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An update on Kalecki–Minsky modelling

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Within post-Keynesian macroeconomics, the Kaleckian streams analysing demand regimes and the Minskyan streams analysing financial instability have proceeded relatively independently. To develop a comprehensive alternative to mainstream economics, post-Keynesians will have to integrate Kaleckian and Minskyan arguments with an analysis of the supply side. This paper gives a partial update on advances in Kalecki–Minsky modelling. First, theoretically, it discusses the notion of pseudo-Goodwin cycles. Second, it presents empirical findings on endogenous debt cycles. Third, it reviews evidence of the relative size of distributional and financial effects on demand. Fourth, it reports results on the drivers of household debt.

Keywords: *post-Keynesian economics, Kalecki, Minsky, debt cycles, household debt*

JEL codes: *E12, E32, E44*

1 INTRODUCTION

The global financial crisis has exposed the weakness of mainstream macroeconomics. One might have thought that it would also lead to increasing interest in post-Keynesian (PK) theory. However, there is hardly any evidence that academic macroeconomics has opened up. Certainly, in combination with austerity policies and the cutting of higher education budgets, non-mainstream economists find it at least as difficult to find permanent university positions or get promoted. Little has changed in academia. However, from the side of the Rethinking Economics student movement (for example, Fischer et al. 2018), and to some extent from the social sciences, there is indeed growing interest. In particular within International Political Economy, which is at the intersection of political sciences and economics, but typically housed in politics departments (Cohen 2008), there is a growing recognition of the shortcomings of the field's reception of economics (Blyth/Matthijs 2017) and a growing recognition of the significance of post-Keynesian economics (PKE). Baccaro/Pontusson (2016) suggest basing comparative political economy, in which the supply-side-oriented Varieties of Capitalism school is presently predominant, on Kaleckian macroeconomics and growth models complemented by a Gramscian-inspired analysis of the state.

It is an interesting question why that is and why there has not been more reception within the economics discipline and policy-makers. A big part of the answer probably lies in the political power and ideology, a part in the organizational structure of universities and the self-reproduction of academic departments, which has allowed research to be considered of excellent quality because it has been published in the leading journals even if the underlying models are substantively irrelevant or misleading. A smaller part of the reason why PKE finds it so difficult to get out of its current niche is that, first, while PKE has indeed most of the ingredients for an analysis of the financial crisis and the extended stagnation thereafter, important parts of the macroeconomic debate, namely the Kaleckian and Minskyan streams, have proceeded relatively independently of each other. PKE has

proceeded allowing ‘horses for courses’ (as has been said in a somewhat different context).¹ As a consequence there are few medium-scale PK models that cover distributional, financial and supply-side aspects. Second, the empirical application of PK models has progressed rather unevenly. Compared to where PKE was, say, 20 years ago, a lot more empirical work is produced now and for the younger cohorts of post-Keynesians econometric analysis has become routine. However, while there are some segments of the PK research agenda where there is an extensive, if at times narrow, empirical literature (for example, on demand regimes or balance-of-payments-constrained growth models), there are other parts (such as models of financial instability) where empirical work is scarce.

Stock–flow consistent (SFC) modelling has produced large-scale models that, through their scope, have to cover goods markets, labour markets, financial markets and the production side. However, in fact most of these models are theoretically oriented and focus on particular aspects, mostly financial issues. SFC models also tend to be so large that they cannot be solved analytically. In my view, their main contribution has been to highlight the central role of some key (financial) stock variables, namely debt, wealth (and, by implication, asset prices) for macroeconomic outcomes, but they have, effectively, not contributed to creating a PK synthesis.

Working towards a PK synthesis, or as we shall refer to it, a Kalecki–Minsky model, is challenging. This paper gives a highly partial (and self-referential) update on advances in Kalecki–Minsky modelling. From the beginning we should be clear that the label is somewhat arbitrary; we could also call it post-Keynesian or Kaldor–Minsky, each of which may entail their own sources of potential confusion or misunderstanding. At the core I’m interested in macroeconomic theory that gives a central role to income distribution and to financial factors. To be more precise, models that allow for the possibility of wage-led demand regimes as well as endogenous financial cycles. The former are associated with Kalecki, the latter with Minsky, thus the label Kalecki–Minsky model. The model should have a third feature, which is that it can give rise to hysteresis or path-dependence, but for reasons of space, this aspect is outside the scope of this paper.

