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**A 'SECOND EDITION' OF KEYNES'S GENERAL THEORY**

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(writing as J.M. Keynes)

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## A 'SECOND EDITION' OF KEYNES'S GENERAL THEORY<sup>1</sup>

### Introduction

It was a splendid idea of Geoffrey Harcourt and Peter Riach to bring together so many distinguished economists who have studied and followed my work - including a few whom I knew as young men - to write a 'Second Edition' of my *General Theory of Employment, Interest and Money* (hereafter, *GT*). The effort has been prodigious, with thirty-nine chapters in two volumes all written with insight and erudition, drawing also on the thirty volumes of my *Collected Writings (CW)*. I feel deeply honoured that there should still be so much interest in the simple ideas that I enunciated more than sixty years ago.

It is a real shame that I died while still in my prime. I was only sixty-two at the time, and fully expected to live as long as my parents. Perhaps I shouldn't have worked so hard with too many fingers in different pies, although Milo Keynes, my nephew and a doctor, always says that hard work never killed anybody. I miss Tilton, and my walks over the Sussex Downs, but I am delighted that my biographer, Lord Skidelsky, has moved in to restore the house to something of its former glory, although it was never of any great architectural merit. I originally bought it to be near Duncan, but what a coincidence that the land originally belonged to my Norman ancestors. Much of my writing of the *GT* was done in the peace and quiet of Tilton, far removed from what I hear to be the pressures of modern academic life. I'm surprised anyone has time to read and write any more, let alone to think clearly. Is this why a second look at my *magnum opus* has taken so long?

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<sup>1</sup> This is a review article of A 'Second Edition' of the General Theory Vols 1 and 2 edited by G.C. Harcourt and P.A. Riach (London: Routledge, 1997).

The purpose of writing the *GT* was to destroy classical employment theory (it takes a theory to kill a theory) and to provide a new way of analysing the functioning of economies at the aggregate level. Almost immediately after I published my two volumes of *A Treatise on Money* in 1930, I realised that economics lacked a theory of the determination of the level of output and employment as a whole. Conventional theory assumed that the only limit to employment was the point at which the real wage equalled the marginal disutility of labour, and most economists and policy-makers believed this. All output produced would be demanded, so that the level of employment could never be limited by effective demand (a term I borrowed from Malthus whose ideas on these matters lay buried for over a hundred years). As I put it in my Chapter 3 on 'The Principle of Effective Demand' - in a simile I'm rather proud of - 'Ricardo conquered England as completely as the Holy Inquisition conquered Spain'. My misogynist colleague at King's, Arthur Pigou, believed this and tried to persuade me that the mass unemployment of the 1930s was the result of workers demanding higher real wages than their marginal product justified. I remember what he wrote as late as 1933:

'with perfectly free competition among workpeople and labour freely mobile ... there will always be a strong tendency for wage rates to be so related to demand that everybody is employed .... The implication is that such unemployment as exists at any time is due wholly to the fact that changes in demand conditions are continually taking place and that frictional resistances prevent the appropriate wage adjustments from being made.'<sup>2</sup>

My friends in the Treasury, responsible for public policy, also believed this classical story that with free competition, economies would tend naturally to full employment. Spending more money to create jobs, which my theory indicated, would, in modern parlance, 'crowd out' private expenditure because economies are fundamentally supply constrained, never demand

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<sup>2</sup> A.C. Pigou, The Theory of Unemployment (London: Macmillan, 1933) p.252.

constrained, so there is only a limited amount of investment resources available. How otherwise sensible men (and women?) could have believed this story in conditions of mass unemployment was beyond me, except that I suppose I must have subconsciously believed this myself for a long time, otherwise I might never have bothered to write my two volumes *A Treatise on Money*. It is always difficult to abandon habitual modes of thought, and I am conscious that I retained some of them in the *GT*, which I rather immodestly billed beforehand as a 'revolutionary' book, and this has given ammunition to some of my classical/neoclassical adversaries in recent times - but more on this later.

When the *GT* was published several of my fellow economists had difficulty in comprehending what I had to say. This is evident from the reviews and the correspondence I had. Pigou described it as 'a matrix of sarcastic comment on other people'! It is true that my prose was not at its best, but in defence, I had several difficulties in writing that book. There was no formal macroeconomics before me, so I had to invent a whole new terminology. My health was also poor, and then I got bogged down with the proper definition of income and units of measurement. I ended up by measuring real income in terms of wage units; that is, taking an hour's employment of one grade of labour as numeraire, with the employment of all other grades then weighted by the ratio of their wage to that of the standardised unit. I now think I made the analysis unnecessarily complicated, and could just as well have used a price index. No-one cares about wage units any more, and as Bradford and Harcourt (Chapter 7) point out, I was in any case lax in applying the concept of wage units consistently. In Chapter 21 on 'The Theory of Prices', I took the aggregate price level for analysis without a second thought.

