors. (Kaldor 1956b, pp. 20–1)

Kaldor pointed to the survival, especially in agriculture, of a 'traditionalist outlook' that discouraged risk-taking and profit-making. Only by a massive effort to raise the level of education in rural areas could poor countries hope to change the attitudes of the agricultural population. He concluded that the problem of economic development could not be left to the economists:

> a true theory of economic growth will require far broader consideration of the purely sociological aspects – an explanation of how different mental attitudes come to develop in society, why traditionalism, at certain stages of society's development, gives way to rationalism, and so on – than is usual in writings on economic theory. It will require, in other words, some kind of integration of economics and sociology. (*ibid.*, p. 21)

With this methodological position, at least, the Marxists could agree.

In later writings Kaldor's focus was slightly different. He now emphasised the role of social stratification in the countryside rather than the backwardness of an undifferentiated rural population. Latin American countries, in particular, had 'a tremendous dead burden to carry in the form of maintaining the "idle rich"' (Kaldor 1964c, p. 486). In effect he was now accepting an important element in Baran's analysis of underdevelopment – the detrimental effects of 'the unbridled greed of an oligarchical ruling class' (Kaldor 1963, p. 418). 'Hence, while their average income per head is low, the fraction of their national income which accrues to a small minority of individuals is frequently greater than in the rich countries; and a much higher proportion of that income is devoted to personal consumption, and a lower proportion to savings' (*ibid.*, p. 411). In most poor countries taxation was also regressive, reflecting a 'failure to tax the wealthier sectors of the community effectively' (*ibid.*, p. 412). When the rich did save, 'the pattern of investment gets distorted. Too much of the capital accumulation is taken up by the expansion of industries and services which cater mainly to the rich' (*ibid.*, p. 415). Again citing the example of

Japan, Kaldor argued that the most effective remedy was to increase taxes on land:

> This is because the land tax yields not just revenue but the right kind of revenue; it enlarges the supply of foodstuffs to urban areas, and thus the amount of employment that can be offered outside agriculture without creating inflation. It also promotes agricultural efficiency. It encourages the more efficient use of land as well as the transfer of ownership from relatively inefficient to efficient cultivators. (*ibid.*, pp. 413–14)

There are clear signs in this 1963 article of Kaldor's affinity with the structuralist school of development economics and the ideas of its principal advocate, the Argentinean economist Raul Prebisch, whom he had met during his visit to the United Nations Commission for Latin America (UNECLA, or CEPAL in Spanish) in Chile seven years earlier. He was impressed by the structuralist analysis of inflation, which stressed the shortage of foreign exchange and the inelastic supply of agricultural output as the principal causes of high and continuing inflation, not only in Latin America but also in other developing countries like India (Harrod 1965, pp. 802–3). Kaldor later recalled that he had predicted in 1956 that Chilean inflation would not be permanently reduced by the restrictive fiscal and monetary policies implemented under the aegis of the International Monetary Fund, and he had been proved correct (Kaldor 1974b, p. 23, citing Kaldor 1964i). The remedy for Latin American inflation, Kaldor concluded, must also be 'structural', above all 'stimulating domestic food production, which requires far-reaching land reform in most countries, as well as public investment and promotional policies, e.g., in irrigation schemes', while the balance of payments constraint could only be overcome through the provision of subsidies to exporters of manufactured goods, possibly financed by taxes on the incomes of primary producers (Kaldor 1974b, p. 25).

Also like the structuralists, Kaldor believed that it would be a mistake for developing countries to rely on the revenue from the export of primary products to provide the foreign exchange needed for imports of food and capital goods. Until the great stagflation of the 1970s forced him to change his mind he was a supporter of the Prebisch-Singer thesis, according to which there exists a powerful long-run tendency for the price of manufactured goods, produced in the rich countries, to increase relatively to the price of agricultural products and minerals, produced in the poor countries: 'Underdeveloped countries sell in

highly competitive markets; they buy in monopolistic markets. They sell in markets where they are the price takers and buy in markets where the sellers are the price makers, and that makes a tremendous difference. They suffer under a structural handicap' (Kaldor 1964c, p. 467). Industrialisation was therefore the only viable strategy for rapid and sustained economic development. Whether this would be in the interests also of the rich countries, as he had argued (against Baran) in 1957, Kaldor did not on this occasion say.

On one final theoretical question he was in broad agreement with the Marxists: they both rejected 'the "intermediate technology" myth', promoted by his old wartime friend E. F. (Fritz) Schumacher in his best-selling book *Small Is Beautiful* (Schumacher 1973). Kaldor was critical of the

> kind of romanticism which exists about technology in underdeveloped countries in my country in particular, but also exists in India. It exists in Mao's China, it exists in all sorts of places so it is not really ideologically conditioned. This romantic notion is that there is some wonderful, secret technology of development – what some call 'intermediate technology'. (Kaldor 1970c, p. 42)

His reaction was a blunt denial: 'I do not believe that this intermediate technology exists' (*ibid*., p. 43). Its supporters argued that, because poor countries had a lot of labour and relatively little capital, they should use techniques of production with a much lower capital–labour ratio than was used in the rich countries. The problem was, however, that this rule was normally *not* consistent with the agreed principle that poor countries should make the best use of what little capital they had:

> It is a mistake to believe however that more primitive or less mechanized techniques which require less capital per worker are also more economical in capital per unit of output. A lower capital/labour ratio does not necessarily imply a lower capital/output ratio – indeed the reverse is often the case. (Kaldor 1969, p. 51)

Although they were very demanding in terms of fixed capital, modern processes often allowed very substantial reductions in the amount of working capital required. 'Just because processing takes so much longer with nonmechanical methods, the working capital locked up per unit of raw-material input, or per unit of final output, is much greater' (*ibid*., p. 51).

Neither were the defenders of intermediate technology justified in their opposition to urbanisation. Kaldor rejected 'the suggestion that since the great majority of their population live in rural areas, [poor countries] should avoid the high economic and social cost of large urban conglomerations and, instead, bring industrialization to the villages'. Such 'cottage industries' had featured prominently in the early stages of Indian economic planning and in the so-called 'great leap forward' in China in the late 1950s. 'Both of these proved a costly failure'. There were good reasons why industry tended to grow in towns, including 'the availability of manifold specialized skills, know-how and easy access to markets which make it profitable for firms using similar or related processes to be located close to each other' (*ibid.*, p. 50).

None of this entailed that underdeveloped countries should always use the very latest type of Western technology. Kaldor identified three reasons why 'latest may not be best'. Older and less complicated machinery was normally easier to maintain, using the available relatively unskilled labour, and would therefore be in productive use for a longer time; second-hand machinery was often available at relatively low prices; and the latest techniques might require a minimum scale of operation well in excess of the market requirements of a poor economy. But 'intermediate technology' did not offer a viable alternative:

> I cannot for the moment see that underdeveloped countries, if they wish to set out successfully on the road to development, enrichment, increased incomes per person, have any shortcuts, or any fundamentally different approaches from the Anglo-Saxon countries, the former dominions of the British Empire, the United States of America, European countries, or Japan. (Kaldor 1970c, p. 46)

They needed to release labour from the land, encourage high productivity cultivators, and induce a dramatic increase in the proportion employed in the manufacturing sector.

6.3 The policy analyst

Although Kaldor was a strong advocate of industrialisation, he was by no means an uncritical supporter of the import substitution industrialisation that had been adopted by many developing countries in response to the collapse of their export markets in the Great Depression. He had no objection to the principle of import substitution or to the protection of infant industries, which had been 'so successfully pursued by the countries of Western Europe, North America, Japan and other

"developed countries" in the late nineteenth century and the present century' (Kaldor 1974b, p. 20). Kaldor drew upon his own work on the theory of economic growth (above, Chapter 4) to argue that productivity in new industries could grow very rapidly 'under the umbrella of protection' because of increasing returns to scale and learning-by-doing. However, to be successful

> the protective measures must be both moderate and discriminating. They must not encourage the mushroom growth of high-cost enterprises, and the protection itself should be reduced with the growth of domestic output as the industries pass beyond their 'infancy', so as to put them in a position to develop an export potential. Moreover, the policy needs to be selectively applied; the 'light industries' (such as textiles), which require less industrial know-how and smaller scale for efficient production, should be established first and the 'heavy industries' (such as chemicals, steel, and engineering) at a later stage, when the 'light industries' have already passed into the stage in which they export an important share of their output. (*ibid.*, p. 21)

This was a veiled but presumably deliberate attack on the Stalinist approach to industrialisation, in which consumer goods production was sacrificed to the rapid expansion of the capital stock, so that priority was given to 'heavy' over 'light' industry. It seems that Kaldor never discussed this issue with his Cambridge colleague Maurice Dobb, who was the most prominent, and easily the most persuasive, Western advocate of the Stalinist model (Dobb 1960).

Unfortunately the Latin American countries had implemented 'an indiscriminate protection of a rather violent kind', which had encouraged the growth of industries that operated at a cost 'many times as high as the external price (or the world price) of such products' (Kaldor 1974b, p. 21). Few if any of the new industries were ever able to compete in export markets, and one of the long-term legacies of import substitution industrialisation in Latin America was therefore a chronic balance of payments problem. Although he was not a free trader, Kaldor's vision of industrialisation in the Third World was always outward-looking, with growing exports of manufactured goods invariably at the heart of his policy prescriptions. His attitude towards the commercial policies of the rich countries, however, was more nuanced. As he wrote in 1964:

> The main obstacle to the industrial development of the underdeveloped countries lies in the unwillingness of the industrially advanced countries to permit the importation of manufactured goods from

> low-wage countries. This is based on a deep-rooted prejudice (which may have had more justification in pre-Keynesian times than in present circumstances) that such imports threaten the employment and the living standards of their own workers in a way in which imports from high-wage countries do not. (Kaldor 1964b, pp. 504–5)

So long as full employment was maintained in the rich countries, Kaldor argued, wage-earners there would benefit from cheap consumer goods imported from low-wage countries. The Stolper-Samuelson theorem, which claimed that they were likely to lose, was based on a fallacy:

> capital is not like 'land', and its quantity cannot be treated as 'given', irrespective of the distribution of output between industries. When the output of 'capital-intensive' industries expands, the total amount of 'capital' necessarily expands with it; the growth of output in industries with a high output per head and a high capital/labour ratio necessarily grows hand in hand with an accelerated rate of accumulation of capital. (*ibid*., p. 505 n2; cf. Stolper and Samuelson 1941)

Hostility to imports from poor countries was thus mistaken:

> In what way does the development of a textile industry in Hong Kong differ from the invention of a synthetic fibre? A country like the United States would never dream of putting any obstacle in the way of the exploitation of a new invention, however much it may threaten some old-fashioned industry; yet it is perfectly ready to invoke the escape clauses of G.A.T.T. [forerunner of the World Trade Organisation] to protect its own textile industry from the threat of 'market disruption' by means that are wholly inconsistent with its proclaimed economic philosophy. (*ibid*., p. 506)

This passage has an amazingly modern ring to it. Replace 'Hong Kong' with 'China', and 'textile industry' with 'steel industry' or 'consumer electronics': the controversy is as fierce today as it was almost half a century ago.

It is unusual to find Kaldor on the 'establishment', free-market side of any policy controversy, especially one as important and as highly charged as this. Buried in a footnote in a paper in the *Journal of Modern African Studies*, his argument would have been read by very few economists, and it had little or no influence on the subsequent debates. Kaldor never repeated it, and in fact he rarely took the mainstream position on

any question of development policy. As we have seen, he was as hostile to the monetarist position on inflation in developing countries as he was to monetarism in Britain or the United States. At a conference in 1971 he criticised the Chicago theorist Larry Sjaastad for ignoring the fundamental problem of the Latin American economies, which was their inability to bring about an improvement in the internal terms of trade, increasing the price of food relative to the price of manufactured goods:

> In agriculture, however, a rise in food prices in money terms would only serve to improve the terms of trade if the rise in food prices did not call forth an equivalent rise in money wages in industry. But if money wages are always adjusted so as to keep in step with the cost of living, real wages are not reduced and the agricultural terms of trade are not improved (as a result of the inflationary process, at any rate) beyond a certain range. ...
>
> As a result, the 'equilibrium' relationship between industrial wages and food prices can never be established, or if it is momentarily established, it cannot be maintained without both wages and prices rising. This is the essence of 'structural inflation'. (Kaldor 1974b, p. 45)

Deflationary monetary policy would reduce employment but it would have very little effect on money wages, and 'there are obviously severe limits to the extent to which any community would tolerate a reduction in employment as a means of dealing with inflation' (*ibid.*, p. 45).

His structuralist position on Latin American inflation led Kaldor to oppose the orthodox prescription of devaluation as an effective remedy for the region's balance of payments problems. Why not let the exchange rate find its own value, as the monetarists proposed? This would fail, Kaldor argued, because of the same problem of real wage resistance by urban workers:

> The point is that if there is a huge gap between import requirements arising out of the existing structure of production and employment and the country's export capacity, this kind of policy is not feasible since in balancing imports and exports through free market forces the real wage bill would be reduced too much, which means that either employment or real wage per worker would be reduced (or both). This is not politically feasible, or rather, the attempt to do so would lead to more inflation and thereby would recreate or perpetuate the old price structure. We saw this happen on several occasions in

> various Latin American countries, in Chile and in Argentina, for example, where every act of devaluation led to new bouts of wage-induced cost inflation. (*ibid.*, p. 46)

The only effective remedy was structural change, improving industrial efficiency and increasing agricultural productivity through far-reaching land reform.

Kaldor's argument was not confined to Latin America. In 1983, writing as a 'guest contributor' in *Finance and Development*, the house journal of the International Monetary Fund and the World Bank, he criticised the Fund's prescription of exchange rate depreciation as a means by which any developing country could adjust its balance of payments:

> The main objection to this approach is that it assumes devaluation is capable of changing critical price and wage relationships that are the outcome of complex political forces and that could not be changed by domestic political and monetary policies. But it is more likely that a large-scale devaluation will end up by reproducing much the same initial price relationships at the cost of a great deal of additional inflation. (Kaldor 1983c, p. 35)

Under these circumstances it was entirely possible that there was *no* equilibrium exchange rate. As an alternative to depreciation, Kaldor therefore proposed a system of dual exchange rates under which exporters of manufactured goods would enjoy a lower exchange rate than exporters of primary products and importers of anything other than essential inputs for local manufacturing.

In the following year he suggested that a similar system would benefit India. He recommended a general or 'X rate' of 10 rupees per $ and a special 'Y rate' of 15 rupees, the latter applying to manufacturing exports and to imports of essential inputs. This would promote industrial development and permit a reduction in direct controls and quantitative restrictions. 'One of the main purposes of the scheme', he wrote, 'is to take an important step towards trade liberalisation' (Kaldor 1984, p. 1094). *Mutatis mutandis*, similar arguments could be applied to developed countries. One of the most interesting aspects of Kaldor's writing on development economics, in fact, is his recognition that the problems facing the rich countries were not, in the final analysis, very different from those that confronted the poor nations. He sometimes advocated a dual exchange rate to deal with the UK's chronic trade deficit, and his

criticism of Margaret Thatcher's monetarism can be interpreted as an indictment of the unacceptably high cost of policies designed to break the real wage resistance of the British labour movement (see Chapter 7).

6.4 The tax adviser

One final and very important aspect of Kaldor's thinking on development policy came through his work as a tax adviser, in which he achieved a certain notoriety on both sides of politics. The riots that his proposals provoked in Ghana and British Guiana were cited against him by the business lobby group Aims of Industry before the 1964 election (George and Bewlay 1964, p. 5) and by the Labour minister Denis Healey two elections later, in March 1970: 'Nicky Kaldor, that fellow is a revolutionary. He caused revolutions in British Guiana, he is a hopeless man with no political sense at all' (Crossman 1977, p. 850). From Kaldor's perspective things were a little more complicated:

> since I invariably urged the adoption of reforms which put more of the burden of taxation on the privileged minority of the well-to-do, and not only on the broad masses of the population, it earned me (and the governments I advised) a lot of unpopularity, without, I fear, always succeeding in making the property owning classes contribute substantial amounts to the public purse. (Kaldor 1964d, p. xviii)

'The main reason for this', he concluded, 'undoubtedly lay in the fact that the power, behind the scenes, of the wealthy property-owning classes and business interests, proved to be very much greater than the responsible political functionaries (whether Presidents, Prime Ministers or Ministers of Finance) suspected' (*ibid*., p. xviii). Thus Kaldor's advice to the governments of Mexico and Turkey was rejected before the legislative stage. In India there was 'a Parliamentary battle which can have few historical parallels, apart perhaps from Lloyd George's famous fight following upon the 1909 Budget' before the Kaldor-inspired programme of tax reform was defeated. In Ceylon his proposals were passed into law but never enforced, and the Prime Minister, Solomon Bandaranaike, was assassinated; 'he was murdered, I am relieved to know, by some fanatical Buddhist monks, on a racial issue, and not by enraged millionaires' (*ibid*., p. xix).[1] In Ghana and British Guiana, opposition to Kaldor's plans was led not by 'enraged millionaires' but by trade unions, revealing

that 'in under-developed countries the moneyed interest is capable of exerting its influence in strange and unexpected ways' (*ibid.*, pp. xix–xx).

Kaldor took his responsibilities as a tax adviser very seriously indeed. His report on *Indian Tax Reform* took up almost 160 pages when reprinted in volume 8 of his *Collected Economic Essays*, and included a substantial theoretical discussion of the arguments for and against a wealth tax, the taxation of capital gains as income, and (summarising the arguments of his recent book on the subject) the introduction of a progressive expenditure tax. Kaldor concluded that both equity and efficiency considerations required 'taxes on (a) income; (b) capital gains; (c) new wealth; (d) personal expenditure; and (e) gifts. All these can be assessed as a single operation, and on the basis of a single comprehensive return provided by the taxpayer' (Kaldor 1956c, p. 51). Small wonder, perhaps, that his proposals aroused strong opposition from rich Indians. Undeterred, he repeated the exercise for the government of Ceylon. This time he avoided any lengthy analysis of tax theory, but his recommendations were essentially the same: taxes should be levied on income (including company profits), capital gains, wealth, expenditure and gifts, with liability for all five taxes to be assessed simultaneously. The expenditure tax was to be levied in place of the higher brackets of income tax, and would give an exemption for saving. The objective, Kaldor reported, was 'to provide incentives to progress at the same time as to bring about greater social and economic equality' (Kaldor 1960f, p. 190). Once again his proposals provoked strong opposition from the wealthy but secured only weak support among poor Sinhalese, who would have benefited from them.

His experience as a tax adviser, Kaldor acknowledged, 'brought me face to face with realities of *power*, in a setting that is not normally within the province of an economist' (Kaldor 1980a, p. xx; original stress). When he visited Ghana in 1961 he was, at first, 'beguiled by the magic charm of Dr. Nkrumah's personality',[2] but soon discovered that

> the ambience of government was that of a mediaeval court, flamboyant, extravagant and corrupt. An initially strong financial position, based on Ghana's rapidly expanding cocoa output and the high world price of cocoa, had been dissipated in a grasshopper's summer of waste, extravagance, corruption and prestige projects recklessly undertaken, on the basis of short and medium term export credit financing, on the blandishments of European (mainly British) business interests. (Kaldor 1980h, pp. xi–xii)

In British Guiana the 'contrast in atmosphere…was extreme', but the outcome proved unhappily similar. 'Here the Cabinet of Dr. Jagan brought to mind the Elders of some Scottish Presbyterian Church: a group of men of strong puritanical convictions and honesty, though with little political experience or sagacity in wielding power' (*ibid.*, p. xiii).[3] In this instance the enemies of reform exploited racial antagonisms to frustrate plans for a more equitable tax system.

In the final analysis Kaldor was unapologetic:

> In retrospect, I do not think that the advice I gave was wrong. In most underdeveloped countries, where extreme poverty co-exists with great inequality in wealth and consumption, progressive taxation is, in the end, the only alternative to complete expropriation through violent revolution. It is the only alternative instrument for curbing the power of wealth, for mobilising resources for development, and for loosening the paralysing hold of traditional social and economic relationships. The progressive leaders of underdeveloped countries may seem ineffective if judged by immediate results; but they are the only alternatives to Lenin or Mao Tse-Tung. (Kaldor 1964d, p. xx)

Pace Denis Healey, these are not the words of a revolutionary. In the early 1960s, with the Cuban revolution still in its infancy and the Cold War being fought by proxy throughout the newly liberated ex-colonial territories in what was now known as the 'Third World', Kaldor's position was widely shared. As the Australian-born US economist Arthur Smithies wrote, in a review of the relevant volumes of Kaldor's *Collected Economic Papers*: 'Landlords and businessmen had evidently read somewhere that the power to tax is the power to destroy. Some of them may wish they had heeded Kaldor when they get Mao' (Smithies 1966, p. 884).

He was no theoretical revolutionary, either. Kaldor's stress on the importance of tax reform sprang from his belief that markets were important, and relative prices matter; he was never an enthusiast for central planning. But he did accept a significant part of the Marxian case. For Kaldor, income (and wealth) effects were no less important than substitution (or price) effects. The productive use of the surplus product was critical, which was why he believed that it must be transferred, via taxation and land reform, to those who would put it to productive use. His recognition of the realities of power meant that Kaldor's analysis of development issues was inevitably multi-disciplinary. He agreed with Joseph Schumpeter that it was senseless to isolate economics

from its social and institutional background (see, for example, Kaldor 1956b, pp. 20–1). It followed that the narrow, 'one-size-fits-all' economics imperialism of the Washington Consensus must be wrong, as it was based on a denial of this essential principle. Kaldor did not live long enough to attack the Washington Consensus by name, but he would most certainly have opposed it, and his writings on development policy continue to strike a chord with twentieth-century opponents of neoliberal globalisation (Skarstein 2007).

6.5 The great stagflation and the North-South model of instability and growth

The 'golden age' of the world capitalist economy came to a sudden end in 1973, as inflation accelerated, output fell and unemployment increased in all the rich countries, with dramatic consequences for the rest (all except those that were large net exporters of oil). Kaldor's explanation of the great stagflation began in the global North, but as it stressed the importance of primary product prices and hence of the global South it makes sense to deal with it here rather than (for example) in Chapter 5 or Chapter 7. His first extended analysis came in his Presidential Address to the Royal Economic Society in July 1976, which was extended and, to some extent, amended in later work, including the Mattioli lectures and the second annual Hicks lecture in November 1985, which was his last major public appearance. As always he wavered on some points (especially on the validity of the Prebsich-Singer thesis), but for the most part he told a clear, consistent and convincing story about what had gone wrong with the world and what might be done to put things right.

The economic problems of the mid-1970s were unparalleled, Kaldor insisted:

> Nothing of this kind has ever occurred before in peace-time – I mean an inflation of that magnitude encompassing not just one or two countries, but *all* the leading industrial countries of the world. ...
>
> This combination of inflation and economic recession is a new phenomenon, the explanation of which presents an intellectual challenge to economists.
>
> In my view it would be futile to look for a single basic cause – such as the increase in the money supply in all countries, or universal cost-push resulting from collective bargaining – and it would be wrong to suppose that the great acceleration of inflation of the last

> few years was the inevitable sequel of the long creeping inflation which preceded it. (Kaldor 1976, p. 704; original stress)

Collective bargaining did, however, have a lot to do with the 'creeping inflation' that had occurred between 1953 and 1967, which Kaldor attributed very largely to the tendency for wage increases in 'certain "leading" or "key" sectors' to set the pace for the rest of the economy. This reflected the fact that 'the percentage rate of wage increases demanded and obtained in the great majority of settlements in any particular period are [*sic*] imitative in character'. Thus 'there is a kind of chain reaction by means of which any particular standard for wage increases communicates itself through the influence of the principle of "fairness" or "comparability" which is the great social force behind the (long-term) constancy of such differentials' (*ibid*., pp. 708–9).[4] The key sectors tended to have a relatively rapid rate of growth of labour productivity. The comparability principle ensured that such wage increases here flowed on to workers in other sectors, where productivity growth was slower. But these industries tended to be rather uncompetitive, so that employers were able to pass on wage increases in the form of higher 'administered' prices, and slow but persistent inflation was the inevitable consequence.

Why, though, had wage inflation accelerated between 1968 and 1971, even in a country like Britain where unemployment was relatively high, and rising? Kaldor cited the work of his Cambridge colleagues Dudley Jackson, Bert Turner and Frank Wilkinson (1972), who had shown the basic cause to be 'increased trade union militancy mainly attributable to the sharply rising deductions from the pay packet for payment of income tax and [social] insurance contributions'. This meant that quite large increases in gross (pre-tax) pay gave the average worker only a small increase in net (post-tax) earnings – which could well be negative once high and rising rates of price inflation were taken into account (Kaldor 1976, p. 710).

The acceleration of wage inflation was followed by a 'real explosion of commodity prices', beginning in the second half of 1972. This was not just the result of supply constraints, but was also evidence of powerful speculative influences:

> To an unknown extent the currency upheavals following the formal suspension of the gold convertibility of the dollar, together with general inflationary expectations, must have induced a great deal of commodity buying as an inflation-hedge – in the same way as the

> outbreak of the Korean War 25 years earlier led to the rapid rise in commodity prices in anticipation of shortages which in the event did not materialise. (*ibid*., p. 711)

Unlike the Korean War boom, the post-1972 commodity price explosion had not collapsed within a year or so, partly because of the very large oil price increases that took place in 1973, though Kaldor was quick to point out that these came after commodity prices had doubled, not before:

> The danger now is that the rise in commodity prices will bring in its train a new inflationary wave in the industrial countries, causing the repetition of the same kind of process as we experienced in 1974 and 1975, but starting from much higher levels of unemployment. The very jumpiness of commodity prices shows that they are increasingly under the influence of inflationary expectations. (*ibid*., p. 712)

Recognising the importance of expectations in the inflationary process, Kaldor insisted, did not commit him to the Chicago position: 'Without being a "monetarist" I do believe in the importance of inflationary expectations; but unlike the monetarists, I believe they are mainly of importance in markets where speculation is important – i.e., in commodity markets, and not in the labour market or the markets for goods with cost-determined prices' (*ibid*., p. 712 n2).

