Does Wealth Inequality Affect the Transmission of Monetary Policy?*

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Abstract

We provide evidence that higher wealth inequality between households is associated with stronger real effects of monetary policy. First, we use state-dependent local projections to show that the US and the UK exhibited stronger real effects of monetary policy in times of higher wealth inequality. Second, we measure wealth inequality within US states and document that economic activity responds more strongly to interest rate changes in states where wealth is distributed more unequally. Third, we show that ECB monetary policy has stronger real effects in Euro Area countries with higher wealth inequality.

Keywords: Wealth inequality, monetary policy, state dependence

JEL Codes: D31, E21, E22, E52

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1 Introduction

In the US, but also in other advanced economies, wealth inequality has risen considerably since the mid-1980s. In general, wealth inequality displays significant variation both over time and across space.[1] At the same time, recent advances in macroeconomics have led to a new discussion about the role of inequality for macroeconomic outcomes. In particular, several models predict that the wealth distribution is important for understanding the transmission of monetary policy.[2] We contribute to this debate by documenting empirically that wealth inequality affects the strength of the monetary transmission mechanism to the real economy. We provide three separate pieces of evidence, each showing that monetary policy has larger real effects when wealth inequality in the population is higher. To the best of our knowledge, we are the first to offer a detailed account of this relationship.

Macroeconomists have long neglected wealth inequality when studying monetary policy. This was largely due to the complexity and computational demands of structural models that feature a non-degenerate wealth distribution. However, quantitative research also supported the view that the distribution of wealth had little importance for macroeconomic dynamics (Krusell and Smith 1998). Recently, however, this view has been challenged, and the question of how "inequality matters for macro" (Ahn et al. 2018) has received new attention.

Several mechanisms have been proposed through which wealth inequality can affect the transmission of monetary policy. Luetticke (2021) finds that greater wealth inequality mutes the response of aggregate investment and amplifies that of consumption, with the overall effect on output approximately canceling out. The reason is that wealthy households with high marginal propensities to invest benefit from contractionary monetary policy, whereas incomes of asset-poor households with high marginal propensities to consume fall. Building on the observation that wealthier households are more likely to invest in stocks, Melcangi and Sterk (2020) argue that, when the rich hold a greater share of wealth, an interest rate cut leads to a stronger rebalancing towards equities. This results in a stock market and investment boom and hence an amplification of the output response. In Matusche and Wacks (2021) we obtain the same result in a model in which rich entrepreneurs invest strongly into their private firm in response to an interest rate decline due to a portfolio rebalancing motive. Here, we ask whether there is a state dependence of monetary policy transmission on wealth inequality in the data that would support the predictions of these studies.

We tackle the question of how wealth inequality alters the transmission of monetary policy in three ways. We first use separate aggregate time series data for the US and the UK, and show that the effects of monetary policy are state-dependent. To this end, we estimate state-dependent local projections, as suggested by Auerbach and Gorodnichenko

1. See, for instance, Hubmer et al. (2021) for the evolution of wealth inequality in the US over the past century, Piketty (2014) for a detailed account of worldwide inequality, and the graphs and statistics we provide in this study.

2. Examples are Gornemann et al. (2016), Kaplan et al. (2018), Auclert (2019), Luetticke (2021), Bilbiie (2020), and Werning (2015).

(2013), using the top 10% wealth share as a state variable. In the US and the UK, we find that in regimes of high inequality monetary policy had a larger impact on real activity than in regimes of low inequality. The differences are quantitatively important. In the US, while industrial production contracted by 2.1 percent in times of high wealth inequality in response to a 25 basis points (bp) increase in the federal funds rate, it shows no statistically significant contraction in times of low inequality. In the UK, responses of output and unemployment to monetary policy are smaller overall but, as in the US, they are relatively larger in times of high wealth inequality.

While the appeal of our first approach is its simplicity and the availability of high-quality data at the aggregate level, the drawback is that we cannot rule out that other variables that have co-moved with wealth inequality over time drive our results. Therefore, we turn to cross-sectional analyses in the second part of our study, which allows us to control for confounding factors. We use estimates provided by the Internal Revenue Service (IRS) on total wealth held by the richest households in each US state to construct measures of state-level wealth inequality. In line with the results on the aggregate level, we find that both output and unemployment in US states that display more wealth inequality react more strongly to common monetary policy shocks. In a third step, we construct measures of wealth inequality for Euro Area countries using data from the ECB's Household Finance and Consumption Survey (HFCS). We find that Euro Area countries with high levels of inequality react more strongly to common monetary policy shocks.

Based on the consistent findings in all three settings and after conducting various robustness checks, we conclude that higher wealth inequality is correlated with a stronger transmission of interest rate changes to the real economy. The strength of this correlation differs between the set-ups we study. Regarding the output response, we estimate the largest effect on the US aggregate level. Here, an increase in the top 10% share by one percentage point is associated with a 0.29 percentage points stronger average contraction in industrial production over the first three years after a 25 bp monetary policy shock. The effect is about 0.01 percentage points in the UK and about 0.05 in the cross section of Euro Area countries. Across US states, we find that a one percentage point increase in the top 1% share comes with an increase in the response of state personal income by 0.013 percentage points.[3] The effect of an increase in the top 10% wealth share by one percentage point on the average response of the unemployment rate ranges from 0.003 percentage points in the UK to 0.041 in the Euro Area. In the cross section of US states, an increase in the top 1% share by one percentage point raises the unemployment response by 0.005 percentage points. These numbers point to an economically meaningful effect of wealth inequality on the transmission of monetary policy to the real economy.

The reduced-form evidence that we provide in this study can inform and discipline the design of future structural models that analyze monetary policy in the framework of hetero-

3. At the US state level, due to data limitations, we only estimate the impact of the top 1% wealth share on monetary policy transmission, not of the top 10% wealth share as in the other sections of this study.

geneous agent models. It is also relevant for the current debate among policymakers about when and by how much to raise interest rates. Given the high levels of wealth inequality observed in many developed countries at the moment, our results indicate that a rate increase would have relatively strong effects on the real economy.

The remainder of our paper is structured as follows. Below, we review related literature. We study the state-dependent effects of monetary policy on the aggregate level in Section 2. In Section 3 we analyze the cross-section of US states, and in Section 4 we turn to a cross-section of Euro Area countries. In Section 5 we conclude.

Related Literature We follow a long tradition of studies that have analyzed the effects of monetary policy empirically using time series methods on an aggregate level (Ramey 2016; Christiano et al. 2005, 1999; Coibion 2012). In particular, in Section 2 we use state-dependent local projections to estimate the dependence of monetary policy transmission on the distribution of wealth. This approach has been advocated by Ramey and Zubairy (2018) and Auerbach and Gorodnichenko (2013), who ask whether government multipliers are larger or smaller during times of economic slack. In the context of monetary policy, state-dependent local projections have been used by Ascari and Haber (2022), Tenreyro and Thwaites (2016), and Alpanda and Zubairy (2019). Most closely related to our study is Alpanda and Zubairy (2019), who find that high levels of household debt mute the effects of interest rate changes. We corroborate this finding in Section 4, but show that wealth inequality influences the strength of monetary policy transmission even when controlling for the debt-to-GDP ratio.

Our analysis of US states in Section 3 is similar to Carlino and DeFina (1998, 1999) and Owyang and Wall (2009). Their findings suggest a role for the industrial composition in a state as well as the share of small firms in affecting the effectiveness of monetary policy. We add wealth inequality as an additional explanatory variable, which we construct based on estate tax returns.[4] Our analysis on Euro Area countries in Section 4 is related to Almgren et al. (forthcoming) who find that countries in which many households hold few liquid assets react more strongly to monetary policy. While we can validate their results in our set-up, we take a somewhat broader perspective by focusing on wealth inequality, and find a significant effect of wealth inequality even when controlling for other explanatory variables. Slacalek et al. (2020) also investigate the effects of monetary policy shocks in the Euro Area but have a different focus than we. They study only the four largest economies and decompose their responses to monetary policy shocks using structural models. In contrast, we look at all Euro Area countries and keep to a reduced-form analysis.

We are not aware of empirical studies that analyze the dependence of monetary policy transmission on wealth inequality, but several authors have investigated the reverse question, i.e., to what extent monetary policy affects inequality. For instance, Coibion et al. (2017)

4. Estate tax returns have been used before to construct measures of wealth inequality at the US aggregate level (Kopczuk and Saez 2004; Kopczuk 2015; Saez and Zucman 2016).

find that expansionary monetary policy tends to lower inequality in income and consumption, though they do not study wealth inequality as an outcome variable because of data limitations. Adam and Zhu (2016) draw similar conclusions using Euro Area survey data from the HFCS. In contrast, Andersen et al. (2021) who use administrative household-level data from Denmark, find that expansionary monetary policy increases inequality.

Lastly, our paper is related to a broader literature that investigates the role of inequality for the transmission of macroeconomic shocks. Brinca et al. (2016) ask how the size of fiscal multipliers depends on wealth inequality and find that higher inequality is associated with larger multipliers. Bahadir et al. (2020) document a positive relationship between income inequality and the response of aggregate consumption to credit shocks. Relatedly, Kohlscheen et al. (2021) find that consumption declines more strongly during recessions when income inequality is high.

2 Evidence from State-Dependent Local Projections

2.1 United States

We obtain measures of wealth inequality for the US from Piketty et al. (2018), made publicly available via the World Inequality Database (WID). Piketty et al. (2018) follow the approach in Saez and Zucman (2016) to estimate the wealth distribution. They define household wealth as the current market value of all assets owned by the household net of all its debt and estimate household wealth primarily by capitalizing income tax data.[5]

As our benchmark measure of inequality we use the wealth share held by the top 10% of the wealth distribution. The 90th percentile of the wealth distribution roughly separates the "wealth middle class" and the very wealthy and is therefore a statistic commonly used to assess the degree of wealth inequality in a given population (Alvaredo et al. 2018). Figure 1 shows the U-shaped evolution of the wealth share of the richest 10% over time. It peaked in 1962 at 71%, declined to 61% until 1986, and rose from then on, reaching 74% in 2012.

In this section we use time series data on US aggregate variables to assess how wealth inequality among households affects the transmission of monetary policy.[6] We estimate state-dependent local projections (Jordà 2005; Auerbach and Gorodnichenko 2013). For each horizon $h \in [0, 1, \ldots, H]$, we estimate the following model:

$$\begin{aligned} y_{t+h} = \alpha_{t,h} + \beta_h \cdot i_t + \beta_h^+ \cdot \Big(\text{ineq}_{t-1} \cdot i_t\Big) \\ + \sum_{p=1}^{P} \Gamma_{h,p} \cdot X_{t-p} + \sum_{p=1}^{P} \Gamma_{h,p}^+ \cdot \Big(\text{ineq}_{t-1} \cdot X_{t-p}\Big) + u_{t+h}, \end{aligned} \tag{1}$$

5. See Saez and Zucman (2016) for a detailed discussion of the definition of wealth used and the capitalization approach.

6. The aggregate time series data for the US and the UK are taken from the Federal Reserve Economic Database (FRED).

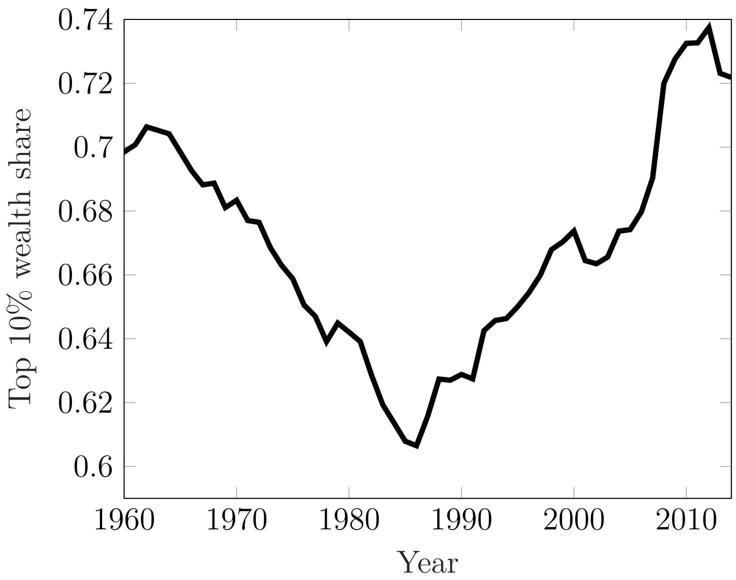

Figure 1: Top 10% wealth share in the US, data from the World Inequality Database.

where y_t is an outcome variable of interest (e.g., the log of industrial production), i_t is the federal funds rate, ineq_t is our measure of wealth inequality, X_t is a vector of control variables and u_t is an error term with $\mathbb{E}[u_t] = 0$.

The non-standard elements in equation (1) are the interaction terms between inequality and the interest rate, i_t, and between inequality and the controls, X_t. We introduce them to capture the potential dependence between wealth inequality and the effects of monetary policy on the real economy.[7] Conditional on a specific value of inequality, $\left(\beta_h + \beta_h^+ \cdot \text{ineq}_{t-1}\right)$ gives the response of y to a 100 basis points increase in the interest rate h months after it occurred. In times of low inequality, the response of y_t to a change in the federal funds rate is primarily governed by the parameters β_h. As inequality rises, the parameters β_h^+ become increasingly important in determining the response of y_t.

To make our results comparable to the empirical literature on monetary policy shocks, our baseline specification follows Ramey (2016). In particular, we use data at monthly frequency, $\alpha_{t,h}$ collects a constant and a linear time trend, we set $P = 2$, and we include as controls in X_t the log of industrial production, the unemployment rate, the log of the consumer price index and the commodity price index, as well as the federal funds rate. As our inequality measure is only available at annual frequency, we assign each month of a given year that year's observation. Since the top 10% share moves relatively slowly, other forms of interpolation lead to virtually the same results. We estimate impulse response functions (IRFs) up to a horizon of $H = 36$ months.

We instrument the federal funds rate with an exogenously identified monetary policy

7. Instead of assuming a simple linear interaction term we have also experimented with more complicated, so-called "transition functions" $F(\text{ineq}_t)$, that map the degree of inequality into a value between zero and one as in Auerbach and Gorodnichenko (2013). The results were very similar to the ones shown here, which is why we opt for the more parsimonious specification using the linear interaction term, i.e., we set $F(\text{ineq}_t) = \text{ineq}_t$.

shock series, i.e., we estimate IV local projections as proposed by Stock and Watson (2018). We employ the narratively identified monetary policy shock series from Romer and Romer (2004), which has been further extended by Coibion et al. (2017). The major advantage of this shock series for our purposes is that it begins as early as 1969m3 such that our sample encompasses two time periods of relatively high wealth inequality (the 1960s and the 2000s), and a period of relatively low inequality in the 1980s (see Figure 1). Our sample ends in 2007m12, right before the onset of the Great Financial Crisis.

Romer and Romer (2004) identify monetary policy shocks using narrative and quantitative records of meetings of the Federal Open Market Committee (FOMC) to derive a measure of intended changes in the federal funds rate around FOMC meetings. They then regress this measure on the Federal Reserve's internal forecasts of several aggregate variables. The residuals of this projection are then used as monetary policy shocks as they are relatively free of endogenous responses of the central bank to the state of the economy. As an instrument for the interaction term between inequality at $t-1$ and the interest rate at t we use the product of inequality at $t-1$ and the Romer & Romer shock at t.

Figure 2 shows the estimated response of industrial production to a 25 bp shock to the federal funds rate. IRFs for other variables can be found in Figure 12 in Appendix A.2. Focus first on the dashed black line in Figure 2. It depicts the unconditional response of industrial production to a monetary policy shock. We obtain it by estimating equation (1), but leaving out the interaction terms with inequality, i.e., we impose that there is no dependence on wealth inequality. We obtain the familiar result that a contractionary monetary policy shock depresses real activity in the economy, especially so at horizons between one and two years. The shape and the magnitude of the IRF are well in line with previous empirical results (Ramey 2016).

Next we allow the effects of monetary policy to be state-dependent, i.e., we reintroduce the interaction terms with inequality in equation (1). The blue line depicts the IRF when a monetary policy shock hits the economy during a regime of low inequality, the red line when it hits during a regime of high inequality. Low and high inequality refer to the first and third quartile of observed wealth inequality values in our sample (a top 10% wealth share of 62.9% and 67.0% respectively). The shaded areas indicate 90% confidence intervals constructed from Newey and West (1987) standard errors.

