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How to promote alternative macroeconomic ideas: are there limits to running with the (mainstream) pack?

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The paper discusses the merits and risks of heterodox economists using mainstream economic models, and especially dynamic stochastic general equilibrium (DSGE) models, to promote economic policy conclusions usually found in post-Keynesian economic thought such as large fiscal multipliers, importance of distributional issues for macroeconomic stability, and the role of endogenous money creation for the explanation of economic downturns after a banking crisis. It argues that using these models can help heterodox researchers to communicate with mainstream economists and might help to further one's personal academic career, but that such a strategy comes along with the risk of having to accept other, very questionable policy conclusions and of stabilizing the use of the DSGE models in mainstream economics, hence potentially delaying a Kuhnian-type 'scientific revolution' in macroeconomics.

Keywords: *heterodox, mainstream, DSGE, austerity*

JEL codes: *A11, B50*

1 INTRODUCTION

Among heterodox economists and especially post-Keynesians, there has been a long debate about how to engage with the mainstream.¹ David Colander (Colander 2010; Colander et al. 2010), for example, has repeatedly recommended that heterodox economists engage more with the mainstream. According to him, one of the reasons why heterodox ideas are not more widely accepted by the mainstream is that heterodox economists usually do not present their ideas in a formal way and in models mainstream economists accept. He advises to follow an 'inside-the-mainstream' heterodoxy under which heterodox economists try to present their ideas using a language mainstream economists can understand and try to engage with the mainstream in a constructive debate in mainstream economic associations. In a similar direction, Fontana/Gerrard (2006) call for the use of up-to-date and rigorous mathematical methods usually applied by the mainstream.

On the other side of the debate, this advice has been strongly rebutted. Vernengo (2010) and Lee (2012) argue that heterodox economists should instead focus on improving the foundations of their own theoretical models and should not focus too much on engagement with the mainstream. According to them, the chances of being accepted by the mainstream remain slim and benefits from an engagement thus remain elusive while valuable time will be wasted in the engagement process. King (2012) takes something of a middle ground, arguing that there is nothing wrong with engaging, yet not much to be gained.

1. See Lavoie (2012) for a structured overview of the arguments.

While this debate has been lucid and interesting, it also lacks some empirical examples. Hyman Minsky is mentioned in one instance (Colander et al. 2010), but it is questionable whether he is really a good point of reference given that he has been appointed at a time when a larger number of (post-)Keynesians were still members of mainstream faculties. In fact, there are few specific recent examples of heterodox economists engaging with the mainstream presented in the debate. Moreover, there is a lack of explicit criteria on how the ‘success’ of any engagement or non-engagement strategies should be measured.

Trying to find examples of heterodox economists following one or other strategy and analysing the results, however, is difficult. The problems start with defining what a heterodox economist is. Is a heterodox economist someone who claims to be ‘heterodox’ or someone who publishes in heterodox journals? Both criteria would potentially rule out certain economists who are heterodox in thinking, but have focused on publishing in mainstream journals. The second question is on what ‘engaging’ means. Does it mean speaking at mainstream economic conferences? Publishing in mainstream journals? The third is the question of success: is it a sign of success to get published in high-ranking, mainstream journals? Or of being appointed at a top department?

In order to overcome these problems, this article takes a slightly different approach: its point of reference is heterodox *ideas*, not heterodox *economists*. Heterodox ideas can be more easily identified as those long present in heterodox writings, but mostly absent in mainstream models until recently. As the debate on heterodox economists mentioned above concerns mainly post-Keynesian writers, this paper also focuses on post-Keynesian economic ideas, yet its lessons might be valuable for other types of heterodox economists as well.

