Money and Banking: Some DSGE Challenges

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PRELMINARY
COMMENTS WELCOME

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Abstract

DSGE models were meant to rise to the Lucas (1976) challenge of constructing general equilibrium models with deep parameters, to be used as workhorse models for e.g. monetary policy analysis. But challenges remain. This paper discusses a few of them. The central issue of the impact of monetary policy on yield spreads and asset prices is often ignored in DSGE models or kept essential separate from allocations. While much progress has been made on considering financial frictions in DSGE models, the models need to allow more room for private markets to address these frictions, rather than call

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on economic policy. The Phillips-Curve tradeoff, so central to thinking about monetary policy and much of the DSGE literature, may hardly be there. New Keynesian models, which have largely become the benchmark approach to thinking about monetary policy in central banks, have strong, but often-ignored neo-Fisherian properties. In sum, the glass is half full. Or half empty. Take your pick.

Keywords: DSGE models, Monetary Policy, asset prices, financial frictions, Phillips curve, New Keynesian, Neo-Fisherian

The purpose of this paper is to provide a bit of a birds-eye perspective and critique of the DSGE approach to thinking about monetary policy and banking. I will pick out a few points or challenges only, in the interest of length. I admit that the selection of these particular challenges are partly driven by a desire of expositing some of my own work. There are many more which could (and perhaps should) be added. The title of this paper may share some similarities with the title of Lindé et al (2016), though the perspective differs considerably. Instead, this paper is perhaps closer in spirit to the perspective of Gilles Saint-Paul (2018), who provides a more formal modelling of the concerns raised here, or the skepticism towards the conventional empirical identification of monetary policy shocks in Uhlig (2005) or the skepticism regarding the effectiveness of monetary policy to influence employment, see McGrattan (2015).

Dynamic stochastic general equilibrium or DSGE models were meant to rise to the Lucas (1976) challenge of constructing general equilibrium models with deep parameters, to be used as workhorse models for e.g. monetary policy analysis. Indeed, the last two decades have witnessed a remarkably successful epoch for this approach, with quantitative New Keynesian models such as Smets-Wouters (2007) being increasingly at the heart of monetary policy discussions in central banks. Likewise, though at a considerably slower speed, they have become of increasing relevance for macro-fiscal policy, in particular for thinking about fiscal multipliers and the impacts of fiscal stimulus in times of economic crisis and zero-lower-bound constraints on monetary policy, see e.g. Coenen et al. (2012) or Drautzburg-Uhlig (2015) for some examples of that literature. The financial crises led to a substantial recent literature of incorporating a variety of financial intermediaries and financial frictions into models of these type, making the recent crop undoubtedly even more attractive, see e.g. Gertler et al (2016), Lindé et al (2016) or Wieland et al. (2016). Finally, DSGE models have become a highly useful structural framework for thinking about and identifying frictions or wedges and for guiding research towards still-missing parts of the theory, see Brinca et al (2016).

One approach for me here would be to contribute yet another victory lap, demonstrating the remarkable success of this branch of economic analysis. At the very least, I could act as a cheerleader for the key recent contributors, building that literature. I do applaud what they do, and going this route was tempting indeed.

Instead, and with some trepidation, I shall point to some remaining challenges instead. This is slippery territory indeed. The literature is vast, and it is impossible for some readers to not feel sleighted by the critique offered here: their work may have just precisely addressed the issue raised here, and it may be my fault entirely of having ignored it or not having presented it sufficiently clearly, and it is my fault entirely of not being sufficiently informed about the current frontier or not discussing these contributions with sufficient care. Mea culpa. However, perhaps the critique here may then intead simply to lead to a yet more concentrated effort of moving these contributions to the forefront of the policy debates.

I shall discuss the following challenges in particular in greater detail in the sections of this paper. The central issue of the impact of monetary policy on yield spreads and asset prices is often ignored in DSGE models or kept essentially separate from allocations, see section 1. Much progress has been made for considering financial frictions in DSGE models. Yet, the models need to allow more room for private markets to address these frictions, rather than to call on economic policy, see section 2. The Phillips-Curve tradeoff, so central to thinking about monetary policy and much of the DSGE literature, may hardly be there, see section 3. New Keynesian models have neo-Fisherian properties, but which are rarely advertised, see section 4. Hopefully, some of these issues get addressed in future research. A constructive approach here might be to apply the approach of Brinca et al (2016) and extend it to the realm of asset pricing to provide a guide, beyond the specific challenges listed here.

The final section 5 offers some summary and conclusions. In brief, it seems to me that the glass is half full. Or half empty. Take your pick.

1. Challenge: Asset pricing

Monetary policy interacts with asset markets, most notably the bond market. The reactions of these asset markets to choices at monetary policy meetings are often a key part of reporting on these events and are now a substantial part of the literature on identifying monetary policy shocks, see e.g. Nakamura-Steinsson (2018). It then seems a priori obvious, that DSGE models tailor-made to address monetary policy matters need to do a decent job at accounting for asset prices, their movements and their implied risk and term premia, ideally matching the intricacies of the term structure of uncertainty exposited in Borovička and Hansen (2016). Figure 1 juxtaposes the Federal Funds Rate to the 10-year yield on US government bonds. There are interesting comvements as well as interesting differences. Figure 2 shows the spreads between the Federal Funds Rate and the 1-year bond rate and the 10-year bond rate, thereby essentially plotting a version of the "curvature" and "slope" of the yield curve. It is reasonable to think that the central bank gets to move the Federal Funds Rate, while the movements in the 10-year yield or movements in the curvature of the yield curve are of greater relevance for investors.

Despite their obvious importance, these yield curve movements are typically absent from DSGE models used for monetary policy anlysis, or treated with considerable neglect. The typical DSGE model views these yields as satisfying the "expectations theory". This theory has been rather soundly rejected by the empirical finance literature. Indeed, Crump-Eusepi-Moench (2018) show rather convincingly, that the movements in the 10-year spread have little to do with expectations about future real short rates or future inflation rates, and are nearly entirely due to movements in a forward term premium. Typical DSGE models have little to say about these movements or about asset prices, more generally.