Perhaps I should have also ordered the chapters differently. As my most faithful interpreter, Paul Davidson, suggests in his *tour de force Post Keynesian Macroeconomic Theory*, it was

definitely a mistake to have the discussion of changes in money wages and employment so late in Chapters 19 and 20 after the restatement of my theory in Chapter 18, which allowed one of my main modern adversaries, Milton Friedman, to claim that everything after this chapter should be regarded as ‘strictly peripheral to the main contribution to the *General Theory*’, when in fact these two chapters are absolutely crucial for rebutting the idea that it is wage inflexibility that is the source of long-run unemployment. In fact, I realised as soon as the book was published that my whole theory needed ‘rewriting and recasting’,<sup>3</sup> and I did have plans to write a short controversy-free book giving a clear statement of what I had contributed in my *GT*, but the war intervened. I wouldn’t have written a ‘Second Edition’ because as I reminded my publisher in 1939, my practice was never to have second editions. When I had something I wished to say, I preferred to write a new book.

While appreciating the difficulties my readers and fellow economists might have had at certain points, the ideas I expressed so laboriously in the book are extremely simple and should be obvious. It is my disciples and acolytes that have made them complicated! My first task was to undermine the twin pillars of classical (full) employment theory: firstly the notion that the real wage equilibrates the labour market so that it is the real wage that determines employment, and employment that determines output via the production function, and secondly that the full employment level of output will always be self-financing because the rate of interest is the price which equates planned saving and planned investment. James Trevithick, one of my successors at King’s, has conveniently called these two beliefs the strong and weak versions of Say’s Law, respectively (that supply creates its own demand).<sup>4</sup>

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<sup>3</sup> The words I used in correspondence with Ralph Hawtrey (CW Vol.XIV: 47).

<sup>4</sup> J. Trevithick, Involuntary Unemployment: Macroeconomics from a Keynesian Perspective (Hemel Hempstead: Harvester Wheatsheaf, 1992).

Nothing that is said in the varied contributions to this 'Second Edition' of my *GT* has changed my mind that I was broadly right in my method of attack which required reexamining the classical assumptions concerning the functioning of the aggregate labour market, and exposing the classical theory of the rate of interest that interest is the reward for abstaining from present consumption determined by the real forces of productivity and thrift. Before I elaborate on this attack, however, I must make my methodological position clear.

### **Methodology**

I have been attacked by some of my critics for imprecision and lack of rigour, or more broadly, a lack of formalism. I am grateful to O'Donnell (chapter 31) for setting the record straight on this score, that I am not against formalism in economics in the form of symbolic representation, mathematics, econometrics, etc. What I am against is superfluous or irrelevant formalism. As I said in the *GT* sixty years ago: 'Too large a proportion of recent "mathematical" economics are merely concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols.' I believe the approach taken by Marshall to be the correct one that when formalism is used it should be used in two stages: formalised beginnings followed by non-formal elaboration. Marshall's letter to Bowley sums up my feeling pretty accurately:

'I had a growing feeling in the later years of my work that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics, and I went more and more on the rules - (1) use mathematics as a short-hand language rather than as an engine of enquiry, (2) keep these until you have done, (3) translate into English, (4) then illustrate by examples that are important in real life,

(5) burn the mathematics, (6) if you can't succeed in (4) burn (3). The last I often did.'<sup>5</sup>

This advice partly explains why there is so little mathematics in my *GT*, but it is only part of the reason. Many of the ideas I present in the *GT* simply do not lend themselves to formalism. The factors that underlie and drive human behaviour associated with ignorance and uncertainty, mass psychology, the acquisition of new information and so on cannot be incorporated into formal models. In this regard, Coates (chapter 35) has a nice discussion of the Wittgenstein-Russell view of the vagueness of concepts and information which makes the mathematical treatment of data too imprecise. The remorseless logic of the deductive method can also be so misleading. I am a realist and empiricist, and most of the theoretical propositions in the *GT* are based on observation or empirical supposition, and are therefore to be tested rather than deduced. For this reason, I am not averse to econometrics; indeed, I wish my followers would do more empirical work to test and validate my ideas. They may then be on stronger ground against the philistines. My apparent hostility to Tinbergen in my review of his book on business cycles was not an attack on econometrics as such but on his specific methodology.

In the *GT* I wanted to (re)establish the link between theory and practice, and to improve the image and respectability of the science of economics. What was wrong with classical theory was not its logic but its assumptions. The failure of the classical model to predict had the effect of diminishing the prestige of economists compared to other scientists. I referred in the *GT* to the 'ordinary man's growing unwillingness to accord to economists that measure of respect which he gives to other groups of scientists whose theoretical results are confirmed by

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<sup>5</sup> G.W. Guillebraud, Marshall's Principles of Economics, Vol. 2 (Notes) p. 775.



observation when they are applied to the facts.’ .... ‘A scientific theory cannot require the facts to conform to its own assumptions.’ Gerrard (chapter 32), in a thoughtful essay, is kind enough to describe my *GT* as not only a revolution in theory but also a revolution in method, and I would like to think this is true.