The core finding is that the IRFs in the two regimes are markedly different. On the one hand, the real effects of interest rate movements were strong during times of high inequality, leading to a contraction of industrial production by 2.1%. During times of low inequality, on the other hand, real activity was hardly affected by monetary policy. At short horizons we even obtain significant positive responses to a supposedly contractionary shock.[8] The IRF then approaches zero and later becomes negative, but it is never statistically significant. On

8. The positive response at short horizons owes to some extent to the fact that we do not make any recursivity assumption in equation (1). As we show in the robustness checks, imposing recursivity leads to negative or insignificant responses at short horizons even in a regime of low inequality.

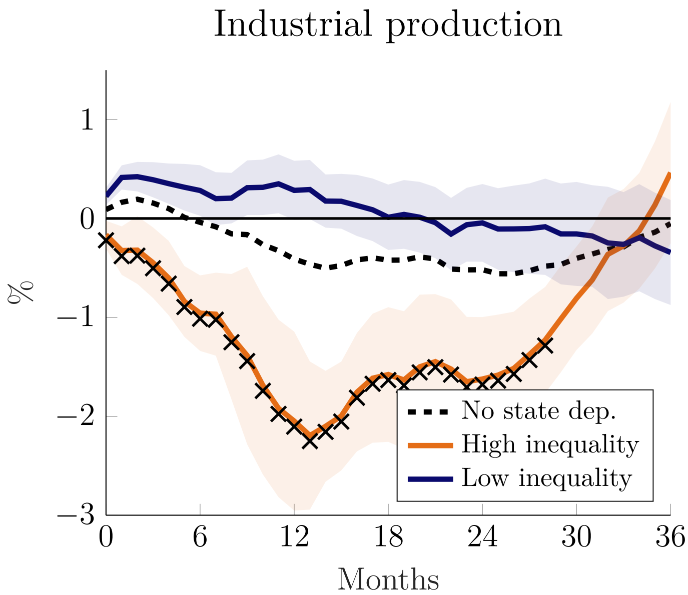

Figure 2: IRF of industrial production to 25 bp change in federal funds rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we can reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

average, the estimated response in the high inequality regime is 1.2 percentage points larger than in the low inequality regime over the first three years. Given the difference in the top 10% wealth share between the two regimes of 4.1 percentage points, an increase in this measure of inequality by one percentage point is associated with a 0.29 percentage points larger average contraction of industrial production in the first three years.

We can formally test whether the effects of monetary policy are independent of wealth inequality with a series of t-tests on the parameters β_h^+ for $h \in [0, 1, \ldots, H]$. At the horizons indicated by the black crosses in Figure 2, we can reject the null hypothesis that $\beta_h^+ = 0$ at a 90% confidence level. We can reject the hypothesis that there is no state dependence at all horizons during the first two years after the shock.

Figure 3 shows the reaction of the unemployment rate to a monetary policy shock. It mirrors the response of industrial production. While unemployment barely rises after the shock under low inequality, there is a pronounced spike in unemployment if the shock takes place in a regime of high wealth inequality. Moreover, we find statistically significant state dependence at several horizons in the first and second year after the shock. Using the same back-of-the-envelope calculation as before, our results indicate that the unemployment response becomes on average 0.025 percentage points larger when the top 10% wealth share grows by one percentage point.

The relationship between the two regimes switches at very long horizons as was the case for industrial production. One potential explanation for this pattern is that at late horizons the local projections pick up the beginning of the next phase of the business cycle. This would account for a more pronounced boom at large horizons under high inequality after

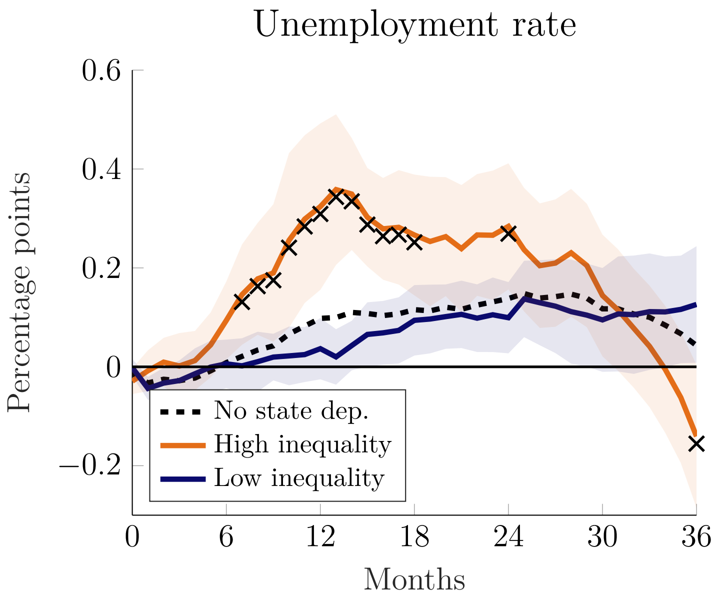

Figure 3: IRF of the unemployment rate to 25 bp change in federal funds rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

the shock caused a more pronounced recession at shorter horizons.

Could it be that the endogenous reaction of the monetary authority is driving the differential results? For instance, the Federal Reserve might lower its rate faster in times of low inequality in response to a contractionary shock, thereby stimulating real activity. Figure 4 shows that this is not the case. It depicts the IRF of the federal funds rate. Over the first six months the responses are not statistically different from each other in the two regimes. At longer horizons, the reaction of the Federal Reserve becomes more accommodating in a regime of high inequality.

Endogeneity of wealth inequality and consistency of IV LP estimator Several recent studies have provided both theoretical arguments and empirical evidence for a causal effect of monetary policy on inequality (Bayer et al. 2022; Adam and Zhu 2016; Areosa and Areosa 2016; Coibion et al. 2017; Doepke and Schneider 2006; Gornemann et al. 2016). Whether an endogenous response of the state variable (wealth inequality in our case) poses a problem for the state-dependent local projections approach is an open question.

When estimating IRFs to a monetary policy shock using local projections for a given value of inequality at the time of the shock, one does not have to assume a path of inequality over the considered horizon. The idea is that the local projection estimator at horizon h gives the response of y_{t+h} to a monetary policy shock in t for an exogenous level of inequality in $t-1$ but taking into account the endogenous response of inequality between t and $t+h$. In contrast, to obtain state-dependent IRFs from Smooth Transition Vector Autoregressions (STVAR), one has to make an assumption about the evolution of inequality over the entire horizon

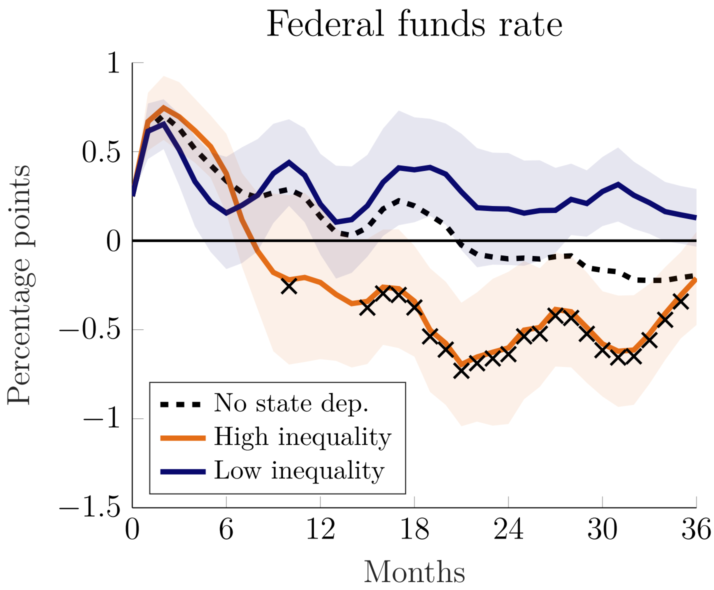

Figure 4: IRF of federal funds rate to 25 bp change in federal funds rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

of the IRF. That state-dependent local projections account for the endogenous response of the state variable has been viewed as an advantage over other methods such as STVARs (Auerbach and Gorodnichenko 2013).

However, Gonçalves et al. (2022) call into question whether state-dependent local projections provide a consistent estimator for the IRF when the state variable is endogenous. They show that exogeneity of the state variable is sufficient for consistency of the state-dependent local projections estimator. While this does not imply that state-dependent local projections are inconsistent when the state variable is endogenous, their numerical simulations suggest that the estimated IRFs are biased in this case. In Appendix A.1 we provide a formal discussion of our approach which combines the state-dependent local projections estimator studied in Gonçalves et al. (2022) with the IV approach analyzed by Stock and Watson (2018). In line with the results of Gonçalves et al. (2022), our estimator is consistent if inequality is exogenous and if the Romer and Romer shocks are valid instruments for the nominal interest rate. If, however, inequality responds endogenously to monetary policy, our estimator of the IRF is not guaranteed to be consistent.

Yet, we do not think that this is a major concern for our findings. It is apparent from Figure 1 that most of the variation in wealth inequality in our sample is of relatively low frequency. For this reason, the endogenous response of inequality to monetary policy in the short run is small relative to its trend.[9] Hence, the potential bias coming from the endogeneous response of the state variable is less concerning in our case. This sets our study apart from the most prominent applications of state-dependent local projections, where the

9. See also Figure 18 in the appendix for the IRF of the top 10% wealth share to a monetary policy shock.

state variable is the state of the business cycle. In this case, monetary or fiscal policies have a potentially strong effect on the state variable. In fact, the state variable is often directly related to the outcome variable of interest, e.g., when the state variable is a recession indicator and the outcome variable is GDP. The concerns about inconsistency of the state-dependent local projections estimator expressed in Gonçalves et al. (2022) are likely more relevant in these cases.

In Appendix A.1 we provide additional robustness checks in which we address the potential endogeneity by lagging inequality in the model by H periods and by including future values of wealth inequality as controls. Also, to sidestep this potential issue of the local projections approach, we estimate state-dependent IRFs using recursively identified STVARs (see Appendix A.2). The results are similar to those obtained using the local projections approach.

Robustness We assess the robustness of our baseline results in several ways. For brevity, we show the corresponding IRFs of industrial production in Appendix A.2. First, we use different measures of inequality, namely the Gini coefficient and the top 1% wealth share. While the results using the Gini index are nearly identical to the baseline results (Figure 13), when we use the top 1% share (Figure 14) the absolute differences between the two regimes' IRFs become even larger.

Second, traditional identification schemes for monetary policy shocks based on timing restrictions have assumed that industrial production, unemployment, and the price level do not respond contemporaneously to changes in the federal funds rate (see, for instance, Christiano et al. 1999). We can impose this recursivity by including contemporaneous values of log industrial production, unemployment, and the log of the price indices in equation (1) (Ramey 2016). As Figure 15 shows, assuming recursivity dampens the positive response of industrial production at short horizons, but it remains the case that the response is stronger when inequality is high.

Third, Coibion (2012) argues that IRFs to monetary policy shocks derived from Romer & Romer shocks are sensitive to the inclusion of the early 1980s. During this time period the Federal Reserve targeted non-borrowed reserves and the federal funds rate was a less suitable measure of the monetary policy stance. We check the robustness of our results by excluding this time period, i.e., we use only the years 1984 to 2007 in our sample. As Figure 16 shows, our results qualitatively still hold for this subperiod, even though the dependence on wealth inequality is statistically significant at fewer horizons.

Lastly, we also consider the Chicago Fed National Activity Index (CFNAI) instead of log industrial production as the measure of real economic activity. The results are shown in Figure 17. While the results are not as stark as for industrial production, there are still several horizons in the first year at which the response is significantly stronger during times of high inequality. At very long horizons, however, the relationship reverses, which to some extent was also the case when using industrial production as the measure of economic

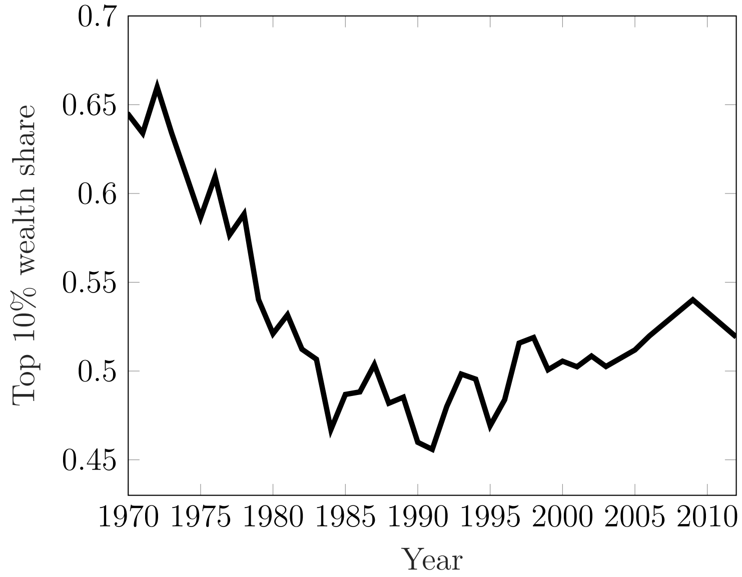

Figure 5: Top 10% wealth share in the UK, data from the World Inequality Database.

activity.

2.2 United Kingdom

In this section, we show that the positive relationship between wealth inequality and the real effects of monetary policy also holds in the UK. The WID provides the data from Alvaredo et al. (2016) on the top 10% wealth share in the UK. We plot it in Figure 5. As in the US, the top 10% wealth share over time exhibits a u-shape, but the rise in inequality since the mid 1980s was much smaller in the UK than in the US. As an instrument for the monetary policy rate set by the Bank of England (BOE) we use the narratively identified shock series by Cloyne and Hürtgen (2016). They adopt the Romer and Romer (2004) methodology to identify monetary policy surprises in the UK between 1975m1 and 2007m12.

Figures 6 and 7 show the estimated IRFs for industrial production and unemployment. Unconditionally, i.e., without the interaction terms with inequality, a contractionary monetary policy shock lowers industrial production and raises unemployment one to two years after its occurrence. While the dynamics are similar to those in the US, the magnitude of the responses is smaller in the UK, but well in line with those depicted in Cloyne and Hürtgen (2016).

Turning to the state dependence of monetary policy transmission, we obtain qualitatively similar results as for the US. At most horizons, changes in the interest rate tend to have more pronounced effects on real activity in times of high inequality, although the relationship is reversed at a few points during the first year. The absolute differences between high- and low-inequality responses, however, are a bit smaller in the UK. We plot the IRFs conditional on the minimum and the maximum observed level of inequality in our sample (a top 10% share of 45.6% and 61.0% respectively). An interest rate change that occurs in the so defined

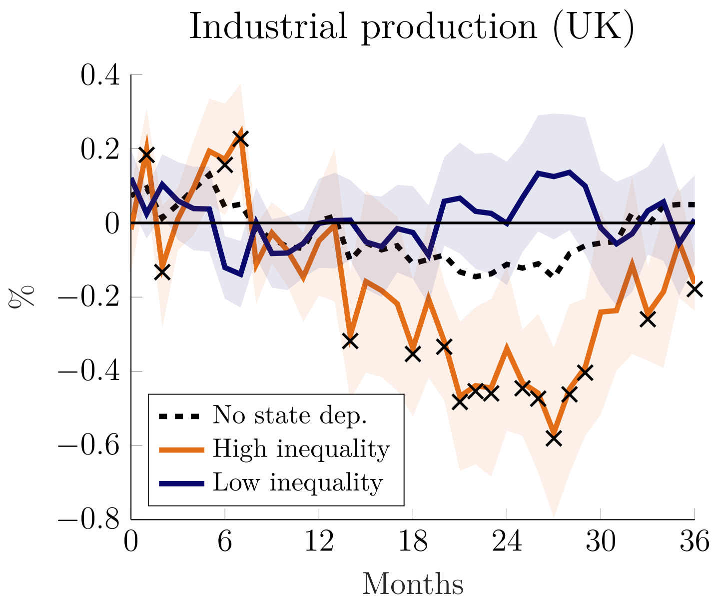

Figure 6: IRF of UK industrial production to 25 bp change in BOE policy rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (minimum of observed inequality). Red: Regime of high inequality (maximum). Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

high-inequality regime lowers industrial production by up to 0.57% and raises unemployment by 0.13 percentage points. In contrast, under low inequality neither industrial production nor unemployment show pronounced deviations from zero at the considered horizons. In sum, an increase in the top 10% wealth share by one percentage point is associated with a 0.012 percentage points larger fall of industrial production on average over the first three years after the shock, and a 0.003 percentage points larger rise in unemployment.

