école d'économie d'aix-marseille
aix-marseille school of economics

Working Papers / Documents de travail

Monetary policy and the top one percent: Evidence from a century of modern economic history

Mehdi El Herradi
Aurélien Leroy

WP 2020 - Nr 47

Monetary policy and the top one percent:
Evidence from a century of modern economic history

Mehdi El Herradi* Aurélien Leroy †

Abstract

This paper examines the distributional effects of monetary policy in 12 OECD economies between 1920 and 2016. We exploit the implications of the macroeconomic policy trilemma with an external instrument approach to analyse how top income shares respond to monetary policy shocks. The results indicate that monetary tightening strongly decreases the share of national income held by the top one percent and vice versa for a monetary expansion, irrespective of the position of the economy. This effect (i) holds for the top percentile and the ultra-rich (top 0.1% and 0.01% income shares), while (ii) it does not necessarily induce a decrease in income inequality when considering the entire income distribution. Our findings also suggest that the effect of monetary policy on top income shares is likely to be channeled via real asset returns.

JEL Codes: E25, E42, E52

Keywords: Monetary policy, Top incomes, Macroeconomic Policy Trilemma, External Instrument

*Aix-Marseille Univ, CNRS, AMSE, Marseille, France. Corresponding author.E-mail: el-mehdi.el-herradi@univ-amu.fr

†University of Bordeaux. E-mail: aurelien.leroy@u-bordeaux.fr

This paper is forthcoming in the *International Journal of Central Banking (IJCB)*. We thank the co-editor, Pierpaolo Benigno and four anonymous referees for helpful comments on an earlier draft. We are also grateful to Jérôme Héricourt, Jakob de Haan, Andrea Colciago, Etienne Farvaque, Paul Hubert, Samuel Ligonnière, Marco Casiraghi, Corentin Laffitte, Fabien Tripier and Gabriele Galati for various discussions. The paper has benefited from seminar and workshop participants at Aix-Marseille School of Economics (AMSE), 35th Congress of the European Economic Association, De Nederlandsche Bank, 19th Doctoral Meetings in International Trade and International Finance, and 68th Annual Meeting of the French Economic Association. This work was also supported by the French National Research Agency Grants ANR-17-EURE-0020.

1 Introduction

The global financial crisis and subsequent central bank measures raised important questions about the side effects of accommodative monetary policies. In a context already marked by rising income and wealth inequality, the distributional effects of monetary policy have become an increasingly popular topic in policymaking circles. This is unusual because it is widely accepted that central banks should not be concerned about inequality: they are independent of the political process, and dealing with distributional matters goes beyond their mandate. Nevertheless, the combination of an ultra-low interest rate environment and large asset purchase programs is argued to have reduced modest household savings and driven up asset prices, which are mainly held by rich households. Are these effects only linked to the context of unconventional measures or do they constitute a structural feature of monetary policy?

This paper presents new empirical evidence regarding the distributional consequences of monetary policy using annual data from 12 OECD economies over the period 1920-2016.[1] The pretax national income share held by the top one percent (P1) is used as a benchmark top income measure.[2] The adopted identification scheme of monetary policy shocks particularly relates to the historical context of this study and uses a quasi-natural experiment approach to estimate how exogenous changes in monetary conditions affect the top 1%. To understand how monetary policy interacts with the rest of the income distribution, we draw on different distributional measures of the top decile and standard indicators of income inequality. Furthermore, we exploit the importance of financial assets and capital returns for top income households to provide insights into some of the underlying transmission mechanisms of monetary policy. Finally, state-dependent effects of interest rate shocks are estimated to study the non-linear effects of monetary policy on top incomes.

Our empirical methodology primarily relies on local projections (LPs) *à la* Jordà (2005). The latter generates dynamic responses of top income shares to an exogenous perturbation in the short-term interest rate. The identification of such shocks is based on the approach recently proposed by Jordà et al. (2020), which responds to the fact that the short-term interest rate and top income shares are potentially influenced by common unobserved factors, biasing the empirical effect of interest. Precisely, our approach exploits an instrumental variable in the context of a local projection-instrumental variable (LP-IV) framework (see, Jordà et al. (2015); Jordà et al. (2020); Ramey and Zubairy (2018)) to isolate exogenous fluctuations in the short-

[1] The 12-country panel includes Australia, Canada, Denmark, France, Germany, Italy, Japan, the Netherlands, Norway, Sweden, the U.K. and the U.S.

[2] Our interest in top incomes and the top 1% in particular stem from the fact that they have largely contributed, since the 1980s, to the rising inequality in the developed world (see, e.g., Alvaredo et al. (2013)).

term interest rate, which are drawn from the well-known macroeconomic policy trilemma. The trilemma states that movements in the base country's short-term interest rate provide exogenous variations in the domestic short-term rate for an open peg. As a result, policy choices regarding capital mobility, exchange rates and interest rates provide a natural experiment to analyse the effect of monetary policy on top income shares. Finally, because LPs easily accommodate non-linearities, we test our model in a state-dependent setting, where we allow the response of top income shares to depend on the regime of a specific variable (i.e., business cycle, the inflation regime, credit cycles, stock return cycles and monetary policy stance).

The empirical literature on the effects of monetary policy on the income distribution is growing rapidly but remains inconclusive. Country-level studies using household-level data suggest that conventional monetary tightening increases income and consumption inequality (see Coibion et al. (2017) for the U.S. and Mumtaz and Theophilopoulou (2017) for the U.K.). Cross-country evidence by Furceri et al. (2018) documents a similar effect while stressing that its magnitude depends on the share of labor income and extent of redistribution policies. Other studies argue that expansionary monetary policy may also have negative distributional implications (see Cloyne et al. (2020) for the U.K. and U.S. and Inui et al. (2017) for Japan). In contrast, most recent research on the distributional effects of unconventional monetary policy shows that the relationship between monetary expansions and inequality is negative but small in magnitude (see, e.g., Casiraghi et al. (2018) for Italy, Guerello (2018) and Samarina and Nguyen (2019) for the Eurozone). Most important, the existing evidence features survey-based estimates of income inequality and mostly focuses on a short time span.

The contribution of our paper departs from the existing literature in two important respects. First, we use tax-based estimates of top income shares from the World Inequality Database (WID) because, as shown by Atkinson et al. (2011) and Burkhauser et al. (2012), such data allow for (i) better coverage of business and capital returns, which constitute the bulk of top incomes, and (ii) provide a more accurate picture of the trend of income inequality since the 1980s. In fact, as noted by Roine and Waldenström (2015), rich households are underrepresented in income and wealth surveys, which leaves out an essential piece of the income distribution for understanding the effects of monetary policy on inequality. We extend the empirical analysis by comparing the effect of monetary policy on the top 1% income share (P1) to other top income indicators, i.e., the share of national income held by the lower 9 percent of the top decile (P09) and the top 0.1% and 0.01% (P01 and P001, respectively) along with a standard income inequality measure, i.e., the Gini index (for market and disposable incomes). While P09 consists of highly salaried workers, the right-tail percentile shares (P01 and P001) could be considered the *ultra-rich*, for whom capital income matters most (see Roine et al. (2009) on the heterogeneity underlying incomes at the top). Second, our paper features a historical analysis covering a century of modern economic history. This approach has the advantage of dealing with several macroeconomic occurrences and covers important events experienced in the developed world, such as the Great Depression and the post-war boom, hence providing more variation in the data and, in particular, top income shares. For this purpose, long series of macroeconomic variables are extracted from the Jordà-Schularick-Taylor Macrohistory Database, developed by Jordà et al. (2016). Using such data is of great interest because they offer a rich set of control variables that could enter as potential determinants of top incomes.