2 A BIT OF BACKGROUND

Before the crisis at least, there was a reasonably well defined mainstream in macroeconomics. For the context of policy-making this was expressed in the New Consensus model (NCM), which had three building blocks: a reduced-form sort-of IS curve, where output is a negative function of the interest rate, a Phillips curve, which relates the change in inflation to output, and a monetary policy response function. On the academic side the mainstream was represented by the New Keynesian dynamic stochastic general equilibrium (DSGE) model, that is, a real business-cycle model, with representative-agent microfoundations and nominal rigidities (Gertler et al. 1999; Smets/Wouters 2003). The two are consistent and the three-equation NCM is often regarded as a reduced form of the full NK-DSGE model. Today the mainstream is more fluid in that there is mounting criticism of the NCM and NK-DSGE, but there is no canonical model to replace it.

1. The notion ‘horses for courses’ has been used in the debates on the logical coherence of PKE as an argument for a broad-tent post-Keynesianism that encompasses a monetary Keynesian, Kaleckian and Sraffian tradition (King 2002: ch. 10). My point here is that this has also meant that debates were relatively independent and thus there is a lack of synthetic models.

There are several thorough PK criticisms of the NCM and NK-DSGE available (Lavoie 2004; Arestis/Sawyer 2008; Dullien 2009): the model does not have a substantive role for the financial sector, bankruptcy and financial instability, it has no role for income distribution and it assumes a stable supply-side-determined equilibrium in the long run. A PK alternative model will have different features: on the foundational level it will be based on behavioural heuristics rather than global optimizing behaviour and it will have heterogeneity among agents, in particular in terms of the class structure (for example, workers vs capitalists), but also within classes with different behavioural rules and in particular expectation formation. In terms of the macroeconomic structure, it will have demand regimes with an active investment function and the possibility of a wage-led demand regime (due to the savings differential between workers and capitalists), it allows for endogenous cyclical dynamics, in particular ones that emanate from the financial sector and, in the long term, the supply side responds to demand (either due to an endogenous inflation-neutral unemployment rate or through endogenous productivity growth). While the overall vision of such a PK macro model has been outlined (Stockhammer 2018) and various partial models are well covered, there is a lack of synthetic models that encompass all three elements.

Let's consider these ingredients, which brings us to Kalecki and Minsky. First, regarding the role of income distribution, the Bhaduri/Marglin (1990) model has become a key reference point for PK modelling of the relation between distribution and demand. It allows for wage-led as well as profit-led demand regimes according to, in a closed economy, the relative size of the savings differential between capital and workers and the profit share sensitivity of investment. The model has become a key reference point because theoretically it builds a bridge between Keynesian/Kaleckian arguments of the core role of wages as a source of aggregate demand and Marxist/classical arguments (which highlight the role of profitability for investment), and because it has provided a useful platform for empirical research and has given rise to a substantial number of empirical studies.

Second, theories of financial instability within the PK tradition are strongly associated with Hyman Minsky, who had long and consistently argued that financial instability is a pervasive and endogenously created feature of capitalist economies. Minsky's writings are complex and highlight the role of optimistic outlooks on the future during a tranquil phase, and the rise of indebtedness and financial vulnerability during a boom; rising debt burdens and cooling optimism as elements of the downturn, which will then lead to an increase in liquidity preference, portfolio recompositions, an attempt to deleverage and fire sales of assets resulting in collapsing asset prices. However Minsky did not provide a baseline model for these mechanisms. Nikolaidi/Stockhammer (2017) survey a heterogeneous literature that builds on Minsky's insights. There is a rich theoretical literature modelling Minskyan arguments, but there is no canonical model.² They identify two broad families of Minsky models that then branch into further versions. In terms of the key mechanisms that give rise to endogenous cycles, Nikolaidi/Stockhammer (2017) group the literature into those where debt plays the key role (for example, Asada 2001; Charles 2008) and those where asset prices are paramount (for example, Taylor/O'Connell 1985; Ryoo 2016). Within the debt-cycle group there are differences in the precise mechanisms, with some authors emphasizing the role of interest rates (Fazzari et al. 2008) and others endogenous debt norms (Jump et al. 2017; Dafermos 2018). While Minsky's original