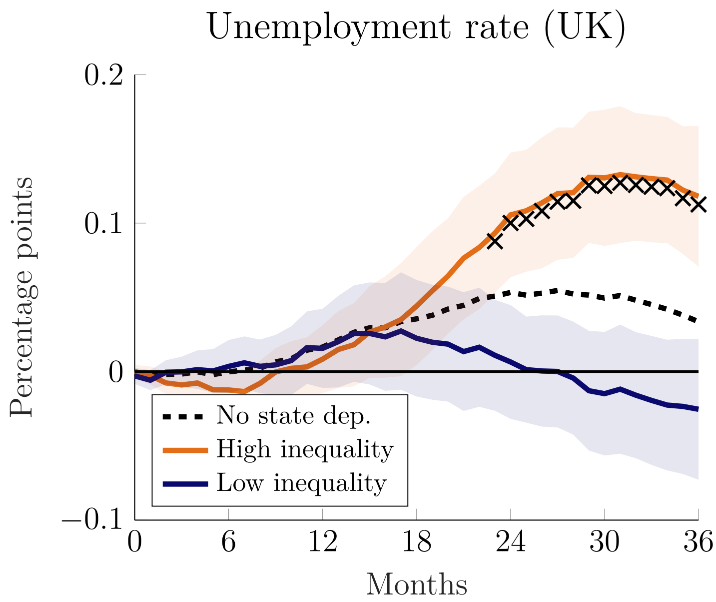

Figure 7: IRF of UK unemployment to 25 bp change in BOE policy rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (minimum of observed inequality). Red: Regime of high inequality (maximum). Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

3 Cross-Sectional Evidence from US States

In this section, we document that state personal income and unemployment respond more strongly to interest rate changes in US states with higher wealth inequality. To this end, we estimate the effect of interest rate changes on personal income and unemployment by state and then regress the estimated effects on states' level of wealth inequality. The advantage of this cross-sectional strategy over the time series approach in the previous section is that it allows us to control for confounding factors. Any variable that evolved in the same u-shape as the top 10% wealth share over the last decades could be responsible for our findings of state dependence in the previous section. In the following, we rule out many alternative explanations by controlling for these variables. The second reason for taking a cross-sectional approach is that the series of wealth inequality by US states that we construct in the next subsection has a low frequency. Between 1969 (when the Romer & Romer shock series begins) and 1986, we observe inequality only twice for every state, and afterwards tri-annually. This precludes a time series analysis as we performed it on the national level where we observed inequality annually. Before we describe the empirical strategy in greater detail, we discuss the construction of the series of wealth inequality by state in the next subsection.

3.1 A measure of wealth inequality at the US state level

To measure wealth inequality at the state level we resort to publicly available data on top wealth holders derived from estate tax returns. The federal estate tax was introduced in 1916 and is a tax on the transfer of wealth from the estate of a deceased person to its beneficiaries. A tax return must be filed by every deceased US citizen whose gross estate, valued on the date of death, equals or exceeds a certain exemption level, which has varied over time (see Jacobson et al. (2007) for a historical overview and detailed description of the federal estate tax). Data on federal estate tax returns have previously been used for research on wealth inequality (Kopczuk and Saez 2004; Kopczuk 2015; Saez and Zucman 2016), but to the best of our knowledge not for a state-level analysis.

Based on estate tax returns, the Internal Revenue Service (IRS) periodically publishes estimates on the total wealth held by so-called top wealth holders. Top wealth holders are defined as those currently alive US citizens for whom an estate tax return would be required upon death given the current exemption level regarding estate taxes. To arrive at the estimates of total wealth held by this group of individuals the IRS uses the estate multiplier technique. In short, every observed estate tax return is inflated by the inverse probability of that person dying in a given year. Therefore, old people (with relatively high probabilities of dying) receive low weights and young people receive high weights. This way, the IRS arrives at an estimate of the total wealth held by the wealthiest part of the population (see Kopczuk and Saez (2004) and Jacobson et al. (2007) for a detailed description of the methodology).

The IRS provides estimates on the total wealth held by top wealth holders for each US state in its Statistics of Income (SOI) Bulletins for the years 1976, 1982, and then every three years from 1986 onward. Given the total wealth held by top wealth holders and the number of top wealth holders, we construct the share of wealth held by the richest 1% of citizens for each state and year that is available. Appendix A.3 lays out the details of this procedure. The key assumption that we make to arrive at our estimates is that at any given point in time and in each US state, the right tail of the wealth distribution follows a Pareto distribution (Hubmer et al. 2021; Kopczuk and Saez 2004; Benhabib et al. 2015). We construct top 1% shares here, instead of top 10% shares, since the exemption level for filing estate taxes has risen significantly over the recent decades and often times only covers a fraction of the wealthiest 1% of citizens. Extrapolating from this information to the wealth share of the top 10% is therefore difficult, and studies that use estate tax income for constructing inequality measures typically only report wealth shares of the top 1% or of even smaller fractions.

3.2 Empirical strategy

The approach we take in this section is to exploit cross-sectional variation between US states, similar to Carlino and DeFina (1998, 1999). We estimate the effects of an interest rate change for every state and then regress this on the top 1% wealth share by state which we constructed above. To that end, for each state s, we first estimate the IRF of a measure of economic activity to a change in the interest rate by the Fed. Similarly to the previous section we estimate IV local projections for horizons $h \in [0, 1, \ldots, H]$, but this time for each

US state s:

$$y^s_{t+h} = \alpha^s_h + \beta^s_h \cdot i_t + \sum_{p=1}^{P} \Gamma^s_{h,p} \cdot X^s_{t-p} + u^s_{t+h} \tag{2}$$

The only difference to Section 2 is that we do not include interaction effects in the regression and that the outcome variable is specific to the state s. The remaining control variables, the deterministic elements, as well as the number of included lags ($P = 2$) remain the same. Hence, X^s_t includes y^s_t, the aggregate unemployment rate, the federal funds rate, and the log of the two aggregate price indices.[10] As before, we instrument the federal funds rate i_t with the Romer & Romer shock series.

As a proxy for economic activity, we use two different outcome variables y^s_t at the state level, the log of real state personal income (as in Carlino and DeFina 1998, 1999) and the state-level unemployment rate.[11] For the IRF of unemployment we use monthly data and consider a horizon of $H = 36$ months as in the previous section. Since state personal income is only observed quarterly, we estimate its IRF from quarterly data for horizons up to $H = 12$ quarters. We measure the impact of interest rate changes on economic activity as the average of the IRF over the first three years after the change in the interest rate, i.e., we compute the cumulative response and then divide by $H + 1$. In the following, we refer to this statistic as "impact measure".

For our baseline analysis, we restrict our sample to start in 1984q1. We choose this starting point for two reasons. First, as highlighted by Coibion (2012) and as explained in Section 2, the Federal Reserve targeted non-borrowed reserves instead of the federal funds rate in the early 1980s, which introduces some noise into the estimation of IRFs to changes in the interest rate. Owyang and Wall (2009) furthermore find that the regional effects of monetary policy in the US in the Volcker-Greenspan era post 1983 differed significantly from its effects in earlier episodes. Second, the IRS consistently published estimates on the top wealth holders on a triennial basis only from 1986 onward. We could use a longer sample to estimate the impulse responses, 1969q1 to 2007q4 for state personal income and 1976m1 to 2007m12 for state unemployment, as observations on unemployment in the states are not available for earlier periods. But in this case, we would use data from a long period, 1969 or 1976 to 1985, for estimating the state-level IRFs during which we observe wealth inequality only twice. When starting in 1984, we instead observe inequality at a constant frequency of three years. As a robustness check, we also estimate IRFs using all available data. We get similar results, which we discuss at the end of this section.

We regress our impact measure of monetary policy on the average top 1% wealth share in the sample period. In these regressions we can control for other variables that might affect the responses of economic activity to monetary policy and that could be correlated with wealth inequality. In particular, Carlino and DeFina (1998, 1999) find that a high

10. Including the logged sum of all remaining states' personal income as an additional control barely changes the results.

11. We obtain all these series from FRED.

Table 1: Summary statistics for variables on the US state level, 1984q1 to 2007q4.

	Mean	S.d.	Min	Max
Avgerage response personal income (in %)	-0.16	0.20	-0.56	0.21
Avgerage response unemployment (in pp.)	0.08	0.08	-0.08	0.28
Top 1% wealth share	0.25	0.05	0.13	0.36
Manufacturing share	0.16	0.06	0.03	0.30
Share small firms	0.49	0.06	0.41	0.67
Share middle-aged	0.51	0.02	0.46	0.56
Share old	0.17	0.03	0.07	0.24
Observations	50			

Notes: S.d. stands for standard deviation.

share of income earned in the manufacturing sector of a state leads to larger responses to monetary policy shocks. They hypothesize that manufacturing is an interest-sensitive sector as purchases of housing, cars and other durable manufactured goods are relatively responsive to changes in the interest rate. Furthermore, in a subset of their regressions they find that the higher the percentage of small firms in a state the stronger the effects of monetary policy. Their proposed explanation is that small firms are more reliant on funding through banks and therefore more exposed to changes in the interest rate than large firms. Lastly, Leahy and Thapar (2019) find that US states with a high share of middle-aged people in the population react more strongly to monetary policy. The explanation they offer is that medium-aged people are relatively likely to be entrepreneurs and that therefore private firm investment becomes more responsive in states where the share of this demographic group is large.

We control for these effects by including the share of state income that is earned in the manufacturing industries, the share of employees who work in firms with less than 250 workers, the share of the population aged 35–65, and the share older than 65 years in a subset of our regressions. For the manufacturing share we divide earnings in the manufacturing industries by total earnings in a state, both provided by the Bureau of Economic Analysis at quarterly frequency. Employment shares are computed on the basis of the Business Dynamics Statistics provided by the U.S. Census Bureau on an annual basis from 1977 onward. Demographic groups are computed using annual data from the U.S. Census Bureau. For all variables we take the average over the sample period. Table 1 displays summary statistics of the variables we use.

3.3 Results

The first four columns of Table 2 show the results from regressions of the average IRF of state personal income on the average top 1% wealth share. In the first column we only use wealth inequality as an explanatory variable and then add explanatory variables in columns

Table 2: Regression results for the cross-section of US states.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Log state personal income				State unemployment rate			
Top 1% share	-1.3***	-1.4***	-1.3***	-1.1**	0.5**	0.6***	0.5*	0.4*
	(0.4)	(0.4)	(0.5)	(0.5)	(0.2)	(0.2)	(0.3)	(0.2)
Manuf. share		-1.1***	-1.1**	-0.7		0.4*	0.3	0.2
		(0.4)	(0.5)	(0.5)		(0.2)	(0.2)	(0.2)
Share small firms			0.2	0.9*			-0.2	-0.4*
			(0.5)	(0.5)			(0.3)	(0.2)
Share mid-aged				-6.0***				2.1***
				(1.4)				(0.7)
Share old				-1.8**				0.3
				(0.9)				(0.4)
Observations	50	50	50	50	50	50	50	50

Notes: Dependent variables are the average IRF of log real state personal income (columns 1–4) and the state unemployment rate (columns 5–8) to a 25 bp increase in the federal funds rate over a horizon of three years. Sample period is 1984q1–2007q4. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2 to 4. In line with the results in the previous section a negative relationship between the average response and wealth inequality emerges. The point estimate in column 1 indicates that an increase in the top 1% wealth share by one standard deviation (5 percentage points) exacerbates the contraction of real state personal income by $0.05 \cdot 1.3 = 0.065$ percentage points on average over a horizon of three years. Figure 8 shows the corresponding scatter plot of the states' average top 1% shares and their cumulative IRFs. In line with Carlino and DeFina (1998, 1999) we find a negative effect of a high manufacturing share, while in contrast to them, we find that a high share of employment in small firms mutes the output response. A large fraction of middle-aged (and to a lesser extent of old-aged) leads to stronger responses to interest rate changes, as in Leahy and Thapar (2019).

Columns 5 to 8 of Table 2 show the results when repeating the analysis for the unemployment rate. In line with our previous findings, we estimate a positive coefficient on the top 1% wealth share, implying a stronger rise in unemployment in states where wealth is distributed more unequally. Column 5 implies that the average rise in unemployment becomes $0.05 \cdot 0.5 = 0.025$ percentage points larger when a state's top 1% wealth share increases by one standard deviation. For comparison, both the mean and the standard deviation of the average unemployment response amount to 0.08 percentage points (see Table 1).

3.4 Robustness

First, we use the longest possible sample periods for estimating the impulse response, from 1969q1 to 2007q4 for state personal income and from 1976m1 to 2007m12 for the unemployment rate. Table 10 in the appendix shows the results. While the point estimates of the coefficient on the top 1% wealth share all have the expected sign, they are somewhat

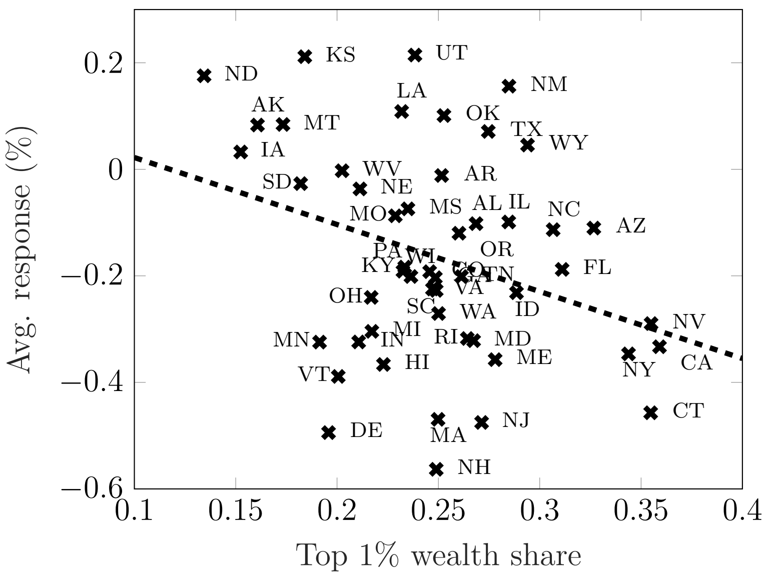

Figure 8: Top 1% wealth shares and average IRFs of personal income in US states.
Notes: Sample period is 1984q1–2007q4. Top 1% shares are the average over the sample period. Average IRFs are taken over the first three years after the shock.

smaller and are statistically significant only in some cases. One possible explanation for this is that time-varying inequality on the state level confounds the results. As was the case for the US as a whole, wealth inequality for each state is not constant but varies over time. Since we have few observations of wealth inequality before 1984, taking the average over all observations does not assign the adequate weight to the time before 1984. For example, a state with high wealth inequality before 1984 and low wealth inequality after 1984 would be assigned a lower level of average wealth inequality than a state with low wealth inequality before 1984 and high wealth inequality after 1984. This could bias our results.

Second, we use the State Coincident Index constructed by the Federal Reserve Bank of Philadelphia as our measure of activity on the state level instead of state personal income or unemployment. The Coincident Index is available at monthly frequency from 1979m1 onward and is computed on the basis of four variables, non-farm payroll employment, average hours worked in manufacturing by production workers, the unemployment rate, and CPI-deflated wage and salary payments. The results, using the average IRF over the first $H = 36$ months after the shock as the impact measure, are shown in Table 11 in the appendix. They are very similar to those obtained when using state personal income as the measure of activity (Table 2).

Third, we measure the impact of monetary policy at the state level not by the average but by the peak response of state personal income. Table 12 in the appendix shows the results. Again, we find that responses are more pronounced in states with high inequality.

Lastly, we exploit the length of the time series and construct a two-period panel of state-level observations. We split the full sample into two subsamples of equal length, 1969q1–1988q2 and 1988q3–2007q4, and for every state estimate both the average top 1% wealth share and IRFs to monetary policy shocks separately for the two subsamples. We use the resulting panel of the average top 1% wealth share and the impact measure of monetary policy to estimate a model with state fixed effects. This allows us to control for all time-invariant characteristics of a state that might affect its response to interest rate changes. Table 13 in the appendix displays the resulting estimates when using the average IRF of state personal income as our impact measure. Even when controlling for state fixed effects a negative point estimate on the top 1% share emerges, though it is not statistically significant (columns 3–6). The first two columns also display the estimated coefficient of the top 1% share for each of the two subsamples. Both are negative and statistically significant. This finding is not surprising for the second subsample (1988–2007) as it almost coincides with the subsample used in our baseline specification, 1984 to 2007. However, this finding also indicates a dependence of monetary policy transmission on wealth inequality in US states before 1988.

