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## Book review

Carlin, Wendy and David Soskice (2015): *Macroeconomics: Institutions, Instability, and the Financial System*, Oxford, UK (638 pages, softcover, Oxford University Press, ISBN 978-0-19-965579-3)

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This book is the new version of the Carlin/Soskice 2006 textbook that popularized and simplified the 3-equation model that arose from the incomprehensible neo-Wicksellian work of Michael Woodford (2003) and which is used nowadays by central bankers. The model has for some time been called the New Consensus model or the New Neoclassical Synthesis. The book is targeted at intermediate and advanced undergraduate students, but as the authors say and as I would argue, it can also benefit graduate students and professional economists. The book is endorsed by well-known economists, such as Aghion, Shin, Blanchard, Goodhart, Haldane and Gertler – a rather uncommon feature for a textbook – so it must somehow be a good representation of the macroeconomic views being entertained by an important segment of the economics profession.

### 1 CARLIN–SOSKICE VERSUS THE NEW KEYNESIAN MODEL

The 3-equation model has attracted the attention of a number of post-Keynesian authors, if only because the model has embedded into it an implicit endogenous money supply with an interest rate determined by the central bank. The model has been described and criticized in many papers by Philip Arestis and Malcolm Sawyer (2002), and it has given rise to a book edited by Giuseppe Fontana and Mark Setterfield (2009), which contains a chapter by Carlin and Soskice as well as a number of chapters by post-Keynesian authors that modify or extend the 3-equation model. This edited book has generated a controversy that filled the pages of this journal, as Sebastian Dullien (2010) expressed his surprise at the fact that post-Keynesians would find New Consensus models appealing. He claimed that the New Consensus model as found in its sophisticated versions – the dynamic stochastic general equilibrium (DSGE) models – has little relationship with either the 3-equation model described by Carlin and Soskice or with post-Keynesian economics. He contended that, at best, DSGE models were real business cycle models with price rigidities – the New Keynesian Consensus model – where changes in output and employment were driven by the decisions of workers to enjoy more or less leisure, as real wages decreased or rose temporarily, with the labour market always being in equilibrium. Thus, for instance, a cut in interest rates leads to higher activity, not because it pushes up investment demand, but because it ends up inducing workers to consume and work more today relative to tomorrow. Thus ‘the central variation in output in DSGE models ... is caused by variation of the labour supply of households, not labour demand as in the traditional Keynesian story’ (Dullien 2010: 268).

Carlin and Soskice devote an entire chapter (the last one) to distinguish their version of the 3-equation model (the 3-ECS model from now on) from the real business cycle model and the New Keynesian Consensus model. While the New Keynesian and 3-ECS models share real and nominal rigidities, due to imperfect competition and sticky prices, with both models thus requiring an interventionist central bank in order to stabilize the economy and bring it back towards equilibrium, the New Keynesian model assumes in addition that the economy is populated with agents that have forward-looking rational expectations and an intertemporal optimization behaviour. Thus, as pointed out by Dullien, the labour supply drives the fluctuations of employment (with only 'voluntary' unemployment), while the New Keynesian Phillips curve has current inflation depending on the inflation rate that is expected to prevail one period ahead (instead of the past inflation rate or the current expected inflation rate).

Carlin and Soskice, besides arguing that their 3-ECS model is more pedagogical, claim that their model is more realistic because in their formalization there can be involuntary unemployment (a claim to be discussed below) and because they assume that only the central bank and financial agents operating on foreign-exchange markets have a forward-looking behaviour and thus are able to 'solve the model', although they end up recognizing that the differences between their model and the New Keynesian model used by central bankers 'are somewhat blurred' when the latter are forced to introduce ad hoc adjustments to capture persistence in inflation and output (Carlin/Soskice 2015: 611).

## 2 INVOLUNTARY UNEMPLOYMENT

In my view, the crucial characteristic of the 3-ECS model is that employment in the short run is determined by aggregate demand in the goods market (formalized in the so-called IS curve, where output depends on the real rate of interest, as in the IS curve of the IS–LM model), as it would be in a post-Keynesian model, whereas employment in the long run (or what Carlin and Soskice like to call the medium-run equilibrium to distinguish it from the equilibrium arising in a growth model) is determined by the supply side, through a *vertical* Phillips curve with a non-accelerating inflation rate of unemployment (NAIRU).

