KU LEUVEN

FACULTY OF ECONOMICS AND BUSINESS

Leaning Against the Wind When Inequality Bites Back

Financial Stability, Monetary Policy and Inequality

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Thesis Submitted to Obtain the Degree of

Master of Economics
Major Research in Economics

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Academic Year: 2018-2019

Abstract

Leuven, June, 2019.

This thesis studies the links between monetary policy, inequality, leverage and financial crises in a Bayesian VAR analysis of a panel of advanced inflation-targeting economies, wherein the policy rate is adjusted for the zero lower bound by means of shadow rate estimates. That way, it finds that monetary policy's ability to foster financial stability by leaning against a build-up in leverage is significantly hampered by that policy's distributional side-effects. Specifically, both discretionary and systematic contractionary monetary policy are found to increase inequality. As inequality is in turn found to be cointegrated with leverage, contractionary monetary policy's efforts to curb the credit cycle are shown to be partially self-defeating, besides leading to welfare losses in the form of both decreased output and increased inequality.

Acknowledgements

I would like to thank my family for their support: my mother, for continuing to support me in what I do, even while bravely fighting breast cancer; my father, for his gentle, reliable presence and calm advice; my sister, for inspiring me to reach beyond the obvious.

Furthermore, I would like to thank dr. Olivier Coibion, dr. Michael Kumhof, dr. Isabel Caíro, dr. Andrew Filardo and dr. Mathias Drehmann for sharing their data and code with me and/or answering my questions. Also, the researchers at the European Central Bank and the Bank of New Zealand, for making their algorithms publicly available.

Lastly, I want to thank my supervisor prof. dr. Ferre De Graeve, for his advice, both academic and intellectual; and drs. Federica Brenna, for her active engagement throughout the whole process of writing this thesis, which is much appreciated.

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Chapter 1

Introduction

There is a long-standing question in the monetary policy debate that asks whether monetary policy should concern itself with matters of financial stability. As monetary policy's main instrument, the interest rate, sets the bar for the economy-wide price of credit, some argue that monetary policy could be effective at pro-actively "leaning against" the build-up of financial imbalances. Critics of the idea, however, argue that monetary policy's strong impact on the real economy would lead to output losses too high to be justified by the gains from improved financial stability. Although an earlier incarnation of this debate seemed to have reached some sort of consensus against the idea of a financial-stability targeting central bank (Bernanke and Gertler, 2001), after the crisis of 2008, the debate has flared up in renewed vigour. The newborn version of the idea shifted focus from the question of whether monetary policy should target asset prices to the suggestion that monetary policy should systematically lean against the so-called 'financial cycle', which is marked by protracted cyclical fluctuations in financial indicators, amongst which most prominently the credit stock. The 2008 crisis also revived the debate about another perennial issue, namely that of economic inequality. Not only did the crisis badly affect vulnerable income groups, inspiring nightmarish reminiscences of the social unrest that followed the Great Depression in the 1930s, but some even argued that the long-run rise in inequality witnessed across advanced economies around the world had been at the root of the fault lines that led to the financial crisis (Rajan, 2011). The excessive build-up in debt that had preceded and ultimately triggered the financial crash was suggested to be the amphetamine that kept a fundamentally unstable income distribution from dragging the economy into a morass of underconsumption and excess savings (Stiglitz, 2016). One particular issue that attracted new attention concerned the distributional impact of monetary policy. As central banks around the world started printing money and purchasing assets at an unprecedented scale in the context of their post-crisis Quantitative Easing (QE) programmes, public and academic interest in the possible unintended inequality effects of such policies surged.

This thesis makes the connection between these two old debates by asking the novel question of to what extent the distributional side-effects of monetary policy affect its ability to target financial stability. The hypothesis under scrutiny is that contractionary monetary policy intended to mitigate excess leverage in the economy has an unintended consequence in the form of higher income inequality. This ultimately leads to higher leverage, as an excess of savings at the top of the income distribution is transformed into credit to meet an excess demand for loans at the bottom. In this light, this thesis finds convincing evidence that both discretionary and systematic contractionary monetary policy lead to higher income inequality, which in turn leads to a long-term increase in the level of credit in the economy. It does so in the setting of a Bayesian vector auto-regressive model estimated on a panel of advanced, inflation-targeting economies. Discretionary monetary policy is studied by way of structural identification based on sign restrictions (Arias et al., 2018), while systematic monetary policy is studied by way of a counterfactual policy experiment. The sample period under study includes the 2007 financial crisis, which is made possible by the estimation of so-called shadow policy rates, which reflect the counterfactual evolution of the policy interest rate, had it been possible for it to fall below the zero lower bound. Meanwhile, the long-run relationship between inequality and credit is studied by way of a panel cointegration analysis. Overall, the combination of these methodological approaches leads to the conclusion that the ability of monetary policy to target financial stability is significantly impaired by its unintended distributional side-effects.

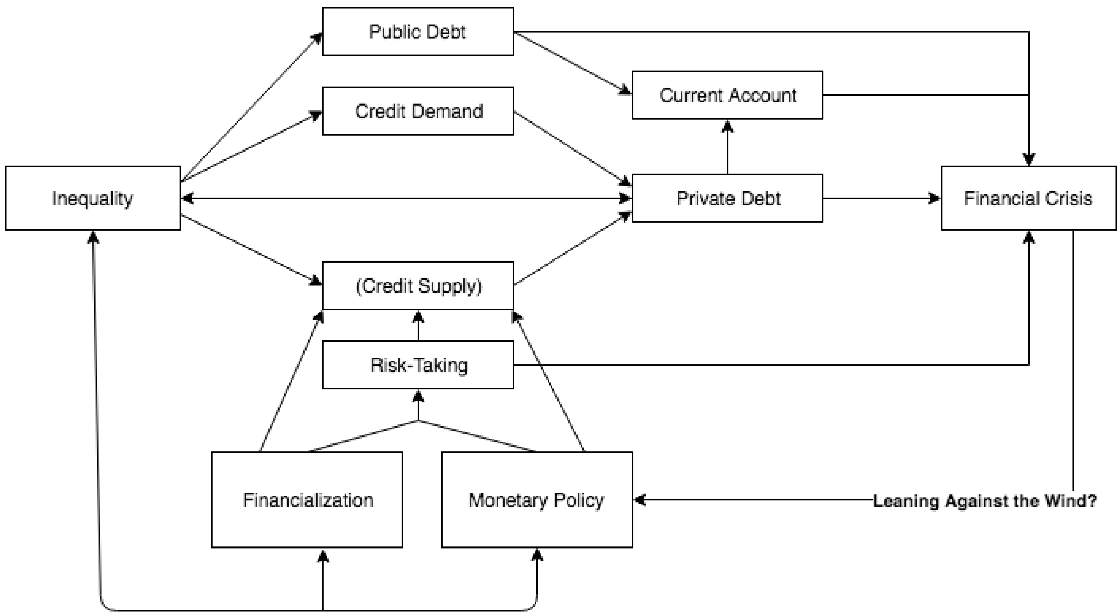
Chapter 2

Inequality and Leaning Against the Wind

This chapter discusses the various strands of the economic literature that touch on the central question of this thesis. As a reminder, that question asks in what way household income inequality affects monetary authorities' capacity to lean against the wind (LAW), that is, to actively manage unhealthy debt growth in the economy by dampening the business cycle. In what follows, I break down the mechanisms involved in this question into three parts. First, I briefly discuss the main tenets of inflation targeting, the monetary policy framework currently prevailing in advanced economies. Second, I review the recent academic debate on LAW. Third, I discuss the newly emerging literature that looks at the distributional effects of monetary policy, and discuss how these relate to the linkages between inequality, leverage and crises that were recently put forward by several authors as long-term explanations of the Great Recession. The hypothetical system of relationships that emerges from these strands of the literature is represented in 2.1 below. Before delving deeper into the strands of literature that treat the relations depicted, I now briefly discuss the broader mechanism underlying them.

Monetary policy lies at the heart of the whole system, by grace of the consensus

Figure 2.1: The Interrelationships Between Monetary Policy, Inequality, Leverage and Crises. Based on Bazillier and Hericourt (2017).



about the short-term non-neutrality and thus efficacy of such policy that prevails in New-Keynesian monetary theory, which is the current dominant paradigm in monetary economics. That same paradigm provides the intellectual justification for inflation targeting, which is discussed in the section 2.1.1 below. Disagreement about the extent of monetary neutrality resurfaces in the LAW debate, treated in section 2.1.2 below. The central question of that debate is whether monetary policy should target financial stability in any of various ways, so as to attenuate the shock of financial crises and reduce their frequency. The literature suggests three main channels through which it could do so, with private non-financial credit demand taking center stage, but risk-taking and credit supply in addition to public debt also playing smaller roles. The reason credit demand takes center stage is that a slew of recent studies have demonstrated that private credit booms have played a major role in the development of financial crises (Schularick and Taylor, 2012; Jordà et al., 2016; Mian et al., 2017).

One important account of a possible underlying driver of credit booms, first suggested by Rajan (2011) for the U.S. in the wake of the Great Recession, is that of an economy marked by increasing income inequality which sustains consumption levels by taking on increasing amounts of private debt. The increased demand for credit would be accommodated by easier supply thereof, encouraged by a political establishment that seeks to aid a vanishing middle class in maintaining its standard of living, which also explains the link with public debt (Rajan, 2011, §1). Some argue that with these evolutions comes an expansion of the financial sector, whose development is encouraged by accommodating political authorities. This financialization of the economy could, in turn, decrease inequality by extending access to finance to the poorest sections of the population (Levine, 2005, pp.866-934); or, on the contrary, increase it, by gobbling up increasing shares of production surpluses (Lin and Tomaskovic-Devey, 2013). I discuss further theory and evidence of how inequality and finance interact in section 2.2.2 below. The connection that closes the system runs between monetary policy and inequality. It might seem surprising that in Figure 2.1, any effect monetary policy has on credit demand runs through inequality. The motivation for that is the consideration that monetary policy, by manipulating the cost of borrowing, fundamentally works through redistribution, be it from debtor to creditor or from borrower to saver. As Andrew Haldane, the current chief economist at the Bank of England puts it: "All public policy is distributional" (Haldane, 2018, p.311). In this sense, the 'inequality' in the figure should be understood broadly, as any kind of distributional difference. Nonetheless, research into the interplay between income and wealth distribution and monetary policy has only recently taken off. I cover the insights from this literature that are relevant to this thesis in section 2.2.1.

2.1 Financial Stability and Monetary Policy

To begin the discussion of the various mechanisms involved, this section sets the recent academic debate about the desirability of leaning against the wind off against the monetary policy regime that currently prevails in advanced economies' central banks, inflation targeting.

2.1.1 Inflation Targeting: The Central (Bank) Paradigm[1]

As a practical approach to the conduct of monetary policy, inflation targeting first found foothold in the Bank of New Zealand in 1990 (Svensson, 2010, §1). From there, it spread to other central banks throughout - mostly - the developed world, culminating in its adoption by the Federal Reserve in the U.S. in 2010.[2] Inflation targeting is characterized by three main features,

> 1) an announced numerical inflation target, 2) an implementation of monetary policy that gives a major role to an inflation forecast and has been called forecast targeting, and 3) a high degree of transparency and accountability (Svensson, 2010, §1).

Three aspects are worth highlighting here. First, the numerical target is often around 2% per annum for the Consumer Price Index, and is not categorical; for example, the European Central Bank defines its target as "below, but close to 2 percent" (European Central Bank, 2019*a*). Additionally, real-world inflation targeting is always "flexible", which means it also puts a weight on other variables besides inflation, most commonly the output gap, and adjusts its conditional inflation forecast only gradually towards the target.(Svensson, 1999, p.338). Second, the need for forecast targeting arises from the lag between the implementation of monetary policy and its effects on target variables.

[1]This section closely follows the exposition of Svensson (2010).

[2]Note that although both banks have explicit inflation targets, they do not explicitly describe themselves as inflation targeters.(Meyer, 2001).

Third, transparency and accountability in central banking serve functions similar to the functions they serve in other democratic institutions: to allow for internal evaluation and external scrutiny, to foster consistency in policymaking, and to provide clearly defined objectives. In addition, such standards also arise from the specificity of the monetary policy exercise: since the latter to a large extent involves "the management of expectations", clearly communicating a future policy path and committing to it helps stabilizing inflation around its target (Svensson, 2003, 2010, p.13, §1.3) and leads to equilibria superior to those attained under discretionary policy (Evans and Honkapohja, 2006). The credibility gained thereby, measured by how close private-sector inflation expectations are to target inflation, ultimately provides the central bank more flexibility in its policy actions (Svensson, 2003, p.9).

The arguments for inflation targeting, aside from some of its inherently desirable features, can be found both in theory and practice. In many theoretical models, price stabilization tends to arise as an objective of the optimal monetary policy even without the policymaker putting weight on it *a priori* (Galí, 2015, p.75). Empirically, inflation targeting has been found to reduce the level and volatility of inflation, which promotes certainty regarding the price mechanism and thereby reduces unproductive distortions in the economy (Svensson, 2010, §2.2). Additionally, price stability avoids arbitrary redistributive effects arising from shocks to inflation, and encourages investment by reducing inflation risk premia European Central Bank (2019*b*). Framed contrapositively, empirical analysis has not found convincing evidence of inflation targeting negatively impacting output growth or volatility Ball and Sheridan (2004). The appeal of inflation targeting is thus supported extensively by both theoretical and empirical research.

Nonetheless, inflation targeting has come under attack in the wake of the Great Recession. The gist of the criticism seems to have been that inflation targeting central banks had become too obsessed with their target, that is, they had become too "strict" and had thereby exacerbated financial stability risks (Giavazzi and Giovannini, 2010). Some argued that the crisis demonstrated the need for central banks to think beyond mere price stabilization towards the prevention and control of financial crises (De Grauwe, 2008). Echoing such concerns, others suggested that price level stabilization also led to instability in real variables, along with unemployment levels that were higher than needed for economic stability, thereby aggravating volatility and inequality (Stiglitz, 2012, p.43-44). The events of the crisis thus prompted a re-evaluation of central bank policy orientation.

In that way, the criticism of inflation targeting contributed to a revival of an older debate that asked whether monetary policy should target asset prices. Early contributions in that debate often relied on models that included a financial accelerator. On that basis, this older debate more or less settled on the consensus that it is undesirable for the central bank to respond to changes in asset prices directly - apart from the impact such changes have on the bank's inflation forecast - as there are little gains in terms of reduced output or inflation volatility (Bernanke and Gertler, 2001). A notable contribution that questioned this consensus was Borio and Lowe (2002). Foreshadowing much of the current LAW debate, this paper postulated that focusing on asset price movements alone is unhelpful as financial imbalances are often disguised by benign economic conditions. Thus such imbalances are only identifiable by looking at the interaction of various symptoms resulting from the progression of a financial cycle distinct from the business cycle, most prominently asset prices and credit growth (Borio and Lowe, 2002, §3). Additionally, the paper contended that while low and stable inflation does promote financial stability, financial imbalances can and do still build up under an inflation targeting regime. Such a build-up could even be indirectly enabled by a credible central bank taking the sting out of the inflationary pressures that would otherwise result (Borio and Lowe, 2002, §4). It is thus clear that the argument for leaning against the wind was not fully rebuked during the period when inflation targeting took hold. In the next part, I discuss the post-crisis reincarnation of this argument.

2.1.2 Leaning Against the Wind: Macroprudential or Monetary Policy?

Macroprudential Policy and Coordination

As the post-crisis LAW debate has evolved, two largely incompatible views have emerged. The first view maintains that monetary policy should not lean against the wind because it is inefficacious in doing so. Monetary policy should at most target financial stability in a secondary fashion, by coordinating with macroprudential policy, which *is* efficacious in leaning against the wind. The second view, by contrast, sees merit in monetary policy leaning against the wind in and by itself, regardless of whether macroprudential policy is better suited to the task.[3] This view is often motivated by citing the uncertainty around macroprudential policy's broad-based effectiveness, the lack of a unified framework to study macroprudential policy, or the larger difficulties with macroprudential policy's implementation.

The first view fits well into what Smets calls the "modified Jackson Hole consensus" (Smets et al., 2014, p.269). The view is heir to the earlier consensus in the asset-price monetary policy debate which argued that financial stability is a concern for monetary policy only insofar is it affects forecasts of price stability and output. Financial stability concerns should be addressed by counter-cyclical macroprudential policy, in line with the "Tinbergen principle" that dictates that each policy instrument should have only one task. The effectiveness of interest rate policy in mitigating financial imbalances is questioned, or considered irrelevant in light of macroprudential policy's greater efficacy in doing so, resulting from its stronger positive effect on financial stability combined with its smaller negative effect on the macroeconomy. In other words, an interest rate

[3] Agur (2018) claims that even proponents of leaning do not think monetary authorities should attach as large a weight to financial stability as macroprudential authorities, and that this implies that when it comes to leaning, "the relevant question is whether introducing some degree of leaning facilitates coordination" (p.4). Whereas I agree with the premise of this statement, I disagree with the conclusion. Though contrarian, there are several papers that argue for monetary leaning in and of itself, as a means to curb the financial cycle.

hike is considered to only reduce excessive lending moderately, while it has a significant impact on output; an increase in for example a minimum capital requirement for banks is considered to materially improve financial stability, while having a smaller negative impact on output. A point that seems uncontentious for both sides of the LAW debate is that the central bank should have a strong macroprudential mandate in addition to its price stability mandate, since granting this mandate to the central bank allows for information-sharing, ensures independence and expertise, and aligns well with the central bank's incentive, as lender of last resort, to prevent financial crises (Smets et al., 2014, p.287). In that light, much of the literature focuses on the coordination of both policy instruments. The suggestions here range on a continuum from complete separation to full coordination of responsibilities. Smets (2014, p.290-91) argues for the former on the basis that coordination could lead to time-inconsistency issues, as the central bank would be tempted to decrease regulation and inflate away the resulting debt overhang with expansionary monetary policy. An argument for the latter is presented in IMF n.d., which sees benefits in coordination, as both tools can attenuate each other's negative side effects when clearly sticking to their own mandates. Other research situates itself in-between the extremes of complete separation and active coordination. Notable contributions include Aikman et. al. (2018, p.20-26), who find that both instruments act as substitutes in a New-Keynesian model with a probabilistic financial crisis, augmented to include a macroprudential policy lever. In their model, an efficacious counter-cyclical capital buffer is tightened in the face of a credit boom while its macroeconomic impact is offset by looser interest rate policy (Aikman et al., 2018, p.20-26). In contrast to IMF n.d., the authors find negligible gains from coordination. Their findings do complement those of Ajello et. al. (2019) which suggest that when just interest-rate policy is available, financial stability concerns imply only a very small amount of leaning. Another paper that supports the in-between case is Agur (2018). It studies coordination in a set-up with multiple equilibria. If one tool is coarse (macroprudential policy) while the other is unconstrained (monetary policy), a game arises where each authority will prefer a different equilibrium. The cost of coordination will then be hump-shaped in the degree of interest-rate leaning (Agur, 2018, §IV-V). As the case for strong leaning is not often made, such a hump-shaped relation would then imply that policy separation trumps policy coordination. Yet another paper by Laureys and Meeks (2018), however, asks the insufficiently examined question of whether rules-based policy still outperforms discretionary policy when there are two monetary policy instruments. The paper, using a DSGE model with banks and borrowing constraints, obtains the intriguing result that, when the authorities play a Nash game, rules-based policy indeed outperforms discretionary policy, conform with traditional results from the literature. Yet when a single authority is in charge of both policy tools (when there is cooperation, in the sense of distinct objectives but a combined loss minimization), the expected central bank loss is much lower for both approaches, but lowest for discretionary policy (Laureys and Meeks, 2018, p.106). The explanation for this result is that rules-based inflation stabilization induces demand-driven volatility in the credit-to-output ratio that macroprudential policy cannot fully off-set by manipulating the credit supply. While this paper does not consider an interest-rate rule with an explicit financial stability objective, it does suggest that a discretionary monetary policy might want to take financial stability into account. Nonetheless, it stands quite apart from the other literature on the topic, which generally points to a separation of the objectives and implementation of rules-based monetary and macroprudential policy.[4]

Systematic Monetary Policy Leaning: Beyond Cost-Benefit?