### **Classical Employment Theory**

What I have said above is no better illustrated than in considering the functioning of the labour market according to classical theory. It is true that I accepted the neoclassical marginal productivity theory of the demand for labour which, with hindsight, I needn't have done, as Brothwell (chapter 1) and Darity and Young (chapter 2) have pointed out, but my decisive blow, which opened up the possibility of a new type of unemployment, was to point out that the supply of labour is not a function of the real wage but the money wage. Workers cannot determine their own real wage, and even if they could they would not necessarily withdraw their labour just because the real wage fell as a result of rising prices. In other words, workers may be off their supply curve for most of the time and would be willing to work for the same money wage (and a lower real wage<sup>6</sup>) if only there was a sufficient demand for labour. I called this involuntary unemployment, and gave a long-winded definition of it. I agree that my original definition was unnecessarily complicated, and I am happy for others to give simpler definitions provided they imply an infinite supply of labour at a given money wage.

If the assumption of diminishing marginal productivity, based on diminishing returns to labour, is dropped then, of course, more employment would not involve a lower real wage;

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<sup>6</sup> Maintaining the assumption of diminishing marginal productivity.

and increasing employment, falling unemployment and constant or rising real wages could all occur together. I agree, therefore, that by accepting the first postulate of classical employment theory, I did give ammunition to my neoclassical critics, and more recently to the so-called ‘new Keynesians’, who claim that my theory of involuntary unemployment depends on sticky money wages, and that a fall in real wages is necessary for a rise in employment. On the other hand, I believe that my critics have deliberately misinterpreted me for their own ends. I make it perfectly clear early on in *GT* that at the *aggregate* level it is not the real wage that determines employment, but employment (determined by aggregate demand) that determines the real wage. The theory of labour demand at the micro-level cannot be transferred to the macro level because there is no telling how wage cuts will affect the aggregate level of employment. I also made clear in my 1939 *Economic Journal* article on ‘Relative Movements of Real Wages and Output’ that I accepted the empirical evidence of my students, Dunlop and Tarshis, that over the trade cycle it appears that real wages and employment, are, indeed, not negatively related, but positively related, and if this is so ‘the practical case for a planned employment policy is considerably reinforced’ because no reduction in real wages is implied. On this point, I am a little surprised that none of the contributors to the ‘Second Edition’ of my *GT* explicitly mention this as a crucial deficiency of the hypothesis of a so-called ‘natural’ rate of unemployment which has been used by Milton Friedman and his followers to challenge my theory of involuntary unemployment and the role of demand management in conditions of high unemployment. The natural rate of employment (and by implication, unemployment) is defined at the point of intersection of the classical demand and supply schedules for labour. But if workers are either off their supply curve, and willing to work for a lower real wage (if necessary), or if employment and real wages are, in fact, positively related, the idea of a natural rate of unemployment, below which there will be real wage resistance and therefore accelerating inflation, loses all meaning. The doctrine of the natural rate is a theoretical

construct based on faulty labour market assumptions which merits a full-frontal assault. No-one addresses my modern-day critics head on, particularly the pernicious doctrines of monetarism, and the new classical macroeconomics which starts from the ludicrous assumption of continual market-clearing, thus denying from the outset the possibility of involuntary unemployment.

Let us turn now to the second pillar of classical employment theory, namely the role of the interest rate in equilibrating saving and investment. Lawlor (chapter 20) provides a fine summary of my objections to classical interest rate theory, and also why my new liquidity preference theory is not the same as the loanable funds theory which still has the rate of interest balancing saving and investment. My points of departure from the classical theory were essentially two-fold. First, I did not believe that the interest rate was the reward for 'waiting'. People may abstain from present consumption without reward because they wish to remain liquid. Secondly, saving is primarily a function of income, so that the level of income has to be known before the interest rate can be known. But the level of income partly depends on the rate of interest via the effect of the rate of interest on the level of investment. The classical theory of the rate of interest is therefore indeterminate. Only if the level of income is given can the rate of interest be said to equilibrate savings and investment, but that assumes what must be proved that income will remain fixed as changes in investment and saving take place.

I replaced the classical theory, as everyone knows, with my liquidity preference theory, in which I define the rate of interest as the reward for parting with liquidity, so that the interest rate is determined not in the goods market, but in the money market by the supply and demand for money. The fact that the money rate of interest may not induce enough investment to

match full employment saving poses severe problems for the achievement of long run full employment in my model.

### **Effective Demand**

Having demolished the classical theory of employment determination, it was necessary to replace it with a more plausible theory which I did through introducing the principle of effective demand. I regard my chapter 3 on this topic as the most important and innovative of the book but, of course, it was only the foundation of a much larger and complex superstructure. In my simple model of the closed economy, national income and employment are determined at the point of effective demand; demand is determined by consumption and investment; consumption is largely determined by income; investment is determined by the rate of interest, and the rate of interest is determined by the supply of money and liquidity preference. The dependent variables of my system are income and employment. The ultimate independent variables consist of: firstly, various psychological propensities concerning consumption, the demand to hold money and expectations about future yields from investing in capital assets; secondly, the wage unit determined by bargains between employers and employed, and thirdly, the quantity of money determined by the actions of the Central Bank. I also treated several factors as given, particularly the state of competition. Most of the contributors to the 'Second Edition' of the *GT* focus on particular constituents of the model, elaborating and extending it in various ways, but the model *as a whole* remains largely unscathed. Given the independent variables, the chain of reasoning runs from the rate of interest to investment to income, and back to the rate of interest unless the monetary authorities stabilise it. It was a blind spot in the *GT* that I treated the rate of interest as an independent variable, given that the demand for money is a function of income, and I should