2. There are substantial differences both on the core mechanisms of endogenous cycles and on the modelling strategy. Skott (1994) and Charles (2008) use small-scale macro models; Nikolaidi (2014) and Dafermos (2018) use large-scale stock–flow consistent models; Delli Gatti et al. (2005) and Chiarella/Di Guilmi (2011) use heterogeneous-agent models.

focus has been on business debt, several authors have formulated Minskyan models with a focus on household debt (for example, Ryoo 2010; 2016; Kapeller/Schütz 2014). In the second family, where asset price dynamics are at the core, the cyclical dynamics are caused by the interaction of two different types of expectations, sometimes referred to as fundamentalists (or mean reverting expectations) and momentum traders (also: noise traders) (see Franke/Westerhoff 2017).

A third ingredient of the PK alternative is the interaction of demand and supply. PKs have long argued that supply responds to changes in demand. This operates either via an endogenous non-accelerating inflation rate of unemployment (NAIRU) (Skott 2005; Stockhammer 2008) or via endogenous productivity growth (Storm/Naastepad 2013; Fazzari et al. 2018). As a result, supply adjusts over longer periods to demand, and the natural rate of output or growth is endogenous (Setterfield 1993; Dutt 2006; Lavoie 2009). However, in the interest of brevity, we will not elaborate on this aspect here, but merely note that it forms part of the PK or Kalecki–Minsky vision.

We note that unlike the Bhaduri–Marglin model for the analysis of the interaction of distribution and demand, there is as yet no canonical Minsky model for the analysis of the interaction of demand and finance. There is also a lack of systematic discussion of how demand, distribution and finance interact. This paper will explore some of the theoretical and empirical features of this model. Ryoo (2010; 2013; 2016) to some extent covers similar ground. Ryoo presents difference versions of Minsky models and inserts them in a PK macro framework. He distinguishes between Kaleckian and Kaldorian closures, which refers to the local instability of the goods market. In contrast, we associate Kaleckian models mainly with wage-led demand regimes. Ryoo analyses real and financial cycles separately, arguing that they have different frequencies. We will be arguing that pseudo-Goodwin cycles can arise in a wage-led Minsky model.