4 Cross-Sectional Evidence from the Euro Area

Next, we analyze the correlation between wealth inequality and the real effects of monetary policy in the cross-section of Euro Area countries. Our empirical approach is similar to the one for US states in Section 3 and to the analysis in Almgren et al. (forthcoming). The additional advantage of using Euro Area data compared to the US state-level analysis is the availability of a microdata set, the HFCS, that has detailed information on households' asset holdings and is representative at the country level. Hence, we can control for a host of potential confounding factors that may be correlated with wealth inequality, such as the share of hand-to-mouth households and the rate of homeownership.

4.1 The effects of monetary policy

We first estimate the real effects of monetary policy for all nineteen Euro Area countries using IV local projections as before. We use monthly Eurostat data from 1999m1, when the Euro was introduced, to 2020m1. For every country c and horizon $h \in [0, 1, \ldots, H]$, we estimate

$$y^c_{t+h} = \alpha^c_h + \beta^c_h \cdot i_t + \sum_{p=1}^{P} \Gamma^c_{h,p} \cdot X^c_{t-p} + u^c_{t+h} \; . \tag{3}$$

As before, y^c_{t+h} is the outcome variable of interest in country c at time $t+h$, i_t is the nominal interest rate and X^c_t is a vector of controls. It contains country c's unemployment rate, the log of its GDP, the log of its CPI, the identified monetary policy shocks described below, and the nominal interest rate for which we take the Euro Overnight Index Average (EONIA) rate. As Almgren et al. (forthcoming), we include the shocks as controls to address the potential problem of serial correlation in the series of shocks. As before, we estimate IRFs up to a horizon of $H = 36$. Following Almgren et al. (forthcoming), we choose a lag length of $P = 3$ and include a country- and horizon-specific constant, α^c_h. We estimate the local projections at a monthly frequency. However, GDP for the Euro Area countries is only available at a quarterly frequency. To obtain a monthly series, we follow Almgren et al. (forthcoming) and employ the Chow and Lin (1971) procedure using data on unemployment, industrial production and retail trade to interpolate quarterly GDP.

We instrument the nominal interest rate using shocks identified from high-frequency movements in Overnight Indexed Swaps (OIS). In particular, we construct a monthly shock series from changes in the 3-months OIS rates around ECB monetary policy announcements, which are provided by Altavilla et al. (2019). Since ECB monetary policy decision are announced at 13:45 followed by a press conference that ends at 15:30, Altavilla et al. (2019) compute the difference in the median price of the OIS in the time between 15:40 to 15:50 and the median price between 13:25 to 13:35. Under the identifying assumption that policy decisions are the only relevant factor driving interest rates in this short time window, these changes constitute exogenous variations in nominal interest rates and are therefore valid instruments.

Using this procedure, Altavilla et al. (2019) construct a daily series of monetary policy shocks that we aggregate to monthly frequency. To do this, we follow the procedure employed in Ottonello and Winberry (2020) and Meier and Reinelt (2020) which accounts for the fact that for a shock that takes place late in a month, little time is left to affect the economy. For example, a shock on the last day of a month is much closer in time to a shock that takes place on the first day of the next month than it is to a shock on the first day of the same month, and we would expect to see this reflected in how the economy responds to the shock. To take this into account, we attribute each shock in part to the next month depending on how many days are left in the month after the day of the shock. In particular, the value of the series in month t, denoted ϵ_t, is constructed according to

$$\epsilon_t = \sum_{\tau \in D(t)} \phi(\tau, t)\tilde{\epsilon}_\tau + \sum_{\tau \in D(t-1)} (1 - \phi(\tau, t-1))\tilde{\epsilon}_\tau,$$

where $\tilde{\epsilon}_\tau$ is the value of the daily series on day τ, $D(t)$ is the set of days in month t and $\phi(\tau, t)$ is the share of days of the month after day τ. In words, a shock that occurs on the first day of a month is fully attributed to that month, whereas for a shock that occurs in the middle of the month, half is attributed to the current month and half to the next month.

Not all current Euro Area countries have been members of the Euro Area since its inception in 1999 such that some were not directly subject to ECB monetary policy in the earlier years of our sample.[12] Nevertheless, for our baseline specification we estimate impulse responses to ECB interest rate changes for all countries based on the whole time period. We think this approach is reasonable because even those countries who joined later had already pegged their exchange rates to the Euro much earlier and were thus strongly affected by the ECB's monetary policy. As a robustness check, we also estimate IRFs only based on those

12. Slovenia joined in 2007, Cyprus and Malta in 2008, Slovakia in 2009, Estonia in 2011, Latvia in 2014 and Lithuania in 2015.

time periods in which a country was a member of the Euro Area. We also consider only the sub-sample of the original eleven Euro Area countries (EA11).

Herbst and Johannsen (2020) argue that the local projection estimator of the impulse response function can be biased if the time dimenion is small. This is unlikely to be a problem for our analyses in the previous sections, where our samples were relatively long. However, it may be of some concern here, even though, with 240 monthly observations, our time series is still more than twice as long as the median in the literature surveyed in Herbst and Johannsen (2020). For a usual hump-shaped IRF, the local projections estimator is biased towards zero and proportional to the cumulative IRF (see 2020, equation 6). If anything, we therefore expect that the bias leads us to understate the effect of inequality on the real effects of monetary policy.

We first estimate (3) for the Euro Area as a whole. Figure 9 shows the estimated IRFs to a 25 basis points change in the nominal interest rate. In response to the interest rate hike, both GDP and unemployment respond as expected. GDP falls, reaching a trough at about -1.5% after one year. Unemployment responds more slowly initially but then peaks at about 0.3 percentage points. The response of GDP is larger than that of industrial production we estimated for the US (Figure 2, black dashed line) but it is similar to the results in Almgren et al. (forthcoming).[13]

4.2 A measure of wealth inequality at the country level

We next turn to the measurement of wealth inequality at the country level. We estimate the top 10% wealth share country by country from the HFCS. The HFCS is a survey of household finances in EU countries similar to the Survey of Consumer Finances in the US. It has been conducted in three waves between 2010 and 2017 by the individual member states and is representative on the national level. Importantly, the core questions of the survey used for our analysis are identical across countries which allows for an easy comparison between countries. To obtain an estimate of the top 10% wealth share for every country, we define net worth for every household as the difference between total assets excluding public and occupational pensions plans and total outstanding household liabilities.[14] Total assets are the sum of total real assets, which consist of real estate, vehicles, self-employed businesses, and other valuables, and total financial assets, consisting of deposits, mutual funds, bonds, non-self-employed businesses, shares, managed accounts, money owed to the household, life-insurance and other pensions, and other assets. Total liabilities consist of mortgage debt and other debt such as credit card debt and consumer loans.[15]

13. Almgren et al. (forthcoming) display IRFs of GDP to an interest rate reduction by one standard deviation of the shock series. The standard deviation of our constructed shock series is 3 basis points, or 0.0003.

14. For the construction of our measure of wealth inequality, as well as for all other variables we use in our analysis below that are derived from the HFCS, we only consider households whose head is aged 20–75.

15. This definition corresponds to variable DN3001 (Net wealth excluding public and occupational pensions) in the HFCS.

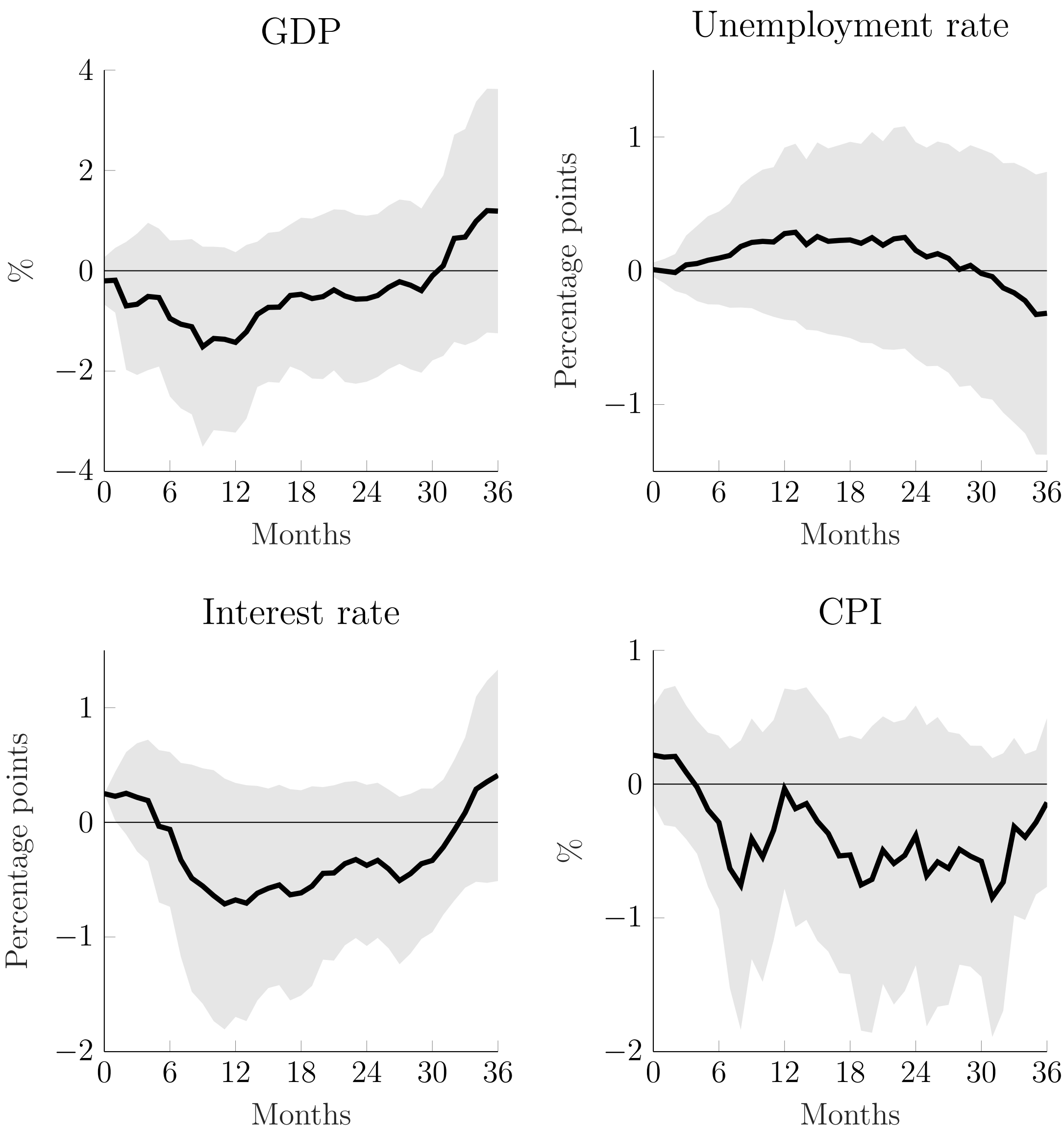

Figure 9: IRFs to a 25 bp increase in the nominal rate for the Euro Area as a whole.
Notes: Shaded areas indicate 90% confidence intervals based on Newey and West (1987) standard errors.

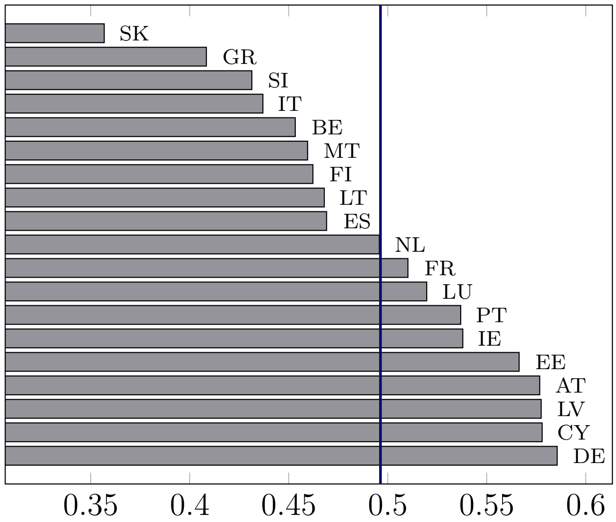

Figure 10: Top 10% wealth share in Euro Area countries.
Notes: Top 10% wealth shares, averaged across all waves of the HFCS for which data on net worth exists in a given country. Solid line: Average across countries.

Table 3: Summary statistics for Euro Area countries, 1999m1 to 2020m1.

	Mean	S.d.	Min	Max
Average response GDP (in %)	-0.47	0.86	-2.26	1.37
Average response unemployment (in pp.)	0.05	0.45	-0.85	0.93
Top 10% wealth share	0.50	0.07	0.36	0.59
Observations	19			

Notes: S.d. stands for standard deviation.

We compute the top 10% wealth share based on this definition of wealth for every country and each HFCS wave and then average over the three HFCS waves to obtain a single measure of wealth inequality for every country. Figure 10 depicts the resulting measure. In the average Euro Area country, the richest ten percent of households hold about 50% of total net worth as indicated by the vertical line. However, there is large variation across countries. The top 10% wealth share is largest in Germany with an average of 59% between 2010 and 2017. It is lowest in Slovakia and Greece at 36% and 41% respectively.

4.3 Results

Table 3 reports summary statistics for both the average GDP and unemployment responses to a 25 bp monetary policy shock as well as the top 10% wealth shares for all 19 Euro Area countries. The means of the average GDP and unemployment responses are in line with the IRFs for the Euro Area as a whole (Figure 9). Compared to the US states, the responses in the Euro Area countries display a much higher standard deviation. This might owe in part to the much shorter sample period over which we estimate the IRFs.

The first column in Table 4 shows the result of a linear regression of the average GDP response on the top 10% wealth share. As for the US states, we estimate a negative re-

Table 4: Regression of average responses to monetary policy shocks on wealth inequality.

	(1) GDP	(2) U	(3) U EA11	(4) U in EA	(5) U peak	(6) GDP	(7) U
Top 10% share	-4.6*	4.1***	3.7	4.9**	4.7**		
	(2.5)	(1.0)	(2.1)	(2.0)	(1.8)		
Top 1% share						-6.1	3.9**
						(3.7)	(1.6)
Observations	19	19	11	19	19	19	19

Notes: Dependent variable is the average IRF of GDP (in %) or unemployment ("U", in p.p.) to a 25 bp increase of the interest rate over a horizon of three years. Column 3: sample restricted to original 11 Euro Area countries. Column 4: sample restricted to countries in the Euro Area at a given point in time. Column 5: peak response of unemployment. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

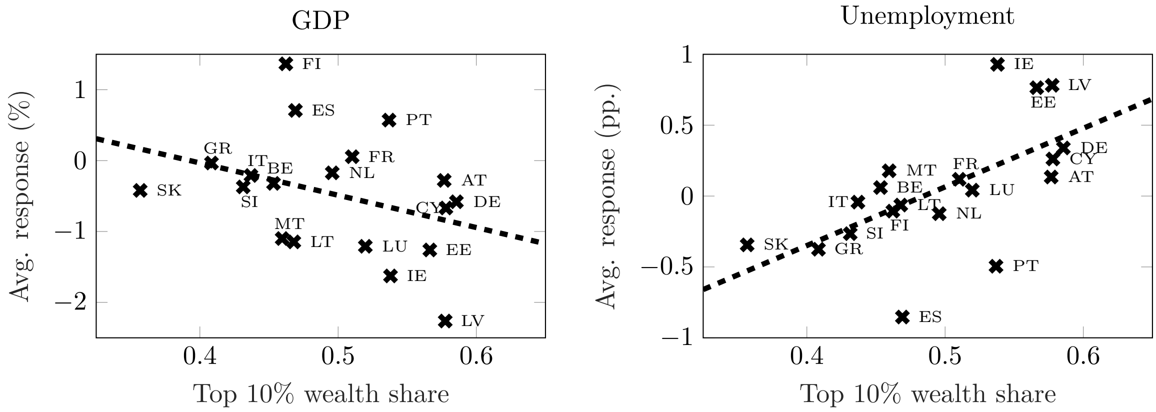

Figure 11: Top 10% wealth share and average response of GDP (left) and unemployment (right) in Euro Area countries.

lationship between inequality and the output response to an increase in the interest rate. The point estimate indicates that an increase in the top 10% wealth share by one standard deviation is associated with a $0.07 \cdot 4.6 = 0.32$ percentage points stronger contraction in GDP in response to a 25 basis points shock.