This paper focuses on cases in which such ideas (such as the effectiveness of fiscal policies or the importance of distributional issues) have been included in mainstream macroeconomic models and have led to policy conclusions usually found in heterodox models. It analyses what kinds of problems potentially emerge by using mainstream models in this way and illuminates the limits of such a strategy. In the following, such a strategy is dubbed ‘running with the pack’ and is defined as using mainstream macroeconomic models (or significant elements thereof) without explicit critical distance to promote economic policy ideas usually found in alternative economic approaches. In order to make this analysis more stringent and focused, it concentrates on the current mainstream of macroeconomics only: the use of dynamic stochastic general equilibrium (DSGE) models.²

In the context of the old debate on how heterodox economists should act, this ‘running with the pack’ is a very specific subset of engaging with the mainstream – it implies using very large parts of the mainstream’s theoretical framework with only small (but potentially crucial) changes to make a very specific point.³

The paper is structured as follows. After presenting some examples of ‘running with the pack’ in Section 2, it will look at potential benefits of ‘running with the pack’ in Section 3 as a first step. In a second step, Section 4 will look at the problems which ‘running with the pack’ can create. In a last step, Section 5 briefly discusses alternative strategies for heterodox economists.

2. See Blanchard (2009), Blanchard (2016) or Romer (2017).

3. It is not entirely clear whether some of the advocates of ‘engaging with the mainstream’ would endorse the ‘running with the pack’ strategy: Colander, for example, asks for engagement with the mainstream, yet has been extremely critical of DSGE models (Colander et al. 2010) and somewhat critical of the mainstream macroeconomists’ methodology which, according to him, limits fields of possible inquiry (Colander 2010).

2 EXAMPLES OF ‘RUNNING WITH THE PACK’

‘Running with the pack’, as defined above, can be seen both in research and in providing teaching material. Due to space constraints, this contribution will focus on examples from research and leave teaching material for future discussion.

One prominent example of ‘running with the pack’ is recent research on the macroeconomic impact of fiscal policy. In earlier versions of the DSGE models used by mainstream economists, deficit-financed increases in public spending led to a (small) increase in GDP, but a *fall* in private consumption. The mechanism here is as follows (Dullien 2012). As the government has to respect its intertemporal budget constraint, government borrowing in the present leads to higher taxes in the future. As representative agents have an infinite horizon and rational expectations, they see an increase of government borrowing today as a reduction of their lifetime disposable income and consumption possibilities. As a consequence, they react with a reduction in their current consumption and their current leisure time. As time has been used either as leisure or as working time, this reaction leads to an increase in working hours offered in the labour market, and – as this market clears by assumption – a higher number of hours worked in equilibrium and hence higher production. Under this assumed causation, the fiscal multiplier (defined as the reaction of GDP to a change in the deficit) is positive, but relatively small and far below 1, especially if the expansion of public spending is only temporary: in order to smooth consumption over the lifetime, the representative agent transforms the expected reduction in disposable lifetime income into a reduction of permanent consumption (and an increase in hours worked) over the whole lifetime, which results in only marginal reductions in current-year consumption and only small increases in hours currently worked (and hence only small increases in GDP). Given these small fiscal multipliers, austerity packages (slashing government expenditure quickly and without regard for the position in the cycle) in these models do not cause large problems.

This is clearly a very different story from the one (post-)Keynesian economists would tell. In a Keynesian world, fiscal policy is able to strongly influence aggregate demand and hence employment. The multiplier for debt-financed fiscal expenditure is seen as being clearly (and in some cases, strongly) above 1, and hence austerity packages as those passed during the euro crisis in Greece and Ireland are seen as a grave danger to macroeconomic stability. Moreover, it has always been a key belief of Keynesian economists that fiscal policy’s effectiveness depends on the state of the economy: when the economy is in a liquidity trap, for example, and monetary policy hence cannot influence output, fiscal policy has a larger multiplier than in normal times or in a boom.

Recently, policy conclusions much closer to the post-Keynesian narrative have been drawn from modified DSGE models. The standard way of doing this is to introduce a certain share of ‘liquidity-constrained’ or ‘rule-of-thumb’ consumers to the DSGE models. These households have no access to financial markets and hence can neither save, run down their savings, nor borrow. As a consequence, they always consume all their income in the period it is earned (note, however, that these households still vary their labour supply in a reaction to changes in the real wages).