The consensus on which much of this research rests, that pure interest-rate leaning irrespective of any macroprudential leaning is unwished-for, has arisen from an influential

[4] One of the few empirical studies on the subject seems to corroborate this preliminary conclusion, as it finds no evidence for the complex interactions between macroprudential variation in capital requirements and monetary policy included in most of the theoretical models (Aiyar et al., 2016).

paper by Svensson (2017*a*), wherein he develops a simple and transparent framework for a cost-benefit analysis of LAW. Based on empirical estimates, taken from the literature, of the key nodes through which interest-rate leaning impacts the economy, Svensson shows that the marginal costs of LAW outweigh the benefits for a large range of such estimates. On the one hand, the main cost factors of LAW are the increase in the unemployment gap in non-crisis times and the crisis loss increase relative to the non-LAW case that arises from the economy being in a worse state at the onset of the crisis. On the other hand, the main benefits from LAW obtain from a lower crisis probability (and thus a reduced crisis frequency) and a reduced crisis magnitude (Svensson, 2017*a*, §3). Though these beneficial effects are an intuitive attraction of a LAW policy, the marginal benefit arising from these effects are quite small. In all, Svensson's extensive use of estimates from well-supported empirical research combined with the transparency and relative simplicity of his cost-benefit framework left a decisive mark on the LAW debate, in favour of the "modified Jackson Hole consensus".

Nevertheless, a second view on the LAW debate, which argues for a systematic, through-the-cycle policy of interest-rate leaning, still stands firm. A common motivation for such monetary leaning over and above any macroprudential leaning is that macroprudential policy can by its nature only reach regulated areas of the financial sector and thus could lead to spill-overs to the shadow banking sector. For example, one empirical study finds that while effective in regulating bank credit, macroprudential policy also leads to substitution effects to non-bank credit (Cizel et al., 2016). Another study finds substantial spill-over effects to foreign banks resulting from the imposition of domestic capital requirements (Aiyar et al., 2014). Monetary policy, on the other hand, "gets in all the cracks" (Stein, 2013). Yet while this argument is often recurred to, one of the few papers that includes a market-based finance sector in its model, does not find support for monetary leaning, but only for less active macroprudential policy than if there were no market-based finance (Aikman et al., 2018, p.41). The approach taking in these and other papers that argue against monetary leaning against the wind, however, is criticized by the Bank for International Settlements (BIS) for its negligence of the persistence of the financial cycle by modeling crises as occurring according to a probability distribution over the degree of leverage (BIS, 2014, 2016). The distinguishing features of such a financial cycle are that is longer than the business cycle; that is phase-dependent and relatively regime-independent; and that features a persistent build-up in credit, consistent with an endogenous risk-taking channel (Filardo et al., 2018, §4-5). As this cycle progresses, financial imbalances build up and the probability of a financial crisis increases. The main difference with the "random-crisis" modelling approach is that in the latter, crisis probability eventually declines after a positive shock to the interest rate, as leverage is mean-reverting; while in a financial-cycle approach, monetary tightening can reduce the crisis probability also further down the road because of the cycle's persistence (Filardo and Rungcharoenkitkul, 2016; Woodford, 2012, §.3.1, p.14). A tightening today thus has persistent effects on the future evolution of the financial cycle, and systematic through-the-cycle monetary leaning becomes an optimal strategy (Filardo and Rungcharoenkitkul, 2016, §.3.2).

Notwithstanding the appeal of the systematic leaning approach, given recent empirical evidence on the distinct nature of financial cycle (Drehmann et al., 2012), it is plagued by two main conceptual issues. First, as Svensson notes, there is no reason why tackling the LAW problem from a systematic leaning perspective should alter its structure, as the temporary interest rate shock that features in the marginal cost-benefit approach can be justified on the basis of a calculus of variations argument. Under such an argument, the optimality of a policy can be tested at the margins by evaluating the impact of a temporary deviation. As a matter of fact, given that studying a systematic leaning approach requires the estimation of a structural model, the cost-benefit approach might even be superior to it, since it provides a more transparent and empirically motivated framework.[5] Second, Svensson criticizes the financial approach taken in Filardo and Rungcharoenkitkul (2016) by pointing out that it fails to take into account the endogenous downturn costs associated with a LAW policy. He then shows that, with exogenous downturn costs and assuming monetary policy has a persistent effect of interest rate changes on debt, a small degree of leaning (17 pp) is indeed optimal (Svensson, 2017*c*, p.14), similar to some of the results in Filardo and Rungcharoenkitkul (2016). Nonetheless, Svensson argues that such a small degree of leaning is not worth overhauling years of central bank credibility for, particularly since he thinks the assumptions it is based on are not realistic. Additionally, when only allowing for non-neutral monetary policy, but leaving out the exogenous downturn cost assumption, Svensson finds no net marginal benefit of leaning, thus partially undermining the argument that persistency in the credit cycle would change the picture (2017*c*, §4.1). This result is supported by Kok and Kockerols (2019), who apply the cost-benefit approach to the Euro area while also taking financial cycle dynamics into account by deploying the ECB's Systemic Risk Indicator. In sum, the main criticisms of the financial cycle approach are that it does not fundamentally alter the structure of the problem, as it claims it does, and that it has failed to take into account endogenous downturn costs.[6]

Four important issues with the foregoing criticism should, however, be noted. First, by Svensson's own calculus of variations argument, it is incorrect to regard the magnitude of the optimal policy rate change obtained by way of the cost-benefit approach as accurate. If, as when allowing for non-neutral monetary policy and a fixed crisis cost, the cost-benefit approach suggests that an increase in net marginal benefit can be obtained by a temporary deviation from the optimal policy rule, it can only really indicate the di-

[5]Implicit in this reasoning is that there *is* a consistent policy rule in place from which can deviated. This is certainly the case for the estimates from the Riksbank's DSGE model Svensson uses. For the estimates of Schularick and Taylor (2012), which go back to 1870, this could be doubted, were it not that several studies using more recent samples obtain similar results (Gerdrup et al., 2016; Flodén, 2014).

[6]Filardo and Rungcharoenkitkul (2016) do study a case with endogenous downturn costs and still find support for leaning, but, as discussed, there is no reason why their approach is superior to the cost-benefit one. Moreover, their alternative loss function for endogenous downturn costs seems to be incorrectly specified (Filardo and Rungcharoenkitkul, 2016, p.33).

rection of the change in policy, that is, raising or lowering the interest rate. In simplistic terms: when, while evaluating a derivative at a given point, one finds that it is not equal to 0, one cannot conclude how far the function's extremum lies from the point, but only in which direction it lies. In terms of the LAW debate: when one finds a net marginal benefit in increasing the policy rate, the difference between evaluating a one-off rate increase and a systematic optimization throughout the cycle becomes important again. Only the latter approach can in that case find a truly optimal policy. Second, it should be noted that Svensson's finding that a large crisis loss increase results from LAW when downturn costs are endogenous is for the most part an artefact of the quadratic loss function. To see this, consider one of Svensson's illustrative examples: if a LAW policy keeps the unemployment gap at 0.5% in non-crisis times, and a crisis increases that gap by 5%, then the crisis loss under LAW is $5.5^2 = 30.25$ while the crisis loss without LAW is $5^2 = 25$. The crisis loss increase due to LAW is thus equal to $30.25 - 25 = 5.25$, a substantial amount (Svensson, 2017*a*, p.2). Yet it could be questioned whether a central bank that deliberately keeps the unemployment gap above zero in normal times attaches the same weight to the deviation it thereby induces as a central bank that aims to keep the gap closed at all times. An alternative loss function could for example be

$$\mathcal{L}_t = \begin{cases} |\tilde{u_t}|, & \text{if } \tilde{u_t} < u_t^{nc}. \\ \tilde{u_t}^2, & \text{otherwise,} \end{cases} \tag{2.1}$$

where $\tilde{u_t}$ is the unemployment gap and u_t^{nc} is the desired level of the non-crisis unemployment gap under LAW, 0.5 in the example. This specification of the central bank's loss function is equivalent to the assumption that crises have a fixed cost, which Svensson criticizes. Yet when specified as above, this assumption might not seem all that unreasonable. While it may seem intuitive that weaker economies are hit harder by crises, one may ask whether this intuition also holds for economies whose performance is deliberately weakened, but is not fundamentally weak. Of course, a counterargument might posit that there is no way to distinguish between these two cases. The fact remains that even in the cost-benefit approach, there are specifications which are not necessarily as unrealistic or inconsequential as Svensson says they are, under which LAW can yield net marginal benefits. For example, the paper by Gourio et al. (2018) deploys a utility-based object function instead of a quadratic loss function and finds, in the context of a New Keynesian DSGE model with financial crises, that systematically responding to credit outperforms both a Taylor-type and an output-gap policy rule. The argument for deploying a utility-based objective function is that quadratic loss functions that minimize unemployment and inflation deviations fail to capture the permanent negative effects a financial crisis has on productivity (Gourio et al., 2018, f.n.23).[7] This constitutes the third issue with Svensson's results, namely that in his calculations a crisis is just a temporary, not a permanent, gap in output. Importantly, the Gourio et. al. paper suggests that this is not an innocuous assumption, as apart from permanent output losses, they do not rely on any of the other assumptions of "pro-leaning" papers. That is, they do not rely on monetary non-neutrality, nor on exogenous downturn costs. Their findings therefore constitute a serious re-consideration of a systematic leaning policy, and deserve further scrutiny. A fourth and last criticism of Svensson's results concerns his use of debt or debt growth as the leading indicator of a crisis. For one, the empirical estimates on which Svensson relies, for example those of (Jordà et al., 2016, Table 1), only include lending from depository institutions, which misses the large share of loans provided by the shadow banking sector. Such an omission is probably quite important, as for example in the U.S. the shadow banking sector was larger than the commercial banking sector Adrian and Shin (2009). An alternative empirical approach that does use a broader measure of credit is taken in Juselius et al. (2017). That paper also

[7]Gourio et al. (2018) also carry out a sensitivity analysis of their results by letting the effect of financial crises on GDP vary between 6 and 14%, which is well in the range of most empirical studies (Jordà et al., 2013; Reinhart and Rogoff, 2009, pp.226-230) Their result continues to obtain, with the optimal degree of leaning increasing monotonically with the magnitude of the effect.

suggests that it is not debt growth per se, but cumulative debt growth above certain thresholds accompanied by other developments such as excessive asset price growth, similar to Borio and Lowe (2002).[8] This alternative empirical approach evaluates the boom-bust dynamics of financial cycles with a Vector Error-Correction model for the United States (1985-2015) that consists of two cointegrating relationships representing roughly a leverage gap and a debt service gap (Juselius et al., 2017, p.5). In this way, the authors show that both excessive leverage and excessive debt burdens depress output growth, and that monetary policy is non-neutral with respect to these variables. By way of a policy counterfactual, which is however not immune to the Lucas critique, they provide suggestive evidence that a systematic leaning policy, had it started in 2003, could have raised output by more than 1% per year relative to the historical trajectory of output (Juselius et al., 2017, p.24). While the policy would have led to output losses during non-crises times, it also would have allowed for a much faster post-crisis recovery, because debt burdens would have been reduced much quicker after the crash (Juselius et al., 2017, p.25). One intriguing finding of the paper is that monetary policy has indirectly been reacting to debt burdens over the sample period (Juselius et al., 2017, p.26). This could strengthen the LAW case if it is better for the central bank to react systematically and transparently to a variable than to do it indirectly and without acknowledgement. In section 5.2.2, I extend the authors' model with a measure of inequality to consider what effect systematic leaning would have thereon. In all, four criticisms remain of the cost-benefit approach to LAW: that it cannot derive an exact magnitude for the optimal policy rate change if such a change turns out to yield benefits; that its findings are partially an artefact of the quadratic loss functions; that it fails to capture a financial crisis's permanent negative effects on output; and that

[8] Juselius et al. (2017) continue to assert that approaching the LAW question from a temporary deviation approach is misleading, and that a systemic leaning approach is better-suited. As I have explained above, such an assertion is only true when a temporary deviation can be shown to produce net marginal benefits. The authors' claim that the cost-benefit approach promotes a discretionary LAW policy misses the point.

its reliance on debt growth as an indicator can be questioned. Two recent papers take alternative approaches that show the importance of these faults, while also avoiding some of Svensson's criticisms of systematic LAW approaches. While these papers do not have the credibility of the cost-benefit approach, their results do suggest the LAW debate has not been fully settled just yet.

2.2 Inequality, Leverage and Monetary Policy

So far, I have discussed the linkages between monetary policy, leverage and financial crises that make up the core of the debate on whether monetary policy should lean against the wind. The novelty of the current paper, however, is to study empirically an endogenous channel through which monetary policy and financial stability are linked, that runs through inequality and leverage. In this section I therefore discuss the links between monetary policy and inequality, inequality and leverage and leverage and financial crises, a loop that ultimately reflects back on the leaning against the wind debate.

2.2.1 Inequality and Monetary Policy

The distributional impact of monetary policy has become a lively research topic since the financial crisis. One of the main insights from the theoretical approaches to this topic has been that monetary policy may have heterogeneous effects on households along the wealth and income distribution, and that this may lead these households to act differently in response to monetary policy (Kaplan et al., 2018). On the empirical side, early studies into the distributional effects of monetary policy tend to find that a contractionary monetary policy shock increases inequality, while an expansionary shock decreases it (Coibion et al., 2017; Bivens, 2015). This finding seems to hold true also for unconventional monetary policy, although the prevailing macroeconomic circumstances at the time of the policy's implementation seem to play an important role in determining the sign of the effect. For example, while in the U.S. the ultra-loose post-crisis monetary policy probably led to an increase in inequality due to excessive stock market gains for the rich, in the E.U. similar policies ended up decreasing inequality, as a comparable stock market rally did not materialize (Lenza and Slacalek, 2018; Van Dijcke and Horion, 2018). This notwithstanding, several studies find that monetary policy affects inequality both on a discretionary and a systematic level (Coibion et al., 2017; Gornemann et al., 2016). Furceri et al. (2018) find that exogenous monetary policy shocks affect inequality asymmetrically, in that a contractionary shock increases inequality more than the reverse shock decreases it; at the same time, the same authors find that endogenous policy changes do not affect inequality much. Overall, the literature finds both theoretical and empirical grounds for the idea that monetary policy has distributional effects, with broadly compatible findings across different studies.

In this paper, I consider four theoretical channels through which monetary policy affects income inequality. Firstly, an employment channel, which makes that decreases in employment as a result of contractionary monetary policy disproportionately hurt poorer households, which are more vulnerable to cyclical fluctuations, as labour earnings make up a larger share of their total income (Carpenter and Rodgers III, 2004; Bitler and Hoynes, 2015). Secondly, a refinancing channel, through which the decreased possibility for mortgage refinancing due to higher rates and possibly lower house prices hurts households at the bottom of the distribution the most, as houses are their main and often only asset. Thirdly, a debt service channel, through which higher interest rates redistribute from debtors to creditors. This channel is reinforced by the decrease in inflation that follows a contractionary monetary shock, as such a decrease increases the real value of debt. Lastly, an asset valuation channel, by which decreases in asset prices hurt rich households most, as realized capital gains make up a higher share of these households' income. Empirically, it seems that only capital gains from equity co-vary with the income distribution, while those from bond prices are spread more evenly across

(Adam and Tzamourani, 2016, §4.3). Clearly, these channels have differential effects on inequality. Moreover, some of them are "direct" partial equilibrium effects of the interest rate on households, conditional on households' income; others arise indirectly, through the macroeconomic fluctuations caused by households reacting differentially to the policy shock across the income distribution (Ampudia et al., 2018). The balance between the contributions of the various channels can tip the sign of the inequality effect, which explains some of the seemingly contradictory findings in the empirical literature.

2.2.2 Inequality, Leverage and Crises

There are several theoretical explanations for the link between inequality and leverage.[9] First, the link might arise from time-varying idiosyncratic income shocks (Iacoviello, 2008). If such shocks become more volatile, but agents are only temporarily subject to them, that is, agents' permanent income remains unchanged, then they will smooth out these shocks by borrowing more. Empirically, such an explanation is supported by research that shows that households use credit to smooth consumption when subject to income shocks (Krueger and Perri, 2009). The fact that consumption inequality has not kept up with income inequality also suggests that inequality leads to increased borrowing (Krueger and Perri, 2006). Nonetheless, the explanation implies that the rise in household debt is mostly a result of increased within-group inequality, as between-group inequality would be not be transitory. This feature puts such an explanation at odds with the prevailing wisdom about inequality, which sees it as a juxtaposition of (at least) two groups: the top 1, 5 or 10%; and the rest. Moreover, empirical studies show that social mobility declined in recent decades, even as income inequality went up (Andrews and Leigh, 2009). A second theory performs better at explaining why households would not adjust their consumption even if their permanent income changes. If the welfare loss induced by the permanent shock is too large, either in absolute terms or relative

[9] This section is based on the discussion in Bazillier and Hericourt (2017).

to the household's previous social standing, then households might increase their borrowing to maintain their previous living standards.[10] Variations of the theory refer to habit-formation, social pressure to 'keep up with the Joneses', or even an "expenditure cascade", where each social class aligns its consumption standards with that of the one just above, making that changes in top earnings can affect expenditure across the whole distribution (Frank et al., 2014; Veblen and Chase, 1912; Duesenberry et al., 1949). The general idea is consistent with empirical findings of a permanent shift of income to the top percentiles in recent decades (Alvaredo et al., 2013), as well as with findings that consumption inequality did not track the increase in income inequality. Third and last, inequality might also affect the credit supply. On this view, which looks to the other end of the income distribution, increased inequality might lead to excess savings for the rich, which in turn leads to excess demand for debt securities, thereby driving up the credit supply (Lysandrou, 2011; Coibion et al., 2014). Additionally, institutional factors might have played a role in increasing the credit supply. The theories on this front go from those positing that politicians can pressure central banks to accommodate increasing inequality, thereby increasing credit growth and financial instability (Rajan, 2011); to those that see increased inequality and credit growth as being independent products of financial liberalization (Atkinson and Morelli, 2011). The former explanation in a sense reverses the mechanisms, as it speculates that increasing inequality may lead to looser monetary policy, in contrast to evidence from the literature which says that expansionary monetary policy decreases inequality. The latter explanation is contradicted by empirical studies that find ambiguous effects of finance on inequality, and finds that the nature of the relation between both depends on the economic development and the institutional quality of the country under consideration (Beck et al., 2007; Claessens and Perotti, 2007). Part of the ambiguity might come from the distinction between finan-

[10] One of the early papers to posit the link between inequality, leverage, and crises, relied on a theory of this type, modelling the top 5 and bottom 95% as two distinct groups with different preferences (Kumhof et al., 2015).

cial development and financialization of the economy, where the former would decrease inequality, while the latter would increase it (Lin and Tomaskovic-Devey, 2013).

When it comes to empirical evidence for the link between inequality, leverage and crises, the signs are mixed, although most studies find a positive association between the two.[11] Christen and Morgan (2005) find that increased income inequality has led to increased consumer borrowing in the United States. This finding is corroborated for a panel of advanced economies by Malinen (2016), who finds evidence of a long-term equilibrium relationship between inequality and credit, where the former Granger-causes the latter, but not the other way around. Coibion et al. (2014) also find that increased inequality leads to more household debt, but favor a more supply-side oriented argument. Particularly, they show that poor households in low-inequality regions borrow less than those in high-inequality regions and argue that this indicates that inequality is used as a signal of creditworthiness by suppliers. Bordo and Meissner (2012), considering the full causal chain from inequality to crises, confirm the evidence that credit booms increase the probability of a banking crisis, but find no link between inequality and credit booms. Their approach has been criticized by several papers, however, for ignoring potential endogeneity concerns. These papers, using more robust methods to account for such concerns, find cross-country evidence for a significant link between inequality and leverage (Klein, 2015; Perugini et al., 2015). A recent study also documents the strong link between long-run inequality, weak productivity and the build-up of macroeconomic imbalances Paul (2018). This supports the argument that it is the long-run trends and levels of inequality and credit that are related, rather than their growth levels (Malinen, 2016). Taken as a whole, there is both theoretical and empirical support for the link between inequality, leverage and crises, although the debate about this link is still ongoing.