Our main results are easily summarized. First, monetary policy has a significant and persistent effect on top income shares: monetary tightening decreases the share of national income held by the top one percent (P1), while expansionary monetary policy has the opposite effect. A normalized +100 b.p. exogenous increase in the short-term interest rate via the external instrument reduces P1 by 0.45 percentage points over a five-year horizon for the full sample, although the effects on top incomes are smaller during the post-WWII era (0.3 percentage points decline in P1 over a five-year horizon). Second, in line with Jordà et al. (2020), we find evidence of considerable attenuation bias in policy responses when we estimate the responses to monetary policy using traditional OLS selection-on-observables versus IV identification. Third, it is shown that the effects of monetary policy on top incomes are (i) heterogeneous and (ii) not necessarily mirrored over the entire income distribution. On the one hand, a positive interest rate shock reduces the shares of national income held by the top 1, 0.1 and 0.01 percent, while its effect on the bottom 9 percent of the top decile is positive (although not statistically significant). On the other hand, monetary tightening increases the Gini index for market and disposable incomes. Fourth, with respect to the literature, we take several steps to explain that our difference with Furceri et al. (2018) is not driven by our sample and is even less tied to the identification strategy; rather, it is very likely due to the different economies considered in our respective panels. Finally, we demonstrate that our baseline finding is arguably channeled via lower (real) asset returns, which is consistent with the income composition channel of Coibion et al. (2017). The results are valid regardless of the state of the economy and hold under a battery of robustness checks.

These findings contribute to Furceri et al. (2018) and highlight that the effects of monetary policy on inequality crucially rely on the distributional indicator examined (tax-based estimates of income shares or synthetic inequality measures), the macroeconomic occurrences covered (historical data vs short period samples) and most important the countries considered along with the identification strategy adopted for monetary surprises.

The paper is structured as follows. Section 2 discusses the estimation methodology and the identification strategy. Section 3 thoroughly describes the data. The fourth section presents the LP results, while the fifth and final section concludes the paper.

2 Local projections

We follow the general method proposed by Jordà (2005) and its very recent application to our context in Furceri et al. (2018) by estimating impulse response functions (IRFs) from local projections (LPs). In its basic form, LP consists of a sequence of regressions of the endogenous variable shifted several steps ahead. As a result, the approach consists of estimating the following equation:

$$\Delta_h y_{i,t+h} = \alpha_i^h + \beta^h \Delta r_{i,t} + \theta^h X_{i,t} + \varepsilon_{i,t}^h \tag{1}$$

where $\Delta_h y_{i,t+h} = y_{i,t+h} - y_{i,t}$ and corresponds to the change in the top income variable from the base year t_0 up to year $t + h$, with $h = 1, ..., H$; $\Delta r_{i,t}$ denotes the change in the short-term interest rate; and $X_{i,t}$ refers to a vector containing a set of control variables. The latter includes the lags of the first difference of $y_{i,t}$ and $r_{i,t}$, together with additional controls that could theoretically explain top income shares and, simultaneously, be correlated with monetary conditions.

It is important to note that each step of the local IRF is obtained from a different equation and directly corresponds to the estimates of β^h. Thus, unlike in a VAR approach, the estimated coefficients contained in θ^h are not used to build the IRF. Instead, they only serve as controls and cleanse the β^h of the effects of past top income and monetary policy changes, in addition to contemporaneous and past changes in other macroeconomic variables (output and CPI, for instance). Moreover, the LP approach is intentionally "model-free" and therefore imposes fewer restrictions — with respect to VARs -– when calculating IRFs. As shown by Jordà (2005), such an approach confers numerous advantages. This estimation technique is actually (i) more robust to model misspecification, (ii) does not suffer from the curse of dimensionality, (iii) can more easily accommodate non-linearities and (iv) can also be estimated with simple regression techniques.[3] In what follows, we describe the benefits of LP with respect to our research question.

First, LPs allow for the addition of several control variables – before encountering dimensionality problems – that may influence top income shares and, simultaneously, be correlated with monetary policy actions. The $X_{i,t}$ vector includes the first difference up to two lags of the log

[3]However, it also has some drawbacks in terms of efficiency (see Ramey (2016) on the efficiency/flexibility trade-off of LP).

of the CPI, real GDP, real consumption, real investment, the government expenditure-to-GDP ratio, house prices, stock prices, total factor productivity, the total loans-to-GDP ratio and a trade openness ratio. In addition to these country-time variables, we include world real GDP growth to parsimoniously remove global business cycle effects.[4]

The second benefit of LP is that it offers an original identification strategy to estimate dynamic causal effects. To build shock series, our strategy relies on external instruments, i.e., variables correlated with changes in short-term interest rates but not with the other macroeconomic shocks affecting the economy. Our aim is to obtain external sources of variation in short-term interest rates to provide quasi-random experiments and thereby more clearly identify causal effects. These types of strategies have recently attracted growing interest in applied macroeconomics (Jordà et al., 2015, 2020; Jordà and Taylor, 2016; Ramey and Zubairy, 2018; Stock and Watson, 2018). Regarding our research question, monetary policy is unlikely to be driven by top incomes; therefore, the dynamic causal effect is clear (no simultaneity bias). However, even if the income distribution is not a target of central banks, both top incomes and monetary policy decisions depend on economic conditions, which may be improperly measured by the set of control variables in our regressions (omitted variable bias) (Furceri et al., 2018). Accordingly, this situation calls for the use of exogenous shocks to domestic monetary conditions rather than short-term interest rates. As is widely agreed upon in the literature, the challenge is to find external factors that would make the variations in monetary conditions a random treatment.

In this paper, we use the local projection-instrumental variable (LP-IV) method proposed by Jordà et al. (2015), Ramey (2016), and Stock and Watson (2018). We couple this method with the identification strategy for external variations in monetary conditions based on Jordà et al. (2015) and Jordà et al. (2020). The purpose here is to use the macroeconomic policy trilemma to find external variations in monetary conditions. The latter states that a country cannot simultaneously achieve free capital mobility, a fixed exchange rate and independent monetary policy. When pursuing any two of these goals, it is necessary to abandon the third. Building on the trilemma (Obstfeld et al., 2004, 2005; Shambaugh, 2004), we trace out episodes where monetary policy is not autonomous and external conditions from the base country can generate perturbations to the domestic short-term interest rate. Such perturbations are considered to be exogenous because the base country, for example, the U.S. during the Bretton Woods era, does not internalize the externalities of its own policy choices on partner countries. This makes the trilemma a source of natural experiments for domestic monetary policy.

[4] As noted by Jordà et al. (2020), adding time fixed effects would require almost one hundred additional parameter estimates.

The trilemma links the domestic interest rate to the base country's interest rate through the exchange rate regime and the intensity of financial openness. A suitable expression for such an instrumental approach is given by:

$$\Delta r_{i,t} = a + b[PEG_{i,t} * KOPEN_{i,t} * \Delta r^{base}_{i,t}] + \Theta C_{i,t} + \mu_{i,t} \tag{2}$$

where $PEG_{i,t}$ defines whether a country has a fixed ($PEG_{i,t} = 1$) or flexible exchange rate ($PEG_{i,t} = 0$); $KOPEN_{i,t}$ indicates whether a country is open ($KOPEN_{i,t} = 1$) or closed ($KOPEN_{i,t} = 0$) to international capital markets; $\Delta r^{base}_{i,t}$ denotes the monetary policy change in the base country; and $C_{i,t}$ is a vector of macroeconomic controls in country i at time t.[5] Equation 2 corresponds to the first-stage IV approach adopted by Jordà et al. (2015) with the term $[PEG_{i,t} * KOPEN_{i,t} * \Delta r^{base}_{i,t}]$ referring to the external instrument ($z_{i,t}$).