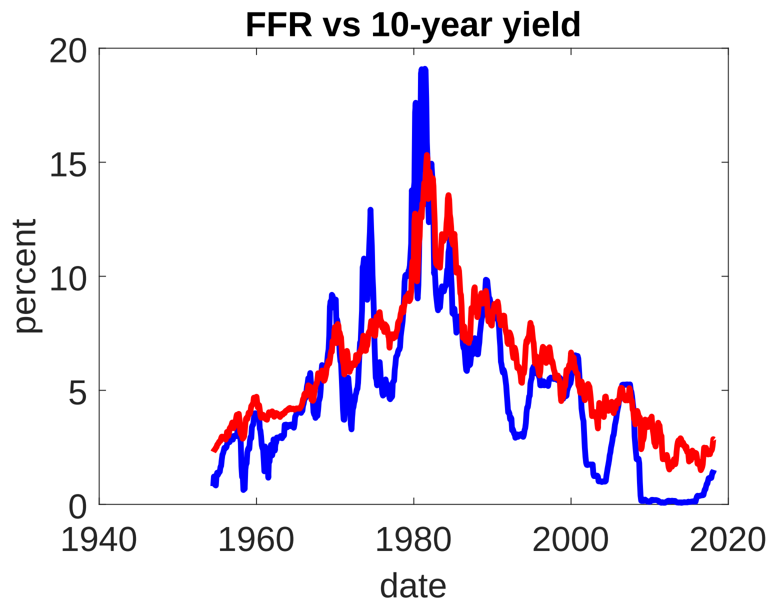


Figure 1: Yield Curves: FFR vs 10-year yields

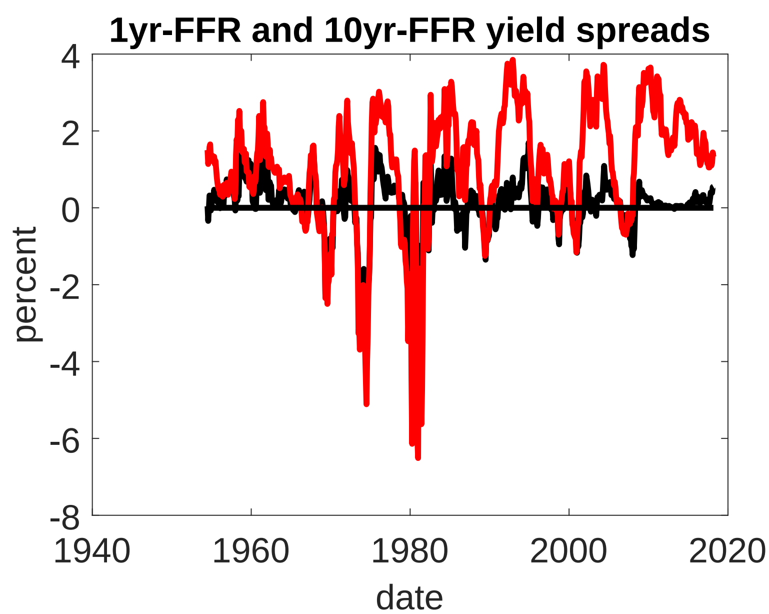


Figure 2: Yield Spreads: 1 yr or 10 yr versus FFR.

Indeed, from the get-go, there is a skeleton in the closet of consumption-based asset pricing, which is hardly, if ever addressed. For its most simple variant, consider a log-linearized solution of some DSGE model for consumption c_t and the return R_t of some asset. Let x_t denote the state of the economy of date t. A recursive law of motion as constructed, say, with the methods in Uhlig (1999) will then result in

$$\begin{aligned}\log(c_{t+1}) &= \log(\bar{c}) + \phi x_t + \epsilon_{t+1} \\ \log(R_{t+1}) &= \log(\bar{R}) + \xi x_t + \nu_{t+1}\end{aligned}$$

for some steady state levels, coefficients ϕ, ξ as well as some possibly correlated one-step ahead prediction errors ϵ_{t+1} and ν_{t+1}. Assume log preferences, discounted at β per period. The standard Lucas asset pricing equation, see Lucas (1978), delivers

$$\begin{aligned}1 &= E_t\left[\beta\left(\frac{c_t}{c_{t+1}}\right)R_{t+1}\right] \\ &= \beta\bar{R}c_t e^{(\xi-\phi)s_t}E_t\left[e^{\nu_{t+1}-\epsilon_{t+1}}\right]\end{aligned}$$

Suppose now, that the difference between the one step-ahead prediction errors is normally distributed, $\nu_{t+1} - \epsilon_{t+1} \sim \mathcal{N}(0, \sigma_t^2)$, conditional on t. Then,

$$E_t\left[e^{\nu_{t+1}-\epsilon_{t+1}}\right] = e^{\sigma_t^2/2}$$

Armed with this formula, one can now derive an expression for the risk premium for the asset and its associated Sharpe ratio. This is exploited frequently for examining asset premia or for the risk-adjustment of steady states, see Lettau-Uhlig (2002), Schmitt-Grohe-Uribe (2004), Coeurdacier et al (2011) or Uhlig (2018). But compare that to the situation, where that difference has a student-t distribution with, say, 1000 degrees of freedom, $\nu_{t+1} - \epsilon_{t+1} \sim t_{1000}(0, \sigma_t^2)$, conditional on t. Then,

$$\begin{aligned}E_t\left[e^{\nu_{t+1}-\epsilon_{t+1}}\right] &= \sum_{j=0}^{\infty}\frac{1}{j!}E_t\left[(\nu_{t+1}-\epsilon_{t+1})^j\right] \\ &= \sum_{j=0}^{\infty}\frac{1}{(2j)!}E_t\left[(\nu_{t+1}-\epsilon_{t+1})^{2j}\right] \\ &= \infty\end{aligned}$$

where I have exploited that a $t-$distribution is symmetric and eventually has moments equal to infinity[1]. $t-$distributions and thus these infinities arise rather naturally in an environment, where variables are normally distributed, but agents need to learn about their variances, see Weitzman (2007). A plot of the densities of the $t-$distribution chosen above relative to a normal distribution with equal mean and variance would not reveal much difference: indeed, they would be hard to tell them apart in the data. But it is now possible to generate infinite risk premia. Indeed, if one is free to choose any distribution "between" the thin-tailed normal distribution and the fat-tailed $t-$distribution, one can generate any desired risk premium. It should be fairly clear, that the calculation does not rest on choosing log-preferences in particular. It also seems plausible, that the argument does not rest on the log-approximation per ser, but simply on the potentially fat tails of the surprise component of log returns: indeed, a substantial layer of sophisticated numerical approximation techniques may ultimately just obfuscate these rather straightforward, but powerful insights. There is a large finance literature documenting fat-tailed asset returns: if anything, a $t-$distribution with 1000 degrees of freedom still errs on the side of caution.