In the 3-ECS model, a rise in aggregate demand generates an increase in output and employment because 'firms do not change their prices at all in response to changes in aggregate demand' (Carlin/Soskice 2015: 64). In the next period, however, wage inflation will speed up, presumably because the bargaining power of workers has risen or because firms feel they have to grant additional wage increases because 'workers will need to expect higher real wages in order to exert effort' (ibid.: 73). In due course, this will also generate accelerating inflation as firms react to unit cost increases. This part of the model is also attractive to post-Keynesians, as it assumes that firms respond to changes in aggregate demand by changing quantities, and because 'rising inflation reflects distributional conflict as different social groups (wage setters/employees and price setters/employers) seek to protect their interests' (ibid.: 85).

As argued by Engelbert Stockhammer (2008), the presence of a NAIRU in a model does not irremediably disqualify the model from a post-Keynesian standpoint, as long as this NAIRU is either unstable or responsive to the actual rate of unemployment. In the 3-ECS model, the central bank sets its policy rate in such a way as to achieve a NAIRU at the target rate of inflation, the monetary reaction (MR) curve thus being *the* mechanism that brings the economy back to the medium-run equilibrium rate of employment (ERE). Thus, roughly speaking, the consequences of the model are similar to those of a Friedmanian model of employment and inflation, where however the

money supply is endogenous and where the central bank plays the role that used to be assigned to the real-balance effect. Any attempt to take the unemployment rate below the NAIRU will lead to accelerating inflation and bring about a period of below-normal GDP as the central bank will be forced to pursue austerity policies to combat rising inflation as 'workers' real wage expectations are constantly frustrated when output is above equilibrium' (Carlin/Soskice 2015: 73). With a vertical Phillips curve, set at a unique rate of unemployment free of any influence arising from aggregate demand, many of the Keynesian aspects of macroeconomics are necessarily gone, despite the presence of endogenous money and a demand-led IS curve; there is very little alternative (TIVLA)!

The medium-run equilibrium rate of employment is to be found at the intersection of the price-setting and wage-setting curves, the PS and WS curves, which were at the heart of the LSE and OECD supply-side models of employment, as developed by Layard et al. (1991) and which were so well criticized by Godley/Anyadike-Danes (1989). The PS curve is supposed to represent the standard profit-maximizing condition, with the equality between the real wage and the marginal physical product of labour, but Carlin and Soskice assume for simplification that this PS curve is flat, either because variable unit costs and hence marginal costs are constant, or because imperfectly competitive firms set prices on the basis of normal unit costs. Thus this part of the labour market seems also to be compatible with post-Keynesian microeconomic theory.

But what about the WS curve, also called the 'no-shirking curve'? It relies on asymmetric information and on the Marxian idea, developed by Bowles et al. (1984), that a higher aggregate level of employment will induce workers to deploy less effort or will induce some workers to deploy no effort at all, so that profit-maximizing firms will need to raise real wages to make sure that the cost of losing one's job is high enough to discourage workers from shirking. At the intersection of the PS and WS curves, firms will be fulfilling both profit-maximizing conditions. For Carlin and Soskice, any discrepancy between the level of employment so defined and the supply of labour at the real wage rate determined by the PS flat curve is thus characterized as *involuntary* unemployment (Carlin/Soskice 2015: 57) – an assessment that Stockhammer (2008: 488) endorses. This 'involuntary unemployment', or the NAIRU, thus depends on supply-side features, with episodes of *temporary* demand-led increases or decreases in unemployment. The PS–WS apparatus thus determines in the medium or long run the equivalent of the long-run *vertical* aggregate supply curve of the standard AS/AD model.

The advantage of the PS–WS apparatus is that it allows Carlin and Soskice to come up with the usual neoliberal statements about the negative impact caused by the lack of labour market flexibility, here measured by the slope of the WS curve and by its intercept. If unemployment benefits or their duration, or any other social benefit, could be lower, employment would be higher and the NAIRU would be lower; weakening labour unions and getting rid of labour market regulations or job protection measures would also reduce the NAIRU (Carlin/Soskice 2015: 58), although it is later pointed out that empirical work, partly done in the past by Soskice himself, does not make this a foregone conclusion (*ibid.*: ch. 15). Finally, an increase in the tax wedge (income tax rates or payroll taxes) shifts the PS curve down (the after-tax real wage rate is lower), thus leading to a fall in the medium-term level of employment and a rise in the NAIRU. Thus, the lessons from this model are clear: to improve the labour market situation, reduce tax rates, reduce social benefits, and weaken trade unions. This is not what post-Keynesians, for instance Storm/Naastepad (2012) or Mitchell/Muysken (2008), would argue for. On the other hand, there is some similitude between Carlin and Soskice and Kaleckians in the claim that more competition in the goods market could bring about lower mark-ups and hence higher real wages and higher levels of employment. The difference is that the

Kaleckian mechanism is demand-led and based on the higher propensity to spend of workers, whereas the WS–PS framework relies on the idea that the higher real wage rate will allow firms to maximize work effort at a higher rate of employment.