The instrument has to fulfil two usual criteria. First, $z_{i,t}$ must have a significant influence on the endogenous variable. In practice, when there is perfect capital mobility and a fixed exchange rate regime, the home country's monetary conditions ($\Delta r_{i,t}$) are perfectly related to those of the base country ($\Delta^{base} r_{i,t}$), which theoretically ensures the relevance of our instrument.[6] Second, $z_{i,t}$ should affect home monetary policy without influencing top incomes. This condition implies that only the international interest rate channel is at play. However, base monetary conditions may impact domestic outcomes other than interest rates. For instance, an increase of the base country's policy rate decreases its real GDP, which can have consequences for the peg countries and, *in fine*, affect top incomes. Such spillover effects lead to failure of the exclusion restriction. To control for such spillover confounding, we follow Jordà et al. (2020) and consider base country policy surprises rather than the change in its interest rate. Therefore, the corresponding two-stage least squares (2SLS) model we estimate is given by:

$$\Delta \hat{r}_{i,t} = a + b[PEG_{i,t} * KOPEN_{i,t} * \Delta \hat{MS}^{base}_{i,t}] + \Theta C_{i,t} + \mu_{i,t} \tag{3}$$

$$\Delta_h y_{i,t+h} = \alpha^h_i + \beta^h \Delta \hat{r}_{i,t} + \theta^h X_{i,t} + \varepsilon^h_{i,t} \tag{4}$$

where $\Delta \hat{MS}^{base}_{i,t}$ corresponds to movements in base country rates unexplained by observable controls. The latter include the current and lagged values of macroeconomic aggregates and the lagged values of the policy rates.[7]

[5] The controls include the contemporaneous and two lags of real per capita GDP growth; the CPI inflation growth rate; real consumption growth; government expenditure growth; real investment growth, stock price growth; house price growth; total factor productivity growth; the change in commercial openness; the change in the ratio of loans to the non-financial private sector to GDP; and world GDP growth.

[6] We discuss in subsection 3.3 the adopted base countries, which are allowed to vary over time.

[7] $\Delta \hat{MS}^{base}_{i,t} = \Delta i^{base}_{i,t} - \Delta \hat{i^{base}_{i,t}}$, where $\hat{i^{base}_{i,t}}$ is the fitted value of a simple linear model estimated by OLS.

The third motivation for using LP is that it easily accommodates non-linearities.[8] This feature allows us to enrich our analysis by checking whether the IRFs of the top income share to a short-term rate shock are state-dependent. This is of great interest because (i) we use historical data that cover different monetary policy regimes, and (ii) it also follows many studies that highlight that the effects of monetary policy vary over the business cycle. In practice, we extend equation 4 and condition the effect of interest rates on the top income variable by a state variable:

$$\Delta_h y_{i,t+h} = \alpha_i^h + \beta_1^h \Delta \hat{r}_{i,t} * State_{i,t} + \beta_2^h \Delta \hat{r}_{i,t} * (1 - State_{i,t}) + \theta^h X_{i,t} + \varepsilon_{i,t}^h \tag{5}$$

where $State_{i,t}$ is a variable indicating a specific regime (i.e., business cycle, the inflation regime, credit cycles, stock return cycles and monetary policy stance).

3 Data description

3.1 Top income shares

Top income data are extracted from the World Inequality Database (WID, 2019).[9] Specifically, the main variable of interest is operationalized by the top one percent's pre-tax national income share (P1) in 12 OECD economies over the 1920-2016 period.[10] The countries considered include Australia, Canada, Italy, Germany, Denmark, France, the U.K., Japan, the Netherlands, Norway, Sweden and the U.S. As noted by Roine and Waldenström (2015), Leigh (2011) and Roine et al. (2009) among others, top incomes present important heterogeneity: the lower parts of the top decile consist of the "*upper middle class*" (high-income wage earners) with stable income shares over time, while those at the top mainly receive capital shares and feature much larger fluctuations. That is why we separate P1 from the bottom nine percentiles of the top decile and test our model on P09, which is the income share of the top 10% less that of the top 1%. Figure A2 in the Appendix plots for each country P1 and P09 over the studied period. In addition, bearing in mind that the income share going to the 0.1% and 0.01% richest grew even faster – notably in the U.S. and Anglo-Saxon countries – than that of P1 (see, e.g., Saez and Zucman (2016)), we extend our analysis by checking how changes in monetary conditions affect the *ultra-rich*. To do so, we mobilize data on the share of national income held by the top 0.1% and 0.01% from Atkinson and Piketty (2014). Another important exercise consists of

[8]The VAR literature also offers some solutions to deal with non-linearities. However, the richer structure of the VAR model entails several complications in computing IRFs, which often makes the estimation intractable in practice if we are outside the baseline framework.

[9]The definition of income includes labor as well as business and capital incomes.

[10]We conduct our empirical analysis while excluding the years of WWII from the sample. Table A1 in the Appendix traces out the data sources and their availability for each country.

comparing the effect of monetary policy on top income shares with the entire income distribution using a synthetic measure of inequality, i.e., the Gini index, which is obtained from the Standardized World Income Inequality Database (SWIID) of Solt (2020).[11] A notable difference between WID and SWIID is that the latter offers data based on disposable income, thereby allowing us to account for redistributive transfers. Finally, we also check how monetary policy affects changes *within* the top of the distribution using the P1/P09 ratio.

3.2 Macroeconomic variables

We exploit the Jordà-Schularick-Taylor (JST) Macrohistory Database, which provides us with a long series of macroeconomic data. In this database, information on several macroeconomic variables is available from 1870 to 2016 and covers 17 developed economies.[12] In addressing the question of monetary policy and top incomes, our paper also departs from the existing literature by building on several macroeconomic controls. The latter are important determinants of top incomes or more generally perceived by the literature as the main drivers of income inequality.

The set of specific control variables used for both LPs and the instrumental variable are summarized in Table A2 (see the Appendix). They cover financial development, globalization, government spending, technological progress and global shocks. The way in which financial development — considered in our paper by the ratio of total loans to GDP -- shapes top incomes remains an open question. While it was widely believed that it would reduce inequality through better access to credit for low-income households, recent findings (see De Haan and Sturm (2017) for a review) argue, on the contrary, that more finance mainly favours top income shares. In this respect, stock prices are also included because top incomes are highly exposed to the dynamics of financial markets (see, e.g., Kuhn et al. (2019)). Aside from financial development, real estate has become a strong factor in driving income inequality. As argued by Dustmann et al. (2018), shifts in housing costs in Germany severely exacerbated the rise in income disparities net of housing expenditures. For this reason, we control for this factor by adding a housing price index. Regarding globalization, Jaumotte et al. (2013) demonstrate, for a panel of 51 countries, that its effect on the income distribution has two offsetting tendencies: while trade globalization is associated with a reduction in inequality, financial globalization is associated with its increase. We control for the first using the ratio of imports and exports to GDP. The ratio of government expenditure to GDP is also included in our control variables. In

[11] The time coverage of the Gini coefficients is fairly shorter than that of our baseline top income variable.
[12] Our sample is restricted only because of the limited availability of top income data.

fact, based on a political economy model and an empirical analysis using data on OECD countries, Azzimonti et al. (2014) show that governments choose higher levels of public spending when inequality increases. Moreover, technological change has been repeatedly identified in the literature as playing a potent role in widening the gap between top and bottom income earners (see Acemoglu (1998) and Card and DiNardo (2002), among others). A standard way to control for this factor consists of mobilizing data on total factor productivity (TFP) per hour worked, which are obtained from Bergeaud et al. (2016). Finally, we include world real GDP growth to account for global business cycle effects.

