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Anwar Shaikh, *Capitalism: Competition, Conflict, Crises.* Oxford University Press: New York, 2016; xxxv + 979 pp., 9780199390632, RRP $55US.

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If there was any justice in the world of economics, Anwar Shaikh would have won a Nobel Prize for his 1974 *Review of Economics and Statistics* 'Note' on the 'Humbug Production Function'. Here, Shaikh (1974) demonstrated that any economy with constant wage and profit shares would generate econometric results consistent with 'a Cobb-Douglas production function having constant returns to scale, neutral technical progress and marginal products equal to factor rewards' – even one in which the input-output data, plotted in output-capital space, spell out the word 'HUMBUG' (p. 118). Although perhaps not necessary, this was certainly sufficient to bury the neoclassical theory of capital and distribution, the aggregate production function and all the alleged econometric evidence confirming these ideas: 'not even wrong', as two later authors titled their book on the subject, 39 years later (Felipe and McCombie, 2013). But there is no justice in the world of economics, and this astonishing six-page paper was ignored by almost all mainstream theorists.

Shaikh spent the next four decades developing a 'classical' alternative to neoclassical theory, and this enormous book is the culmination of his life's work. Part I, *Foundations of the Analysis*, has six chapters. After a long introductory chapter, there is a brief and extremely interesting empirical chapter on 'Turbulent trends and hidden structures', which provides evidence on the persistent but highly unstable growth of output, productivity and living standards in 'successful' (i.e. advanced) capitalist economies: 'in this system', Shaikh observes, 'order is achieved through the collision of disorders. This is how the invisible hand works' (p. 72). The 45-page Chapter 3, on 'Micro foundations and macro patterns', is much less satisfactory; I shall come back to it shortly. It is followed by three chapters in which micro- and macro-themes alternate: 'Production and costs' (Chapter 4); 'Exchange, money, and price', which, despite the title, is primarily on macroeconomics (Chapter 5); and 'Capital and profit' (Chapter 6), in which Shaikh offers 'a General Solution to the Universal "Transformation Problem"' (Section IV, pp. 221–229).

Part II, titled *Real Competition*, is largely (but not entirely) devoted to microeconomic issues. It begins with a discussion of 'The theory of real competition' (Chapter 7), which is followed by four long chapters on 'Debates on perfect and imperfect competition' (Chapter 8), 'Competition and inter-industry industrial prices' (Chapter 9), 'Competition, finance, and interest rates' (Chapter 10), and 'International competition and the theory of exchange rates' (Chapter 11). By the start of Part III, *Turbulent Macro Dynamics*, we are already at page 539. The six chapters of Part III (amounting to another 209 pages) deal with 'The rise and fall of modern macro' (Chapter 12); Shaikh's alternative to it, 'Classical macro dynamics' (Chapter 13); 'The theory of wages and unemployment' (Chapter 14); 'Modern money and inflation' (Chapter 15); and 'Growth, profitability, and recurrent crises (Chapter 16). The brief Chapter 17 is somewhat misleadingly titled 'Summary and conclusions', but it ends with some entirely new material, on Thomas Piketty and on questions of economic development and underdevelopment. All up, there are 761 pages of text and a further 138 pages of appendices; the references take up 38 pages, the 'note on abbreviations' another 11 pages, and the subject and author indices a final 29 pages. I counted 121 figures and 86 equations, though I would not want to be held to these statistics. Suffice it to say that this is a work of quite remarkable scholarship.