In conclusion, the various strands of the literature that cover parts of the causal loop

[11] Having briefly discussed the strong evidence from the literature on the link between credit and crises in section 2.1.2 above, I do not consider it further here.

between monetary policy, inequality, leverage and crises postulate a slew of theoretical channels through which these variables interact, besides providing preliminary empirical evidence confirming the existence of such channels.

Chapter 3

Data

In this chapter, I describe the data I use for the several variations of the panel vector autoregressive model (PVAR) that I estimate below. As explained in Section 2.1.1, inflation targeting only took off as an official monetary policy framework in the 1990's, although some central banks have been implicit inflation targeters since much earlier (Clarida et al., 1998). In what follows, I therefore use a longitudinal sample with annual data running from 1995 to 2016 that consists of all advanced economies whose monetary policy can approximately be described as inflation targeting.[1] The motivation for having the sample start in 1995 is that this is two years after the collapse of the European Monetary System (EMS). As the events leading to the break-up caused large divergences in interest rates, European central banks in that period could scarcely be described as inflation targeters. Clarida et al. (1998), however, show that the Bundesbank had been an implicit inflation targeter since 1979, and other major European central banks were following similar policies in the years prior to German reunification in 1990, which it is argued ultimately led to the EMS's collapse (Eichengreen et al., 1993). Moreover, interest rate setting in the countries of the later Euro Area was coordinated in the run-up to the

[1] These countries are, in alphabetical order; Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Great Britain, Greece, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland and the United States.

currency union (Gorter et al., 2008). Also, by 1995, most non-European countries in the panel had either already converted to inflation targeting, or were preparing to do so.

3.1 Macroeconomic Variables

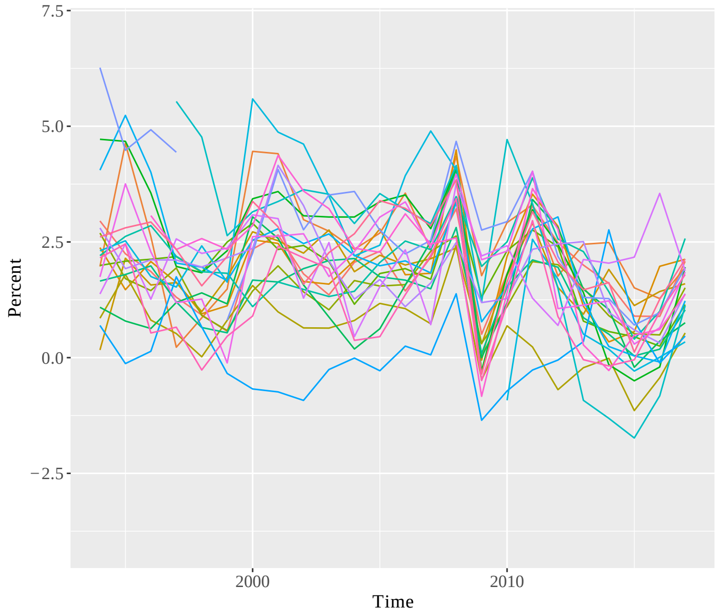
The PVAR models include several common macroeconomic indicators: gross domestic product (GDP) (OECD, 2019*a*), consumer price index (CPI) inflation,[2] the harmonized unemployment rate (OECD, 2019*b*) and share prices (OECD, 2019*c*). Gross domestic product is in constant chain-linked local currency; using a harmonized unemployment rate ensures international comparability; the share prices variable reflects the evolution of the leading stock market index for each country.[3] All series for these variables were obtained from the statistics website of the Organization for Economic Development (OECD), a club of advanced economies. To illustrate the co-movement of these macroeconomic variables across the countries included in the panel and to underline the stable inflation that has resulted from the approximate inflation-targeting policies in place in these countries, I plot their CPI inflation rates together in Figure 3.1.

The graphs shows that most of the countries in the sample had relatively stable inflation rates over the last two decades, with a range of 2-2.5%, as would be expected from a sample of inflation-targeting, advanced economies. The line that creeps at the bottom of the pack is Japan, which has been combating deflationary pressures and a ZLB for two decades. After the crisis, another line distinctively joins Japan in the deflationary regions, which is Switzerland. While most countries suffered deflationary pressures after the crisis due to the general economic slow-down, Switzerland suffered stronger such pressures because of money fleeing the Eurozone, leading to an appreciation in its currency and cheaper imports as a result. Nonetheless, Switzerland's economy has

[2]The geographic coverage of the CPI is the whole country for all economies included in the sample, except for Korea, Australia and the United States, where the index only covers urban areas.

[3]Table A.1 in the Appendix provides descriptive statistics for the main variables and the countries for which they are available.

Figure 3.1: CPI Inflation Rates in All 21 Countries in the Panel.



[1] Source: Author's Calculations, OECD.

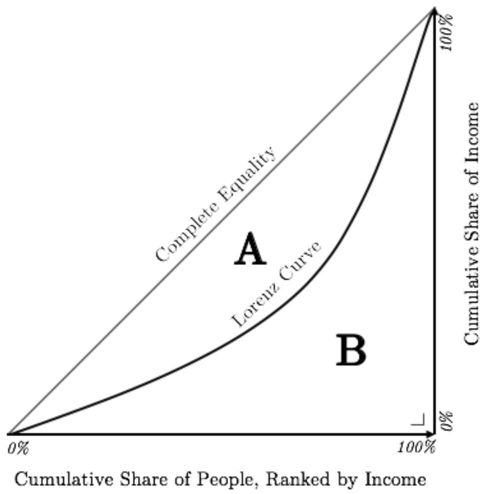
performed relatively well despite these deflationary pressures, making it a case example for those disputing the customary association made between deflation and economic performance (Borio et al., 2015).

3.2 Inequality

To capture the evolution of inequality, I include country-specific Gini coefficients in the sample. These are obtained from the Standardized World Income Inequality Database (SWIID) (Solt, 2019). As a brief refresher, the Gini coefficient is an inequality metric first proposed by Gini (1912) that measures the dispersion of the income distribution. It is often presented by way of the Lorenz Curve, as in Figure 3.2.

In the figure, the 45 degree line represents complete equality: the cumulative share of total income maps one-to-one into the cumulative share of the population. The Lorenz curve reflects the actual distribution, where a more convex curve signifies greater

Figure 3.2: Graphical Representation of Gini Coefficient



[1] Source: Author's Calculations.

inequality, as higher income percentiles possess larger shares of total income. The Gini coefficient, by this representation, is the size of the area between the Lorenz curve and complete equality (A), divided by the total area (A+B). It thus reflects the percentage deviation from complete equality. A mathematically equivalent definition expresses the Gini as half of the relative mean difference of incomes, which is (Sen et al., 1997, pp.30-31)

$$\frac{\Sigma_{i=1}^{n}\Sigma_{j=1}^{n}|y_i - y_j|}{2n^2\mu}, \tag{3.1}$$

where yi is the income of person i and μ is the average income. This formula illuminates why some researchers prefer to use a different inequality measure than the Gini when inequality is suspected to occur mostly at the extremities of the income distribution. Since in the formula the Gini is expressed relative to the mean of the distribution, it is clear that the Gini puts a larger weight on the middle of the distribution than measures such as the 90/10 ratio.[4] Conversely, a desirable feature of the Gini that the 90/10 ratio

[4]Note that this is not the same as the common claim that the Gini puts more weight on transactions between households in the middle of the distribution than between households at the extremities, which is not the case (Gastwirth, 2017).

lacks is that it can capture changes in inequality occurring at any point in the income distribution. Given that there are other issues with the Gini coefficient, such as the fact that different distributions can produce the same Gini, it should then not be surprising that there is a wide range of variously defined Gini coefficients.

This very possibility for dissimilarity between Gini coefficients is one of the main reasons for why the SWIID is a top-notch source of inequality data. Its hallmark is rigorous comparability between Gini coefficients across time and space. One of the main issues with inequality data, apart from its low frequency, is that compiling it often entails a trade-off between coverage and comparability (Solt, 2016). As the construction of high-grade inequality metrics requires accurate and granular data, most inequality databases use household panel surveys or similar micro-level datasets to calculate distributional statistics. The Luxembourg Income Study (LIS) brings together 300 such datasets, mostly compiled by national statistics offices, across 50 different countries, and harmonizes them with the purpose of fostering cross-national comparability.[5] With comparability thereby guaranteed, the SWIID sets out to expand the LIS's coverage by filling in the gaps in the data with 'LIS-compatible' data points generated on the basis of a model-based multiple imputation method (Solt, 2019, pp.1272-4). As inputs for this multiple imputation, the SWIID tries to use all Gini indices available from other sources for years that the LIS is available as well. These sources include anything from academic studies to census data, and are tested against pre-specified standards before inclusion.[6] The relationship between these Gini indices and the LIS baseline is subsequently estimated, and missing data points are filled in with synthetic observations that are 'LIS-compatible'. To give an idea of the accuracy of this process, (Solt, 2019, p.12) compares previously unavailable LIS data points with earlier SWIID estimates of these data points and finds that only in 7% of the cases a difference larger than two Gini

[5]Solt recounts an anecdote that generating a single data point of LIS data requires on average ten person-months of labour Solt (n.d.).

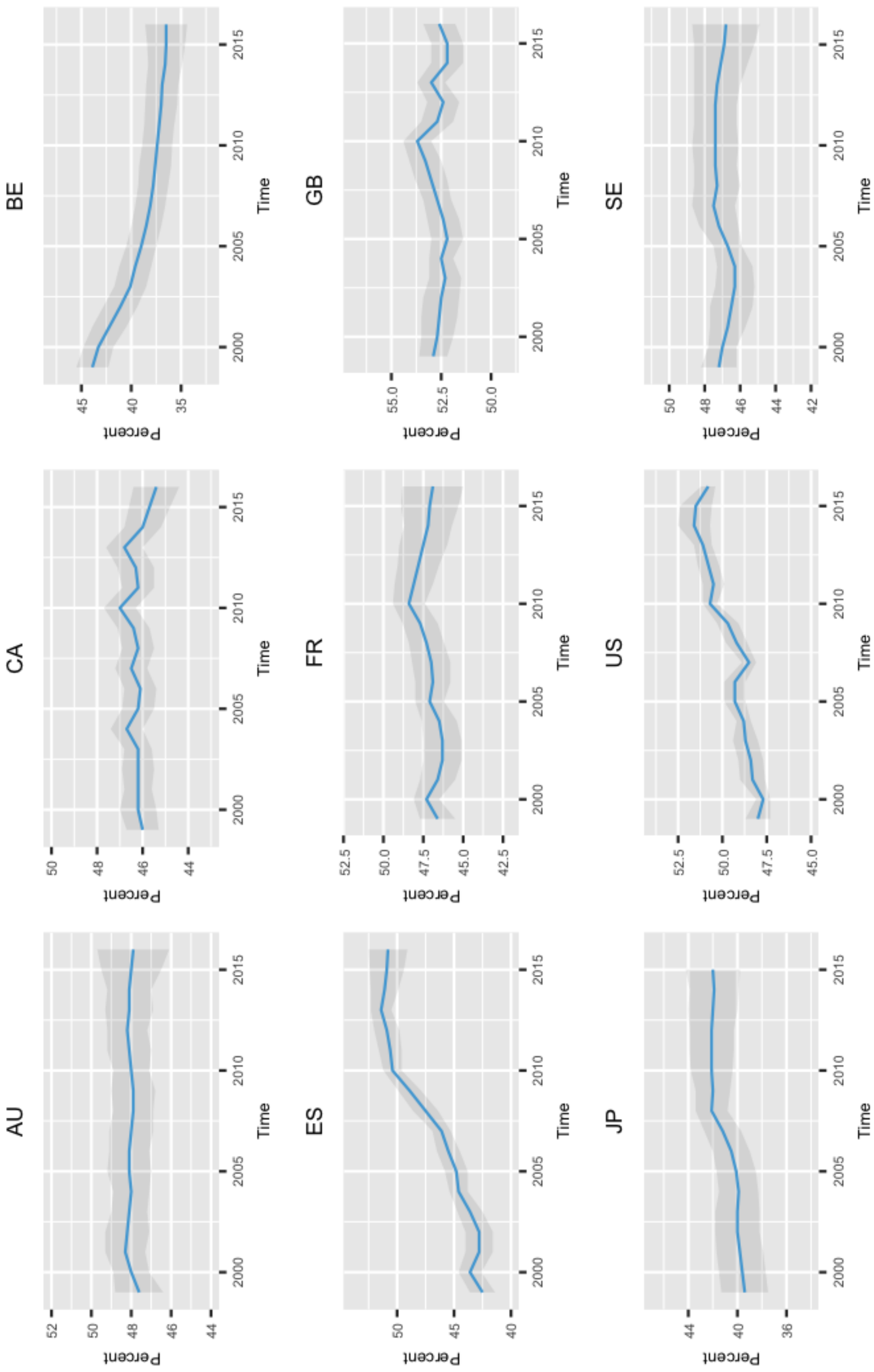
[6]Sources include the OECD Income Distribution Database, Eurostat, the World Bank, etc.

percentage points is statistically significant. The SWIID's goal of providing comparable inequality data with broad coverage thus seems quite successful. The biggest strength of the LIS, however, is also its greatest weakness. While the Gini coefficient, due to its popularity, is the safest bet for constructing a long, internationally comparable dataset, it comes at the cost of a loss of information on the heterogeneity underpinning income inequality. As explained above, a redistribution from the 10th to the 50th quantile could generate the same change in the Gini as one from the 50th to the 90th. Obviously, the welfare implications of both are quite different. One other reliable dataset that tries to address this, the World Inequality Database (WID), provides information on income shares across the distribution. Nonetheless, its coverage is less extensive, both over time and across countries. I therefore only use the WID to test for the robustness of my results when using a different inequality measure. The evolution of the top 10% income shares for a selection of countries can be seen in Figure A.2 in Appendix.

When it comes to its format, the SWIID provides data on pre-tax and post-tax Gini coefficients in a Bayesian format, that is, as 100 draws from the posterior distribution of the generated Ginis, together with their standard errors. To make matters easier, in my main analysis I use the single summary estimates provided alongside the Bayesian format. In Figure 3.2, I plot the pre-tax Gini indices for selected countries from the panel, together with their standard errors.

The plots portray a generally increasing trend in the Gini coefficients across advanced economies, with the notable exception of Belgium, where the Gini coefficient has seen a remarkable decline of almost 10%. The U.S., as expected, has seen a strong rise in inequality over the past decade, as did Spain. Most countries that exhibit a rising trend in the Gini see an acceleration of that trend right before the Great Recession. Note that the evolution of the post-tax Gini coefficients, absent any new re-distributional policies, should display similar but slightly tempered dynamics. Overall, the SWIID's careful construction process makes that it is one of the best databases around when it comes to

Figure 3.3: Evolution of Pre-Tax Gini Coefficient in Selected Countries.



[1] Source: Author's Calculations.
[2] Shaded Region Is One-Standard Deviation Confidence Interval for SWIID Measurement Error.

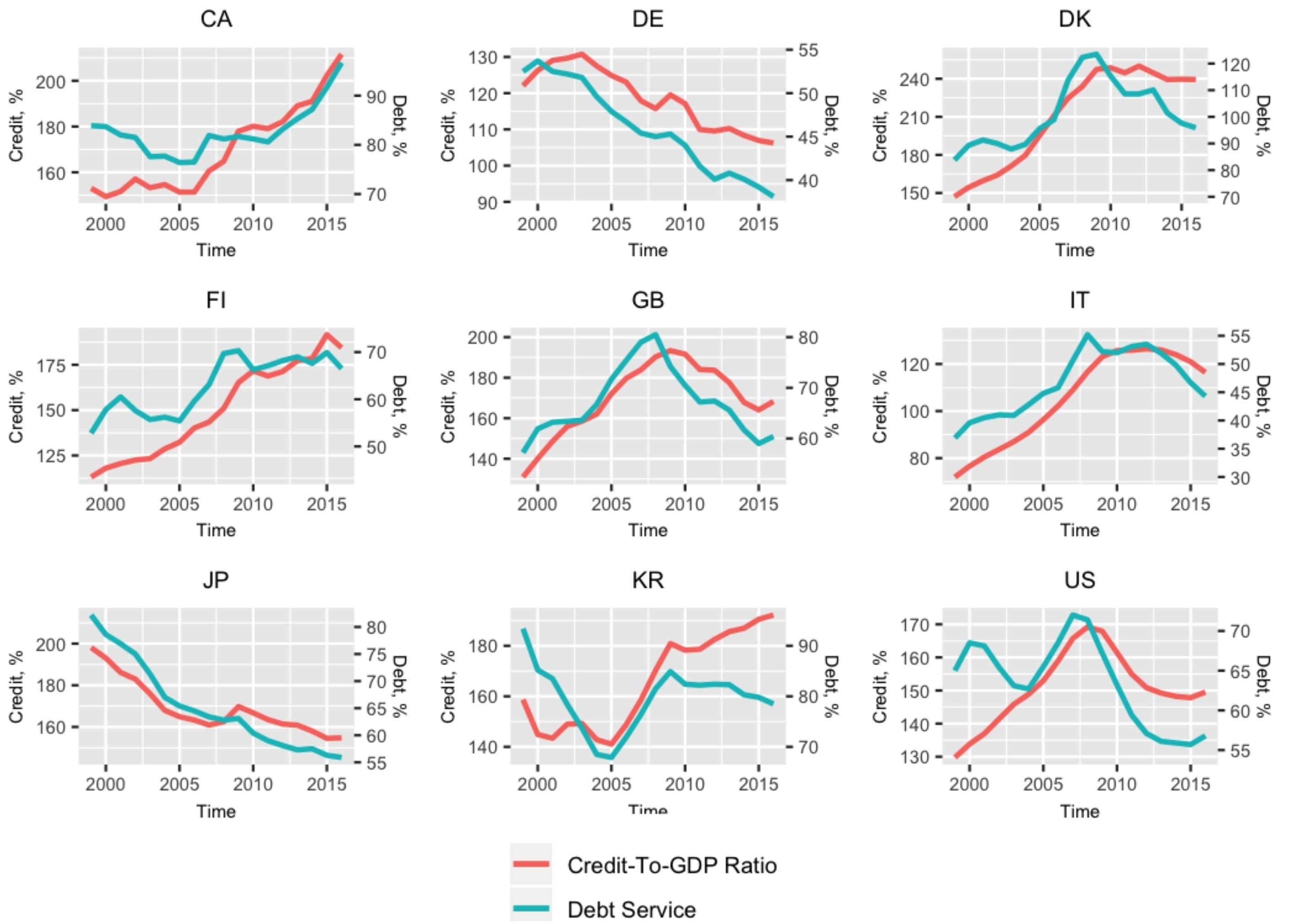
trading off comparability and coverage, as it is virtually unrivalled in both.

3.3 Debt and Credit

To capture the connection of both monetary policy and inequality to leverage, I include two different measures of leverage in the VAR models. First, I include the BIS's measure of total credit to the non-financial private sector (private sector for short) to capture the push-pull effects of monetary policy and inequality on leverage, where a higher interest rate should decrease credit growth, but increase inequality, which increases credit growth.[7] Total credit here truly means total, in that it captures credit from all sources, including shadow banking - a unique feature of the BIS's credit data. Second, I test for the robustness of this measure by alternatively using the credit-to-GDP ratio. This ratio equals total credit to the non-financial private sector, divided by GDP. It does not capture excess leverage to complete satisfaction, as when the economy is booming the rapid GDP growth can make that the credit-to-GDP ratio drops, even as financial imbalances continue to build up. A better measure would be the leverage gap from Juselius et al. (2017), which divides total private credit by total private assets. For that, however, one needs to dig into the national accounts, which unfortunately are incomplete for several countries in my sample, and incompatible for others. To assay this concern, in the extended VAR in section 5.2 I include a real residential property price index (RPPI) for each country, as constructed by the BIS, so as to proxy dynamics in non-financial assets, in addition to those in financial assets captured by the share price indices described above. Third, also in the extended VAR, to capture the cascade from monetary policy through interest rates to debt service and ultimately inequality, I include a new measure recently proposed by the BIS (Drehmann and Juselius, 2012), the debt service ratio (DSR). This ratio is constructed with the aim of tracking the financial

[7] The non-financial private sector, by the 2008 System of National Accounts, includes two sub-sectors: 1) households and non-profits serving households and 2) non-financial corporations.