have said more about the process of money supply creation. Sheila Dow (chapter 24) rightly points out, however, that while I treated the money supply as one of my three ultimate independent variables, I did not mean that the money supply is exogenous. It is 'given' in the sense of being determined by the banks' willingness to extend credit as influenced by the monetary authorities, but certainly not outside the system. But it would have been helpful to have said more about its endogeneity, notwithstanding the much more primitive banking system that prevailed in the 1930s compared to today. It is possible that this might have thwarted the resurrection of the quantity theory of money by Friedman, although I doubt it. Contrary to Friedman's facile claim, money always mattered in the *GT*. Today, as a realist, I would position myself between the horizontalists and the verticalists, and draw a stepped function of the relation between interest rates and the supply of money combining the power of the Central Bank to determine interest rates with elements of endogeneity. As long as the supply of money is not fully endogenous, in the sense of fully demand determined, my monetary theory expounded in the *GT* retains all its essentials.

Let us return to the principle of effective demand. King (chapter 23) rightly says that I owe the notion of effective demand, and the idea of under-consumption, to Malthus, but to be fair, I did acknowledge a debt to Malthus. It also appears to be the case that J.A. Hobson precisely anticipated my own theory, but I have to confess that I never studied Hobson's writings until 1935 when the first twenty-two chapters of the *GT* had already been written. If I had done, I might have put large sections of my chapter 23 on 'Notes on Mercantilism etc.' much earlier in the book. Similarly, Sardonì (chapter 36) thinks I could have drawn more on Marx because in *Das Kapital* he raises the possibility of deficient aggregate demand, or a 'realisation crisis' as he called it. Marx was also aware that a barter economy is different from a money economy, because money can lie idle. I must confess, I never read Marx seriously, perhaps

because our ideologies differed so much. As I wrote to George Bernard Shaw in 1935: ‘I’ve made another shot at old KM [Karl Marx] last week without making much progress ... if you tell me that they [Marx and Engels] have discovered the clue to the economic riddle, still I am beaten. I can discover nothing but out of date controversialising.’ Tobin (chapter 25) suggests I might also have included in chapter 3 sections of what I have to say about demand in chapter 16 on ‘Sundry Observations on the Nature of Capital’. Certainly the uncertainty associated with money, and producers of consumption goods not knowing when idle balances will be spent, is a major cause of failures of effective demand (see later).

I think Clower (chapter 3) is unnecessarily harsh on me when he argues that I provided no answers (only insights) to the central question raised in my book of how professional economists can best make theoretical sense of contemporary monetary economies. I thought I provided a pretty definitive answer, at least to the central question I was addressing on why economies may remain in a condition of sub-normal activity without any tendency towards recovery - namely, a deficiency of aggregate demand. Equally, Pasinetti (chapter 6) appears to split hairs when he says about my chapter 3, I never talk about the ‘principle’ of effective demand, only about the point of effective demand. The principle is as I stated in my letter to Sraffa: ‘any expansion of output gluts the market unless there is a *pari passu* increase in investment appropriate to the community’s marginal propensity to consume; and any contraction leads to windfall profits to producers unless there is an appropriate *pari passu* contraction of investment.’<sup>7</sup> The point of effective demand is where my expected receipts schedule cuts from above the necessary receipts schedule. The ‘point’ implies the ‘principle’, and vice versa.

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<sup>7</sup> CW Vol. XXIX: 159-160.

Now how does the state of competition affect my central message about the causes of unemployment? As Marris (chapter 4) and Shapiro (chapter 5) point out, in the *GT*, I take the state of competition as given because I couldn't see 'how on earth' the degree of competition would matter.<sup>8</sup> Despite what Marris says, I still think my theory is valid on any microeconomic foundations because while in some respects imperfect competition would strengthen my case, in other respects it would weaken it. I have some sympathy with the point made by Weitzman in the early 1980s<sup>9</sup> (although none of the contributors mention him) that in a perfectly competitive world it would be easier for the unemployed to become self-employed provided capital markets are also perfect, although elements of uncertainty would still remain. Indeed, as Shapiro emphasises, flexible prices make events more uncertain, so that imperfect competition can be good for employment by making investment and profits more stable. The state of competition can work in conflicting ways on the level of employment. The really important link between micro-structures and demand is highlighted in the work of Kalecki who, as Kriesler (chapter 38) describes, independently discovered the principle of effective demand. I make no mention of Kalecki in the *GT* because he was virtually unknown outside Poland at the time. The degree of competition must affect demand through the level of the markup on costs which affects the distribution of income. I have virtually nothing to say on the functional distribution of income in the *GT* (although I did, of course, in *A Treatise on Money*), but clearly it is an important determinant of consumption and investment, and plays a prominent role in the extension of my static analysis to the analysis of equilibrium growth.

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<sup>8</sup> CW Vol. XIV: 190.

