

Minsky's 'induced investment and business cycles'

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This paper reviews the recently published PhD thesis of Hyman P. Minsky, summarising its main contributions to methodology and microeconomics. These are aspects of economics with which Minsky is not usually associated, but which lie at the foundation of his later work. They include critical remarks on Cambridge economics. The paper then draws out some antecedents of Minsky's ideas in the work of Henry Simons, and highlights the Marshallian monetary analysis that he adopted. It is argued that this analysis is incompatible with the Kaleckian theory of profits that Minsky was later to adopt.

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Apart from its significance for the history of economic thought, the publication of Hyman P. Minsky's PhD dissertation, *Induced Investment and Business Cycles* (Minsky, 1954/2004) is a major contribution to contemporary economics, and something of a coup for its publishers, Edward Elgar. Few theoreticians have been able to transcend, as Minsky did, the incidental aspects of economic activity that inspire such prolix discussion in academic journals today, to grasp at the fundamental relationships of the modern capitalist economy, namely those relationships that determine its character and dynamics.

Hyman Minsky wrote his thesis at Harvard University, under the supervision of Joseph Schumpeter. On the death of Schumpeter in 1950, supervision was taken over by Wassily Leontief. Minsky initially proposed to investigate the relationship of market structure and banking with the business cycle. The actual thesis that emerged may be variously seen as an extension of Schumpeter's monumental *Business Cycles* or, alternatively, a precursor of Minsky's later financial instability hypothesis. In fact, perhaps the most intriguing feature of the thesis is the absence of those elements with which we most associate Minsky today—endogenous financial fragility induced by the speculative financial positions that firms are obliged to take in the face of uncertain future returns—and the presence in the thesis of some unexpected elements that we do not really associate

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with his work. In particular, the reader is struck by the extent of Minsky's serious reflections on microeconomics (somewhat unexpected to those who regard his theory as a macroeconomic theory of finance), and his considerations of economic methodology. With the exception of Vercelli (2001), there is little published discussion of Minsky's methodology, and even less on his microeconomics in the three volumes of essays on Minsky that have appeared in recent years (Bellofiore and Ferri, 2001; Fazzari and Papadimitriou, 1992). The section that follows expands on the methodological aspects of Minsky's thesis. This is followed by a section on the microeconomic theory that Minsky propounds in his thesis. A third section considers the third influence, unacknowledged in this thesis, on Minsky (after Schumpeter and, perhaps, Leontief), namely his Chicago teacher Henry Simons. It is argued that Simons is the key link between Minsky and the views on the monetary business cycle of Hawtrey and Keynes. A fourth section looks at how Minsky's monetary analysis, in this thesis and subsequently, differs fundamentally from that of his mentor Schumpeter, and the consequences that this difference has for the consistency of some of Minsky's later analysis.

1. Methodology: complexity and empiricism

Minsky's methodology is an aspect of his work that he did not trouble himself to explain in detail in his publications, despite criticism that a later, Kaleckian, feature of his theory relied rather too much on national income identities (Goldsmith, 1982). In fact there is very little modern economic theory that does not rely on tautology, which, if applied with imagination and understanding, can be quite illuminating. The methodology of Minsky's later published work is insightfully discussed and compared with Keynes's methodology in Alessandro Vercelli's paper as part of a comprehensive discussion of Minsky's work in two volumes of essays edited by Piero Ferri and Riccardo Bellofiore (Vercelli, 2001, in Bellofiore and Ferri, 2001). In his thesis, and undoubtedly under the influence of Schumpeter, Minsky appears to have considered economic methodology and expressed conclusions that may be seen as the foundation of some of his later work.