Finally there is the whole issue of the claim that an efficiency–wage theory can define involuntary unemployment. It is true that one could argue that if firms could just accept lower profit margins, there would be more employment, hence unemployment is no fault of the workers and is not caused by overly high real wages, in contrast to previous mainstream explanations of unemployment. But the real problem seems to be that workers are always on the verge of loafing. If there was no shirking on the part of workers, any point on the flat PS curve would be an equilibrium from the standpoint of the firm: an increase in aggregate demand, within the framework of such a model, would not lead to an increase in inflation and would not induce a negative reaction from the central bank until the PS curve intersects the labour supply curve, thus bringing the NAIRU equilibrium at the full-employment level. One may say that in some sense unemployment is involuntary, that firms are responsible and that workers are victims, but ultimately unemployment is caused by the bad behaviour of (some of) the workers, so the fault is all theirs, as employers are simply trying to avoid the detrimental consequences of the shirking and quitting of their employees (De Vroey 2004: 196–197). There is thus some similitude with the mainstream justifications for persistence or hysteresis in labour markets, where the blame is put on unions and insiders or when such phenomena are attributed to the behaviour of laid-off workers who decline to keep up their skills while unemployed. Solutions to these problems are always found on the supply side, whereas Keynesian involuntary unemployment requires demand-side solutions – a modification of income distribution, an increase in government expenditures or the government as an employer of last resort.

### 3 OPEN-ECONOMY MACROECONOMICS

An important and substantial contribution of Carlin and Soskice is their extension of the 3-equation model to the case of the small open economy. Whereas one is in quite familiar territory when the closed-economy case is being discussed, the open-economy variant is much more innovative but also much more complicated, as can be assessed by a comparison with the 3-equation model of Romer (2013). As in the closed-economy model, interest rates affect aggregate demand with a lag, and so does the real exchange rate. However, it is assumed that participants to foreign-exchange markets are forward-looking and fully rational: ‘the profit-seeking motivation of foreign exchange traders will produce an outcome that can be captured by the rational expectations assumption’ (Carlin/Soskice 2015: 308). But as the authors themselves recognize, this is a dubious assumption to make, as ‘we know that financial markets are affected by fads and manias’ (ibid.: 308). Their justification for doing so is that ‘it provides a useful basic model of a very complex situation’ (ibid.: 308). Carlin and Soskice further assume perfect substitutability between foreign bonds and home bonds. But the key assumption is the relevance of the uncovered interest parity (UIP) condition.

Whereas a good deal of empirical evidence, even if somewhat controversial, is being put forward to support several of the main components of the book, none is being advanced in support of the UIP condition. Apart from a theoretical argument about interest-rate arbitrage, the only piece of evidence is a survey that says that traders react to a change in interest rates within one minute (ibid.: 312). But this does not say how they react. An increase in interest rates could signify that the economy will enter into a recession and hence that it would be best to move one’s marbles to another country or region where the stock market

is more likely to produce capital gains, as was often observed in the early 2000s when the European Central Bank was raising interest rates. But Carlin and Soskice interpret this as meaning that the domestic currency will immediately appreciate by the full percentage value of the interest-rate increase, and then that it will gradually depreciate to a similar extent during the course of the year, so as to achieve UIP. The one-time brisk appreciation of the currency in and of itself is not a problem, as a very similar result is being achieved in the Godley/Lavoie (2007: ch. 12) stock-flow consistent open-economy model. The problem is assuming that UIP holds and hence that, barring an expected appreciation or depreciation of the exchange rate, it is impossible for domestic interest rates to be any different from world interest rates. In reality, what holds is the covered interest parity (CIP). The UIP condition and the unbiased efficiency hypothesis do not hold: the forward exchange rate is no indication of the future spot rate.