Rannenberg et al. (2015), some of whom have regularly been present at the annual conferences of the FMM network in Berlin, apply such a DSGE model to the austerity applied during the euro crisis. In a well written and carefully designed model with such rule-of-thumb consumers and additional financial frictions, they come to the conclusion that multipliers during the euro crisis have been much higher than standard DSGE models would have predicted. Moreover, most of the output costs of fiscal consolidation ‘could have been avoided if it had been postponed until the zero lower bound constraint on

monetary policy was no longer binding, and under such conditions the government debt-to-GDP ratio could have been reduced much more quickly' (ibid.: 2) – a conclusion many post-Keynesians probably would have drawn as well.

Another example for the spread of non-mainstream ideas in DSGE models is Jakab/Kumhof (2015). In this paper, the two authors argue that if one allows for the ability of commercial banks to create money through credit expansion (rather than just intermediating loanable funds), banks can be shown to behave pro-cyclically and shocks to the financial sector have much larger consequences for the real economy.

Standard DSGE models usually do not say much about banks. In fact, most of the first-generation models do not explicitly include a banking sector. Instead, in these models, the central bank sets an interest rate as a reaction to output and price trends, and this interest rate influences the economy via households' variation of their consumption plans. If money enters the picture at all, it is as a money supply which is set exactly at the level which fulfils the households' demand for money holdings at the interest rate set by the central banks. All the complications of credit supply, credit demand, and banks creating money in the process are completely ignored. Some more elaborate DSGE models include banks, but these banks are usually acting just as intermediaries between households which save money somehow dropped into the economy and firms investing the money, rendering banks' balance sheets meaningless for the economic cycle.

This again is very different from the post-Keynesian view on money creation: here, commercial banks are seen as the creators of money when they grant credit, and this process is crucial to understanding economic fluctuations (see, for example, Lavoie 2014: 186ff). If this process is impaired, for example because a financial crisis wipes out banks' equity and banks become reluctant to lend, economic activity is severely impacted. The behaviour of financial institutions is often seen as a driving force for booms and busts.

Jakab/Kumhof's model now takes this argument from the post-Keynesians, explicitly quoting Moore (1979; 1983), Minsky (1986; 1991) and Graziani (1990), and includes it into a state-of-the-art DSGE model. The two authors model a banking sector which can create money by extending loans to firms and which is constrained by regulations (enforced with monetary fines) to keep a minimum capital adequacy ratio. As a result, they get a model with much more realistic reactions of macroeconomic variables to shocks, for example to borrowers' riskiness.

A third example for the spread of heterodox ideas into mainstream DSGE models is the inclusion of effects of the income distribution on macroeconomic variables by Grüning et al. (2015). These authors – again authors regularly and actively present at the FMM conferences in Berlin – show how shifts in the distribution of incomes between households and firms can lead to changes in the current account of an economy.

In simple standard DSGE models, shifts in income between different groups are usually neglected. Representative agents are usually assumed to be identical, with the same income and wealth. Even if DSGE models would just assume differences in incomes (for example, because of different labour productivities), changes in inequality between the agents would not necessarily have an impact on aggregate consumption or GDP: as all individuals smooth their consumption over their lifetime, a redistribution of income between groups basically just shifts consumption from one group to the other and does not have macroeconomic impacts.

This is in stark contrast to post-Keynesian works: distributional issues and their impact on macroeconomic variables such as GDP, consumption and investment have long featured very prominently in post-Keynesian economics, and shifts in the distribution both between different groups of wage earners or between wage earners and households earning profits usually have substantial effects.