Minsky introduces methodology right at the very start of his thesis. The starting point in this work is a critique of business cycle theory, which takes up the short first chapter, entitled 'The analysis of business cycles: the problem and the approach', and the chapter that follows 'Some accelerator-multiplier models'. Extraordinarily, Minsky appears to reject Schumpeter's view in *Business Cycles* that the business cycle is either a purely statistical phenomenon à la Slutsky, or else is caused by industrial innovation (Schumpeter, 1939). Instead, Minsky argues that 'a theory of business cycles, to be consistent with the observable material and the inherited doctrines, should be a blend of the analytical material which deal with the interrelations between a few broad aggregates—which traditionally has been the approach of monetary theory—and the analytical material which deals with the behavior of individual economic units and of particular markets—which has been the sphere of price and distribution theory.' Such 'an eclectic business cycle theory' he proposes to construct using 'a number of elements drawn from inherited economic analysis.' (Minsky, 1954/2004, p. 1). This notion, that advances in economic theory are implicit in 'inherited economic analysis', is deeply Schumpeterian. One recognises the intellectual presupposition behind the *History of Economic Analysis* whereby the economic theory of the past, taken together and with the error sifted out of it, amounts to the economic theory of the present. (One may also wonder whether hiding his ideas behind 'inherited economic analysis' would have got Minsky, or anyone else, a PhD degree in an

Anglo-Saxon university today, where the originality of even commonplace opinion must be rather more stridently proclaimed.)

Minsky examines the Hansen–Samuelson (multiplier–accelerator) model, a Hicksian (floors–ceiling) model, the Goodwin model, a non-linear model similar to Hicks', a stochastic error model and a stochastic coefficient model. He concludes that the stochastic coefficient non-linear accelerator model, which is an accelerator model with variable, non-linear parameters, is 'consistent with the observed irregularity and non-symmetry of business cycle experience'. An econometric theoretician today might therefore have concluded that his mathematical model was empirically, and promptly sent it to a reputable journal for publication. Minsky argued that such empirical verification merely makes such a model 'a worthy hypothesis for further examination'. He did not examine any actual statistical data, but relied on more implicit empirical verification. He merely asserted with reasonable plausibility the evolutionary character of business cycles, i.e., that business cycles change, so that each successive one is different from its predecessors: 'Inherent in the nature of the business cycle is the statement that the relevant parameters for the firm's behavior do not remain the same over the cycle' (Minsky, 1954/2004, p. 71). In particular, he argued, 'an effort must be made to see whether the theory of the firm and the theory of financial and monetary behavior will lead to a selection of the nature of the non-linearity of the accelerator generating relation' (Minsky, 1954/2004, p. 60). In his view Goodwin's models in particular suffer from an absence of foundation in 'the behavior of business firms' (Minsky, 1954/2004, p.37). Hicks too is criticised for propounding a model without a theory of market process (Minsky, 1954/2004, pp. 2–3).

The next chapter is then a critique of those accelerator theories that posit what he regards as a 'mechanical relationship' between investment and output or sales. Minsky's critique is crucial for the prospects of empirical verification. As mentioned above, Minsky does not present any empirical data but seems content, like Veblen, whose work Minsky knew but does not cite in his thesis, to assume that the relevant facts are well known. [Veblen (1923) states: '...the essay... makes no use of recondite information and makes no attempt to penetrate beyond the workday facts which are already familiar to students of these matters.'] This is because fitting a standard stylised relationship to aggregate data, and treating deviations from values predicted by that relationship as insignificant residuals (or even reducing residuals from postulated statistical relationships to insignificance) was, in his view, a wrong procedure. The theory had to be able to explain the known historical irregularity of cycles. Hence, according to him, models that do not allow for systematic variation in the accelerator relationship are incomplete.

Minsky's critique anticipates and answers a criticism that Tobin advanced of Minsky's later financial instability hypothesis. In a review of *Stabilizing an Unstable Economy*, Tobin argued that, without a formal model, 'readers cannot judge whether an undamped endogenous cycle follows from the assumptions or not' (Tobin, 1989; Hart, 1992 examines this and other criticisms of Minsky). Tobin's criticism is, in fact, beside the point. As every student of business cycle was taught, and Minsky points out, whether a model gives damped or explosive cycles depends on the *parameters* of the model, rather than its underlying, and unverifiable, assumptions (Minsky, 1954/2004, pp. 22–23). What might be called the 'formalisation imperative' of Tobin is in fact a test of the parameters of a model, rather than its assumptions. Thus, more than one model may have its parameters so calibrated as to obtain a good fit with aggregate data. More profound insight, according to Minsky, is obtained by showing consistency with microeconomic assumptions. In turn,

for Minsky, those micro-economic assumptions concern the way in which firms manage their balance sheets (see section 2 below). (The later campaign for 'microeconomic foundations' of New Classical and New Keynesian macroeconomics ended up offering the somewhat meagre foundations of a household theory of the firm and the rational choices of economic 'agents'. For Minsky, the essence of capitalism was its domination by firms rather than households.)