He derived a broader and much more important theoretical lesson from the events of the mid-1970s. Global macroeconomics had to be done using two-sector models, and must place the terms of trade between primary products and manufactured goods at the centre of the analysis. It was also important to distinguish market-determined from cost-determined prices:

> In the field of primary production the market price is given to the individual producer or consumer, and prices move in direct response to market pressures in the classical manner described by Adam Smith. Changes in prices act as 'signals' for the adjustment of production and consumption in the future. In industry, on the other hand – at least in a modern industrial society where the greater part of production is concentrated in the hands of large corporations – prices are 'administered', i.e. fixed by the producers themselves, and [t]he adjustment of production to changes of demand takes place independently of price changes, through a stock-adjustment

> mechanism....Such 'administered prices' are cost-determined, not 'market-determined'; they are arrived at by applying various percentage additions to direct labour and material costs on account of overheads and profits. Neither profit margins nor labour costs in the industrial sector are particularly responsive to changes in demand. (*ibid.*, p. 705)

This distinction was critically important, since it meant that

> the burden of any maladjustment between the growth of primary production and the growth of manufacturing activities is thrown almost entirely on the commodity markets, the behaviour of which is erratic owing to the large influence of speculative expectations on the holding of stocks, as well as on account of the price-inelasticity of demand, and of the time-lags involved in the adjustment of supply to price changes. (*ibid.*, p. 705)

To make things worse, there was an important asymmetry at work:

> *any* large change in commodity prices – irrespective of whether it is in favour or against the primary producers – tends to have a dampening effect on industrial activity; it retards industrial growth in both cases, instead of retarding it in the one case and stimulating it in the other. (*ibid.*, p. 706)

A decline in primary product prices reduced the incomes of producers, with the consequence that their demand for manufactured goods declined. That was quite straightforward. The consequences of a sharp rise in commodity prices were more complicated, since they involved an increase in industrial prices and profit margins, wage increases to pacify the trade unions, accelerating price inflation, and (at the end of the causal chain) deflationary fiscal and monetary policies that reduced both consumer demand and business investment (*ibid.*, pp. 706–7; cf. Kaldor 1978b, p. 257).

Any satisfactory policy proposals had to begin with the necessity for commodity price stabilisation, and the recognition that this would not be produced by the free market: 'the market mechanism is a highly inefficient regulator for securing continuing adjustment between the growth of availabilities and the growth in requirements for primary products in a manner conducive to the harmonious development of the world economy' (*ibid.*, p. 707). Not for the first (or last) time, Kaldor

concluded that international commodity price agreements were essential, that they should be supported by the holding of substantial buffer stocks of the most important foodstuffs and raw materials, and that these stocks should be used as backing for a new international currency. As he suggested in 1973 in an article in *The Banker*:

> the new reserve asset must be a new creation – some form of a credit balance with the International Monetary Fund, such as Special Drawing Rights at present, which, once created, cannot disappear; even if some of it were issued in connection with the making of a loan, it would not be cancelled through the re-payment of that loan. This primary reserve asset would be more akin therefore to the 'bancor' of the original Keynes plan for an international currency union which envisage that each member country be given an initial quantity of it; any reduction of which for any single country (through an excess of payments over receipts) would automatically swell the balances of other member countries. (Kaldor 1973, p. 990)

Since the buffer stocks would be increased in weak market conditions, as the international authority purchased commodities to prevent a fall in price, and reduced in strong markets as commodities were sold to keep prices down, the system would also serve as a powerful international mechanism for stabilising income and employment in both North and South (Kaldor 1976, pp. 713–14; cf. Kaldor 1952, 1983c, 1996; Hart, Kaldor and Tinbergen 1964).

As Kaldor subsequently noted, there had been continual intervention in grain markets by European and US governments during the golden age, partly to support local farmers and partly to stockpile for strategic purposes. This had maintained prices, even though 'the progress of land-saving agriculture technology proceeded much faster than in any previous period of history' (Kaldor 1978b, pp. 258–9). There were long-term as well as short-term advantages of such interference with the market mechanism, since 'the very stability of prices thus created, by reducing the subjective risk of producers and investors, is likely to call forth greatly enhanced supplies' (*ibid.*, p. 261).

In his Mattioli lectures Kaldor formalised the argument somewhat, without adding anything substantial to it (Kaldor 1996, pp. 39–54).[5] Finally, in the 1985 Hicks lecture, he returned to the important question of the long-run trend in commodity prices, which he had avoided in 1976, rejecting the neo-Malthusian approach taken by the Club of Rome and reaffirming his support for the Prebisch-Singer thesis.

Although the short-run elasticity of supply of primary products was almost certainly lower than that for industrial goods, there was no reason to suppose that this was also the case in the long run: 'indeed, historical experience shows the opposite, since the prior requirements for primary products could be satisfied with a *steadily falling proportion* of labour and capital devoted to the primary sector'. Since primary industries were highly competitive, while oligopoly prevailed in manufacturing, 'the benefits of technical progress of *both* sectors tend to accrue to the *industrial* sector' (Kaldor 1986c, pp. 195, 197; original stress).[6] This was sufficient to cast serious doubt on the fears of the neo-Malthusians (*ibid*., p. 195 n11). Kaldor concluded that 'the physical limits on growth ... have continued to be set by the availabilities of labour in the advanced industrial countries' (*ibid*., p. 197). Here he was returning to a position that he had defended in his 1966 inaugural lecture but had long ago abandoned.

Kaldor's North-South model spawned a substantial academic literature (King 1994, part IV). At the time of writing (June 2008), with oil selling at more than $US130 per barrel and all other commodity prices booming, it may well prove to be more relevant than ever. Late in 2007 the Organisation of Petroleum Exporting Countries (OPEC), the oil-producers' cartel, was being urged to undertake the massive investment needed for a substantial increase in production capacity. 'In some ways you can't blame OPEC for procrastinating', one well-informed journalist wrote 'Prices may seem to be on a relentless march upwards, but in the late 1990s they were in the low teens. If the cartel invests billions in raising capacity just as the world goes into recession and demand falls off a cliff, it could bankrupt its members' (Webb 2007). My point precisely, Kaldor would have replied.

More radical critics of the existing global economic order will probably object that he evaded the central question, which is how to resolve the fundamental conflict of interest between North and South. Kaldor himself would have denied that such a conflict existed: the global recession of the 1970s and 1980s was so profoundly damaging to rich and poor countries alike that anything that could be done to prevent a recurrence would bring huge benefits to everyone. Neo-Marxists like Paul Baran (who died in 1964) might well have asked him why he had ignored the problem of international exploitation. Kaldor said nothing about the transfer of economic surplus from the South to the North, which was an inevitable consequence of unequal exchange between high-wage and low-wage regions.[7] He could also be criticised for failing to say anything about the *level* (as opposed to the greatly reduced

volatility) of commodity prices under his new regime of international agreements supported by buffer stocks. How much should people in the rich countries be expected to pay for primary products from poor countries? What, precisely, is 'fair trade' in a grossly unequal world? Kaldor did not say. Further complaints could be raised against his primary/sector dichotomy which, while highly relevant to the 1950s and 1960s, was already being made to look outdated by the 'new international division of labour' that was already clearly emerging in the 1970s. Low-skilled, low-wage manufacturing now has many of the characteristics of agriculture as far as the poor countries' disadvantages are concerned (Wood 1995), and similar questions of international economic justice arise.

The Kaldor plan for commodity price stabilisation, linked to a far-reaching reform of the international monetary system, was widely propagated at the time (see, e.g., Kaldor 1983d) and continues to attract the attention of scholars (Ussher 2007), but there was never the slightest possibility that it would be adopted by Western governments or by the IMF and the World Bank. One can speculate on the reasons for this, apart (of course) from the growing reluctance in a neoliberal age to countenance any new form of market intervention. Experience with individual commodity price schemes was seldom favourable;[8] international cooperation is always difficult to elicit; and the United States has proved extremely reluctant to give up the seigniorage benefits of maintaining the dollar as the only genuine international currency. In the 1980s there was of course an additional reason for neglecting Kaldor. This was the monetarist panacea that he himself attacked so vigorously (see Chapter 7).

6.6 What development taught Kaldor

If Kaldor made significant contributions to development economics, his work in the area also contributed to his own intellectual development. The Myrdal principle of circular and cumulative causation taught him a lot about the limitations of equilibrium analysis, and his experience of poor countries made him realise that economics of any sort had to be nested in a deeper understanding of the social and political framework of each individual country. These lessons from the problems of development gave Kaldor an aversion to the closed-system modelling that appealed so strongly to the great majority of mainstream economists, and conditioned his own thinking on questions of economic methodology (see Chapter 8, Section 3).

There was also much to be learned at the level of economic theory. First, Kaldor came to realise that the classical theory of international and inter-regional trade needed to be completely rethought, since many of its assumptions were false and its predictions of mutual benefits from trade had not been confirmed by the experience of the global South in its economic relations with the North (Skarstein 2007). Second, there was even more reason to be sceptical of the value of the one-sector growth models that had fascinated him in the 1950s. At the very least, the distinction between agriculture and industry should never be suppressed in the interests of mathematical tractability. For this reason Kaldor concluded that neither the Harrod-Domar nor the neoclassical growth model was at all satisfactory – and the same objections applied to his own Mark I models (see Chapter 4, Section 4). On this question, at least, he thought Michał Kalecki superior to Keynes. Third, the very uneven progress of the poor countries testified to the importance of demand constraints, and the relative unimportance of supply constraints, in the long run as much as in the short run. But it was *export* demand that mattered, since all the components of domestic demand were, in the final analysis, endogenous. If this was a Keynesian message, it was a very unorthodox form of Keynesianism (see also Chapter 7, Section 5).

Finally, Kaldor's thinking on questions of economic policy owed something to his work on development. He drew on his early rejection of the dogma of free trade as applied to the Third World when considering alternatives to devaluation in Britain after 1974. His development experience must also have contributed to his thinking on monetary questions. Kaldor first encountered monetarism in Latin America, where it was regarded by its adherents as a guide to the practice of macroeconomic policy rather than as a purely theoretical construction. This prepared him for its introduction in the West, and qualified him to become the standard-bearer of opposition to monetarism in the 1970s, in the United Kingdom and beyond.

7
The Scourge of Monetarism

7.1 What's wrong with the Quantity Theory?

Kaldor first became interested in economics through his personal experience of a major monetary event. In 1923, aged 15, he was on a family holiday in the Bavarian Alps during the great German hyperinflation. The young tourist watched with fascination as prices were increased several times each day, at an accelerating pace:

> At the same time I noted that translated into dollars, or other stable currencies, the prices of things, despite their constant revision, were extraordinarily low. There was a yawning and widening gap between the prices of goods in terms of local currency and their prices in foreign currency, which were very much lower. These extraordinary phenomena aroused all my curiosity. (Kaldor 1988a, p. 11)

There is no reason to doubt his memories of this youthful adventure, but Kaldor's theoretical recollections have proved to be more contentious. At the end of his life he claimed to have anticipated, in the 1930s, the post-1970 literature on endogenous money to which he made such an important contribution (Kaldor 1982a, p. 22). I shall return to this claim later in this chapter; it has proved to be highly contentious.[1]

When, in the early 1930s, Kaldor was a student and disciple of Friedrich von Hayek, he was therefore also an adherent to the Quantity Theory of money in its Austrian (or neo-Wicksellian) form. As we saw in Chapter 2, he soon broke with Hayek to become a convinced, if always rather idiosyncratic, Keynesian. In 1936 he concluded an article on wage subsidies as a remedy for unemployment with a sceptical account of the views that he had until very recently himself accepted. 'The present writer is not one of

those who believe that the maintenance of a lower level of interest rates necessarily involves a process of "cumulative inflation"'. On the contrary, 'the cumulative effects of an "inflationary" monetary policy need not come into operation so long as there is unemployed labour to draw upon at a given level of wages' (Kaldor 1936c, p. 741). Already we can see Kaldor criticising (if only implicitly) the notion that money causes inflation.

His critique of Pigou, published in the following year, contained a footnote in which Kaldor drew a Hicksian IS-LL diagram, with an upward-sloping LL curve that shifts to the right when money wage rates are reduced, as in Figure 7.1. His argument is worth quoting in full:

> A reduction in money wages cannot affect the position of the IS curve, but it will shift the LL curve to the right; for, by reducing the size of 'working balances' at a given level of real income, it enhances the size of 'idle balances', and thus reduces the interest rate consistent with that level of output. Its effect therefore is exactly the same as that of an increase in the quantity of money or a reduction in liquidity preference. It is, in fact, nothing more than an alternative way of increasing the quantity of money in terms of wage-units. (Kaldor 1937d, p. 752 n2)

Just when it seemed that Kaldor was indeed following Keynes in postulating an exogenous money supply, he opened up a quite different

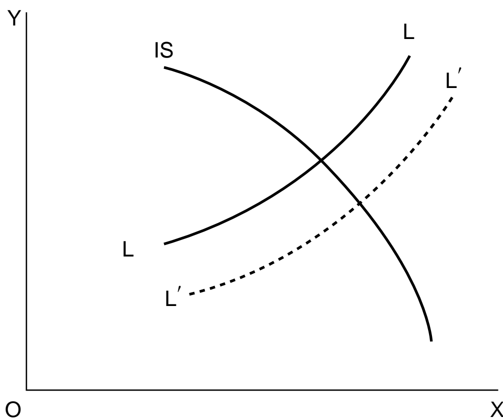

Figure 7.1 Kaldor's IS-LL diagram
Adapted from Kaldor 1937d.

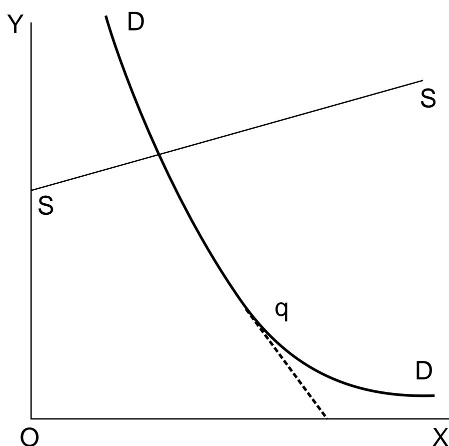

Figure 7.2 Kaldor's 1939 money market diagram
Adapted from Kaldor 1939e.

possibility. The passage just quoted continued as follows: 'If the banking system follows a policy aiming to keep the rate of interest constant, the LL curve will be horizontal and the effect on employment will be nil' (*ibid*.). Kaldor the horizontalist, in 1937! His second macroeconomic diagram came in the justly famous article on 'Speculation and Economic Stability', and is given as Figure 7.2. The money supply curve now *looks* to be almost horizontal (Kaldor 1939a, p. 14 n1). Again, it is open to conflicting interpretations.[2]

In the same year Keynes, as editor of the *Economic Journal*, published Kaldor's review of *The Theory of Prices* by Arthur Marget. The purpose of Marget's book was 'to defend the old-fashioned quantity equations (of the $MV = PT$ type) against the criticisms of Mr. Keynes in Volume I of the *Treatise on Money*' (Kaldor 1939f, p. 496). Thus it did not deal with the *General Theory*, on which a second volume was promised.[3] Marget criticised Keynes for confusing the quantity *equations* with the Quantity *Theory*. According to Marget, Kaldor noted:

> The quantity equations, by themselves, do not carry any such implication as that 'if the quantity of money were to double prices will double', this would only be true if in addition, *M*, *V* and *T* were assumed to be independent variables.... To the charge that the equations thus interpreted are mere 'identities' or 'truisms', he replies that though they are identities, they are not thereby rendered useless.

> They 'represent a summary of the slow growth, over a period of centuries, of our knowledge with respect to the forces determining prices' (*ibid*., p. 496, citing Marget 1938, p. 90)

'With the formal position thus adopted', Kaldor concedes, 'it is not easy to quarrel'. He continued:

> It is obviously possible to give an interpretation to the expression $MV = PT$ which is proof against any conceivable objection or exception. But most readers will continue to associate the quantity equations with the quantity theory, and thus be guilty of the same confusion as Mr. Keynes. Under the assumptions of the quantity theory, where the volume of goods sold per unit of time, the quantity of money, and the real value of cash balances, are determined by forces mutually independent of one another, the equation $MV = PT$ does exhibit the forces determining the price-level in an illuminating manner. In the absence of those assumptions it is difficult to see what purpose it serves. (Kaldor 1939f, pp. 496–7)

But Kaldor went much further than this:

> In fact, continued use of the $MV = PT$ type of equation (or of the $n = pk$ type), even when it is shorn of its wings, as in Professor Marget's interpretation, is positively harmful rather than helpful. It engenders habits of mind which make one oblivious to some of the most fundamental *modi operandi* of economic forces. For people who are used to thinking in terms of these quantity equations it is extraordinarily difficult to bear in mind such propositions as that a change in the quantity of money has (normally) no direct, but only an indirect, effect on the flow of money payments (the effect depending on a consequential change in the rate of interest and on the effect of this change on the scale of investments), or that the effect of a change in the flow of money payments is predominantly on the volume of goods sold, and not on prices, or that the level of prices is determined by the scale of money remunerations of the factors of production, and not by the flow of money payments. All these things are concealed, not exhibited, by the quantity equations. That Professor Marget himself is not entirely free from these habits of mind is shown by his choice of 'The Theory of Prices' as a title. (*ibid*., pp. 497–8)

I have quoted at some length from this rather obscure review of a rather obscure book because Kaldor's attitude towards the Quantity Theory

was set out here more clearly than in any of his other (pre-1970) writings. Money influences the economy only indirectly, through changes in the rate of interest. Changes in the money stock affect output and employment, not prices. The price level depends on the money wage rate, not on the stock of money. Throughout his life these were Kaldor's principal objections to monetarism as an economic theory.

He took relatively little interest in monetary questions for the next two decades, until the British government set up the Radcliffe Committee to inquire into the operation of the monetary system. In his written evidence (dated 23 June 1958), Kaldor started off almost exactly where he had left off in 1939:

> It cannot be emphasised too strongly that there is no direct relationship in a modern community between the amount of money in circulation (whatever definition of 'money supply' is adopted in this connection) and the amount of money spent on goods and services per unit of time. To proceed from the one to the other it is necessary to postulate that changes in the supply of money leave the frequency with which money changes hands (the so-called 'velocity of circulation of money') unaffected.... There are no valid grounds however for any such supposition. (Kaldor 1958c, p. 146)

IMF data indicated that velocity not only varied substantially across countries at any point in time but also, and crucially, that it was far from constant over time. 'Thus in the U.K. there has been a spectacular rise in the velocity of circulation, particularly since 1955 which fully compensated for the failure of the money supply to expand *pari passu* with the rise in prices and in money incomes'. Indeed, the increase in velocity had been the *result* of restrictive monetary policy:

> It could not seriously be maintained that this change in the velocity of circulation was in any sense an *independent* phenomenon which happened to coincide in time with the change in monetary policy. It was simply a reflection of this policy: if the supply of money had not been restricted, the increase in the velocity of circulation would not have taken place and it is a matter of doubt, to say the least, whether the course of prices and incomes would have been any different. (*ibid.*, p. 146; original stress)

As in 1939, Kaldor insisted that money did not have a direct effect on output: 'it is through the consequential changes in interest rates that we

must look for the effects of changes in the money supply on the demand for goods and services' (*ibid*., p. 147). The impact on the price level was even more tenuous. As he stated in his verbal evidence to Radcliffe, in October 1958:

> My own feeling is one of considerable scepticism about the effect of interest rates on the pace of inflation, or even on what one may call inflationary pressure: the pressure upon resources at any one time. I do not believe that the cheap money policy of the Dalton era made inflationary pressure in the years 1947 and 1948 much worse than it would have been in any case, and I do not believe that the rise in gilt-edged rates had a great deal to do with the undoubted easing of pressure on resources which occurred in later years. (Kaldor 1960c, p. 714)

What, then, was responsible for the postwar inflation? In his surprisingly hostile review of the Committee's final *Report*, Kaldor reiterated the central themes of his 1939 analysis. First, there was a 'chronic tendency of money incomes (both wages and profits) to increase at a faster rate than production, thus causing a continued upward drift in money costs and prices'. Second, there was 'volatility in expectations concerning short-period trends in commodity prices', which also contributed to the dangerous instability of inventory investment. Kaldor criticised Radcliffe for neglecting both phenomena and also for pulling its punches on the Quantity Theory, which 'is still the most commonly accepted hypothesis on the relationship between money and prices among the great majority of the world's bankers and a disconcerting number of its economists' (Kaldor 1960d, p. 18). Again he stressed the variability of the velocity of circulation:

> The basis of the quantity theory, and of the whole 'monetary' approach to economic policy which follows from it, is the belief that there is some 'normal' velocity, firmly grounded in long-standing habits and conventions, which brings it about that changes in the *quantity* of money in circulation enforce corresponding variations in the *flow* of monetary expenditure. (*ibid*., p. 19; original stress)

But this was 'a mirage', since 'velocity can be speeded up or slowed down to an almost indefinite extent without any alteration in the habitual frequency of various types of money payment'. This, he concluded, should have been made clear in the *Report*.[4] It was an implicit criticism

of the Quantity Theory, which assumed that velocity was stable. Kaldor did not dwell on the point, and in fact wrote little or nothing on money or monetary policy in the 1960s.[5]

7.2 The new monetarism

This neglect is easily explained. There were few if any supporters of the Quantity Theory in the UK at this time, at least amongst academic economists. Overseas there was more support for the Theory, as Kaldor acknowledged. Its most tireless defender was, of course, Milton Friedman, who would later claim (tendentiously) that he was merely continuing an 'oral tradition' that had begun in Chicago in the interwar years. Friedman's December 1967 Presidential Address to the American Economic Association announced the arrival of Chicago School monetarism in the mainstream of US – and, very quickly, also world – macroeconomics (Friedman 1968). It took root very rapidly in Britain, where the soil had been prepared by free market think-tanks like the Institute of Economic Affairs (IEA), maverick Conservative politicians like the racist Enoch Powell, and a handful of academic economists, most notably Alan Walters (first at Birmingham, then at the LSE) and David Laidler and Michael Parkin (both based at Manchester University before their well-publicised departure for the University of Western Ontario in 1975).[6] Friedman's address was, however, crucial. It came at exactly the right time, just as inflation was beginning to accelerate, and it was very widely read.

Friedman himself was a frequent visitor to the United Kingdom, where the IEA hosted his lectures and published his papers. Monetarist ideas were soon propagated in the 'quality' press, especially by two articulate and persuasive ex-Keynesian journalists, Samuel Brittan of the *Financial Times* and Peter Jay of *The Times*. Jay was especially influential. The son-in-law of James Callaghan, who was Chancellor of the Exchequer (1974–1975) and then Prime Minister (1975–1979), Jay remained a Labour Party supporter throughout the 1970s, and his writings ensured that monetarism won powerful converts well beyond Conservative ranks. It was Jay who wrote Callaghan's infamous speech for the 1976 Labour Party conference announcing the death of Keynesian macroeconomic policy. Further to the right, monetarism was even more gladly seized upon, with Keith Joseph and his Centre for Policy Studies being an important conduit in the mid-1970s. After the debacle of the Heath government, most Tories took very little persuading (Smith 1987; Cockett 1994).

The attraction of Friedman's argument operated on three levels. *Theoretically*, monetarism purported to offer a choice-theoretic model of the demand for money that was consistent with the fundamental tenets of neoclassical microeconomic analysis, and the theory of inflation that he derived from it appeared to be derived from the same, doctrinally respectable, microfoundations.[7] Similarly, Friedman's account of the so-called 'natural rate of unemployment', which was supposedly 'ground out by the Walrasian equations of general equilibrium' (Friedman 1968, p. 8), had an impeccable theoretical lineage even if (as Kaldor was soon to complain) it made very little sense in the real world. *Practically*, Friedman's critique of the Phillips curve and his insistence on the importance of inflationary expectations provided an explanation of the acceleration in wage and price inflation that was just becoming noticeable. Indeed, Friedman claimed to have predicted this acceleration, which his Keynesian opponents certainly had not. In *policy* terms, he offered a simple, easily understood and apparently almost painless remedy for inflation. Abandon Keynesian demand management, which could only hold unemployment down below the natural rate at the cost of an ever-increasing rate of inflation. Instead, enforce the rule that the rate of growth of the money stock should be no greater than the trend rate of growth of real output. Then the price level would soon cease to rise, with only minor damage to output and employment in the brief transition period from inflation to price stability.