Carlin and Soskice provide an interesting discussion about how export prices are set, based on home-cost pricing or based on world pricing. In the second case, when exchange rates change, firms absorb losses or take advantage of higher profit margins; in the first case, there is a 100% pass-through. The authors assume the latter. This means that a depreciation of the domestic currency implies a higher consumer price index, thus leading to a decrease in the real wage of workers, in terms of the basket of consumer goods that now includes imported goods. Thus, keeping in mind the PS–WS model, this implies that the medium-run equilibrium rate of employment will be lower – a result that seems best to fit semi-industrialized countries where the pass-through is very high, and where devaluation is indeed accompanied by a fall in the wage share. I must admit that I was left puzzled by the complexity of the full open-economy model and thus will say no more about it despite lengthy developments in the book.

#### 4 THE CRISIS AND THE FINANCIAL SYSTEM

The authors point out in the introduction that the new version of their textbook is a reaction to the global financial crisis. The typical response of macroeconomics instructors has been to devote more time to the banking and financial systems. A considerable amount of space is devoted to this task. Carlin and Soskice briefly mention that the money supply is endogenous. As I had done myself previously (Lavoie 2009), they add a twist to their 3-ECS model by introducing a lending interest rate, which is assumed to equate the policy rate set by the central bank plus a mark-up, in analogy with the way goods are priced on the basis of unit costs. The mark-up is said to depend on the risk of the borrower, the bank's tolerance for risk, and the bank's equity-to-asset ratio. I found that the story told by Carlin and Soskice (2015: ch. 5) about credit and money creation was consistent with that conveyed by post-Keynesian or circuitist authors, with the initial creation of credit and new investment generating saving and bank deposits (an exception being a passage on p. 491, where the authors imply that it will be easier for banks to make loans if they hold excess reserves).

The explanation for a financial crisis associated with excessive bank lending and a real-estate crash is based on the paradox of tranquillity highlighted by Hyman Minsky. Carlin and Soskice (*ibid.*: 213) describe this as a period in which 'when everyone believes risk has gone down, they behave in such a way that makes the system riskier'. They call this the paradox of credibility. They emphasize the role of the financial accelerator, with more credit being granted to borrowers generating rising real-estate prices, thus leading to higher collateral values, and hence more bank credit; a similar process occurs on the side of the banks, as their retained earnings rise, and as this is amplified by more leverage-taking.

The authors refer to the possibility of balance-sheet recessions, as put forth by Richard Koo (2013), which is in line with the post-Keynesian view.

As one would expect from authors who rely on the vertical Phillips curve and a variant of the neo-Wicksellian model, the inability of the monetary authorities to provide a remedy to the recession caused by the financial crisis is blamed on their inability to achieve a policy interest rate which is negative enough in real terms to achieve the equilibrium rate of employment under those dire circumstances. This is no different from Patinkin's (1948) explanation, according to which the investment and full-employment saving functions intersected at a negative real rate of interest, so neoclassical theory and the belief in the existence of a natural rate of interest have not changed since. In modern terms, the central bank is unable to be on its reaction function curve. This, as is now well known, is due to the zero lower bound and to the large interest-rate spreads that disconnected the policy rate from the lending rate. Quantitative easing is thus presented by the authors as a means to achieve a flattening of the spreads and of the yield curve, in the hope of achieving low enough lending or long-term interest rates. Carlin and Soskice (2015: 489) seem to be persuaded by empirical research showing that quantitative easing had a significant impact on real GDP, both in the US and in the UK.

An entire chapter is devoted to the eurozone and its peculiar common currency area. Carlin and Soskice explain that eurozone members faced with country-specific shocks are left without a monetary reaction channel, and must therefore rely on the stabilizing forces of the external competitiveness channel to go back to equilibrium, while endogenous changes to real interest rates will bring the country away from equilibrium. Thus, in such a set-up, fiscal policy becomes more important. The authors recall that the economic indicators of the UK and Spain were not much different in 2010, when the eurozone sovereign debt crisis started. Still, Spain suffered from a crisis of confidence in the sustainability or liquidity of its sovereign debt, while the UK did not. They conclude, as would neo-Chartalist authors, that 'the taxpayer base is the ultimate guarantee of the solvency of the government and of the central bank's ability to buy government bonds in unlimited quantities. ... The big difference is that for a European member, it could not rely on a lender of last resort to support its bond sales if required' (ibid.: 456–457). The authors thus welcome the decision of the ECB to modify its stance with regards to its links with governments, while deploring the conditionality attached to Outright Monetary Transactions programmes.