The difficulty with the accelerator relationship, in Minsky's view, is that it is an ambiguous relationship at a high level of aggregation. This makes it difficult to make meaningful inferences from the theory. It is ambiguous because the accelerator can have at least three different meanings. It can be merely a structural parameter linking statistical aggregates, for example, the change in national income and actual fixed capital formation in a given period. It can be a coefficient of 'induced investment', i.e., the amount of additional investment induced, but not necessarily realised, by a change in output or income. This might be contingent or intended investment. Thirdly, it can be a coefficient of realised investment, i.e., the amount of investment that regularly coincides with a given change in output or income, or the amount of investment that, on average, is statistically associated with a change in average income.

From the very beginning Minsky criticised attempts to clarify these ambiguities at an aggregative level. In his view, there are only two ways in which such conceptual issues can be resolved effectively. The first is by a process of complex disaggregation, which leads to 'mathematical complexity'. This can be overcome by adding supplementary sets of relationships to a 'core model'. These supplementary relationships determine the parameters of the core model. He identified this kind of disaggregation procedure with the work of Tinbergen (Minsky, 1954/2004, pp. 6–7). Core-supplementary model disaggregation has recently returned to favour with economic model-builders, for example, in the new Bank of England macroeconomic models. However, these reverse the direction of parameter determination, having the core model determining the parameters for more detailed supplementary models (Bank of England, 2004; this obviously has serious methodological implications. Minsky's core-supplementary model determination is designed to give his theory microeconomic foundations in market process, whereas the Bank of England's models preclude such foundations, because the core model is used to determine the aggregate variables that are then the foundation for supplementary models). The alternative was 'a retreat to empiricism'. It is evident from the absence of empirical statistics that Minsky was not going to attempt this latter course of action, and it is doubtful whether his supervisors, Schumpeter and Leontief, would have encouraged it.

2. Microeconomic foundations

The bulk of Minsky's thesis turns out to be an extended examination of the consequences of financial liabilities on the investment behaviour of firms. In this respect, he clearly adhered to the common view of the business cycle that emerged from the 1930s depression, attributing macroeconomic fluctuations to extreme movements in business investment. However, for Minsky investment activity could not be separated from market process, and this in turn was determined by market structure. In particular, the accelerator theory tends to break down under monopoly. Firms with market power, operating with excess capacity, have a weak output response to changes in demand for their products. But they may respond more strongly if the change in demand is greater than a certain amount, or if the change is sustained. Shifts in the demand curve therefore cause discontinuities of

response by firms. A weaker accelerator relationship becomes a function of financing costs. These in turn are summarised in 'planning curves', long-run marginal cost curves which firms use to determine the scale of production, and hence their investment (Minsky, 1954/2004, pp. 118–9).

Minsky's treatment of monopoly itself is worthy of note, because of his rejection of the imperfect competition analysis that emerged in the 1930s from the work of Chamberlin and Joan Robinson. Minsky criticised these theories for retaining the traditional presumption that firms in perfect competition maximise profits. Yet, he argued 'if the necessity for profit maximization follows from the structure of the perfectly competitive market, then the analysis of the behavior of a firm selling in a market that is not perfectly competitive cannot be based upon the assumption that a firm necessarily maximizes profits.' (Minsky, 1954/2004, p. 92). In a later footnote he criticised the adoption of profit-maximisation as a means of obtaining tractable equations:

A reason which can be advanced for the ready acceptance by economists of profit-maximizing behavior, independently of the changes in the market structure which are introduced in their analysis, is that profit-maximizing behavior leads naturally to mathematics in which derivatives of the difference between total cost and total revenue are set equal to zero. In this sense, under profit-maximization the behavior of the competitive and non-competitive firm are formally identical—the mathematical set-up is the same. The complexity added by non-competitive firms is resolved by the introduction of the demand elasticities confronting the firm at appropriate places in the analysis. In general equilibrium analysis, the existence of monopoly does not lead to any adjustment in the equilibrium relations if profit-maximizing is assumed; rather the effect of different degrees of monopoly is in the distribution of income and the allocation of resources. . . The passing over of the 'rationale' for profit-maximizing in much of the analysis of monopoly can be imputed to the substitution of a tool of analysis for the problem.' (Minsky, 1954/2004, p. 100, note 20).