At first Kaldor did not take monetarism very seriously as an alternative, anti-Keynesian macroeconomic policy position, outside Latin America at least. His initial attack on the new monetarism came in a public lecture in March 1970, which was published four months later in the widely circulated *Lloyds Bank Review*. There were four elements of the monetarist 'creed', Kaldor suggested. First, only money matters in determining 'money things': money output, the price level and the level of money wages. Hence 'other things – such as fiscal policies, taxation, trade union behaviour, etc. – do not (or do not really) matter'. Second, money cannot change 'real things', except temporarily. 'There is a unique real equilibrium rate of real interest, a unique real equilibrium real wage, an equilibrium level of real unemployment' (Kaldor 1970a, p. 5). Principles one and two were taken from the Austrian school of the 1920s, from Hayek and von Mises.[8]

The third proposition, Kaldor continued, was that changes in money work only with a time lag of between two quarters and eight quarters. 'This is what the regression equations show' (*ibid*., p. 6). One feature

distinguishing the new monetarism from the old was the way in which Friedman invoked

> some rather glittering evidence in terms of 'scientific proofs', obtained through empirical investigations summarised in time-series regression equations. Indeed, the characteristic feature of the new school is 'positivism' and 'scientism'; some would say 'pseudo-scientism', using science as a selling appeal. They certainly use time-series regressions as if they provided the same kind of 'proofs' as controlled experiments in the natural sciences. (*ibid.*, p. 4)[9]

The fourth principle of the new monetarism was the policy rule already alluded to. Contra-cyclical monetary policy was both futile and dangerously inflationary. Restrict the rate of growth of the money supply to 2 per cent, the monetarists claimed, and 'sooner or later, everything will fall into line. There will be steady growth without inflation' (*ibid.*, p. 6).

Kaldor began his critique with a methodological objection: correlation does not entail causation.[10] He gave a simple monetary example. 'Every schoolboy knows' that cash holdings increase at Christmas and fall in January, yet '[n]obody would suggest (not even Professor Friedman, I believe) that the increase in note circulation in December is the cause of the Christmas buying spree' (*ibid.*, pp. 8–9). In fact it is, of course, the effect, and 'the "money supply" is "endogenous," not "exogenous"' (*ibid.*, p. 12). As for the time-lag between monetary expansion and increased economic activity, that was quite irrelevant to the issue of causation. Firms could be expected to borrow to finance investment decisions, so that monetary expansion would normally occur *before* economic activity increased. 'There is every reason for supposing, therefore, that the rise in the "money supply" should precede the rise in income – irrespective of whether the money-increase was a cause or an effect' (*ibid.*, p. 14).

Kaldor's second line of attack was empirical. The velocity of circulation was not in fact stable, so that the 'money multiplier' linking money to expenditure was highly variable. If the monetary authorities tried to prevent the annual Christmas buying spree by restricting the note issue, they would certainly fail. 'There would be chaos for a few days, but soon all kinds of money substitutes would spring up', eventually giving rise to 'a complete surrogate money-system and payments-system ... which would exist side by side with "official money"' (*ibid.*, pp. 9–10). In practice the authorities would never allow this to happen, but would accommodate the demands of the public for cash – just as they do every

December to meet the requirements of Christmas shopping. The Bank of England, like the Federal Reserve in the United States, was in 'the position of a constitutional monarch', possessing 'very wide reserve powers on paper, the maintenance and continuance of which are greatly dependent on the degree of restraint and moderation in their exercise' (*ibid*., p. 12).

Friedman's economic history was no more convincing than his econometrics, Kaldor continued. The severity of the Great Depression in the United States had not been primarily the result of unwarranted monetary contraction carried out by an inept Federal Reserve, as Friedman and Schwartz had claimed. They themselves had demonstrated that this claim was false. In the early 1930s money GNP fell as fast in Canada as in the United States, but with no bank failures and a very much smaller decline in the stock of money. The velocity of circulation thus fell much faster in Canada than in the United States. 'This clearly suggests that the relative stability in the demand for money is a reflection of the instability in its supply; if the supply of money had been kept more stable, the velocity of circulation would have been more *un*stable' (*ibid*., p. 19; original stress).

What, then, for Kaldor, determined the rate of change of the money supply in the United Kingdom? It was not 'under the direct control of the monetary authorities, regulated through the rate of creation of bank reserves' (*ibid*., p. 19). On the contrary, changes in the money supply were determined by the rate of change of money incomes, and so depended on 'all the forces, or factors, which determine this magnitude: the change in the pressure of demand, domestic investment, exports and fiscal policy, on the one hand, and the rate of wage-inflation (which may also be partly influenced by the pressure of demand), on the other hand'. Kaldor emphasised one factor as particularly important: the 'borrowing requirement' of the public sector, which 'has been subject to very large fluctuations year by year' (*ibid*., p. 20).

He would soon change his mind on the link between the Public Sector Borrowing Requirement (PSBR) and the growth of the money supply, but he never wavered in his hostility to monetarism, responding scornfully to Friedman's assertion that 'reverse causation' (from changes in expenditure to changes in the money supply) had always been accepted by the Chicago School (Friedman 1970; Kaldor 1971b). If anything, his hostility intensified after 1970. In what was unusually strong language, even for him, Kaldor accused Friedman (and his co-author Anna Schwartz) of deliberate dishonesty. Reading the devastating

criticisms of their monetary history of the United States by David Hendry and others 'makes it difficult to believe that in writing these elaborate yet worthless defences the authors were intellectually honest in the pursuit of the truth' (Kaldor 1981a, p. xix). 'Since my [1970] paper was published', he wrote in 1978,

> Friedman has gained further influential adherents – politicians of the Right, ranging from General Pinochet in Chile to Sir Keith Joseph in England, numerous important stockbrokers, financial journalists and distinguished editors like Mr. Rees-Mogg of *The Times* and, last but not least, the five economists of the Nobel Prize Committee of the Swedish Academy of Science, who awarded last year's Economic Nobel Prize to Friedman. This last event evoked much the same reaction among the majority of the world's professional economists who have not been converted to the new creed (or not yet) as would have occurred among biologists if Lysenko had been given the Nobel Prize in Physiology and Medicine. (Kaldor 1978a, pp. vii–viii)

And the consequences were no less harmful: 'I regard "monetarism" as a terrible curse', he wrote, 'a visitation of evil spirits…' (Kaldor 1981a, p. 3).

7.3 Monetarism Mark I and Mark II

Once the pernicious Chicago doctrines began to be put into practice in the United Kingdom, Kaldor became a tireless critic of monetarism both as theory and as policy. In lectures at the University of Wales (Kaldor 1981a) and Warwick University, and in evidence to the Treasury and Civil Service Committee (published together in Kaldor 1982a), he criticised the theoretical basis of monetarism in a more considered and more comprehensive way than he had done in 1970. There were four principal elements in his attack.

First, he made a great deal of the important distinction between commodity money and credit money: The Quantity Theory had been devised at a time when 'money' meant gold and silver:

> For the modern monetarists, however, the theory is applied not to commodity money but to credit-money; in other words, to money which consists of the I.O.U.s (or 'promises to pay') of the central bank and of the clearing banks which provide facilities of drawing on accounts by means of transferable certificates of debt (of which

> 'cheques' are the most common example). Such credit money has no 'supply function' in the production sense (since its costs of production are insignificant if not actually zero); it comes into existence as a result of bank lending and is extinguished through the repayment of bank loans. (Kaldor 1981a, p. 15)

It followed that the monetarists were quite wrong to claim that inflation was caused by an excess supply of money, which economic agents hastened to dispose of by spending it. In a credit-money system there could be no such excess supply:

> If ... more money comes into existence than the public, at the given or expected level of incomes or expenditures, wishes to hold, the excess will be automatically *extinguished* – either through debt repayment or its conversion into interest-bearing assets – in a way in which gold could not be made to disappear from existence merely because particular persons find that they have too much of it. They can pass it on to others, but if they have less, others will have more. (Kaldor 1982a, p. 22; original stress)

Second, Kaldor insisted that the monetary authorities could therefore not control the money supply, even if they wished to. In a credit money economy 'the very idea of monetary policy being focused on "money supply targets" (which came into fashion in recent years) is incongruous since the money supply, on any but the narrowest of definitions of this term, is not under the control of the monetary authorities' (Kaldor 1981a, p. 19). Wicksell had been quite clear on this question, although Keynes had been 'curiously blind' to it (*ibid.*, p. 20). As we shall shortly see, this was not the only aspect of the *General Theory* that Kaldor now began to criticise.

His third and most memorable innovation was the money market diagram reproduced in the right-hand panel of Figure 7.3. The left-hand panel shows the conventional Keynesian treatment of the money market, with a set of downward-sloping money demand curves (one for each level of income) and a vertical money supply curve, the stock of money being determined by the decisions of the monetary authorities. Friedman and his followers might have drawn the money demand curve rather differently, with a much lower interest-elasticity, but their money supply curve was also vertical, reflecting their belief (shared with the Keynesians) that it was exogenously determined. The right-hand panel shows Kaldor's endogenous money view. Setting aside his reservations

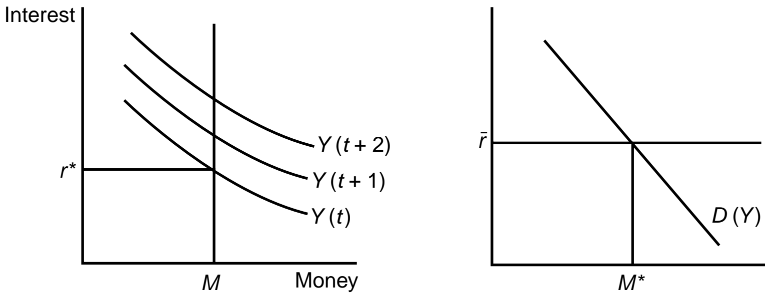

Figure 7.3 Kaldor's 1981 money market diagrams
Adapted from Kaldor 1982a.

about the meaningfulness of a supply curve of credit money, Kaldor drew it as a horizontal line:

> Now, in the case of credit money the proper representation should be a *horizontal* 'supply curve of money' not a vertical one. Monetary policy is represented *not* by a given quantity of money stock but by a *given rate of interest*; and the amount of money in existence will be demand-determined. Demand will vary with incomes as before, and it is possible that the rate of interest of the Central Bank…will be varied upwards or downwards as a means of restricting credit or making credit easier, but this does not alter the fact that at any time, or at all times, the money stock will be determined by demand, and the rate of interest determined by the Central Bank. (Kaldor 1982a, p. 24; original stress)

Kaldor's horizontal money supply curve proved to be a remarkably effective rhetorical and pedagogical device, and was soon being reproduced in classrooms, and eventually in textbooks, around the world. The battle lines between Horizontalists and Verticalists (Moore 1988) were now clearly drawn, representing respectively the supporters of endogenous and exogenous money.

Fourth, Kaldor now linked his attack on monetarism to the more general critique of Walrasian general equilibrium theory and its methodological presumptions that he had begun several years earlier. As he told the Treasury and Civil Service Committee, there was a model of the economy underlying the monetarist view: 'The monetarist propositions could be applied to an imaginary economy, such as it [*sic:* is?] postulated

in Walras's famous model of general equilibrium' (Kaldor 1982a, p. 42). There were many grounds for objecting to this model (Kaldor 1972a; see also Chapter 8). Most pertinent to the critique of monetarism was Walras's treatment of credit money, which Kaldor regarded as entirely unsatisfactory: 'It is not clear from Walras's account just what the function of money is under the highly abstract assumptions of static general equilibrium' (Kaldor 1982a, p. 43 n6), which take no account of time or uncertainty:

> In strict logic Walras's model is timeless: *all* transactions take place at a point of time, at the equilibrium system of prices (which was established beforehand); as all prices are 'market clearing', the intermediation of money appears otiose. Walras, who grafts money as *encaisse desireé* to his general equilibrium system at a late stage, makes no attempt to show how it can be fitted into it. (*ibid*., p. 43 n6)

Kaldor reinforced this point with a critique of New Classical economics, or as he called it (following James Tobin) 'Monetarism Mark II', which was more consistently Walrasian than Friedman had ever been, and therefore threw the absurdity of monetarist reasoning into even sharper focus:

> The great advantage of Monetarism Mark II over Mark I is that its protagonists need not attempt to justify their theories by empirical tests – though they use econometric models in plenty, these serve to quantify predictions (of the type that 'a 1 per cent rise in unemployment lowers the inflation rate *ultimately* by 5 per cent') *given* the specifications of their model, but not to test the specification itself. Indeed, they could hardly be asked to do the latter since the behavioural equations depend so largely on expectations concerning the future, and expectations and their changes have the peculiar property that they cannot be empirically *observed*, which puts their models beyond the realm where they can be refuted. Whatever happens, you can always infer some set of expectations (or better, some set of *changes* in expectations) which explain observed events within the framework of the model. (*ibid*., p. 30)

In his onslaught against Monetarism Mark II Kaldor invoked as allies not only Tobin, a mainstream Keynesian, but also his own Cambridge colleague, critic and friend Frank Hahn, himself a reflective and self-critical practitioner of Walrasian economics.

While they would have agreed with his objections to the New Classical doctrine of rational expectations, which 'goes beyond the basic untestable axioms of the theory of value' to assume that everyone understands the workings of the economy in all their intricate detail (Kaldor and Trevithick 1981, p. 15), neither Tobin nor Hahn could have been counted on to support Kaldor in his broader struggle with orthodox economic theory. In this he was much closer to Post Keynesians like Paul Davidson and Sidney Weintraub, who had also questioned the Chicago assumption that money was much more frequently cause than effect (Davidson and Weintraub 1973). Already in his 1970 paper Kaldor had anticipated both the 'accommodationist' and the 'structuralist' versions of the Post Keynesian theory of endogenous money developed in the 1980s by Marc Lavoie, Basil Moore and others (Pollin 1991; Hewitson 1993). The accommodationists stressed the willingness of the monetary authorities to allow the stock of money to increase at the rate required to maintain acceptable levels of output and employment, so that the money supply 'accommodated itself' to developments outside the money market, in particular to movements in money wages. This had always been an important part of the theory of cost inflation, which relied on the possibility of ' "disequilibrium" price and wage increases being validated by expansive monetary and fiscal policies, resulting from organized pressure on monetary and fiscal authorities' (Bronfenbrenner and Holzman 1964, p. 65). Kaldor went further, arguing that the monetary authorities *had* to take an accommodating position, or they would place at risk the stability of the entire financial system. Since 'the central bank's primary function is to maintain sufficient liquidity in the banking system to prevent a collapse of the credit pyramid' (Kaldor 1981a, p.18), its ability to raise interest rates and restrict the growth of demand for credit money was severely limited. Thus 'the Central Bank cannot close the "discount window" without endangering the solvency of the banking system; they must maintain their function as a "lender of last resort" ' (Kaldor 1982a, p. 25).

The structuralists, for their part, identified financial innovation as the principal reason why credit money was necessarily endogenous. Economic agents, they argued, systematically undermined attempts by the authorities to restrict the money supply by using existing money substitutes and creating new ones. As we have seen, Kaldor recognised the importance of this in his Christmas spending spree parable (Kaldor 1970a, pp. 9–10). Subsequently he pointed to the growth of the Eurocurrency market and the increasing use of credit cards, noting that 'the very rise in interest rates causes the spread of new money substitutes

which have the effect of reducing the public's need for money for making payments' (Kaldor 1981a, p. 18). In a sense, he now argued, structuralism was the most enduring theoretical contribution of the Radcliffe Report, which had stressed 'liquidity', broadly defined, rather than money, narrowly defined (Kaldor 1982a, pp. 1–16).

The other 'ism' that inspired debate among Post Keynesian monetary theorists in the 1980s was Horizontalism. Many writers otherwise sympathetic to Kaldor's analysis of endogenous money believed that he had gone too far by insisting on a horizontal money supply curve. In their view, endogeneity required only that the elasticity of the money supply curve was significantly greater than zero, not that it was infinite (compare Goodhart 1989 and Moore 1991a). These debates reached their most intense point only after Kaldor's death, and there is no way of knowing just how deeply committed he was to the Horizontalist position. But his perfectly elastic money supply curve certainly did make for effective exposition of the endogenous money case.

7.4 An angry old man

These were theoretical questions. What, precisely, was to be expected from monetary *policy*? The short but consistent answer that Kaldor gave to this question, from the late 1930s to the late 1970s, was: not much. As we have seen, he believed that monetary policy worked only indirectly, via the rate of interest, and then affected only investment spending (Kaldor seems never to have been very interested in the possibility that consumer spending might be sensitive to interest rates). He thought that there were two reasons for doubting the strength of this influence. First, it was the short rate that was 'determined by the demand for cash and banking policy', while the long rate – which was much more important for business investment – depended on 'the existing state of expectations' (Kaldor 1941b, p. 467). Second, investment was essentially a matter of expected profitability, and was rather inelastic with respect to changes in interest rates, short or long. 'More powerful measures' would thus be needed to restrain investment in wartime – either restrictive fiscal policy or direct controls (Kaldor 1939c, p. 153). It is not surprising that Kaldor's 58-page Appendix to the Beveridge Report on *Full Employment in a Free Society* was devoted almost entirely to fiscal policy and had nothing to say about monetary policy. The continuation of the wartime policy of cheap money was assumed, but more for its beneficial effects on income distribution than for its contribution to good macroeconomic management (Kaldor 1944a, p. 399).

The postwar Labour government in the United Kingdom did emphasise fiscal policy, as Kaldor acknowledged in 1955. 'Since 1951', however, 'the policies adopted by the Conservative Government have meant that more reliance has been placed on instruments of monetary control and less reliance on fiscal policy than before' (Kaldor 1955a, p. 103). It was not clear whether 'the revival of monetary control through the Bank Rate mechanism' had in fact restored balance of payments equilibrium, as supporters of the government were claiming:

> The experiments with monetary policy in the first half of 1955 thus rather tend to suggest that when the pressure on the economy results from excessive demand, the ordinary weapons of monetary policy may be ineffective in counteracting the trend, or can only be made effective with considerable delay. (*ibid.*, p. 105)

Kaldor concluded that, while monetary policy should be used alongside fiscal policy to control the economy, it would be 'futile to place the main reliance on monetary policy without deliberately using fiscal measures to regulate effective demand' (*ibid.*, p. 108).

He was considerably more sceptical than this in his evidence to Radcliffe. Here he repeated his 1941 objections: the long rate was not easily controlled by policy, and investment spending was relatively interest-inelastic (Kaldor 1958a, p. 147).[11] Only 'much more drastic' changes in interest rates than those hitherto applied in the United Kingdom would allow 'money and credit policy ... to be relied on as the principal instrument of control'. But this would generate a high degree of instability in bond prices, making the capital markets 'far more speculative' and hence much less efficient as an instrument for allocating savings (Kaldor 1958c, p. 148). Kaldor concluded that 'monetary and credit policy represents, at best, a crude and blunt instrument for controlling inflationary and deflationary tendencies in the economy which should be employed only in circumstances in which, and to the extent to which, no superior instrument of control are [*sic*] available' (*ibid.*, p. 149). In his review of Radcliffe, he attributed a similar opinion to the Committee itself: 'This last conclusion – that monetary policy should play a purely passive role in the (short-term) regulation of the economy – is nowhere explicitly put, though the *Report* contains plenty of passages indicating that something like this was at the back of the Committee's mind' (Kaldor 1960d, p. 17).

Kaldor would, of course, eventually be forced to reconsider his view that monetary policy was 'relatively harmless' (Kaldor 1955a, p. 108). In

the 1970 article, however, he still placed relatively little emphasis on monetary policy:

> What, if anything, follows from all this? I have certainly no objection to Friedman's prescription that the best thing to do is to secure a steady expansion of *x* per cent a year in the money supply. But I doubt if this objective is attainable by the instruments of monetary policy in the U.S., let alone in the U.K. (Kaldor 1970a, p. 21)

If Friedman's monetary goal was to be attained, it would be as the result of a successful fiscal policy that tamed the stop-go cycle, together with an incomes policy that held money wage growth in check.

The vicissitudes of the 1974–1979 Labour governments were briefly described in Chapter 5, Section 6. They included a rather half-hearted flirtation with monetarism, which Kaldor heartily disapproved of. When a serious attempt was finally made to implement monetarist ideas, it was by the most extreme administration in living memory. Kaldor was resolute in his opposition to the Thatcher government's economic and social policies. He used his position in the House of Lords to attack the class war that the Conservatives were waging against organised labour and to denounce the huge and quite unnecessary costs of their efforts to overcome inflation. Twenty of his speeches were published in a slim volume entitled (with an obvious bow to Keynes) *The Economic Consequences of Margaret Thatcher* (Kaldor 1983a). He did not worry unduly about causing offence.[12] Thatcher's economic policies could be compared, Kaldor believed, with the deflationary measures introduced by Philip Snowden and Ramsay MacDonald in 1931. Her administration was as bad as the disastrous Brüning government that had paved the way for Hitler's rise to power in the Germany of the early 1930s (*ibid.*, p. 68). At first Kaldor, like many contemporary political commentators, did not believe that Thatcher could win her war. Britain's industrial relations problem was indeed 'one of the most serious and deep-seated in our society' (*ibid.*, p. 59); class antagonism had engendered growing levels of alienation and unacceptable restrictive practices by the trade unions. But Thatcher's approach was bound to fail: 'There is no way of restoring the situation to what it was before trade unions were first established' (*ibid.*, p. 61).

Thatcher, of course, was trying to do precisely this, and eventually she would succeed. In one speech Kaldor quoted from the *Communist Manifesto*: 'The history of mankind is the history of class struggles'. 'I cannot say that I ever believed in this', he told their Lordships

sardonically, 'but the present White Paper [on industrial relations law] makes me feel that there must be more to it than I had once thought' (*ibid.*, p. 40). Eventually Kaldor came to accept that Thatcher was serious, and also began to realise that monetarism itself was increasingly being used as a cover for her class war:

> The centrepiece of the Government's economic strategy, the control of the money supply, however genuinely believed in by some people, is really only a façade or a smokescreen. The important consequence of the strategy is to alter the balance of bargaining power, to weaken the trade unions through the intensification of unemployment and through the loss of jobs, through factory closures and bankruptcies, and thereby to succeed in bringing wage settlements well below the rate of inflation; that is to say, to reduce real wages. (*ibid.*, p. 62)

For all its protestations to the contrary, the government was really fighting cost inflation (*ibid.*, p. 111), imposing a strict wages policy in the public sector and intimidating workers in the private sector by creating 'a "reserve army" as Marx would have called it' (*ibid.*, p. 85). In what Kaldor described as 'a quiet intellectual U-turn by the Friedmanites and the Thatcherites' (*ibid.*, pp. 98–9), mass unemployment was now regarded as a necessary element in the implementation of monetarist policy. The inflation rate had at last been brought down, but only at a massive cost in terms of falling or stagnating output, falling or stagnating living standards, the disappearance of whole industries and the general smell of poverty and decay' (*ibid.*, p. 111). GNP had fallen by 6 per cent, and manufacturing output by no less than 20 per cent, in a policy-induced recession even worse than that of the early 1930s. Kaldor ended by citing Tacitus: 'They create a desert and call it stability' (*ibid.*, p. 114).

Friedman's response to criticism of the Thatcher experience was to claim that monetarist policy had not been intelligently implemented:

> Friedman has admitted that as far as the United Kingdom is concerned, the money supply is *not* exogenously determined by the monetary authorities but he attributed this to the 'gross incompetence' of the Bank of England. However, this puts an entirely new complexion on monetarism. It was nowhere stated in the writings of Friedman or any of his followers that the quantity theory of money *only* applies when the monetary authorities are sufficiently 'competent' to regulate the money supply. (Kaldor 1981a, p. 23)

What were the criteria for central bankers to be 'competent', Kaldor wondered. 'And what happens if they are not. Surely we need a theory of money and prices to cover the cases of countries with incompetent central banks, such as Britain?' (*ibid.*, p. 24).

Much of the damage had been done by deflationary fiscal policy – 'The inversion of Keynes' (Kaldor 1983a, p. 110) – and by a massive rise in the value of the pound, for which the previous Labour government bore much of the responsibility. Denis Healey's decision to float the currency in 1977 had been 'a fatal mistake; even more fatal than the famous return to the gold standard in 1925' (*ibid.*, p. 79). If monetary policy had contributed to the depth of the Thatcher recession it was largely indirectly, through the effects of increased interest rates on the value of the currency. Contrary to Thatcher, Kaldor insisted that there *was* an alternative. 'The great economic recovery of the 1930s' (*ibid.*, p. 86) had been possible only after the devaluation of the pound and the introduction of import controls, which Kaldor now endorsed for the medium term. In the short term, he called for a 6 per cent limit on wage increases in all sectors, combined with cuts in indirect taxation and subsidies on essential goods. There was, he argued, no cause for concern if the PSBR were to increase as a result, since any linkage between public borrowing and the money supply had been severed with the introduction of financial deregulation in 1971 (*ibid.*, p. 67; Kaldor 1982a, pp. 49–52).[13]

In the longer term, a great improvement in the quality of British management was essential, together with an assault on the dominance of finance capital over industrial enterprise and a concerted effort 'to dethrone the accountant as the most important person in industry and enthrone the scientist and the engineer, as in other countries' (Kaldor 1983a, pp. 33, 73, 15).[14] Britain should learn from the experience of other countries, Kaldor urged, where consensus politics and a high degree of state intervention in the economy had produced far superior outcomes. He singled out Germany, Austria, the Netherlands, Scandinavia (especially Norway) and Japan as examples to follow (*ibid.*, p. 92, pp. 94–6). Kaldor was now vigorously defending the public sector – 'We require more public enterprise, not less' (*ibid.*, p. 74) – and, possibly for the first time in his life, showing an interest in economic planning (*ibid.*, p. 95). The failed monetarist experiment had moved him significantly to the left. This was also true with respect to foreign policy, where he now followed his daughter Mary Kaldor in supporting the campaign for European Nuclear Disarmament (END) and opposing the Reagan administration's new and massive escalation of the nuclear arms

race in Europe. Kaldor devoted several of his speeches in the House of Lords to this theme (Kaldor 1981d, 1981e). This was reflected in his views on the Labour Party's 1983 election campaign: 'He feels it is a mistake to be moderate', one Labour politician reported, 'and to draw back from the Manifesto. We should concentrate on two issues – nuclear disarmament and withdrawal from the Common Market – and seek to win over public opinion by presenting them as radical, but vital changes of policy'.[15]

7.5 Limitations of the *General Theory*

He was also forced to reassess his opinion of Keynes. Kaldor had been elected a Fellow of the British Academy in 1963. In May 1982 he gave the Academy's annual Keynes lecture, taking as his title 'Limitations of the *General Theory*' and identifying four principal defects in Keynes's analysis. The first was 'his failure to cut himself loose from one of the main tenets of the quantity theory of money' (Kaldor 1982b, p. 261). Although an avowed critic of the Quantity Theory, Keynes had 'retained the assumption that the "money supply" – the amount of bank money in circulation whether in the form of bank notes or bank deposits – is exogenous, it is independently determined by the monetary authorities' (*ibid.*, p. 262). Kaldor repeated this criticism, at some length, in his address to the Keynes centenary conference at King's College, Cambridge in the following year, and indicated the changes to the theory of liquidity preference that would have been needed in order to incorporate endogenous money into the analysis (Kaldor 1983b, pp. 17–22).

