

# Not the Devil's Decade: Nicholas Kaldor in the 1930s

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**Abstract:** At the beginning of the 1930s Nicholas Kaldor (1908-1986) was a third-year undergraduate student at the London School of Economics. By the end of the decade he was an established economic theorist with an international reputation and a string of well-received publications to his name. In this paper I focus on Kaldor's four most important articles from this period: 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. All four articles, I argue, are of considerable continuing interest.

## 1 Introduction

The Anglo-Irish journalist Claude 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 the life and career of the subject of this paper. Nicholas (Miklos) Kaldor was born in Budapest on 12 May 1908 into a prosperous middle-class Jewish family.<sup>1</sup> In 1925-27 he studied economics at the University of Berlin, his interest in the subject having been aroused by his experience of the German hyperinflation during a family holiday in Bavaria in 1923. Dissatisfied with the standard of teaching in Berlin, Kaldor returned to Hungary before enrolling at the London School of Economics in the autumn of 1927. Here he came under the influence first of the idiosyncratic American Marshallian, Allyn Young, and then of Young's charismatic successor to the chair of Economics, Lionel Robbins (Kaldor 1986a, p. 12; 1986b, pp. 31-4, 38-9).<sup>2</sup> On 1 January 1930 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); he was enrolled as a research student in 1930, appointed to an Assistant Lectureship in 1932 and, belatedly, promoted in 1938 to full Lecturer at the School.<sup>3</sup>

Kaldor 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.<sup>4</sup> 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). He was also co-translator of Friedrich von Hayek's extended critique of underconsumption theory (Hayek 1931). Kaldor's 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* (1932a). Over the next eight years he published 22 journal articles, 8 substantial book reviews and a translated book.<sup>5</sup> 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 particularly elegant (Dorfman 1961, p. 495), and 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. Of the rest, 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 n. 7), 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 criticises 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 concludes, 'sounds very strange to people who were brought up on the marginal productivity analysis' (1932a, p. 190). Unemployment is due not to technical change but to 'monopolistic interference with the price system', in this case – presumably – by trade unions (*ibid.*, p. 195).<sup>6</sup> Reviewing Carl Landauer's *Planned Economy and Market Economy*, Kaldor invokes Ludwig von Mises' 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. (1932b, p. 379)

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 adopted the moderate Fabian socialist beliefs that he was to hold for the rest of his life.

In later life Kaldor read little and rarely reviewed books, but in the 1930s he was a very active reviewer, in particular for the LSE house journal, *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,<sup>7</sup> and H. von Stackelberg's *Market Form and Equilibrium*, which impressed him greatly (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'.<sup>8</sup> 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 a Minskyian theory of financial instability, 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.<sup>9</sup> For the most part, however, Kaldor's interpretation follows 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; Oskar Lange and Nicholas Georgescu-Roegen were the other European recipients that year. This permitted him to travel widely in the United States in 1935-6. 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 (1936c, p. 271 n. 1) 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* (1937a). For the most part this is an intense, respectful and rather uninteresting account of the analytical differences between Hayek and the Chicago capital theorist Frank Knight,<sup>10</sup> but there is a sting in the tail. In the final two pages of the survey Kaldor slides 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'. (1937b, 232; original stress)

This, however, can only be done if both the fixed factors and the final output were themselves homogeneous.<sup>11</sup> In general, Kaldor concludes, the 'degree of roundaboutness' is not a useful concept, even under static conditions. Once capital accumulation comes under consideration, the Austrian theory is 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, he would claim that it was, on the contrary, born at the LSE (King 1998, p. 428).

Kaldor's early contacts with Cambridge economists, and their influence over his thinking on macroeconomics, will be touched on in sections 4 and 5 below. 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,<sup>12</sup> is both complimentary and critical. She has neglected oligopolistic interdependence between producers, Kaldor complains, and has little to say about selling costs. 'Under the circumstances it is not surprising that her

book ... inevitably becomes a treatise on monopoly' (1934c, p. 335). Although he doubts whether monopsonistic exploitation of labour is still of practical significance, given the strength of the British trade unions, he praises 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 (1934b) and the relationship between market imperfection and excess capacity (1935), and in a protracted reply to Chamberlin's criticisms (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,<sup>13</sup> and they have little abiding interest for non-specialists.<sup>14</sup> 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 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*<sup>15</sup> in February 1934, the very same month in which the Dollfuss *putsch* destroyed democracy in Austria. Kaldor's theme is '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' (1934a, p. 122). He identifies three grounds for concern; in effect they are the now-familiar questions of existence, uniqueness and stability. Kaldor's treatment of the existence problem is both original and idiosyncratic.<sup>16</sup> He is concerned not with the properties of mathematical systems – his discussion is 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 is always a possibility, Kaldor notes, unless any move from one position of equilibrium to another is ‘immediate’ (p. 126). Where such a move requires 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)