Minsky's argument here is a particular example of a more general methodological criticism that his second dissertation supervisor, Wassily Leontief, had earlier levelled against the school of economics that Alfred Marshall had established at Cambridge, UK. In a paper published in 1937, well before Minsky had commenced his economics studies, Leontief had accused the 'neo-Cambridge School' of 'implicit theorizing'. By this he meant a process of introducing concepts without deriving them from clear and unambiguous axioms. In this way criticisms of error can be evaded by a succession of subtle shifts in the meaning of particular terms, because statements that cut out the logical steps by which they are derived from fundamental propositions, can be restated as fundamental postulates, which are then impervious to logical criticism (Leontief, 1937; it is clear from his paper that Leontief, at that time at least, thought of theory as merely the logical development of axioms and the incorporation in it of statistical data). Keynes (for his use of the wage unit as a standard of quantity), Hicks (for his use of the term elasticity of substitution in his wage theory) and Kahn (for his notion of the marginal utility of money) are all accused of 'implicit theorizing'. In Leontief's view, Keynes was especially prodigal ('an embarrassment of plenty') in his output of implicit theories. Among those 'logical mistakes' are perhaps the two most lasting concepts that Keynes bequeathed to economics, aggregate supply and aggregate demand:

Mr. Keynes's equations of aggregate supply and aggregate demand are removed from a great number of steps from any basic assumption and data. Even so Mr. Keynes himself would hardly deny the obvious observation that both functions depend upon an identical set of primary data, that is, they are fundamentally interdependent.' (Leontief, 1937)

In the case of Joan Robinson, Leontief held up her use of ‘corrected units’ in her *Economics of Imperfect Competition* (Robinson, 1933, p. 332: the corrected unit is the physical quantity of any factor of production that, added to any quantity already employed, would increase output by the same fixed amount as some standard unit of any factor of production). Leontief pointed out that this ‘simplification’ makes marginal products of factors constant, even though increasing quantities of them may be added. He also noted that Joan Robinson admitted the illegitimacy of her procedure very soon after the publication of her book.

Leontief’s paper is not listed in Minsky’s bibliography. Indeed, the only reference to his second supervisor’s work concerns the more technical issue of disaggregating aggregates in the discussion summarised in the previous section. But Minsky had found an even better example for Leontief, than Keynes’s polemical gyrations, in Joan Robinson’s application to non-competitive situations of profit-maximising behaviour that requires perfect competition for its enforcement.

Apparently unaware of Leontief’s reservations concerning Cambridge economics, Minsky reached back into that well of ideas for an approach that is most unexpected, in view of his background and later developments and interpretations of his work. This is the notion of ‘conditional monopoly’ that Marshall put forward in his book *Industry and Trade*. Marshall had recognised that, apart from natural or legal monopolies, such as land-ownership, firms with a dominant position in their respective markets do not usually plunder their customers for the maximum profit that they can secure, if only because such high profits will attract other firms into the market. Marshall therefore put forward his own combination of what we would now call ‘satisficing’ and limit pricing. In other words, monopolies can only remain so on condition that they do not maximise profits, but hold their prices low enough to discourage *potential* competition (Marshall, 1919, Book III chapter 1). The theory of ‘conditional monopoly’ is more commonly known today as that of ‘contestable markets’.

There follow five chapters, the vast bulk of Minsky’s thesis, that are taken up with the discussion of how firms ‘survive’ under different financing and market conditions, under threat of ‘vulnerability’ to a fall in demand for their products or an increase in competition in their market. The originality of this part of the thesis lies in the way in which Minsky incorporated financing costs into various cost curves so that the traditional Marshallian apparatus of short-, medium- and long-term cost curves is made more complex by the financing commitments that firms must enter if they are to undertake investment. Minsky argued that ‘the usual economic theory ignores financing problems and assumes a unique behavior principle for all firms (profit maximization), leaving only the trivial problem of the choice of the product to be produced by the firm... We will treat the problem of the financing technique to be used by a firm as the problem of balance sheet structure’ (Minsky, 1954/2004). In this way Minsky introduced the theory of the firm that was later to be the core of his analysis of financial fragility, i.e., the notion of the firm as a balance sheet of assets and liabilities, as opposed to the notion of the firm as an entrepreneur making production decisions that is the foundation of textbook economics of the firm. In his thesis, Minsky placed the balance sheet in the background of a Marshallian apparatus of cost and revenue curves. In his later work he shifted his attention to the balance sheet operations of firms, as opposed merely to the revenue and cost flows from given balance sheets.