The second criticism of the *General Theory* was microeconomic, and Kaldor regarded it as considerably more serious. Keynes had not repudiated perfect competition. Instead he

> seems to have been unaware of the importance of imperfect competition to his theory – he was content to assume, with Marshall, that each producer maximizes his profits by equating the market price with his marginal costs, ignoring the fact that this condition implies the *full utilization* of capacity of individual firms, and that without excess capacity, production will be supply constrained, irrespective of whether there is full employment or not. (Kaldor 1982b, p. 264; original stress)

If production was to be constrained by demand there must be excess capacity, and this was not consistent with perfect competition. Keynes

had inexplicably failed to recognise this, or to understand that in the real world most industrial markets were oligopolistic.[16] In these circumstances changes in demand affected quantities rather than prices, since in oligopoly prices were typically set by the addition of a constant percentage mark-up to the firm's direct costs of production. Only in the markets for primary products did an increase in demand have an immediate and significant effect on prices (*ibid.*, pp. 266–7). His insistence on oligopoly was also

> one of the respects in which Kalecki's original model is intellectually superior to Keynes's *General Theory*. However, it is very doubtful, to say the least, whether in the absence of Keynes's personality, style and ability to command attention, the ideas alone would have been sufficient to bring about the intellectual break-through which the 'Keynesian revolution' created. (Kaldor 1980a, p. xv n3)

There was more than a hint in Kaldor's discussion of this issue of J. R. Hicks's (1974) distinction between 'fixprice' and 'flexprice' markets, and he did make a brief reference to his old LSE friend and colleague, along with a more substantial acknowledgement of the similar analysis of another Hungarian economist, Janos Kornai (Kaldor 1982b, pp. 266, 264). In the Keynes centenary conference paper, however, Kaldor made much more of the work of the Harvard theorist Martin Weitzman, who:

> has demonstrated that constant returns to scale, strictly interpreted, are a sufficient condition for the absence of 'involuntary unemployment'. The latter arises because a worker who is not offered a job cannot turn himself into his own employer (in the manner originally suggested by Wicksell) since he cannot compete effectively with firms organised for large-scale production. (Kaldor 1983b, p. 12)

In other words, Weitzman had shown that Keynesian macroeconomics, which hinged on the principle of effective demand, was not consistent with Keynes's own assumption of perfect competition. 'There is a sense therefore in which the natural habitat of effective demand macroeconomics is a monopolistically competitive micro-economy. Analogously, perfect competition and classical macroeconomics are natural counterparts' (Weitzman 1982, p. 801, cited by Kaldor 1983b, p. 13). This assertion, which was a fundamental principle of what would soon be

called New Keynesian economics, proved to be highly contentious (see Chapter 8, Section 5).

Kaldor's third criticism of the *General Theory* concerned Keynes's 'failure to deal with all problems connected with international or interregional trade' (Kaldor 1982b, p. 267). This omission had serious theoretical implications. Contrary to Keynes's argument, investment was endogenous, not exogenous, as Britain's disastrous postwar balance of payments problems had demonstrated all too clearly: 'Investment…in part at any rate, was not a truly autonomous factor; it was *induced* by variations in export demand' (*ibid.*, p. 269; original stress). Inevitably, failure to recognise this also had unfortunate implications for economic policy:

> The result of this neglect was that exports as the *main* source of autonomous demand tended to be ignored; the methods of economic management were concentrated on regulating the domestic pressure of demand, aiming at a level which left adequate resources available for exports looked at simply as the means of paying for imports. (*ibid.*, p. 267; original stress)

In the *General Theory* Keynes had 'adopted the traditional fiction of a closed economy', so that there was no mention of Harrod's foreign trade multiplier, 'despite the fact that the latter made its first appearance three years earlier in a Cambridge Economic Handbook of which Keynes was the General Editor' (Kaldor 1983b, pp. 23, 25, referring to Harrod 1933, pp. 106–7). Thus there was no indication in Keynes that 'exports and their rate of growth…are powerful factors determining the level of employment – more powerful perhaps than investment' (Kaldor 1983b, pp. 24–5).

The fourth problem with the *General Theory* followed from the previous two. This was Keynes's 'failure to recognize that owing to the importance of increasing returns in manufacturing, the development of an industrial system is largely self-generated'. Hence 'the postulate of a clearly determined maximum output given by full employment and a "natural" growth potential, given exogenously by the rate of increase in the supply of labour and capital and by the rate of technical progress, is illegitimate' (Kaldor 1982b, pp. 262, 270). Since the growth of labour productivity in manufacturing was closely correlated with the growth of manufacturing output, 'the *effective* supply of labour is itself enlarged beyond the growth in actual numbers'. To this must be added the effects of internal and international labour mobility. Thus 'it is safe to assume that, from the point of view of any particular industrial growth-region,

there are potentially unlimited supplies of labour' (Kaldor 1983b, p. 26). For this reason 'there are no long-run limits to growth on account of supply constraints' (Kaldor 1988b, p. 157). From all this Kaldor drew a profound methodological conclusion: history matters, and 'events of the recent past can only be explained in terms of the actual sequence through which the system has progressed; history enters into the causation of events in an essential way' (Pekkarinen 1979, p. 112, cited by Kaldor 1982b, p. 262). This principle of path-dependence, apparently unknown to Keynes, formed a cornerstone of Kaldor's own critique of neoclassical equilibrium theory, as we shall see in Chapter 8.

These were his theoretical (and methodological) objections to Keynes. The defects in the *General Theory* invited misinterpretation, which had very soon arrived:

> The period following the Second World War was in some ways a period of 'counter-reformation' of economics. Coming after the stirring 1930s, when the whole traditional theory came under attack (partly on account of the theories of imperfect or monopolistic competition and partly as a result of Keynesian macroeconomics) the 'mainstream' effort of the post-war era was to resuscitate traditional theory and to isolate (if not eliminate) the effects of the intellectual revolution of the thirties. (Kaldor 1978c, p. vii)

Although it was fair to say that '[t]he real author of the so-called "neoclassical synthesis" was not Paul Samuelson, it was Keynes himself' (Kaldor 1983b, p. 47), Kaldor had never been impressed by it:

> I found this 'synthesis' of Keynesian macro-economics with Walrasian (or Marshallian) micro-economics intellectually barren and irrelevant. I felt instinctively and intuitively that the validity of the main propositions of traditional value theory are confined within the narrowly defined framework of static economics with perfect markets, perfect competition, perfect foresight, the universal rule of constant returns to scale, and so on, and hence that it is hopeless to build on them or to marry them with the tools of macro-economics which operate with empirically measurable concepts and aim at the formulation of testable hypotheses – not in an imaginary world, but in the world actually observed'. (Kaldor 1978c, pp. vii–viii)

He had fewer reservations about Keynes's work as a policy adviser. His indictment of the return to gold in 1925 was 'the earliest paper of Keynes

which could have been written today, and which would be just as powerful as a criticism of the current policies of Mrs. Thatcher as those of Stanley Baldwin' (Kaldor 1982c, pp. 6–7). Monetary policy, Keynes had written prophetically, was 'simply a campaign against the standard of life of the working classes' (Keynes 1925, p. 28, cited by Kaldor 1982c, p. 7). Kaldor drew on the recently published oral evidence that Keynes had given to the Macmillan Committee in 1929, which he described as 'a masterly effort', far superior to the *Treatise on Money* and in certain respects also better than the policy sections of the *General Theory*. Although he had been a passionate free-trader in the early 1920s, by 1929 Keynes had become 'a reluctant protectionist' (1982c, p. 12). Just two years later he was advocating devaluation of the pound and the introduction of protective tariffs to reduce unemployment (Kaldor 1982c, pp. 12–21).

As we saw in Chapters 5 and 6, this was not far from Kaldor's own position on questions of trade policy in the final decade of his life. He vacillated on the question of the exchange rate, sometimes doubting (after the failure of the 1967 devaluation) whether currency depreciation had anything to be said in its favour but insisting on the disastrous consequences of the Healey-Thatcher appreciation of 1977–1981. Kaldor's own preference was for Britain to adopt a dual exchange rate, which he had long advocated for developing countries. On the necessity for selective import controls, however, he never wavered.

7.6 A Pyrrhic victory

Kaldor did not win his war on monetarism, but he did not exactly lose it either. By the mid-1990s the 'new consensus' in mainstream macroeconomics included a rejection of the LM half of the old IS-LM model, a rather grudging recognition that the money supply was endogenous after all, and a new 'Taylor rule' equation, to replace the LM curve, in which governments (or 'independent' central banks acting on their behalf) used interest rates as their monetary policy instrument. This was Horizontalism in everything but name (Kriesler and Lavoie 2007). If Kaldor had been vindicated at the theoretical level, however, he had been resoundingly defeated in terms of practical politics. It was not just that Margaret Thatcher survived her creation of the worst recession since the 1930s to win resounding election victories in 1983 and 1987, though that was bad enough. More fundamentally, monetarist values triumphed with her, among politicians and professional economists alike. It came to be generally believed that fiscal policy was inevitably

ineffective (Setterfield 2007), and that in the long run (and perhaps also in the short run) monetary policy had no lasting effect on anything except the inflation rate. This was a tacit – often an explicit – endorsement of Say's Law and a denial of the essence of Keynes's *General Theory*, even by economists who claimed to be Keynesian. By the early 1990s these pre-Keynesian ideas were hegemonic, inside and outside of academia, in the Labour Party no less than the Conservatives.

Thus Kaldor's victory over Friedman was a Pyrrhic one. None the less, his important contribution to the war on monetarism was often recognised in the period immediately after his death in 1986. In her brief but comprehensive intellectual history of money endogeneity theory, Gillian Hewitson quite rightly emphasised Kaldor's role in the initial Post Keynesian response to Chicago macroeconomics (Hewitson 1993, pp. 150–1). Basil Moore went further, describing Kaldor as 'the first English-speaking economist to have fully perceived [the] relationship' between an increase in income and the resulting endogenous increase in the money supply (Moore 1988, p. 4 n2). And Meghnad Desai, writing in the Kaldor memorial issue of the *Cambridge Journal of Economics*, concluded that 'Kaldor's intuition about the nature of empirical support for monetarist theories was entirely sound' (Desai 1989, p. 178). Once again, policy concerns had led Kaldor to make a critical reassessment of economic theory and the methodological assumptions that lay behind it. Kaldor's attack on equilibrium economics is the subject of Chapter 8.

8
The Irrelevance of Equilibrium Economics

8.1 Economics in crisis

By the early 1970s there was widespread discontent with the state of academic economics. To a very large extent this was a product of the radical student movement of the previous decade, which produced a generation of young graduate researchers and junior academic staff whose thorough-going rejection of neoclassical economics was ideologically informed, with (somewhat tangled) roots in Marxist, feminist and populist thought. Mainstream economists still remember those years with discomfort (Barber 1996, p. 24). They were one or two spectacular converts, like the prominent monetary theorist John Gurley, who defended radical economics and denounced the complacency of the neoclassicals at the 1970 meetings of the American Economic Association (AEA) (Gurley 1971). Movements such as the Union for Radical Political Economics (founded in 1968) in the United States, and the Conference of Socialist Economists (founded in 1970) in the United Kingdom, were widely supported, especially by younger economists. They successfully established journals – the *Review of Radical Political Economics* and the Conference's *Bulletin*, soon renamed *Capital and Class* – for the dissemination of radical ideas (Lee 2001, 2007).

Dissatisfaction with the state of mainstream economic analysis had never been greater. A flood of articles appeared, in leading journals and under the signature of eminent economists, all of them asking, with Benjamin Ward (1972), *What's Wrong With Economics?* Wassily Leontief (1971) devoted his December 1970 Presidential address to the AEA to an articulation of his worries about the contemporary state of the discipline. Leontief complained about excessive formalism, the unthinking use of mathematics and the apparent lack of interest in generating data

for use by empirical researchers. In his 1971 Presidential address to the Royal Economic Society, entitled 'The Underdevelopment of Economics', Henry Phelps Brown (1972) accused the profession of theoretical involution. The ever more elaborate models of resource allocation and growth, and increasingly refined econometric techniques, were very largely irrelevant to the real-world problems of economic development, cost inflation and environmental crisis. Real progress required the adoption of an openly multidisciplinary approach, with a substantial historical content and a radical change in professional values to reward powers of observation much more highly relative to powers of abstraction.

David Worswick (1972) made similar criticisms in his Presidential Address to Section F of the British Association for the Advancement of Science. He doubted that economists were any better qualified in 1971 than they had been twenty years earlier to deal with such issues as the causes of inflation or international differences in growth rates. Abstract economic theory, he complained, had become increasingly detached from reality. These complaints were echoed by Oskar Morgenstern (1972), who bemoaned the over-formalism of economic theory, its empirical irrelevance and the unwarranted emphasis placed on models of competitive general equilibrium. In his book Ward (1972) offered an even more comprehensive critique. Neoclassical economic theory was ideologically contaminated, he argued. This restricted both the problems it was able to consider and the techniques used to address them, so that the discipline was in a state of Kuhnian crisis. In *The Structure of Scientific Revolutions* Thomas Kuhn (1962) had set out what proved to be a highly influential theory of scientific paradigm change, in which the growth of anomalies generated a state of crisis that could be resolved only by a scientific revolution, ushering in a new and fundamentally incommensurable paradigm to replace the old, discredited one. The most celebrated example, Kuhn suggested, was the Copernican revolution in astronomy.

Even staunch defenders of orthodox economics like Frank Hahn had to concede the seriousness of the problem: 'All this is not to say that there is not, indeed, a kind of crisis in economics at present. The gap between theory and fact is far too large, and in some sense becoming larger' (Hahn 1972, p. 206). Kaldor's colleague and erstwhile friend Joan Robinson was in the thick of it. Her *Economic Heresies* offered a provocative and wide-ranging survey of contemporary developments and discontents in economic theory, in which she dismissed Walrasian general equilibrium models as relevant only to prison camps where there was no production, merely exchange of unwanted items from the inmates'

regular Red Cross parcels (Robinson 1971, pp. 4–6). In December 1971 Robinson gave the keynote Richard T. Ely address at the New Orleans meeting of the AEA (Robinson 1972), where she dissected what she termed 'the second crisis of economic theory'. For Robinson the first crisis had been the profession's failure to make any sense of the Great Depression, and it had been resolved by the Keynesian revolution in macroeconomics. The second crisis involved the inability of neoclassical economic theory to deal with global environmental problems, and would culminate (she maintained) in the adoption of a radically new analytical framework.[1] Robinson's lecture 'was greeted by an overflow audience with enthusiasm rarely seen at academic gatherings' (Fels 1972, p. ix; cf. Turner 1993, pp. 182–4). 'The young ones got the points', she reported to Richard Kahn, 'and everyone clapped and cheered. I was looking round to see if anyone had the moral courage to remain seated at the end but I think no-one did'.[2]

8.2 The irrelevance of equilibrium economics

Kaldor was never quite as flamboyant as this, and he would have been uncomfortable with the Marxist company that Robinson was keeping. But he did make a sustained contribution to the radical attack on mainstream economics, beginning with an *Economic Journal* article in 1972 attacking 'The Irrelevance of Equilibrium Economics'. More specifically, his target was the general equilibrium analysis of Walras and Debreu, which was probably then just past the height of its prestige and influence within the academic economics profession.[3] He regarded Walrasian theory as '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, whether induced by political action or by other causes' (Kaldor 1972a, p. 1237). Even worse:

> I should go further and say that the powerful attraction of the habits of thought engendered by 'equilibrium economics' has become a major obstacle to the development of economics as a *science* – meaning by the term 'science' a body of 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. (*ibid*., p. 1237; original stress)

A Popperian would object at this point that hypotheses can only be falsified; verification is in principle impossible (Popper 1959). Kaldor

could, however, have conceded this important point without seriously damaging his argument.[4]

In science, he continued, the basic assumptions were always based on observation of the phenomena that were being studied, while in contrast

> the basic assumptions of economic theory are either of a kind that are unverifiable – such as that producers 'maximise' their profits or consumers 'maximise' their utility – or of a kind which are directly contradicted by observation – for example, perfect competition, perfect divisibility, linear-homogenous and continuously differentiable production functions, wholly impersonal market relations, exclusive role of prices in information flows and perfect knowledge of all relevant prices by all agents and perfect foresight. (*ibid.*, p. 1238)

Far from making progress, Kaldor maintained, economics was actually going backwards in terms of its scientific status: 'the ship appears to be much further away from the shore now than it appeared to its originators in the nineteenth century' (*ibid.*, p. 1239). He acknowledged that there had been a considerable refinement of statistical techniques. But ' "econometrics" leads nowhere – the careful accumulation and sifting of statistics and the development of refined methods of statistical inference cannot make up for the lack of any basic understanding of how the actual economy works' (*ibid.*, p. 1240).

This was the crucial methodological failure of equilibrium economics. Its fundamental error in terms of substantive theory had been made 'when the theory of value took over the centre of the stage – which meant focusing attention on the *allocative* functions of markets to the exclusion of their *creative* functions – as an instrument for transmitting impulses to economic change' (*ibid.*, p. 1240; original stress). This mistake, Kaldor suggested, was closely related to another one: the assumption of constant returns to scale instead of increasing returns. Here he returned to the work of his old teacher Allyn Young, which he now believed to be profoundly subversive of the entire Walrasian project:

> The first and most important casualty is the notion of 'general equilibrium' as such. The very notion of 'general equilibrium' carries the implication that it is legitimate to assume that the operation of economic forces is constrained by a set of exogenous variables which are 'given' from the outside and stable over time. It assumes that economic forces operate in an environment that is 'imposed' on the system in a sense other than being just a heritage of the past – one could almost say an environment which, in its most significant characteristics, is independent of history. (*ibid.*, p. 1244)

Young's analysis shattered this presumption, which had to be replaced by the concept of path-dependence:

> Once however we allow for increasing returns, the forces making for continuous changes are *endogenous* – 'they are engendered from within the economic system'[5] – and the actual state of the economy during any one 'period' cannot be predicted except as a result of the sequence of events in previous periods which led up to it. (*ibid.*, p. 1244; original stress)

A broadly similar conclusion had been reached by Gunnar Myrdal (1957). To take it seriously, Kaldor continued, would entail a far-reaching reappraisal of the very nature of economic theory:

> When every change in the use of resources – every reorganisation of productive activities – creates the opportunity for a further change *which would not have existed otherwise*, the notion of an 'optimum' allocation of resources – when every particular resource makes as great or greater contribution to output in its actual use as in any alternative use – becomes a meaningless and contradictory notion: the pattern of the use of resources at any one time can be no more than a link in the chain of an unending sequence and the very distinction, vital to equilibrium economics, between resource-creation and resource-allocation loses its validity. (*ibid.*, p. 1245)

Kaldor concluded that the principle of cumulative causation must be grafted onto Keynesian macroeconomics. This 'marriage of the Smith-Young doctrine of increasing returns with the Keynesian doctrine of effective demand' (*ibid.*, p. 1251) would have some important implications:

> First, the sharp distinction made by Keynes between a 'full employment' situation where real income is confined by resource-endowment, and an unemployment situation where it is limited by effective demand, disappears in the presence of increasing returns. Except in a purely short-term sense, total output can never be *confined* by resources. (*ibid.*, p. 1251; original stress)

In the long-run, output is constrained only by demand. Kaldor immediately qualified this sweeping conclusion, restricting it to labour (which can always be redeployed to greater productive effect) and capital (which

can always be accumulated more rapidly when required). If the rate of growth is, none the less, constrained, 'it must be on account of the scarcity of natural resources' in the context of 'an insufficiency of land-saving innovations' (*ibid.*, p. 1251).

The second implication concerned the way in which the market worked in a capitalist economy:

> it is evident that the co-existence of increasing returns and competition – emphasised by Young and also by Marx, but wholly excluded by the axiomatic framework of Walrasian economics – is a very prominent feature of de-centralised economic systems but the manner of functioning of which is still a largely uncharted territory for the economist. We have no clear idea of *how* competition works in circumstances where each producer faces a limited market as regards *sales* and yet a highly competitive market as regards *price*. (*ibid.*, pp. 1251–2; original stress)

The rare favourable reference to Marx is intriguing, but Kaldor did not follow it up.[6] He went straight on to the third important implication, which was that 'the "self-sustained growth" of decentralised economic systems, largely directed, not by exogenous factors, but by the growth and the constellation of demand, is a fragile thing which will only proceed in a satisfactory manner if a number of favourable factors are present simultaneously'. These included appropriate behaviour by merchants and manufacturers, and 'a "passive" monetary and banking system which allows the money supply to grow in automatic response to an increased demand for credit' (*ibid.*, p. 1252). Most important of all, Kaldor concluded, self-sustained growth required active government intervention to allow the continuous growth of real purchasing power, through Keynesian fiscal policy and through government-operated buffer stocks to maintain commodity prices.

In a Harvard lecture two years later he added two further criticisms of general equilibrium theory. First, the Walrasians exaggerated the role of substitution, which had been 'elevated to the central principle on the basis of which both the price system and the production system are explained; and it is implied that the world is one where elasticities of substitution are all important' (Kaldor 1975a, p. 348). This, he objected,

> ignores the essential complementarity between different factors of production (such as capital and labor) or different types of activities (such as that between primary, secondary, and tertiary sectors of the

> economy), which is far more important for an understanding of the laws of change and development of the economy than the substitution aspect. (*ibid*., p. 348)

Kaldor's second new criticism concerned the treatment of labour in models of general equilibrium. Unlike the prices of all other commodities and resources, he noted, the real wage could never fall to zero when labour was in excess supply, but must remain at least equal to the conventional (or in some cases the physiological) minimum of subsistence. This was a condition for the reproducibility of the entire economic system. But this rather obvious point had fatal consequences for Walrasian thinking, since it entailed that there might be circumstances in which it was not possible for all markets to attain simultaneous equilibrium (*ibid*., p. 351–2). Even on its own terms, general equilibrium theory simply did not work.

8.3 Kaldor on method

The critics of neoclassical theory were also making arguments about methodology. In Kaldor's case this did not come easily. The philosophy of science did not play a major role in his intellectual life; there are few if any references to philosophical issues in his published work, and none of his biographers discuss this question in any depth. Other Cambridge economists had a profound and well-documented interest in philosophical and methodological questions. This is of course true of Keynes himself (O'Donnell 1989) and of Piero Sraffa, whose influence over Wittgenstein is a topic of abiding interest (Sen 2003). Joan Robinson's very first publication was on methodology (Robinson 1932). Thirty years later she published a small but incisive book with the title *Economic Philosophy* (Robinson 1962), and questions of methodology were raised, explicitly and repeatedly, throughout her theoretical work.

The position was quite different with Kaldor. He 'was never an avid reader' (Thirlwall 1987, p. 24), and is unlikely to have picked up much philosophy in the course of his undergraduate or postgraduate studies. Philosophy 'became central to LSE only in 1945' (Dahrendorf 1995, p. 205), with the arrival of Karl Popper. Before then the subject was taught by Abraham Wolf, a Spinoza scholar and historian of science whose appointment was a joint one with University College. As an undergraduate Kaldor would presumably have attended Wolf's lectures, for which the prescribed text would have been his *Essentials of Scientific Methodology*, a rather old-fashioned book which covered the standard

topics of induction, probability, the deductive-inductive method, elementary statistics and scientific laws. Its recommended reading included work by John Stuart Mill, John Neville Keynes, John Maynard Keynes, Karl Pearson, John Venn and George Yule, but it was certainly not at the cutting edge of contemporary developments in the philosophy of science (Wolf 1925).

Of Kaldor's other teachers at the LSE, Allyn Young seems to have had no serious philosophical interests (Blitch 1995). Lionel Robbins, although the author of a famous text on economic methodology, believed himself only to be defending common sense (the fact-value dichotomy) and rudimentary Austrian economics (the principle of scarcity) in his *Nature and Significance of Economic Science* (Robbins 1934; cf. Robbins 1971). With Hayek the story is slightly more complicated. He was close to the Vienna Circle in the 1920s and claimed to have developed 'views on the philosophy of science rather similar to, but of course much less clearly formulated than, those which Karl Popper formed from much the same experience' (Kriesge and Wenar 1994, p. 49). So Kaldor may well have acquired a Popperian version of positivism from Hayek in the early 1930s. But, as Terence Hutchison has emphasised, at this stage in his intellectual development Hayek himself was less than entirely consistent, making 'strong claims for prediction and forecasting' that he would later repudiate (Hutchison 1981, p. 211). Kaldor may also have learned about positivist thinking from his long friendship with John von Neumann, though this is not mentioned in his own memoir of the great Hungarian mathematician (Kaldor 1986b, pp. 43–6; 1989).