3.3 External instruments

As discussed above, our IV strategy – based on the trilemma faced by policymakers – identifies exogenous changes in domestic monetary conditions resulting from estimated surprise movements in the base country interest rate ($\Delta M\hat{S}_{i,t}^{base}$). Therefore, our sample implicitly includes three subpopulations: the first group concerns *base* countries whose monetary policy is relatively autonomous; the second group contains *pegs*, that is, economies that import monetary policy and for which the *base* country's currency serves a *focal anchor*; and the last group relates to *floats*, which are the economies allowing their currency to be determined freely in the market. This subsection briefly discusses the definition of base countries, the source for applicable exchange rate regimes and the adopted capital mobility index.

In the interwar period, we follow Obstfeld et al. (2004) in using a hybrid "gold standard" short-term interest rate, which is a combination of U.S. and French rates. Similar to Jordà et al. (2020), we consider the U.S. as the base country for the entire panel in the Bretton Woods (BW) regime except for Australia, which is associated with the sterling bloc. The same selection is implemented during the post-BW period for the dollar bloc (Australia, Canada, Japan, Norway), while Germany is the base country after 1973 for the remaining European economies. The definitions of *pegs* prior to WWII are extracted from Obstfeld et al. (2004, 2005) and follow Ilzetzki et al. (2019) after WWII. Tables A3 and A4 in the Appendix list for each period and country of our sample the base countries along with the applicable exchange rate regime. The indicator for capital mobility status builds on the index (which ranges from 0 to 100) initially introduced by Quinn et al. (2011). As in Jordà et al. (2015) and Jordà et al. (2020), we use this index rescaled to the unit interval, with 0 meaning fully closed and 1 fully open. Figure A3 in the Appendix plots, for our panel, changes in home interest rate $\Delta r_{i,t}$ against the constructed LP-IV.

4 Local projection results

4.1 Baseline

Our empirical setup builds primarily on LP estimation, along with an identification strategy of monetary surprises that is consistent with the historical context of this study. The first step is to assess the strength of our instrument. To do so, we estimate, in the context of equation 3, a first-stage regression of changes in the short-term interest rate on the instrument $z_{i,t}$ and the aforementioned macroeconomic controls, including country fixed effects. The first-stage regression results are reported in Table 1 and underline the importance of the pass-through from base to home interest rates over several periods. The coefficient estimates of the instrument $z_{i,t}$ are statistically significant at the 1% level and range between 0.52 and 0.58 from year 0 up to year 5, both across the full and post-WWII samples. Similarly, the F-statistics feature high values across samples. Note that Stock et al. (2002) recommend a threshold of 10 for the first-stage F-statistic. Thus, we can now proceed to analyse the LP responses of the top one percent income share to exogenous fluctuations in the short-term interest rate.

The results obtained from the estimation of equation 4 by LPs are presented in Figure 1. The four graphs illustrate IRFs (in percentage points) of P1 – relative to their initial value in year 0 – to a normalized +100 b.p. increase in the short-term interest rate, with the associated 90% confidence bands, which are constructed from clustered-robust standard errors. The impulse responses of P1 are reported using both the instrumental variable and OLS for the full and post-WWII samples.

An initial glance at the IRFs suggests that monetary tightening significantly and durably decreases the share of national income held by the top one percent.[13] Inasmuch as our empirical model is linear, the exact opposite effect holds with respect to monetary easing. Precisely, an exogenous increase of +100 b.p. in the short-term interest rate via the instrument (graph (a) on the left) reduces P1 by 0.4 percentage points three years after the shock. This effect is economically considerable, given that the average of P1 across the sample over the studied period amounts to 10 percent. The post-WWII sample follows a similar path, but the effect on P1 over a five-year horizon is smaller (graph (b) on the right).[14] The negative effect on P1 is, interestingly, more than halved across both samples when using an OLS estimation: a

[13]The persistence of our results is particularly consistent with the recent evidence of Jordà et al. (2019) indicating that monetary policy – based on a historical panel data for 17 advanced economies and using the macroeconomic policy trilemma to identify monetary surprises – affects TFP, capital accumulation, and output for a very long time.

[14]This result also holds for the post-Korean war sample presented in Figure A6 of the Appendix, which corresponds to the last episode of large spikes in government spending due to wars.

short-term interest rate shock in graph (c) reduces P1 by 0.19 and 0.16 percentage points four and five years following the shock, respectively.

These differences are clearer in Table 2, which jointly reports coefficient estimates of OLS and LP-IV. We compare the results obtained by the two methods to assess the degree of attenuation bias in the OLS estimation. In doing so, we notice that the impulse responses under both methods exhibit relatively similar patterns. However, the coefficient estimates obtained via OLS are less statistically significant and substantially smaller than those produced by the IV. Note that Jordà et al. (2020) document the same observation and of a fairly similar magnitude. How should one account for such discrepancy between the OLS and LP-IV coefficient estimates? Some limitations of OLS regression may be at work and explain this contrast. As noted in section 2, given that monetary policy is not driven by top incomes, simultaneity bias is not a concern. However, both variables are affected by economic conditions, some of which may be omitted from the set of control variables.

Finding that monetary tightening decreases P1 over a long time span sets our paper apart from the literature. Particularly, our baseline result contradicts the cross-country evidence of Furceri et al. (2018), who document that contractionary monetary policy has negative distributional effects for a panel of 32 advanced and emerging market countries over the period 1990-2013.[15] Hence, it is important to understand what explains such differences. First, we demonstrate that our result holds for a shorter sample period (see Figure A4 in the Appendix).[16] Second, we estimate equation 4 using the benchmark income inequality indicator considered by Furceri et al. (2018), i.e., the Gini index. Graphs (a) and (b) in Figure 2 indicate – as in Furceri et al. (2018) – that contractionary monetary policy increases the Gini index for market and disposable incomes. This is a fairly standard result, but it has two important implications: (i) our difference with respect to Furceri et al. (2018) does not depend on the sample period or the identification strategy, but it is most likely driven by the sample of economies considered; (ii) the impact of monetary policy on P1 is not necessarily mirrored on the entire income distribution. On the one hand, this is perhaps because the Gini index attaches greater importance to households in the middle of the distribution, who are likely to become unemployed following monetary tightening. On the other hand, unlike tax-based estimates from WID, SWIID relies on survey data and therefore features a lower representation of top income households.

[15] The identification shock scheme adopted by Furceri et al. (2018) is difficult to replicate in the context of our study because (i) our country sample is much smaller than theirs (ii) and macroeconomic forecasts offered by *Consensus Economics* are only available as of the 1990s.

[16] Such evidence should be cautiously interpreted, as Herbst and Johannsen (2020) recently find that impulse responses from LPs can be severely biased in the context of small sample sizes.