One might draw the conclusion that the much-loved consumption-based asset pricing theories, which are in turn at the core of DSGE modeling, do not offer us much guidance on calculating risk premia or their movements over time. Now what? We will choose to do what the vast part of the literature has done: ignore this issue, keep calm and carry on. But as this paper is meant to be a critique, it ought to be clear, that this issue desperately needs a thoughtful resolution.

With this caveat out of the way, the issue of risk premia in DSGE models or, generally, in consumption-based asset pricing models has given rise to a huge literature. While risk premia are still often ignored in quantitative DSGE models, the landscape has shifted from these premia being a puzzle,

[1]There is a subtlety here regarding the undefined higher odd moments. A precise version of the calculation in the text requires taking a limit.

see Mehra-Prescott (1985), to the availability of several approaches generating observable risk premia without affecting the macroeconomic dynamics too adversely. One approach exploits the disaster-risk specification of Barro (2006). Incorporating these disasters into a quantitatively reasonable DSGE model then requires a simultaneous disaster in capital as well as productivity, see Gourio (2012): one feels that more can be done here. A second approach builds on habit formation, see e.g. Uhlig (2007) for an example. The complications of the ensuing calculations often lead researchers to turn elsewhere. In particular, Epstein-Zin preferences, originally formulated by Epstein and Zin (1989) offer a third and very popular approach. The literature on these preferences with their consequences for asset pricing, see e.g. Bansal-Yaron (2004) or Piazzesi-Schneider (2007), and the related literature on ambiguity or robustness and their consequences for asset pricing, see Tallarini (2000) and Hansen and Sargent (2007), has grown substantially in recent years. It is then not surprising that many DSGE authors, who seek to incorporate reasonable risk premia in their models, are drawn to the Epstein-Zin paradigm.

What may be less understood, that Epstein-Zin preferences make the job of generating risk premia almost too easy. The details of the following calculations are in Uhlig (2018), but a brief sketch of the argument shall suffice.

I shall skip the well-known specification of Epstein-Zin preferences. Assume that the intertemporal elasticity of substitution equals unity and that the risk aversion coefficient is given by η. One can log-linearize the Epstein-Zin equation for the evolution of the value function‘V_t and the associated distorted expectation $\mathcal{R}_{\mathbf{t}}$ just as any other equation and obtain

$$\begin{aligned}
\hat{V}_t &= (1-\beta)\,\hat{c}_t + \beta\hat{\mathcal{R}}_t \\
\hat{\mathcal{R}}_{\mathbf{t}} &= E_t\left[\hat{\mathbf{V}}_{\mathbf{t+1}}\right] \\
\hat{M}_{t+1} &= \hat{c}_t - \hat{c}_{t+1} + (\eta-1)\left(\hat{\mathbf{V}}_{\mathbf{t+1}} - \hat{\mathcal{R}}_{\mathbf{t}}\right)
\end{aligned}$$

where c_t is consumption, M_t is the stochastic discount factor, β is the discount factor and hats denote log-linearization around a steady state. With that, note that

$$E_t[\hat{M}_{t+1}] \;\; = \;\; \hat{c}_t - E_t[\hat{c}_{t+1}]$$

Some thinking about this equation reveals that Epstein-Zin preferences have no influence on macro-dynamics (up to first order). In some ways, this is an encouraging result. It implies that one can take any standard DSGE model, solve it with log-linearization, and then "slap on" the Epstein-Zin structure afterwards to get some asset pricing implications. In essence, there is now a dichotomy between "macro" and "asset pricing". I feel, though, that this makes the interaction between asset pricing and allocations, and, by implications, the interaction between yield-curve-impacts of monetary policy and real allocations too trivial and uninteresting: the macroeconomic allocations do not depend on them, at least up to first order. This needs sorting out. It certainly means that one should probably apply the Epstein-Zin paradigm in DSGE models with a degree of caution. Two more caveats are in order. First, if labor is part of utility, it necessarily shows up in asset pricing. This is not much appreciated: for the details, the reader is pointed to Uhlig (2018).

Second, higher-order solution methods are surely appropriate, given the strong curvature in preferences here, and may then generate the asset-pricing-real-allocation interaction missing above. This is routinely done in the associated literature, and perhaps that means that there is light at the end of the tunnel here. Piazzesi-Schneider (2007) argue that negative correlations between consumption and inflation induces a positive risk premium for long-term nominal bonds. They employ Epstein-Zin preferences to make that quantitatively large. Rudebusch-Swanson (2012), van Binsbergen et al (2012), Ulrich (2015), Zhao (2018) as well as Kliem-Meyer-Gohde (2018) are recent entries in that literature. The latter authors combine several recent branches of yield-curve accounting (nominal, real) for full-info estimation of small-scale New Keynesian model. As for preferences, they combine Epstein-Zin with considerations for Barro disaster risks. It may well be that these developments eventually result in a new benchmark DSGE model, allowing for a suitable understanding of this interaction of monetary policy, asset pricing and real allocations. Given that the key effects for the Epstein-Zin portion are then above and beyond the linearized intuition, understanding the exact nature of these results is then more crucial than in other applications of sophisticated numerical approximation methods, such as those summarized in Fernandez-Villaverde et al (2016).

A summary of this asset pricing challenge is this. There are skeletons in the closet, concerning consumption-based asset pricing. We keep ignoring them. Monetary policy has much to do with yield curve movements, yet the literature focusses nearly entirely on the short end, with some listed exceptions. Full quantitative accounting for yield curve movements is crucial to achieve progress. The interaction between the asset pricing side and the real allocation side merits considerable attention and a thoughtful treatment.

2. Challenge: Financial Frictions

It is hard to think about monetary policy without thinking about frictions or "wedges", see Brinca et al (2016), which are modified by it. There is a substantial literature pointing to pricing frictions ("sticky prices", "sticky wages"): more on that in sections 3. It seems plausible, though, that financial frictions and the impact of monetary policy on these are the more powerful channel. Considerable advances have been made in the recent literature addressing these. Agent heterogeneity and idiosynchratic shocks are now taken into account in the new benchmark HANK literature, see Kaplan et al (2018), building New Keynesian structures on top of an Aiyagari (1994) model. Financial intermediaries such as banks are routinely introduced in another branch of the literature, building in particular on the benchmark Gertler-Karadi (2011) and Gertler-Kyotaki (2011) or, in short, GKK framework. Increasingly, the interrelationship between credit market booms and busts and real allocations, such as housing market booms and busts, are addressed, see e.g. Guerrieri-Uhlig (2016). This is impressive progress.