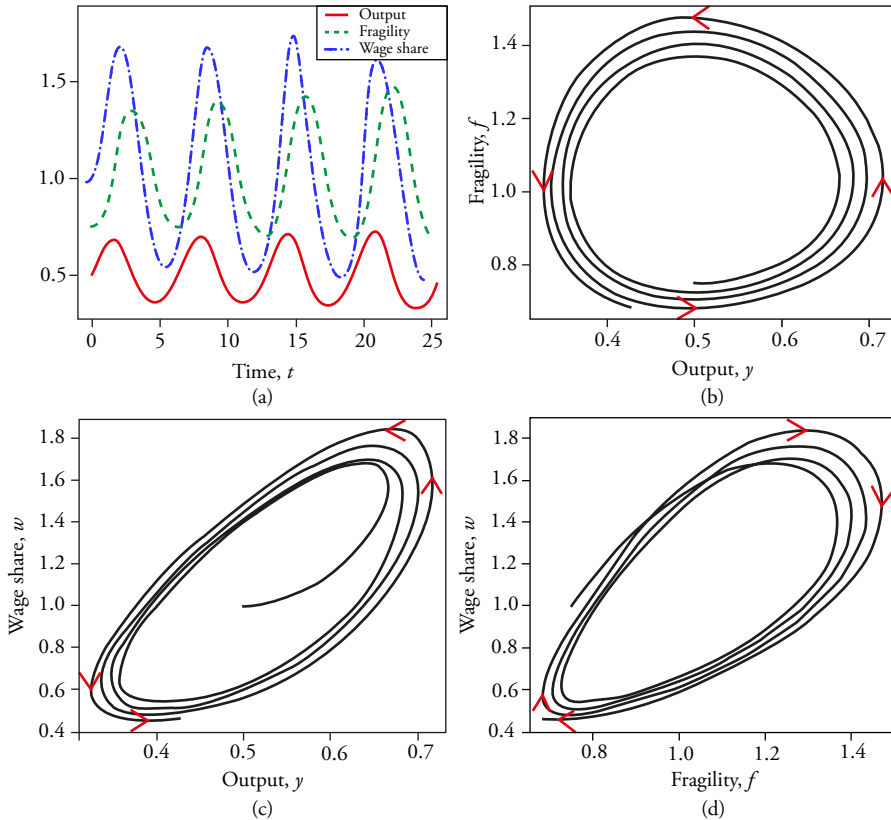
3 PSEUDO-GOODWIN CYCLES

Stockhammer/Michell (2017) analyse a particular aspect of a Kalecki–Minsky model: they analyse the relation between Minsky cycles in a Kaleckian economy and Goodwin cycles. Goodwin cycles are based on Marxist theory and the cycle emerges from the interaction of distribution and output: higher output leads to a rising wage share (because workers' bargaining power increases due to the declining industrial reserve army), and a higher wage share leads to lower investment (in Goodwin's original model all profits got reinvested). In modern formulations this is equivalent to assuming a profit-led demand regime. Goodwin cycles thus are based on a profit-led demand regime and a reserve army distribution function. Given that the Kaleckians emphasise wage-led demand, this has led some authors to counterpose Goodwin cycles of profit-led economies to anti-Goodwin cycles of a wage-led demand regime with a pro-cyclical profit share and interpret these as Kaleckian (Diallo et al. 2011; Kiefer/Rada 2015). But Stockhammer (2017) argues that that is a misunderstanding as the Kaleckian business-cycle theory was focused on investment dynamics, but is not distributionally driven.

Stockhammer/Michell analyse whether a pseudo-Goodwin model can arise in a wage-led Minsky economy. They define a pseudo-Goodwin cycle as a counter-clockwise movement in wage-share–output space that is driven by a different cycle mechanism, that is, does not rely on the profit-driven investment and reserve army interaction. Simply put, a pseudo-Goodwin cycle looks like a Goodwin cycle, but causally it is not. Stockhammer/Michell (2017) present a minimalistic three-equation model, where a Minsky component generates cycles through the interaction of a financial variable

and demand. The financial variable, easiest thought of as the debt-to-income share, positively depends on output. Demand reacts negatively to an increase in the financial variable. In a predator–prey type formulation this generates persistent fluctuations. The Minsky model is complemented by a Marxist industrial reserve army distribution equation, that is, the wage share depends positively on output (because employment, which increases workers’ bargaining power, is positively related to output). Stockhammer/Michell analyse several versions of the model, all of which generate pseudo-Goodwin cycles. In the simplest version there is no (direct) effect of income distribution on demand (in other words, the demand regime is distributionally neutral). Then a wage-led demand regime is introduced. Importantly, the model still generates pseudo-Goodwin cycles in wage-share–output space, as depicted in Figure 1. Depending on parameterization, the wage-led demand regime may lead to instability, that is, explosive fluctuations.

The generalization of this finding is that pseudo-Goodwin cycles will arise in any cyclical economy with a reserve army distribution function. In other words, income distribution



Note: These are simulations of a Minsky model with endogenous financial–real cycles, a wage-led demand regime and a reserve army distribution function. The model generates pseudo-Goodwin cycles. *Source:* Stockhammer/Michell (2017: fig. 4).