Given the heterogeneity in the top decile, we decompose the effect of monetary policy on three distributional indicators presented in the data section: the national income shares held by the top 9% (P09), 0.1% (P01) and 0.01% (P001). The impulse responses displayed in graphs (c) and (d) are in sharp contrast to the baseline result, as they report a positive, albeit not statistically significant, effect on P09.[17] A plausible explanation for this finding is that provided by Roine et al. (2009), who find that changes in per capita GDP growth yield opposite effects for the top percentile (P1) and the bottom of the top decile (P09). In this respect, it can be assumed that output losses following monetary tightening strongly spill over onto capital income and performance-related payments (stock options, bonus programs, etc.), which constitute a significant share of P1's total income – this assertion is also confirmed for the *ultra-rich*, where capital income is bound to be larger (see the responses of P01 and P001 shown in graphs (e) to (h)). In contrast, P09 is much less linked to developments in the economy because highly salaried workers hold smaller capital shares and are arguably more protected by labor market settings, which makes such income groups less sensitive to unanticipated changes in the monetary policy stance. Hence, our empirical analysis reveals that the effect of monetary policy on top income shares would depend on the segment that is examined: income shares going from the top 1 to 0.01 percentiles or the residual part of the top decile (P09). Another useful exercise on distributional measures can check how monetary policy affects income changes *within* the top of the distribution. Figure A5 in the Appendix depicts the responses of the P1/P09 ratio for the full and post-WWII samples, suggesting that monetary tightening narrows the gap among top decile households.

4.2 Robustness checks

The identification strategy adopted to estimate dynamic causal effects obviously requires checking the reliability of the instrument $z_{i,t}$. In the first step, we overidentify the estimated model by including the lag of $z_{i,t}$ as an additional instrument. The IRFs presented in graphs (a) and (b) of Figure 3 are consistent and confirm that the maximum impact on P1 is smaller for the post-WWII sample. Furthermore, the related estimations fail to reject the null hypothesis of the Hansen-Sargan over-identification test, hence suggesting that the exclusion restriction holds. Second, the instrumental variable is constructed using the base country definitions of Ilzetzki et al. (2019), which allows us to exploit different sources of changes in monetary conditions. In fact, Ilzetzki et al. (2019) do not consider, for instance, Norway as belonging to the dollar bloc during the post Bretton Woods era. Graphs (c) and (d) show that the negative effect of

[17] This contrasts with the responses of the top 10% income share (P10), which are likely to be driven by that of P1 (see IRFs reported in Figure A8 of the Appendix).

monetary tightening on P1 is still robust to a different base country definition for both samples. Finally, we control for potential spatial correlations in the residuals by including the jackknifed average of P1 as a control variable. The estimated IRFs are depicted in graphs (e) and (f) of Figure 3 and confirm the stability of our baseline estimate.

Having assessed the strength of our instrumental variable, we perform additional sensitivity analyses on the full and post-WWII samples. These are shown in Figure 4 and include different model specifications plus a pre-crisis analysis. The first test consists of estimating equation 4 with country fixed effects while omitting the rich set of control variables. This exercise is valuable because it is also a way to assess whether the IV exclusion restriction is violated. In fact, a correctly specified instrument would be sufficient to avoid potential endogeneity bias. The evidence depicted in graphs (a) and (b) does not contradict our main result. The second test presented in graphs (c) and (d) reduces the lag number of equation 4 and suggests that the LP framework remains robust to different lag numbers. The third test excludes the base countries from the sample and retains only observations from pegged regimes. The IRFs displayed in graphs (e) and (f) are in line with the main result. Another concern that may arise relates to the fact that our sample period covers episodes where the short-term interest rate reaches the lower bound and becomes inadequate to measure the monetary policy stance. To address this issue, we perform two robustness checks: first, we use long-term interest rates as the monetary policy instrument, and second, the Great Recession period is omitted from the sample. The results are reported from graphs (g) to (j) and show strong consistency with the LP-IV baseline finding. We note, however, that the impact on P1 is not statistically significant in the short run when considering the post-WWII sample.

4.3 Insights on the *income composition channel*

Our baseline result suggested that monetary tightening lowers the share of national income held by the top one percent (P1). That said, it is relevant to investigate one of the underlying transmission mechanisms of monetary policy towards top income shares. Specifically, we demonstrate here that our evidence can support the *income composition channel* introduced by Coibion et al. (2017). That is, considering the heterogeneity in income sources between households, monetary policy will probably affect the income distribution if it disadvantages some types of income. With respect to the top percentile, the idea we would like to support in this paper is straightforward: if the top 1% receive an important share of their total income from capital, then the effect of monetary policy is likely to be channeled through one or several assets' returns. For this purpose, we estimate the effect of monetary tightening on the (real) returns of different financial and real assets. The latter consists of returns on (i) total wealth (weighted average of housing, equity, bonds and bills) and (ii) risky assets (weighted average of housing and equity).[18] We prefer capital returns over stock prices because the former include dividends and rents while the latter are expected to have more impact – through asset valuations – on wealth than on income.

Figure 5 reports the non-cumulated IRFs of asset returns to a normalized +100 b.p. exogenous increase in the short-term interest rate via the instrument for the full and post-WWII samples. The impulse responses depicted in graphs (a) and (b) show that the (real) total return on wealth is lowered by 3.7 percentage points in the full sample, while this reduction is slightly stronger during the post-WWII period. It is also worth noting that unexpected monetary tightening induces a more sizeable reduction in risky asset returns (see graphs (c) and (d) in section 6). This means that monetary policy shocks will have a stronger effect on top income households if they hold a large share of risky assets in their portfolios.

Another way to investigate the *income composition channel* is to estimate the impact of monetary policy on the gap between returns on capital r and the real GDP growth rate of the economy g, i.e., $r - g$.[19] Such an approach makes it possible to appraise whether the negative effect of monetary tightening on (real) asset returns is more than proportional to that on growth. The results reported in Figure 6 yield similar outcomes to the IRFs previously discussed: regardless of whether r is approached through total wealth or only risky assets, a positive interest rate shock narrows the gap $r - g$ for the full and post-WWII samples.[20]

4.3.1 State-dependent effects

The results we have reported thus far support that monetary policy tightening decreases P1 and vice versa. There is, however, a potential pitfall because our sample encompasses very different economic regimes. Moreover, several studies indicate that some economic variables, such as the short-term interest rate, may, for instance, behave very differently during economic downturns. To overcome this limitation, we take advantage of the fact that the LP-IV framework easily accommodates non-linearities. It is convenient to explore whether the effects of changes in monetary conditions on top income shares are state-dependent. Thus, we

[18]These variables are obtained from the Jordà-Scularick-Taylor (JST) Macrohistory database. Their construction and historical dynamics are lengthily discussed in Jordà et al. (2019).

[19]The main idea of Piketty (2014) states that inequality and the capital share of national income systematically move up as $r - g$ grows. However, Piketty considers this gap to have a more potent effect on wealth inequality.

[20]It should be noted that a positive shock affecting the gap $r - g$ increases P1 in our sample. This contradicts Acemoglu and Robinson (2015), who find no correlation between $r - g$ and the top one percent share, which could be explained by the fact that they proxy returns on capital by the yields of long-term government bonds, while we adopt a measure that is more relevant to top incomes (returns on equity, housing, bills and bonds).

allow the impact of monetary policy on the top income variable to depend upon the state of another variable (see equation 5). In this way, we can compute conditional impulse responses in a particular regime to a normalized +100 b.p. increase in the short-term rate.