We may perhaps draw two conclusions from all this. First, Kaldor was essentially self-taught as an economic methodologist. Second, his methodological views emerged from his critique of orthodox economic theory, and did not precede or precipitate this critique. Indeed, we can go a little further and suggest that it was not until Kaldor recognised himself to be a heterodox economist, fundamentally at odds with mainstream theory, that he began to reflect in any depth on methodological issues. Thus his writings on the philosophy of economics were much more extensive and much deeper in the final 15 years of his life, when the dissident nature of his ideas was most obvious to him, and to others.

There was one exception to this rule. In 1955 Kaldor published a two-page 'Rejoinder' in response to David McCord Wright's 'methodological footnote' to his first major paper on growth and fluctuations (Kaldor 1954). Wright complained about the highly abstract nature of Kaldor's work, asking, bluntly, 'where is reality?' (Wright 1954, p. 624). In his

reply Kaldor stated a principle that would later form an important part of his critique of mainstream equilibrium theorising: 'simple analytical models should merely be regarded as a starting point; and they should serve as a guide to, and not a substitute for, empirical research' (Kaldor 1955b, p. 158). He went on to insist on the need for (relatively) realistic assumptions: 'A "static" model of the trade cycle is not intended to "explain" non-existing phenomena of a purely imaginary world: the purpose of the static abstraction, here and elsewhere, is to enable us to isolate the relevant factors from the irrelevant ones *in the world as it exists*' (*ibid*., p. 158). The emphasis here is Kaldor's. It was a point that he would make over and over again in subsequent work.[7] He already viewed theory as a construction that should reflect the innovative use of insights generated from empirical data: 'Scientific hypotheses are invented in order to account for the phenomena actually observed' (*ibid*., p. 158).

As we saw in Chapter 4, Kaldor's Mark I and Mark II models of economic growth were highly simplified, one-sector affairs that he subsequently rejected as insufficiently realistic, and as unhelpful for policy purposes. There are, however, many references in his theoretical writings of the 1950s and early 1960s to the dangers of excessive abstraction. For one thing, he always believed (like Keynes) that economics was a policy science, so that an understanding of 'causative influences' was essential if economists were to be able to employ their 'powers of control' to achieve desirable policy outcomes. As he told a Chinese audience in 1956, 'Western Socialists like myself believe that men can control the endogenous forces of human society in much the same way as through science we can control the forces of nature', even if there was still a long way to go in understanding the causes of cost inflation or the tendency towards oligopoly in the great majority of industries (Kaldor 1957a, p. 175). Moreover, his emphasis on the use of 'stylised facts' in theory construction was a constant theme in his writings on growth (even if his critics sometimes quibbled over the factual basis of some of his empirical generalisations, like the constancy of relative income shares or of the capital-output ratio). He always believed that 'theoretical analysis, to be fruitful, must be closely related to, and firmly based on, empirically derived "laws" or regularities' (Kaldor 1978c, p. viii). For this reason Kaldor rejected theoretical assumptions that were introduced solely to improve the mathematical tractability of particular economic models:

> I can readily believe that it is always possible to introduce such additional assumptions into any particular 'model' as would destroy

> the '*a priori* necessity' of its conclusions. But if these assumptions are merely chosen at random (without any regard to their relevance to actual situations) they do not prove anything for or against an hypothesis. ... they should serve as a guide to, and not a substitute for, empirical research. (Kaldor 1955b, p. 158)

He was not, however, a naïve empiricist. As he told his Chinese audience in 1956: 'It is not sufficient to appeal to historical facts in order to refute a theoretical scheme. We want to know why things happen in a certain way and why they do not happen in some other way – in the way in which Marx predicted them. Without a theoretical scheme which is capable of explaining historical developments we are merely groping in the dark' (Kaldor 1957a, p. 175).

Kaldor's attitude towards econometrics was somewhat ambivalent. He was not a confident practitioner, having required private tuition in mathematics in order to pass his first-year examination in the subject at the LSE, at the second attempt (Thirlwall 1987, p. 19). Yet he did occasionally call on his research associates for assistance with the use of econometric techniques, for example in his 1966 inaugural lecture (Kaldor 1966a, pp. 33–40). Twenty-two years earlier his long Appendix to the Beveridge Report, written in collaboration with Tibor Barna,[8] had been derived from what was, in effect, a rather primitive large-scale econometric model, and Kaldor remained proud of it:

> If the number of econometric models that have been constructed since is any guide, the method has certainly established itself as a working tool, even though the basic shortcoming of all such estimates (which lies in assuming that the future will be like the past in all relevant matters ...) can never be wholly overcome by more extensive statistical knowledge or further theoretical refinement. (Kaldor 1964d, p. x)

The reference to 'assuming that the future will be like the past' is especially problematic. It can be interpreted either as a root-and-branch dismissal of the very possibility of forecasting economic variables in a non-ergodic universe,[9] or as a simple acknowledgement that parameter values frequently change over time. Kaldor always recognised the difficulty of making accurate predictions. The crucial point is that he never regarded prediction as the most important – still less, the only – task of the empirical economist.

This raises the very interesting question of Kaldor's relationship to Critical Realism, a school of thought associated with the philosopher of science Roy Bhaskar (1989) that has attracted considerable interest from Post Keynesians and other heterodox economists.[10] Critical Realists are opposed to all forms of positivism, including the highly influential instrumentalist variant advocated by Milton Friedman (1953), and also to the post-modernist and post-structuralist methodologies that are favoured by many feminists (Hewitson 1999). They make six important claims about the nature of science.[11] First, the objects of enquiry exist independently of their investigation and are separable from the enquirer. Second, relations of cause and effect are involved; merely establishing relations of 'mutual dependence' is not sufficient. Third, priority must be given to explaining observed events or data; prediction may or may not be possible but it is never primary, or sufficient. Fourth, assumptions of closure are partial and provisional; open-system thinking is strongly encouraged.[12] Fifth, Critical Realists assert the principle of historical specificity: economic theories apply to particular social, geographic and historical contexts, and need to change when the context changes. Sixth, and closely related to this, social institutions are not exogenous to human agency, and cannot safely be taken as 'given' for the purposes of social (or economic) analysis. The diverse nature of economic research may mean that not all these criteria are relevant to all research projects. For example, some research programmes relevant to macroeconomic events may be able to proceed without emphasising specific assumptions about human agency. On the other hand, human agency may be a key component of interest in research about the operation of particular markets. The relevance of each criterion to a particular research project depends on the type of analysis being undertaken.

In the Kaldor memorial issue of the *Cambridge Journal of Economics* Tony Lawson (1989) argued persuasively that he should be seen as having an approach to economic theorising that is broadly consistent with Critical Realism. Kaldor himself seems never to have taken a position on this issue, at least in print, but Lawson has a strong case. Kaldor's approach to economic theorising and empirical research fits rather well within a Critical Realist framework (Jefferson and King 2009). The structure of much of his work was characterised by identifying particular economic events and then developing theoretical approaches that contribute to understanding the possible causes and explanations of these events. While he never invoked arguments about closed and open systems, his approach did suggest that economic explanations applied to specific types of social institutions, which changed through time,

and therefore also applied to specific historical and geographic contexts. Thus Kaldor's methodological position both inspired and reinforced his post-1972 critique of mainstream equilibrium economics. But what alternative did he have to offer?

8.4 Economics without equilibrium

Kaldor had described his project in the introduction to the second volume of his *Collected Essays*:

> The concern with growth problems, in turn, led to a re-appraisal of the whole structure of economic theory – extending into fields which the 'Keynesian revolution' itself left untouched – and to a new economics of non-stationary states, which is no longer an extension, but a replacement, of static theory, and which is still in the process of formulation. (Kaldor 1960b, p. 2)

His most systematic attempt to develop this 'new economics' came almost a quarter of a century later, in the Okun lectures that he delivered at Yale University in October 1983.[13] Arthur M. Okun (1928–1980) was a well-liked liberal Democrat who had served as Chairman of the Council of Economic Advisers under President Johnson and whose increasing dissatisfaction with contemporary economic theory had been expressed in his *magnum opus*, *Prices and Quantities* (Okun 1981). Kaldor shared his concerns, and in the memorial lectures often expressed his own ideas in the language used by Okun.

Once again he began with a methodological critique. Like Okun, Kaldor insisted on the need to establish the 'stylised facts' of the situation before constructing a theory to explain them. They had to be stylised, 'because in the social sciences, unlike the natural sciences, it is impossible to establish facts that are precise and at the same time suggestive and intriguing in their implications, and that admit to no exception' (Kaldor 1985a, pp. 8–9). This had important consequences for the research methods that economists should use:

> In other words, contrary to the prevailing trend, one should subordinate deduction to induction, and discover the empirical regularities first, whether through a study of statistics or through special inquiries that include 'informal conversations with the owners or executives of small businesses'[14] (and I presume, the executives of large businesses as well). One should also seek the most reasonable

> explanation capable of accounting for these 'facts', independently of whether they fit into the general framework of received theory or not. (*ibid*., p. 8)

This was a clear bow in the direction of Alfred Marshall, who had spent a lifetime engaged in 'special inquiries' of this sort, relying heavily on 'informal conversations' with businessmen and inspiring a mass of similar qualitative research by loyal disciples like Philip Andrews (1949). As a research method it had met with increasing scorn from equilibrium economists. The Chicago price theorist George Stigler, for example, is said to have defined a political scientist as 'someone who thinks the plural of "anecdote" is "data"'.

Kaldor, by contrast, had become suspicious both of 'hard facts' and of the closed-system thinking that they encouraged. When speaking of 'stylised facts', he insisted,

> we do not imply that any of these 'facts' are invariably true in every conceivable instance but that 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. Such hypotheses relate to particular *aspects* of the economy and they may be suggestive of others. They may be discarded if they prove inconsistent with other observed features and then be replaced by something else'. (Kaldor 1985a, p. 9)

He acknowledged that 'this kind of inductive-deductive theorizing may appear pedestrian'. But it was more likely to lead to an understanding of economic reality than 'the all-embracing principles of the 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' (*ibid*., p. 9). For this reason Kaldor shared Marshall's suspicion of mathematics as a tool of economic reasoning and his resistance to mechanical analogies. 'Marshall realized that human societies are subject to continuous evolution, the precise direction of which can never be predicted; and he frequently emphasized that economics has far more in common with biology than with mechanics' (*ibid*., p. 59). There were, again, lessons to be learned from this concerning the most fruitful research methods. Economists should 'make greater use of knowledge gained through personal contact and on-the-job investigations, and less on the testing of formal models through statistics and econometrics' (*ibid*., p. 54).

In microeconomics, Kaldor suggested, some significant stylised facts had already been established. The first and most important was that

real-world markets were never 'perfect' in the sense required by Walrasian theory. This was true even of the markets for primary commodities, 'which come nearer to the "auction markets" of general equilibrium theory than all the other "markets" in the economy. Yet they fail to satisfy the theoretical requirements from more than one point of view'. Inventories were invariably held by insiders, and changed from period to period, so that the market never cleared, 'even in the shortest of periods'. Such behaviour was 'quite un-Walrasian' (*ibid*., p. 18). The second crucial fact was that, 'even in the most organized markets, business dealings are far from anonymous'. Here again the influence of Marshall was apparent.[15] Goodwill was important in all business transactions, Kaldor argued: 'it is part of human nature for buyers to have customary suppliers'. There was nothing irrational in this, once it was no longer assumed that only information about prices was of any value to potential purchasers. In fact 'a decision to buy is influenced, even in the simplest of cases, by a complex set of information (other than that relating to price) for which the buyer is mainly dependent on the knowledge, acumen, honesty, and reliability of the dealer' (*ibid*., p. 19). Third, adjustments to changes in demand often took the form of variations in quantities rather than in prices. Here Kaldor cited work by Kornai (1980), who had stressed the importance of changes in stocks as a signal to producers that they should increase or decrease their production levels. Changes in order books, Kaldor suggested, could play a similar role (Kaldor 1985a, pp. 32–3). These three stylised facts were added to the long-standing Kaldorian themes of imperfect knowledge, increasing returns to scale (*ibid*., p. 63), mark-up pricing, with the profit margin loosely related to the degree of expected competition (*ibid*., p. 52), and the central role of fairness in wage determination (*ibid*., p. 39), to constitute the elements, at least, of a non-equilibrium approach to microeconomics.[16]

The rudiments of a non-equilibrium macroeconomics can also be found in the Okun lectures, if perhaps less clearly expressed than in the Mattioli lectures. Again Kaldor emphasised the distinction between resource-constrained and demand-constrained economies. Even in conditions of full employment the demand constraint remained, as was demonstrated by another stylised fact, the continuing existence of hidden or disguised unemployment (*ibid*., p. 35). Thus the principle of effective demand continued to apply (*ibid*., p. 33). To this extent, at least, Kaldor's approach was Keynesian in nature. The other stylised facts that formed the core of his non-equilibrium macroeconomics were less obviously compatible with the *General Theory*. These were increasing

returns, path-dependency, endogenous technical progress and historical specificity.

He found new support for increasing returns in Okun's Law, which stated that labour productivity tends to be pro-cyclical, with output rising faster than employment in a boom and falling faster than employment in a recession. This was a short-run phenomenon. Its long-run counterpart, Kaldor suggested, was Verdoorn's Law (Kaldor 1985a, p. 45). Both reflected the importance of Myrdal's principle of circular and cumulative causation, which in turn underpinned the Kaldorian principle of export-led growth. 'Success breeds success; regions or "countries" whose industrial exports increase faster than world net exports have a faster rate of economic growth; this tends to depress the rate of growth of the regions whose share of world trade is diminishing in consequence' (Kaldor 1979a, p. 290).[17] This point was central to development economics:

> Industrialization is the key factor in economic development. All rich countries with high incomes per capita are industrialized countries. Myrdal's principle explains why rapid growth tends to be concentrated among a relatively small number of 'successful' areas, and also, why, within that fortunate group of areas, the relative wealth and standard of living are subject to continuous change – poorer areas with lower efficiency wages overtake areas which were initially richer, but, owing to high wages in relation to their productivity, are unable to stand up to the competition of others. Both the growing polarization of the world between developed and underdeveloped, or rich and poor countries, and the remarkable shifts in the relative positions of individual 'rich' countries are, in my view, to be explained by the same basic principle. (*ibid.*, pp. 290–1)

The third stylised fact was path-dependency. This was a theme that Kaldor had first explored in the early 1930s,[18] and it was central to his mature thinking on how to construct a non-equilibrium macroeconomics:

> we must begin by constructing a different kind of abstract model, one that recognizes from the beginning that time is a continuing and irreversible process; that it is impossible to assume the constancy of anything *over* time, such as the supply of labor or capital, the psychological preferences for commodities, or technical knowledge. All these things are in a continuous process of change but the forces that

> make for change are endogenous not exogenous to the system. The only truly exogenous factor is *whatever exists at a given moment of time*, as a heritage of the past. (Kaldor 1985a, p. 61; original stress)

Thus path-dependency entailed endogenous technical progress, and this was inconsistent with the Harrodian notion of a 'natural' rate of growth, determined by the supposedly exogenous growth rates of the labour force and of technical change (Kaldor 1996, p. 36). This, in turn, implied that macroeconomic theory could not be timeless, derived from a set of universal axioms about rational human behaviour, but must instead be historically specific (*ibid.*, pp. 4, 41–2; cf. Hodgson 2001). It followed, Kaldor maintained, that economists had to be modest about their predictive abilities:

> The heritage of the past is the one truly exogenous factor, and its influence will determine future events to an extent that varies *inversely* with the distance of the future period from the present. Thus our ability to predict what *can* happen or what is likely to happen becomes progressively less as we consider the more distant future as against the nearer future. (Kaldor 1985a, p. 62; original stress)

One final element of Kaldor's alternative macroeconomics remains to be discussed. This is his attack on the orthodox theory of international trade and his consequent critique of the presumption that protection was always welfare-reducing:

> Under the benign rule of constant returns to scale, competition and free trade would benefit all participants, leading to a general equalisation of returns even in the absence of free mobility,[19] that is to say, to a convergence of living standards and growth rates. But in reality the existence of increasing returns to scale makes the picture far more complicated. Free trade tends to enlarge differences in comparative costs instead of reducing them, and contrary to Mill's famous principle, trade need not be advantageous to all trading partners – it may be ruinous to some, to the greater benefit of others. Instead of a convergence, it may lead to a divergence – to an increasing gap between prosperous and depressed areas. (Kaldor 1978c, p. xxiii)

As we saw in Chapter 6, Kaldor always favoured intelligent (that is, export-promoting) rather than indiscriminate protection (Kaldor 1996,

p. 66), but in a profession where free trade has long been an article of faith Kaldor's heresy was profound and shocking.

Taken together, these ideas do not add up to a comprehensive and coherent alternative to equilibrium economics. Indeed, Kaldor never aspired to anything of the sort. In the introduction to the sixth volume of his *Collected Papers* he apologised for the 'overlapping and repetition of theorems and propositions' that the volume contained:

> I could have avoided that only if I had devoted my energies to the writing of a treatise in which my ideas were put together in a systematic and comprehensive way within a single conceptual framework in the manner of the great economists of the nineteenth century. I have not done this because 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 1978a, p. xxix)

There was to be no definitive treatise, then. But Kaldor had supplied a large set of rich and provocative ideas, positive as well as negative, that could be used in the construction of an alternative to equilibrium economics.

8.5 Kaldor and the Post Keynesians

Kaldor was not, of course, the only theorist engaged in this project. There were several dissident schools of thought that offered alternatives to the neoclassical paradigm. Among them were the Austrians (Vaughn 1994), who were totally unacceptable to Kaldor on political grounds, opposing as they did the mildest social democratic reforms as dangerous steps down the slippery slope to serfdom. Added to this was Kaldor's personal hostility towards Hayek,[20] which was reinforced when his former boss reinvented himself in the 1970s first as a fervent monetarist and then as Margaret Thatcher's personal guru. In some ways this antagonism was unfortunate, for Kaldor did share common ground with the Austrians on some important questions, including their opposition to general equilibrium analysis, insistence on human creativity and interest in market processes rather than market outcomes. Politics apart, however, his theoretical differences from the Austrians were simply too great, especially with respect to Keynes and to macroeconomics more generally.[21]

He was no closer to the Marxists. Kaldor's long-standing resistance to Marxian economics has already been noted,[22] and it continued into his

final years, despite one or two favourable references to Marx in the 1980s. Again this was in some ways a pity, since on issues like the nature and significance of technical progress in modern capitalism and the structural weaknesses of the British economy they were not very far apart. In the 1970s and 1980s Marxian political economy was extremely fragmented, not to say intolerantly sectarian, especially in Britain. The label was appropriated by individuals and groups with a very wide variety of positions, ranging from dogmatic Trotskyists (themselves deeply and often bitterly divided) to ecumenical Eurocommunists. They came together, uneasily, in the Conference of Socialist Economists (Lee 2001), an organisation that held absolutely no appeal for Kaldor. He also ignored the Alternative Economic Strategy that was propagated in the early 1980s by the more reform-minded members of the CSE, even though it was a social democratic programme in everything but name (Aaronovitch 1981). In Cambridge the most prominent Marxian economist was Robert Rowthorn ('Red Bob', as he was known in 1975, when Kaldor crossed swords with him on the question of Verdoorn's Law).[23] There was never any question of collaboration between them.

Neither did the institutionalists or their intellectual cousins, the evolutionary (or neo-Schumpeterian) economists, arouse his interest. There was, once more, a long list of issues on which they fundamentally agreed, including the principle of historical specificity, the endogeneity of consumer preferences and technical change, and the importance of Joseph Schumpeter as a theorist of economic development. I suspect that Kaldor never read anything by Thorstein Veblen, John R. Commons or Clarence B. Ayres, the formative influences on institutional economics in the United States (Rutherford 1994). Despite a friendship that dated from 1945, there was no sign either that he had any great respect for John Kenneth Galbraith's ideas on countervailing power, the implications of mass affluence, the 'technostructure', the 'planning system' or the new industrial state (Stanfield 1996). As for the neo-Schumpeterians, they had barely emerged as an organised grouping by the time of Kaldor's death.

This leaves Post Keynesianism, itself a fractious and divided school of thought. A little background on the emergence of Post Keynesian economics might be helpful at this point. After 1936 economists were rapidly polarised, and soon the gulf between Keynesians and anti-Keynesians was not only very wide but also came to dominate the discipline. Kaldor was unequivocally in the former camp, but his admiration for Keynes was never uncritical, as we saw in the previous chapter. The *General Theory* itself was in any case a long and very

complex book, open to different and sometimes conflicting interpretations that sometimes reflected Keynes's own ambivalence on important theoretical questions, not to mention his inability to make a clean break with the analytical techniques and methodological presuppositions of his predecessors. In retrospect – though it was not clearly recognised at the time – there were two very different and very largely incompatible versions of 'Keynesian' economics that could legitimately be derived from the *General Theory*. One was the neoclassical-Keynesian synthesis that originated with J. R. Hicks's IS-LM model, and the other was the Fundamentalist Keynesian interpretation of George Shackle and Paul Davidson that focused on fundamental uncertainty and the unique characteristics of money. Added to this was the pervasive influence of the Polish émigré Michal Kalecki, who added a Marxian tinge to the new macroeconomic canvas. Thus there were by the outbreak of war three distinct strands of 'Keynesian' theory in the making (King 2002, chapters 1–2).

None of this, to repeat, was fully evident at the time. When he returned to academic life late in 1949 Kaldor would almost certainly, had he been asked, have described himself as a mainstream Keynesian. Ten years later this would no longer have been possible, since a new and quite unbridgeable chasm had opened up between the Cambridge (UK) and Cambridge (US), whose leading theorists took radically different positions on questions of capital, growth and distribution theory. Kaldor was deeply involved in these controversies, with his bold repudiation of the aggregate production function and the marginal productivity theory of distribution. Slowly there emerged in Cambridge (UK) a more or less coherent alternative to the neoclassical synthesis, originally described (especially by Joan Robinson) as the 'Anglo-Italian school' but eventually taking on the sobriquet of 'Post Keynesian economics'.[24] Kaldor tended to avoid labels wherever possible, but he was clearly an important member of the Post Keynesian tendency, even though he had by the early 1960s fallen out with its other two most prominent advocates in Cambridge, Robinson and Richard Kahn (King 1998b). Indeed, he was never a loyal party man when it came to matters of economic theory, and some of the positions that he took were decidedly anti-(Post) Keynesian: the full employment assumption in the phase I growth models of the 1950s and early 1960s, the imperfect competition requirement in the 1980s and (arguably) the export-led growth analysis from the late 1960s onwards, which made business investment expenditures entirely endogenous and raised the spectre of a 'supply-side Keynesianism' that was hard to reconcile with

the principle of effective demand. Even as a Post Keynesian, then, Kaldor was something of a maverick.

In the first major survey of Post Keynesian economics, published two years after Kaldor's death, Omar Hamouda and Geoff Harcourt (1988) distinguished three sub-groupings, the Fundamentalist Keynesians, the Kaleckians and the neo-Ricardians. As they noted, there were many individuals who belonged in none of these camps; Nicholas Kaldor was singled out as constituting a one-man school of his own. He was not inclined, either temperamentally or doctrinally, to be a Fundamentalist Keynesian. Despite his immense respect for Keynes, and the huge significance of the *General Theory* in the early development of his own ideas, Kaldor never believed that the man or the book had solved all the important problems in macroeconomics. Towards the end of his life, as we saw in the previous chapter, he stressed more and more what he termed 'the limitations of the *General Theory*'. These included Keynes's failure to drop the assumption of an exogenous money supply; his closed economy analysis, which was more tractable but much less interesting than focusing on an open economy; and his refusal to accept that imperfect competition was a necessary condition for the principle of effective demand.[25] He was, however, on very good terms with the two most prominent Fundamentalist Keynesians in the United States, Sidney Weintraub and Paul Davidson (King 2008). In 1977 Kaldor was one of the luminaries appointed by Weintraub and Davidson to the Academic Board of the newly established *Journal of Post Keynesian Economics*, which stood above the Editorial Board and was essentially ceremonial. It was a notable tribute. The *Journal* later published a celebratory article on Kaldor by Luigi Pasinetti (1983) and a laudatory (if also critical) symposium in its Spring 1983 issue on his 'growth laws'.[26] In the twenty-first century Davidson became much less tolerant of other strands of thought within Post Keynesianism, especially Joan Robinson and the Kaleckians, and regarding the Sraffians as wholly outside the pale. Davidson still thought that Kaldor had been on the side of the angels (Davidson 2003–2004). But he was never a true Fundamentalist Keynesian.