However, when assuming frictions and then analyzing policies to address these, it is important to be beware of the "chicken paper conundrum", attributed to Ed Prescott. A "chicken paper" works as follows. Assumption 1: households enjoy consuming chicken. Assumption 2: households cannot produce chicken. Assumption 3: government can produce chicken. Conclusion: government should produce chicken. Once one has understood the basic structure of a "chicken paper", it is remarkable, how often one sees sophisticated examples of these, sometimes even published in excellent journals or part of serious policy discussions.

There may be nothing wrong per se with a "chicken paper" approach, provided the authors argue well enough, why they view such an approach as sensible and appropriate for the question at hand. Generally for providing policy guidance, though, it is important to argue, why agents cannot address these frictions on their own or to examine what happens, if one allows them to do so. Consider the popular variants of the Aiyagari (1994) model, for example. If income fluctuations are known, full insurance should be possible. One could, in principle, appeal to asymmetric information to argue, why this is not happening, but so far, this is an outside-of-the-model argument, and thus problematic. Or consider the GKK type models. There, if net worth might get destroyed and this is of considerable concern, it may be best to write insurance contracts against such events. Such insurance markets are often absent in these models. Alternatively, firms may seek other sources of financing, providing another valve of adjustment, see e.g. de Fiore and Uhlig (2011, 2015).

I argue that we therefore need DSGE models with privately fully-optimal contracts, in order to give private markets the benefit of doubt and to then subsequently analyze the remaining scope for, say, monetary policy. As we often assume agents to be alive for multiple periods, these contracts need to be allowed to be long-term a priori. For example, lending relationships in banks are often long term in practice, and there are good reasons why this is so. I feel that it is important to incorporate that in quantitative

DSGE models geared towards monetary policy guidance, building, say, on the approaches surveyed in Golosov et al (2016). This is a thorny research agenda, but some recent progress has been made.

Let me allow myself to advertise some recent own work with Dirk Krueger (2018) here. To understand our approach, think of an Aiyagari environment, i.e. suppose that agents are endowed with two-state Markov process of labor units, fluctuating between some fixed positive amount $n > 0$ and 0. We formulate it in continuous time: so introduce the transition rates $\xi\, dt = P(n \to 0)$ and $\nu\, dt = P(0 \to n)$. There is an aggregate production function $Y = K^\theta N^{1-\theta}$, where N are the units of labor used in production. Capital depreciates at rate δ. For preferences, assume discount log-utility with ρ.

In contrast to Aiyagari (1994), we build on Krüger-Uhlig (2006) and assume long-term insurance contracts, with one-sided commitment. More precisely, we assume that there are competitive intermediaries, who can commit long-term, while agents can walk anytime, and instantly sign up with the next one. One may think of these intermediaries as firms, committed to labor contracts, while their workers can walk away, or as banks, offering long-term contracts to their firms or households, who may be able to walk away from the deal at any time (or walk away more easily than the contracting bank). This is the only contracting friction in our environment. Otherwise, we have full information. In the spirit of the competitive market of intermediaries, we assume that they do not collude on some “credit history punishment”. The equilibrium contracts will then feature that payments from agent are front-loaded, while payments from intermediary are backloaded. Intermediaries invest payments from agent in capital, which in turn finances the entire capital stock. So far, we have only calculated steady state comparisons. In that case and under the assumptions here, we obtain a closed form solution for everything, including the equilibrium interest rate on capital. This is quite remarkable and in contrast to the Aiyagari-type models, which typically involve numerical computation. Analytical solvability is no longer of such great importance, given the advances in numerical solution methods, of

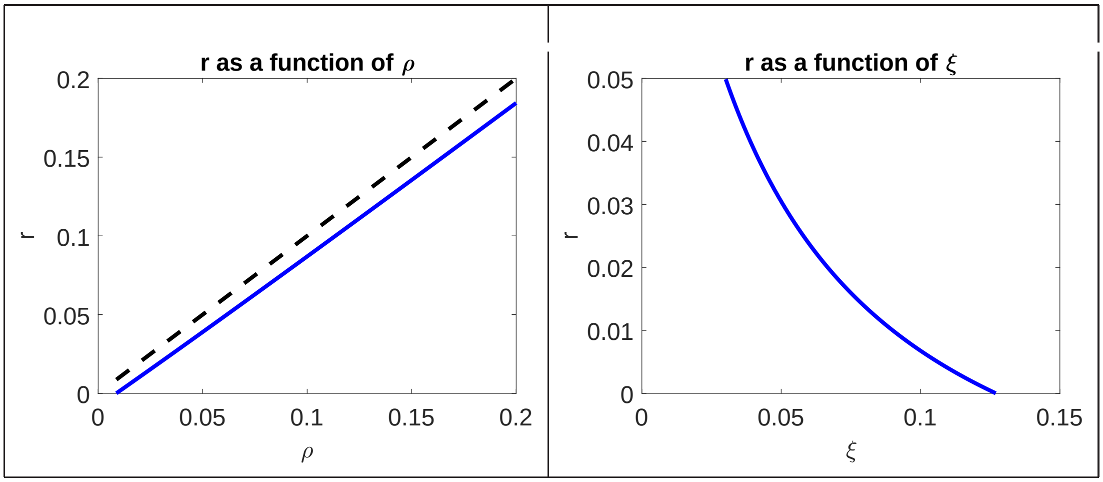


Figure 3: *Some comparative statics for the Krüger-Uhlig (2018) model. Recall, that* $\xi\, dt = P(\zeta \to 0)$.

course. It nonetheless aids the understanding of the model.

Figure 3 shows some results taken from that paper. The left panel shows that the equilibrium interest rate is below the benchmark representative agent economy interest rate, qualitatively similar to the key insight in Aiyagari (1994). The right panel shows the intuitively plausible result, that the equilibrium interest rate is lower with a higher transition rate into unemployment, as insuring against the now more likely unemployment state requires saving up more capital at the intermediaries. These are meant to provide some examples for the type of results emerging from that analysis.

Our model might provide an environment and potential alternative to Aiyagari (1994) as a future workhorse model. Obviously it is currently far from suitable for analyzing policy (as Aiyagari was, when it was orignally published): exploring these is the task of future research. More broadly, other types of contracting frictions are worth exploring.

A summary of this financial friction challenge is this. Monetary policy is about frictions, e.g. financial frictions. It may be misleading to assign roles to (monetary) policy for frictions, which agents could resolve themselves ("chicken paper"). DSGE models with privately optimal, dynamic contracts are then needed to eventually analyze monetary policy or other policies, when "chicken paper" assumptions are to be avoided.