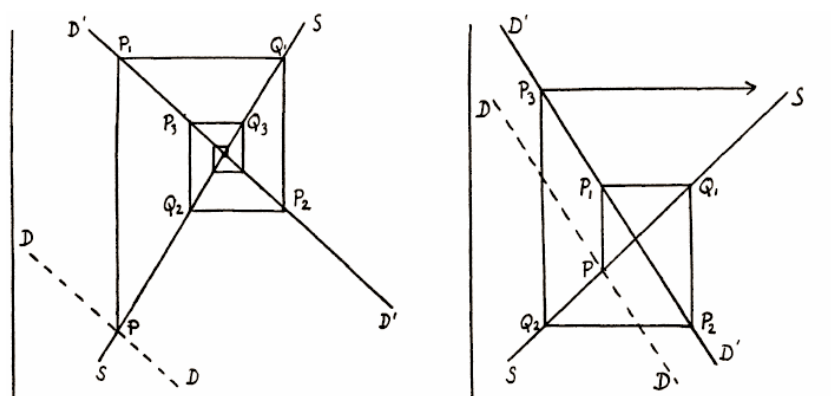
Here Kaldor cites a book by the Austrian theorist Hans Mayer and Appendix F of Alfred Marshall’s *Principles* (*ibid.*, p. 128 n. 1, n. 2), together with classic texts by William Stanley Jevons, Léon Walras and F. Y. Edgeworth and papers by his friend John Hicks in both English- and German-language journals.<sup>17</sup> Avoiding path-dependence, he continues, requires ‘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.<sup>18</sup>

Kaldor’s discussion of multiple equilibria includes the relatively familiar cases of ‘backward-rising’ supply curves and ‘backward-falling’ demand curves that had been recognised by Walras, Knut Wicksell, Marshall and Hicks (*ibid.*, pp. 129-30). Significantly, Kaldor points out that non-uniqueness is 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 are thus intimately connected.

The third problem is ‘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 draws upon published work (all in German) by his LSE colleague Paul Rosenstein-Rodan, by Henry Schultz and by Umberto Ricci, and also cites an Italian paper by Mauro Fasiani. He distinguishes 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). This is illustrated in Figure 1, which reproduces Kaldor’s own Figures 2 and 3.

**Figure 1**

Kaldor claimed to have originated the term ‘cobweb theorem’,<sup>19</sup> and this is confirmed by Pashigan (1987). The conclusions are in 2007 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 elasticities of supply and demand are the same, there will be a constant range of fluctuations. (Kaldor 1934a, p. 135)<sup>20</sup>

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

Kaldor was always proud of this paper (1960, p. 4), and justifiably so. Its most important contribution, however, is 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 was emphasised by Allyn Young in his LSE lectures (Blitch 1990; Sandilands 1990). But Kaldor brings out its microeconomic significance with exceptional clarity, even if he fails to recognise just how subversive of neoclassical economic theory it really is (see Kaldor 1972 for a belated acknowledgement of this point; cf. Harcourt 1988). He does 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 (1975, 1985; cf. Setterfield 1998). By this time, of course, 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 monetarists 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 and W. Brian Arthur (1994). All this was implicit in the work of the 25-year old Nicholas Kaldor.

### 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 interwar 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.<sup>21</sup> 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 Kaldor’s boss at the LSE, Lionel Robbins (1932, pp. 138-41), and it caused some unhappiness among his less conservative colleagues. The Fabian socialist Barbara Wootton, for example, made Robbins’ argument a centrepiece of her bitter *Lament for Economics*, published in 1938. It was not just a question of rich and poor: 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 interpersonal comparisons of utility, more than any of the other difficulties that she identified in contemporary economic theory, that led Wootton to abandon the discipline in favour of sociology, which seemed to offer a more coherent approach to social policy-making (King 2004). Hers was an extreme case, but Wootton was by no means the only left-leaning economist to be worried by Robbins’ 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 stultified ...’ (*ibid.*, p. 397).