<sup>9</sup> M.L. Weitzman, Increasing Returns and the Foundations of Unemployment Theory, Economic Journal, December 1982.

## **My Model**

I turn now to the major building blocs of my theory of the determination of the level of output and employment: consumption; the multiplier; investment; the role of expectations; liquidity preference, and the rate of interest. The concept of the consumption function was undoubtedly one of my most important innovations in the *GT*. It divorced for the first time the decision to save from the decision to invest. It led on to the concept of the multiplier, and to my formal demonstration that it is changes in income, not the rate of interest, that equilibrates savings and investment, and this equilibrium can be at less than the full employment level. With the consumption function, I effectively destroyed Say's Law. Admittedly, I held wages and prices constant, as well as the rate of interest, and by concentrating on the short period, I gave further ammunition to my neoclassical critics that in time adjustment will take place with sufficient wage and price flexibility. I concentrated on short run static analysis for the reason I gave in my *Tract on Monetary Reform* (1923), that it is in the short run that we have our being. But clearly Say's Law is a long run proposition, and I am pleased that Rogers (chapter 19) thinks I did enough to destroy it for my analysis to be interpreted as presenting a long period equilibrium. That was certainly my intention. Going back to my analysis in chapter 3 on effective demand, the aggregate demand and supply curves can easily be defined as long period curves given the state of long-term expectations, the money wage and the rate of interest. The question of the stability and persistence of long run equilibrium then depends on the dynamic factors that I discuss in chapter 19 on 'Changes in Money Wages.'

Thomas (chapter 9) reflects my view that I would not want to make any major revisions to my three chapters on the propensity to consume. I made consumption a function of the absolute (current) level of income, and others have subsequently related consumption to alternative



measures of income such as relative income, life-cycle income and permanent income, which in no way detract from my central thesis. Empirical studies of consumption and saving, both time series and cross section, show the overwhelming importance of income as the dominant determinant compared with the rate of interest or other variables. If I had not confined my analysis to a closed economy, the only major addition I would have needed to make to these chapters would have been to extend my multiplier analysis to include foreign trade, perhaps on the lines suggested by my good friend Roy Harrod in his book *International Economics* published three years before my own. Davidson (chapter 30) has an Appendix on this point, and I agree with those who say that in modern day open economies with free trade, a gap between planned imports and planned exports is probably much more difficult to plug than a gap between planned saving and planned investment, and thus the level and growth of income is largely driven by the foreign trade multiplier.<sup>10</sup>

Investment clearly plays a dominant role in my model, because given the propensity to consume, it is investment that determines the equilibrium level of income. It is the decision to invest that drives a capitalist economy. I needed to develop a theory of investment demand, and I did so with the concept of the marginal efficiency of capital (by which I really meant the marginal efficiency of investment). This concept was originally confused with the marginal product of capital, but as Pasinetti (chapter 13) makes clear, it rests on entirely different foundations and has a completely different meaning. Pasinetti wonders why I never reacted. I suppose the answer is that while it is true that the marginal product of capital is used to determine the rate of interest in classical theory, I didn't have to destroy the neoclassical theory of capital in order to undermine it. It was enough to attack the savings supply schedule.

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<sup>10</sup> See A.P. Thirlwall, Macroeconomic Issues from a Keynesian Perspective (Edward Elgar, 1997) Chapter 15.

The reswitching debate in capital theory was before my time, but Gordon (chapter 29) presents a scathing attack on the neoclassical theory of investment demand which posits a unique relation between the demand for investment and relative factor prices. Eisner (chapter 12) is kind enough to say that my chapters on the inducement to invest have stood the test of time and need little rewriting, except that he would focus on the totality of a nation's investment, not just on private, business investment. I agree that this is important for considering the most expeditious way to full employment, and also for considering how to stimulate business investment through public investment, and I have things to say on this in my chapter 24 on 'Concluding Notes on the Social Philosophy Towards which the General Theory Might Lead.'

I defined the marginal efficiency of capital in my *GT* as that rate of discount which equates the present value of (expected) prospective yields with the supply price of capital goods. One element of supply price is user cost which I defined as 'the reduction in the value of equipment due to using it as compared to not using it, after allowing for the cost of the maintenance and improvements', or, in other words, a measure of the sacrifice of expected future profits due to using equipment today rather than in the future. User cost, therefore, is one of the mechanisms by which expectations of the future will affect the present by altering supply price. It would be true to say that my discussion of this concept in the Appendix to chapter 6 has been largely ignored since I introduced it, so I am grateful to Torr (chapter 6) and Kregel (chapter 12) for resurrecting it.