The Grüning et al. paper now manages to get an effect from changes in the income distribution to macroeconomic variables into the DSGE model by introducing a ‘minimum acceptable level of consumption’ for workers and a ‘corporate veil’ which separates corporations’ retained earnings from household consumption decisions. As a result, growing inequality in the model economy can lead to less domestic demand and a growing current-account surplus.

3 WHERE ‘RUNNING WITH THE PACK’ CAN HELP YOUR CAUSE

Before looking at benefits and potential problems of a ‘running with the pack’ strategy, it is helpful to define why one might conduct academic macroeconomics (including the development of formal models and the publication of academic articles) in the first place.

Four motivations come to mind. First, one might want to spread certain ideas. The target audience might be other academics, policy-makers or the broader public. Second, academic macroeconomic work might have a pedagogical goal: it might help to better explain economic mechanisms to students. Third, academic macroeconomics might provide tools to help policy-makers to evaluate policy options and to predict economic developments (which are important for business decisions). Fourth, academic macroeconomics might be conducted because publishing academically might enhance one’s individual career, as it might help to get into (better) academic positions, get tenure, or get better access to third-party research funding. Note that this fourth argument is not necessarily an egoistic one: in principle, getting into more prestigious positions and gaining better access to research funds allows one to further spread economic (policy) ideas. Without his publication record and mainstream academic career, Paul Krugman would never have been able to achieve a level of publicity similar to the one he is enjoying today (nor the Nobel Prize).

If we go through these motivations, some of the possible benefits are immediately clear. If the goal is to spread certain policy ideas to as many other academic economists as possible, it is very helpful to speak their language and present the argument in a modelling framework they are familiar with and they can relate to. As one mainstream economist put it during a panel discussion: ‘I accept any economic idea which is presented in a proper model, that is, in a proper DSGE framework’.

Also, if one manages to get papers with one’s ideas published in rather high-ranking mainstream journals, it is much more likely that they get attention. Given the large number of journals published these days, very few economists follow (or even read) more than a handful of journals. In the mainstream of the profession, the top-ranked journals get the most attention. At the same time, papers are usually only accepted in the top journals if they are based on what is perceived as ‘cutting-edge’ modelling – in macroeconomics this usually means DSGE models.⁴

Similarly, for personal career prospects, being successful in publishing with mainstream journals with high rankings clearly is important. Economics departments today often hire according to the number of publications a candidate has managed to place in journals with high impact factors (or high ranking on some kind of journal list), and these journals are overwhelmingly, if not exclusively, mainstream journals.

How far ‘running with the pack’ is useful for spreading ideas to policy-makers is less obvious. Clearly, this strategy does not help directly in reaching this audience: policy-makers do not read academic economics journals and even less technical papers with DSGE models containing dozens of complicated equations. It is also questionable whether

4. For criticism of this custom by a mainstream economist, see Wren-Lewis (2016).

these models help one to explain the underlying ideas to policy-makers as the models contain a number of highly questionable economic mechanisms, which policy-makers will have a hard time swallowing (see below). However, being able to present standard models with certain policy conclusions and publishing them in high-ranking mainstream journals might *indirectly* help to spread ideas: policy-makers do read rankings, and if someone comes from a high-ranked university or features well on ranking lists, this person has a much higher *a priori* credibility. The rankings, in turn, are usually constructed with a heavy bias towards mainstream publications.