Neither was Kaldor a Kaleckian. His acquaintance with the work of the Polish theorist began as early as 1933, when he was in the audience at the inaugural conference of the Econometric Society in Leyden, in the Netherlands, when Kalecki presented the first version of his celebrated proto-Keynesian trade cycle model (Kaldor 1986b, pp. 62–5). There was a family resemblance between this model and the much less sophisticated one that Kaldor published in 1940, which included

substantial critical comments on Kalecki (Kaldor 1940c, pp. 89–92). With this exception, however, the relatively few references to Kalecki in Kaldor's early writings were rather cool. Later in life he took a more favourable approach, including Kalecki in the select list of younger colleagues from whom he had learned during his LSE years (Kaldor 1960a, p. 15)[27] and – admittedly only in a footnote – comparing Kalecki's theoretical system favourably with that of Keynes (Kaldor 1980a, p. xv n4). On the question of income distribution there were both similarities and an important difference. Kalecki's macroeconomic theory of profits foreshadows Kaldor's 1956 model, since in aggregate it is capitalist expenditure that determines capitalist incomes: workers spend what they get, while capitalists get what they spend (Kalecki 1942). But this was a theory of the *level* of profits. Kalecki explained the *share* of profits in terms of the average degree of monopoly, which Kaldor always dismissed as a pure tautology (Kaldor 1956a, p. 93).[28] 'Despite his criticisms of Kalecki's theories of distribution and the business cycle', Ferdinando Targetti concludes, 'Kaldor always admired the Polish economist (though not to the same extent as Joan Robinson), and he wrote his obituary in *The Times* (21 April 1970) (Targetti 1992, p. 108 n8). There was a lasting ambivalence, however, in his attitude. Just before he died Kaldor turned down the opportunity of reviewing Malcolm Sawyer's intellectual biography of Kalecki (Sawyer 1985) for *Oxford Economic Papers*, apparently because he did not want to criticise Kalecki in print. In private correspondence he repeated the same objections to Kaleckian models of cycles and income distribution.[29]

The third Post Keynesian stream identified by Hamouda and Harcourt were the neo-Ricardians, sometimes also described as 'Sraffians' but preferring the title 'classical economists' or 'surplus theorists'. Their inspiration was Piero Sraffa's slim volume, *Production of Commodities by Means of Commodities* (Sraffa 1960), which sought to rehabilitate classical economics as a 'circular' theory of production with a physical surplus of outputs over inputs as the starting point (Kurz and Salvadori 1995). Sraffa's masterpiece was an elegant piece of deductive reasoning, almost unbelievably terse, with devastating implications for the neoclassical approach to price and distribution theory but with almost nothing to say about the real-world questions of economic policy that Kaldor was committed to answering. Sraffa was Kaldor's best friend in Cambridge, at least after his break with Joan Robinson and Richard Kahn in the late 1950s, but it cannot be said that he was ever in any sense a Sraffian. He paid tribute to the enigmatic Italian in the long obituary that he wrote for the *Proceedings of the British Academy* (Kaldor 1985b), but although

he claimed that *Production of Commodities* grew on him over time, there is no evidence that the book influenced his own work in any direct or obvious way. Kaldor seems never in fact to have been attracted by the prospect of a revival of classical economics. John von Neumann was another good friend, at least in the 1930s, but von Neumann's own 'classical' growth model seems also to have had no influence on Kaldor's intellectual development, even though he was responsible for having it translated and published in the *Review of Economic Studies*.[30]

One final point, this time a contentious one, concerns Kaldor's relationship with the 'New Keynesians', a school that emerged in the early to mid-1980s in reaction to the success of monetarism and New Classical economics in the United States (Blinder 1988). What little Kaldor saw of New Keynesianism, he liked, in particular its insistence on imperfect competition as a necessary part of the microfoundations of Keynesian macroeconomics (see Chapter 7, Section 5). I doubt, though, whether it would have been 'Keynesian' enough for him. He would not have been greatly impressed by the theoretical consensus that had been reached a decade after his death, with its quasi-Walrasian multi-period equilibrium microfoundations, IS curve (something that Kaldor never had much time for) and downward-sloping short-run Phillips Curve (to which the same remark applies). He would, however, have appreciated the abandonment of the LM curve and (perhaps) its replacement by a Taylor rule for monetary policy that clearly recognised the endogeneity of money and hinted at a horizontal money supply curve.[31] Kaldor would, however, have insisted on employment targeting, rather than inflation targeting, as the goal for monetary policy, and he would have objected strongly to the New Keynesian abandonment of incomes policy as the chief instrument for controlling domestic inflation. He would also have criticised the exclusively supply-side nature of New Keynesian growth theory, and denied that demand constraints were irrelevant in the long run. He might well have reacted more favourably to the 'comparative political economy' strand of New Keynesian thinking, which stressed the importance of social institutions, wage-fixing arrangements and industrial relations systems in explaining differences in macroeconomic performance, over time and between countries (Carlin and Soskice 2006).[32]

On balance, the judgement of Hamouda and Harcourt concerning Kaldor's relationship with the Post Keynesians seems about right. As Mark Blaug put it, at almost the same time, 'his is essentially a one-man research programme (Blaug 1989, p. 92). Kaldor, then, was too much of an individualist to have been part of any school (a bit like George

Shackle in this respect), even though many of his ideas were Post Keynesian in spirit and many of his arguments continue to resonate in Post Keynesian circles. That said, he clearly *was* outside the mainstream, decisively so after he launched his attack on Milton Friedman in 1970, and he himself was of course aware – and defiantly proud – of the fact. Thus Kaldor was, and to some extent remains, an inspiration not only to Post Keynesians but also to other dissident streams of thought in economics.

His first biographer, A. P. Thirlwall, specified six criteria for membership of a broad but minimally coherent Keynesian church.[33] The 'six central messages of Keynes's vision', he suggested, were the propositions that output and employment are determined in the product market, not the labour market; involuntary unemployment exists; an increase in savings does not generate an equivalent increase in investment; a monetary economy is fundamentally different from a barter economy; the Quantity Theory holds only under full employment, with a constant velocity of circulation, while cost-push forces cause inflation well before this point is reached; and capitalist economies are driven by the animal spirits of entrepreneurs, which determine the decision to invest (Thirlwall 1993, pp. 335–7). Kaldor might have had problems with the sixth proposition, since he had convinced himself that investment was endogenous and exports were the only truly exogenous source of demand, but he would have regarded the other five propositions as a reasonable minimum platform.

8.6 From insider to outsider

By the time of his death the five propositions would have been denied, explicitly or implicitly, by the great majority of academic economists (including many who professed themselves to be 'New Keynesians'). Kaldor's attack on equilibrium reasoning struck at the core of mainstream economic theory. In any other discipline, at any other time, it would have been an unusual act of aggression from someone in his mid-sixties who had already reached the commanding heights of his profession and was in a very real sense an Establishment figure. As suggested at the beginning of this chapter, the profession itself was in crisis in the early 1970s. In fact it is hard to think of any other academic discipline where so many distinguished senior practitioners have turned on their colleagues and denounced both their methods and the substance of their research. Like the other critics mentioned in the previous section, Kaldor was no outsider. From the mid-1930s he had been an

important part of the emerging Keynesian orthodoxy, publishing in (almost) all the best journals,[34] from the *Economic Journal* to the *Quarterly Journal of Economics*. He was a member of a Royal Commission in the 1950s, elected Fellow of the British Academy in 1963, appointed to a personal Chair at Cambridge three years later, chosen to head section F of the British Association in 1970, and asked to serve three Labour governments as an official adviser. Even after he had denounced the irrelevance of equilibrium economics Kaldor was still elected President of the Royal Economic Society for the calendar year 1974, two years before his elevation to the peerage. Almost the only honour to which he might reasonably have aspired that did not come his way was the Nobel Prize (on which, see Chapter 9, Section 2).

These, however, were rewards for services rendered in the past – many of them in the distant past. Although the specifically Walrasian version of economic analysis did prove to be unworkable and was soon abandoned, this was on account of internal problems and did not entail any concessions to Kaldor's external critique.[35]

Neither the method of axiomatic reasoning in terms of mathematics, nor the equilibrium theorising based upon it, succumbed to the attacks of Kaldor and the other critics. On the contrary, the mainstream consolidated itself, replaced the assumption of perfect competition with various forms of imperfect competition (including oligopoly), applied game-theoretic models of increasing mathematical sophistication, and became increasingly intolerant of any departure from the canons of orthodoxy. This was especially evident on questions of economic policy, where by the time of Kaldor's death neoliberal principles were triumphant (Backhouse 2005). In 1986 the increasing irrelevance of non-equilibrium economics was painfully obvious even in Cambridge, where the (Post) Keynesian generation led by Kahn, Kaldor and Robinson had very largely failed to reproduce itself.

9
Kaldor in His Time – and Ours

9.1 What the others said

With the exception of *An Expenditure Tax*, all Kaldor's books were collections of essays, speeches and reports, most of them previously published.[1] The earlier volumes, at least, were widely reviewed, by some of the most prominent theorists of the day. Some of the plaudits for *An Expenditure Tax* from authorities like Arnold Harberger and Richard Musgrave were reported in Chapter 5. Another reviewer concluded that, '[i]n the true Marshallian tradition, even the footnotes are immensely interesting' (Break 1956, p. 177). This did not prevent the reviewers from being critical – in some cases, sharply critical – of the substance of the book. Musgrave objected that the distinction between consumption and saving was less clear-cut than Kaldor supposed: 'Why are outlays for schooling and health to be considered "spending", while those for housing or rare pictures are considered "investment"?' (Musgrave 1957, p. 202). Although Musgrave did not say so, this highlighted a more general problem with Kaldor's economics: a complete lack of interest in human capital, which might (and probably should) have played an important part in his analysis of economic growth. Musgrave also raised doubts about the administrative difficulties associated with the taxation of expenditure, a criticism developed at some length in the *Economic Record* by the Australian tax official D. Steele Craik (1957).[2]

The first two volumes of Kaldor's Collected Essays were also widely, and favourably, reviewed. In the *Economic Journal*, Richard Lipsey wrote that

> Reading Mr. Kaldor's collected works, one is left with the impression of an economist in the grand manner; one who ranges over the entire

> field of economics, writing seminal articles on welfare, value theory, cycles and growth; one who, although a theorist, seldom loses sight of the real world; and one who takes pleasure at the challenge thrown up when a pet theory is upset by the discovery of hitherto unsuspected facts. (Lipsey 1962, p. 687)

For *Economica*, E. J. Mishan (1962) wrote an equally sympathetic appraisal of Kaldor's growth and distribution theories, while William Baumol's four-page review in the *American Economic Review* opened thus: 'No-one can fail to come away impressed from a perusal of these two volumes, or even just their tables of contents. For they remind us at once of the very broad range of subjects to which Mr. Kaldor has made significant contributions, and the number of his papers which have become classics' (Baumol 1961, p. 409). Most remarkable was Robert Dorfman's praise for Kaldor in the *Journal of Political Economy*: 'since 1934 he has displayed one of the sharpest minds, one of the most skilful techniques, and one of the most fertile imaginations in the profession' (Dorfman 1961, p. 495). It is revealing that Dorfman, himself a mathematical economist of some distinction, and co-discoverer of linear programming,[3] should praise Kaldor's *technique*. Evidently in the 1950s this term had not yet become synonymous with 'mathematical ability'. After some substantial criticism of Kaldor's growth and distribution theories, Dorfman's review concluded with the judgement that '[t]hese volumes recapitulate the first half of a career of continuing fruitfulness; they and their contents are a handsome gift to the profession' (*ibid*., p. 497). These statements, to repeat, came from mainstream economists at the peak of the profession.

Once again, praise was mixed with some often quite fierce criticism. Thus Dorfman found it odd that 'Kaldor, who emphasizes his debt to Keynes, should have attempted to cope with macroeconomic problems without subjecting his thought to the discipline of a coherent, consistent macroeconomic model' (Dorfman 1961, p. 496). Where Kaldor had set out formal models, in his analyses of economic growth from 1954 onwards, they had not been convincing:

> These models are very clever and elegant, yet we cannot accept that a three-equation model can describe adequately an economy in general when we know that Klein and Goldberger's laboriously constructed model of twenty-odd equations did none too well for a specific economy. Of course, Kaldor can maintain that he has the *right* equations. This may be so, but until the necessary empirical

> work has been done Kaldor cannot expect his models to be greeted as a substantial insight into the real world. (*ibid.*, pp. 495–6)[4]

Baumol, too, objected to the 'rather obviously extreme oversimplification of the model as a theory of the complex growth phenomenon', which, he concluded, 'seems to me to rob it both of explanatory power and of usefulness to the policy-maker'. He rejected Kaldor's 'astonishing conclusion that, despite technological change, producers will *in perpetuo* seek to maintain the same capital-output ratio' (Baumol 1961, p. 411). Baumol was also unimpressed by Kaldor's 'proffered explanation of the constancy of the share of wages ... since it requires both a constant ratio of investment to output (even the acceleration principle does not predict this) and historically constant marginal propensities to save out of profits and wages' (*ibid.*, p. 411 n2). In both cases, Baumol suggested, Kaldor's analysis 'assumes what it sets out to explain!' (*ibid.*, p. 411). Precisely the same criticism was made by another reviewer, Daniel Hamberg, writing in the *Southern Economic Journal*: 'Throughout he ends up "proving" characteristics he virtually assumes at the outset'.[5] Neither was Kaldor's technical progress function a satisfactory replacement for the aggregate production function: 'Virtually all the problems laid at the feet of the traditional concept arise to plague Kaldor's', he concluded (Hamberg 1962, p. 308).

The same mixed verdict was typical of the reviews of the third and fourth volumes of the *Collected Essays*. In the *American Economic Review*, Arthur Smithies, while highly critical of some of Kaldor's theoretical opinions, declared himself to be on balance very favourably impressed: 'Kaldor observes the economic scene with the cold detachment of a Ricardo.... Like Ricardo also, great intellectual power is exhibited with an absence of wit, a solemnity of purpose, and hasty drafting. Spectacular conclusions emerge from the "stylising" (to use Kaldor's word) of facts and premises' (Smithies 1966, p. 881). This was a somewhat backhanded compliment, and – at least on the matter of Kaldor's wit – less than fair. I suspect, though, that he would have been pleased by the comparison with Ricardo. The *Economic Journal* had already published Roy Harrod's major (nine-page) review, which offered a detailed critique of Kaldor on money, Keynes, growth theory and development economics. Although by no means uncritical, Harrod was also very complimentary, not least when appraising Kaldor's literary talents: 'Mr Kaldor's volumes belong to the great tradition of the best works on political economy published in this country in showing a firm mastery of *English* prose. The style is fluent and usually pellucidly clear. It often

has a vigour and verve which keep the reader refreshed' (Harrod 1965, p. 794).

At the same time, Harrod complained that 'Mr. Kaldor is somewhat too headstrong when dealing with fundamental expositions', (*ibid.*, p. 800), and criticised his over-confidence:

> But when he is working at the level of most fundamental theory and offering original propositions his supremely self-confident stance may be a handicap. He does not allow enough for the 'yes, but; wait a minute' that may run from time to time through the reader's mind'. (*ibid.*, p. 794)

Like Keynes, Harrod concluded, Kaldor was too much of an iconoclast:[6]

> Mr. Kaldor also has a *fault* in common with Keynes: he is too ruthless in scrapping theories that have been endorsed by many fine minds. Keynes needlessly jeopardised the general acceptance of his own contributions by not making sufficient efforts to see how they could be fitted in, rather than the other way round. Similarly, Mr. Kaldor's rejection, stated more than once, of the idea that factor prices are equal to their marginal products is too sweeping. (*ibid.*, pp. 799–800)

On this issue Baumol was much less critical of Kaldor, on the grounds that the marginal productivity principle offered a theory only of the individual firm's employment decisions, and not of the distribution of income between labour and capital as a whole; thus there was indeed a gap to be filled (Baumol 1961, pp. 411–12).

These extensive commentaries all date from the period 1957 to 1965. Later, as books became less important, relative to journal articles, and leading economists lost interest in reading them, the reviews tended to dry up. This was a reflection both of changes in the propagation of economic ideas and also of the decreased respectability of Kaldor in the global economics profession. Thus there were fewer reviews of the final four volumes of his *Collected Essays*, and few indeed of the 1980 reissue of volumes 1 and 2. Michael Artis, though critical of Kaldor's change of heart on the effectiveness of currency depreciation, concluded that volumes five and six 'show one of the most restive and creative minds in economics at work; they are an invaluable record of endeavour, candidly surveyed and lucidly introduced by the author himself' (Artis 1980, p. 395). The American Post Keynesian Hyman Minsky, every bit as much an individualist as Kaldor himself, praised him for his emphasis

on the role of investment as 'the central determinant of system behavior', but identified a major weakness in his neglect of money and finance in his formal growth and business cycle models (Minsky 1981, p. 1577). As noted in Chapter 5, Carl Shoup (1981) took a rather detached view of volume VII (*Reports on Taxation I*), while John Kay (1981), reviewing volumes VII and VIII, was greatly impressed by the 1955 Minority Report. Kaldor would have been pleased by Kay's comparison of his work on the Royal Commission with that of Keynes and the Webbs. Taken as a whole, Kay concluded, the two volumes provided 'an impressive illustration of how economic analysis can be brought to bear on practical policy problems, from one of the most influential, as well as one of the most distinguished, economists of our time' (*ibid.*, p. 582). There were a couple of brief but favourable reviews of his writings on monetarism, which will be discussed below (Miles 1983; Peston 1983). Geoff Harcourt wrote what seems to have been the only published review of the Okun lectures, in which he complained of 'the shameful neglect of Kaldor's contributions by the profession' (Harcourt 1986, p. 541). I have not been able to trace a single review in any mainstream journal of the Mattioli lectures, which were, however, given a favourable reception by Post Keynesians (Harcourt 1997; Skott 1999).

There was also a certain amount of friendly fire, from critics who were in broad sympathy with Kaldor's way of thinking. As early as 1953, well before the term 'Post Keynesian' had taken on its present meaning, Geoff Harcourt published a substantial critique of the Mark I and Mark II growth models, objecting in particular to the full employment assumption that Kaldor had felt impelled to make. Briefly a PhD student of Kaldor's,[7] Harcourt acknowledged his 'detailed comments' on one section of the paper (Harcourt 1963, p. 83 n), but no formal response was ever forthcoming. Neither did Kaldor reply to another friendly critic, Kurt Rothschild, who objected to the limited number of variables that Kaldor considered, the simplicity of their functional relationships and the neglect of historical, sociological and institutional factors. Like Harcourt, he also rejected Kaldor's assumption of full employment (Rothschild 1959). I have outlined elsewhere the increasingly acrimonious discussions that Kaldor had with Joan Robinson after 1956; by the mid-1960s they could no longer be described as friends (King 1988b). In 1973 the American Jan Kregel, then a good friend and close intellectual ally of Robinson, summarised the differences between her approach to the theory of economic growth and that of Kaldor. 'For Kaldor stability is a natural property of long-period analysis', Kregel noted, while 'for Professor Robinson it is a myth' (Kregel 1973, p. 187). That led Kaldor to

his highly contentious assumptions of full employment and neutral technical progress. 'In reality Kaldor's model attains and retains steady growth more by assumption than by logical analysis' (*ibid.*, p. 192). These strictures are relevant to the Mark I and Mark II models but not to Kaldor's later thinking on growth, and it is possible that Robinson's criticisms, faithfully reflected in Kregel's summary, did finally sink home. But he never showed any great interest in the quintessentially Robinsonian themes of the determination of the rate of profit and the 'choice of technique' in terms of capital-intensity (*ibid.*, p. 190).

9.2 Strengths and weaknesses

By the end of the 1930s Kaldor had established himself at or very close to the peak of the economics profession, as we saw in Chapter 2. His 1934 paper on the concept of equilibrium was not fully appreciated at the time, not even by its author. But it contained the seeds of much of his later work on theory and method, above all the concept of path-dependence and the (closely related) rejection of equilibrium theorising. Kaldor came to realise the full significance of this for the analysis of economic growth only in the late 1960s, and then only after a long 'struggle of escape from habitual modes of thought and expression' (Keynes 1936, p. viii). The two 1939 articles were almost as important. 'Welfare propositions of economics' became a foundation stone of the 'new welfare economics', which Kaldor never repudiated, despite his break with most other aspects of neoclassical theory later in his life. 'Speculation and economic stability' remains a remarkably fertile extension and critique of Keynes's *General Theory*, albeit complex and not easily assimilated into either mainstream or heterodox thinking. The 1940 trade cycle model was also a considerable achievement for its time, though unlike the other three it has been completely overtaken by subsequent developments in the literature; business cycle theory represents an obvious exception to the Kaldorian rule that mathematics is generally useless and often dangerous in economic argument.

With the exception of Keynes, no-one made a greater contribution than Kaldor to an understanding of the economics of total war, beginning with his August 1939 paper on 'emergency finance' and ending with his 1945–1946 appraisal of the Nazi economy in wartime. As suggested in Chapter 3, Kaldor showed how the new macroeconomics could be applied to a full employment economy, thus demonstrating that it was not merely 'the economics of depression', as Hicks had claimed, and that it was not 'mere theory' but also constituted a guide both to

macroeconomic policy and to coherent thinking about that policy. Kaldor's more popular wartime writings made a significant contribution to the creation of the huge social democratic majority that resulted in the Labour landslide of 1945. His greatest wartime achievement was probably his Appendix to William Beveridge's *Full Employment in a Free Society*. Even if the econometric model presented there was extremely primitive, and Kaldor's projections for Britain's postwar economic future soon proved to be much too optimistic, he had pointed persuasively to the possibility of a genuinely new and better form of society. Compared with Hayek's reactionary jeremiad in *The Road to Serfdom*, Kaldor's cheerful egalitarianism was much more realistic and (to my mind) still reads very well today. Any serious discussion of fiscal policy, in either the short term or the long term (as for example applied to the problems posed by population ageing) would have to start where Kaldor left off, discarding the shibboleths of 'Ricardian equivalence' and 'sound finance' that he rejected. His work for the United Nations in the late 1940s on the international implications of full employment failed at the time to overcome the deflationary bias that US pressure had imposed upon the Bretton Woods institutions. However, it made the case for the 'employment approach' to global monetary policy extremely clearly, and resonates well with the criticisms of the Washington Consensus and Post Washington Consensus made by advocates of 'alternative globalisation' in the twenty-first century.

In the 1950s Kaldor was still in the mainstream of his profession, and also still at its cutting edge, engaging in animated controversy with the world's leading theorists (by this time largely American) and giving at least as good as he got on the theories of growth and distribution in many of the leading journals. The emergence of so-called 'new growth theory' in the years immediately before his death was a substantial vindication of his insistence that technical change could not be treated as exogenous and that returns to scale were increasing, not decreasing (at least in manufacturing). But the neoclassical proponents of 'endogenous growth' also made many of the serious errors that Kaldor had identified many years before, disinterring the aggregate production function and resurrecting the marginal productivity theory of distribution, both of which should have been laid to rest in 1966 when the Cambridge (US) side conceded defeat in the great capital controversies.

There were problems with Kaldor's own, post-1966 growth models. First, he always worked on the assumption that dynamic increasing returns to scale were inseparable from the special role of manufacturing. It could, however, be argued that there are two separate propositions. There may

well be increasing returns to scale in modern corporate agriculture – Kaldor never took account of the great differences between agribusiness and peasant farming – and in many business service activities that have themselves become very closely integrated with manufacturing, more narrowly defined. The latter point was well known at the time (Gershuny 1978), but Kaldor did not respond to it. A second and related criticism concerns the increasing diversity of the activities that Kaldor lumped together as 'manufacturing', which range from elementary 'screwdriver assembly' operations carried out by unskilled workers to the production of 'elaborately transformed manufactures' with a very high input of scientific and technical skill. Related to this was Kaldor's failure to take into account in his thinking on growth the 'new international division of labour', in which 'unskilled manufacturing' has largely moved to the South, leaving 'skilled manufacturing' in the North. This has had unfortunate consequences for low-skilled workers in the advanced capitalist countries (Wood 1995), but has been massively beneficial for the North as a whole. 'Who needs manufacturing?', Kaldor's mainstream critics ask. 'Leave it to the Chinese'.[8] At any rate, the word 'manufacturing' probably conceals as much as it reveals. For this reason all empirical work on Kaldor's growth laws may prove to have been mis-specified. A third, and again related, criticism is that Kaldor entirely ignored intellectual property and the income accruing from its ownership, which was important in his lifetime and has become massively more important since his death (Webster 1999; Baumol 2002). It is also true that he did not especially emphasise the role of human capital (as opposed to physical capital) in thinking about economic growth.

Fourth, and finally, there is a very important question about the direction of causation:

> the fact of a positive correlation between industries' growth of total (or net) output and of productivity is consistent with two conflicting causal interpretations – interpretations that lead to widely differing policy conclusions. One can argue, with Kaldor and others, that output growth causes productivity growth, through the attainment of scale economies, the reduction of the average age of the capital stock, etc. Or one can hold that productivity growth causes output growth, essentially by shifting the industry's supply curve downward and causing a larger quantity of its output to be sold (given some elasticity in the demand curve). Neither argument is theoretically implausible, and both could in fact be valid and empirically significant. (Caves 1968, p. 297; cf. Blaug 1989; Crafts 1991)

Some critics concluded that Kaldor had never really got to grips with this problem. All this said, however, the fact remains that dynamic increasing returns to scale are important, and do seem to be more prominent in some sectors than in others, and it does make sense for economic theorists to identify the sectors where they occur and for policymakers to encourage their growth.

Kaldor's macroeconomic theory of distribution was ridiculed by the mainstream at the time of its invention, as we saw in Chapter 4. It has a slightly old-fashioned look to it today, when social class seems to matter much less, in politics and culture, than it once did. But there *are* still capitalists and workers. Indeed, the gap between them in income and wealth has grown steadily, in most advanced capitalist countries, since the 1970s. Their savings propensities *do* differ, and macroeconomic balance *does* still require that the distribution of income between labour and capital is such that planned saving is equal to planned investment. We still live in a capitalist society, and Kaldor's strictures about the necessary conditions for positive profits continue to apply.[9] It could with some justice be objected, though, that the model is much too simple. For one thing, the classic Marxist predictions about an increasingly polarised society have proved to be false; sociologists in the Marxian tradition identify a number of 'intermediate class positions' (Wright 2000), and it is not immediately apparent how this might be incorporated into a macroeconomic distribution theory. In addition, several of his most important 'stylised facts' proved not to be facts at all, as Blaug (1989, p. 78) complained. The steady increase since the mid-1970s in the capital share in national income, and corresponding decline in the labour share, are not easy to interpret from a Kaldorian perspective. They cast real doubt on the constancy of the propensities to consume out of wages and profits, and highlight Kaldor's failure to provide any sort of theory explaining these propensities.