There is also the question of the finance of investment. Classical theory was deeply steeped in the notion that saving leads to an equivalent amount of investment, and it is a matter of regret that much of mainstream growth and development theory still is. One of the major messages

that comes out of my *GT*, however, is that it is not saving that leads to investment, but investment that leads to saving via variations in the level of income. Investment does not require prior saving, it requires finance. One of the major contributions of my post Keynesian followers has been to continually emphasise this point. It should be obvious that saving cannot finance investment because, as Chick (chapter 11) points out, finance is required at the beginning of the period and saving is generated through the period during which output increases. Saving must ultimately fund investment, but doesn't finance it. I should have said more in the *GT* about the role of the banking system in financing investment, which could also have been linked in nicely with a discussion of endogenous money. I did belatedly recognise a finance motive for holding money in my two *Economic Journal* articles in 1937,<sup>11</sup> but a proper discussion of the role of finance as a determinant of investment is lacking. Kriesler (chapter 38) may be right that Kalecki got closer to the truth in focusing on the availability, rather than the cost, of finance as the more important determinant, and it is the banks that control the money supply process.

### **Expectations**

My major achievement in the analysis of investment behaviour was to introduce for the first time the role of expectations in a fundamental way. It is expectations about the future that introduces uncertainty into economic analysis which makes it difficult, if not impossible, to attach numerical probabilities to economic outcomes. Expectations also mean that the future affects the present and this has profound implications for the understanding of current events and economic policy. In the *GT* I distinguished between the state of short-term and long-term

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<sup>11</sup> 'Alternative Theories of the Rate of Interest', *Economic Journal*, June 1937, and 'The Ex-Ante Theory of the Rate of Interest', *Economic Journal*, December, 1937.

expectations. I would stick with this distinction, but as Hoover (chapter 14) reminds me, I did say in some lecture notes in 1937 that if I were writing the book again I would start with the assumption that short period expectations are always fulfilled, and then have a subsequent chapter showing what happens when expectations are disappointed. Saying that short-term expectations are fulfilled would, I suppose, put me in the rational expectations camp, but I could never, of course, be a rational expectationist in the long run because it is not feasible that uncertainty can be quantified in any meaningful sense, or that people behave on the basis of pure mathematical expectation. As I said in the *GT* ‘if human nature felt no temptation to take a chance, no satisfaction (profit apart) in constructing a factory, a railway, a mine or a farm, there might not be much investment merely as a result of cold calculation’ ... ‘thus if animal spirits are dimmed and the spontaneous optimism falters, leaving us to depend on nothing but mathematical expectation, enterprise will fade and die.’

When it comes to explaining economic behaviour in the face of uncertainty, there is no sensible alternative to employing rules of thumb and conventions. Acting on the basis of convention means believing that the existing state of affairs will continue indefinitely, except in so far as there are specific reasons to expect a change. Conventional behaviour is therefore perfectly rational. I like Davidson’s terminology of ‘sensible expectations’<sup>12</sup> based on ‘evidential weight’. Howitt (chapter 15), Davis (chapter 33) and Runde (chapter 34) are broadly sympathetic to these views of mine. Davis wishes that I had said more in the *GT* about the role of confidence in determining particular patterns of conventional judgement. In fact, in my 1937 *Quarterly Journal of Economics* article,<sup>13</sup> I did draw attention to the fragility of

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<sup>12</sup> P. Davidson, *Sensible Expectations and the Long Run Non Neutrality of Money*, *Journal of Post Keynesian Economics*, Fall 1987.

<sup>13</sup> ‘The General Theory of Employment’, *Quarterly Journal of Economics*, February, 1937.

individual expectations, and how people conform to the behaviour of the majority. The herd instinct will always remain a basic human trait.

Even if the notion of rational expectations is conceded, it is important to emphasise that it turns out to be a red herring in the attack on my theory and policy recommendations by the neo-classical macroeconomics school, originally led by Lucas. It is perfectly possible to have rational expectations unemployment equilibria, in which it is in no party's interest for the status quo to be changed. The policy ineffectiveness conclusion of the new classical macroeconomics depends not on the assumption of rational expectations but on the assumption, without proof, that all markets automatically clear on the basis of voluntary exchange.

The volatility of investment is a major cause of cyclical fluctuations in economic activity, and I devoted chapter 22 to 'Notes on the Trade Cycle'. I tried to explain the 8-10 year trade cycle in terms of fluctuations in the marginal efficiency of capital driven by waves of optimism and pessimism. Jarsulic (chapter 21) dissects this chapter and compares my theory with others. In none of the theories, let alone my own, is there any convincing explanation of the *periodicity* of the cycle, if, indeed, there are regular cycles to explain. I remember Irving Fisher once saying that he saw no more reason to believe in regular cyclical fluctuations in economic activity than in regular cyclical fluctuations in luck at Monte Carlo! I liked Tobin's (chapter 25) attack on real business cycle theory. Tobin, like me, is a realist. The empirical evidence available simply does not support an interpretation of output and employment fluctuations based on unexpected price changes around a full employment norm.

## **Economic Policy**

It does seem that governments have managed to some extent (at least up to now) to even out the worst excesses of the trade cycle through demand management policies, and I am happy to take some credit for this. I now find the lack of commitment to full employment disappointing, however, particularly in continental Europe. I agree with my ex-pupil, Reddaway (chapter 26) that we have got our priorities wrong. The obsession now seems to be with the control of inflation. I was never an inflationist, but I still believe, as I said in 1931, that 'it is worse in an impoverished world to provoke unemployment than to disappoint the rentier.'<sup>14</sup> It always surprised me, in the 'golden age' after 1945, that the level of unemployment was maintained so low, but I did warn about the dangers of inflation. My chapter 21 on 'The Theory of Prices' which Brown (chapter 27) reviews, warns of both bottlenecks (structural inflation) and of rises in the wage unit before the full employment level of output is reached (cost-push inflation). In this chapter there is also a strong critique of the quantity theory of money, which wouldn't need much alteration for an attack on modern monetarism, especially as I attack the quantity theory on its own ground by assuming the money supply to be exogenous. My instinct is, however, that monetarism as an intellectual fashion is now dead.