The pedagogical value of putting heterodox policy ideas into DSGE models is much less clear as the underlying economic mechanism in these models is often highly implausible to students (and other lay persons who have not gone through a decade of training in these models). This has much to do with the fact that the core of the DSGE models still is a real business cycle model in which business cycle fluctuations are mainly explained by (implausible) exogenous shocks and rational individuals' variation of their working time in reaction to year-to-year changes in resulting real wages (see Dullien 2011). If one tried to put the economic mechanism of positive fiscal multipliers in a DSGE model into verbal prose, the result would read roughly like this: 'If individuals see that the government starts repairing public school buildings and borrows money to pay for this, they realize that they will have to pay higher taxes in the future. As a consequence, some households will decide to work more and cut back consumption and save the resulting extra funds in order to be able to pay for taxes in the future. This additional work input increases output in the economy. About half of all households, however, do not have access to bank accounts and because of this cannot save even though they would like to. These households will just continue spending all the money they earn. As the additional government demand drives up real wages – nominal wages here are flexible, but prices sticky – these households will start supplying more working hours, resulting in more income and more consumption spending than before. All this together leads to an increase in output and consumption'. As this chain of causation goes against what many (post-)Keynesian economists believe to be how the economy works, it is difficult to see how such a model could be a valuable pedagogical tool to convey the workings of the macro economy to students. (In addition, I would have some doubts whether students would be especially happy with this explanation of the workings of fiscal policy.)

Finally, for evaluating policy options or even forecasting economic developments, the resulting DSGE models are also of questionable value. First, one should be extremely careful to use a tool for forecasting out-of-sample observations with a model that only fits in-sample observations because of the inclusion of clearly absurd assumptions (such as half of the population being excluded from financial markets such in a way that they cannot save at all).⁵ Second, DSGE models usually do not perform very well in forecasting, sometimes not much better than an AR(1) process, and not persistently better than alternative statistical models, and some proponents of DSGE models even dismiss forecasting ability as a quality criterion for this model class (Wickens 2014). Third, one should be aware that – despite the fact that DSGE models have been partly developed as a reaction to the Lucas critique – DSGE models are not immune to that critique: Lucas (1976) had cautioned economists that models which are built on perceived statistical relationships between macroeconomic variables cannot be used to evaluate policy shifts as the underlying parameters might shift. As a solution, economists had moved towards using microfoundations

5. The problem is not that of absurd (or clearly wrong) assumptions *per se*, as all models make some simplifying assumptions. The problem is whether a certain important feature of a model only emerges if a certain, clearly wrong assumption is included.

with supposedly ‘deep’ parameters which are unchanged over time and even in the wake of shifts in economic policy regimes. Formally, the parameters in utility and production functions are usually seen as such deep parameters. Yet, as has been shown recently, some of these ‘deep parameters’ in DSGE models have exhibited significant drifts, questioning the stability of the DSGE models’ microfoundation (Hurtado 2014).

4 PROBLEMS OF ‘RUNNING WITH THE PACK’

Yet limits of certain models should not necessarily mean that one should not work with them. So, as ‘running with the pack’ provides some benefits, one might argue that the areas in which these models are less useful (such as the questionable pedagogical value or the inability of these models to produce reliable forecasts) can just be ignored. The problem is, however, that using these models as a tool to promote certain policy ideas which one might have originally drawn from other (heterodox) economic models comes with some serious drawbacks.

4.1 Questionable policy conclusions

One issue with including just single elements in models which have fundamental problems (as one might argue in the case of DSGE models – see Dullien 2011) is that the resulting model gives *some* policy conclusions which are in line with one’s underlying economic thinking, but others which are highly questionable. One example would be the question of austerity and the fiscal multiplier.

As discussed above, one of the reasons for the strong impact of austerity measures in modified DSGE models is that a significant share of households lacks access to financial markets. In a baseline DSGE model without liquidity constraints, government cutbacks to spending and borrowing would result in households seeing their lifetime income increasing (as they would expect less taxes in the future). As rational households are smoothing consumption over their lifetime, they would thus increase consumption immediately and finance this increased expenditure either by running down their savings or by borrowing. In contrast, in a model with liquidity-constrained households, the cut in government spending would lower aggregate demand and real wages, causing these households to work less and consume less, leading to a contraction in consumption and output.

Of course, one conclusion from such a model could be – as Rannenberg et al. (2015) elaborated – that austerity should not have been applied in a time of weak economic growth with nominal interest rates already close to zero. However, the model would also allow for the conclusion that prior to austerity, households should have been given more access to financial markets and especially consumer loans. This way, the liquidity-constrained households (or ‘rule-of-thumb’ households) would have been transformed into ‘normal’ households. They would have reacted to austerity with an increase of private consumption (financed by borrowing), and austerity would have had a much less detrimental impact overall.