3. Challenge: Inflation

The friction that most dominates typical analyses of monetary policy are pricing frictions, of course. Considerations of sticky prices and sticky wages abound in much of the recent New Keynesian literature and work horse models for much of the DSGE work done at central banks for the purpose of advising monetary policy, see e.g. Gali (2008) for a textbook treatment. That literature views itself as the modern version and continuation of the traditional Keynesian thinking, starting with Keynes (1936). That, however, may precisely be its problem. To paraphrase Keynes himself, Academic thinkers surely do not believe themselves to be exempt from intellectual influences. But in this case, the New Keynesian advocates may have indeed become slaves of a defunct economist named John Maynard Keynes, distilling their world view from this particular academic scribbler of quite a few years back. Intellectual history can be an imprisonment.

At the heart of the traditional Keynesian analysis, and thus by implication, at the heart of the New Keynesian literature, is some kind of Phillips curve tradeoff between a measure of economic activity ("employment", "output gap") or lack thereof and inflation. To shed some simple light on this matter, let me use the unemployment rate as a measure of "slack" in the economy. This choice can be critized on many grounds, of course, but if the substantial difference is in the choice of the appropriate indicator, then the literature might wish to devote considerable attention to understanding why.

Figure 4 is based on artificially generated data for unemployment and inflation to plot an example, for how a textbook version of the Phillips curve ought to look like. In that figure, there is a clearly visible negative relationship between the two. One might then be tempted (as indeed is done in the traditional Keynesian analysis) to treat this tradeoff as a menu: create a bit

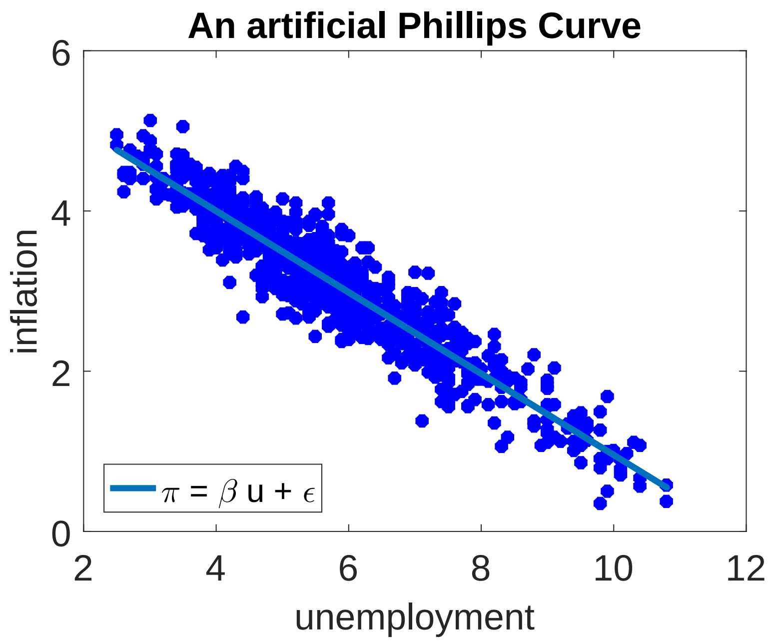


Figure 4: Classic Phillips Curve: the textbook version. Artificial data generated per $\pi_t = 6 - 0.5u_t + \epsilon$, $\epsilon \sim \mathcal{N}(0, 0.3^2)$.

more inflation and thereby reduce unemployment. The rational expectations revolution was born out of showing the non-sequitur in that traditional argument: its resurrection in the recent New Keynesian literature has addressed these rational-expectations concerns (and are now fully in the rational expectations world).

But how does this Phillips Curve tradeoff look like in the data? And how does it look like, once modified to take into account recent developments of the literature? Figure 5 provides a visual answer, using monthly US data from 1948 to 2016. The top left panel shows the traditional version of the Phillips Curve, juxtaposing inflation and unemployment. It probably is fair to call this a "Phillips cloud" rather than "Phillips curve". It probably is also fair to claim, that it would be hard to publish a paper these days, which would argue for a strong negative relationship based on such a relationship.

Put differently, if we only now had looked at that relationship, it is doubtful, that a "Phillips curve" would be central to any debates at all, as already emphasized in Uhlig (2012).

There are various ways in the literature to try to address this lack of a systematic relationship. A number of authors have proposed to look at the non-accelerating inflation rate of unemployment or NAIRU. The top right panel of figure 5 therefore juxtaposes the change in the inflation rate to the unemployment rate. But once again, it is hard to see much of a systematic relationship here.

New Keynesians may scoff at these comparisons. The New Keynesian Phillips Curve takes the form

$$\pi_t = \beta E_t[\pi_{t+1}] + \kappa x_t \tag{1}$$

where π_t is inflation, x_t is the output gap (think: negative of the unemployment rate) and $\kappa > 0$ is a coefficient. The presence of the expectation of future inflation is a key difference with the classic version of the Phillips curve. Define

$$\epsilon_{t+1} = -\beta(\pi_{t+1} - E_t[\pi_{t+1}])$$

as the one-step ahead prediction error for future inflation, scaled with $-\beta$, and rewrite equation (1) as

$$\pi_t - \beta\pi_{t+1} = \kappa x_t + \epsilon_{t+1} \tag{2}$$

This suggest a regression of the future change in the inflation rate on the current unemployment rate. The corresponding scatter plot is shown in the lower left panel of figure 5. That regression line is nearly flat. Some researchers interpret this figure and that regression result as showing a high sacrifice ratio, i.e. that large increases in unemployment are required in order to reduce inflation somewhat. My visual impression of that figure is instead, that there is little relationship here, and that this line of thinking about the inflation-unemployment relationship is instead doomed to failure.