Figure 3.4: Co-Movement Between Credit-to-GDP Ratio and Debt Service Ratio.



[1] Credit=Left Axis, Red Line, Debt=Right Axis, Blue Line. Coverage: Non-Financial Private Sector.
[1] Source: Author's Calculations, BIS Data.

constraints that excessive indebtedness imposes on the private sector. Not only is this measure an accurate leading indicator for financial crises, it also aligns well with the debt service channel from monetary policy to inequality, as described in section 2.2.1. The DSR ratio is defined as

$$DSR_t = \frac{i_t D_t}{(1 - (1 + i_t)^{s_t})Y_t}, \tag{3.2}$$

where D_t is the total private credit stock, i_t is the average annual lending rate on that stock, s_t is the average remaining annual maturity on the stock and Y_t is annual aggregate income (Drehmann and Juselius, 2012, p.23). The average lending rate i_t has the desirable property that it embeds the stock of all outstanding contracts, and in that way reflects both current and past lending conditions (Juselius et al., 2017, p.62). To give a sense of the cyclical behaviour of these measures, I plot the credit-to-GDP ratio against the DSR in Figure 3.3 for a selection of countries from the panel. The co-movement between the two measures is striking, though it should be noted that I graphed the two measures on different scales.

3.4 Monetary Policy: Shadow Rates

Since at least Christiano et al. (1999), it is common practice in the macroeconomic vector auto-regression (VAR) literature to use the short-term benchmark policy rate as a measure of monetary policy. When the policy rate is constrained by the zero lower bound (ZLB), however, and central bankers recur to unconventional instruments such as Quantitative Easing and Forward Guidance to transmit monetary policy, this rate ceases to be a good measure of monetary policy. As the years where the policy rate hit the ZLB in several advanced countries in the panel are of special interest to study the dynamics between monetary policy, inequality and financial stability, an alternative monetary policy measure would be of use. The literature proposes several solutions to this issue by constructing a so-called shadow short rate (SSR), which represents the policy rate that would have been equivalent in its effect to the unconventional monetary policies actually implemented. Note that if the estimated shadow rate turns out to be negative, this implies the effectiveness of unconventional monetary policy, which is well-documented (see f.e. Krishnamurthy and Vissing-Jorgensen (2011)).

The original idea of a shadow rate goes back to at least Black (1995). That paper proposed the idea that the nominal short rate r_t is merely an option, in the sense that people always have the choice between either holding currency and earning an interest of 0 or investing and earning the "shadow real interest rate", s_t, which reflects "investment opportunities" (Black, 1995, p.1371). Thus, $r_t = min\{s_t + \pi_t, 0\}$, with π_t inflation. In other words, the shadow rate can go into negative territory while the nominal short rate, constrained by the ZLB, cannot.

Several recent contributions have proposed methods to construct such a shadow rate. An older workhorse model that was concerned with describing the yield on assets of varying maturities was the Gaussian affine term structure model (GATSM) (see Diebold and Rudebusch (2013) for a survey). Wu and Xia (2016), however, argue that this method, as well as Black's proposed Shadow Rate Term-Structure Model (SRTSM), face problems in the ZLB environment. They therefore propose a multi-factor SRTSM to address these issues, which they show outperforms the GATSM in tracking interest rates in a conventional environment, while exhibiting dynamic correlations, similar to those of the various interest rates, with macroeconomic variables (Wu and Xia, 2016, p.254). Commenting on this work, Krippner (2015*a*) shows that the shadow rates calculated from the three-factor SRTSM Wu and Xia use is not robust to the choice of lower bound parameter and sample period. Krippner, in consequence, proposes using a two-factor SRTSM instead. While this reduces the shadow rate's fit with respect to the yield curve data, it also improves on the rate's robustness, which Krippner argues is a worthwhile trade-off. In all, using a shadow policy rate allows one to capture the stance of monetary policy even when the nominal policy rate hits the ZLB. Specifically, the shadow rate reflects the expansionary effect of any additional unconventional policy measures such as Quantitative Easing or Forward Guidance on long-term interest rates, over and above the expansionary effect of a close-to-zero policy rate (Krippner, 2016, p.3).

I now briefly discuss the specific model I use to calculate the SSRs for those countries that hit the ZLB but were not included in the dataset published by the Bank of New Zealand. The model is an approximation of a continuous-time GATSM based on an arbitrage-free Nelson and Siegel model (ANSM) with two state variables, $L(t)$ and $S(t)$ (Nelson and Siegel, 1987; Krippner, 2016, p.5). As shown by Krippner in several papers 2010; 2015*b*, ANSMs provide a parsimonious approximation of any GATSM, however specified. As explained above, the choice for two factors guarantees empirical robustness. The resulting model is a Krippner-ANSM(2) or K-ANSM(2) model. It estimates the shadow rate as

$$r(t) = L(t) + S(t), \tag{3.3}$$

where $L(t)$ is a state variable representing the level of the shadow yield curve, and $S(t)$ a state variable representing the slope of the same. These state variables, under a physical measure (also known as probability measure) as used in financial securities pricing, evolve as a correlated vector Ornstein-Uhlenbeck process (Krippner, 2016, p.5). With these state variables, the model can also produce estimates of alternative rates measures, but I do not use these. To obtain the model parameters and the SSR estimates, I modified the algorithm provided by Krippner on the website of the Bank of New Zealand. This algorithm uses an iterated extended Kalman filter to approximate the laws of motion of the state variables, which allows for non-linearity of the model's interest rates with respect to these state variables (Krippner, 2016, p.7). The estimates of the state variables are based on daily yield curve data I downloaded from Bloomberg.[8] Specifically, I

[8]I am grateful to Vlerick Business School for providing me access to their Bloomberg Terminal.

downloaded the complete available daily yield curve series for zero coupon sovereign rates and forward overnight index swap (OIS) rates, and this for a range of maturities.[9] I then used a program provided by Krippner to splice these two rates together in order to obtain long time series of OIS rates in the lower bound environments in the countries involved. The splicing is necessary because the full OIS curve is often only available for about 10 years back in time. Based on this data, the model's parameters are obtained by way of an unconstrained optimization carried out with a Nelder-Mead simplex search method. In that way, daily estimates of the SSRs and their standard errors are obtained for each of the 6 countries in the dataset whose interest rate hit the ZLB in the period between 1999 and 2016, and for which SSR estimates are not publicly available.[10]

Subsequently, in the empirical analysis in the next section, I construct a policy rate variable i_t^p which is equal to the actual benchmark policy rate when the latter is above zero and equal to Krippner's shadow rate when the actual policy rate is below 0.5%. The benchmark policy rates are obtained from the Statistics Warehouse of the BIS, which brings together data on these rates from central banks' websites. With regards to the shadow rate: the estimates for Japan, the U.K., the U.S. and Europe are obtained from the Bank of New Zealand's website (Krippner, 2019). For other countries that experienced ZLB episodes, I use Krippner's algorithm to calculate the shadow rate, as explained.[11]

Despite the fact that SSRs allow a researcher to capture monetary policy measures at the ZLB in one single, familiar metric, as noted above, there has been some debate about how serviceable a shadow rate is as a measure of monetary policy (Christensen and Rudebusch, 2014). A notable recent contribution to this debate includes Garcıa and Skaperdas (2018). That paper infers a shadow rate from real activity using VARs, thereby relaxing the assumption implicit in SRTSMs of a one-to-one mapping from fi-

[9] 0.25, 0.5, 2, 3, 4, 5, 7, 10 and 30 years.

[10] For further technical details, see (Krippner, 2015*b*).

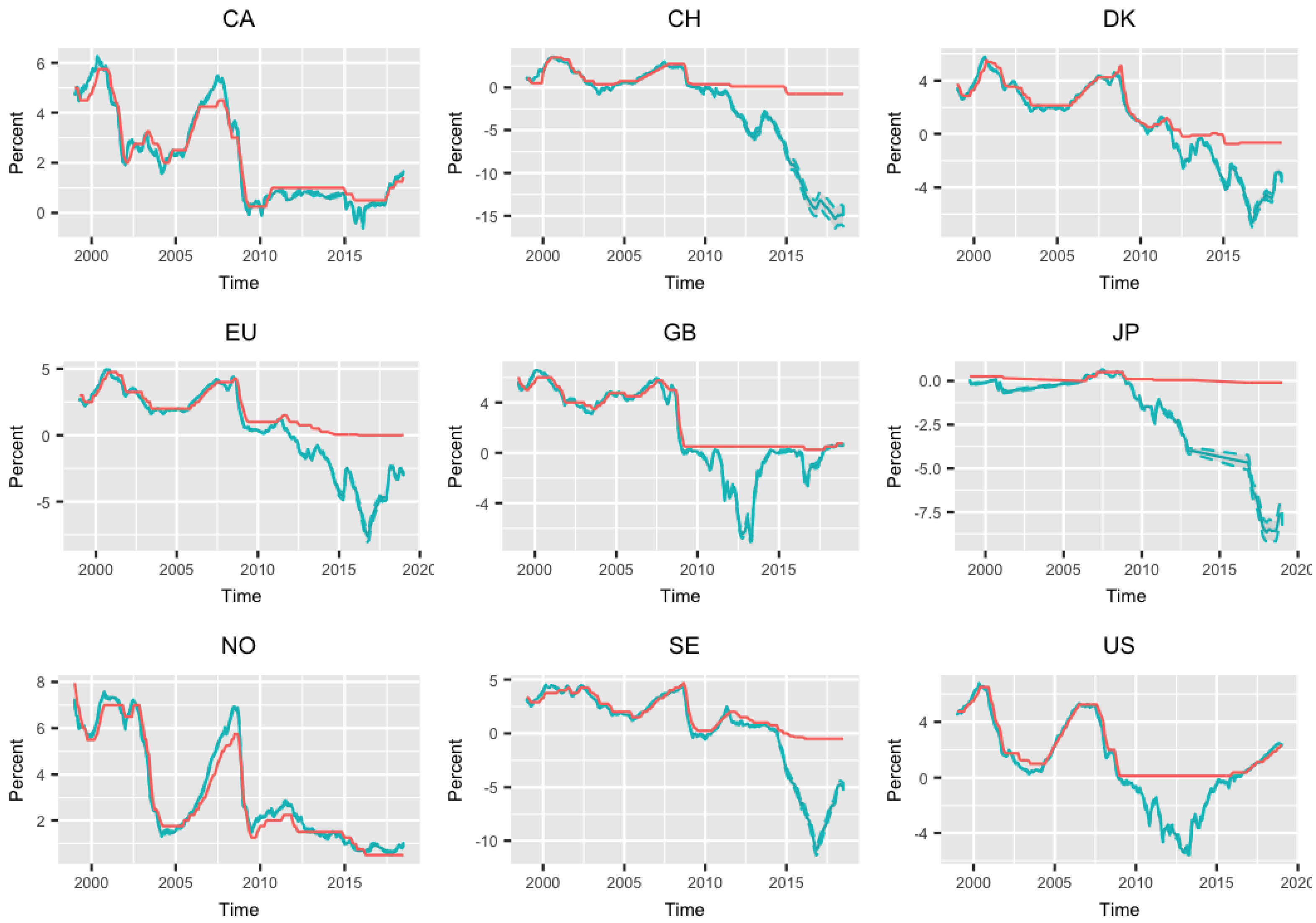
[11] These countries are Australia, Canada, Denmark, Norway, Sweden and Switzerland.

nancial markets to real effects following conventional policy rate changes (Garcıa and Skaperdas, 2018, p.3). The shadow rate obtained by the paper agrees closely with the stance of policy instruments, as measured by Lombardi and Zhu (2014). Moreover, the paper's shadow rate estimates, deduced from real activity, are close to Krippner's estimates, vindicating my use thereof (Garcıa and Skaperdas, 2018, p.36). In addition, Krippner (pp.3-4, 2016) notes several favourable characteristics of SSRs; first, estimates of SSRs from K-ANSM(2)s with different specifications have similar profiles; second, negative SSRs are strongly correlated with unconventional monetary policy events; third, Krippner's SSR series have similar levels as those generated by a Taylor rule. In conclusion, the empirical and theoretical frameworks underpinning the use of a shadow short rate to capture the monetary policy stance of countries at the ZLB are robust and well-motivated. Figure 3.4 plots the evolution of the estimated SSRs for the nine countries that hit the ZLB against that of the policy rate of these countries.

The plots lend additional support to the use of the SSR as a measure of monetary policy, as it tracks the policy rate remarkably well during normal times. Moreover, the estimates are precise in the sense that the confidence intervals are small compared to the range the SSRs span. It might seem surprising that the SSR for Switzerland takes a deeper plunge than any of the other SSRs, but it becomes less so when one considers that under its post-crisis QE policy, the Swiss National Bank increased its balance sheet nearly eightfold, as compared to fivefold for the ECB.[12]. It also undertook foreign exchange interventions by pegging the Swiss currency against the Euro, and later enjoyed sizeable spill-over effects from the ECB's own large-scale QE program (Falagiarda et al., 2015). In contrast to the large fall in the Swiss shadow rate, Norway does not see its SSR dip into negative territory at all. This again lines up well with the historical events, as Norway has not undertaken its own QE programme. During the crisis, Norway had enough leeway with its standard policy instruments to combat the downward economic

[12]To underscore this point: during a certain period in its bond-buying programme, Switzerland was buying half of the entire Eurozone's sovereign bond issuance (Reuters, 2012).

Figure 3.5: Policy Rates (Red) and Estimated Krippner Shadow Rates With One-Standard-Error Confidence Intervals (Blue) for Economies at the ZLB



[1] Source: Author's Calculations, Bank of New Zealand, BIS.
[2] The shadow rate estimates for the U.S., Japan, the U.K. and Europe were provided by Krippner on the website of the Bank of New Zealand; the others I calculated using his algorithm.

pressures, which hit Norway while its economy was in an upswing (Olsen, 2018). Finally, Canada is included in the model because it flirted with the zero lower bound, but the estimated SSR barely turns negative throughout the sample and never drops below 0.5%, so I never replace the policy rate with the SSR. The fact that the estimated SSR does not drop much below zero is completely in line with the fact that the Canadian central bank scarcely expanded its balance sheet in response to the Great Recession.

Chapter 4

Empirical Methodology

Good inequality data, as explained above, is a scarce commodity. Although several serious efforts have been undertaken in recent years to provide high-grade inequality data, almost all are of annual frequency. This often severely constrains sample size, especially when studying relatively recent macroeconomics policies such as inflation targeting. Therefore, I exploit the increasing cross-sectional availability of inequality data in a pooled panel vector auto-regression setting. To identify the effect of a contractionary monetary policy shock, I rely on a mix of sign and inequality restrictions. This allows me to examine the interplay between inequality, leverage and monetary policy in the wake of a discretionary monetary policy shock. In the first section of this chapter, I delineate the specification of the model used. In the second section of this chapter, I lay out the empirical strategy used for a policy experiment carried out with the estimated VAR model, in the spirit of Juselius et al. (2017). The aim of the policy experiment is to see if and how inequality comes into play when monetary policy systematically leans against the wind throughout the financial cycle. With the empirical methodology thereby set up, the next chapter proceeds with an exposition of the main empirical results.

4.1 Bayesian Panel Vector Auto-Regression

The model used to identify the effect of an exogenous monetary policy shock is a simple pooled panel version of the standard VAR model. It can be written as (Dieppe et al., 2018, p.135):

$$\begin{pmatrix} y_{1,t} \\ y_{2,t} \\ \vdots \\ y_{N,t} \end{pmatrix} = \begin{pmatrix} A^1 & 0 & \cdots & 0 \\ 0 & A^1 & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \cdots & A^1 \end{pmatrix} \begin{pmatrix} y_{1,t} \\ y_{2,t} \\ \vdots \\ y_{N,t} \end{pmatrix} + \begin{pmatrix} \epsilon_{1,t} \\ \epsilon_{2,t} \\ \vdots \\ \epsilon_{N,t} \end{pmatrix}, \tag{4.1}$$

where $y_{i,t}$ is the vector with the endogenous variables for country i at time t,

$$y_{i,t}^T = \begin{pmatrix} \Delta GDP_{i,t} & \pi_{i,t} & \Delta D_{i,t} & \Delta i_{i,t} & \Delta I_{i,t} \end{pmatrix}, \tag{4.2}$$

where, in turn, GDP_t is real GDP; π_t is CPI inflation; D_t is the credit indicator; i_t is the policy rate; and I_t is the inequality metric. The coefficient matrix is a block matrix of size $N \times N$, with N the number of countries. Each non-zero partition of the block matrix is a coefficient matrix for the VAR system of a country. The implications of pooling the panel dimension of the VAR model can clearly be seen from this formulation. All the non-diagonal elements in the coefficient matrix are equal to 0, thereby excluding cross-sectional dynamics. Moreover, the fact that the partitions of the coefficient matrix are all equal makes that this specification estimates a single VAR model for all countries. As a matter of fact, the pooling approach collapses the panel VAR to a conventional VAR. Thus, pooling the panel increases the data points available for the estimation of a given set of parameters (or conversely, reduces the number of parameters to be estimated with a given dataset), but comes at the cost of reducing the richness of the dynamics the model can capture. The concomitant assumptions are that the dynamics between inequality, monetary policy and leverage most play out on the intra-country level, and that they are similar across countries. The former seems a reasonable assumption; the latter can be tested by checking how well the VAR can track the historical evolution of the variables. Reassuringly, it performs relatively well at this task, given the fact that there is a large crisis included in the sample period. The model is estimated with Bayesian methods. The essence of the Bayesian approach is that it enriches parameter estimation with the researcher's prior beliefs about the distribution of the parameters of interest, which parameters are regarded as stochastic, as opposed to 'given in nature', like in the frequentist approach. The prior beliefs are captured in the prior distribution of the parameter vector of interest, $g(\theta)$, which is specified by the researcher. This prior distribution is then combined with the likelihood function of the parameter given the data, $l(theta|y)$, to estimate the posterior distribution of the parameter given the data, $g(\theta|y)$, which captures all the information the researcher has available on the parameter vector θ, including his/her beliefs. This process can be summarised in the equation (Kilian and Lütkepohl, 2017, p.141)

$$g(\theta|y) \propto g(\theta)\ell(theta|y), \tag{4.3}$$

where $\propto$ means 'is proportional to'. This is basically a reformulation of Bayes' rule, hence the name Bayesian estimation. There exist several algorithms to compute the posterior distribution, repeated sampling from which forms the basis for further statistical inference. I use the Gibbs Sampler algorithm implemented in the Bayesian Estimation, Analysis and Regression (BEAR) Toolbox, which was constructed by researchers at the ECB to facilitate state-of-the-art Bayesian analysis (Dieppe et al., 2018). The prior I use is the traditional normal-inverse Wishart, which in the context of the panel VAR is specified for a pooled residual variance matrix. The hyperparameters for the prior are set to the default values of the toolbox, which are values often found in the literature.[1] The

[1] In particular, the autoregressive coefficient $\rho = 0.8$, the overall tightness parameter λ_1=0.1, the lag decay parameter λ_3=1 and the exogenous variable tightness λ_4=100.

implication of using an alternative parametrization is scrutinized in Appendix, Figure A.2.