There is a small difficulty with this kind of theory of the firm in that it does not distinguish between plants (i.e., factory or production facilities) and accounting firms. Issues like the scale of production are clearly decisions implemented, even though they may

be made elsewhere, at plant level. A balance sheet, however, is managed by a firm that may have more than one plant in operation. Or, to put it another way, every firm has a balance sheet, but not every plant has one that can be independently operated by the managers of the plant. If the manipulation of balance sheets is the essence of modern corporate management, as Minsky's later work suggests, then cost and revenue *curves*, as opposed to actual costs and revenues that form the basis of industrial asset valuations, require more careful justification. Covering up the gap in economists' knowledge of firm behavior by adopting a profit-maximisation principle of firm behaviour is wrong because it confuses positive with normative economics. However, suggesting that cost and revenue curves may be obtained by imputing balance sheet data into plant costs and revenues is illogical. If it is assumed that production decisions are made at the plant level independently of decisions being made in other factories, then this clearly cannot happen among the factories of a multi-plant firm. In this case, the cost and revenue curves of each factory are interdependent. Alternatively a corporation acting as one accounting unit may make plant level production decisions. But in that case, aggregated corporation cost and revenue curves cannot be independent of the allocation of production among different plants.

This inconsistency in Minsky's work is only apparent in his doctoral dissertation. He developed his balance sheet analysis in his extended essay published in 1964, 'Financial Crisis, Financial Systems and the Performance of the Economy' (Minsky, 1964). Here he expounded the balance sheet analysis of the firm over 14 pages. But this time, he dropped the 'planning curve' analysis that took up so much of his dissertation.

Minsky's use of Marshall is also surprisingly selective. Marshall discussed in his book not only questions of monopoly, but also those of company finance (Marshall, 1919, Book II, chapter IX). Here he raises an issue that had emerged in the early years of the twentieth century of the over-capitalisation of companies. Similar discussions of this subject may be found in the work of Lavington and Hobson, in the UK, and in the USA in Veblen's classic *Theory of Business Enterprise*. Excess capital seems a very natural foundation for Minsky's later analysis of fragility in firms' balance sheets. Yet Minsky does not appear to have picked up this discussion. The reason may be that at the time when he was writing his thesis, the US capital market was still constrained by the fall-out from the 1929 crash, and firms were therefore financing expansion by bank credit and internal finance.

This is perhaps largely of historical interest. Seven years after Minsky completed his thesis, Merton Miller and Franco Modigliani published their mathematical proof that the value of a firm is independent of its financing structure or, more strictly, that firm balance sheets reflect 'market imperfections'. In this way the Miller-Modigliani hypothesis 'substituted a tool of analysis for the problem', taking the question of balance sheet financing out of mainstream economics and redirecting the discussion of such financing into a hunt for market imperfections.

3. Simons and Chicago

Antecedents of Minsky's thought in the work of John Maynard Keynes are well known. Here it is worth mentioning that Keynes gets relatively few references in this thesis, and they are mainly the predictable ones: Keynes on uncertainty, consumption, liquidity preference and the liquidity trap. All of these references are to the *General Theory*, and none show the insights into Keynes' work that were to be developed in Minsky's *John Maynard Keynes* (1975). Another later influence, the Polish business cycle theorist Michał Kalecki, has his Principle of Increasing Risk correctly cited as a theory of the size of firms. However,

Kalecki's internal finance constraint on investment, which was to feature greatly in Minsky's analysis after he returned in 1970 from his year in Cambridge, UK, is mistakenly attributed to the monetary business theorist Ralph Hawtrey following a citation from the econometrician Sho-Chieh Tsiang (Minsky, 1954/2004, p. 72). Schumpeter's influence is discussed in sections one and four of this paper. However, the economist who arguably directed Minsky towards his consideration of macroeconomic financial disturbance, the Chicago liberal Henry Simons, is not mentioned in this book at all.