Figure 1 Pseudo-Goodwin cycles in a wage-led Minsky model

will be dragged along by output and if output behaves in a cyclical fashion, so will income distribution. The resulting co-movement will give pseudo-Goodwin cycles. An empirical implication of this is that the existence of a counter-clockwise movement of wage share and output are not conclusive evidence for Goodwin cycles or a profit-led demand regime.

Stockhammer/Michell's model is highly stylized, but has an unexpected feature that could not be readily derived from its constituent ingredients. More generally it illustrates some of the complexities that even a simple Kalecki–Minsky model can exhibit.

In Stockhammer/Michell's model, there is only one cycle-generating mechanism. Yilmaz/Stockhammer (2019) analyse an (again highly stylized) economy, which has a multiplier accelerator cycle mechanism as well as a Minskyan debt cycle. In other words, there is a 'real' and a 'financial' cycle mechanism at work, which both impact on a real activity. The paper analyses how the two interact, in particular under which conditions the parameters that give a stable cycle (more precisely: closed orbits) in isolation can generate instability or stability in the combined system. They find that the stability properties of the combined system depend on the respective periodicities of the cycle mechanisms and on the relative size of the impact on the joint variable. In other words, combining two 'stable' cycle mechanisms will usually give a stable or unstable system. If the shorter cycle mechanism has the stronger impact on the common variable, the combined system is stable. One implication of this is that if, as seems to be a stylized fact, financial cycles are longer than real cycles, an increasing weight of financial considerations will destabilize the combined system.

4 TESTING FOR ENDOGENOUS FINANCIAL–REAL CYCLES

This leads to the question of how solid the evidence for Minsky cycles is and how to operationalize them. Issues of financial stability have gained prominence. There is ample historical evidence for recurring financial crises and their strong economic impact (Kindleberger 1978; Goodhart 2016). Whereas NK-DSGE and other mainstream models typically interpret financial crises as the outcome of exogenous shocks, the Minskyan tradition highlights the endogenous nature of financial fluctuations. As discussed in Section 2, these can be grouped as debt cycles, which are based on financial–real interactions, and asset price cycles, which are based on financial–financial interactions. In the simplest version of the debt cycles, a pro-cyclical leverage ratio and a negative effect of debt on investment interact to generate cycles. In the asset price cycles, two valuation strategies interact, with momentum traders providing the overshooting force, to generate cycles. In contrast to the rich theoretical literature, there is, as yet, only a handful of empirical studies and none of these explicitly test for endogenous cycles. Palley (1994) and Kim (2013; 2016) estimate vector autoregressive (VAR) models with GDP and household debt and report positive short-run effects and negative long-run effects of household debt. Greenwood-Nimmo/Tarassow (2016) enrich a conventional monetary policy VAR to include some Minskyan arguments. They estimate a VAR, which includes several interest rates, corporate loans and share prices and which tries to identify monetary policy and macroprudential shocks. There are several empirical attempts to test specific Minskyan assumptions such as rising financial fragility (Davis et al. 2017) or the negative effect of debt on investment (Ndikumana 1999), which by design cannot evaluate endogenous cycles.

On the mainstream side, Drehmann et al. (2012) and Aikman et al. (2015) have provided evidence for the existence of financial cycles by means of univariate filtering techniques. That is, they look at financial variables (or a composite of financial variable indices) in isolation. They find evidence for the existence of financial cycles for various countries

for long historical periods. These cycles tend to be longer than regular business cycles, with roughly double their duration.

Stockhammer et al. (2018a) test for endogenous cycle mechanisms based on simple debt cycle Minsky models, where cycles are generated from the interaction of a financial and a real variable, by using a VAR methodology for seven advanced economies. They estimate a series of 2D systems and test for complex eigenvalues and whether the relevant coefficients have the predicted signs. They investigate cycles between GDP and business debt, GDP and household debt, and GDP and interest rates. They find robust evidence for GDP–business-debt cycles for the USA and Australia, and for GDP–interest cycles in the UK and Canada,³ but no evidence for GDP–household-debt cycles. Stockhammer et al. (2019) find evidence for GDP–business-debt cycles for the USA with long historical data.