4.1.1 Panel Unit Root Tests

Before estimating the model, I test for stationarity of the variables by means of panel unit root tests. Such tests are the panel data equivalent of the standard Augmented Dickey-Fuller test (ADF). Table A.1 in Appendix reports the t-statistics of the tests. Three different unit root tests are given because the tests differ in how they treat the autoregressive parameters in the Dickey-Fuller regression. The Levin et al. (2002) (LLC) test treats the parameters as common across units by pooling observations, thus effectively focusing on the time dimension. The test by Im et al. (2003) is a mean-group test for the units, which implies that the parameters vary across units but are constant over time. Finally, the Fisher-type test was suggested by Maddala and Wu (1999) and combines the p-values of the i independent ADF tests to test a joint hypothesis on the i units, thus allowing the parameters to vary across both units and time (Kleiber and Lupi, 2011, pp.3-5). The tests are reported for the variables in (log) levels and in first (log) differences, both with either only an intercept or with an intercept and a time trend. Overall, the tests strongly suggest stationarizing all variables by taking first differences, as the null hypothesis of a unit root is almost never rejected for the variables in levels, and almost always for the variables in differences. [2] Consequently, I take first differences of all variables, except for i_t, which I leave as it is to facilitate the interpretation of the monetary policy shock. While in general, the variables in a VAR should be stationary to avoid spurious correlations and ensure that the model is valid (in that the residuals resemble white noise), in Bayesian estimation the parameters are not really affected by non-stationarity, unless the data is also cointegrated (Fanchon and Wendel, 1992). As

[2] In the LLC test there are a few quirky rejections for variables that are intuitively non-stationary such as GDP. These can be attributed to the fact that the test tends to over-reject when there is cross-sectional dependence, which there naturally is, with several European countries included in the panel (Kleiber and Lupi, 2011, p.9).

a result, it is not uncommon in the literature to estimate Bayesian VARs in levels, and some argue that such VARS in levels even outperform VARs in differences (Carriero et al., 2015; Bańbura et al., 2010). Nonetheless, I take all variables except the interest rate in first differences so as to conform with traditional VAR estimation methods.

Chapter 5

Results

5.1 Structural Analysis: Monetary Policy Shock

5.1.1 Structural Identification

It is common in VAR research to impose a parsimonious amount of restrictions on the variance matrices of the system's shocks (the ϵ's above) so as to disentangle the effect of these shocks - which are usually correlated - on the system. The correlated versions of these shocks, as estimated without any restrictions, are called the reduced-form shocks, while the disentangled versions are called the structural shocks. Most often, economic interest goes out to the pure, uncorrelated shocks to the economy, that, is, the structural shocks. For the purpose of identifying these, many different methods have been proposed. I make use of a combination of short- and long-term sign restrictions, by way of the algorithm of Arias et al. (2018). The restrictions imposed are summarised in Table 5.1.1.

The attractiveness of sign restrictions is that they allow the researcher to remain agnostic about the economic effects she is investigating. The approach by Arias et al. (2018) ensures that this is the case by making sure the prior used for inference does not introduce any restrictions beyond the ones specified by the researcher (Kilian and

Table 5.1: Sign Restrictions on Structural VAR Model

Response	Monetary Policy Shock
GDP	- (1)
Gini	?
Credit	- (1-1)
Inflation	- (1)
Policy Rate	+ (1)

[1]Note: Signs indicate response to +100 basis point shock.
[2] All variables are in (log) differences, except the policy rate.
[3] Numbers in parentheses indicate final period wherein restriction applies. No parenthesis means only on impact. Two numbers means from period X to period X+s.

Lütkepohl, 2017, pp.452-3). To stay true to this approach, I do not restrict the response of inequality, and impose the other restrictions based on uncontroversial estimates from the literature. I opt for a partial identification strategy, identifying only the monetary policy shock.[1] Thus, I impose that an increase in the interest rate (last column) decreases GDP and inflation for two years after impact, in line with the lower 95% bound of the impulse responses from the traditional DSGE model of (Smets and Wouters, 2003, p.56), as well as the FRBNY-DSGE model of the Federal Reserve of New York (Del Negro et al., 2013). Also in line with these models, I restrict the temporary shock to the policy rate to persists for two years as well. This can alternatively be understood as the central bank smoothing out interest rate changes (Rudebusch, 2002). Furthermore, I impose that the total real credit to the non-financial private sector declines between the first and second year after the interest rate rise. This agrees with several empirical estimates in the literature on the effect of monetary policy on credit (Bauer and Granziera, 2016; Riksbank, 2014; IMF, 2015). Nonetheless, several other empirical studies in the literature suggest that contractionary monetary policy can actually increase real credit growth. Indeed, when leaving credit growth unrestricted under an alternative full identification

[1]While I experiment with richer identification schemes, none of the IRFs of a shock to inequality obtained thereby are very informative about the effect of inequality on credit, which is of interest here. I come back to this issue in the next paragraph. For completeness, I report the full identification scheme and the corresponding IRFs in Appendix, Table A.4 and Figures A.2 and A.2.

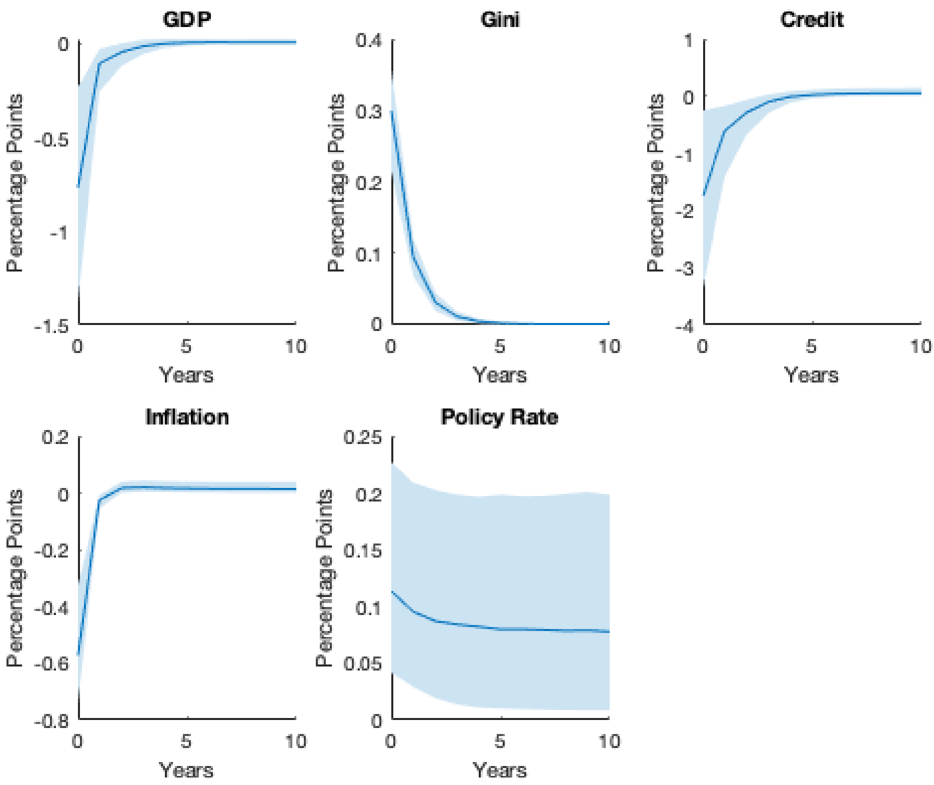
approach, the IRF obtained is positive. The purpose of still constraining the credit response to be negative is to stack the cards in favour of leaning against the wind, which the hypothesis this paper studies would undermine. I return to the issue of positive credit growth in section 5.2.

5.1.2 Impulse Response Functions

Figure 5.1.2 presents the baseline impulse response functions (IRF) from the model specified above.[2] First, the differenced Gini coefficient clearly and significantly rises in response to a monetary policy shock, rising by 0.3 percentage point (pp) on impact and remaining above steady state for 4 years after the shock. Since the model's estimated steady state growth of the Gini is around 0, this simply means that the Gini coefficient rises by about 0.3 pp in the first year after a contractionary monetary policy shock, and by around 0.5 pp over the whole horizon after the shock. These estimates are remarkably similar to those in Coibion et al. (2017), who find, using household survey data for the U.S. from 1980Q1:2008Q4, that the Gini increases by a cumulative 0.5% after 20 quarters (p.78). Contrary to their findings, however, the Gini here increases immediately on impact. This difference might simply be attributable to differences in the data and methods used. It is also possible that it is due to the fact that their policy rate measure only includes conventional monetary policy, while mine also includes unconventional monetary policy, which potentially has a more direct impact on inequality. Lenza and Slacalek (2018), for example, studying the effect of ECB QE on inequality, find that it decreases the Gini by about 0.2% one year after the shock. Taking into account the possible asymmetric distributional effects of monetary policy (Furceri et al., 2018), where contractionary shocks have a larger effect than expansionary ones, their results also align neatly with mine. Overall, the estimated effect of the monetary policy shock on the Gini is closely in line with other estimates from the literature. Given that the

[2] Countries for the baseline IRFs include AT, AU, BE, CA, CH, DE, DK, ES, FI, FR, GB, GR, IE, IT, JP, NL, NO, NZ, PT, SE, US. Sample period is 1995-2016.

Figure 5.1: Baseline Impulse Reponse Functions to a +100 Basis Point Monetary Policy Shock, Annual

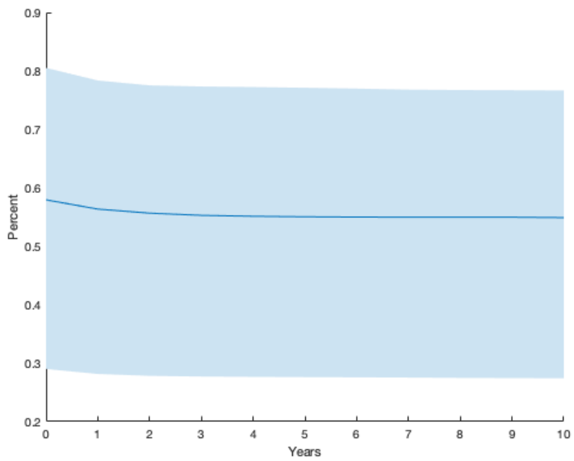


[1]Blue area is 68% confidence interval.[a] N=462, lag=1. Sample period=1995-2016.
[2] All variables except policy rate are in (log) differences.

[a]Note that, as is standard in Bayesian VAR estimation, this confidence interval is the region where 68% of the highest posterior distribution's probability mass is concentrated (Kilian and Lütkepohl, 2017, p.142). The upside of this approach is that the confidence interval (the credibility set, to be entirely correct) includes both identification and estimation uncertainty; the downside is that, since the estimates are only set identified, the confidence interval excludes parts of the parameter's identified set, and is in that sense narrower than a frequentist confidence interval (Moon and Schorfheide, 2012).

effect is obtained under an agnostic identification approach, this supports the credibility of the model's results. An additional result the SVAR obtains, which also consistent with the literature (Coibion et al., 2017, fig.8), is that monetary policy is responsible for about half of the variation in the forecast errors of the Gini coefficient, as depicted in Figure 5.1.2.

Figure 5.2: Contribution of Monetary Policy to Forecast Error Variance of Gini Coefficient



[1] Blue area is 68% confidence interval. Baseline specification.
[2] Source: Author's Calculations.

Second, credit growth responds negatively to a contractionary monetary policy shock, decreasing by about 1.5%, which decrease persists for 4 years. Note that the fact that credit growth does not turn positive at any point on the horizon implies that monetary policy is not neutral with respect to credit, confirming the persistence of the credit cycle Drehmann et al. (2012). Both the magnitude and the duration of the credit IRF are in line with similar estimates from the literature (IMF, 2015, par.24 & f.n 19) (Svensson, 2017*a*, fig.7). The confidence interval is quite wide, potentially reflecting differential credit dynamics across the panel. Note that in section 5.2, the extended reduced-form VAR finds that higher policy rates actually increase credit. As I explain there, the literature is not clear on the point, with several studies finding support for both a negative and a positive effect of the policy rate on credit. As a matter of fact, under an alternative identification scheme that does not restrict credit, the SVAR still obtains the result that monetary policy increases the Gini, but the effect on credit growth is now reversed. Nonetheless, I continue to restrict the credit response to be negative so as to stack the cards against the hypothesis that the distributional side-effects of monetary policy diminish its capacity to lean against the wind. This leads to the strong result, which I will elaborate further below, that *even if* LAW can decrease credit growth on the short to medium run, its effects are largely reversed on the long run because of its distributional side-effects.

Third, GDP and inflation respond negatively to a contractionary monetary shock, as imposed. The magnitude and duration of the responses are well in line with conventional estimates (Smets and Wouters, 2003). GDP growth declines by 0.7% in the first year and then gradually returns to steady state after about 3 years. Inflation declines by 0.6% and returns to steady state at a similar rate. Finally, the policy rate increases by about 0.1% on impact after a 1% exogenous shock to it, and remains at that level over the whole 10 year horizon. This response is again in line with estimates from the literature (Boivin and Giannoni, 2002, fig.3). Note that the fact that the rate does not increase 1-to-1 with the shock to it reflects the fact that after the initial shock, monetary policy responds endogenously to the drop in inflation and output in the first year after impact. The wide confidence interval probably reflects differences in the conduct of monetary policy across the panel, and the non-stationarity of the interest rate. The fact that the rate does not return to steady state most likely reflects the non-stationarity of the shadow-rate-augmented policy rate measure used in this paper. Alternatively, it can also reflect the path-dependence of the interest rate.

Robustness

- *Alternative Model Specifications*

Initial robustness checks suggest that the structural VAR is stable to alternative specifications. First, the findings are robust to changing the imposed persistence of the shock on GDP, inflation and credit, as well as to imposing a more complete identification scheme (see Appendix, Figure A.2). Second, I experiment with alternative hyperparameter values for the prior. Although a more systematic robustness check is needed in this respect, the similarity of the results for an alternative, reasonable parametrization in Appendix, Figure A.2 suggests the findings are robust to alternative parametrizations. Third, increasing the lag order does not affect the findings much, apart from increasing the width of the confidence intervals, probably due to the larger number of parameters that need to be estimated.

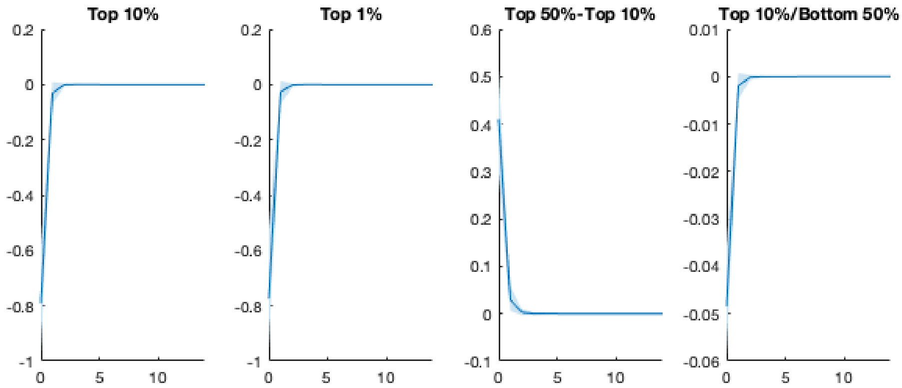
- *Different Inequality Measure*

Fourth, using an alternative inequality measure preserves the inequality-increasing effect, with some qualifications. Specifically, using different percentiles' share of total income with the same baseline identification scheme, the SVAR makes the precise prediction that a contractionary monetary policy shock increases the income share of the 50th to 90th percentile, while decreasing that of the top 1%. This can be seen in Figure 5.1.2 from the fact that the top 10% and top 1% income shares go down by about the same amount, suggesting that it is largely the drop in the top 1% share that is responsible for this decline.[3]

While other studies of monetary policy and inequality find that the top 10% share increases after a contractionary monetary policy shock, or vice versa (Coibion et al.,

[3] I only report the response of the income shares, as the other IRFs are very similar to the baseline results.

Figure 5.3: Impulse Reponse Functions to a +100 Basis Point Monetary Policy Shock, Alternative Inequality Measures



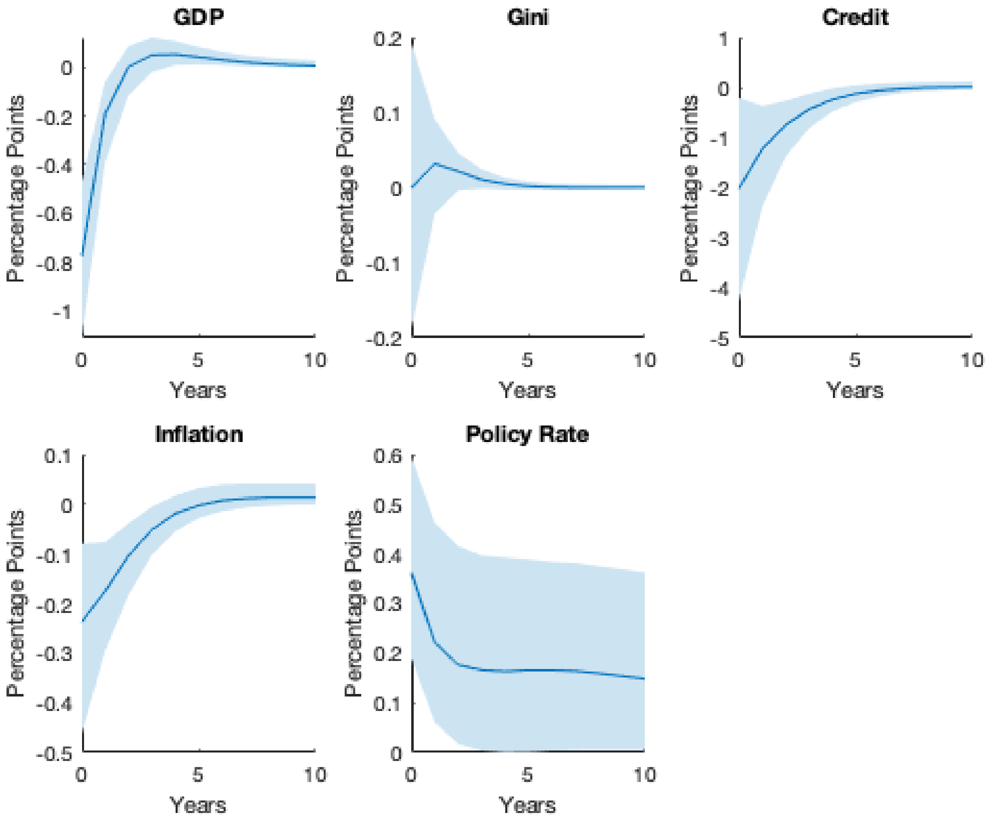
[1] Blue area is 68% confidence interval. N=380, lag=1. Sample period=1995-2014.
[2] Income shares are pre-tax.
[3] Source: Author's Calculations.

2017; Lenza and Slacalek, 2018), these are mostly based on panel data, which rarely representatively include the top 1%. An explanation for the decline in the top 1%'s income share could be that the contractionary shock depresses the price of stocks. If the top 1%'s income were strongly dependent on capital income from equities, this would explain the drop in their income share. This is certainly the case for the U.S., where the top 1% derives 60% of its income from capital, compared to 40% for the top 10% earners (Alvaredo et al., 2018). In Germany, the top 1% derives about 20% of its income from capital. Most German businesses are unincorporated, however, and business income, which consists of a capital and a wage component, counts for around 40% of income (Bartels, 2017). It thus certainly seems possible that a decrease in stock prices would result in a loss of the top 1%'s income. Moreover, several studies find that the most equity holdings are strongly concentrated at the top of the income distribution, and that these top earners are the only ones directly active in financial markets (Adam and Tzamourani, 2016; Denk and Cazenave-Lacroutz, 2015). Looking further at Figure 5.1.2, it can be seen that the ratio of the 90th percentile's income share to the 50th percentile's income share does not change much, suggesting that the bottom 50% of the income distribution does not gain much, or even loses out, from a contractionary policy shock. In general, however, it is not easy to interpret the IRFs for the percentiles' income shares, as they are expressed in growth rates (first differences), and these rates differ strongly depending on which percentile one looks at. In other words, a 0.8% decline in the IRF of the top 1% income share does not necessarily imply that this percentile's income share also actually decreases by exactly 0.8%. Moreover, given the worse coverage of the WID compared to the SWIID, the sample size is a lot lower with these alternative inequality measures, leading to both a lack in comparability with respect to the earlier IRFs and potentially less precise estimates. The purpose of this exercise, then, should not be pushed beyond the goal of examining the robustness of the findings to an alternative inequality measure. In that light, the increase in the income share of the 50th-90th percentile would be in line with an increase in the Gini coefficient. If the gains in the income share of these percentiles comes from both the bottom 50% and the top 10%, this would lead to an increase in the Gini, as transactions between income groups farther away from each other on the distribution affect the Gini more than transactions between income groups closer to each other. In conclusion, then, examining the IRFs of the structural VAR using income shares as an inequality metric delivers findings consistent with the hypothesis that the Gini coefficient goes up after a contractionary monetary policy shock.