I carefully avoided using the word 'equilibrium' in the previous paragraph, but Kaldor's 1956 distribution paper clearly did set out a model of macroeconomic equilibrium, and therefore sits uneasily with his later insistence on the irrelevance of equilibrium economics. Similar objections can be raised to his writings on taxation, all of which seem to rest on assumptions about business responses to price changes which, if not they do not constitute explicitly equilibrium theorising, are at least arguably quite compatible with it. But this was the way that Kaldor's mind worked. He was unwilling, and perhaps also constitutionally unable, to integrate his various insights into a coherent and systematic system. His early failure to connect his ideas on money and

finance with his trade cycle theory, which I noted earlier, is just one example.[10] He tended to make a virtue out of his inconsistency, telling his Mattioli audience proudly that 'I never feel bound in my thought today by what I thought yesterday. I always like to look at problems afresh. If I am alive in five years' time, you will probably find many things that I have said today, I will not agree with' (Kaldor 1996, p. 107). I suspect that there is some justice in Joan Robinson's harsh assessment, made in private correspondence in 1952: 'The trouble with Nicky is that he never combs out his own head to make his various ideas consistent with each other. Anything he has ever thought about is left lying there and is apt to pop out however much some other idea has since made it obsolete' (King 1998b, p. 416).

To some extent this is a matter of taste. To use an artistic metaphor, Kaldor was Jackson Pollock to Robinson's Gilbert and George: wild and undisciplined, but also intensely alive.[11] As he himself put it: 'Ideas are like living organisms: while they are alive they gradually but inevitably change shape, colour or structure – there can never be any finality about them. When they become frozen and attain the status of a rigid doctrine it is a sure sign that their force is spent. Creative insight cannot survive the icy touch of established orthodoxy.' (Kaldor 1960a, p. 1). He would have said that consistency was indeed the hobgoblin of little minds, but his failure to display it must have reduced his influence in an economics profession that was increasingly giving priority to rigour over relevance. In one sense, however, his grasshopper mind was a source of strength; it allowed him to range over the whole of economics, being original, provocative and invariably interesting. Even Frank Hahn, a deeply committed formalist and severe critic of many of Kaldor's ideas, could none the less be fascinated by some of them:

> Many problems that Kaldor tackled are still unresolved and some of his penetrating insights are still very much worth having. His attempts to free himself from what he regarded as prevailing orthodoxy are often of great interest and vastly superior to similar attempts by his intellectual inferiors. (Hahn 1988, p. 1747; cf. Hahn 1989)[12]

Kaldor's willingness to take extreme positions was very well illustrated by his post-1966 emphasis on exports as the *only* exogenous source of effective demand, which I criticised in Chapter 4, Section 7. The closely related balance-of-payments-constrained growth models seem, at first glance, to be more relevant to a (long-departed) world of fixed exchange rates than to the world we live in, with floating currencies

and huge payments deficits (and surpluses) that can apparently be sustained indefinitely. Kaldor would probably have responded to this with four counter arguments. First, he would have claimed that it was true only of rich countries with currencies that were sufficiently attractive for foreigners to hold to make continuing large deficits sustainable (for example the United States, the Euro bloc, Britain and Australia). The balance of payments constraint remained binding for small, poor countries with unattractive currencies (Bolivia, Zambia, Thailand). Such countries also continued to be dependent on the IMF and the World Bank, which dictated deflationary responses to their payments deficits, including (but not confined to) devaluation. In any case, Kaldor would have insisted, the external constraint continued to operate at the regional or sub-national level: poor, relatively backward regions cannot depreciate against richer, more productive regions within their own country (this was why he had advocated the Regional Employment Premium).

Second, Kaldor would have invoked his habitual 'elasticity pessimism' to deny that (at least for countries with initially serious problems of international competitiveness like the United Kingdom) currency depreciation would have the stimulating effect on exports, and the dampening effect on imports, that mainstream theory dictated. In this he would have been vindicated by evidence concerning the 'Kaldor paradox': countries that devalued in the 1950s, 1960s and 1970s tended to lose market share in world trade, while those countries whose currencies appreciated gained in market share (McCombie and Thirlwall 1994, pp. 289–99). Third, he might well have accepted Paul Davidson's argument that a floating exchange rate regime introduced an unwelcome element of uncertainty into the world economy, discouraging investment and slowing growth across the board, so that a return to fixed exchange rates was both possible and desirable (Davidson 2006).[13]

Fourth, and decisively, Kaldor would have argued that there were *two* channels through which the balance of payments constraint operated, not one. In addition to the policy channel ('stop-go', or demand deflation in response to payments deficits), there is an automatic process through which poor export performance leads to sluggish aggregate demand and thence to low business investment and slower output and productivity growth, in the absence of any policy response whatsoever. This fourth defence hinges on the controversial proposition that, for any individual country or region, exports are the only exogenous source of demand, with investment (and consumption) being entirely endogenously determined. This is a very strong assumption, but if it is

accepted, the external constraint on growth becomes binding in all circumstances, and cannot be written off as historically or geographically specific, or confined to the Bretton Woods epoch (see also McCombie and Thirlwall 1994).

Kaldor was a central figure in both the formulation and the critique of British economic policy in the 1960s and 1970s, as outlined in Chapter 5. His career in Whitehall testifies both to the potential influence of academic economists in government and to its limitations. Some of his most cherished proposals were never implemented (the progressive expenditure tax is the most obvious example), and others were either whittled away (the capital gains tax, by 2008 reduced to a derisory flat rate of 18%) or repealed (Selective Employment Tax and its cousin, the Regional Employment Premium). Kaldor's advice on devaluation was ignored when it was most needed (in late 1964 and early 1965), and his egalitarian and potentially election-winning 1970 budget proposals were rejected, with fatal consequences for his Party and government. In 1974–1976 his Keynesian commitment to full employment was overwhelmed by the lethal combination of global stagflation, social conflict and the proto-monetarist thinking of James Callaghan and Denis Healey. Kaldor's Whitehall years cannot be counted as a success. At least he tried.

Much the same can be said of his work on development. As I suggested in Chapter 6, Kaldor's ideas on development economics were a rather unstable mixture of neo-Marxism, structuralism and neoclassical economics. The neo-Marxian component came via his (unacknowledged) adoption of Paul Baran's analysis of the economic surplus and its productive, or unproductive, uses. The structuralist elements included the critical distinction between agriculture and industry, the inelastic supply of food, and the importance of real wage resistance in the urban sector. There was also some neoclassical theorising, since Kaldor never ceased to believe in the importance of taxation, price signals and openness to the outside world, even if he would also have been a severe critic of the Panglossian view of the 'free market' that underpinned the Washington Consensus. The neo-Marxian and structuralist aspects of Kaldor's thinking were much more characteristic of the mid-twentieth century than of the twenty-first, though they have made something of a comeback – a well-deserved comeback – with the emergence of the 'alter-globalisation' movement mentioned earlier in this chapter.[14] This is not to deny that Kaldor had nothing to say on some of the principal issues raised by left-wing critics of global capital: the Tobin tax, for example, the implications of 'fair trade' for the terms of trade between

North and South, or the relative contributions of aid and trade to the process of economic development. Neither did he explore the challenge to neoclassical models of global and regional convergence (Barro and Sala-i-Martin 2004) posed by the new economic geography, which predicts divergence under some (plausible) circumstances (Baldwin et al. 2003).[15] There was only so much that one man could do, in one lifetime.

Of all Kaldor's policy writings, those attacking monetarism had the greatest influence, inside and outside the narrow world of academic economics. His *Scourge of Monetarism* was not as widely reviewed as it should have been, but it did attract a couple of favourable notices. The mainstream Keynesian, Maurice Peston, found the negative aspects of Kaldor's argument more convincing than the positive proposals for an alternative macroeconomic policy, which in 1982 included a price and wage freeze, subsidies on imported food and a dual exchange rate. 'My own acquaintance with ministers is more limited' than Kaldor's, he concluded, 'but on the basis of that I am bound to say that they would greet a scheme of that sort with a combination of bewilderment and frank disbelief' (Peston 1983, p. 422).[16] At a more theoretical level, affinities were noted between Kaldor's thinking and that of 'global monetarists' like Robert Mundell and Ronald McKinnon, who also accepted that 'a country's money supply is demand-determined, so that the domestic money supply is an irrelevant policy variable for controlling inflation' (Miles 1983, p. 1017). Kaldor never responded to this point. It is unlikely that he would have been greatly impressed by the associated policy position: 'The global monetarists agree on the importance of the nominal interest rate, but argue it should be stabilized, not subjected to discretionary changes. Goods prices should also be stabilized, by stabilizing the value of domestic money in foreign exchange or commodity markets' (*ibid.*, p. 1017).[17]

As I have argued elsewhere, Kaldor won the battle with the Chicago monetarists but lost the war (King 2002, pp. 179–80). He won the battle, since the great majority of mainstream economists soon came to accept a number of Kaldorian arguments, including the endogeneity of credit money; the variability of the velocity of circulation; the infinite (or near-infinite) elasticity of the money supply curve and the consequent necessity of using the interest rate rather than the stock of money as the relevant policy instrument; the need for discretion in monetary policy rather than the use of rigid policy rules; and the likelihood that sharply contractionary monetary policy will affect output and employment, possibly severely, and not just painlessly reduce the inflation rate. But

he lost the war, since neither Western governments nor the great majority of academic economists now share his lifelong commitment to full employment, cheap money and democratic (that is, parliamentary) control of monetary policy. It has also become apparent that with ruthless targeting of inflation monetary policy may be highly effective in a downwards direction once real wage resistance has been broken, both in advanced capitalist countries and in Third World. Kaldor believed that monetarism was less a defensible economic doctrine than a thinly disguised attack on organised labour; it eventually proved to be a most successful one.

And yet it was Milton Friedman, not Kaldor, who won the Nobel Prize and who, on his death in 2006, was hailed as 'one of the great economists of all time' by an obituarist who really should have known better (Goodhart 2006).[18] As demonstrated in Chapter 7, Kaldor got the better of Friedman at every point, from the theoretical analysis of a credit-money economy to the practical implications of trying to use an endogenously determined 'money supply' as a policy instrument. It is not necessary to endorse Kaldor's comparison of Friedman with Lysenko (though there certainly are parallels) in order to feel that a serious injustice was done to him by the Swedish academy.[19] He was not, of course, the only British economist of broadly Keynesian sympathies to be overlooked in favour of lesser figures. Joan Robinson, Roy Harrod and (perhaps) Richard Kahn were the other obvious victims. Political prejudice must have played some part, though not in the case of the increasingly conservative (and eventually Conservative) Harrod. So far as Joan Robinson is concerned, the Swedish establishment may well have feared (correctly, I suspect) that she would have made a scene in front of the King at the ceremony. Like the Oscars, the Nobel is supposed not to be a lifetime achievement award, and it could be argued that Kaldor's work was too diffuse, and lacked the single 'great contribution', that the prize required.[20] By the early 1980s this argument was becoming increasingly difficult to maintain, as the deepening world recession vindicated his critique of monetarism as both theory and policy. Perhaps the Swedish academy was simply too embarrassed to admit that, in the case of Friedman, they had made a terrible mistake.

Kaldor's attack on equilibrium economics was probably the final straw. Most mainstream economists would have agreed with Blaug that he had taken this much too far, having

> failed to emphasise that the repudiation of equilibrium economics involves not just abandoning orthodox microeconomics but also

> Keynesian macroeconomics and all varieties of growth theory, including that of Kaldor [Mark] II, leaving little else but Kaldor [Mark] III growth laws as the sum of the content of economics. Needless to say, this is a prospect which will not be welcomed by everyone. (Blaug 1989, p. 91)

There have been a few attempts to model a 'Myrdal-Kaldor economy', for example by Peter Skott (1990, 1999), but up to now they have not progressed very far. Still, we can perhaps permit Hahn the last word on this question. There were some issues on which modern theory had caught up with Kaldor, he noted. 'Since he was a man of exceptional intuitive powers it may well be that it will also catch up with some, at least, of his later work' (Hahn 1988, p. 1746).

9.3 Kaldor in his time – and ours

What, then, is the final verdict on Kaldor's life's work? He ranged very widely, in a very long career, over issues of theory, methodology and policy. Viewed as *theory*, there are four parts of Kaldor's *opus* that have withstood the test of time. First there are the Mark I and II growth and (especially) distribution theories of the late 1950s and early 1960s, some aspects of which continue to form an essential part of heterodox thinking on these important issues, whatever one's misgivings about their equilibrium nature. The second part of Kaldor's theoretical *oeuvre* to have survived is the post-1966 growth theory, culminating in the North-South models of the late 1970s and 1980s, in which dynamic economies of scale played an even more prominent part, and the special role of manufacturing was emphasised. Kaldor's third theoretical contribution was the concept of balance-of-payments-constrained growth, which was explicit in the early Harrod but largely ignored by the later Harrod, by Keynes and by most neoclassical and Post Keynesian writers on growth theory (with the notable and persistent exception of A. P. Thirlwall and his collaborators and students). This was derived from Kaldor's experience as an observer, and then as a policy adviser, in Britain in the three decades after 1945, when growth clearly *was* constrained by external payments problems (the 'stop-go cycle'). As I suggested earlier, however, this was to some extent a historically specific problem, much more severe in the era of fixed exchange rates and considerably weakened by the ending of the Bretton Woods system. Kaldor's fourth theoretical contribution came with his analysis of money, and his comprehensive defeat of Milton

Friedman and the monetarists. As we have seen, however, this was a Pyrrhic victory.

In terms of economic *methodology,* Kaldor's attack on equilibrium theorising remains persuasive and important. The same can be said of his insistence on cumulative causation and path-dependence, even if has yet to lead to a coherent and comprehensive alternative to mainstream economics. Kaldor can also legitimately be described as an early practitioner of Critical Realism in economics, advocating positions with respect to methodology that would subsequently become extremely influential among Post Keynesian and many other heterodox theorists.

But for Kaldor it was *policy* that really mattered. In terms of politics he was remarkably consistent throughout the final half century of his life, with his over-riding social democratic commitment to full employment and social justice, complemented as far as possible by allocative efficiency at the microeconomic level. There were shifts in emphasis over time, especially after 1976 when his experiences in Whitehall led him to stress market failure more than state failure, and to endorse more regulation and control. But this was a matter of emphasis. Thus we can end the book where we began, with the four principal policy proposals of his Mattioli lectures: expansionary fiscal policy, cheap money, incomes policy and commodity price stabilisation. These were derived from a more or less consistent, more or less Keynesian, view of the global economy. In my view they would have worked in 1984, and they would have allowed the world to avoid the recession of 1989–1992. They may yet be needed as the long boom that began in 1992 comes to an end.

Against all the odds, Kaldor himself was basically an optimist. 'Is there any reason for thinking that we now stand on more solid ground?', he asked, rhetorically, at the end of the long 'Introduction' to volume V of his *Collected Economic Papers*. 'Has economics made any real progress or is it just going round in circles?' His answer to 'these major questions' was a positive one:

> In fact, we now have a much better insight into the problem of managing the economy than we had in the aftermath of the Keynesian revolution... there is a far better understanding of the true functions of the market, not just in allocating resources, but in generating and transmitting impulses to technological change and new investment. Once a new consensus emerges (as I am sure it will), we shall be far better equipped in knowing how to run our affairs than we were in

> the past 25 years, and that in turn was a considerable advance over the age of *laisser faire* prior to World War I, or of the primitive interventionism of the years prior to World War II. (Kaldor 1978c, pp. xxviii–xxix)

Perhaps in the very long run he will prove to have been correct.

Notes

1 An Economist from Hungary

1. Biographical information can be found in Pasinetti (1983), Thirlwall (1987) and Targetti (1992), and autobiographical material in Kaldor (1980a, 1986a, 1986b). The most detailed source is Kaldor (1986b), the text of conversations with Maria Cristina Marcuzzo in March 1984. Except where otherwise stated, translations from this Italian text are my own. There is an English transcript in the Kaldor Papers at King's College, Cambridge (NKP 3/138).
2. See Chapter 3, Section 6.
3. 'But he continued to take his examinations at the University of Berlin, and in 1929 was awarded his degree there' (Targetti 1992, p. 2).
4. On Young, see Blitch (1995) and Kaldor's notes from his 1928–1929 lectures at the LSE (Blitch 1990; Sandilands 1990).
5. This paper is discussed in some detail in Chapter 2, Section 2.
6. This is a direct (but unattributed) quotation from Young (1928, p. 533).
7. Extracts can be found in Blitch (1990).
8. See Robbins (1971) for his own account of the Austrian influence upon his early thought.
9. See Thirlwall (1987, chapter 1).
10. For Hayek's subsequent transformation into a fierce critic of many aspects of neoclassical economics, see Caldwell (2004).
11. Kaldor's own account of these experiences will be considered in Chapter 6, Section 4.
12. Such a device featured in 1963 in Stanley Kubrick's brilliantly satirical film on the Cold War, *Doctor Strangelove*.
13. Crossman 1975, pp. 294, 305. The context in which this accusation was made is described in Chapter 5, Section 4.
14. On Kaldor's socialism, see Chapter 5, Section 2; on his increasing radicalism after 1979, see Chapter 7, Section 4.
15. On this brief but important episode in the history of Chinese Communism, when internal debate was encouraged and foreign visitors were welcomed, see Goldman (1987, pp. 242–53).
16. A more serious oversight soon affected Galbraith's own sabbatical plans. He had intended to spend the summer at King's College, Cambridge and work there on the manuscript of his book, *The Affluent Society*: 'Nicholas Kaldor had suggested it but had forgotten to tell the college. Richard Kahn seemed surprised when I called him from London and doubted that there was any space available. So I went instead to Switzerland and joined Kitty, Emily and the children in Gstaad' (Galbraith 1981, p. 335). Galbraith seems to have suffered no long-term damage from this great hardship.
17. T. Inamaru to Nicholas Kaldor, 12 June 1956; Nicholas Kaldor Papers 3/17/3.

18 N. Kaldor to T. Imamaru, 14 August 1956; Nicholas Kaldor Papers 3/17/3.
19 This is of course a reference to the character in Kenneth Grahame's famous comical book for children, *The Wind in the Willows*.
20 See Chapter 5, Section 4.
21 One of the few subjects in which he took very little interest was the economic progress of the Second (or Communist) World.
22 A ninth volume appeared after his death, edited by his two biographers, Ferdinando Targetti and A.P. Thirlwall (Kaldor 1989a).
23 See note 1 above.
24 A definitive bibliography would have to include also Kaldor's many letters to *The Times* and other daily and weekly papers, including the *New Statesman*, together with his speeches in the House of Lords.

2 Not the Devil's Decade

1 In later life Kaldor was a tireless writer of letters to *The Times*: 'several hundreds' of them altogether (Kaldor 1980a, p. viii).
2 This was the English version of Hayek's *Monetary Theory and the Trade Cycle*, translated by Kaldor and Honor Croome (Hayek 1933).
3 Kaldor's review of Lederer's *Outline of Economic Theory* was less critical, though he rejected the German socialist's theory of interest as an unsuccessful attempt to synthesise Schumpeter and Marx (Kaldor 1932d, p. 481).
4 See Chapter 5, Section 2, for a more extended discussion of Kaldor's political beliefs, in the context of his views on economic policy.
5 'There is a deep underlying wisdom in the vagueness of Marshallian economics which seems to escape altogether the precision of the mathematical mind' (Kaldor 1936a, p. 97).
6 This is reported in the 'List of Theses in Economics and Allied Subjects in Progress in Universities and Colleges in the British Commonwealth of Nations' published in the August 1931 issue of *Economica* (p. 373), with Kaldor's 'probable date of completion' given as 1932. Twelve months later the details were unchanged. In the final list to be published (August 1933, p. 366) the title had been broadened (to 'Studies in the Economic Policy of Central European States since the War') and the completion date had been deleted.
7 See especially Kaldor (1932c, p. 29). But he never returned to this topic, and seems always to have been unaware of, or uninterested in, Minsky's ideas.
8 But see Cohen (2006) for a very different assessment of this article.
9 All this is, of course, straight Marshall.
10 For details of their first meeting, in April 1933, see King (1998b, p. 412).
11 As noted by the reviewer of the first two volumes of Kaldor's *Collected Papers* (Dorfman 1961, p. 495). Strangely the same word ('Talmudic') had been used by Joan Robinson in correspondence 28 years earlier to describe his verbal exposition of marginal productivity theory (King 1998b, p. 412).
12 See however Targetti and Thirlwall (1989, pp. 2–3), who take a much more favourable view of Kaldor's writings in this area.
13 See Thirlwall (1987, p. 29) for details of Kaldor's involvement with the *Review of Economic Studies*, a journal which formed an important bridge between the young economists in Cambridge and at the LSE.

14 The term is admittedly anachronistic. Kaldor himself did not use it, instead describing the three problems as being whether equilibrium is 'determinate' or 'indeterminate'; 'unique' or 'multiple'; and 'definite' or 'indefinite' (1934a, p. 125).

15 He may also have drawn on Marshall's 'Pure Theory of Domestic Value' (Marshall 1930), privately printed for the author in 1879 and reprinted by the LSE in 1930 in its classical reprints series.

16 Again Kaldor himself did not use this term, but it is transparent in what he did explicitly argue.

17 The name 'occurred to me in the course of an oral exposition of that theorem at the L. S. E. seminar' (Kaldor 1960a, p. 4). On Robbins's celebrated graduate seminar, see Kaldor (1986b, pp. 38–9), Galbraith (1981, p. 78) and McCormick (1992, p. 30).

18 Note that Kaldor also made the standard undergraduate error of confusing elasticity with slope.

19 These qualifications were later formalised by John Rawls (1971) in his *maximin* principle.

20 It might be supposed that there is a substantial philosophical literature on the possibility of making inter-personal comparisons. Interestingly enough, Harsanyi cites no philosophical sources. In general, it may be said that moral philosophers recognise the difficulty but refuse to take it seriously: 'because we often make rough and ready comparisons between the effects of decisions on different persons, utilitarians continue to suppose that interpersonal comparisons of utility are generally possible' (Lyons 1992, p. 1265).

21 The relevant passage has been translated by John Chipman (1999 [1976], p. 178). It is a remarkably turgid piece of prose; as a rhetorician, Kaldor beats Pareto hands down. (I owe this reference to Michael McLure.)

22 During the War Kaldor worked with Wootton on William Beveridge's 'technical committee' on the economics of full employment, but they do not seem to have been close friends and I know of no evidence that they had met in peacetime (though as they were both Fabian socialists living in central London the possibility cannot be ruled out).

23 As Chipman notes, this was an unfortunate example, since an optimal tariff would have proved superior to both the pre-1846 tariff on corn imports and to free trade, as Kaldor himself acknowledged in a later article. Thus free trade was only a second-best solution (Chipman 1987, pp. 526; cf. Kaldor 1940f). Kaldor continued to claim, correctly, that the pre-1846 tariff had been the least advantageous of the three alternatives (Kaldor 1960a, pp. 5–6).

24 And Barone, whose paper was translated in 1935 as part of Hayek's anti-socialist anthology *Collectivist Economic Planning* (Hayek 1935). The Italian's contribution, however, 'seems not to have struck home' at the time (Chipman 1987, p. 525).

25 His friend J. R. Hicks followed him in the very next (December) issue of the *Economic Journal* (Hicks 1939).

26 See Graaf (1957, pp. 84–90) and Irwin (1996, pp. 186–8) for a full discussion of this literature.

27 First, a 16-page appendix on 'Keynes' theory of the own-rates of interest' was cut from the original manuscript by the editor, Ursula Hicks, on

grounds of length. This was first published in 1960 (Kaldor 1980c). Second, Kaldor revised his analysis of the relationship between expected price, current price and forward price in response to criticism by Christopher Dow (1940) and Ralph Hawtrey (1940). Just to complicate matters even further, his response to them (Kaldor 1940d) appeared, in a 'Symposium on the Theory of the Forward Market' in the June 1940 issue of the *Review*, immediately before their criticisms, which covered both his original article and his response to theirs. Third, he made further significant changes to the text for the 1960 book edition. However, all the important issues were on paper, if not yet in print, by 1940. For this reason, and to keep the exposition reasonably uncluttered, I have decided to treat the 1960 published text as canonical. All references to Kaldor's preferred (1960) version are to the 1980 reprint (Kaldor 1980c, pp. 17–74), except where it has been necessary to cite the 1940 article (Kaldor 1940d).

28 On which see Stabile (2005) and Toporowski (2005). Mainstream finance theory began much later with Markowitz (1952), at least according to the Nobel laureate Merton Miller (1999).

29 Hawtrey complained that Kaldor still had not taken this point seriously enough (Hawtrey 1940, p. 205).

30 Pollin 1991; cf. Rochon 2000 for a contrary view. I shall have much more to say on this controversy in Chapter 7.

31 I owe this important point to Geoff Harcourt.

32 This was a common feature of the trade cycle models of the 1940s and early 1950s, as Hyman Minsky (1957) complained.

33 'In reality, however, the rate of expansion of firms is confined by their financial resources (quite independently of the behaviour of market rates of interest), which means that they cannot take advantage of large investment opportunities as quickly as of small ones' (Kaldor 1951b, p. 839). Kaldor linked this point to Kalecki's 'principle of increasing risk' (*ibid.*, p. 841), and in 1951 stressed his affinities with Kalecki to a much greater extent than he had done in 1940.

34 This was, in effect, a return to Keynes's own emphasis on business expectations as the driving force in the trade cycle (Keynes 1936, pp. 315–20).