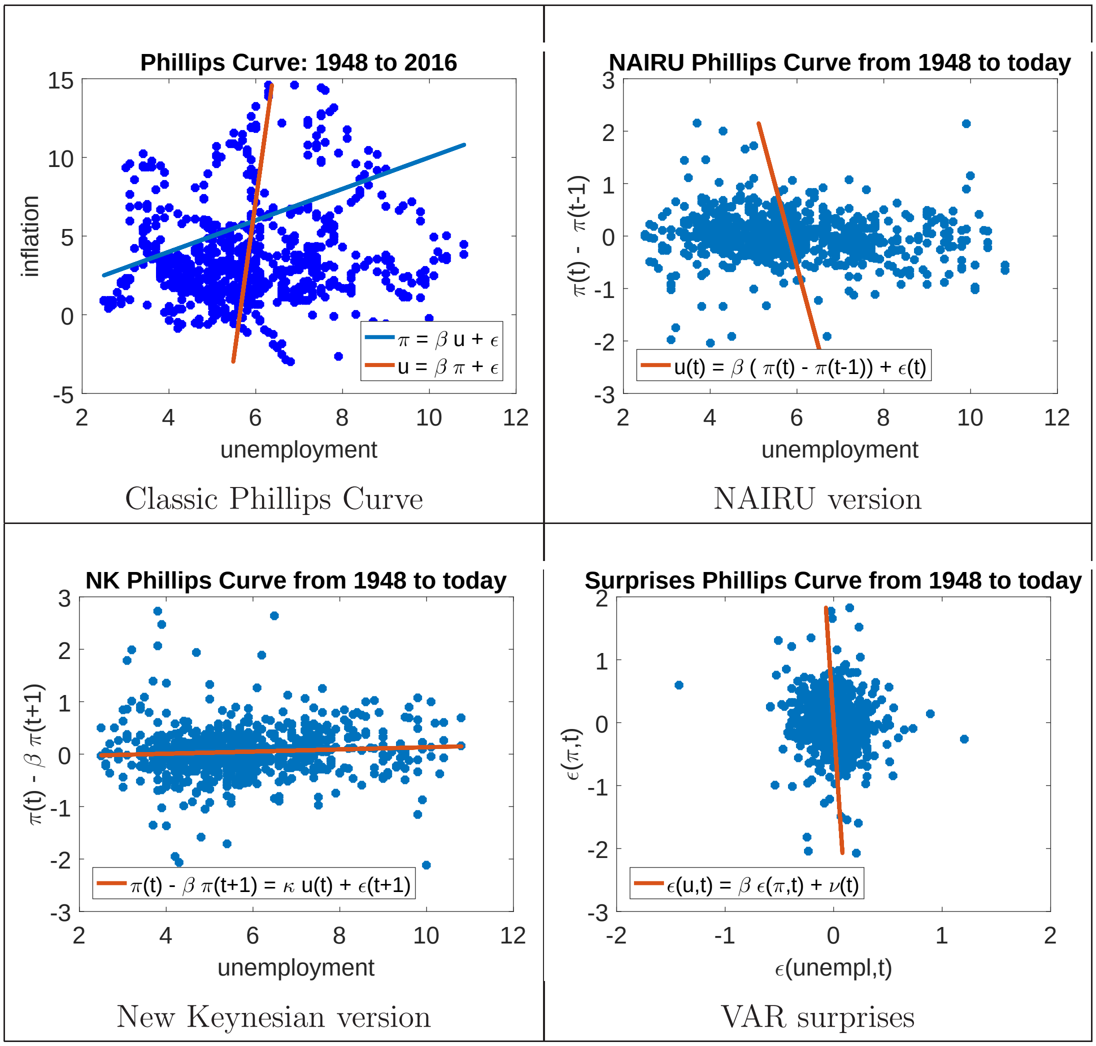


Figure 5: Four versions of the Phillips Curve in the data. For the New Keynesian version, note that according to the NK model, $\pi_t = \beta E_t[\pi_{t+1}] + \kappa x_t$. Rewrite: $\pi_t - \beta\pi_{t+1} = \kappa x_t + \epsilon_{t+1}$. Here, we use $x_t = -u_t$, $\beta = 0.99$. The VAR surprises version is based on a VAR in (π_t, u_t) with 4 lags and constants, and constructed, using the residuals.

As a last check, the bottom-right panel juxtaposes surprises in inflation with surprises in the unemployment rate, thereby eliminated VAR-based predicted movements. Once again, it is hard to see a systematic relationship.

Perhaps the bottom left panel is too simple yet, and not tuned well-enough to the current crop of NK DSGE models. To examine this in more detail, Fratto-Uhlig (2018) seeks to account for inflation, using the pre-2008-crisis benchmark medium-scale NK model by Smets-Wouters (2007), which in its original form or some variant is a popular tool in many central banks. By construction, that model decomposes movements of the macroeconomic time series under investigation into the various components and model-based shocks. One can thus ask, which shocks account for the movements in inflation. Figure 6 and table 3 provide the key results. Prior to the crisis, price markup shocks and wage markup shocks account for pretty much the entire movements in inflation: inflation, in essence, dances to its own music. Remarkably, it is only post crisis, that other economic shocks show an influence on inflation: so, if anything, as a non-own-music-account for inflation, the Smets-Wouters (2007) model works better after the crisis than before. One can try to fix this up in various ways, see e.g. del Negro et al (2015). Nonetheless, I strongly encourage any DSGE model analysis, meant to deliver guidance for monetary policy, to provide such a decomposition. It may well be that monetary policy is simply too predictable to have had any measurable impact on inflation. But given the large changes in the inflation rate in the post-war U.S. history, I am not sure that this is an attractive interpretation to live with. It sure would be nice to see that monetary policy did something about it, sometimes.

Despite all this, there is a large literature, estimating Phillips curves, their slopes and their movements over time, and to put some version of Phillips-curve thinking at the core monetary policy analysis. I do not wish to entirely exclude that the simple pictures above and my analysis in Fratto-Uhlig (2018) obfuscate a truly powerful relationship, or that one cannot make the data confess to such a relationship, if one just tortures it long enough. I

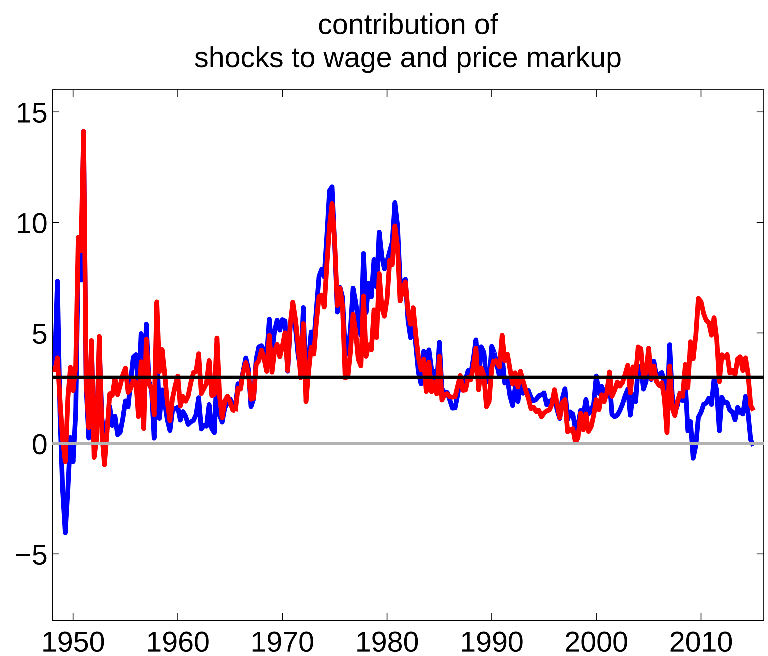


Figure 6: Inflation vs contrib. from wage + price markups. Source: Fratto-Uhlig (2018)