Much of it is extremely well done. The critical analysis in Chapter 4 of the derivation of cost curves by both mainstream and heterodox economists is original, absorbing and totally convincing, and the discussion of 'real competition' in Chapter 7 is also original and persuasive:

> Real competition generates its own characteristic patterns. Prices set by different sellers are roughly equalized as each tries to gain an advantage over the others. Profit rates on new investments are also roughly equalized over somewhat longer periods. Both of these processes result in perpetual fluctuations around various moving centers of gravity. This is the classical notion of *turbulent equilibration*, very different from the conventional notion of equilibrium as a state-of-rest. (p. 260)

Some unexpected and intriguing connections are made, for example, between Karl Marx's theory of the firm and that articulated a century later by Philip Andrews and Elizabeth Brunner (p. 318). The key features of the theory of the firm in 'real competition', Shaikh maintains, 'can be credited to P.W.S. Andrews and (to a lesser extent) to Roy Harrod'. The crucial point, again, is that competition 'equalizes the rates of return on new investment, not those on average capital which includes all older vintages' (p. 272). Having had first Andrews and (after his premature death) Brunner as my head of department at the University of Lancaster, I can imagine these two Conservatives being appalled at the comparison with Marx. But Shaikh has convinced me.

His treatment of one important methodological issue is less satisfactory. Much of Chapter 3 is devoted to the question of the 'micro foundation' (Shaikh always uses two words rather than one) of macroeconomic theory. Why he insists on using this misleading language is a mystery, as he is quite explicit in rejecting Lucas' claim that 'macro must be dissolved into micro' (p. 76). Shaikh's extended discussion of this important question is not as clear as it might have been. He makes repeated reference to emergent properties, without pointing out that these are features of complex systems. The fallacy of composition, which underpins his critique of the micro-foundations dogma, does not require complexity, while almost all of Shaikh's own formal analysis involves simple (though often quite elaborate) theoretical models from which complexity is absent. I could find only one brief mention of downward causation, which provides another compelling reason for rejecting micro-foundations: 'The individual must be conceived as socially situated, structured and shaped by nationality, gender, ethnicity, and class' (p. 110). Very true, and Shaikh could have made much more of this (see King (2012) for an extended discussion of all these issues).

To a large extent, this is a verbal, not a substantive, problem. It does not compromise the real virtues of Shaikh's own macroeconomics, which has a strong Post Keynesian flavour: investment is a function of expected profitability, and fluctuations in the relative shares of wages and profits play an important role in the business cycle. For all its undoubted merits, however, Shaikh's distinctive version of 'classical' macroeconomics also has some serious problems. His account in Chapter 12 of the rise and fall of modern macroeconomics occupies 59 pages, but this proves not to be enough. New Keynesian theory is dismissed in less than one page, and there is no discussion of Joseph Stiglitz and almost nothing on Paul Krugman. The New Neoclassical Synthesis is not mentioned, and the name of John Taylor is missing from the author index (though the work of the structuralist Lance Taylor is discussed at some length). Some aspects of Post Keynesian macroeconomics are critically assessed, not always satisfactorily, both others are glossed over or ignored. There are only two passing references to Hyman Minsky, whose financial instability thesis is not mentioned, though it would have strengthened Shaikh's own analysis of the turbulent nature of capitalist finance.

His criticisms of Keynes and Kalecki are continued in the 40-page Chapter 13, on 'Classical Macro Dynamics', but they do not all appear to be well-founded. Shaikh objects to the Keynesian account of the multiplier effects of increased investment on the grounds that the monetary aspects are neglected: 'where would the extra funds come from?' (p. 603). His argument seems to rest on the (unstated) classical/monetarist assumption that the velocity of circulation is constant. However, as Nicholas Kaldor pointed out as long ago as 1958 in his evidence to the Radcliffe Commission, there is absolutely no reason to suppose that this is the case. In fact, his evidence revealed that velocity was far from constant over time:

> Thus in the U.K. there has been a spectacular rise in the velocity of circulation, particularly since 1955 which fully compensated for the failure of the money supply to expand *pari passu* with the rise in prices and in money incomes. (Kaldor, 1958: 146)

To answer Shaikh's question: existing funds can be brought into use, or used more intensively. It also seems quite wrong to suggest that 'the standard multiplier story requires Ponzi finance for changes in investment' (p. 603; a similar claim is made on p. 39, in the introductory chapter). In this context Shaikh's total neglect of Minsky is a serious weakness, since it leads him to ignore the use of non-Ponzi, 'hedge' and 'speculative finance' in the earlier, pre-crisis stages of the business cycle. Shaikh nowhere defines 'Ponzi finance', a term that is missing from the subject index. While 'Ponzi, Charles' does feature in the index, nothing is said about him here or elsewhere in the book, though there is an enigmatic bracketed statement in the introduction: '(let us never forget Ponzi or Madoff)' (p. 13).