In the second column we repeat the analysis, this time using the average response of unemployment as the impact measure. The estimated effect is positive and statistically significant at the 1% level. The point estimate implies that an increase in the top 10% wealth share by one standard deviation is associated with an increase in the unemployment response to a 25 basis points rate hike by $0.07 \cdot 4.1 = 0.29$ percentage points. Given that the standard deviation of the average unemployment responses across countries is 0.45 percentage points, this is a substantial effect. We return to a discussion of the remaining columns of Table 4 below. Figure 11 shows the correlation between the degree of wealth inequality in Euro Area countries and their average response of GDP (left panel) and unemployment (right panel).

Additional controls There are several alternative explanations for the observed differences in the response to policy rate changes, and the availability of comparable country-level

Table 5: Regressions of impact measures on wealth inequality and controls.

	(1) GDP	(2) U	(3) GDP	(4) U	(5) GDP	(6) U	(7) GDP	(8) U	(9) GDP	(10) U
Top 10% share	-4.1*	4.0***	-5.4	4.3***	-7.1**	4.4***	-3.6	4.2***	-6.9**	5.0***
	(2.0)	(0.9)	(3.4)	(1.2)	(3.0)	(1.3)	(2.1)	(1.0)	(2.5)	(1.0)
Htm share	-3.0**	0.8*								
	(1.3)	(0.5)								
Emp. protect.			0.3	-0.3**						
			(0.3)	(0.1)						
Home own.					-3.0**	0.3				
					(1.3)	(0.7)				
Adj. mortg.							-0.7	-0.1		
							(0.6)	(0.3)		
Debt/GDP									1.6**	-0.6*
									(0.7)	(0.3)
Observations	19	19	16	16	19	19	18	18	19	19

Notes: Dependent variable is the average IRF of GDP (columns with "GDP") or unemployment ("U") to a 25 bp increase of the interest rate over a horizon of three years. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

data in the Euro Area allows us to investigate some of them by including additional controls.[16] Since our sample is substantially smaller than on the US state level, we only include one control at a time.

First, we add the share of hand-to-mouth households as control, i.e., households who hold only very few liquid assets. Kaplan et al. (2018) argue that a higher share of liquidity constrained households raises the aggregate marginal propensity to consume and thereby strengthens the indirect effects of monetary policy. The first two columns in Table 5 confirm the findings in Almgren et al. (forthcoming): both GDP and the unemployment rate respond more strongly to interest rate changes in countries with a higher share of hand-to-mouth households. However, the estimated coefficients on wealth inequality do not change much relative to the baseline when we control for the hand-to-mouth share, and they remain statistically significant.

Second, labor market institutions may be important for the response to monetary policy, especially when we use the unemployment rate as a proxy for economic activity. Therefore, we add an index measuring strictness of employment protection constructed by the OECD (2020) as an additional control.[17] The index measures the strictness of regulation on collective dismissals, and high values indicate a stronger protection of the workforce. As shown in columns 3 and 4 we only find a statistically significant (dampening) effect on the unemployment response and adding the index as a control does not change the conclusion

16. We obtain from Eurostat data on household debt to GDP, the share of hours worked in the manufacturing sector, of employees in small firms, and of middle-aged households. We construct the remaining controls from the HFCS, unless stated otherwise.

17. The index is not available for Cyprus, Lithuania, and Malta.

Table 6: Regressions of impact measures on wealth inequality and controls, continued.

	(1) GDP	(2) U	(3) GDP	(4) U	(5) GDP	(6) U	(7) GDP	(8) U
Top 10% share	-4.7*	4.3***	-4.6	4.1***	-6.0**	4.1***	-5.1**	4.2***
	(2.6)	(1.1)	(2.7)	(1.1)	(2.5)	(1.1)	(2.1)	(1.1)
Manuf. share	-1.6	1.8						
	(2.8)	(1.4)						
Share small firms			-3.1	0.2				
			(2.2)	(0.8)				
Share mid-aged					-10.2**	-0.5		
					(4.6)	(2.0)		
Stock market part.							4.1**	-0.3
							(1.7)	(0.6)
Observations	19	19	19	19	19	19	19	19

Notes: Dependent variable is the average IRF of GDP (columns with "GDP") or unemployment ("U") to a 25 bp increase of the interest rate over a horizon of three years. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

about the role of wealth inequality.

Beraja et al. (2019) and Wong (2021) suggest that the rate of homeownership and the share of adjustable-rate mortgages is important for the transmission of monetary policy. Homeowners are likely to have mortgages and are therefore particularly affected by interest rate changes. This effect could be even stronger if many mortgages have adjustable rates since monetary policy has a direct effect on existing mortgages in this case. Thus, we control for the homeownership rate and the share of households with adjustable-rate mortgages in columns 5 to 8.[18] The results we obtain are mixed. If anything, a higher share of homeowners leads to stronger effects of monetary policy as expected, but we do not find a significant role for the share of adjustable-rate mortgages.

For the US, Alpanda and Zubairy (2019) find evidence that higher levels of household debt are associated with smaller effects of monetary policy shocks. Columns 9 and 10 show that this relationship also holds in the cross-section of Euro Area countries. The point estimates for the effect of wealth inequality become even larger when controlling for household debt.

Next, as described in the previous section, regions with a large manufacturing industry might respond more strongly to monetary policy. We do not find a statistically significant effect of the share of total hours worked in the manufacturing sector, though the point estimates have the expected sign (Table 6, columns 1 and 2). In contrast to the results on the US state level, we find that a large share of employment in small firms is associated with larger effects of monetary policy, though the relationship is not statistically significant (columns 3 and 4).

As explained before, Leahy and Thapar (2019) find demographics to be important for the transmission of monetary policy in the US, so we control for the share of the population

18. Data on mortgage types are not available for Finland.

aged 35–65 in columns 5 and 6. We find a statistically significant amplifying effect of the share of middle-aged households on the response of GDP also in the Euro Area, but there is no significant effect on the unemployment response. Lastly, we control for stock market participation, measured as the share of households that either hold stocks directly or via mutual funds. Contrary to the results in Melcangi and Sterk (2020), we find that, if anything, higher stock market participation weakens the effects of monetary policy on real activity.

In sum, many of the factors that were found to affect the strength of the monetary transmission mechanism turn out to be important also in our data. Wealth inequality, however, appears to influence monetary policy transmission beyond its working through either of these factors.

4.4 Robustness

We conduct a number of robustness checks shown in columns 3 to 7 of Table 4. First, in column 3 we restrict the sample to the original eleven member states. We estimate a similar effect on the unemployment response as in the baseline but since the sample only consists of eleven countries in this case, it is not statistically significant. The fourth column corresponds to the case where we estimate responses to monetary policy using only those country-month observations where a given country was a member of the Euro Area. In this case we find an even stronger effect of wealth inequality on the effectiveness of monetary policy, but the estimate also has a larger standard error.

Next, in column 5 we look at the peak response of unemployment instead of its average response. The point estimate in this case is similar to the estimate for the average response. Lastly, we regress our impact measures on the top 1% wealth share instead of the top 10% share (columns 6 and 7). We find that a higher top 1% share is associated with stronger responses of GDP, but not of unemployment. However, the standard errors are larger than when considering the top 10% share such that only the effect on the unemployment response is statistically significant.

5 Conclusion

This paper was motivated by two strands of literature that have received a lot of attention over the past decade. The first one documents significant variation across countries and time in the degree of wealth inequality in the population. The second emphasizes the role of household heterogeneity for short-run phenomena such as the transmission of monetary policy. We studied empirically how the strength of monetary policy transmission depends on the degree of wealth inequality, and we found that the effects of interest rate changes are state-dependent. More unequal distributions of household wealth are associated with stronger effects of monetary policy on real variables, such as GDP, industrial production and unemployment. This relationship holds in all three contexts we considered, on the aggregate level in the US and the UK, in the cross-section of US states and in the cross-section of Euro Area countries.

Our empirical analysis implies that the distribution of wealth matters for monetary transmission. This provides an argument for using Heterogeneous Agent New Keynesian (HANK) models to analyze monetary policy. Our study can help guide the development of such models. Analyzing further which underlying forces are responsible for our empirical results and designing corresponding HANK models appear like interesting avenues for future research.

Policymakers in central banks take an increasing interest in the interaction of monetary policy and household heterogeneity (BIS 2021; Dossche et al. 2021). As mentioned in the beginning, our findings are relevant for their current discussions. Based on our results, the negative output and employment effects of raising interest rates are larger today when wealth inequality is at historically high levels than they were on average in the past.

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Appendix

A.1 Aggregate time series: identifying assumptions

We start with a general state-dependent VAR,

$$y_t = A_1(ineq_{t-1})y_{t-1} + A_0(ineq_{t-1})\varepsilon_t$$

with MA representation

$$y_t = \mathbf{B}(L)\varepsilon_t + \mathbf{B}^t(L)\varepsilon_t,$$

where ε_t is a vector of structural shocks assumed to be independent of each other and across time, and where $\mathbf{B}^t(L) = B_0^t(ineq_{t-1}) + B_1^t(ineq_{t-1}, ineq_{t-2})L + B_2^t(ineq_{t-1}, ineq_{t-2}, ineq_{t-3})L^2 \dots$. In the following we drop the explicit dependence of the coefficients on inequality, which we understand to be implied by the superscript t. Keep in mind, though, that B_s^t depends on inequality between $t-1$ and $t-s-1$. B_s^t is a matrix with rows $\beta_{s,1}^t$, $\beta_{s,2}^t$, $\dots$, where $\beta_{s,j}^t$ contains the response of $y_{j,t}$ to the vector of shocks ε_{t-s}. A typical element of $\beta_{s,j}^t$ is $\beta_{s,jk}^t$, which determines the response of $y_{j,t}$ to shock $\varepsilon_{k,t-s}$ together with the corresponding element of $\mathbf{B}$.

Interest lies in the impulse response function (IRF) of $y_{2,t}$ to $\varepsilon_{1,t}$ given a level of inequality and the history of past states of the economy $\mathcal{Y}_{t-1} = \{y_{t-1}, y_{t-2}, \dots\}$. We define the IRF for each horizon $h \in [0, 1, \dots, H]$ as

$$\begin{aligned} IRF(h, ineq_{t-1}, \mathcal{Y}_{t-1}) &= \mathbb{E}\left[y_{2,t+h} | \varepsilon_{1,t} = 1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[y_{2,t+h} | \varepsilon_{1,t} = 0, ineq_{t-1}, \mathcal{Y}_{t-1}\right] \\ = \beta_{h,21} + \sum_{s=0}^{\infty} &\left\{ \mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s} | \varepsilon_{1,t} = 1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s} | \varepsilon_{1,t} = 0, ineq_{t-1}, \mathcal{Y}_{t-1}\right] \right\}. \end{aligned} \tag{4}$$

The IRF at horizon h is the difference in the expected value of $y_{2,t+h}$ between a scenario with a shock to the nominal interest rate in t and a scenario without a shock. In terms of the notation in Gonçalves et al. (2022), there exists a baseline path $\{y_t\}$, implied by the sequence of structural disturbances and states

$$\{\dots, \varepsilon_{1,t-1}, \varepsilon_{1,t}, \varepsilon_{1,t+1}, \dots, \varepsilon_{2:n,t-1}, \varepsilon_{2:n,t}, \varepsilon_{2:n,t+1}, \dots\} \cup \{\dots, ineq_{t-1}, ineq_t, ineq_{t+1}, \dots\}$$

with $\varepsilon_{1,t} = 0$, and there exists a hypothetical sample path $\{y_{t+h}^*\}$ corresponding to

$$\{\dots, \varepsilon_{1,t-1}, \varepsilon_{1,t}^*, \varepsilon_{1,t+1}, \dots, \varepsilon_{2:n,t-1}, \varepsilon_{2:n,t}, \varepsilon_{2:n,t+1}, \dots\} \cup \{\dots, ineq_{t-1}, ineq_t^*, ineq_{t+1}^*, \dots\}$$

where $\varepsilon_{1,t}^* = 1$ Here, the notation 2:n denotes all elements of a vector except the first. Hence, the IRF could alternatively be expressed as $IRF(h, ineq_{t-1}, \mathcal{Y}_{t-1}) = \mathbb{E}[y_{2,t+h}^* - y_{2,t+h} | ineq_{t-1}, \mathcal{Y}_{t-1}]$. Observe, that the hypothetical sample path $\{y_t^*\}$ differs from the baseline path $\{y_t\}$ not only because of the difference between the shocks $\varepsilon_{t,1}$ and $\varepsilon_{t,1}^*$ but also because of the differences in the sequence of the states induced by the differences in these shocks.

Next, we derive the estimator for the IRF implied by our IV LP approach. Observe that

$$\begin{aligned} y_{1,t} =& \beta_{0,11}\varepsilon_{1,t} + \beta^{t}_{0,11}\varepsilon_{1,t} + \{\varepsilon_{2:n,t}, \varepsilon_{t-1}, \varepsilon_{t-2}, \dots\} \\ &+ \{\beta^{t}_{0,1,2:n}\varepsilon_{2:n,t}, \beta^{t}_{1,1}\varepsilon_{t-1}, \beta^{t}_{2,1}\varepsilon_{t-2}, \dots\}, \end{aligned}$$

where we follow Stock and Watson (2018) in denoting by $\{\dots\}$ a linear combination of the elements inside the braces. With the unit effect normalization $\beta_{0,11} = 1$ and $\beta^{t}_{0,11} = 0$, we have that

$$y_{1,t} = \varepsilon_{1,t} + \{\varepsilon_{2:n,t}, \varepsilon_{t-1}, \varepsilon_{t-2}, \dots\} + \{\beta^{t}_{0,1,2:n}\varepsilon_{2:n,t}, \beta^{t}_{1,1}\varepsilon_{t-1}, \beta^{t}_{2,1}\varepsilon_{t-2}, \dots\},$$

Similarly, we have that

$$\begin{aligned} y_{2,t+h} =& \beta_{h,21}\varepsilon_{1,t} + \beta^{t+h}_{h,21}\varepsilon_{1,t} + \{\varepsilon_{t+h}, \varepsilon_{t+h-1}, \dots, \varepsilon_{t+1}, \varepsilon_{2:n,t}, \varepsilon_{t-1}, \varepsilon_{t-2}, \dots\} \\ &+ \{\beta^{t+h}_{0,1}\varepsilon_{t+h} \dots \beta^{t+h}_{h-1,1}\varepsilon_{t+1}, \beta^{t+h}_{h,1,2:n}\varepsilon_{2:n,t}, \beta^{t+h}_{1,1}\varepsilon_{t-1}, \beta^{t+h}_{2,1}\varepsilon_{t-2}, \dots\}, \end{aligned}$$

such that

$$y_{2,t+h} = \beta_{h,21}y_{1,t} + \beta^{t+h}_{h,21}y_{1,t} + u_{t+h} \tag{5}$$

with

$$\begin{aligned} u_{t+h} =& \{\varepsilon_{t+h}, \varepsilon_{t+h-1}, \dots, \varepsilon_{t+1}, \varepsilon_{2:n,t}, \varepsilon_{t-1}, \varepsilon_{t-2}, \dots\} \\ &+ \{\beta^{t+h}_{0,1}\varepsilon_{t+h} \dots \beta^{t+h}_{h-1,1}\varepsilon_{t+1}, \beta^{t+h}_{h,1,2:n}\varepsilon_{2:n,t}, \beta^{t+h}_{1,1}\varepsilon_{t-1}, \beta^{t+h}_{2,1}\varepsilon_{t-2}, \dots\} \\ &+ \beta^{t+h}_{h,21} \cdot \{\varepsilon_{t+h}, \varepsilon_{t+h-1}, \dots, \varepsilon_{t+1}, \varepsilon_{2:n,t}, \varepsilon_{t-1}, \varepsilon_{t-2}, \dots\} \\ &+ \beta^{t+h}_{h,21} \cdot \{\beta^{t+h}_{0,1}\varepsilon_{t+h} \dots \beta^{t+h}_{h-1,1}\varepsilon_{t+1}, \beta^{t+h}_{h,1,2:n}\varepsilon_{2:n,t}, \beta^{t+h}_{1,1}\varepsilon_{t-1}, \beta^{t+h}_{2,1}\varepsilon_{t-2}, \dots\} \end{aligned}$$

Write (5) as

$$y_{2,t+h} = (y_{1,t}, ineq_{t-1}y_{1,t}) \begin{pmatrix} \beta_{h,21} \\ \bar{\beta}^{t+h}_{h,21} \end{pmatrix} + u_{t+h}, \tag{6}$$

where $\bar{\beta}^{t+h}_{h,21} \equiv \frac{\beta^{t+h}_{h,21}}{ineq_{t-1}}$. Premultiplying by the instrument $Z_t = (RR_t, RR_t ineq_{t-1})$ and taking expectations yields

$$\mathbb{E}\left[Z'_t y_{2,t+h}\right] = \mathbb{E}\left[Z'_t\left(y_{1,t}, ineq_{t-1}y_{1,t}\right)\begin{pmatrix} \beta_{h,21} \\ \bar{\beta}^{t+h}_{h,21} \end{pmatrix}\right] + \mathbb{E}\left[Z'_t u_{t+h}\right].$$

This equation always holds. The question is, under which conditions we can use it to obtain an estimate of the IRF given by (4). Gonçalves et al. (2022) have recently pointed out that consistency of the estimates in state-dependent local projections is not guaranteed when the state variable is endogenous. We therefore present first the case in which we assume inequality to be exogenous.