This provides evidence for the existence of Minsky cycles in the form of business-debt–GDP cycles, in particular for Anglo-Saxon countries. The lack of evidence for cycles based on household debt is intriguing. At first sight it fits uneasily with the central role that household debt (rather than business debt) played in the 2008 global financial crisis as well as with evidence that higher household debt leads to deeper recessions (Mian et al. 2017). However, it is important to keep in mind that what Stockhammer et al. test for is whether household debt and GDP interact such as to give rise to endogenous cycles (the answer is negative), not whether household debt has any effects at all.

5 EXTENDING BHADURI–MARGLIN: THE SIZE OF DISTRIBUTIONAL AND FINANCIAL EFFECTS

Since Bhaduri/Marglin (1990) proposed a macro model that allows for wage-led as well as profit-led demand regimes, the model has become a benchmark in PK macroeconomics and given rise to an extensive empirical literature (Blecker 2016; Stockhammer 2017). The literature differs in terms of the econometric estimation strategy, in particular reduced form vs behavioural equations, but few studies control for debt or other financial variables (as an exception: Onaran et al. 2011). This is reflective of the fact that the Minskyan and Kaleckian traditions have proceeded relatively independently of each other. This may be due to attempts to keep models tractable and simple, but has weakened the PK's ability to present comprehensive models.

Stockhammer/Wildauer (2016) and Stockhammer et al. (2018b) present empirical Bhaduri–Marglin-inspired analyses that cover distributional as well as financial factors. Stockhammer/Wildauer analyse the period since 1980 for a panel of OECD economies and are particularly interested in the boom that preceded the global financial crisis. They estimate consumption and investment equations that include the wage share and personal income inequality measures (for income distribution) as well as real estate prices, share prices, household debt and business debt. They find modest (positive) effects of the wage share, negligible effects of personal distribution, and substantial effects of household debt, business debt and real estate prices. They report positive effects of household debt on consumption and negative effects of business debt on investment.

For the pre-crisis boom, the financial effects were substantially larger than the distributional effects. In particular, for the Anglo-Saxon and Southern European countries in the decade before the crisis, financial variables explain approximately half of GDP growth, whereas change in the wage share had rather small growth contributions. This is consistent

3. Strictly speaking, Stockhammer et al. (2018a) test two necessary conditions for the cycles and test the interaction mechanism that can generate a cycle.

with other results. For example, Stockhammer et al. (2009) report an overall effect of 1 percentage point change in the adjusted wage share of 0.3 per cent of GDP on private excess demand. Wage shares only vary by a few percentage points over the business cycles and the secular decline of the wage share from 1980 to the early 2000s was around 10 percentage points in European countries. In contrast, during a real estate boom, real estate prices can double over a decade. With a conventional wealth effect in consumption of 0.05, this would give substantially larger financial effects. The effect of real estate prices on investment seems even larger. Stockhammer/Wildauer report positive effects of household debt on consumption that would further amplify these effects. In short, these results suggest that financial effects are large compared to distributional effects (given the variability of the underlying variables).

Stockhammer et al. (2018b) analyse distributional and financial effects on investment and consumption for a long historical period based on the Piketty/Zucman (2014) data set, which reports private financial wealth for the USA, the UK, France and Germany. Thus different from Stockhammer/Wildauer, Stockhammer et al. (2018b) only use a single financial variable and thus cannot distinguish between asset price and debt effects. They find wage-led domestic demand regimes for all countries. Wealth effects differ by country: for the USA and the UK the effect of wealth on consumption is positive, whereas that on investment is negative. For Germany and France, wealth effects on consumption are small and those on investment are positive. This suggests that national financial institutions do play an important mediating role and that negative financialization effects on investment have been a long-standing feature of market-based financial systems.