We consider five factors that potentially lead to different IRFs of monetary policy: the state of the economy over the business cycle, the inflation regime, credit cycles, stock market cycles and the monetary policy stance. The episodes of the business cycle are identified using the HP filter and take value one in the case of an economic expansion and value zero during recessions. The same approach is adopted to identify credit and stock market surges/slowdowns. With respect to inflation, a high-inflation episode is defined as a period during which inflation is above its country-specific fourth quartile. Conversely, a country features a low-inflation regime when inflation is below its first quartile. With regard to the monetary policy stance, we define a binary variable taking value one when there is positive variation in the short-term interest rate (i.e., monetary tightening) and 0 in the case of negative variation (i.e., monetary easing). Finally, we check whether the responses in the aforementioned regimes are significantly different from each other by conducting a Wald Chi-squared test.

Figure 7 reports the IRFs estimated with the state-dependent effect model and the instrumental variable (equation 5) for the first 4 factors previously described. Overall, the displayed IRFs do not conflict with the previous results: the effect of monetary policy on P1 continues to hold, *irrespective of the state of the economy*. As shown in graphs (a) and (b), monetary policy has more immediate effects on P1 during expansions than during recessions. This is an expected outcome, as Jordà et al. (2020) find that the effect of monetary policy on output is quite strong in booms but considerably weaker in slumps. Interestingly, it also appears from graphs (c) and (d) that monetary tightening has a strong effect in the medium run under a high-inflation regime. This makes sense considering that inflation itself is a redistributive tool, which, according to Paarlberg (1993) *"...steals from widows, orphans, bondholders, retirees, annuitants, beneficiaries of life insurance, and those on fixed salaries, decreases the value of their incomes"*.

In addition, the impulse responses presented in graphs (e) and (f) show that the impact of changes in short-term rates on P1 is not affected by credit cycles. In fact, during episodes of credit booms and slumps, restrictive monetary policy produces very similar impacts on P1. Another non-linear experiment addresses the idea that during periods of high volatility in stock markets, investors are less willing to hold stocks, and the effects of monetary policy shocks could be limited. That is why we test the state-dependent effect on P1 in the context of stock market booms/slumps. Graphs (g) and (h) of Figure 7 show that there is no difference in the response to monetary policy regardless of whether stock prices experience a boom or bust episode. However, the response of P1 during stock market slowdowns is not statistically significant, thereby lending some credence to this assumption. Finally, Figure A7 in the Appendix supports our baseline result when considering potential asymmetries between expansionary and contractionary monetary policies. Table A5 in the Appendix, which reports the Wald test results for each regime along with their respective p-values, indicates that the responses in the respective regimes are not significantly different from each other. Overall, this confirms that monetary tightening reduces the national income share of P1 and vice versa for a monetary expansion, regardless of how the economy behaves.[21]

5 Conclusion

This paper sought to investigate the distributional consequences of monetary policy via top income shares between 1920 and 2016 in 12 OECD economies. The central idea that guided this paper's argument is that the existing empirical literature on the distributional effects of monetary policy mainly uses survey-based estimates of income inequality and covers a shorter period. This approach translates into lower inequality estimates – particularly due to the underestimation of business and capital incomes of rich households – and a lower coverage of exceptional macroeconomic occurrences (recessions, sovereign defaults, etc.). We address these shortcomings by studying how changes in the short-term interest rate over a century impacted the share of national income held by the top one percent while controlling for the determinants of inequality and top incomes. To do so, we combined two large datasets: (i) the World Inequality Database (WID) to extract tax-based data on top income shares and the Jordà-Schularick-Taylor (JST) Macrohistory Database, which allows us to access a large series of macroeconomic and financial variables.

Our empirical strategy is based on local projections (LPs) to obtain the impulse responses of top income shares to a normalized +100 b.p. exogenous increase in the short-term interest rate via the instrument. The motivation for this empirical setup is threefold: (i) LP is a "model-free" approach, which allows us to control for several factors that may affect top income shares and, simultaneously, be correlated with monetary policy actions; (ii) it offers a quasi-natural experiment as an identification strategy, where exogenous perturbations to the short-term rate are driven by factors unrelated to domestic economic conditions; and (iii) it easily accommodates non-linearities, thereby allowing us to estimate potential state-dependent effects of monetary policy on the top one percent.

[21] We are unable to conduct such a test for the monetary policy stance because the effect of each regime (i.e., monetary tightening/easing) is estimated in a separate LP specification.

The results obtained from the LP estimates indicate that *tight monetary conditions strongly decrease the top one percent's income share and vice versa for an expansionary monetary policy.* In fact, following a positive perturbation to the domestic short-term rate via the external instrument, the share of national income held by the richest one percent decreases by 0.13 to 0.44 percentage points. This effect is economically sizeable and statistically significant in the medium run. It is also shown that changes in monetary conditions produce a stronger effect on right-tail percentile shares (P1, P01 and P001) than the bottom 9 percent of the top decile. We also demonstrate that this negative effect on top incomes is not reflected in the whole income distribution, as the Gini index (for market and disposable incomes) responds positively to monetary policy shocks. Furthermore, regarding the transmission mechanism of this effect, the reduction in the top one percent's share is arguably the consequence of lower (real) asset returns (on equity, housing and other safe assets), which is consistent with the income composition and indirect income channels.

The baseline results hold under a battery of robustness checks, which (i) introduce an empirical setting that is similar to that of Furceri et al. (2018), (ii) overidentify the estimated model using the lagged term of the instrument, (iii) change the base country definition, (iv) control for spatial correlations in the residuals, (v) remove the rich set of control variables, (vi) test different lag numbers, (vii) estimate the empirical model only on pegged regimes, (viii) use long-term interest rates as the monetary policy instrument and (ix) omit the Great Recession from the sample. Finally, the state-dependent effects version of our model suggests that our conclusions are robust, regardless of the state of the economy.

In future research, we would like to test the effects of monetary policy on different income deciles to focus exclusively on poor and middle-class households (i.e., the bottom 5% or 1% with the lowest market incomes). From the same perspective, are the results obtained here also valid for wealth inequality? This aspect is important because wealth is more unevenly distributed than income. Moreover, while we use pre-tax data, policymakers may be interested in the effects of monetary policy on the income distribution net of the contribution of fiscal policy. Finally, in the spirit of the corresponding literature, the empirical approach adopted in this paper considers only the global effects of monetary policy on the income distribution. That is, we do not identify all the transmission channels through which monetary policy affects top incomes. Ultimately, what policy implications can we draw from these findings for the ongoing debate on monetary policy and the income distribution? Central bankers need to be attentive not only to the aggregate consequences of monetary policy but also to their side effects.

6 Main figures and tables

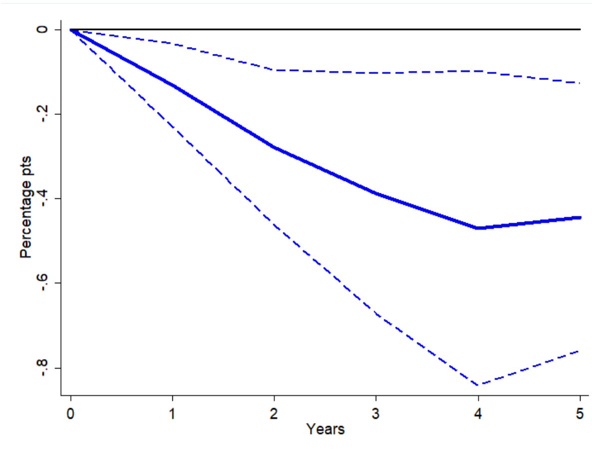
Table 1: Local projection-IV: First-stage results.