Exogenous inequality We make the following assumptions.

Assumption 1 (Exogenous state variable)

$$\mathbb{E}\left[ineq_{t+s}|ineq_{t-1}, \varepsilon_{1,t}, \mathcal{Y}_{t-1}\right] = \mathbb{E}\left[ineq_{t+s}|ineq_{t-1}\right] \quad \textit{for all } s,\ t.$$

The assumption that the state variable is exogenous implies that inequality at all horizons is unaffected by the monetary policy shock $\varepsilon_{1,t}$. In the notation of Gonçalves et al. (2022) introduced above, it holds that $ineq_s = ineq_s^*$ for all s.

Assumption 2 (Linearity in the state variable)

$$\mathbb{E}\left[\beta_{h,21}^{t+h}|ineq_{t-1}, \varepsilon_{1,t}, \mathcal{Y}_{t-1}\right] = \tilde{\beta}_{h,21} ineq_{t-1} \quad \textit{for all } t,\ h.$$

where $\tilde{\beta}_{h,21}$ is a parameter. Inequality at time $t-1$ can affect the conditional expectation of $\beta_{h,21}^{t+h}$ directly or because it is informative about future inequality which matters for $\beta_{h,21}^{t+h}$. Assumption 2 requires that the overall effect is linear in inequality at $t-1$. Observe that the linearity assumption implies that $\tilde{\beta}_{h,21} = \mathbb{E}\left[\bar{\beta}_{h,21}^{t+h}|ineq_{t-1}, \varepsilon_{1,t}, \mathcal{Y}_{t-1}\right] = \mathbb{E}\left[\bar{\beta}_{h,21}^{t+h}\right]$.

Assumption 3 (Relevance)

$$rank\, \mathbb{E}\left[Z_t'\left(y_{1,t}, ineq_{t-1} y_{1,t}\right)\right] = 2$$

Assumption 4 (Exclusion restriction)

$$\mathbb{E}\left[Z_t' u_{t+h}\right] = 0.$$

Under exogenous inequality the IRF (4) reduces to

$$IRF(h, ineq_{t-1}, \mathcal{Y}_{t-1}) = \beta_{h,21} + \mathbb{E}\left[\beta_{h,21}^{t+h}|ineq_{t-1}, \mathcal{Y}_{t-1}\right],$$

because the structural shocks are independent from each other. Remember that $\beta_{h,21}^{t+h}$ depends on inequality between $t-1$ and $t+h-1$. The linearity assumption ensures that inequality at time $t-1$ affects the conditional expectation of $\beta_{h,21}^{t+h}$ linearly, such that

$$IRF(h, ineq_{t-1}, \mathcal{Y}_{t-1}) = \beta_{h,21} + \tilde{\beta}_{h,21} ineq_{t-1}. \tag{7}$$

Gonçalves et al. (2022) consider the case, where structural shocks are observed, such that (6) can be estimated by OLS with $y_{1,t} = \varepsilon_{1,t}$. They show that the IRF can be consistently estimated in this case, if the VAR is linear in the state variable and if the state variable is exogeneous. These requirements correspond to our assumptions 1 and 2.

In contrast to Gonçalves et al. (2022), we follow Stock and Watson (2018) and take the view of that structural shocks are unobserved. In this case, we have to rely on an instrument Z_t for $y_{1,t}$ and $ineq_{t-1}y_{1,t}$ to estimate (6). This is where the relevance and exclusion restrictions are important. Together with exogenous inequality, they imply

$$\mathbb{E}\left[Z_t' y_{2,t+h}\right] = \mathbb{E}\left[Z_t'\left(y_{1,t}, ineq_{t-1}y_{1,t}\right)\right]\begin{pmatrix}\beta_{h,21}\\ \tilde{\beta}_{h,21}\end{pmatrix} + \mathbb{E}\left[Z_t' u_{t+h}\right].$$

Hence, under assumptions 1 to 4, IV estimation of (6) with $Z_t = (RR_t, RR_t ineq_{t-1})$ as an instrument yields consistent estimates of $\beta_{h,21}$ and $\tilde{\beta}_{h,21}$, we can use these to construct the state dependent IRF as

$$IRF(h, ineq_{t-1}) = \beta_{h,21} + \tilde{\beta}_{h,21} ineq_{t-1}.$$

Since inequality is exogenous, the exclusion restriction (assumption 4) holds if the sequence of identified shocks RR_t satisfies

(i) $\mathbb{E}\left[RR_t \varepsilon_{2:n,t}\right] = 0$ (contemporaneous exogeneity)

(ii) $\mathbb{E}\left[RR_t \varepsilon_{t+j}\right] = 0$ for $j \neq 0$ (lead-lag exogeneity).

These are the same conditions as for the LP IV approach without state dependence stated in Stock and Watson (2018). In the case of the Romer & Romer monetary policy shocks, exogeneity follows from the narrative identification approach used by Romer and Romer (2004) which we briefly outlined in our main text.

Endogenous inequality Recent evidence in Coibion et al. (2017), Areosa and Areosa (2016), and Bayer et al. (2022) suggests that (wealth) inequality endogenously responds to the state of the economy, in particular in changes in the interest rate. In fact, we document in Figure 18 that in our data, the top 10% wealth share rises following a hike in the interest rate, i.e. wealth inequality goes up following contractionary monetary policy.

We therefore consider next the case in which inequality endogenously reacts to the structural shocks ε_t. Gonçalves et al. (2022) show that in this case, state-dependent local projection estimates of IRF coefficients can be inconsistent. To see why this is the case, rewrite the IRF (4) as

$$\begin{aligned}
&IRF(h, ineq_{t-1}, \mathcal{Y}_{t-1})\\
&\quad= \beta_{h,21} + \mathbb{E}\left[\beta_{h,21}^{t+h}\varepsilon_{1,t}|\varepsilon_{1,t}=1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[\beta_{h,21}^{t+h}\varepsilon_{1,t}|\varepsilon_{1,t}=0, ineq_{t-1}, \mathcal{Y}_{t-1}\right]\\
&\quad+ \sum_{j>1}\left\{\mathbb{E}\left[\beta_{h,2j}^{t+h}\varepsilon_{j,t}|\varepsilon_{1,t}=1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[\beta_{h,2j}^{t+h}\varepsilon_{j,t}|\varepsilon_{1,t}=0, ineq_{t-1}, \mathcal{Y}_{t-1}\right]\right\}\\
&\quad+ \sum_{s=0}^{h-1}\left\{\mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s}|\varepsilon_{1,t}=1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s}|\varepsilon_{1,t}=0, ineq_{t-1}, \mathcal{Y}_{t-1}\right]\right\}\\
&\quad+ \sum_{s=h+1}^{\infty}\left\{\mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s}|\varepsilon_{1,t}=1, ineq_{t-1}, \mathcal{Y}_{t-1}\right] - \mathbb{E}\left[\beta_{s,2}^{t+h}\varepsilon_{t+h-s}|\varepsilon_{1,t}=0, ineq_{t-1}, \mathcal{Y}_{t-1}\right]\right\}.
\end{aligned}$$

The first line consists of the state-independent effect, $\beta_{h,21}$, and the state-dependent effect. The latter depends on the shock directly but also indirectly through the effect of the shock on future inequality. The second line is the effect of the monetary policy shock on the transmission of other contemporaneous structural shocks through its effect on future inequality. The third line is the effect of the shock on the transmission of future shocks through its effect on inequality. The fourth line is the effect of the shock on the transmission of past shocks into the future through its effect on inequality.

A few points are noteworthy. First, since B_s^{t+0} only depends on inequality up to $t-1$, the sum reduces to the expression for the exogenous inequality case above when $h=0$. This is because inequality at and before $t-1$ is known and conditioned on at time t. Hence, we consistently estimate IRFs on impact, i.e. for horizon $h=0$, which is in line with the results of Gonçalves et al. (2022).

For horizons $h>0$, the above expression cannot easily be reduced. Note, however, that even for the last horizon that we are interest in, $h=H$, it is only inequality at or before period $t+H-1$ that enters into the expectations above via the coefficients B_s^{t+H}. This fact motivates two robustness checks.

First, we lag inequality in the model by H periods, effectively assuming that the VAR is $y_t = A_1(ineq_{t-H-1})y_{t-1} + A_0(ineq_{t-H-1})\varepsilon_t$. Since $\varepsilon_{1,t}$ is independent of inequality at or before $t-1$, the above expression again reduces to the case where inequality is exogenous.[19] Figure 20 shows that our results are robust to this approach, though the point estimates of the high and the low inequality regime are a bit closer in this case.

The intuition for this robustness check is that wealth inequality is a slow-moving variable: most of its variation is at long horizons, not at business cycle frequency. Hence, inequality three years ago is a reasonable proxy for inequality today. This sets our application apart from the one discussed in Gonçalves et al. (2022), where the state variable is the state of the business cycle (boom or recession). In this case, endogeneity of the state variable appears more problematic.

Our second approach, is to include future inequality as an additional control in our IV LP. In particular, we add $ineq_{t+12}$, $ineq_{t+24}$ and $ineq_{t+36}$ as controls in (1). To the extent that a monetary policy shock alters the transmission of other shocks because it influences inequality (today and in the future), we control for this by conditioning on future inequality. Figure 20 shows that our results are largely unchanged.

19. This connects to Gonçalves et al. (2022) arguing that the state variable at time t is allowed to be a function of past values of the outcome variables, but only of values up until $t-H$.

A.2 Additional aggregate results for the US

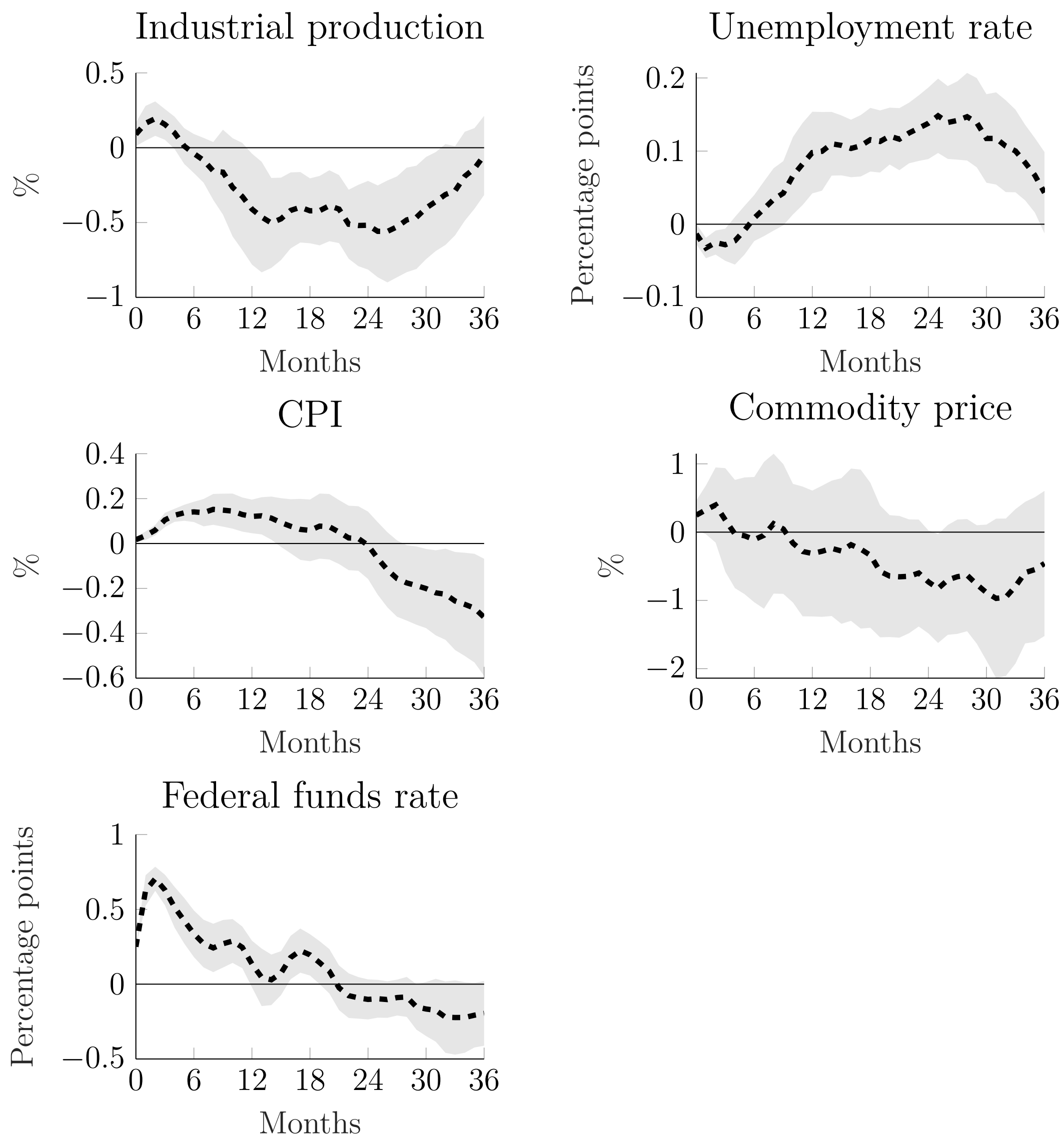
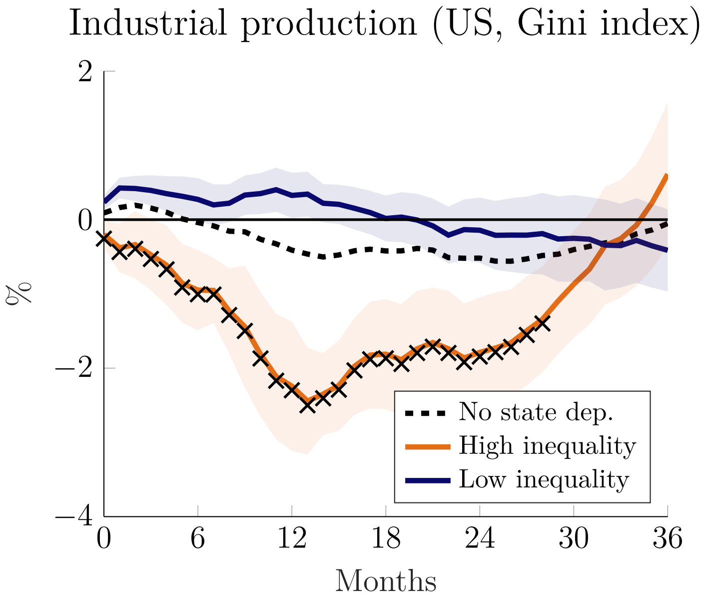


Figure 12: Unconditional IRFs to 25 basis points change in federal funds rate.
Notes: Shaded areas are 90% confidence intervals based on Newey and West (1987) standard errors.



Notes: ineq_t is measured by the Gini index. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

Figure 13: Impulse response to 25 basis points change in federal funds rate.