The problem with this policy conclusion for post-Keynesians is rather obvious: as austerity in their view is detrimental not because households cannot effectively borrow against lower tax liabilities in the future, but because of a disruption of the income-creation process, a policy mix of liberalizing access to consumer loans and austerity is not helpful (and might, rather, set the stage for a household-debt crisis in the future). Yet, if one has accepted such a model uncritically for policy analysis, it is difficult to accept one policy conclusion from it, but not others.

A second example for this problem is two papers co-authored by Michael Kumhof, one of today's leading modellers of DSGE models. In Kumhof/Benes (2012), Kumhof demonstrates in a DSGE model how a shift to 100 per cent reserve banking (the 'Chicago Plan') could increase economic stability and steady-state output. The main argument is that a 100 percent reserve banking would allow the government to control very closely the credit cycle in the economy through a targeted issuance of currency, hence getting rid of much of the volatility emanating from pro-cyclical lending behaviour of the financial system. Moreover, as all of the existing money stock would have to be backed by currency, the government would have huge seigniorage windfall gains by providing this additional money. This seigniorage could be used to pay down public and private debt, lowering the net debt level in the economy and the government's need for taxes (as it does not have to pay interest on its debt any more). According to the model used, this would lead to lower real interest rates and less deadweight losses from taxation, increasing steady-state output by around 10 per cent.

In Jakab/Kumhof (2015), an endogenous money approach is embraced in which commercial banks are the main creators of money when they extend loans to firms wanting to invest, with an explicit reference to post-Keynesian authors such as Moore, Graziani and Minsky (see above). The result is that an economy with endogenous money creates much larger volatility as a result of a shock.

The problem here is that – while not obvious at first sight – the conclusions from the two papers are mutually incompatible. As Fontana/Sawyer (2016) explain, the central assumptions of the Chicago Plan are in fundamental conflict with the post-Keynesian analysis of endogenous money. The post-Keynesian approach sees money creation as a complex interaction of households', firms' and banks' decisions, while the Chicago Plan assumes that the demand for broad monetary aggregates is a given and that holdings of endogenous money (which is not net wealth) can just be replaced by holdings of exogenous money (which is net wealth). As Dullien (2004) argues, according to post-Keynesian thinking on endogenous money, money creation needs the cooperation of banks and firms, while the existence of a certain stock of money as a final asset needs the willingness of households to hold this money.

More specifically, if banks were required to hold 100 per cent of their deposits in reserves, they would not be able to offer transaction accounts for free (as they often do today), but would have to charge their customers a fee (Fontana/Sawyer 2016). This would greatly reduce the attractiveness of money as an asset and would lead to the innovation of new 'quasi-moneys' (combining some kind of non-negative return with a slightly lower degree of liquidity than cash) which would be much more attractive to customers. As a consequence, one would expect the demand for money holdings to contract, and quasi-banks to move into the business of providing payment services on investment accounts. As a result, the amount of currency needed after the implementation of a Chicago Plan would be much less than predicted by the Kumhof/Benes (2012) model, and the economy much less stable, wiping out the benefits of the plan. A thorough embracement of post-Keynesian theory would have pointed out these problems, yet just using a standard DSGE model and adding single elements from heterodox thinking blurs these issues and leads to questionable conclusions.

4.2 Delaying scientific revolutions

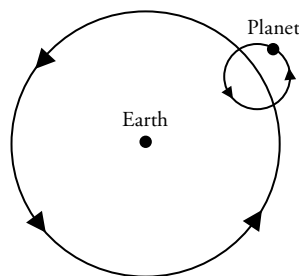
A second point is that using a certain model class might actually stabilize its use in the profession, and that heterodox economists might well take this possibility into account

when promoting their policy conclusions using certain mainstream models such as DSGE models.