Δ ***Short-term interest rate***	Year 0	Year 1	Year 2	Year 3	Year 4
Full sample - IV	0.57***	0.58***	0.55***	0.55***	0.56***
	(0.12)	(0.12)	(0.12)	(0.13)	(0.13)
F-statistic	21.97	23.06	19.94	18.25	18.01
Observations	633	619	607	595	582
Post-WWII sample - IV	0.53***	0.53***	0.52***	0.52***	0.52***
	(0.10)	(0.10)	(0.10)	(0.11)	(0.11)
F-statistics	27.06	27.03	24.90	23.42	21.38
Observations	565	552	540	528	515

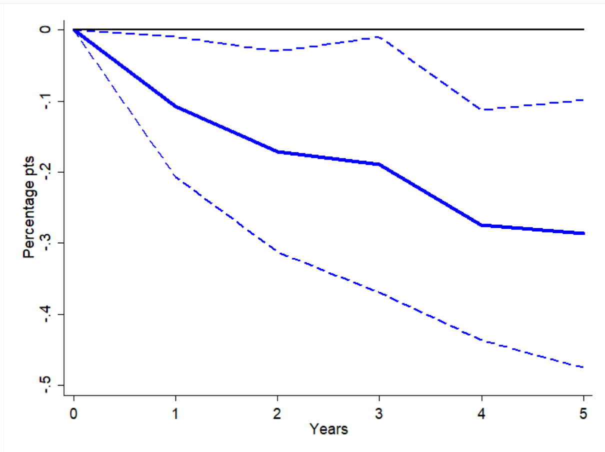
Notes: *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. Country-based cluster-robust standard errors in parentheses. The first difference of the short-term interest rate is regressed on the instrumental variable, using country fixed effects and macroeconomic controls (the change in the short-term interest rate; real per capita GDP growth; the CPI inflation growth rate; real consumption growth; government expenditure growth; real investment growth; stock price growth; house price growth; the level of commercial openness; the change in the loans to non-financial private sector to GDP ratio; total factor productivity growth; and world GDP growth). We include contemporaneous terms and two lags.

Figure 1: Top one percent LPs to a positive short-term interest rate shock

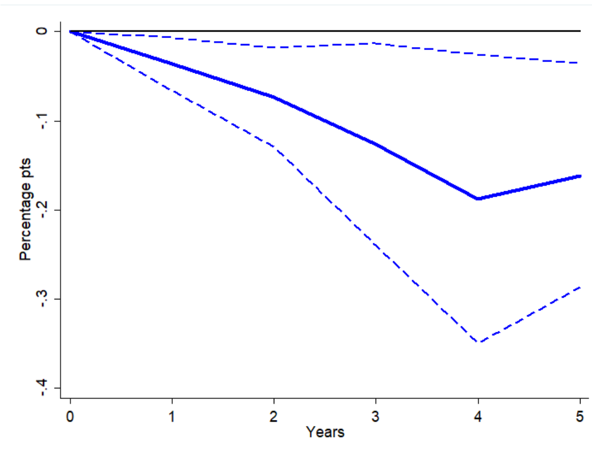
(a) Full sample - IV



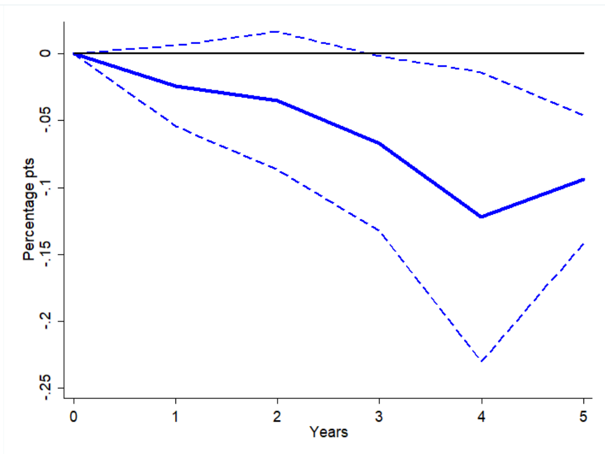
(b) Post-WWII - IV



(c) Full sample - OLS



(d) Post-WWII - OLS



Notes: The figures show the responses (in percentage points) of the top one percent's income share – relative to its initial value in year 0 – to a normalized +100 b.p. increase in the short-term interest rate via the instrument. We report IV and OLS estimates for the full sample along with the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

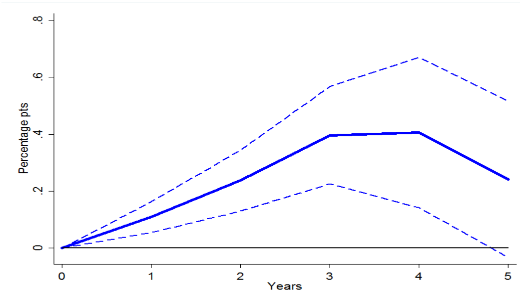
Table 2: Local projection: OLS and IV estimation results

IV estimates - P1	Year 1	Year 2	Year 3	Year 4	Year 5
Full sample					
$\Delta\hat{i}$	-0.13**	-0.28**	-0.39**	-0.47**	-0.44**
	(0.06)	(0.11)	(0.17)	(0.23)	(0.19)
R^2	0.119	0.115	0.124	0.147	0.161
Kleibergen-Paap	4.89	4.89	4.66	4.54	4.47
Observations	633	619	607	595	582
Post-WWII sample					
$\Delta\hat{i}$	-0.11*	-0.17**	-0.19*	-0.27***	-0.29**
	(0.06)	(0.09)	(0.11)	(0.10)	(0.11)
R^2	0.118	0.139	0.168	0.174	0.186
Kleibergen-Paap	4.46	4.49	4.35	4.30	4.11
Observations	565	552	540	528	515
OLS estimates - P1					
Full sample					
Δi	-0.04**	-0.07**	-0.13*	-0.19*	-0.16**
	(0.02)	(0.03)	(0.07)	(0.10)	(0.08)
R^2	0.119	0.125	0.136	0.173	0.211
Observations	656	641	629	617	604
Post-WWII sample					
Δi	-0.02	-0.04	-0.07*	-0.12*	-0.09***
	(0.02)	(0.03)	(0.04)	(0.07)	(0.03)
R^2	0.130	0.158	0.178	0.188	0.206
Observations	565	552	540	528	515

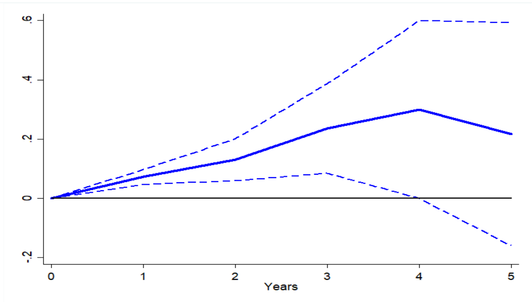
Notes: Country-based cluster-robust standard errors are reported in parentheses below the coefficient estimates. The controls include the twice-lagged terms of (i) the change in the short-term interest rate; (ii) the change in top income share; and the contemporaneous and twice-lagged terms of (iii) real per capita GDP growth; (iv) the CPI inflation rate; (v) stock price growth; (vi) real per capita consumption growth; (vii) the level of financial development; (viii) the level of commercial openness; (ix) house price growth; (x) government expenditure; (xi) real investment growth; (xii) total factor productivity growth; and (xiii) world GDP growth. We report the Kleibergen and Paap (2006) statistic for weak instruments. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Figure 2: Alternative distributional indicator responses to an interest rate shock