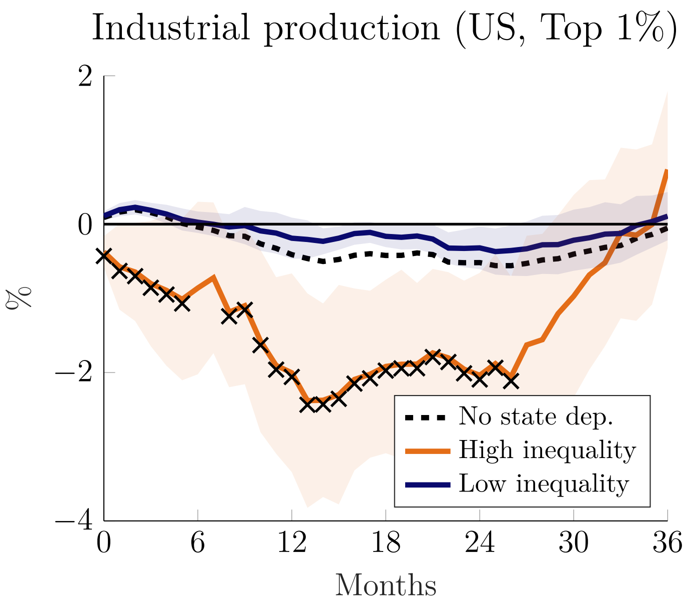


Figure 14: Impulse response to 25 basis points change in federal funds rate.

Notes: ineq_t is measured by the top 1% wealth share. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

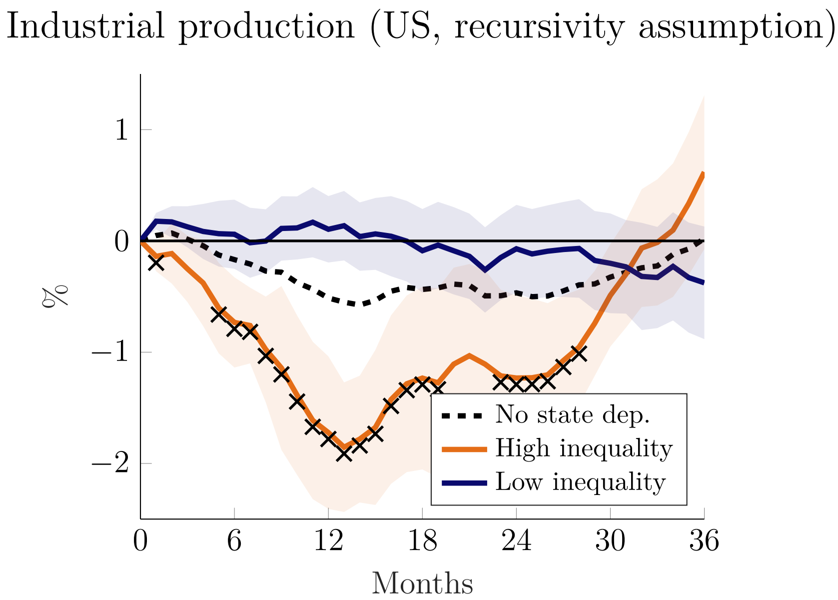


Figure 15: Impulse response to 25 basis points change in federal funds rate.
Notes: Recursivity is imposed. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

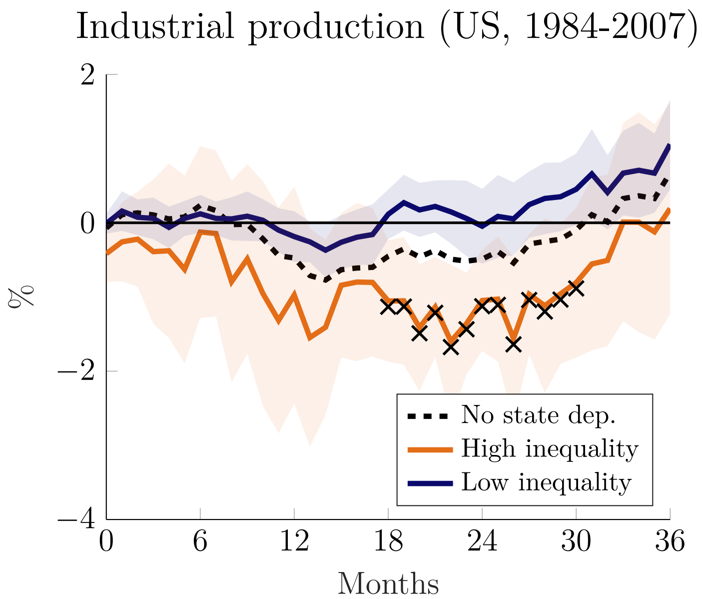


Figure 16: Impulse response to 25 basis points change in federal funds rate.
Notes: Sample period: 1984m1–2007m12. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

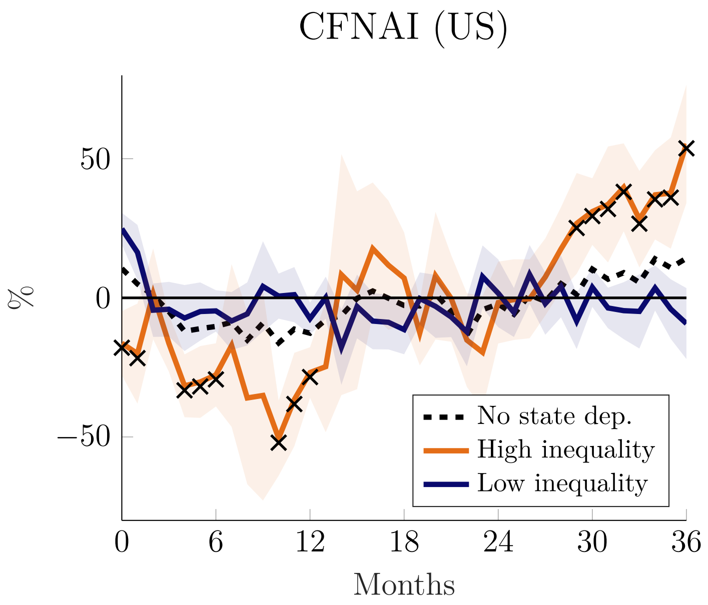


Figure 17: Impulse response to 25 basis points change in federal funds rate.
Notes: Measure of output is the Chicago Fed National Activity Index. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

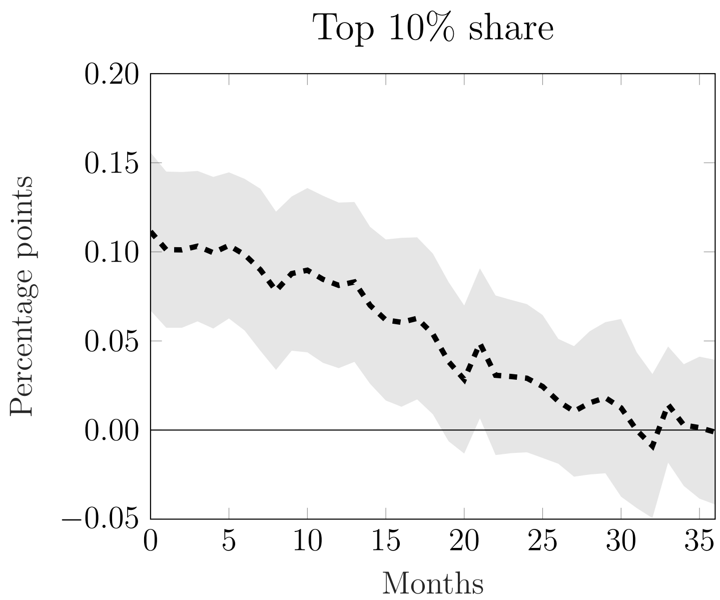


Figure 18: Impulse response of top 10% share to 25 basis points change in federal funds rate. We use the same specification as for the unconditional responses in the main text but with the top 10% wealth share as the dependent variable, $ineq_{t+h} = \alpha_{t,h} + \beta_h \cdot i_t + \sum_{p=1}^{2} \Gamma_{h,p} \cdot X_{t-p} + u_{t+h}$.

Industrial production (inequality 3 years prior)

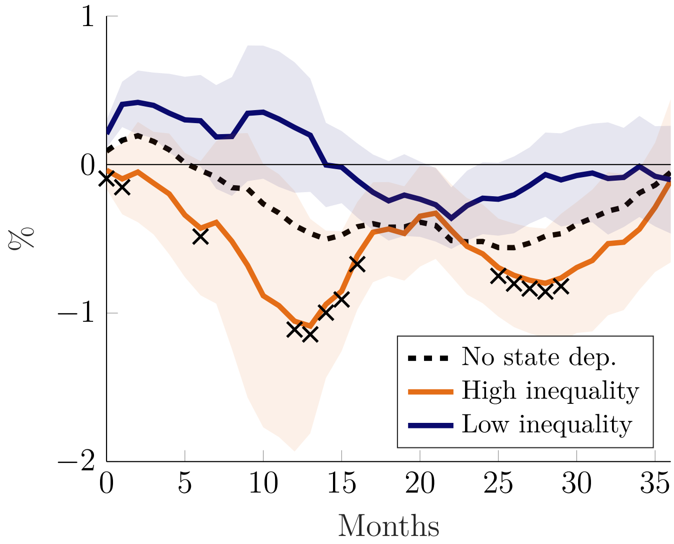


Figure 19: Impulse response to 25 basis points change in federal funds rate.

Notes: Alternative specification where we use inequality three years before the shock, both in the model and for the instrument. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

Industrial production (controlling for inequality between t and $t + H$)

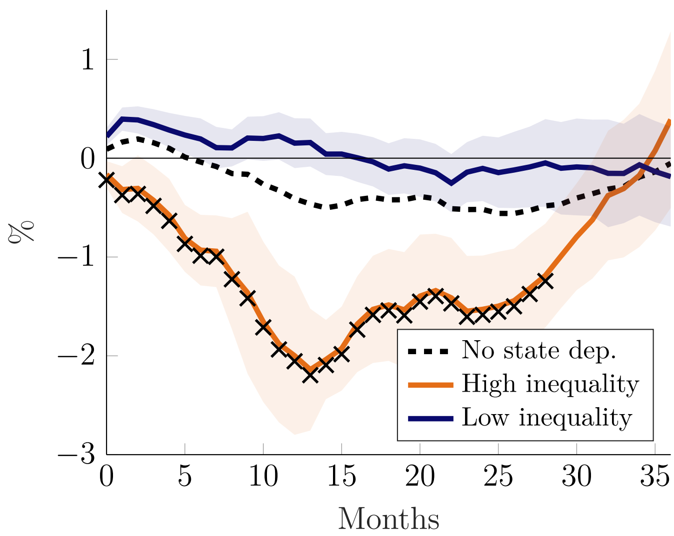


Figure 20: Impulse response to 25 basis points change in federal funds rate.

Notes: Alternative specification where we add inequality 1, 2 and 3 years after the shock as controls. Black dashed: No state dependence is imposed. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Confidence intervals are at the 90% level and based on Newey and West (1987) standard errors. Black crosses indicate horizons at which we reject equality of the IRFs at 90% confidence level ($H_0 : \beta_h^+ = 0$).

Smooth Transition VAR We estimate a Smooth Transition VAR as in Auerbach and Gorodnichenko (2012) and Teräsvirta and Yang (2014)

$$y_t = \mu + \nu t + \sum_{p=1}^{P} A_p y_{t-p} + F(ineq_{t-1}) \left(\mu^+ + \nu^+ t + \sum_{p=1}^{P} A_p^+ y_{t-p} \right) + u_{t+h}, \tag{8}$$

where, as before, F is chosen to be linear in inequality[20]

$$F(ineq_t) = ineq_t. \tag{9}$$

The vector y_t collects the log of industrial production, the unemployment rate, the logs of the consumer price index and of a commodity price index (CRB), and the federal funds rate. This selection of variables follows Coibion (2012) and is also extensively used in Ramey (2016). We add a constant and a deterministic linear trend, and set the autoregressive order to $P = 2$. As for the local projections in the main text, we use monthly data from 1969m3 to 2007m12. Figure 21 shows the resulting IRFs for a 25 basis points shock to the federal funds rate, identified using timing restrictions with the federal funds rate ordered last. The STVAR approach confirms the finding from the local projections in the main text: the response of monetary policy is stronger when inequality is high. Quantitatively, the estimated responses are smaller than those found using the local projections approach. This is not surprising since local projection with narratively identified shocks are known to yield larger responses than the recursively identified VAR approach (Ramey 2016).

A.3 Construction of the top 1% wealth share

For each US state, the SOI Bulletins provide estimates of two numbers that we use in our computation, the total wealth held by the top wealth holders and the number of top wealth holders. In some years, however, the data provided by the IRS is restricted to a subset of the top wealth holders that lie below or above certain cut-off levels in terms of the gross assets or net worth they possess. Table 7 lists these cut-offs.

In a first step, we therefore adjust the given values for total net worth of top wealth holders and the number of top wealth holders to correspond to the given threshold levels in terms of *net worth*. For instance, in 1976 we ask what fraction of the top wealth holders' wealth is owned by the top wealth holders whose net worth exceeds \$120,000. This is only a subset of the top wealth holders whose gross assets lie beyond \$120,000, for which the value is known. The SOI Bulletins provide information on how much net worth is owned by top wealth holders with gross assets exceeding but net worth falling short of \$120,000 on the aggregate US level. We can therefore compute the fraction f of net worth owned by these (poorest) top wealth holders. We then multiply the net worth of top wealth holders in each

20. We also use a more flexible exponential form $F(ineq_t) = 1 - exp\left[-\gamma\left(ineq_t - \min(ineq_t)\right)^2\right]$, where γ is an additional parameter. This yields almost identical results.

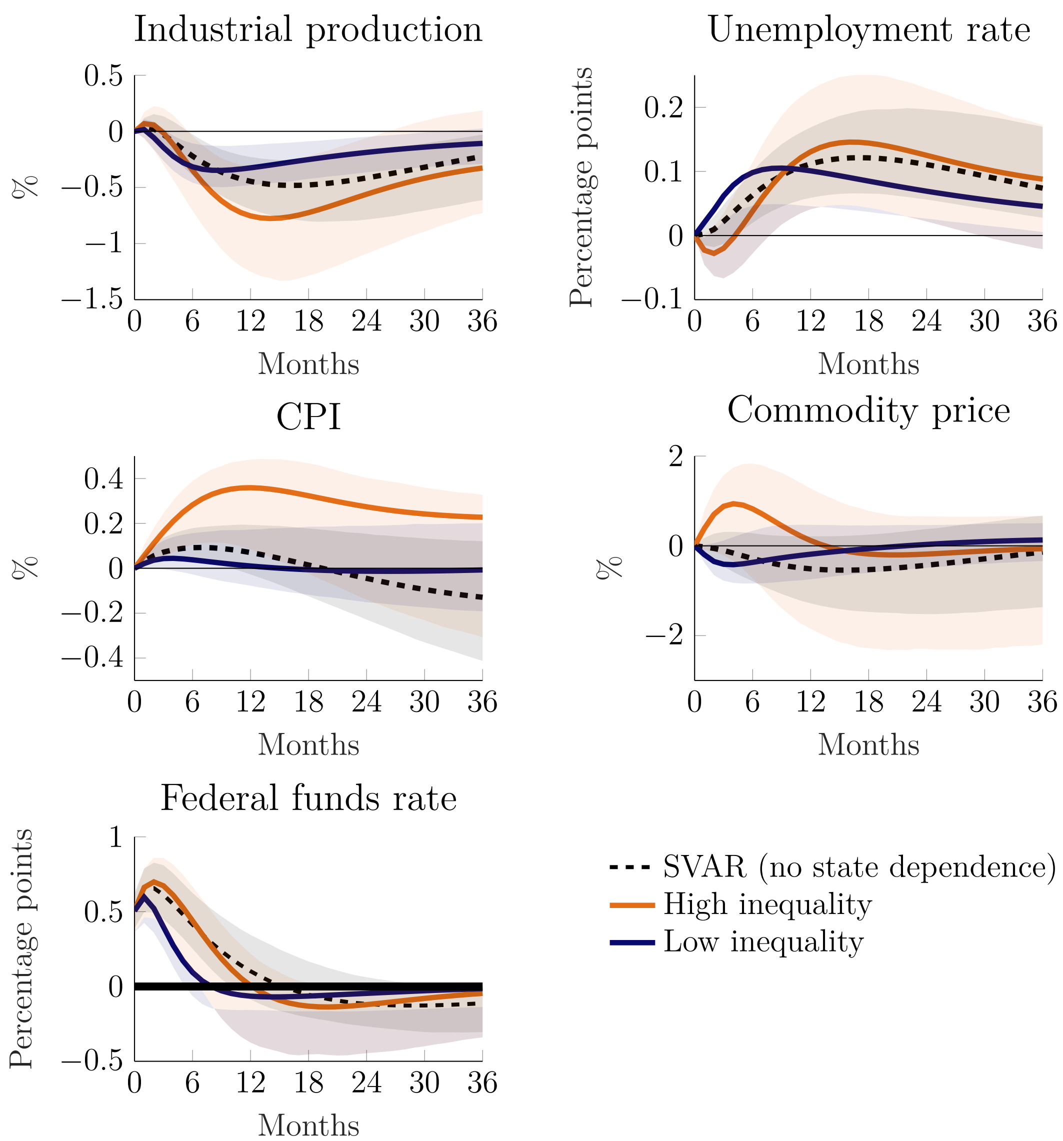


Figure 21: Impulse responses to a 25 basis points change in federal funds rate.
Notes: Black dashed: No state dependence. Blue: Regime of low inequality (first quartile of observed inequality). Red: Regime of high inequality (third quartile). Shaded areas are 90% confidence intervals from pairwise block bootstrap.