Thomas Kuhn (1970) famously describes a ‘scientific revolution’ or paradigm shift as a sociological phenomenon when the scientific community changes the methods, models and framework which are seen as acceptable for scientific inquiry. While usually a scientific revolution is triggered by a growing number of empirical observations at odds with an existing paradigm, Kuhn underlines that (a) a paradigm shift might occur without a new model being more precise in its predictions and that (b) the old framework’s failure to deal with certain phenomena alone is not a sufficient condition to trigger a scientific revolution.

Kuhn illustrates his theory with a shift from the Ptolemaic world view in which the Earth was seen as the centre of the universe to the heliocentric model of Copernicus, Galileo and Kepler, which acknowledged that the Earth moves around the Sun. As elaborated in Kuhn (1957), by the early sixteenth century (when Nikolas Copernicus started his work on planetary movements), astronomers used an elaborate geocentric system (the Ptolemaic) which was well able to explain a large number of empirical observations quite accurately. According to this world view, the Earth was at the centre of the universe and the stars were part of a sphere which was rotating around the Earth. The Sun, the Moon and the planets were circling around the Earth on additional spherical trajectories.

Early versions of the Ptolemaic model had failed to accurately predict planets’ positions, and especially failed to explain empirical observations about the planets’ movements. While seen from the Earth, the stars as well as the Sun and the Moon usually move across the sky in straight motions (which can easily be explained by spheres rotating around the earth), planets such as Mars or Venus sometimes show retrograde motions by which they seem to move back on their trajectory. These problems had been addressed within the geocentric model by the late fifteenth century by introducing paths for single planets which deviated from a simple circular shape, for example by adding epicycles to the circular trajectory of a planet. As is explained by Kuhn (1957: 59), adding an epicycle to a planet’s movement (see Figure 1) creates a rather complicated trajectory which, observed from the Earth, would give the impression of a retrograde movement of the planet in question. In addition, Ptolemaic scholars introduced theories of varying speeds of rotation of the planets around the Earth. With these (increasingly complicated) modifications, the Ptolemaic model actually managed to explain the movements of the planets reasonably well, even though there were always inaccuracies left and the positions of the planets could not be predicted with great precision. At the same time, the Ptolemaic model quite well predicted the positions of the stars (the far larger part of what can be seen in the sky), and hence made Ptolemaic astronomy highly valuable for practical purposes such as maritime



Source: Author’s own creation based on Kuhn (1957: 59).

Figure 1 A planet circling around the Earth on an epicycle

navigation. The model thus had a decent empirical fit, even though the model was clearly wrong and fundamentally flawed. With its core unchallenged, this model was a point of reference for astronomers for more than 1000 years.

Interestingly, Copernicus's attack of the model did not mainly come from the empirical failings of Ptolemaic astronomy, but from the observation that the model had grown excessively complicated while still failing to explain all the observations. As Kuhn (1957: 139) put it:

An honest appraisal of contemporary astronomy, says Copernicus, shows that the earth-centered approach to the problem of the planets is hopeless. The traditional techniques of Ptolemaic astronomy have not and will not solve that problem; instead they have produced a monster; there must, he concludes, be a fundamental error in the basic concepts of traditional planetary astronomy.

A different example, in which a theory did not fit empirical facts, but was correctly maintained, can be seen in explanation of an 'anomalous' orbit of Uranus. In the nineteenth century, astronomers had observed that the orbit of Uranus did not fit with the predictions of Newton's theory of gravity. Yet, instead of discarding this theory, they looked for alternative explanations. They came up with the conclusion that there must be an additional, as yet unknown, planet in the solar system, and upon closer investigation, the planet Neptune was discovered, exactly at the position derived from Newton's law of gravity.

In the context of Kuhn's concept of scientific revolution, it is important to see that a theory is not necessarily discarded just because it does not fit certain empirical observations. At the same time, not all of a theory's failure to explain empirical observations actually warrants its demise.