One interesting detail in the findings of both Stockhammer/Wildauer (2016) and Stockhammer et al. (2018b) is that they report positive or non-negative effects of the wage share on investment, which is at odds with the standard assumption of the Bhaduri–Marglin model. Stockhammer et al. (2018b) provide further evidence for corporate investment (for the UK and France, where data are available) and find the expected negative effects. To interpret these findings, it is important to keep in mind that investment consists of business investment and residential investment. The two are likely to have different determinants. The standard argument of negative effects of the wage share on investment refers to business investment. In contrast, the wage share can have a positive effect on residential investment, if a working-class household owns real estate property. A higher wage share can lead to higher mortgages and higher investment. Unfortunately the distinction between business and residential investment has been neglected by heterodox as well as mainstream macroeconomics. Leamer (2007) highlights the importance of residential investment for economic performance for the USA. Stockhammer et al. (2018b) suggest that one implication of the distinction is that the effect of wages on investment need not be negative.

6 THE DRIVERS OF HOUSEHOLD DEBT: REAL ESTATE PRICES AND CONSUMPTION CASCADES

In most of the work of Minsky and the literature that develops this further, the core variable for financial instability is business debt. Businesses invest and in times of optimism accumulate debt. In contrast, during the global financial crisis household debt and mortgages, and the derivatives on these, have been at the centre. It is fair to say that both in mainstream and heterodox economics household debt has not featured prominently in the analysis of financial stability. Thus, in recent years, this has become an area of intense research. As the neoliberal period was also one of rising inequality, the relation between

income inequality and household debt has been much debated. There are two arguments that claim that rising inequality has contributed to household debt. Building on Frank et al.'s (2014) notion of upward-looking consumption norms, van Treeck (2014) and Kapeller/Schütz (2014) have argued that rising top income has led to a consumption boom as those below the super-rich are trying to emulate their consumption behaviour. The middle classes then try to emulate the consumption pattern of the rich. The result of this is a consumption cascade, where rising income inequality at the top results in increased consumption throughout the income distribution. This argument contrasts with standard Kaleckian hypothesis that rising inequality leads to less (aggregate) consumption as the rich have higher marginal propensity to save. Redistribution to the top leads to an increase in the average propensity to save. In contrast to such an inequality-driven explanation of household debt, explanations based on speculative bubbles on housing markets emphasize that debt is primarily mortgage debt, which responds to real estate prices.

Stockhammer/Wildauer (2016) and Moore/Stockhammer (2018) investigate these arguments for a panel of OECD countries. They analyse the effect of changes in inequality, real estate prices, interest rates and financial regulation on household debt. Stockhammer/Wildauer (2016) use a panel of 13 OECD countries for the period 1980–2011. They find that real estate prices, interest rates and financial deregulation have the expected signs and are statistically significant, whereas income inequality, which is measured by the top 1 per cent income share and by the Gini coefficient of personal income distribution, does not have robust or statistically significant effects.⁴ Real estate prices are the largest single explanatory factor of changes in household debt. Stockhammer/Wildauer (2016: 108) also report that when the sample is split, real estate price effects are substantially larger in countries with highly developed financial sectors. While the real estate price elasticity of household debt is close to unity in the sample of countries with highly developed financial sectors, the elasticity is below 0.4 for countries with less developed financial sectors. This suggests that financialized economies have created a much tighter link between real estate prices and household debt.

These results are in contrast to Perugini et al. (2016), who report positive effects of inequality on household debt in a panel analysis, however they do not control for real estate prices (and their dependent variables include business debt). Klein (2015) also reports a positive relation, based on bivariate cointegration analysis, which is likely to suffer from omitted variable problems. Belabed et al. (2018) and Kumhof et al. (2012) investigate the effect of rising inequality on the current-account balance and find that rising inequality leads to current-account deficits. Again, they do not control for real estate prices.

The explanation that these findings suggest is one where speculative dynamics in the housing market along the lines of Dieci/Westerhoff (2012) and Shiller (2015) drive real estate prices and feed into real economic activity, consistent with our leaky mental accounts or wealth effects, and also explain rising household sector indebtedness in OECD countries. Such speculative dynamics have been supported by loose monetary policy and credit market deregulation while the distribution of income only plays a minor role in explaining household indebtedness.