Table 7: Wealth cut-offs in between which estimates of wealth holdings are available.

Year	Cut-off below (≈ percentile)	Cut-off above (≈ percentile)	SOI Bulletin	Author(s) of SOI Bulletin
1976	$120,000 (6.1)	— (0)	Summer 1983	M. Schwartz
1982	$325,000 (2.8)	— (0)	Spring 1988	M. Schwartz
1986	$500,000 (1.6)	$10m (0.012)	Spring 1990	M. Schwartz & B. Johnson
1989	$600,000 (1.9)	$10m (0.020)	Spring 1993	B. Johnson & M. Schwartz
1992	$600,000 (2.0)	$10m (0.020)	Winter 1998	B. Johnson
1995	$600,000 (2.5)	$10m (0.023)	Winter 2000	B. Johnson
1998	$1m (1.4)	— (0)	Winter 2003	B. Johnson & L. Schreiber
2001	$1m (1.7)	— (0)	Winter 2006	B. Johnson & B. Raub
2004	$1.5m (1.0)	— (0)	Fall 2008	B. Raub
2007	$2m (0.8)	— (0)	Winter 2012	B. Raub & J. Newcomb

Notes: Numbers are underlined if they correspond to net worth and not underlined if they correspond to gross assets.

US state by $(1-f)$ to arrive at the adjusted total net worth of top wealth holders. We proceed analogously for the number of top wealth holders.

For each state and each year for which the IRS provides information on top wealth holders we are now equipped with the following information:

- The (adjusted) definition of a top wealth holder: A person is a top wealth holder if her net worth lies between a and b dollars, where a and b correspond to the cut-offs shown in Table 7.
- The total net worth of top wealth holders in a state: Denote this number by NW.
- The share of top wealth holders in the population: Denote this number by s.

The goal is to use this information to construct a measure of inequality that is comparable across states. We make the following assumptions.

Assumption 5 *The distribution of net worth x for some state at some time is given by the probability distribution function f. Its right tail is assumed to follow a Pareto distribution, i.e., for some $x_M < a$*

$$f(x) = \begin{cases} \phi(x) & x \leq x_M \\ g(x)\,(1-\Phi(x_M)) & x > x_M \end{cases},$$

where g is the density of the Pareto distribution with scale parameter x_M and shape parameter k and ϕ is some unknown pdf with Φ the corresponding cdf.

Denote by N the total population in a state. It follows

$$\begin{aligned} NW = N\int_a^b xf(x)\,\mathrm{d}x &= N\int_a^b xg(x)\,\mathrm{d}x\,[1-\Phi(x_M)] \\ &= Nx_M^k\frac{k}{1-k}\left(b^{1-k}-a^{1-k}\right)[1-\Phi(x_M)] \end{aligned}$$

and

$$s = \int_a^b f(x)\,\mathrm{d}x = (G(b)-G(a))\,[1-\Phi(x_M)] \tag{10}$$

where $G(\cdot)$ is the cdf of the Pareto distribution. The average net worth of a top wealth holder $NW/(N\cdot s)$ implicitly defines the shape parameter k of the Pareto distribution

$$\frac{NW}{N\cdot s} = \frac{\int_a^b xg(x)\,\mathrm{d}x}{G(b)-G(a)},$$

The total wealth of the top T wealth holders, for $T < 1-\Phi(x_M)$, is given by

$$TopTPercWealth = N\int_t^\infty xf(x)\,\mathrm{d}x,$$

with $t = x_M(\frac{1-\Phi(x_M)}{T})^{1/k}$. This yields

$$TopTPercWealth = N\frac{k}{k-1}x_M T^{\frac{k-1}{k}}\left[1-\Phi(x_M)\right]^{\frac{1}{k}},$$

and using (10)

$$TopTPerWealth = N\frac{k}{k-1}T^{\frac{k-1}{k}}s^{1/k}\left(a^{-k}-b^{-k}\right)^{-1/k}. \tag{11}$$

Dividing (11) by the state's total wealth gives the wealth share of the richest $T \cdot 100\%$. As a measure of the total population size N we use data from the U.S. Census Bureau (population aged 20 and above).

Lastly, we require a measure of total wealth on the state level, which is not readily available. We therefore use the following procedure. We obtain total US household wealth that corresponds to the items included in the net worth measures used by the IRS from Kopczuk and Saez (2004).[21] We then use data on capital income from the Bureau of Economic Analysis (item "Dividends, interest, and rent") which is available both for the US and on the state level. We then divide total US net wealth by capital income, backing out an aggregate interest rate. We use this interest rate to capitalize capital income on the state level, i.e., we multiply capital income on the state level with the aggregate interest rate. Implicitly we therefore assume that the portfolio of assets in each state earns the same interest rate in a given year.

Figure 22 plots our self-constructed top 1% share for the US, as well as the estimate from Kopczuk and Saez (2004) (extended by Saez and Zucman (2016) for the years 2001 and 2004) who rely on confidential individual estate tax return data. While our measure displays a somewhat higher level as well as larger amplitude over time than theirs, overall, both the level and the dynamics of our wealth share are broadly consistent with Kopczuk and Saez (2004) and Saez and Zucman (2016).[22] Note also that for the identification of the effects of wealth inequality on the states' responses to monetary policy in Section 3 we only require that we do not make any systematic error in measuring wealth inequality *across* US states. Over- or underestimating the top 1% share at any point in time *in all states* would not confound our results.

Figure 23 shows the top 1% wealth share, averaged over time, for all US states. Nevada is the state with the highest estimated average wealth inequality (top 1% share of 35.7%),

21. Their time series on total wealth ends in 2002, and we therefore miss two observations, 2004 and 2007. Since the ratio of total US household net worth as measured by the Federal Reserve Board (series TNWBSHNO) to Kopczuk and Saez (2004)'s measure has historically been very stable (mean: 1.36, max: 1.41, min: 1.32), we divide the Fed's measure by the mean of this ratio to obtain total net worth for the two last years in our sample.

22. The fact that estimates of top wealth shares using the estate tax multiplier method do not show as stark an increase in inequality since the 1980s as do estimates based on capitalizing income (Section 2) or based on the Survey of Consumer Finances is well documented. Kopczuk (2015) and Saez and Zucman (2016) discuss potential reasons for this, among others a rising mortality gradient in age over time and increasing estate tax planning.

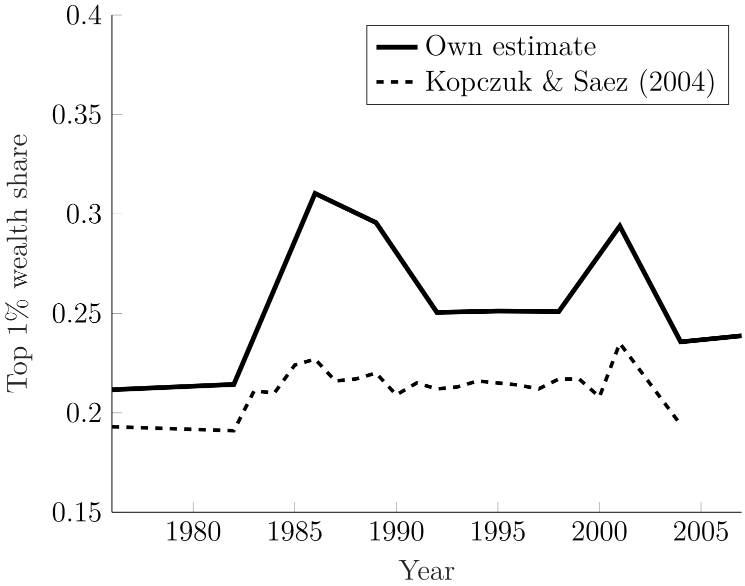


Figure 22: Top 1% wealth share in the US, own estimates and estimates from Kopczuk and Saez (2004).

followed by California, Connecticut, and New York. Both Texas and Florida also feature above-average inequality. In the mid-western states, most notably North Dakota (top 1% share of 14.3%), Iowa, Montana, and South Dakota wealth is rather equally distributed. These patterns are broadly in line with the concentration of top wealth holders or millionaires in the population, reported graphically in several of the SOI Bulletins.

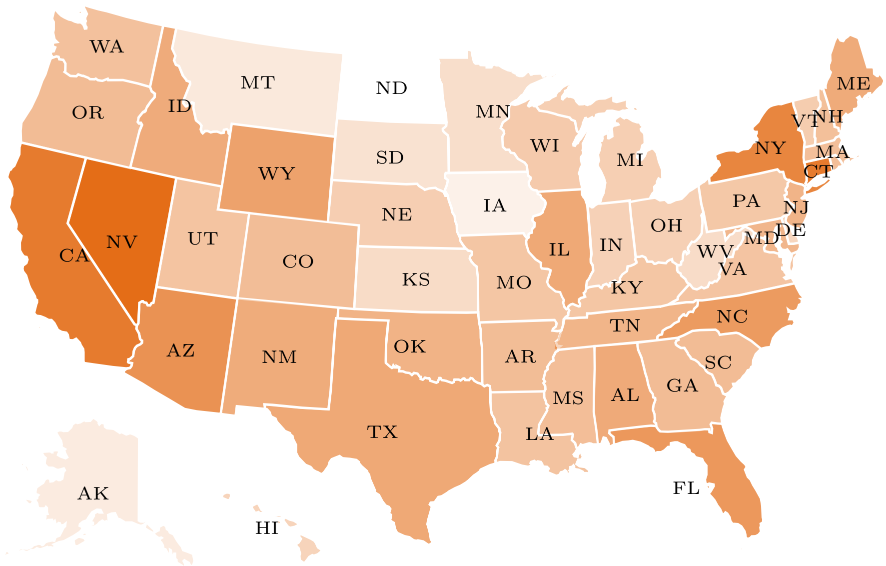


Figure 23: Average top 1% wealth share in US states 1976–2007, darker = higher.

A.4 Additional state-level results

Table 8: Summary statistics for variables on the US state level, 1976q1 to 2007q4.

	Mean	S.d.	Min	Max
Average response unemployment (in pp.)	0.08	0.04	0.01	0.17
Top 1% wealth share	0.24	0.04	0.14	0.36
Manufacturing share	0.18	0.07	0.04	0.32
Share small firms	0.50	0.06	0.41	0.67
Share middle-aged	0.49	0.01	0.45	0.53
Share old	0.17	0.03	0.07	0.24
Observations	50			

Notes: S.d. stands for standard deviation.

Table 9: Summary statistics for variables on the US state level, 1969m1 to 2007m12.

	Mean	S.d.	Min	Max
Average response personal income (in %)	-0.12	0.08	-0.27	0.18
Top 1% wealth share	0.24	0.04	0.14	0.36
Manufacturing share	0.19	0.08	0.04	0.34
Share small firms	0.50	0.06	0.41	0.67
Share middle-aged	0.49	0.01	0.45	0.52
Share old	0.17	0.03	0.06	0.24
Observations	50			

Notes: S.d. stands for standard deviation.

Table 10: Regression results for the cross-section of US states (longer sample).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	log state personal income				state unemployment rate			
Top 1% share	-0.4	-0.5**	-0.4	-0.2	0.1	0.2	0.08	0.07
	(0.3)	(0.2)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)
Manuf. share		-0.7***	-0.6***	-0.4**		0.4***	0.3***	0.3***
		(0.1)	(0.2)	(0.2)		(0.06)	(0.08)	(0.09)
Share small firms			0.2	0.4*			-0.2	-0.2
			(0.2)	(0.2)			(0.1)	(0.1)
Share mid-aged				-1.8***				0.05
				(0.6)				(0.3)
Share old				-0.9*				0.006
				(0.5)				(0.2)
Observations	50	50	50	50	50	50	50	50
Sample	’69–’07	’69–’07	’69–’07	’69–’07	’76–’07	’76–’07	’76–’07	’76–’07

Notes: Dependent variables are the average IRF of log real state personal income (columns 1–4) and the state unemployment rate (columns 5–8) to a 25 basis points increase in the federal funds rate over a horizon of three years. Sample periods are 1969q1–2007q4 (state personal income) and 1976m1–2007m12 (unemployment). Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Regression results for the cross-section of US states (State Coincident Index).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top 1% share	-1.8**	-1.9***	-1.7**	-1.5*	-0.4	-0.6*	-0.3	-0.2
	(0.7)	(0.6)	(0.8)	(0.8)	(0.3)	(0.3)	(0.3)	(0.4)
Manuf. share		-1.2*	-0.9	-0.7		-0.9***	-0.7**	-0.6*
		(0.7)	(0.8)	(0.8)		(0.2)	(0.3)	(0.3)
Share small firms			0.5	1.1			0.5*	0.6*
			(0.9)	(0.9)			(0.3)	(0.3)
Share mid-aged				-5.7**				-1.1
				(2.3)				(1.0)
Share old				-1.5				-0.5
				(1.3)				(0.6)
Observations	50	50	50	50	50	50	50	50
Sample	'84–'07	'84–'07	'84–'07	'84–'07	'79–'07	'79–'07	'79–'07	'79–'07

Notes: Dependent variable is the cumulative IRF of the log State Coincident Index (constructed by the Federal Reserve Bank of Philadelphia) to a 25 basis points increase in the interest rate over a horizon of three years. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Regression results for the cross-section of US states (peak response).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top 1% share	-1.2*	-1.3*	-1.3*	-1.0	0.09	0.05	-0.003	0.2
	(0.6)	(0.7)	(0.7)	(0.8)	(0.2)	(0.3)	(0.3)	(0.3)
Manuf. share		-0.8	-0.8	-0.5		-0.5***	-0.5**	-0.3
		(0.5)	(0.6)	(0.6)		(0.2)	(0.2)	(0.2)
Share small firms			0.04	0.7			-0.10	0.1
			(0.7)	(0.7)			(0.3)	(0.2)
Share mid-aged				-5.5**				-2.5***
				(2.5)				(0.8)
Share old				-1.5				-1.0**
				(1.3)				(0.4)
Observations	50	50	50	50	50	50	50	50
Sample	'84–'07	'84–'07	'84–'07	'84–'07	'69–'07	'69–'07	'69–'07	'69–'07

Notes: Dependent variable is the minimum response of log real state personal income to a 25 basis points increase in the federal funds rate over a horizon of three years. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Regression results for the cross-section of US states (fixed effects regressions).

	(1)	(2)	(3)	(4)	(5)	(6)
Top 1% share	-0.6*	-1.8***	-0.8	-0.5	-0.5	-0.6
	(0.3)	(0.6)	(0.6)	(0.5)	(0.5)	(0.5)
Manuf. share				3.0***	2.7***	2.6***
				(0.4)	(0.4)	(0.6)
Share small firms					1.9*	1.1
					(1.0)	(1.4)
Share mid-aged						-1.8
						(1.3)
Share old						5.2*
						(2.8)
Observations	50	50	100	100	100	100
Sample & FE	'69–'88	'88–'07	'69–'07, FE	'69–'07, FE	'69–'07, FE	'69–'07, FE

Notes: Dependent variable is the cumulative IRF of real state personal income to a 25 basis points increase in the interest rate over a horizon of three years. Full sample is cut in half and then a fixed effects regression is conducted (columns 3–6). The first two columns indicate results from estimating equation (2) for each of the two subsamples separately. Robust standard errors are reported in parentheses. A * indicates $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.