Returning to the question of recession and unemployment, Cornwall (chapter 22) correctly points out that I never argued for a State-controlled economy as a solution to the trade cycle, as those on the political right sometimes maintain. I always believed it was better for a man to tyrannise over his bank balance than other people. It is true that I said 'that the duty of ordering the current volume of investment cannot be left solely in private hands', but equally

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<sup>14</sup> J.M. Keynes, Essays in Persuasion (Macmillan 1931).

‘no obvious case is made out for a system of State Socialism which would embrace most of the economic life of the community.’ Skidelsky (chapter 24) is disingenuous in his contribution to my views on the role of the State. He pretends to be writing as me, but he seems to use me as a mouthpiece for his own philosophy. I would not say that ‘my own theory would have justified an increase in thriftiness, on the part of both the individual and the State, to maintain a sufficient rate of investment in the post-1945 conditions of full employment. Instead, the vast expansion of State consumption and transfer payments which started in the 1960s was ... a major cause of the slowing down in the rate of capital accumulation from the 1970s onwards.’ This represents a misunderstanding of my theory. It is not saving that leads to investment, but investment that leads to saving. In the British context, there has been a chronic unwillingness of the entrepreneurial class to invest. I would have done much more to encourage private sector investment, and I would have tried to ensure that North Sea oil revenue was not squandered (as it was). I have not become disillusioned with the role of the State because I believe that ‘the loss in economic efficiency produced by misguided State investment policies has come to outweigh the gain in stability.’ This is Skidelsky speaking (who now takes the Conservative whip in the House of Lords) not me. In the light of recent events in Britain and the world economy, I would support greater discretion in economic policy-making, not less, and a greater degree of intervention in free markets. The UK government’s decision to relinquish control over monetary policy to an independent Bank of England was a mistake, and public spending and taxes in Britain are not high by international standards. At the international level, casino-capitalism needs tight control, and I agree with Cornwall that some of the institutions I helped to create during and after the War need repairing if there is to be a return to the ‘golden age’ conditions prior to 1973.

## Money

I now turn to the role of money in my theory. One of the major purposes of my *GT* was to bridge the dichotomy in classical theory between money and value theory, in which money was only a veil and affected nothing real. In classical theory the rate of interest is determined by real forces and there is the assumption of long run full employment, so that money affects only the price level. In my theory, money affects the real economy in two major ways: through the determination of the interest rate and by providing a haven for liquidity. An economy in which goods exchange for money is fundamentally different from a barter economy in which goods exchange for goods, and perhaps I should have retained in the *GT* the distinction I made in the drafts between the co-operative economy, the neutral economy and the entrepreneurial economy, as many of my followers would have liked. As it is, I admit that my two main chapters on money are obscure to say the least, and not fully integrated into the rest of the book. I firmly believed that it was the money rate of interest and nothing else that set the limit to employment which is what led to chapter 17 on ‘The Essential Properties of Interest and Money’, and the complex and tortuous discussion of ‘own rates of interest’. I felt it was necessary to enquire whether it was only money which has a rate of interest, and what would happen in a non-monetary economy. Money rules the roost because only the money rate of interest cannot be negative, whereas own rates of interest on other assets can be. I have to agree with Barnes and Caspari (chapter 17) and Rogers (chapter 19), however, that the discussion turns out to be tangential to the main consideration of the cause of unemployment, and not much would have been lost if the chapter had not been written, because it is ultimately the liquidity trap that matters. There may be a rate of interest compatible with full employment but it cannot be reached. My critics countered by introducing real balance effects from price reductions in conditions of excess capacity. I had already conceded the effect of falling prices on the real money supply but, of course, this ‘Keynes effect’ will not work in the



liquidity trap. All that is left is the Pigou effect of real wealth increases on consumption, but this may easily be offset by the negative effect of falling prices on investment by reducing the net worth of firms. Endogenous money would also weaken the strength of real balance effects. As Pigou eventually conceded, price reductions to secure full employment are a fragile reed to cling to ‘with very little chance of ever being posed on the chequer board of actual life’.<sup>15</sup>

More pertinent, I believe, are the points I make in chapter 16 on ‘Sundry Observations on the Nature of Capital’. As I put it at the beginning of the chapter:

‘An act of individual saving means - so to speak - a decision not to have dinner today. But it does not necessitate a decision to have dinner or buy a pair of boots a week hence or a year hence or to consume any specified thing at any specified date. Thus it depresses the business of preparing today’s dinner without stimulating the business of making ready for some future act of consumption. It is not a substitution of future consumption-demand for present-consumption demand, - it is a net diminution of such demand.’