4. Interacting the top 1 per cent income share with the measure of credit deregulation is one of the few specifications where effects are statistically significant. The effect of inequality is negative, the effect of the interaction is positive. The latter is consistent with the consumption cascade argument (the former is not), however effects are small and, economically speaking, do not explain much of the change in household debt. The coefficient of other variables are not affected (Stockhammer/Wildauer 2016: 104).

This view is sceptical towards the enthusiasm with which some heterodox macroeconomists (Carr/Jayadev 2014; Kapeller/Schütz 2014; Kim et al. 2015; Belabed et al. 2018) have embraced upward-looking consumption norms, despite some sympathy for its behavioural elements. Thus it will be useful to clarify what the disagreement with the consumption cascade argument is. There are two main points. First, the consumption cascade argument is essentially focused on consumer debt. People consume more, based on the emulation of consumption behaviour of richer households. In formal models this is expressed in the debt equation which states that the change in household debt is consumption expenditures minus the income of households. This misunderstands the nature of household debt. In fact, household debt is overwhelmingly mortgage debt and related to real estate transactions. Second, the consumption cascades argument is essentially an argument about the demand side of credit and, indeed, there is some plausibility for that. Households may wish to take out more credit because they want to emulate the behaviour of others. However, the consumption cascades argument is less convincing as a theory of credit supply. Banks will consider the income and the asset of the credit applicant. Simply put, while it is plausible that a household wants to consume in order to keep up with the neighbours, this is not a plausible motivation for the bank to lend to that household. Thus under conditions of rising real estate prices, consumption cascades might be a contributing factor, but the ultimate reason for the debt boom is the real estate boom, not the consumption emulation.

Our results endorse a view that regards household debt as an outcome primarily of asset and real estate transactions while the distribution of income only plays a minor role in explaining household indebtedness. This has important macroeconomic implications because it means that macroeconomic models have to explicitly model asset prices, and specifically real estate prices, because they have become a key determinant of macroeconomic outcomes. For Kalecki–Minsky modelling, an important implication is that the determinants and dynamics of business debt and household debt seem to differ.

7 CONCLUDING COMMENTS

This paper has given a selective report on the analysis of Kalecki–Minsky synthesis models. We have argued that the fact that Kaleckian and Minskyan debates have proceeded relatively independently of each other is to some extent a weakness which future research should aim to overcome. It turns out that merging Kaleckian and Minskyan models raises some analytical challenges and the resulting models have some properties that do not directly follow from existing (separate) Kaleckian and Minskyan models. We need research both on the theoretical properties of such synthesis models and more rigorous empirical analysis, with an eye on establishing empirical facts, that can then feed back into theoretical modelling.

On the theoretical front we have summarized work on wage-led Minsky models that can generate pseudo-Goodwin cycles. On the empirical front, we have summarized work on testing endogenous cycles. This has provided evidence for cycles between GDP and business debt (and GDP and interest rates), but not GDP and household debt. Second, at least for the recent period, financial effects on demand seem to be an order of magnitude larger than distributional effects. Third, we found that household debt booms have been driven primarily by real estate prices, but not by rising personal income inequality.

These findings vindicate the importance that Minskyans assign to the role of finance and financial cycles, but they also suggest important differences between the role of business

debt and household debt. Business debt dynamics seem to conform to debt–investment cycles, where business debt leads to lower investment, and higher growth (and investment) leads to higher business debt. However, household debt dynamics seem to be closer to speculative asset price cycles, where different expectation formation processes generate cyclical dynamics. The nature of the two financial cycles thus differs.

Much work is still to be done towards a PK synthesis model. First, this paper has discussed aspects of integrating Kaleckian and Minskyan models. A third important ingredient of a PK synthesis model is a supply side that responds to demand pressures, either via endogenous labour productivity or via changing distributional norms and thus an endogenous inflation-neutral unemployment rate. Second, the models reported here are all concerned with private-sector dynamics. For realistic applications they will need more explicit treatment of the government policies. Third, the models are closed-economy models or focusing on domestic demand. Policy-relevant models will need a more systematic treatment of the international interactions.

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