- *Excluding Europe*

Fifth, in Figure 5.1.2, I examine the consequence of excluding all but one (Germany) of the European countries. This robustness test serves to assure that the results are not driven by common European dynamics. While this obviously decreases the precision of the estimates, the IRFs for the variables other than inequality are very close to the baseline. The interest rate responds more strongly to a monetary policy shock. This is

Figure 5.4: Impulse Reponse Functions to a +100 Basis Point Monetary Policy Shock, Excluding Europe

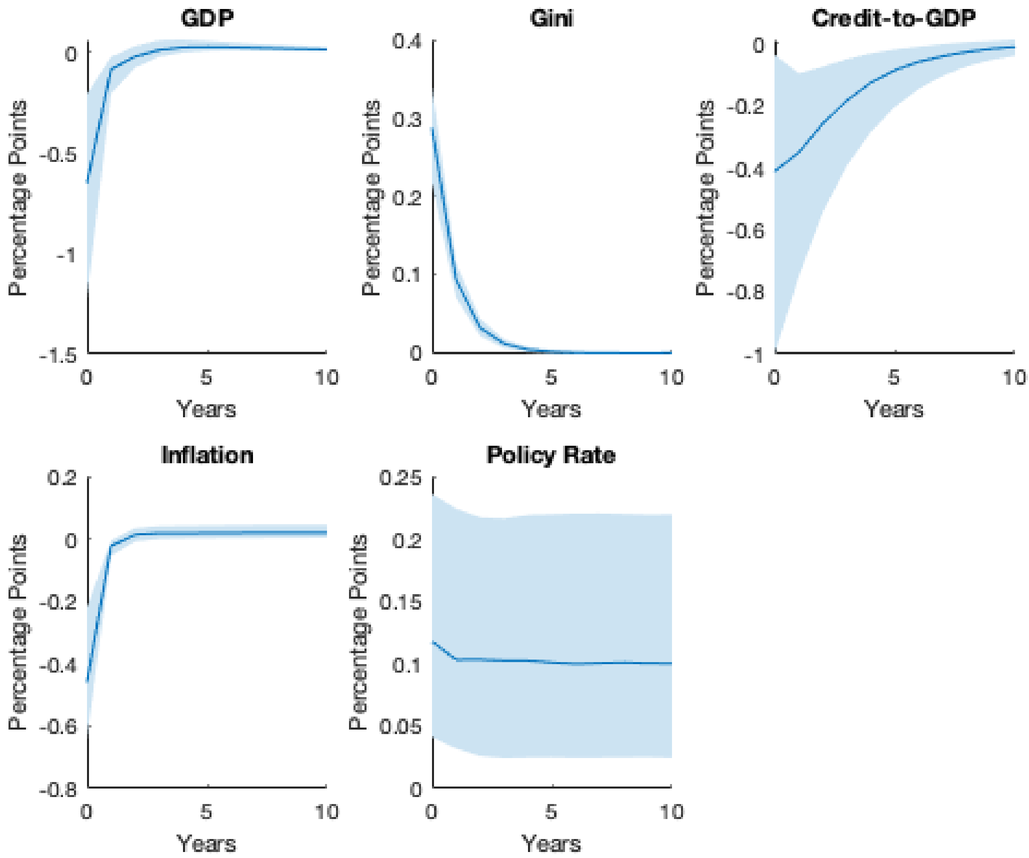


[1]Blue area is 68% confidence interval. N=231, lag=1. Sample period=1995-2016.
[2] Source: Author's Calculations.

probably because the estimated endogenous dynamics of the interest rate are diminished when including the European countries, as the ECB does not often respond explicitly to the idiosyncratic macroeconomic dynamics of one particular Euro Area country. The IRF for the Gini, meanwhile, retains the same sign and duration, although its confidence interval is much wider. In sum, these results suggest that the dynamics of the VAR's monetary policy responses are not driven purely by common European dynamics.

- *Different Credit Measure*

Figure 5.5: Impulse Reponse Functions to a +100 Basis Point Monetary Policy Shock, Alternative Credit Measure



[1]Blue area is 68% confidence interval. N=231, lag=1. Sample period=1995-2016.
[2] Source: Author's Calculations.

Sixth and last, I test for the use of an alternative credit measure. Specifically, in Figure 5.1.2, I plot the IRFs for a contractionary monetary policy shock with the percentage deviation of the credit-to-GDP ratio from its Hodrick-Prescott filtered trend included as credit measure. As can be seen from the graphs, the responses of all variables are very similar in magnitude and duration. The credit-to-GDP ratio's response has a similar sign and duration as credit growth, although the magnitude of its decrease is smaller. Intuitively, this is because the contractionary shock decreases both the numerator and the denominator of the ratio. While it is possible that the credit-to-GDP actually increases in the first quarter after a contractionary monetary policy shock, the results indicate that in the first year after impact, the effect is to decrease the ratio, in line with other findings in the literature (Bauer and Granziera, 2016). The reasoning behind this is that the impact on GDP growth reverts to the mean quicker than the impact on credit growth, which is marked by a degree of financial-cycle persistence. Alternatively, the effect of the higher interest rate on credit growth might accumulate over time. The cumulative effect on the credit-to-GDP ratio also aligns with other literature, in that it is relatively small and close to being statistically insignificant if one considers the 90% confidence interval (Svensson, 2017*b*). Specifically, for the median response, the credit-to-GDP ratio decreases by about a cumulative 2% over the whole horizon. The estimated impulse responses for all variables are thus fairly robust to the use of an alternative measure of credit.

5.1.3 Cointegration Analysis

As demonstrated, monetary policy has a statistically significant effect on inequality, and a moderately significant one in economic terms. The next question is: how does inequality affect credit? On this point, however, the panel VAR is not very informative. Under most specifications, it fails to provide statistically significant responses of credit to a shock in the Gini coefficient, although the effect is generally positive (see Appendix, Figure A.2). This, however, does not necessarily imply that inequality has no effect on leverage. The relationship between inequality and credit might be a long-run relationship, where it is the changes to trend inequality that matter for the evolution of credit, not necessarily inequality as such (Paul, 2018). For that reason, I estimate a cointegrating relationship between the Gini coefficient and household credit growth in levels. Thereby, I supplement other research that estimates such a relationship for the top percentiles' income shares (Malinen, 2016). Since the time dimension in the PVAR is not very large, I extend the observations for the credit-to-GDP ratio and the Gini coefficient as far back as they are available, which leaves me with a bivariate unbalanced panel spanning the period from 1960 to 2017. To begin with, I carry out a set of seven panel cointegration tests, as developed by Pedroni (1999, 2004), which are reported in the Appendix, Table A.1. I do not time demean the variables in the test equations, as this would destroy the cointegrating relationship, which should be expected to be specific to each country. Moreover, in line with the earlier argument that inequality and credit dynamics are not marked by large cross-country dependencies, time demeaning the variables would be superfluous. The tests strongly suggest the presence of a cointegrating relationship between the Gini and the credit-to-GDP ratio, with 6 of the 7 tests rejecting the null of no cointegration at the 5% significance level, of which 4 tests at the 1% level. Furthermore, the panel and group ADF tests, which have the best power properties when T < 100 (which is the case), reject at the 5% and 10% level Pedroni (2004). Hence, the pre-tax Gini coefficient and the private credit-to-GDP ratio are cointegrated, suggesting there is a long-run equilibrium relationship between the two. Therefore, I proceed to estimate the cointegrating coefficient using three different panel cointegration regression techniques: fully modified ordinary least squares (FMOLS) (Phillips and Hansen, 1990); Pedroni's dynamic OLS (PDOLS) (Pedroni, 2001); and Canonical Cointegration Regression (CCR) (Park, 1992).[4] The three techniques propose different ways to estimate the cointegrating relationships (Wang and Wu, 2012):

$$CGDP_t = Gini_t'\beta + d1'\gamma_1 + u_{1t} \tag{5.1}$$

$$Gini_t = \Gamma_1 d1t + \Gamma_2 d_{2t} + \epsilon_t \tag{5.2}$$

$$\Delta\epsilon_t = u_{2t}, \tag{5.3}$$

with d_{1t} and d_{2t} the deterministic trend regressors, u_{1t} the cointegration error and

[4] To this end, I gratefully used the user-written Stata commands *xtcointreg* (Khodzhimatov, 2018), *cointreg* (Wang and Wu, 2012) and *xtpedroni* (Neal, 2014).

u_{2t} regressor innovations. In these equations, β is the parameter of interest. The three techniques all estimate variations of this equation that allow for heterogeneous short-run dynamics, while assuming the long-run parameter β to be homogeneous across countries. The reason for using these techniques is that they correct the endogeneity and serial correlation problems that arise in OLS when the equation variables exhibit long-run correlation. To this end, all three methods use kernel estimates of the variables' long-run variance. Particularly, first, FMOLS applies a semi-parametric correction to account for the problems OLS encounters (Wang and Wu, 2012). Second, PDOLS includes lags and leads of the regressors to achieve the same goal. Third, the CCR is very similar to the FMOLS, except that it transforms both regressors and regressand, while FMOLS only transforms the latter (Wang and Wu, 2012). In the panel version of these techniques, the cointegration parameters are estimated for each unit in the panel. They are then averaged over the panel by way of Pedroni's group-mean method (Neal, 2013). This results in a single estimate of the panel's cointegrating vector, which is reported for each technique in Table 5.2.

Table 5.2: Estimates of Cointegrating Vector Between Credit-to-GDP Ratio and Gini Coefficient

	FMOLS	PDOLS	CCR
β	7.05***	7.9***	6.97***
T-stat	43.15	20.95	43.73

[1] p < 0.001 ***. T-statistics are distributed N(0,1).
[2] PDOLS includes one lead and one lag and does not include time dummies. Lag selection for all long-run variances is based on Bayesian Information Criterion.

As one can see, the estimates lie in a very close range and suggest that on the long-run, a 1% rise in the Gini is accompanied by a 7% rise in the credit-to-GDP ratio. The similarity and significance of the estimates suggests a degree of robustness, although of course the techniques are not very dissimilar.[5] Though one should be cautious with

[5]Obviously, these estimates cloak a lot of heterogeneity. For brevity, and because it is not of crucial interest here, I do not report the unit-specific estimates. Inspecting them, however, reveals that in all

giving a causal interpretation to these estimates, if even half of the estimated relation were to be causal, it is clear that this would seriously hamper monetary policy's long-term ability to lean against the wind in a discretionary fashion. Figure 5.1.2 suggests such policy can decrease the credit-to-GDP ratio by a cumulated 2% with respect to its trend. Yet, if half of the cointegrating relationship is causally efficacious, the estimated 0.5% increase in the Gini following a contractionary monetary policy shock would lead to a 1.75% long-term increase in the credit-to-GDP ratio. Thus, in the long-run, nearly all of the beneficial effects of discretionary leaning would be off-set by the distributional effects of monetary policy. Though, again, caution is required, as we are comparing a trend-related increase to a deviation from trend, it is clear that the perverse inequality effects of monetary policy will hamper its long-term ability to lean against the wind in an economically significant way, even if the policy's inequality effect is of only moderate economic magnitude. Moreover, insofar as all countries in the panel were approximately inflation targeters during the sample period, the results are also relevant to systematic leaning considerations. Specifically, the discretionary policy shock can be interpreted as a one-off deviation from the optimal policy. From a calculus of variation argument, if such a temporary deviation has marginal benefits, it suggests the true optimal policy lies in the direction of the deviation, as discussed in section 2.1.2. Since the distributional side-effects of monetary policy decrease its effect on credit growth - which is estimated to be similar to the estimates in Svensson (2017*a*) - in the long run, the total marginal benefits of discretionary leaning would be lower than those estimated in Svensson (2017*a*). In that way, taking into account the distributional effects of monetary policy further weakens the argument for a deviation from inflation targeting towards a systematic LAW policy.

As a preliminary conclusion, the results in this section provide relatively robust evidence for a panel of advanced economies that a contractionary monetary policy shock has a positive effect on inequality, as measured by the Gini coefficient. Moreover, that

three techniques just 2 of the 22 estimates are negative, strongly supporting the finding of a general positive cointegrating relationship between the Gini and the credit-to-GDP ratio.

same shock is found to decrease credit growth, but only marginally so, and with the caveat that under alternative specifications, credit growth actually increases. Both findings are in line with other estimates from the literature. The findings are based on the Bayesian estimation of a structural vector-autoregressive model with sign restrictions. While the model does not find a statistically significant impact of an inequality shock on credit, a long-run cointegration analysis suggests that increases in the Gini can lead to measurable increases in the credit-to-GDP ratio. That suggests that monetary policy's perverse distributional side-effects can in the long run seriously hamper its ability to lean against the wind.

5.2 Policy Experiment: Systematic Leaning

5.2.1 A Credit-Targeting Taylor Rule

To supplement the structural analysis of a temporary monetary policy shock on inequality and credit - which, as mentioned in Chapter 2, is not considered to be a desirable LAW policy - I use an extended reduced-form version of the VAR model to carry out a policy experiment for the U.S., in the spirit of Juselius et al. (2017). Their approach is to impose a backward-looking Taylor-style policy rule on the interest rate in their quarterly VAR for the U.S., augmented with a weight on debt service. The rule takes the form

$$i_t = \rho i_{t-1} + (1-\rho)(r^\star_{t-1} + \pi_{t-1} + 0.5(\pi_{t-1} - \pi^\star) + 0.5\tilde{y_{t-1}} - \lambda \widetilde{dsr}_{t-1}), \tag{5.4}$$

where ρ is a smoothing parameter, $r^\star_t$ is the natural interest rate, $\pi^\star$ is the inflation target, $\tilde{y}_t$ is the output gap, $\widetilde{dsr}_t$ is the gap between the debt service ratio and its long-term average, and λ represents the weight the central bank assigns to financial stability, equal to 0.75 in their baseline. The authors' choice for the smoothing parameter is 0.85, which aligns with the higher end of the range of estimates in the literature (Kendall et al., 2013, p.7) (Hofmann and Bogdanova, 2012, p.43). Because I use annual data, which by construction is already smoother, I set this parameter to 0.7. The other parameters are set at the same level, that is, they follow the standard Taylor rule estimates with a 0.75 additional weight on the debt service gap.

The authors subsequently update the interest rate iteratively based on this rule while counterfactually re-estimating the VAR starting from different points in the past, the choice of which then determines when in the boom of the early 2000s the counterfactual central bank starts leaning. Their estimates for the natural rate and the output gap are based on an alternative filtering system that takes the financial cycle into account. I use the traditional estimates of these variables from Holston et al. (2017), available from the Federal Reserve Bank of New York.[6] Note that I also add the exogenous monetary policy shock back to the counterfactual Taylor-rule policy rate, as the experiment is one of the changing the systematic policy, not the discretionary policy shocks. It should be noted that the approach is not immune to the Lucas critique, meaning it cannot be guaranteed that the coefficients of the system would remain the same if the counterfactual policy was actually implemented. Nonetheless, the authors provide two reasons for why the critique might hold less force: first, they find that the Federal Reserve has in fact been reacting slightly to debt burdens over their sample period and two, they argue that if agents were to internalize the changes in financial market dynamics resulting from the counterfactual policy, this might just lead to an intensification of the policy's effect (Juselius et al., 2017, p.75). In all, the counterfactual results I present should be taken as more of a thought experiment to facilitate further speculation about the inequality effects of systematic leaning than as an exact empirical exercise. As such, however, the results do offer valuable insight into the mechanisms at work.

[6] For simplicity, I use the historical measure of the output gap, as differences in output measures used make it difficult to combine counterfactual output with the output trend estimates from the Federal Reserve. Though this is not ideal, given that the counterfactual policy rate is quite similar to the one in Juselius et al. (2017), it seems to not matter too much.

The extended VAR includes as endogenous variables:

$$\begin{pmatrix} \Delta GDP_t & \Delta p_t^A & \pi_t & \Delta D_t & \Delta I_t & \Delta p_t^H & \Delta \tilde{dsr}_t & \Delta i_t & \Delta u_t \end{pmatrix}, \tag{5.5}$$

where the same five variables as in the baseline SVAR are included, except that inflation is now measured by the change in the GDP deflator, as this is the inflation measure included in the classical Taylor rule. This baseline is supplemented with a stock market index p_t^A, a house price index p_t^H, a measure of the deviation of the debt service ratio from its sample mean $\tilde{dsr}_t$, and the unemployment rate u_t. The inclusion of these variables is meant to capture the links between monetary policy, inequality and leverage. The effect of monetary policy on leverage runs through the DSR and credit, as well as the price indices, which measure the value of assets in the economy. The effect of monetary policy on inequality runs through the unemployment rate, the debt service ratio and the house and stock price indices. These reflect an unemployment channel, a debt service channel, a mortgage refinancing channel and an asset valuation channel, as discussed in section 2.2.1. I estimate the system with one lag, similar to the four-quarter lags in Juselius et al. (2017).

5.2.2 Counterfactual

This paragraph presents the results of the counterfactual exercise with the credit-targeting Taylor rule described above. One issue that arises in the estimation of the model when the below-zero policy rate is set equal to the shadow rate is that the Taylor rule does not track the evolution of the historical rate very well. This should not be too surprising, as the Taylor rule pre-dates many of the current issues with the zero lower bound. Therefore, I first re-estimate the model without adjusting the policy rate for the ZLB. As shown in Figure A.2 in Appendix, the classic Taylor rule with a smoothing parameter equal to 0.7 tracks the historical evolution of the policy rate remarkably well. At the start of the crisis, the no-leaning Taylor rule is slightly above the historical rate, but it later dips into negative territory. The counterfactual evolution of the other variables is nearly identical to their historical evolution. By contrast, when augmenting the Taylor rule with a weight on the debt service gap in Figure 5.2.2, the path of the counterfactual rate is quite different. In the baseline set-up, the central bank starts leaning against the wind in 2001. Similar to the results in Juselius et al. (2017), the counterfactual central bank leans against the wind a little at the onset of the pre-crisis boom, but starts leaning *with* the wind (LWW) when the debt service gap begins to rise. It continues to do so when the housing crash materializes and the policy rate hits the ZLB. Thereafter, however, the counterfactual policy rate quickly rises again, before accommodating further deflationary pressures around 2014. The effect of the counterfactual policy on the other variables is mixed. The LWW before the crisis helps buffer the impact of the shock on GDP, but the later LAW hurts the post-crisis recovery of GDP. Unemployment follows a similar path as GDP. The stock market booms because of the LWW, but this boom is reverted when the LAW kicks in post-crisis. Counterfactual inflation does not change much. Remarkably, real credit actually falls under the LWW policy and rises under the LAW policy. It is possible that this is due to the reduced-form VAR wrongly picking up on the positive correlation between credit and the interest rate during the crisis. Nonetheless, the VAR is quite accurate in its forecasts for nearly all the countries in the panel, even with a crisis episode included in the sample period, which suggests it captures the dynamics between the variables pretty well. An alternative argument, then, also made by Svensson (2013), is that a higher policy rate actually induces real credit to rise instead of fall. The reasoning is that the stock of credit reacts only slowly to the rate rise, while the negative effect of a rate rise on prices materializes much faster, which leads to an increase in real credit, as its denominator decreases. The strong fall in stock prices in Figure A.2 when the post-crisis LAW policy kicks in does support this explanation. At the same time, property prices do not react much when the LAW policy kicks in, which further supports the argument, since falling property prices would lead to less refinancing and thus lower credit growth.[7] Furthermore, there are several empirical studies that find credit to react positively to higher interest rates, instead of negatively (Alpanda and Zubairy, 2017; Gelain et al., 2017; Robstad, 2018). In line with the increase in real credit, the debt service gap scarcely budges in response to the counterfactual policy. As result, LAW brings almost no gains in terms of a reduced magnitude or duration of the crisis, which Juselius et al. (2017) find results from a dampening in the debt service's fluctuations.[8] Lastly, the counterfactual Gini coefficient skirts its historical path from below during the crisis LWW episode, but ends up about 0.2% above its historical path at the end of the forecast. This reinforces the finding from the discretionary policy experiment in section 5.1.2 that contractionary monetary policy increases inequality, and extends it to the case where monetary policy is endogenous. Moreover, given that the degree of leaning is moderate and interspersed with an episode of LWW, this result indicates that the accumulated inequality effect of a sustained LAW policy might be tangible. It also provides support for the earlier-mentioned finding from the literature that the impact of monetary policy is asymmetric, with contractionary policy increasing inequality more than vice versa Furceri et al. (2018).[9] As a result, the outcome from the discretionary policy experiment seems to translate to the case of a systematic LAW policy: the distributional side-effects of contractionary monetary policy will hamper the long-term capacity of that policy to lean against the wind.