Tobin could equally have suggested that I might switch to chapter 3 some sections of chapter 17 on ‘The Essential Properties of Interest and Money’ on the same grounds. There I talk about the essential properties of money which make a monetary economy different from a barter economy, so that when agents switch from goods to money there is a net loss of demand because there is no industry that ‘produces’ money (zero elasticity of production). And even if the price of goods falls, people still want to hold money (zero elasticity of substitution).

‘Unemployment develops, that is to say, because people want the moon; - men cannot be employed when the object of desire (i.e. money) is something which cannot be produced and the demand for which cannot readily be choked off. There is no remedy but to persuade the public that green cheese is practically the same thing and

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<sup>15</sup> A.C. Pigou, Economic Progress in a Stable Environment, Economica, August 1947.

to have a green cheese factory (i.e. a central bank) under public control.'

(Rereading my *GT*, I am impressed by my wit and turn of phrase!) One of the purposes of Leijonhufvud's book *On Keynesian Economics and the Economics of Keynes* published in 1969 was to highlight the monetary foundation of my theory of effective demand and to nail the lie that my model depends on the assumption of rigid money wages and prices, otherwise the Pigou effect will get an economy to full employment irrespective of a liquidity trap. As Littleboy (chapter 39) describes, Leijonhufvud and my post Keynesian followers seem to have fallen out, but on this major point they can surely agree.

### **The Open Economy**

I am very conscious that I said very little in my *GT* about the open economy. In fact, Harry Johnson once said rather unkindly (I forget where now) that the greatest disservice I did to economics was to cast my model in a closed economy context; but I couldn't do everything at once. In my chapter 23 on 'Notes on Mercantalism etc.' I do have things to say about the importance of the balance of payments and the wisdom of free trade in conditions of unemployment, and in my evidence to the Macmillan Committee in 1931 I had much to say about the difficulties of reconciling internal and external balance. My war-time writings also provide ample evidence of my awareness of the international dimension of full employment policy. The Bretton Woods system which I helped to construct provided a degree of stability to the world economy not previously enjoyed, and it is no accident, as Davidson (chapter 30) points out, that world economic conditions and unemployment have worsened since the collapse of the system in 1971. If my proposals at Bretton Woods for the penalisation of surplus countries and the control of capital movements had been adopted, the system could probably have survived, and the world wouldn't be in the mess it is today. To those who now

call for the control of capital movements, let me remind readers what I said in 1941: ‘Loose funds may sweep around the world disorganising all steady business. Nothing is more certain than that the movement of capital funds must be regulated; - which in itself will involve far-reaching departures from laissez-faire arrangements.’<sup>16</sup>

I like Davidson’s plan for the reform of the international monetary system, not the least because it bears a strong resemblance to my own plan for an International Clearing Union at Bretton Woods which was rejected. Like mine, Davidson’s plan is designed to promote global expansion and to eliminate deflationary bias; to control movements of flight capital and to provide for the automatic expansion of international liquidity. It would consist of four main elements: Firstly, a new unit of account (the International Monetary Clearing Unit or IMCU) would be created to be used as the reserve asset of the system, and held by Central Banks. Secondly, each country’s currency would be convertible into IMCUs at a fixed rate. Thirdly, overdraft facilities would be available through the clearing system; and lastly, excess credit balances would be spent automatically either on foreign goods, foreign investment or aid. This is a perfectly feasible and workable plan which should be given serious consideration in discussions now taking place for a new Bretton Woods.

## **Conclusion**

These are two handsome and magnificent volumes which both elucidate and elaborate the ideas I formalised in the *GT* over sixty years ago. The world has changed since then, but the suppositions and assumptions on which I based my analysis, and the conclusions I derived, seem to be as relevant as they ever were. I realise I have largely been consigned to the history

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<sup>16</sup> CW: Vol.XXV: 31.

of economic thought by the majority of the economics profession, but the concepts I introduced of the consumption function, the multiplier, the marginal efficiency of capital and liquidity preference still form the backbone of macroeconomics, and it is pleasing that there are signs of a revival of interest in my theoretical conclusions and policy ideas. The counter-revolutions launched against me in the 1960s and 1970s by the monetarists and the new classical macroeconomists seem to have died a slow death. These two volumes, therefore, should not be thought of as simply a book written by Keynesians for Keynesians, but of significance for the economics profession at large, especially for those seeking a more amenable intellectual home.

My *GT* had many faults which the contributors have not been reticent to expose. Major worries include lack of clarity; giving ammunition to my neoclassical adversaries; poor ordering of the chapters; the state of competition assumed; the assumption of an exogenous money supply; neglect of the open economy, and a vagueness as to the time period over which my analysis is relevant. I have tried to address these points in a simple and sympathetic way, and they are surely points that I would take more account of if I was writing the *GT* today. The main structure, however, would remain intact for the basic reason I give in chapter 1:

‘the postulates of the classical theory are applicable to a special case only [and] the characteristics of the special case assumed by the classical theory happen not to be those of the economic society in which we actually live, with the result that its teaching is misleading and disastrous if we attempt to apply it to the facts of experience.’