3 Kaldor's War

1 The scale of activities was necessarily considerably reduced, a peacetime complement of 90 academics and 3,000 students declining to 9 professors, 35 lecturers and 500 undergraduates. Student numbers rose slightly in the later stages of the war, while the number of staff continued to fall (Abse 1977, p. 47; cf. Dahrendorf 1995, chapter 6).

2 Cited from the English typescript of Cristina Marcuzzo's interview with Kaldor (Nicholas Kaldor Papers, King's College, Cambridge, NKP 3/138, p. 48).

3 The theorem was first published in Danish in 1941, by Johann Gelting; its first English appearance was in a 1945 paper by Trygve Haavelmo, with 'an important early contribution' having been made in the previous year by Henry Wallich (Peston 1987, p. 178).

4 For further discussion of Kaldor's views on incomes policy, see Chapter 5, Section 1.

5 Evidently a market failure is involved in this claim, which implies that the opportunity to insure against these risks was not being offered by private providers. The standard arguments concerning adverse selection and (perhaps) moral hazard certainly apply to social security, as Kaldor would have readily agreed. He probably regarded this point as being too obvious to require explicit statement.
6 Kaldor wrote two articles, which were published anonymously on successive days (Kaldor 1943c, 1943d). His authorship has been established by Targetti (1992, p. 94).
7 A decade later the idea that there were substantial differences in consumption and savings propensities of workers and capitalists would prove fundamental to his theoretical models of income distribution and growth (Kaldor 1956a; Chapter 4, Section 3, below).
8 Kaldor further amended the numbers by assuming an increase in exports and imposing the requirement that there must be no balance of payments deficit, but this added nothing important to the argument (Kaldor 1944a, pp. 364–59).
9 This was later emphasised by James Meade (1945) in the criticism of the principle of functional finance that he made in his review of Abba Lerner's *Economics of Control*.
10 Two decades later Kaldor oversaw the introduction of a Selective Employment Tax to achieve precisely this end (see Chapter 5, Section 4 below, and also Thirlwall 1987, pp. 241–6).
11 The formula is $r < g$, where r is the real rate of interest and g is the rate of growth of output (Burger 2003, chapter 2).
12 F. von Hayek to Kaldor, 25 January 1942, 29 January 1942; Kaldor to A. M. Carr-Saunders, 4 February 1942; Carr-Saunders to Kaldor, 7 February 1942: Nicholas Kaldor Papers, King's College, Cambridge, NKP 3/30/85.
13 L. Robbins to Kaldor, 8 June 1944; Kaldor to Robbins, 18 June 1944: Nicholas Kaldor Papers, King's College, Cambridge, NKP 3/9/2.
14 For the politics, both academic and international, behind this refusal see Thirlwall (1987, pp. 104–5).
15 See his reminiscences in Kaldor (1980a, pp. xix–xxi), and also Thirlwall (1987, pp. 100–11) and Targetti (1992, pp. 10–12). The potentially radical implications of the Report are emphasised by Turnell and Ussher (2008).

4 A Return to Theory

1 See Harcourt (2006, especially pp. 102–19) for an exceptionally lucid account of the theoretical questions discussed in this and the following four sections.
2 'The tragedy of investment is that it causes crises because it is useful. Doubtless many people will consider this theory paradoxical. But it is not the theory which is paradoxical, but its subject – the capitalist economy' (Kalecki 1939 [1990], p. 318).
3 See Thirlwall (2001) for a discussion of the six possible cases linking the warranted, natural and balance of payments-constrained rates of growth.
4 In the context of the early 1950s these terms are, of course, anachronistic. While 'neoclassical' was in widespread use at the time, it was not commonly

contrasted either with 'heterodox' or with 'Post Keynesian' theory. On the rather complicated history of the latter term – with and without the hyphen, with and without a capital 'p' – see King (2002, pp. 9–10).

5 On the origins of Kaldor's macroeconomic distribution theory, see Targetti (1992, pp. 107–10). Kaldor's own accounts can be found in Kaldor (1960b, p. 8 n1; 1978c, p. ix n1; and 1980a, pp. xxiii–xxiv).

6 It was not at all secret, but attendance was by invitation only.

7 Full details are given in King (1995b, unpaginated, reference I26).

8 The ensuing controversy is described in great detail by Harcourt (1972, chapter 5).

9 As Luigi Pasinetti put it at the time, in a paper not published until 1974, there are three logical possibilities. First, if $s_w < g_n/v$, there is no difficulty, and the Cambridge equation holds. Second, if $s_w > g_n/v$, equilibrium growth is impossible. Third, if $s_w = g_n/v$, the Harrod 'knife-edge' applies, with capitalists eliminated as a class by the workers, who save so much that they end up owning everything (Pasinetti 1974, p. 134).

10 See the two introductory essays in Bliss, Cohen and Harcourt (2005), which set out the respective positions of the Post Keynesians (Cohen and Harcourt) and the neoclassicals (Bliss) and illustrate very clearly the continuing absence of any common ground, almost forty years later.

11 These were some of the 'stylised facts' that were to play such an important part in his later methodological work (see Chapter 8 below).

12 The profit share is P/Y, and the capital-output ratio is K/Y. If both are constant, it follows that the rate of profit, $r = P/K$, is also constant.

13 In steady-state growth, where output and capital grow at the same rate, $g/\ s_p = I/K\ /\ S/P = P/K = r$.

14 Kaldor (1957b) 'started as a joint paper with Champernowne, but the outright rejection of neoclassicism was too much for his co-author who withdrew into the background, helping only with the mathematics' (Thirlwall 1987, p. 182 n6).

15 See Kaldor (1980a, pp. xxv–xxix) for his most detailed and self-critical assessment of the 1956–1962 growth models.

16 There is a substantial literature on the Verdoorn Law. Three useful sources are Shaw (1992), Ros (2000, Appendix to chapter 4) and especially McCombie, Pugno and Soro (2003).

17 This is an allusion to the arcane marking scale then used at both Oxford and Cambridge.

18 A similar point had been made earlier by J. N. Wolfe, who noted that Kaldor's unorthodox analysis could be used to support the quite orthodox policies of increasing saving and reducing government expenditure to promote growth (Wolfe 1968, pp. 125–6).

19 See McCombie (1983), Thirlwall (1983), and also Kaldor (1975c) for his reply to Rowthorn.

20 See Chapter 5, Section 5 and Chapter 7, Section 5 below.

21 The paper had already appeared in 1976, in Spanish, in the *Festschrift* for which it had originally been written (Kaldor 1977a, p. 193n).

22 A comprehensive survey of the debate on British's post-1870 economic performance is provided by Dintenfass (1992, 1999). See also Crafts (1991, pp. 279–83).

23 The other five items in his bibliography were the classic *Principles* texts of David Ricardo and John Stuart Mill, two papers of his own (Kaldor 1971a, 1976) and an important article by Robin Matthews on the reasons why Britain had enjoyed full employment after 1945 (Matthews 1968). None of this was economic history, as conventionally understood.
24 This proposition formed the basis of Rosa Luxemburg's theory of global capitalist development and the explanation of imperialism that she derived from it. Kaldor, however, nowhere refers to Luxemburg or to her *Accumulation of Capital* (Luxemburg 1913; cf. Howard and King 1989, chapter 6).
25 If it is assumed to be constant, m represents both the marginal and the average propensity to import.
26 Surprisingly he did not suggest that they should be regarded as the *effects* of slow growth and not as the causes.
27 This important lecture is discussed at some length in Chapter 5, Section 5.
28 See McCombie and Thirlwall (1994) for a comprehensive exposition and defence of the continuing relevance of this formula.
29 On Okun's Law see Chapter 8, Section 4.
30 As Dintenfass (1992, 1999) explains, some economic historians deny that there was any such 'poor performance', in which case attempts to explain it are obviously superfluous.
31 Once again, however, he never referred to the voluminous Chicago-inspired literature on the determinants of consumption expenditure.
32 This question is discussed further in Chapter 5, Section 5.

5 The British Economic Disaster, 1964–1979

1 Too much should not be made of this latter problem: productivity growth was affected much more by low investment and the consequent ageing of the capital stock than by indiscipline in the factories.
2 Kaldor's only attempt to rebut this accusation is not very convincing. It came in a brief footnote, where he asserted that 'share values invariably reflect *expected* dividend payments; a long-term control on dividend increases will therefore reduce the rate of accrual of capital gains as well as of dividend income' (Kaldor 1964d, p. xv n1).
3 I am very grateful to Mary Kaldor for information about her father's early life and political beliefs.
4 His sister was in a Stalinist labour camp in the 1950s and his brother-in-law was also imprisoned in Hungary (again I owe this information to Mary Kaldor).
5 This is a reference to George Woodcock and H. L. Bullock, who in 1955 were co-signatories of Kaldor's minority report on the taxation of profits and income.
6 'So far as I know they [the Conservatives] have no concrete plans for the denationalisation of any particular part of the existing public sector' (Kaldor 1980d, p. 4). This was presented at a conference in January 1978 and was therefore presumably written in the previous year.
7 It could not formally be termed a Minority Report, as it had only three signatories (Kaldor 1980a, p. x).

8 This is an exaggerated claim, to say the least. To achieve a given target level of aggregate expenditure, the numerical rates of taxation must differ depending on whether it is income, consumption or total expenditure (consumption plus investment) that is subject to tax, but the inherent difficulty of achieving the target is not affected by the nature of the tax base.

9 To put this figure in context, it should be remembered that in the early 1950s the 'surtax' on very high incomes meant an effective marginal rate of income taxation of 95 per cent.

10 Balogh's position on this issue was more complex than Kaldor's. He had opposed the 1949 devaluation on the grounds that its benefits would soon be lost through higher inflation due to real wage resistance (Balogh 1963, pp. 173–8). Right up to the October 1964 general election he remained hostile to a further devaluation, arguing that an incoming Labour government would be able to introduce a successful incomes policy, with trade union support, reducing the inflation rate and restoring the country's international competitiveness without any need to reduce the value of the currency (Crosland 1982, p. 120; Pimlott 1992, p. 352). 'Three weeks after the election Tommy switched', and now advocated devaluation as an unpleasant necessity that should be got out of the way as quickly as possible (Crosland 1982, p. 135; cf. Crossman 1975, p. 71). Balogh himself subsequently regretted that the 1967 devaluation had come 'belatedly', but continued to express strong reservations about the permanent benefits of currency depreciation (Balogh 1982, p. 197). As we shall see in Chapter 7, Kaldor soon came to agree with him on this score.

11 Unemployment did rise after 1964, but only to a peak of 2.6 per cent in 1970, the year that Labour lost office (Caves and Krause 1980, p. 6, table 6).

12 Wilson's government finally bowed to the inevitable and announced a 15 per cent devaluation in November 1967.

13 There is a lengthy account by Kaldor of his experiences as a tax adviser in the United Kingdom in the introduction to volume 7 of his *Collected Essays* (Kaldor 1980g).

14 In 1970 Supplementary Benefit was the principal form of social welfare payment used to bring very poor citizens up to a minimum subsistence level of income; its present-day equivalent is Income Support.

15 Kaldor uses the old Marshallian term, 'efficiency wages' (Kaldor 1970b, p. 320); this should not be confused with the later mainstream usage of the term to denote the ability of wage increases to elicit higher productivity through increased effort and commitment on the part of the individual worker (Stiglitz 1987).

16 What is now called the European Union (EU) took this name only in 1993. Between 1967 and 1993 it was known officially as the European Community (EC) and from 1958 to 1967 as the European Economic Community (EEC). When British membership was a live political issue, roughly from 1963 to 1975, it was generally referred to as the Common Market.

17 The text was originally presented at a seminar in Denmark organised by the International Confederation of Free Trade Unions.

18 In 1978 he described the other arguments as 'secondary'; they 'could be brushed aside if the long-run effects of membership on Britain's

manufacturing industry and on our capacity to provide employment were favourable' (Kaldor 1978a, p. xxvii).

19 He subsequently amended the metaphor slightly, holding out the prospect that Britain might become 'the "Northern Ireland" (or Sicily) of Europe' (Kaldor 1978d, p. 201).
20 Ireland, which joined the Common Market along with Britain in 1973, enjoyed remarkably rapid economic growth over the next 35 years. Perhaps Kaldor intended his geographical metaphor as nothing more than a figure of speech, but in that case he should have specified the socioeconomic characteristics of an 'offshore island' with much greater care.
21 I am grateful to Harvey Armstrong for these references.
22 'I would like to state that I feel I am as much responsible for that failure as any other economist of the time' (Kaldor 1971a, p. 5).
23 Though he allowed it to be reprinted, without editorial comment, in volume 5 of his *Collected Economic Papers*.
24 He had done the same thing in his 1940 model of the trade cycle, but with the level of income, not the rate of change of consumption, as the principal determinant of investment (see Chapter 2, Section 5).
25 Denis Healey's was however 'a bastard kind of monetarism, like the bastard Christianity of the American Indians who worship pagan gods along with the true God' (Kaldor 1980f, p. 3). Healey still believed that excessive wage increases were an important cause of inflation, something that the Friedmanites denied, and therefore continued to operate an incomes policy, which they regarded as both ineffective and inefficient.
26 This was a rather different version of the 'political business cycle' than that originally presented by Michal Kalecki (1943), who emphasised the electoral cycle and the hostility of capitalists to sustained full employment, and said nothing about the balance of payments constraint.
27 Earlier in the book Healey had described Kaldor as 'the most brilliant economist of his generation in Britain' (Healey 1989, p. 368).

6 Kaldor and the Third World

1 On the background to the Bandaranaike assassination, see Alles (1986) and Manor (1989).
2 On Kwame Nkrumah, the first President of Ghana, see James (1977) and Davidson (1989). For an account of Ghana's difficulties from the perspective of the eminent development theorist W. Arthur Lewis, see Tignor (2006).
3 On Cheddi Jagan and the early years of independence in British Guiana (later Guyana), see Spinner (1984).
4 As noted in Chapter 5, Section 1 above, Kaldor's neglect of social norms, comparisons and the importance of fairness was the principal weakness of his otherwise compelling 1950 memorandum on wages policy.
5 A very thorough formalisation was, however, provided by Peter Skott (1999) in his review of the Mattioli lectures.
6 Kaldor made a bow in the direction of his old friend and colleague by invoking the Hicksian distinction between 'flexprice' and 'fixprice' sectors (Kaldor 1986c, pp. 193–4; cf. Hicks 1974).

7 See also Emmanuel (1972) and Howard and King (1992, chapter 10).
8 The International Wool Agreement, for instance, collapsed after the (principally Australian) producers insisted on setting a wildly unrealistic floor price for the commodity (Bardsley 1994).

7 The Scourge of Monetarism

1 A partial bibliography on this question would include Thirlwall (1987, chapter 12), Desai (1989), Lavoie (1991), Minsky (1991), Moore (1991b), Pollin (1991), Targetti (1992, chapter 11), Hewitson (1993), Musella and Panico (1993), Rochon (2000), and Bertocco (2001).
2 Compare Rochon (2000, pp. 197–200) with Minsky (1991, pp. 211–12) and Musella and Panico (1993, pp. 40–5).
3 The second volume was published in 1942; there is no evidence that Kaldor took any interest in it.
4 Despite this hostility, there is reason to suppose that Kaldor had exerted some influence on the final Radcliffe report (Targetti and Thirlwall 1989, p. 5).
5 He was criticised for this neglect at the time (Harrod 1965, pp. 797–8).
6 Subsequently, influential academic support for monetarism came from Brian Griffiths (City University) and Patrick Minford (Liverpool University).
7 The history of this important but question-begging term has yet to be written. Major landmarks include the volume edited by the monetarist Edward S. Phelps (1971) and the proceedings of the 1975 S'Agora conference of the International Economic Association (Harcourt 1977). In 1968 many economists felt uneasy about the apparent inconsistency between their non-market-clearing Keynesian macroeconomics and their market-clearing neoclassical microeconomics, but the search for 'microfoundations' was still in its very early stages, and had not yet hardened into the dogma that it would eventually become.
8 Although Kaldor did not say so, they represent the 'classical dichotomy', which in fact goes back at least as far as Ricardo.
9 This did not prevent Kaldor from reporting regression equations of his own in an Appendix (Kaldor 1970a, pp. 22–3), though he made very little of them.
10 This point had been made, even more vigorously, by J. K. Gifford, Professor of Economics at the University of Queensland, who (rather surprisingly) managed to publish it in the *Journal of Political Economy* (Gifford 1968; King and Millmow 2008).
11 See also (Kaldor 1960c, p. 715) and, even more emphatically, Kaldor and Mirrlees (1962, p. 189).
12 He could also be extremely funny. In one speech he called for a large increase in conventional defence spending, accompanied by nuclear disarmament. 'Otherwise, if it were a credible notion that the Russians should want to colonise an island as bankrupt and as unruly as Britain – their troubles with Afghan tribesmen would pale into insignificance when confronted with British trade unionists – they could do the dirty on us' (Kaldor 1981d, p. 2).
13 Kaldor had changed his mind on this last question. As noted earlier, in 1970 he had identified changes in the PSBR as the principal cause of changes in the stock of money (Kaldor 1970a, p. 20).

14 This was precisely the sort of 'supply-side' interpretation of British economic history that he had earlier repudiated (see Chapter 4, Section 7). In the final years of his life, however, Kaldor frequently denounced poor management, defective technical education and the dominance of finance over manufacturing (see, for example, Kaldor 1981c).
15 N.B. (Norman Buchan?) – to Peter Shore, 24 May 1983, in Peter Shore papers, London School of Economics, SHORE 13/164.
16 See also Kaldor (1980a, p. xv) for another very clear statement of the need to assume imperfect competition in deriving the principle of effective demand.

8 The Irrelevance of Equilibrium Economics

1 Robinson and Eatwell (1973) was her attempt to provide an introductory textbook statement of the new paradigm. It was a resounding failure (King and Millmow 2003).
2 J. Robinson to R. Kahn, undated but probably late December 1971, Richard Kahn Papers, King's College, Cambridge, 13/90/0/196–7.
3 Though not, except very indirectly, among business and government economists.
4 As noted in Section 8.3, Kaldor was not well-read in the philosophy of science. He seems to have been unaware of the Lakatosian criticism of falsification as applied to individual scientific hypotheses, or of the implications of this criticism for general equilibrium theory viewed as a comprehensive scientific research programme (Lakatos 1978; see also Blaug 1992).
5 This is a direct quotation from Young (1928, p. 530).
6 Austrian economists might complain that Joseph Schumpeter, Friedrich von Hayek and Israel Kirzner had in fact made some progress in exploring both the market as a process and the creative functions of the entrepreneur. Others might wonder where the Hicksian notion of 'fix-price markets' (see Chapter 7, Section 5) has gone in all this.
7 See, for example, Kaldor (1960e, p. 181); Kaldor (1961, pp. 3–4); Kaldor (1966c, pp. 309–10); Kaldor (1972a, pp. 1238–40); Kaldor (1981b, p. 433); Kaldor (1996, pp. 101, 132).
8 See Chapter 3, Section 6.
9 On the implications of non-ergodicity, see Davidson (1996).
10 There is a huge literature on Critical Realism and its application to economics. Bhaskar's work is, to say the very least, heavy going, but accessible introductions are provided by Dow (2003), Lawson (1997, 2003) and Austen and Jefferson (2006).
11 In this paragraph, and at many other places in this chapter, I have drawn heavily on Jefferson and King (2009). I have learned a great deal from discussions with James Doughney and Therese Jefferson on these questions.
12 On the distinction between closed-system and open-system thinking, see the brilliant article by Chick and Dow (2005).
13 Many of the arguments can also be found in the Mattioli lectures, delivered only seven months later. There was, inevitably, a certain amount of repetition in Kaldor's later published work. He rarely turned down an opportunity to lecture or to publish and – despite his professed aversion to

fixed theoretical systems – there were only a finite number of ways in which any particular idea could be expressed.

14 Here Kaldor seems to have been quoting from Okun (1981), but he does not provide a page reference.
15 Here Kaldor cited another Marshallian, Ralph Hawtrey (1926, p. 39) – 'a highly stimulating book that is rarely read nowadays' (Kaldor 1985a, p. 19).
16 Several of these themes can be found also in the first Mattioli lectures (Kaldor 1996, pp. 3–19).
17 This is the text of a lecture that Kaldor gave in 1973, which he reprinted six years later in a *Festschrift* for his old LSE friend (and fellow Hungarian) Tibor Scitovsky. Similar statements can be found in both the Okun lectures (Kaldor 1985a, pp. 72–4) and the Mattioli lectures (Kaldor 1996, pp. 55–71).
18 See especially Kaldor (1934a), Setterfield (1998), and Chapter 2, Section 2.
19 Here Kaldor cited Stolper and Samuelson (1940).
20 See Chapter 3, Section 7.
21 See Dow (1996) on the near-oxymoronic character of Austrian macroeconomics.
22 See Chapter 5, Section 2 and Chapter 6, Section 2.
23 See Chapter 4, Section 6.
24 On the history of the term, and the various ways of writing it, see King (2002, pp. 9–10).
25 The third of these criticisms was false, and the second, although true of the *General Theory*, did not apply to Keynes's post-1936 work, which laid the foundations for a coherent open economy macroeconomics (Vines 2003).
26 See McCombie (1983) and Thirlwall (1983).
27 The others were Erwin Rothbarth, David Champernowne and Tibor Scitovsky.
28 Wrongly so, according to Kriesler (1987). Kaldor made the same point in correspondence with Joan Robinson: Kaldor to Robinson 12 December 1956, Nicholas Kaldor Papers, NKP 3/30/175.
29 The relevant correspondence is in the Kaldor papers: NKP 3/30/126.
30 Despite his long-standing friendship with von Neumann, he seems never to have thought of game theory, evolutionary or otherwise, as relevant to his own concerns.
31 The 'new consensus' in macroeconomics was sanctified by a high-powered symposium at the 1996 American Economic Association meetings. See Blinder (1997), Eichenbaum (1997), Solow (1997) and Taylor (1997), who all sing from essentially the same hymn-sheet.
32 For rather similar arguments, but from an explicitly Post Keynesian perspective, see Cornwall (1994).
33 See Sawyer (2007) for a more recent attempt to specify the theoretical core of heterodox (read, Post Keynesian) macroeconomics.
34 But never, apart from one book review, in the *American Economic Review* (or, after 1936, in the *Journal of Political Economy*).
35 On the Debreu-Mantel-Sonnenschein critique, which demonstrated the inability of general equilibrium models to generate even the most banal predictions, see Lavoie (1992, pp. 36–41). To repeat: these were practitioners of Walrasian economics, and their self-criticism owed nothing to Kaldor or to any other protestors from outside.

9 Kaldor in His Time – and Ours

1. There is just one further exception, a detailed and entirely empirical investigation of the British advertising industry and the revenues of the press (Kaldor and Silverman 1948). This volume is so unlike anything else that Kaldor ever published that it is safe to conclude that it was largely written by his co-author, Rodney Silverman, under Kaldor's (probably rather light) supervision.
2. Despite all these reservations, the case for a progressive tax on consumption expenditure remains compelling (Frank 2005, 2007).
3. He was the joint author of the classic text on the subject, Dorfman, Samuelson and Solow (1958).
4. The first part of this criticism, concerning the small number of equations, did not apply to Kaldor's Mark II model (Kaldor and Mirrlees 1962). On the early history of large-scale macroeconometric modelling, see Morgan (1990); Dorfman's call for empirical testing of growth models in the early 1960s was a very big ask.
5. Oddly neither Baumol nor Hamberg registered any doubts concerning the empirical validity of these 'stylised facts', which, as Mark Blaug was later to observe, were often not facts at all (Blaug 1989, pp. 78–80).
6. This was a criticism that Harrod had made in his review of the *General Theory* (Harrod 1937).
7. For Harcourt's description of this unhappy experience, see King (1995a, pp. 168–70).
8. Even this slogan will soon need revision, as China moves increasingly into elaborately transformed manufactures and low-skilled manufacturing moves to Vietnam, or Indonesia, or Africa.
9. See Chapter 4, Section 3.
10. See Chapter 2, Section 5.
11. Aficionados of jazz might find analogies with John Coltrane and Paul Desmond more enlightening – late Coltrane, naturally.
12. Hahn also noted with some justice that 'Kaldor on numerous occasions had insights which were not accessible to "common sense" and required some mathematics to establish as correct' (Hahn 1988, p. 1747).
13. Note, however, his stated belief that a return to Bretton Woods in the late 1970s would have made things worse (Kaldor 1978c, p. xxi). Not all Post Keynesians agree with Davidson on this important point, Wray (2006) for example arguing that flexible exchange rates are a necessary condition for national economic sovereignty.
14. On which, see Klein (2002, 2007) and Monbiot (2003).
15. I am grateful to Harvey Armstrong for this reference.
16. As we have seen, the proposals put forward in the Mattioli lectures were considerably less detailed and ambitious than these.
17. There is just a hint, here, though, of Kaldor's own proposal for international monetary reform and the stabilisation of commodity prices via buffer stocks. It is unfortunate that he did not explicitly consider the ideas of the 'global monetarists'.
18. This bizarre claim was echoed by Paul Krugman in a lengthy and for the most part critical obituary in the *New York Review of Books*, which

unaccountably concluded by describing Friedman as 'one of the most important economic thinkers of all time' (Krugman 2007, p. 12). (I am grateful to John Shannon for bringing this article to my attention.)

19 As early as 1979 the *Economist* (20 January) had described Kaldor as 'the best known economist in the world not to have received the Nobel Prize'. (I owe this reference to A. P. Thirlwall.)

20 Note, however, that Richard Kahn made two such contributions, his 1929 dissertation on the economics of the short period and his renowned 1931 article on the multiplier (Kahn 1931, 1990), and was overlooked nevertheless. (He probably did not help his cause by failing to publish the dissertation for 61 years.)

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