'The problem', Shaikh continues, 'arises from the implicit assumption that the business savings rate is *independent* of the financial needs of firms' (p. 604). And again, 'Both Keynes and Kalecki … adopt the standard neoclassical assumption that firms dispense all of their net income to households, so that all savings is done by the latter' (p. 604). I doubt whether this is a legitimate criticism of Kalecki, at least. More important, it is not clear to me that more recent Post Keynesian (or New Keynesian?) models rest on this assumption, and there is a substantial literature dating back to Harcourt and Kenyon (1976) and Eichner (1987) that explicitly rejects it. As I read this section of the chapter for the first time, I began to think that there was a real danger that Shaikh would end up by restoring Say's Law, which relies precisely on saving responding passively to investment. Sure enough, on the very same page, there is a numerical example illustrating the conditions under which '*the multiplier would be zero*' (p. 604; original stress). Evidently, there are real dangers in 'classical' macroeconomics.

Post Keynesians may fairly be criticised for making the simplifying assumption that the savings ratio is constant, and thus that it is invariant with respect to changes in investment, but it is not clear that they *require* this assumption. Shaikh refers to a paper on this question, co-authored with Wynne Godley, that identifies this as 'an important inconsistency in the standard macroeconomic model', but this seems to have made little or no impact on the broader Post Keynesian literature; for example, it does not appear in the references in the recently published 660-page treatise by Godley's close collaborator Marc Lavoie (Godley and Shaikh, 2002; Lavoie, 2014). Shaikh is certainly correct to note that if an increase in investment does lead to an increase in business saving, and hence in the marginal propensity to save, the multiplier will fall, but this is an empirical question rather than a fundamental theoretical error. It is not true that the endogeneity of business saving 'reduces the scope of the multiplier argument' (p. 617); it simply reduces the size of the multiplier, as Shaikh himself acknowledges two sentences later. He does not provide any numerical examples or simulations to indicate the possible magnitude of the problem.

The dangers inherent in 'classical' macroeconomics are soon revealed again, in Shaikh's discussion of the 'normal' rate of capacity utilisation, which eliminates the role of demand in determining the rate of growth and thereby restores Say's Law once again (p. 607). He claims (contentiously) that 'normal' capacity utilisation is a feature of Harrod's theory of growth, so that '*There is no "knife-edge" on the Harrodian warranted path*' (p. 607; original stress). And yet Shaikh's verbal 'summary of the classical theory of growth', which concludes the chapter, concentrates on the role of demand and endorses the 'Keynesian' (no qualifying adjective) view that departures from full employment can be severe and prolonged. 'The emphasis here is on demand stimulation through policy', which

> can keep a boom going for a long time. With a decline in profitability held in abeyance and the interest rate reduced to its lower reaches, private debt and sovereign debt burdens become the critical factors. When these reach their limits, the whole system shudders and various parts fall off. The global crisis of 2007 was just the latest instance of this recurrent problem. (pp. 636–637)

With the possible exception of the reference to 'sovereign debt problems', there is nothing here that a Post Keynesian would disagree with. Interestingly, Shaikh makes no reference to the supposed 'micro foundations' that he has spent several hundred pages to establish.