This is a most surprising omission. Simons had taught Minsky at Chicago. In his later memoir of his Chicago years, published in the *Banca Nazionale del Lavoro* in 1986, Minsky recalled the very personal relationship that he had with Simons. It was Simons who introduced Minsky to the idea that the financial system in the USA was structurally flawed and explained how it had contributed to the Great Depression, without resorting to tales about incorrect monetary policy, or imbalances between saving and investment. When Minsky finished his military service in 1946, he was offered a generous fellowship to return to Chicago, but turned it down for a less lucrative studentship at Harvard. His reason was that the three economists whom he most admired at Chicago were no longer there: Viner had gone to Princeton; Lange, whose socialist commitment had inspired Minsky to study economics had, to Minsky's disgust, thrown in his lot with the Polish Communists; and Simons was dead (Minsky, 1988). Simons, who was prone to melancholy, had committed suicide in despair at the onset of Keynesianism. Six years before he wrote his memoir, in his Preface to his 1982 volume of essays, Minsky had mentioned Simons even ahead of Lange and Schumpeter, as an influence: 'As a student, I was most influenced by Henry C. Simons, Oscar Lange, and Josef Schumpeter' (Minsky, 1982, p. 5).

Simons deserves some brief consideration here not only because Minsky appears so inexplicably to have omitted him from his thesis. Such consideration is further justified because many of those who have heard of him today know him from the very partial account of his work given by Milton Friedman. (Moreover, due to pressure to complete my own *Theories of Financial Disturbance* Simons was, unfortunately, omitted from that book.)

Hayek was later to suggest that Simons shared Hawtrey's views on the monetary business cycle (see below). But, whereas Hawtrey stressed the natural instability of credit as a factor in business cycles, Simons argued that the structure of the financial system was a key factor in exacerbating disequilibrium in the non-financial sector of the economy. In his classic article 'Rules versus authorities in monetary policy' published in the *Journal of Political Economy* in 1936, the late-twentieth century discussion on central bank independence is turned on its head. Sensible to the financial debauchery and collapse of the first four decades of the twentieth century in the USA, Simons was a strong critic of the kind of financial entrepreneurship that Minsky later also criticised. Simons believed that such entrepreneurship was the result of liberal banking policies that encouraged excessive credit and discouraged investment by requiring business to keep liquidity tied up against a possible inability to roll over short-term loans. Simons concluded that financial intermediation needs to be subject to strict rules, and that the fiscal authorities need to have discretion over monetary policy in order to be able to regulate credit. This discretion had to be with the fiscal authorities because their open market operations determine the reserves of the banking system (Simons, 1936). Simons had even argued for the abolition of central banking, because he believed that its functions are more effectively carried out by government treasuries. The elimination of central banking also followed from his adherence to the doctrine of full reserve banking. If banks are obliged to hold the equivalent of all their deposits as reserves, then there is clearly no need for provision of

reserves by a central bank. (The discussion around this is perceptively examined by Ronnie J. Phillips in a book, *The Chicago Plan and New Deal Banking Reform*, to which Minsky wrote a Preface; Phillips, 1995.)

After the publication of his monetary history of the USA, Milton Friedman gave the critical reassessment of Simons referred to above. Friedman argued that Simons had failed to realise the disastrous consequences of the contraction of bank credit in 1930–3, which Friedman revealed in his history. In fact, Simons could not have been unaware of the contraction: Irving Fisher had been arguing much the same around 1933 and both Fisher and Simons were involved in the discussions around the reform of the Federal Reserve System to stabilise the faltering US banking system (Phillips, 1995, chapters 3 and 4). However, Friedman drew a conclusion that was directly contrary to that of Simons. In Friedman's view, consistently argued since 1948, it was the monetary authorities that had to be bound by rules on credit expansion, because the relationship between reserves and credit is essentially stable (Friedman, 1967). It goes almost without saying that, in the monetarist analysis, the relationship between financial intermediation and the real economy is essentially benign and speculation results from loose monetary policy rather than loose banking. Friedman's claim, that these doctrines were part of the 'oral tradition' of Chicago, had already drawn Patinkin's famous defence of a broader tradition at Chicago (Patinkin, 1961).