In the Appendix, I include the case where the policy rate is allowed to drop below the zero lower bound by extending it with the shadow rate estimates. Figure A.2 shows the evolution of the variables under a Taylor rule if $\lambda = 0$, which implies no leaning against

[7] The fact that inflation does not change much in the counterfactual does not necessarily undermine the argument, as in this extended VAR, inflation is measured by the GDP deflator, while credit is deflated by the CPI.

[8] They argue that such a dampening facilitates faster post-crash deleveraging and thus a diminished pungency of Fisher-style debt deflation.

[9] By contrast, the result that endogenous monetary policy affects inequality is at odds with the results in Furceri et al. (2018).

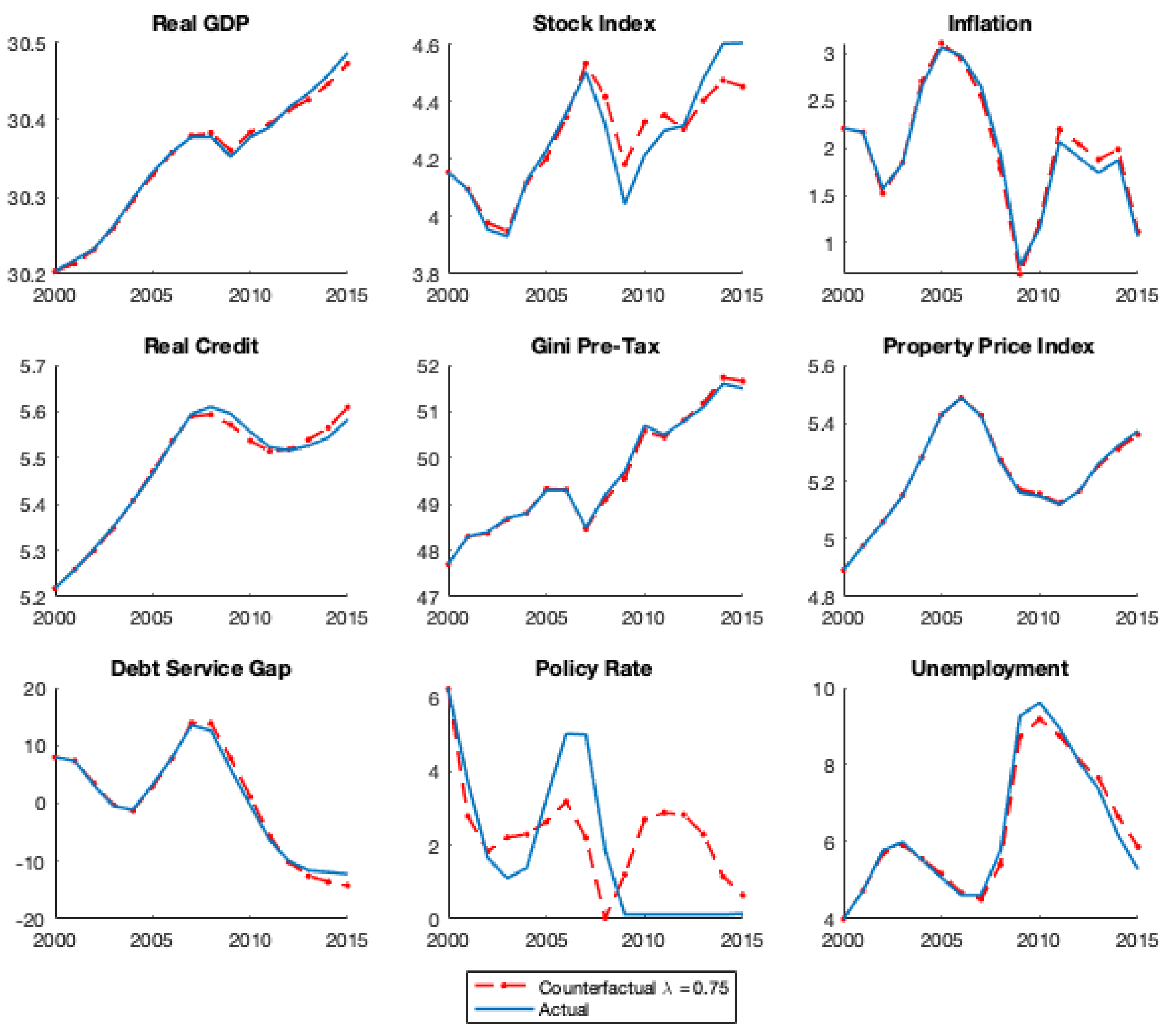
the wind. The parameters of the Taylor rule have been set to make the counterfactual rate match the historical ZLB-adjusted rate as close as possible. This leads to the very high values of $a_\pi = 2$ for the inflation term and $a_y = 2.25$ for the output gap term, compared to 0.5 for both in the classic Taylor rule.[10] Given the magnitude of the QE programmes carried out by the Federal Reserve, the largeness of these parameter values is not too surprising. The fact that the output gap term is larger than the inflation term suggests an increased concern with output stabilization over the forecast period, which is also not unexpected in crisis times. This alternative parametrization makes that the counterfactual evolution of the other variables in the VAR tracks their historical evolution quite well. Figure A.2 subsequently augments the Taylor rule estimated in this way with a debt service weight $\lambda = 0.75$. The resulting counterfactual realizations are quite similar to the ones in the case where the interest rate is constrained by the ZLB, so the same considerations apply.

In conclusion, the counterfactual policy experiment suggests that both discretionary and systematic monetary policy tightening increases inequality, as measured by the Gini coefficient. In the light of the earlier evidence that the Gini is cointegrated with the credit-to-GDP ratio, this indicates that the capacity of both types of monetary policy to lean against the wind will be attenuated in the long run by the distributional side-effects of these policies. Moreover, the counterfactual evolution of the other variables in the extended VAR suggests that even without taking into account these distributional concerns, the efficacy of a LAW policy in reducing credit growth is limited, and might even be negative. Two important caveats to these findings are; first, that the counterfactual experiment is not immune to the Lucas critique; second, that the counterfactual evolution of the variables is conditional on the assumption that the dynamics of the variables involved is similar, in the United States, to the average dynamics for the panel of advanced economies. It is very well possible that the credit and inequality dynamics

[10] The smoothing parameter is again set equal to 0.7.

in the U.S. are more pronounced than in other economies in the panel. This could, then, explain the difference between my results and those of Juselius et al. (2017), who estimate their VAR with U.S. data alone and find benefits to leaning systematically against the wind. Even so, my results suggest, in the least, that such benefits from systematic LAW would fail to materialize in the average advanced economy. What is more, the distributional side-effects of monetary policy, for whose presence I find strong evidence, will further diminish any potential benefits from LAW.

Figure 5.6: Counterfactual vs. Actual Evolution of Variables in Extended VAR with ZLB, Leaning



[1] Counterfactual is Credit-Targeting Taylor Rule. N=256, lags=1.
[2] Source: Author's Calculations.

Chapter 6

Conclusion

This thesis has scrutinized the interaction between two concerns relevant to monetary policy. These are; on the one hand, the long-standing question of whether monetary policy should actively tackle financial stability concerns; on the other hand, the new question of whether monetary policy should take into account its own distributional side-effects. By studying the perverse distributional side-effects of contractionary monetary policy in both its discretionary and its systematic incarnation, this thesis finds, for a panel of advanced inflation-targeting economies, that such side-effects can tangibly impair monetary policy's long-term capacity to lean against the wind. Leaning against the wind, in this setting, means curbing the cyclical fluctuations in the credit stock through contractionary interest rate policy with the purpose of fostering financial stability. By uncovering an additional channel through which monetary policy indirectly affects the credit stock, this thesis provides further support for the consensus in the literature, which prescribes that monetary policy should focus on its traditional mandates of price and output stability, and leave financial stability concerns to macroprudential policy.

Though this thesis makes use of a suite of statistical methods to empirically study the intricate web of relations that runs between monetary policy, inequality and financial stability, the results obtained should not be interpreted overly rigidly. Specifically, as they are based on Bayesian methods for a pooled panel analysis, the results are subject to substantial statistical uncertainty. Moreover, this thesis's evaluation of a systematic financial stability-targeting monetary policy rule by way of a counterfactual policy experiment is not immune to the Lucas critique. These caveats notwithstanding, the links between monetary policy and inequality, and inequality and leverage, arise with remarkable statistical significance, given the cross-country approach, suggesting that the findings in this thesis hold quite generally.

A point that is left for future research is whether the ability of financial stability-targeting monetary policy to reduce the magnitude of crises can alleviate the distributional concerns plaguing such policy. More generally, the research into policies that can alleviate cyclical fluctuations without causing too much output losses remains extremely relevant for the welfare of citizens in both advanced and emerging economies. Though this thesis suggests that monetary policy is ill-suited to the task, causing welfare losses not only in the form of reduced output, but also in the form of increased inequality, this does not imply other policy tools, such as macroprudential policy, could not perform better at pre-empting crises. As both crises and output losses can significantly harm vulnerable income groups, the search for a macroeconomic policy that can mitigate one without worsening the other remains of high priority.

Appendix A

Appendix

A.1 Tables

Table A.1: Pedroni Cointegration Tests for Credit-to-GDP and Gini

Test Stats	Panel	Group
v	-2.014	.
rho	2.692	3.942
t	3.174	4.219
adf	1.993	1.575

[1] Tests are based on Pedroni (1999) and Pedroni (2004). Tests are without time demeaning to preserve specificity of cointegrating relationship in each country. All t-statistics are distributed N(0,1).

Table A.2: Summary Statistics for All Countries in Panel, Selected Variables

	Credit-to-GDP		Gini		π		Shares		i		RPPI		u	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
AT	130.7	14.5	46.0	1.6	1.9	0.8	85.0	38.4	1.9	2.7	*NA*	*NA*	4.9	0.6
AU	158.4	30.3	47.7	0.6	2.6	1.2	71.2	22.7	4.8	1.5	225.6	100.2	6.2	1.4
BE	164.0	38.8	40.2	3.4	1.9	1.0	65.9	21.2	1.9	2.8	179.7	60.5	8.2	0.9
CA	165.0	19.8	46.0	0.7	1.8	0.7	68.5	23.3	2.9	1.9	176.7	69.5	7.6	1.1
CH	201.4	14.9	40.8	1.5	0.6	0.8	71.1	20.0	− 0.6	4.2	115.9	22.3	4.0	0.7
DE	117.4	8.0	49.7	2.3	1.5	0.7	64.5	18.9	1.9	2.7	98.5	6.3	7.9	2.0
DK	195.0	45.4	45.3	1.8	1.9	0.8	43.2	24.6	2.1	2.9	204.7	67.5	5.7	1.3
ES	152.8	51.5	46.3	3.4	2.5	1.5	82.3	29.4	2.5	3.5	201.8	77.4	16.6	5.7
FI	144.0	26.3	46.7	1.3	1.5	1.1	78.9	35.0	1.9	2.7	195.8	58.0	9.7	2.7
FR	154.9	22.2	47.1	0.6	1.4	0.8	73.5	21.3	2.0	2.9	182.3	63.3	9.9	1.5
GB	158.2	27.1	52.7	0.4	2.0	0.8	80.8	16.1	3.6	2.5	236.5	90.2	6.3	1.4
GR	86.2	37.5	48.8	1.8	0.5	10.5	298.4	181.7	3.6	5.5	*NA*	*NA*	*NA*	*NA*
IE	201.6	96.0	50.2	1.1	2.1	2.3	74.3	29.2	2.2	3.1	251.2	97.6	9.0	4.1
IT	97.7	23.2	47.5	0.8	2.2	1.3	99.5	33.1	2.7	3.8	145.5	31.7	9.6	2.0
JP	178.5	21.4	40.5	1.5	0.1	0.9	80.4	18.1	− 1.1	1.7	76.1	14.9	4.2	0.7
KR	161.9	18.8	33.8	0.9	3.2	1.7	62.5	27.8	*NA*	*NA*	133.3	31.6	3.6	1.1
NL	233.8	35.8	44.0	1.0	1.9	0.9	81.8	23.0	1.8	2.7	211.2	59.5	5.5	1.5
NO	190.3	34.5	43.5	1.3	2.1	0.9	49.7	29.8	3.6	2.0	255.8	115.6	3.8	0.7
NZ	160.2	26.2	46.4	0.6	2.1	1.1	75.9	14.9	5.4	2.3	201.0	85.3	5.6	1.3
PT	173.1	43.8	50.9	0.7	0.4	8.3	93.3	26.9	2.3	3.4	*NA*	*NA*	9.3	3.4
SE	182.6	39.8	47.1	0.5	1.2	1.1	53.7	23.0	2.3	3.6	224.4	101.8	7.6	1.2
US	144.3	16.0	49.3	1.3	2.2	1.0	65.7	21.2	2.0	3.3	168.1	46.0	5.9	1.6

[1] Statistics are computed across sample period 1994-2016.
[2] RPPI=Residential Property Price Index, u is unemployment, i is policy rate adjusted for ZLB with Shadow Short Rate, π is CPI inflation. For detailed data description, see Chapter 3.

Table A.3: T-Statistics of Three Panel Unit Root Tests for All Variables in VAR

	Levels		Δ	
	Intercept	Trend	Intercept	Trend
Levin, Lin and Chu (2002)				
Log Unemp.	2.53	-2.04*	-5.43**	-4.55**
Interest Rate	8.35	6.04	2.02**	-3.26**
Log CPI	-1.18	-2.2**	-4.13**	-4.51**
Log Credit	-1.54	-0.52	-3.21**	-6.3**
DSR	-1.18	0.21	-1.71**	-1.83**
Log GDP	-2.98**	-3.27**	-4.75**	-3.87**
Log Shares	-2.46**	0.52	-0.36**	-6.2**
Log House	-2.05**	-3.67**	-3.38**	-1.01
Gini	-0.69	-3.18**	-3.34**	-2.08**
Cred-to-GDP	0.0878	8.2516	1.8921	3.9832
Im, Pesharan and Shin (2003)				
Log Unemp.	0.93	-0.16	-4.29**	-2.67**
Interest Rate	0.9	3.76	0.62	0.06
Log CPI	0.51	-0.36	-3.99**	-2.58**
Log Credit	0.46	2.54	-3.44**	-4.43**
DSR	-0.64	2.82	-0.94	-0.25
Log GDP	-0.02	-1.15	-4.92**	-3.38**
Log Shares	-1	0.62	-3.93**	-7.22**
Log House	-0.56	-0.92	-2.71**	-0.18
Gini	1.05	-0.53	-3.16**	-1.2**
Fisher-Type Test				
Log Unemp.	-3.05**	-0.86	-4.07**	-2.1**
Interest Rate	3.96	5.78	-3.79**	-3.15**
Log CPI	0.68	0	-5.25**	-3.77**
Log Credit	0.24	1.49	-4.66**	-3.59**
DSR	0.6	2.93	-3.03**	-2.26**
Log GDP	0.6	0.1	-5.14**	-2.66**
Log Shares	-2.71**	0.06	-6.02**	-5.55**
Log House	-1.13	-1.99**	-4.1**	-2.91**
Gini	0	-0.78	-5.91**	-4.25**
Cred-to-GDP	0.4061	0.4061	-4.3170**	-3.6624**

[1] * $p < 0.5$, ** $p < 0.1$.
[2] Null hypothesis is presence of a unit root, alternative hypothesis is stationarity. T-statistic for Fisher-type test is under inverse normal distribution.
[3] All tests are with fixed effects.
[4] First two tests: lag selection based on Bayesian Information Criterion. Last test: lag=1.
[5] Im, Pesharan and Shin test not available for credit-to-GDP due to excessive lag selection.

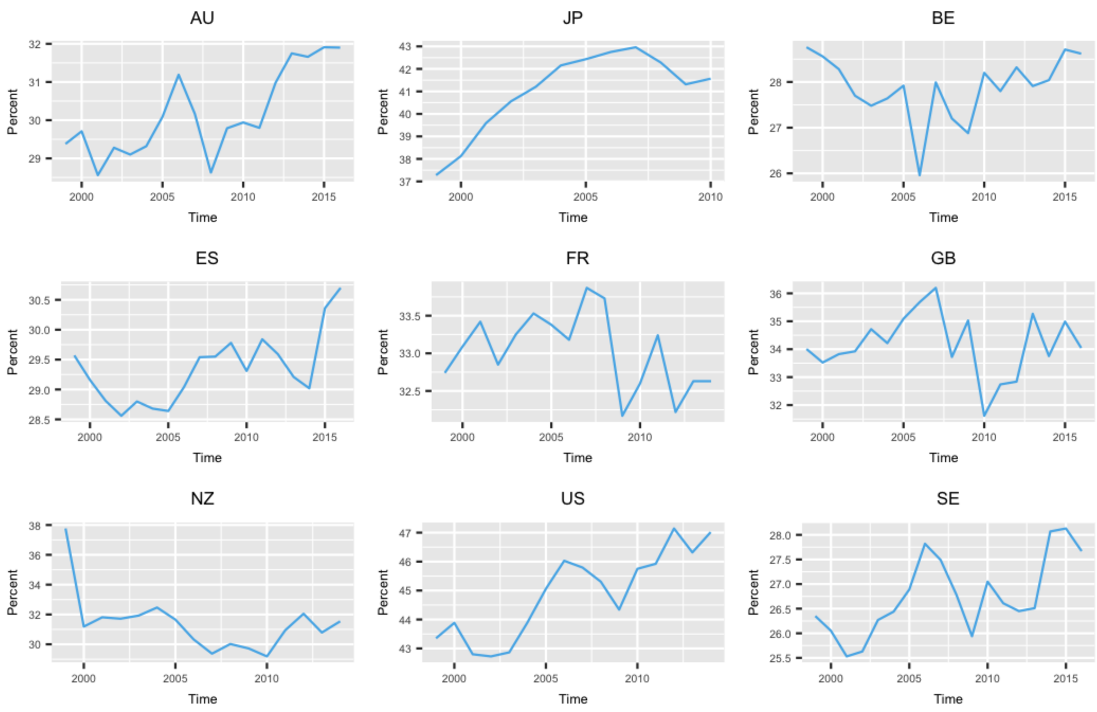
Table A.4: Alternative, Full Set of Sign Restrictions on Structural VAR Model

			Shock		
Response	Output	Inequality	Credit	Inflation	Monetary
GDP	+				- (1)
Gini		+			
Credit			+ (1)		- (1-1)
Inflation	+			+ (1)	- (1)
Policy Rate	+			+	+(1)

[1] Signs indicate response to +100 basis point shock.
[2] All variables are in (log) differences, except the policy rate.
[3] Numbers in parentheses indicate final period wherein restriction applies. No parenthesis means only on impact. Two numbers means from period X to period X+s.