Half a century later John Harsanyi dismissed Robbins’ 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)<sup>22</sup>

Those who agreed with Robbins, however, needed an alternative foundation for their policy prescriptions. A solution was, in fact, at hand. In 1894 and 1896-7 Vilfredo Pareto, writing in Italian and French (respectively), 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.<sup>23</sup> This principle of *Pareto optimality* (to use modern terminology) did not require interpersonal 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, in 2007 all this is known to every intermediate student of microeconomics; 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.<sup>24</sup> He is in 'entire agreement' with Robbins, Kaldor confirms, that interpersonal comparisons of utility are in principle impossible, but this does not entail that 'economics as a science' can say nothing about policy. Take a classic example: the repeal of the Corn Laws, which benefited capitalist and workers at the expense of the landlords.<sup>25</sup> This was evidently not a Pareto improvement, but it was a good thing nonetheless, 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 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 no-one 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)

It follows, Kaldor continues, that Pigou was 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 Jules 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,<sup>26</sup> Kaldor was the first economist writing in English to identify compensation as a general principle for judgements concerning social welfare. 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 1960, p. 5)<sup>27</sup>

By the end of the 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.

#### 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.<sup>28</sup>

The intention of the paper is clearly expressed in the title. Kaldor defines 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 price relatively to the ruling price and not a gain accruing through their use, or any kind of transaction effected in them or their transfer between different markets. (Kaldor 1980b, p. 17)

The term 'goods' is slightly misleading, as it soon becomes apparent that financial securities play a major part in Kaldor's story. In fact the 1939 paper constitutes an important – if generally unheralded – contribution to the theory of finance. It is very much in the heterodox tradition,<sup>29</sup> especially since, by 1940, Kaldor was convinced by his critics, Christopher Dow (1940) and Ralph Hawtrey (1940), that expectations cannot successfully be modelled in terms of a single representative agent and that analysis of the forward market requires at the very least a distinction between bulls and bears (Kaldor 1940d, p. 200).<sup>30</sup> It is also a critique of the *General Theory*, taking in not only Keynes' chapter 17 discussion of own-rates of interest but also extending to the theory of liquidity preference and the multiplier. Finally, Kaldor touches on the theory of money and the analysis of monetary policy, using arguments that are sometimes (contentiously) interpreted as foreshadowing his post-1970 Post Keynesian views on endogenous money.

Objects of speculation, Kaldor notes, 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 sets 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 draws heavily on Keynes' 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' ideas in the *Treatise* and in the *General Theory* (Kaldor 1960b, p. 4). There follows 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).

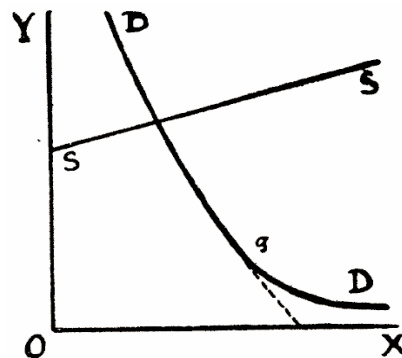
Writing in 1980, Kaldor described his article as an attempt

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 n. 2).

Figure 2



If Kaldor’s argument casts doubt on the theory of liquidity preference, it also has significant implications for the operation of monetary policy. These are brought out in section 4 of the article, which concludes that ‘we must expect the bank rate mechanism, as an instrument of economic policy, to become increasingly ineffectual’. While it is ‘still available for dealing with an occasional boom’, the 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. Figure 2 indicates a highly elastic money supply curve (though not a horizontal one), and Kaldor comments that:

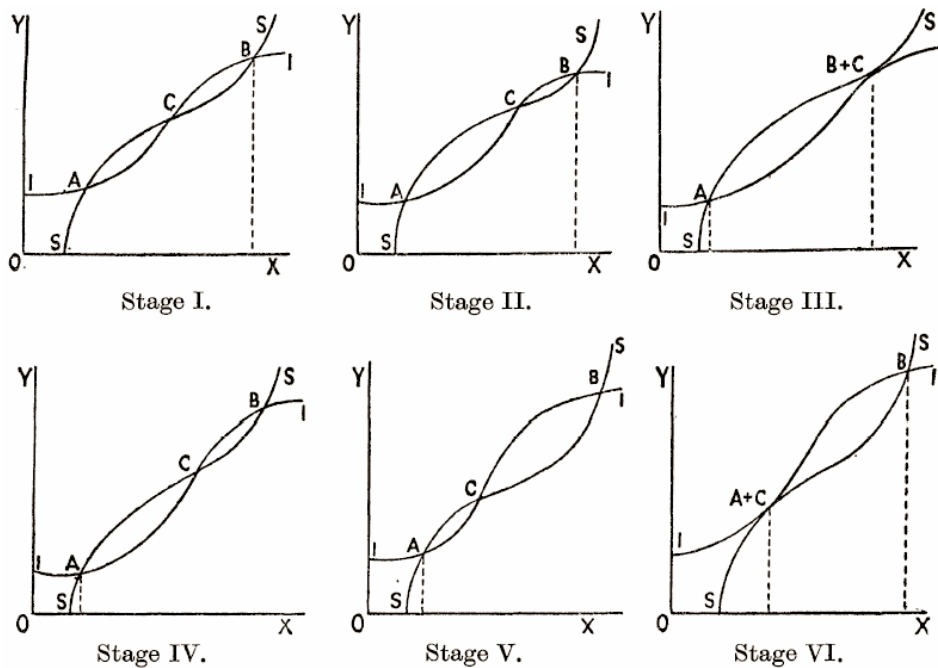
The elasticity of the supply of money in a modern banking system is ensured partly by the open market operations of the central bank, partly by the commercial banks not holding to a strict reserve ratio in the face of fluctuations in the demand for loans, and partly it is the consequence of the fact 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 n. 2)



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 (for example, Harrod 1936; Steindl 1937; Samuelson 1939). First, since the savings function is non-linear the marginal propensity to save, and thus also the multiplier, varies over the cycle. Second, investment depends not on the rate of change of income (or consumption) but on its level. A third difference from the Harrod model is that there is no trend; in 1940 (though not in subsequent work) Kaldor is happy to model the cycle in the context of a static and not a growing economy.

The mechanism is quite straightforward. In Figure 3 there are two stable equilibria (A and B) and one unstable equilibrium (C). 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. This is illustrated in Figure 4 (*ibid.*, Figure 6, p. 84). When activity is high, as at B (Stage I), the S function shifts upwards and the I function starts to fall, bringing B and C closer together (Stage II). When the two functions become tangential, B and C are identical and equilibrium becomes unstable downwards (since S exceeds I in both directions). In the ensuing slump, equilibrium is restored at A. The level of activity is now low (Stage III), leading the I function to shift upwards and the S function to fall. A and C now come closer together (Stages IV and V), gradually increasing the level of activity until A and C are identical and the economy becomes unstable upwards (Stage VI, where a boom occurs and equilibrium is reached at B). 'Thereafter the curves gradually return to the position shown in Stage I, and the cyclical movement is repeated' (*ibid.*, p. 85).

Figure 4



There are 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 (that is, 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 concludes that the forces acting to bring a slump to an end are 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 is set out without any mathematics; when the model was eventually subjected to formal analysis it proved to have serious weaknesses.<sup>32</sup> Apart from one brief reference to the difficulty of obtaining finance as a constraint on investment in a boom there is also no monetary or financial component; it is as if 'Speculation and Economic Stability' had been written by someone else altogether.<sup>33</sup>

What are the prospects for controlling the cycle through macroeconomic policy? Keynes 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 takes 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 suggests, 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 at its greatest; (Kaldor 1940c, p. 88). On balance Kaldor is 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 Michal 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, John 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 n. 2)

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 long ago have discarded’ (Kaldor 1951, p. 837). The capital-output ratio is not a technical constant, he now maintained, but an economic variable, which depends on the financial resources of firms and on entrepreneurial expectations (*ibid.*, pp. 839-41).<sup>34</sup> Thus a non-linear relationship between investment and income, as in the 1940 Kaldor model, is much more plausible than the constant accelerator coefficient used by Hicks.

The second half of Kaldor’s review is 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’ *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’ 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).<sup>35</sup>

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 notes that a non-linear investment function was used by both Hicks and Richard Goodwin (1951) in their own cycle models. Kaldor reasserts his objections to the accelerator principle and acknowledges that Goodwin, at least, 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 1980, p. xvi). This seems an entirely reasonable assessment.