Simons was therefore the missing link between Hawtrey and Minsky. Hayek hinted at this in criticising Friedman's suggestion that the Great Depression predisposed both Keynes and Simons to fiscal activism. Simons' fiscal schemes were explicitly designed to regulate the liquidity of the financial system, rather than regulating aggregate demand (Simons, 1942). Hayek wrote to Friedman:

I believe you are wrong in suggesting that the common element in the doctrines of Simons and Keynes was the influence of the Great Depression. We all held similar ideas in the 1920s. They had been most fully elaborated by R.G. Hawtrey who was all the time talking about the 'inherent instability of credit' but he was by no means the only one. . . (Friedman, 1967, p. 88).

Minsky, like Patinkin, objected to this narrow interpretation of the Chicago tradition. In a 1969 paper in the *Journal of Finance* Minsky contrasted Simons' view that the '...depression-proof good financial society requires the radical restructuring of the financial system...' with Friedman's view that 'the establishment of the good financial society requires only the adoption of a stable money growth rule by the Federal Reserve System, given that the reform represented by the introduction of deposit insurance had already taken place. . . Simons had a financial system rather than a narrow monetary view of the "Banking" problem.' (Minsky, 1982, pp. 279 and 289).

4. Monetary analysis

Finally there is Minsky's monetary analysis. This takes up the last chapter of Minsky's thesis, just over one eighth of the whole work. In it he argues the dependence of the investment accelerator on accommodating banking and monetary policy. This too is essentially Marshallian and Keynesian, arguing that the central bank decides the supply of money, demand for which is determined by Keynes's liquidity preference. Minsky later published his analysis of the interaction between the accelerator principle and monetary policy in 1957, in the *American Economic Review* (Minsky, 1957). As it originally appears in Minsky's thesis, it is not quite Keynesian, as Minsky never was, because in one respect it

goes far beyond Keynes and Marshall. This is in considering the effects of banks' purchases of securities. Minsky suggested that, by making markets for company securities more liquid in this way, banks could make companies more liquid and facilitate investment, *without even advancing them credit directly*:

...bank purchase of securities ... affects business firms only through affecting the liquidity of firms and households... The open market model of bank operations involves a substitution of one asset, bank money, for another asset, bonds in the portfolios of households and firms. There is no immediate and direct impact upon investment. Any effect which such operations have upon investment depends upon the reaction of business firms to the improved liquidity and perhaps lower borrowing rates that follow. In such a world the liquidity preference relation, which is a shorthand for the substitution relation between money and other assets, becomes the appropriate tool to use in the analysis of the behavior of the monetary system.' (Minsky, 1954/2004, pp. 232–3)

By this credit inflation banks make capital markets more liquid.

Minsky's use of the Keynesian liquidity preference relationship is understandable, in view of the pre-eminence of Keynesian monetary theory at the time when he was writing. His later work on Keynes (Minsky, 1975) indicated the theoretical affinity for English monetary analysis that Minsky continued to develop through his life after Chicago. In one respect, however, it proved to be incompatible with his macroeconomics.

In his earliest writings, Minsky argued that monetary policy or a deficiency of bank reserves (an unacknowledged borrowing from Hawtrey) would provide a ceiling to an economic boom, just as fiscal policy would prevent an indefinite fall in production. He combined this with an essentially Fisherian view of financial crisis in which exogenous 'displacements' set off speculative booms and break them. Such displacements can be technological innovations that alter profit expectations, but can also be induced by monetary policy changes, low interest rates encouraging speculation or high interest rates draining speculative credit (Fisher, 1933). After he had finished his study of Keynes in the first half of the 1970s, Minsky adopted Kalecki's profits theory, in which profits are determined by capitalists' expenditure, principally on investment. This theory allowed Minsky to make financial fragility endogenous, because the profits theory shows how the cash flow of the corporate sector declines as investment falls off after the investment boom peaks. The reduced cash flow then makes it more difficult for firms to settle their financial commitments, thus causing a crisis of over-indebtedness. The theory appeared in 1978 as Minsky's 'financial instability hypothesis' (Minsky, 1978).