What precisely does all this imply for macroeconomic policy? Global capitalism was much less turbulent in the golden age – what Piketty describes as the *trente glorieuses* (30 glorious years) between 1945 and 1975 – than it had ever been before, and considerably less turbulent than it would become in the subsequent era of neoliberalism. Can capitalism be (re-)reformed? These questions are addressed, rather obliquely, at various points in the final four chapters. In the final section of Chapter 14, Shaikh first denies that 'the unemployment rate can be maintained at a socially desired minimal level', and even notes a 'similarity with the Friedman-Phelps conclusion … that pumping up aggregate demand will not permanently eliminate unemployment because there are internal mechanisms that replenish the pool of the unemployed' (p. 674). But then he discusses four ways in 'the state can have a major positive influence on macroeconomic outcomes', (p. 675), which include an increase in aggregate demand and an incomes policy on Swedish lines (pp. 675–676; cf. p. 744). Similar uncertainty is evident in the unduly brief concluding section to Chapter 16, on 'Policy lessons and possibilities: Austerity versus stimulus'. Here, Shaikh first denounces austerity (pp. 740–742) but then denies the possibility of maintaining full employment for any length of time in the advanced capitalist countries in the face of a 350-million strong global pool of unemployed workers (p. 745). Nowhere does he discuss the golden age (a term which is missing from the index, but does make a fleeting appearance as a column heading in Table 16.1, p. 730) or make any reference to the classic treatment of its rise and fall in Marglin and Schor (1990), another significant omission from the references. The discussion of long waves in Chapter 16 is entirely in the context of long downturns; nothing is said about long booms. Whether we might expect – or, better still, *create* – one in the 2020s, Shaikh simply does not say.

This is a very long book. In some ways, it is rather obviously *too* long, as the 55-page 'Introduction' suggests. It would have benefited from more rigorous editing, if only to remove unnecessary repetition. For example, 'ergodicity' is defined at least three times (pp. 446 n4, 545 n2, 588); any two of these definitions could have been replaced by a cross-reference to the third. There are three almost identical approving references to Paul Davidson's denial that Post Keynesian macroeconomics rests on the assumption of imperfect competition and (via Peter Kriesler) to the fact that Michał Kalecki took a similar position (pp. 348, 359 n14, p. 600). Incidentally, Shaikh contradicts himself on this question in the final pages of the book: 'Post Keynesian economics … uses imperfect competition to provide a foundation' (p. 745), and this 'puts me at odds with the dominant traditions in orthodox and heterodox economics, both of which have come to rely heavily upon an "imperfectionist" view of the system' (p. 747).

And yet in some ways, the book is also too *short*. I have already noted the deficiencies in Shaikh's account of the history of macroeconomic theory, which probably needed another 50 pages to put right. Even the indexes might well have been much longer. There is no index reference to 'ergodicity', for example, and neither the subject nor author index entries have any sub-headings. Thus, the 'Piero Sraffa' entry runs to 10 lines of page numbers, and that on 'demand' to no less than 19 lines, with no indication of content. To discover whether Shaikh refers to Kalecki's 1943 paper on the political implications of full employment, which is missing from the references, I had to plough through all 39 entries in the author index (he does not). The reader deserved better than this.

But it would be wrong to end on a negative note. This book is a remarkable achievement, and there is a great deal to be learned from it – more in the micro-chapters than in the macro, I suspect. It is always absorbing, if occasionally also infuriating. It is the record of a life well spent, and not too many economists can claim that.

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Stuart Macintyre, *Australia's Boldest Experiment: War and Reconstruction in the 1940s*. NewSouth Publishing: Sydney, NSW, Australia, 2015; Paperback, 604 pp.: 9781742231129, RRP: AUD34.99.

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The Labor Governments of 1941–1949 hold a special place in the hearts of many Australians, both for their courage and leadership during the Second World War and for establishing a fairer and more progressive society after the war. Their most significant and enduring contribution in that area was the effective elimination of unemployment using aggregate demand management to keep the unemployment rate below 2%, a policy that successive governments maintained until its deliberate abandonment in the mid-1970s.