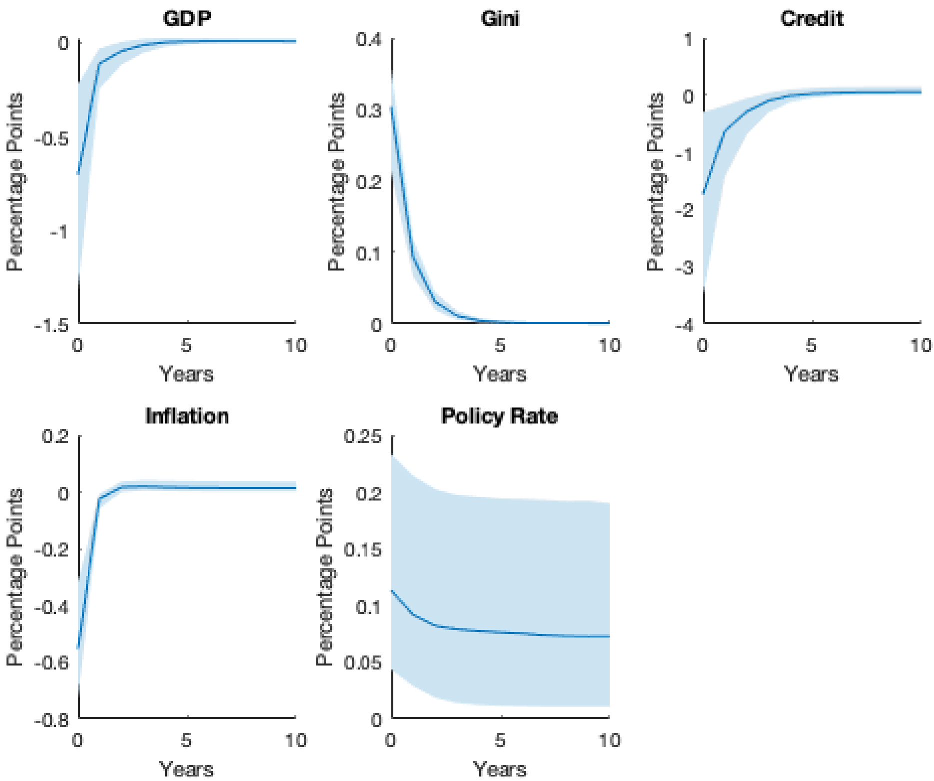
A.2 Figures

Figure A.1: Evolution of Top 10% Income Share, Selected Countries



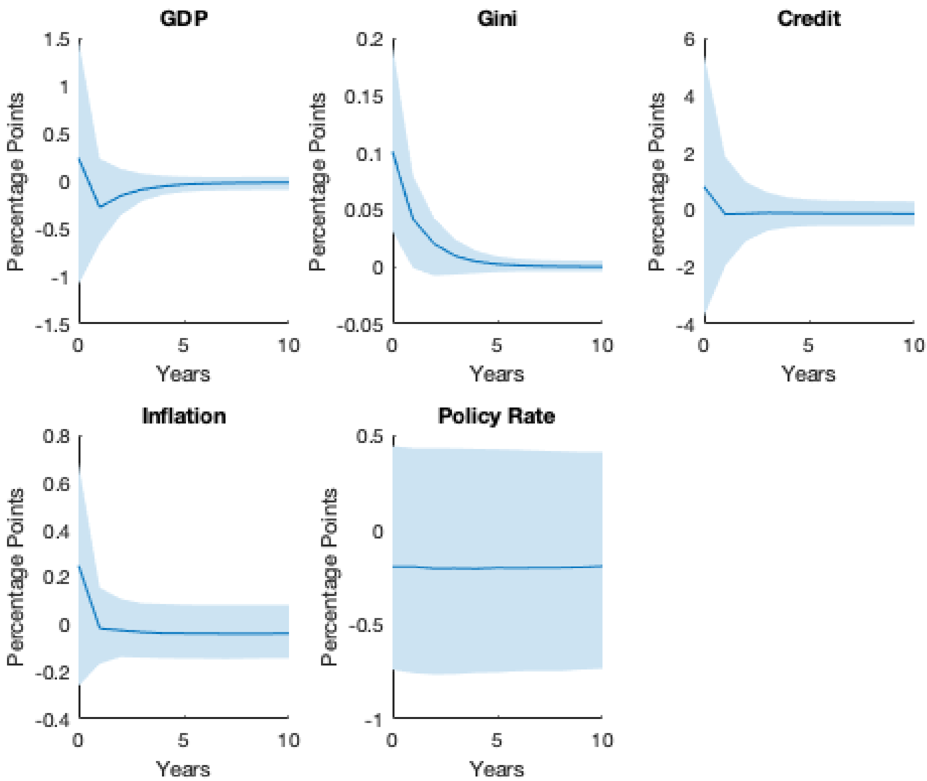
[1] Source: World Inequality Database, Author's Calculations.

Figure A.2: Full Identification Impulse Reponse Functions to a +100 Basis Point Monetary Policy Shock, Annual



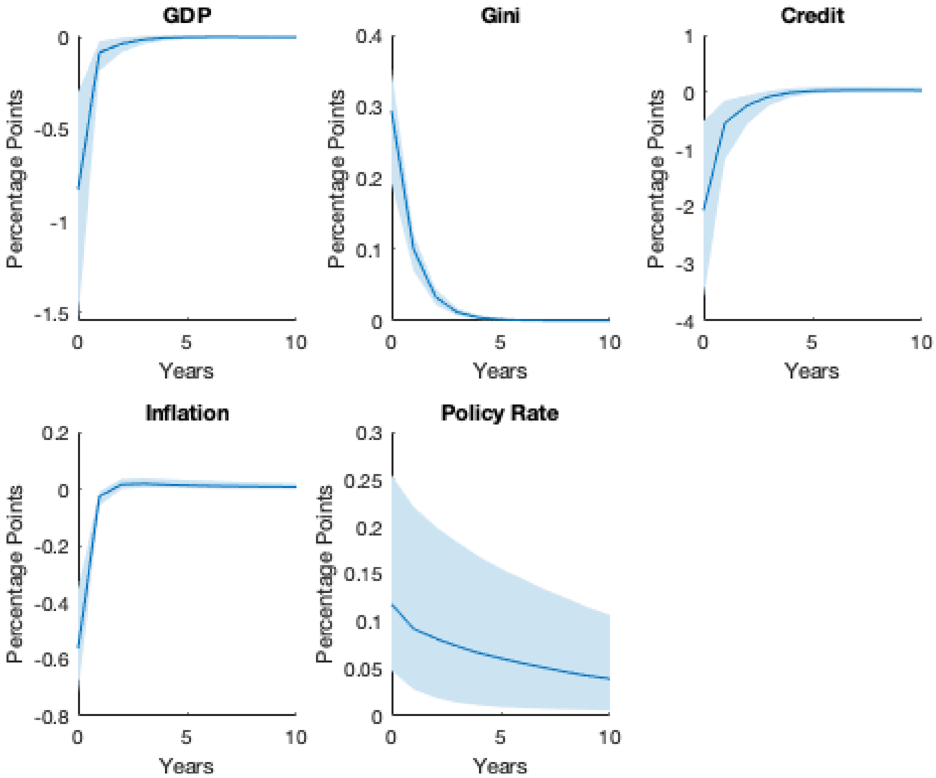
[1]Blue area is 68% confidence interval. N=462, lag=1. All variables except policy rate are in (log) differences.
[2] Source: Author's Calculations.

Figure A.3: Full Identification Impulse Reponse Functions to a +100 Basis Point Shock to the Gini Coefficient, Annual



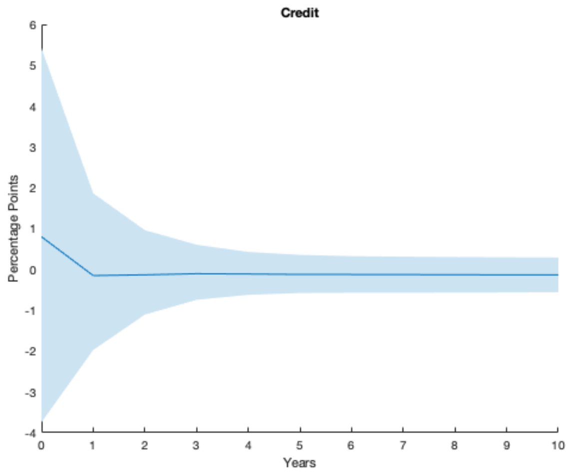
[1]Blue area is 68% confidence interval. N=462, lag=1. All variables except policy rate are in (log) differences.
[2] Source: Author's Calculations.

Figure A.4: Impulse Response Functions to a +100 Basis Point Monetary Policy Shock, Alternative Parametrization



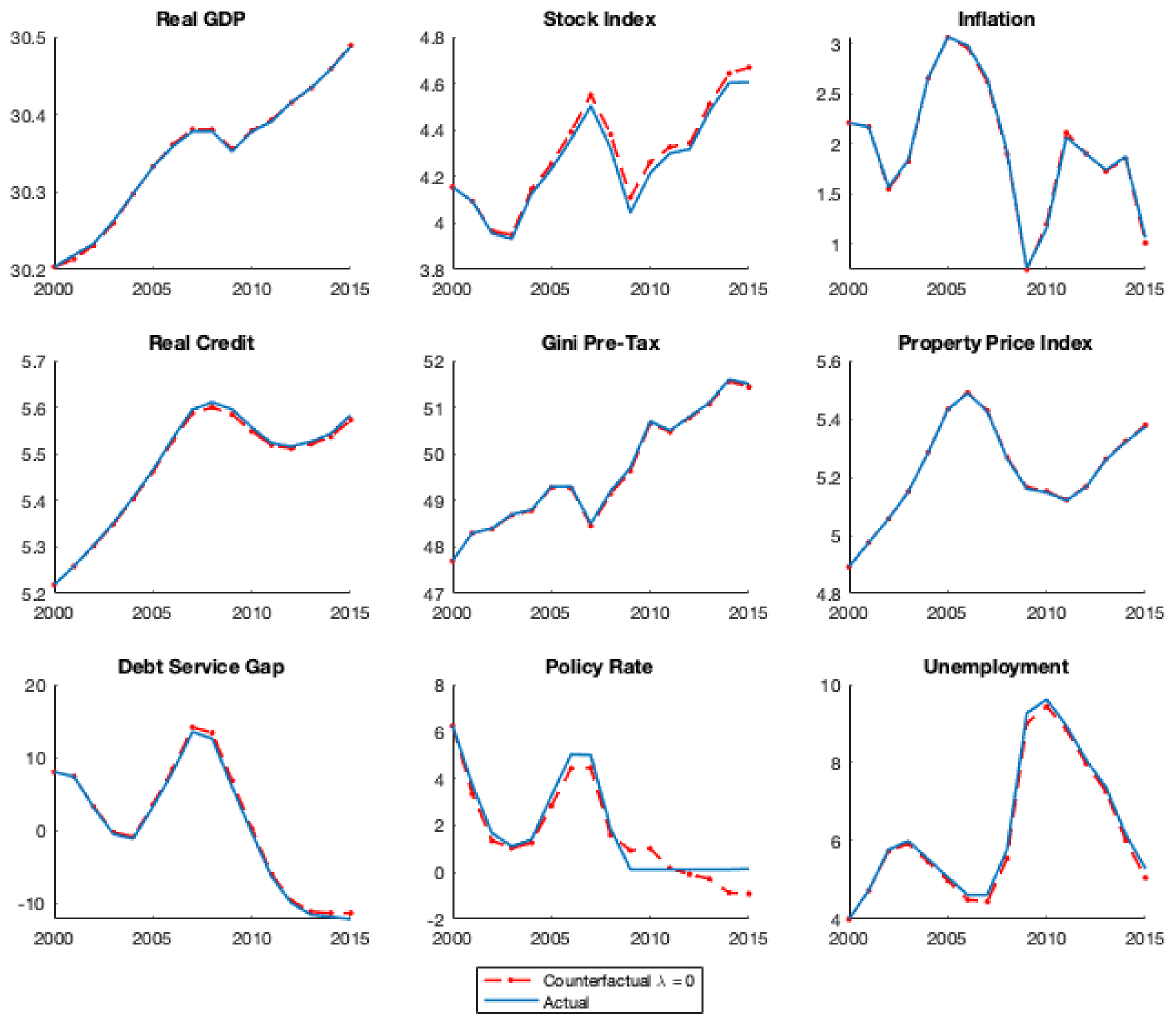
[1] Blue area is 68% confidence interval. N=462, lag=1. All variables except policy rate are in (log) differences.
[2] Alternative parameter setting: prior AR coefficient=0.5; overall tightness=0.05, lag decay=2.
[3] Source: Author's Calculations.

Figure A.5: Impulse Response Function of Credit for a +100 Basis Point Shock to the Differenced Gini Coefficient



[1]Blue area is 68% confidence interval. N=462, lag=1.
[2] Source: Author's Calculations.

Figure A.6: Counterfactual vs. Actual Evolution of Variables in Extended VAR with ZLB, No Leaning



[1] Counterfactual is Credit-Targeting Taylor Rule. N=256, lags=1.
[2] Source: Author's Calculations.

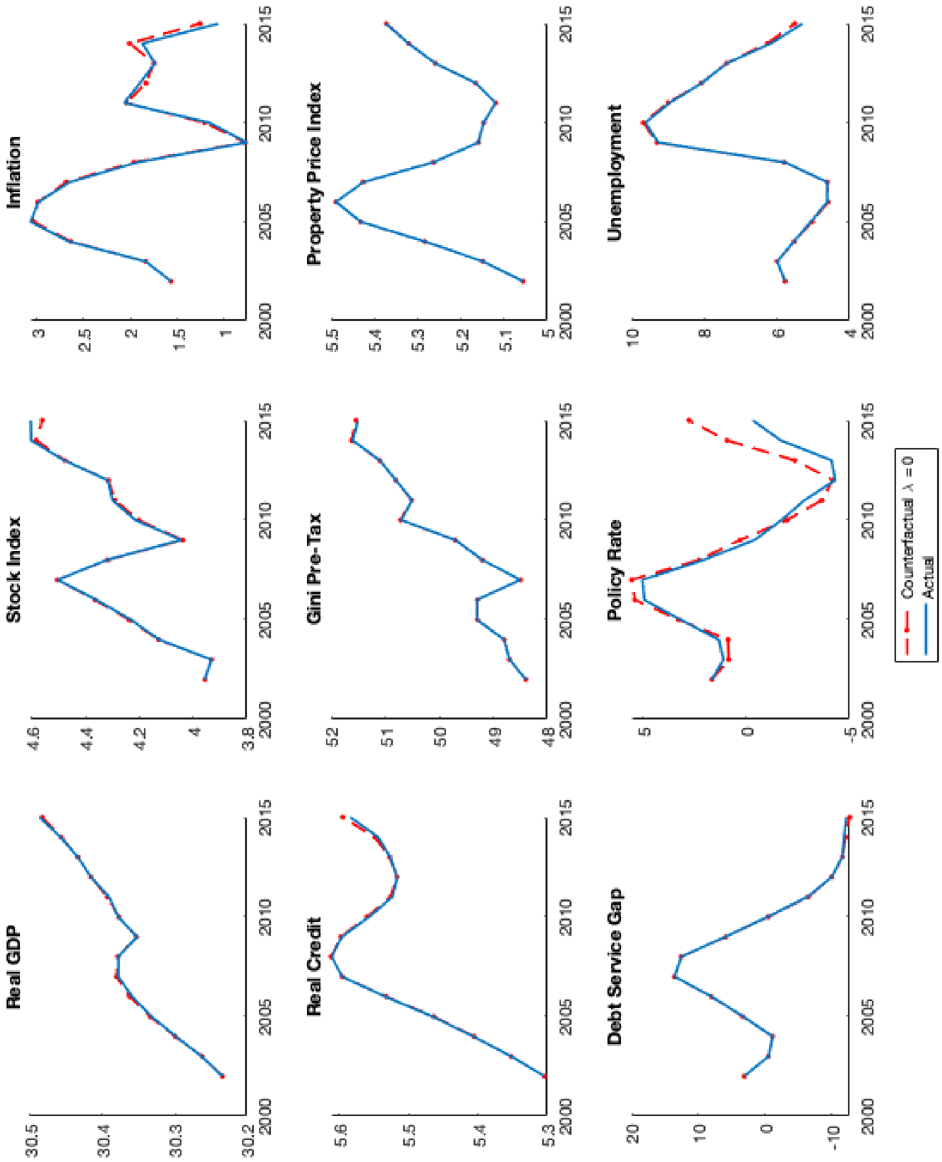
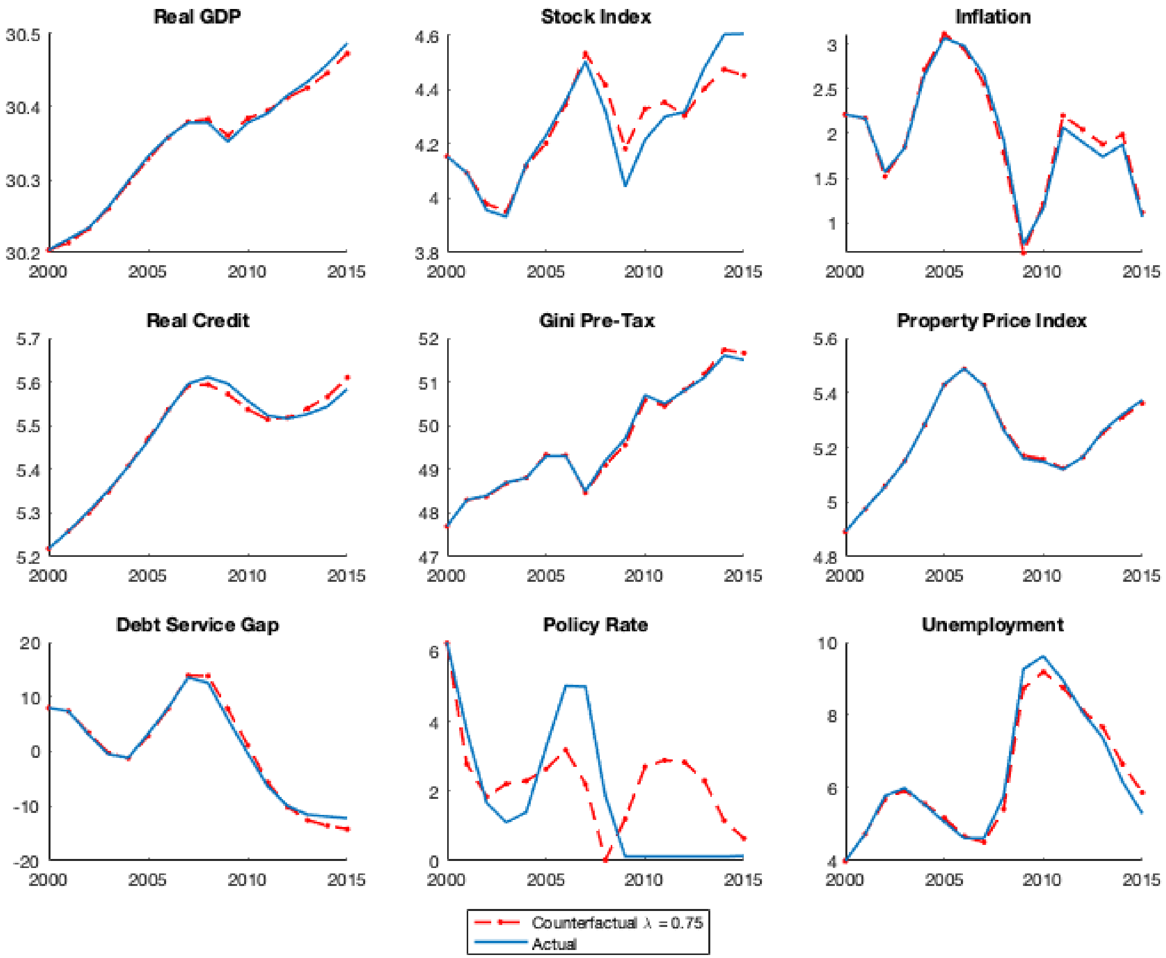


Figure A.7: Counterfactual vs. Actual Evolution of Variables in Extended VAR with SSR, No Leaning

[1] Counterfactual is Credit-Targeting Taylor Rule. N=256, lags=1.
[2] Source: Author's Calculations.

Figure A.8: Counterfactual vs. Actual Evolution of Variables in Extended VAR with SSR, Leaning



[1] Counterfactual is Credit-Targeting Taylor Rule. N=256, lags=1.
[2] Source: Author's Calculations.

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