Minsky's adoption of Kalecki's profits theory is problematic because it ignores crucial monetary and credit aspects of that theory. Minsky continued throughout his publications to insist that an investment boom must entail rising company indebtedness. (In his 1957 paper in the *American Economic Review* Minsky argued that rising indebtedness could be offset by deficit financing in the public sector.) But Kalecki's profits theory shows how expenditure on investment adds to the net cash flow of the corporate sector: investment expenditure, even if financed by credit, is received as income by capital goods-producing firms that, on delivery of the goods, have no further financial or business liabilities arising out of the transaction. Thus, even if rising investment entails rising indebtedness, it also entails rising liquidity and bank deposits held by companies. The corporate sector balance sheet expands on both asset and liability sides, with the asset side becoming more, not less, liquid as debt-financed investment proceeds.

The source of the incompatibility between Minsky's Keynesian/Marshallian monetary theory and Kalecki's profits theory is essentially a fallacy of composition. The

Keynesian/Marshallian theory implicitly supposes that firms enter the market for credit to finance investment all at the same time. In this sense, the theory is based on a notion of a representative firm. However, the monetary analysis implicit in Kalecki's profits theory is based on firms differentiated by their market power and engaging in continuous production. Thus, an investment boom, whether financed from credit or companies' own reserves, distributes liquidity around companies. Minsky, like Kalecki, did not presuppose that firms start off with no money or credit. Minsky accepted that internal finance is the foundation of investment within 'the margin of safety' (Minsky, 1986, pp. 188–92). But he overlooked the addition that investment makes to internal finance through profits. The crucial determinant of financial fragility is the distribution of that liquidity. Kalecki argued that this was in accordance with firms' respective 'degree of monopoly' in their markets. He suggested, following Hilferding, that the economy could be divided up into a competitive sector, and a monopolistic one. Competition reduces the profits of competing firms. Hence, profits tend to accumulate in the more monopolised industries (Kalecki, 1968). Financial fragility arises in the corporate sector because those firms that are getting into debt in order to invest may not be accumulating the profits and liquidity that investment puts about. Thus, it is the net indebtedness of individual firms that is the critical indicator of fragility, and not the gross indebtedness of the company sector as a whole, as Minsky was to argue.

[One of the referees of this journal has suggested that the notion that investment makes the corporate sector as a whole more liquid amounts to 'a sort of Say's Law' in which 'investment finances itself by creating equivalent profits' so that financial fragility is excluded. This would only happen if the whole non-financial corporate sector were one firm. There is no reason why any particular investing firm should receive the profits equivalent to its investment. And even if the whole non-financial corporate sector were one firm, this would not protect that firm from a fall in profits when it reduces its investment. In both Minsky and Kalecki, the process is driven by effective investment demand, and therefore Say's Law of the Markets would not apply. The cash flow analysis of this is given in a key paper by Josef Steindl (Steindl, 1982).]

One is not surprised that Minsky did not examine these implications of Kalecki's profits theory. The monetary analysis in Kalecki's theory is virtually unknown, even among his own followers. However, the ideas in it were not so inaccessible to Minsky. In fact there is little difference between Kalecki's monetary analysis and that of Schumpeter, because both drew it from the German monetary discussions of the early part of the twentieth century (Schumpeter as a participant). In Schumpeter's case, the theory may be found in both of his works that Minsky cites in his thesis: *The Theory of Economic Development and Business Cycles*. In retrospect, perhaps, Minsky might have better spent his PhD years researching Schumpeter's monetary theory. In fact, his later 1964 essay on 'Financial Crisis' suggests that he may have picked up some of Schumpeter's monetary ideas. In that essay Minsky divides money flows into three 'transaction types': balance sheet flows (money derived from the sale of assets, and issue of liabilities); portfolio flows, from transfers of financial assets; and 'income flows' derived from the circular flow of income (Minsky, 1964, pp. 234–9). The key monetary feature of fixed capital investment is that it transfers money from balance sheets into the circular flow of income. Had he further developed this analysis, Minsky might have avoided his later inconsistency with Kalecki's theory of profits.

A more serious methodological conclusion may also be drawn from this part of the discussion. Merely sifting error from 'inherited economic analysis' as both Minsky and Schumpeter were doing may not be sufficient if you are left with inconsistent 'truths'. All

serious economists strive to make their theories consistent. As they develop and integrate their ideas, it becomes more difficult to separate out individual concepts and relationships to combine with the ideas of other economists. Intellectual eclecticism leads to the kind of muddle and inconsistency that is found in economics textbooks. This is why sustained, critical work, like Minsky's PhD studies, is necessary in economics.

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