## 5 SUNDRY ISSUES

I close this review with a few miscellaneous issues. Carlin and Soskice seem to hold on to the mainstream view that discretionary fiscal policy should be a move of last resort, only validated in the case of deep recessions. This is so despite the fact that they do support the notion of a balanced budget unitary multiplier and that they do seem to believe that government multipliers are in the range of 1 to 1.5. On the Ricardian equivalence theorem, the authors think that the world economy is becoming more and more subjected to it, thus leaving even less room for discretionary fiscal policy in the future. They also provide a good deal of space to the now-discredited belief in expansionary fiscal consolidation, leaving the reader with the conclusion that 'fiscal consolidation is likely to be beneficial for GDP in the long term' (ibid.: 530). This rather conservative view of fiscal policy is reinforced when they argue that 'monetary financing is a last resort and a state of extreme political dysfunction that only arises when the government cannot raise money through taxation or borrowing', adding that 'the creation of new money by

the central bank ... increases the growth rate of the money supply, which, if maintained, ultimately causes inflation to rise' (ibid.: 517). These statements seem to be a remnant of a monetarist viewpoint, and are inconsistent with the endogenous money supply view.

As is traditional in mainstream macro textbooks, Carlin and Soskice also devote one chapter to growth models, more specifically the Solow growth model, with a few pages devoted to the endogenous growth models of Romer and of Aghion-Howitt. The presentation of the Solow model, starting with a no-growth situation, is very clear, as the authors underline the key assumptions of the model, in particular the assumption that saving is identically invested. An increase in saving leads to a higher stock of capital, and hence, through the standard properties of the neoclassical production function, to a lower rate of return (and a lower real rate of interest) as well as a higher real wage rate and employment level, as the PS-WS model would tell us. There is no hint that the Cambridge capital controversies could jeopardize this nice story. Carlin and Soskice, however, connect the Solow model to the 3-equation model. They point out that in the real world the paradox of thrift will kick in, so that a higher saving rate will generate a drop in aggregate demand. The authors tell us that if the central bank understands the Solow model and the paradox of thrift, it will immediately lower the real interest rate, so as to generate the required increase in investment. What they are really telling us is that the central bank will understand that the Wicksellian natural rate of interest has fallen, thanks to the greater thriftiness of agents, and that as a consequence it must lower the policy real rate of interest in line with this natural rate. But how can the central bank distinguish between a drop in bond rates associated with a decrease in the natural rate and one due to a change in liquidity preference?

In conclusion, this book is certainly worth reading. It is an impressive piece of work. It provides many technical algebraic details that are useful to students of macroeconomics. It offers a clear depiction of an apparently reasonable description of macroeconomic theory. It demonstrates the appeal of neoclassical theory, with its neat formalization. It does mention Keynes, Minsky and radical uncertainty, and even offers a critique of its own key assumption – the existence of a NAIRU, independent from any influence from aggregate demand – by suggesting the possibility of hysteresis in the penultimate chapter. But this is too late: hysteresis should have been introduced in the early chapters as a key component of the model, since meta-regression analyses (Stanley 2013) provide clear evidence of unemployment hysteresis and demonstrate that expected inflation does not lead to a one-on-one increase in the rate of inflation, in contrast to what is assumed in the textbook; however, this would substantially modify the neat apparatus created by Carlin and Soskice, as well as their conclusions.

Dullien (2010), despite his misgivings about the Fontana/Setterfield (2009) book, had hoped that Carlin and Soskice's peculiar 3-equation interpretation of the New Consensus model could provide an opportunity for improved communication between orthodox authors and post-Keynesian economists. The chapters in Fontana and Setterfield's book did provide a number of possible formalized or graphical amendments to the 3-ECS model, which would have modified many of its Friedmanian conclusions. Unfortunately, 5 years later, only one of them – the distinction between the policy rate and the lending rate – has been adopted by Carlin and Soskice (2015). And so, the book, despite all of its refinements and some of its more realistic assumptions, as well as its introduction of an endogenous supply of money, still appears as a defence of orthodox economics as it first got redefined with the arrival of the monetarist counter-revolution and its extension by Lucas's new classical economy, based as it is on the concept of a unique natural rate of unemployment – defined as a strong attractor of the actual rate of unemployment – accompanied by a unique natural rate of interest.



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