Kuhn goes to great lengths to explain why the Copernican revolution happened in the sixteenth century and not earlier, given that empirical problems with the Ptolemaic theory had been evident for a long time. He explains the possibility of a paradigm shift with a combination of societal and political factors which changed the environment under which academics work and which allowed Copernicus's claims to be accepted by other academics and finally be adopted as a new paradigm. Kuhn (1957: 132) writes: 'Any possible understanding of the Revolution's timing and of the factors that called it forth must ... be sought principally outside of astronomy, within the larger intellectual milieu inhabited by astronomy's practitioners'.

Against this background, whether heterodox economists should see it as a good approach to embrace the DSGE model and work with it depends on what one perceives DSGE models to be: if one believes that DSGE models are the equivalent of Newton's law of gravity and the vision of the solar system of the nineteenth century (which was basically sound and could be improved by adding Neptune to the picture), a strategy of embracing these models might be a good one, as it might help to make the models better. If one believes, however, that today's DSGE models are, rather, the equivalent of the Ptolemaic model before the Copernican revolution, adding elements to the model which improves the empirical fit for certain data points might actually just delay a Kuhnian scientific revolution.

This latter argument might be especially relevant as, in the mainstream itself, the DSGE models have lately come under heavy fire, and one could argue that the aftermath of the Great Recession of 2007–2009 constitutes a time of turmoil. While some economists such as Krugman (2009) or Solow (2010) had already criticized DSGE models shortly after the Great Recession of 2007–2009, the debate has gained new thrust with Paul Romer's polemic against DSGE models (Romer 2017), a number of shorter blog contributions by Paul Krugman and Bradford de Long and a defence of DSGE by Olivier Blanchard (2016). If mainstream economists are fighting about the direction of the

profession and the use of DSGE models, there might well be a ‘scientific revolution’ in the making,⁶ and using DSGE models for non-traditional arguments at this time might just give this model class more credibility.

5 CONCLUSIONS: ALTERNATIVES TO ‘RUNNING WITH THE PACK’ FOR HETERODOX ECONOMISTS

So, how can we evaluate ‘running with the pack’, then, and what are possible alternatives? ‘Running with the pack’ clearly has some advantages for communicating certain policy ideas to mainstream economists. On the personal level, it has the potential to further one’s career. However, it comes with serious risks, including that of using models from which very questionable policy conclusions can be drawn, and of potentially being accused of intellectual dishonesty if one embraces one policy conclusion from the model used, but discards others in a seemingly arbitrary manner.

When relating this to the broader discussion about potential strategies for heterodox economists, these conclusions do not invalidate the advice to engage with the mainstream. However, heterodox economists might want to be careful about *how* to engage. One form of engagement could be to focus on empirics and econometrics: if certain phenomena which cannot be replicated in mainstream models can be shown to be relevant empirically, this can lead to publications in (empirically oriented) mainstream journals. While this approach does not necessitate the adoption of formal macroeconomic models used by the mainstream, it certainly requires a thorough knowledge of statistical and mathematical methods. So, such an approach would be in line with Lavoie’s (2012) recommendation to try to improve the ability to work with statistical methods.

An alternative strategy (which would engage the mainstream only indirectly) would be to address practitioners and policy-makers and try to convince them of the superiority of non-mainstream approaches. If post-Keynesians manage to develop macroeconomic models which succeed better in forecasting macroeconomic developments, policy-makers and non-academic professional economists in the private sector are likely to turn to these models. Again, the advice by Lavoie (2012) to work with rigorous statistical methods would be a precondition for such a strategy. It is difficult to imagine a macroeconomic model capable of forecasting which is based on shaky formal foundations.

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6. A ‘scientific revolution’ does not necessarily mean a shift from today’s DSGE models to post-Keynesian models. In fact, such a shift is rather unlikely. However, it would mean a shift to some *other* model class.

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