	1948-2015	1948-2007	2008-2015
Technology	3.90	3.99	3.72
Price Markup	51.09	54.34	26.92
Wage Markup	27.04	22.09	61.72
Preferences	7.65	8.32	2.80
Inv.Spec.Tech.	3.54	3.88	1.65
Gov't Exp	0.43	0.48	0.16
Monetary Policy	6.33	6.88	3.01

am just skeptical that this is the most fruitful way to proceed. As a scientific community, it may be worth a thought to ditch a tradeoff, which hardly seems to be there in the data, rather than desperately clinging to it and celebrating any judiciously chosen analysis seeming to demonstrate it. It may be the lack of a well-spelled-out alternative and the fear of the unknown that prevents many from taking that leap. If so, the development of such an alternative would seem to be a crucial task and challenge.

A summary of this inflation challenge is this. One needs strong imagination to tease a Phillips Curve out of the data. This applies to the classic version, the NAIRU version, the NK version and the VAR surprise version alike. The benchmark medium-scale-NK Smets-Wouters model implies that inflation is nearly entirely driven by price and wage-markup shocks (except since 2008). While there are various fixes in the recent literature, one may nonetheless remain skeptical that the current crop of DSGE models generates a sufficiently reliable link between monetary policy and inflation.

4. Challenge: Neo-Fisherian features of New Keynesian models

For better or worse, the New Keynesian model and its various varieties have become the workhorse model and the benchmark approach for thinking about monetary policy in many central banks and academic circles alike. Part of the attraction of that approach is that it advertises itself to be in the tradition of conventional Keynesian analysis, which (for better or worse) is still the bread-and-butter framework taught in many undergraduate textbooks on macroeconomics and thus is likely to form the economics training of practical central bankers. The New Keynesian paradigm is advertised as being up to date and at the frontier, incorporating rational expectations and variety of other demands on modern macroeconomic theorizing, while keeping such traditional relationships such as Phillips curves and IS curves. The New Keynesian IS curve, for example, is a little-disguised version of the Lucas asset pricing equation. One surely does have to applaud the New Keynesian literature for a marketing campaign well done. Congratulations.

What is less appreciated is that this "updating" of the Keynesian paradigm leads to implications that feel quite distinct from that conventional reasoning. I shall exposit one of them here in particular: the Neo-Fisherian features of New Keynesian models. There is some recent literature, in particular by Cochrane (2016) pointing this out. One feels that these insights are sometimes dismissed by New Keynesian advocates as weird implications, arising for unusual parameters or unconventional solution techniques. It may therefore be worth pointing out, that they arise remarkably easily in the most conventional of settings of the simple benchmark three-equation New Keynesian model. I suspect that these features are well known among New Keynesian advocates. I suspect even more, that these features are not widely advertised to policy makers.

That benchmark three equation New Keynesian model is given by

$$\begin{aligned} \text{IS:} \quad x_t &= E_t[x_{t+1}] - \frac{1}{\sigma}\left(i_t - E_t[\pi_{t+1}] - r_t^n\right) \\ \text{Phillips:} \quad \pi_t &= \beta E_t[\pi_{t+1}] + \kappa x_t \\ \text{Taylor:} \quad i_t &= \rho + \phi \pi_t + \xi x_t + \nu_t \\ \text{Persistence:} \quad \nu_t &= \psi \nu_{t-1} + \epsilon_t \end{aligned}$$

where x_t is the output gap, r_t^n is the "natural" real interest rate, i_t is the nominal interest rate and $\sigma, \beta, \kappa, \rho, \phi, \xi, \psi$ are parameters. There is a small wrinkle here. There is a forth ("persistence") equation, specifying the driving disturbance ν_t to the "Taylor rule", i.e. the monetary policy rule, to be given by an AR(1) process, with autoregressive parameter ψ and iid innovation ϵ_t. The process is often not explicitly specified, when examining the first and standard three equations. For the purpose of guiding monetary policy analysis, it certainly is entirely appropriate to add such a specification, though: surely, policy makers want to understand the impact, if they temporarily deviate from the tight prescriptions of the Taylor rule, and if they allow that deviation to linger for a while. This is what the model is meant to allow (if it is to provide any guidance for monetary policy at all) and this is what is therefore done here.

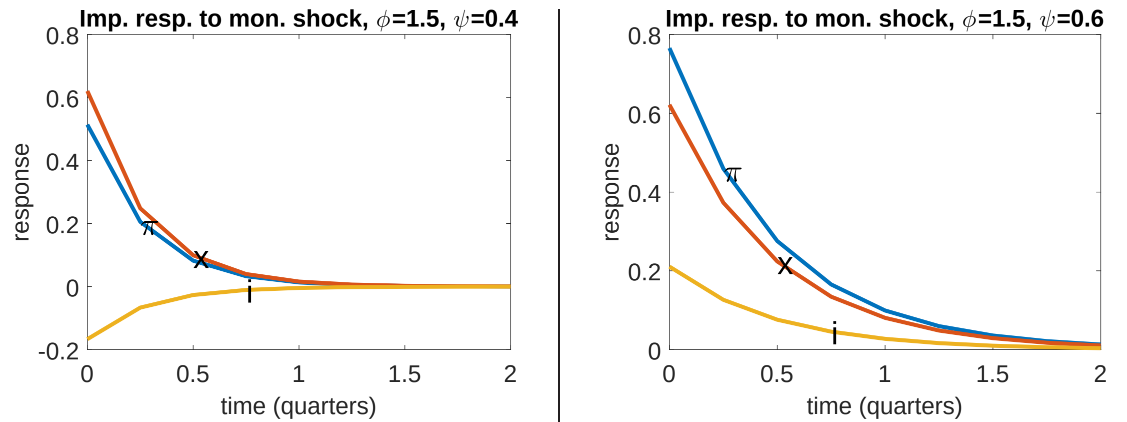
I shall pick the following parameters. First, I shall set $r_t^n \equiv 0$: this is fine for the purpose of computing impulse responses, which I will do next. For the same reason, I set $\rho = 0$: the impulse responses should then be read as deviations from a steady state. I set $\beta = 0.99, \kappa = 0.5, \sigma = 1$, $\xi = 0.1$, $\phi = 1.5$ and either $\psi = 0.4$ or $\psi = 0.6$. These parameters strike me as fairly conventional. One might view $\xi = 0.1$ as a rather low value. $\xi = 0.5$ might have been a better choice, but that one gives even weirder results. I interpret a period as corresponding to a quarter.