## 6 Conclusion

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 London School of Economics and turning away from theory to concentrate on applied economics and issues of policy. The shift is already apparent in his short but perceptive analysis of war finance, published in August 1939 in *The Banker* (1939c). 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.

For all its strength, Kaldor’s early work also points to a general and continuing weakness in his work as a theorist: an unwillingness, or perhaps an inability, to integrate his various insights into a coherent and systematic system. His failure to integrate his ideas on money and finance with his trade cycle theory has already been noted, and it is just one example of his inconsistency. 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 1998, p. 416). Kaldor would have replied 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, Kaldor’s grasshopper mind was a source of strength; it allowed him to range over the whole of economics, being original, provocative and invariably interesting.

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## Notes

1 Biographical details can be found in Pasinetti (1986), Thirlwall (1987) and Targetti (1992), and autobiographical material in Kaldor (1980, 1986a, 1986b).

2 On Young, see Blicht (1995) and Kaldor’s notes from his 1928-9 lectures at the LSE (Blicht 1990; Sandilands 1990).

3 See Thirlwall (1987, chapter 1). 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’.

4 In later life Kaldor was a tireless writer of letters to *The Times*: ‘several hundreds’ of them altogether (Kaldor 1980, p. viii).

5 This was the English version of Hayek’s *Monetary Theory and the Trade Cycle*, translated by Kaldor and Honor Croome (Hayek 1933).

6 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 (1932d, p. 481).

7 ‘There is a deep underlying wisdom in the vagueness of Marshallian economics which seems to escape altogether the precision of the mathematical mind’ (1936a, p. 97).

8 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.

9 See especially 1932a, p. 29. But Kaldor never returned to this topic, and seems always to have been unaware of, or uninterested in, Minsky’s ideas.

10 But see Cohen (2006) for a very different assessment of this article.

11 As an anonymous referee has pointed out, all this is ‘straight Marshall’.

12 For details of their first meeting, in April 1933, see King (1998, p. 412).

13 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 1998, p. 412).

14 See however Targetti and Thirlwall (1989, pp. 2-3), who take a much more favourable view of Kaldor’s writings in this area.

15 See Thirlwall (1987, p. 29) for details of Kaldor’s involvement with the *Review of Economic Studies*, a journal which (as an anonymous referee has reminded me) formed an important bridge between the young economists in Cambridge and at the LSE.

16 The term is admittedly anachronistic. Kaldor himself does 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).

17 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. (I owe this point to an anonymous referee.)

18 Again Kaldor himself does not use this term, but it is transparent in what he does explicitly argue.

19 The name ‘occurred to me in the course of an oral exposition of that theorem at the L. S. E. seminar’ (Kaldor 1960, p. 4). On Robbins’s celebrated graduate seminar, see Kaldor (1986b, pp. 38-9); Galbraith (1981, p. 78); and McCormick (1992, p. 30).

20 Note that Kaldor also makes the standard undergraduate error by confusing elasticity with slope!

21 These qualifications were later formalised by John Rawls (1971) in his *maximin* principle.

22 It might be supposed that there is a substantial philosophical literature on the possibility of making interpersonal 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).

23 The relevant passage has been translated by John Chipman (1976 [1999], 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.)

24 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).

25 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, p. 526; cf. Kaldor 1940f). Kaldor continued to claim, correctly, that the pre-1846 tariff had been the least advantageous of the three alternatives (Kaldor 1960, pp. 5-6).

26 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).

27 See Graaf (1957, pp. 84-90) for a full discussion of this literature.

28 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).

29 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).

30 Hawtrey complained that Kaldor still had not taken this point seriously enough (Hawtrey 1940, p. 205).

31 I owe this important point to Geoff Harcourt.

32 The necessary and sufficient conditions specified by Kaldor are defective, and the persistence of cycles depends crucially on parameter values, the speed of adjustment, initial disturbances and the positions of the S and I functions (Chang and Smyth 1971). In his reply, 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 does not hold, no Keynesian equilibrium is possible (Kaldor 1971, p. 45).

33 This was a common feature of the trade cycle models of the 1940s and early 1950s, as Hyman Minsky (1957) complained.

34 '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 1951, p. 839). Kaldor links this point to Kalecki's 'principle of

increasing risk' (*ibid.*, p. 841), and in 1951 stresses his affinities with Kalecki to a much greater extent than he had done in 1940.

35 This is, 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).

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