Figure 7 shows the impulse responses to a $\epsilon_0 = -1$. As is the tradition in that literature and despite the critique of Cochrane (2016), I shall present the impulse responses arising from the stable roots of the system (which here is simply the root ψ for the exogenous process η_t). Let me call the shock $\epsilon_0 = -1$ a "first round" surprise drop in nominal interest rates. The NK model is a system of simultaneous equations, ultimately determining the outcome for the nominal interest rates. The equilibrium nominal interest rate takes into account these "second round" effects of the repercussions of that "first round" shock on all the variables in the system. This logic manifests itself in these impulse responses.

The left panel shows a rather conventional version and impact of such a surprise "first round" cut in the nominal interest rate. Inflation and the output gap both go up. Because they do and because the Taylor rule endogenously reacts to these increases with some tightening and interest-rate increase, the initial drop in the nominal rates equals to about -0.2 and does not equal the first round surprise: that first round surprise is mitigated a bit.

For the right panel, the persistence of the monetary policy shock is increased a bit, from $\psi = 0.4$ to $\psi = 0.6$. Put differently, after one year, the disturbance η_t to the Taylor rule equals -0.13 rather than -0.03. This is still a small number, indicating a rather temporary disturbance overall.

While inflation and the output gap still increase, as they do in the left



Impulse responses to $\epsilon_0 = -1$, if $\psi = 0.4$ | Impulse responses to $\epsilon_0 = -1$, if $\psi = 0.6$.

Figure 7: *Impulse responses in a three-equation New Keynesian Model.*

panel, nominal interest rates now increases as well. The endogenous second-round effects now more than compensate for the first-round decrease of the nominal interest rate. Put differently, the inflation now rises so much, that the Taylor rule endogenously forces the central bank to raise rates, in order to combat these movements. One can imagine these dynamics to come about from a somewhat topsy-turvy discussion in the governing council of a central bank, running as follows: "let's stimulate inflation and the output gap, by cutting the interest rates and doing so with some persistence. We obviously all understand, that we then have to tighten interest rates in order to cool off these developments. As a results, we shall end up raising rather than lowering rates now." I can see how this could be confusing. It may be easier to approach this from the outcome of the calculations right away. Following those deliberations, the central bank president could then announce to the eagerly waiting press, that it was decided to raise nominal interest rates and somewhat persistently so, as it is the judgement of the council, that this will increase inflation rates and the output gap.

There are a number of ways to react to these calculations in the right panel and to such an announcement by a central bank president. One reaction is to view them as entirely logical. Of course, there is a Fisherian effect! If the inflation is higher, nominal interest rates need to be higher too. However, from a conventional Keynesian analysis, the right panel does look odd: it looks as if a somewhat persistent rise in nominal rates rather than a somewhat persistent drop in nominal rates will stimulate the economy.

These calculations also point out, that the zero lower bound should have hardly been a concern. In order to stimulate the economy and to move away from the specter of deflation, all that the central banks needed to have done post-2008 is to raise nominal interest rates with some persistence.

I do not wish to judge here, whether I trust this conclusion as a matter of practical policy advice, though my intuition tells me to attach ample warnings to it: driven though, possibly, by faulty intuition due to the influence by that defunct academic scribbler mentioned previously. More importantly, though, I just wish to point out that this is a straightforward implication of the most basic, standard New Keynesian model, for an entirely reasonable set of parameters.

One can treat this all as a "bug" of course. There are "fixes" in the literature to address this implication, see e.g. Garin et al (2016), as well as other "odd" implications of the benchmark New Keynesian literature, such as the forward guidance puzzle, the reliance on Euler equations and the like. One has to wonder, whether the direction to deviate from the simple three-equation New Keynesian model is unique. If not, then it should be likewise equally legitimate to treat these properties as a feature and not a bug, and be allowed to deviate from the baseline model such as to enhance them. Which one then is most appropriate for monetary policy advice? Why is the "bug" perspective more appropriate than the "feature" perspective? Is it legitimate to return to conventional Keynesian logic and intuition time and again as a model selection device? We may risk being stuck in groundhog day forever. This echoes the concerns more formally addressed in Gilles Saint-Paul (2018).

A summary of this neo-Fisherian challenge is this. The New Keynesian model is "sold" as a modern version of the textbook IS-LM thinking. It works quite differently, though: perhaps very differently. Consider for example, the zero lower bound problem. The benchmark three equation model implies that there shouldn't be much of a zero lower bound problem, if you ever land there by accident: just raise rates and promise to keep them above the Taylor rule target for a little while, in order to increase inflation and economic activity. New Keynesian theory is a lot more Neo-Fisherian than their advocates may lead us to believe, see also Cochrane (2016). The New Keynesian model generally appears to have several "odd" features, which may call into question whether it is all that useful as a reliable guide for monetary policy as its advocates have us believe.

5 Conclusions

DSGE models were meant to rise to the Lucas (1976) challenge of constructing general equilibrium models with deep parameters, to be used as workhorse models for e.g. monetary policy analysis. But challenges remain. This paper has discussed a few of them. The central issue of the impact of monetary policy on yield spreads and asset prices is often ignored in DSGE models or kept essentially separate from allocations. While much progress has been made on considering financial frictions in DSGE models, the models need to allow more room for private markets to address these frictions, rather than to call on economic policy for their resolution. The Phillips-curve tradeoff, so central to thinking about monetary policy and much of the DSGE literature, may hardly be there. New Keynesian models, which have largely become the benchmark approach to thinking about monetary policy in central banks, have neo-Fisherian properties. These are rarely advertised or, alternatively, treated as "bugs" by researchers, driven by conventional thinking about how monetary policy works, rather than an opportunity to escape the groundhog day of repeating conventional insights. A constructive approach here might be to apply the approach of Brinca et al (2016) and extend it to the realm of these and other challenges to provide a guide to future research. In sum, the glass is half full. Or half empty. Take your pick.

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