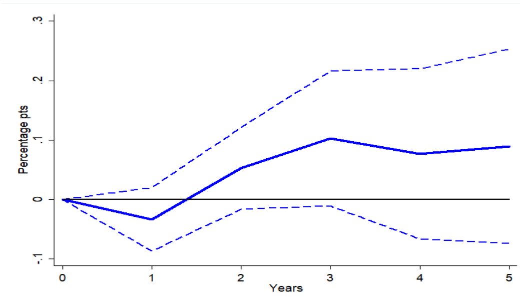
(a) Gini (market income)



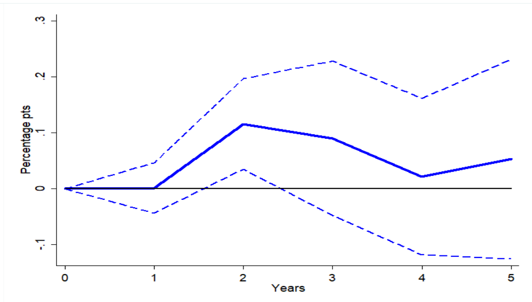
(b) Gini (disposable income)



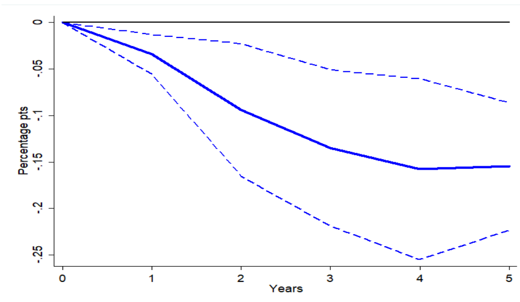
(c) Full sample - P09



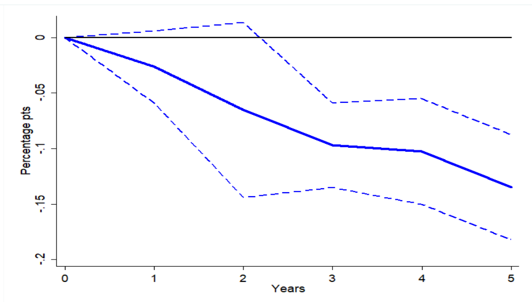
(d) Post-WWII - P09



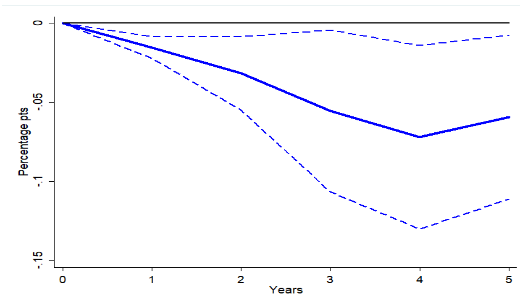
(e) Full sample - P01



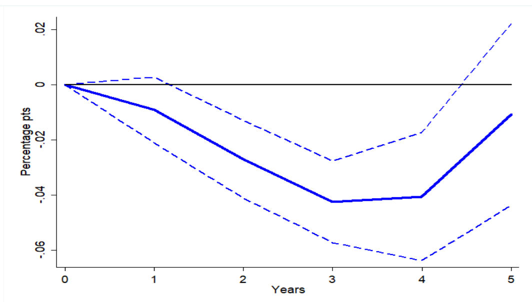
(f) Post-WWII - P01



(g) Full sample - P001



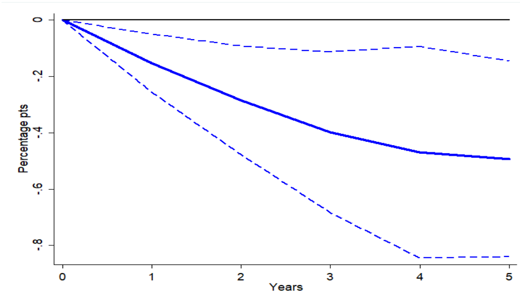
(h) Post-WWII - P001



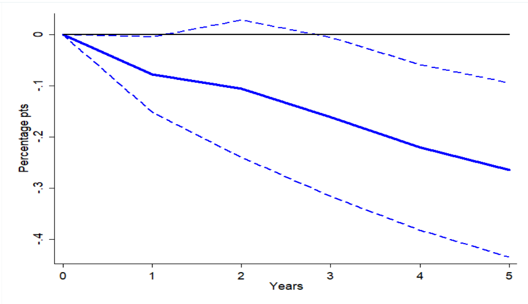
Notes: The figures show the responses (in percentage points) of various distributional indicators – relative to their initial values in year 0 – to a normalized +100 b.p. increase in the short-term interest rate via the instrument. We report LP-IV results for the full sample along with the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure 3: Top one percent LP-IV responses to interest rates: Instrument robustness

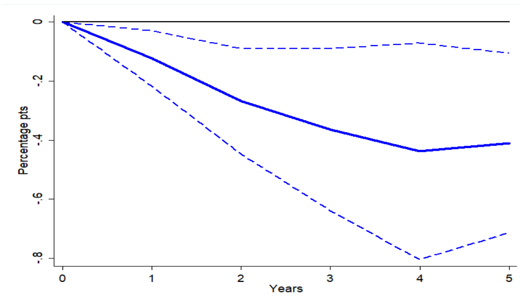
(a) Full sample - Model overidentification



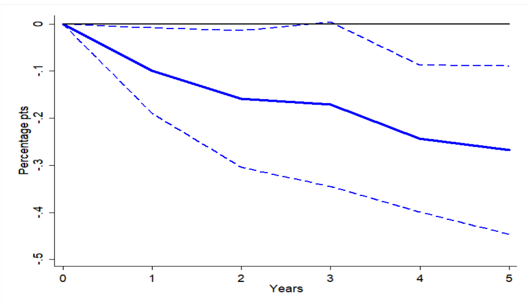
(b) Post-WWII - Model overidentification



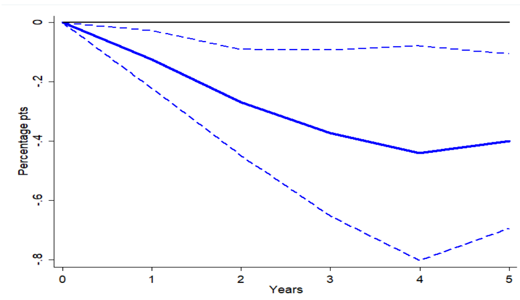
(c) Full sample - Base country definition



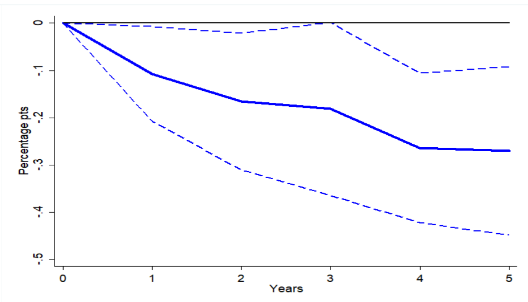
(d) Post-WWII - Base country definition



(e) Full sample - P1 average



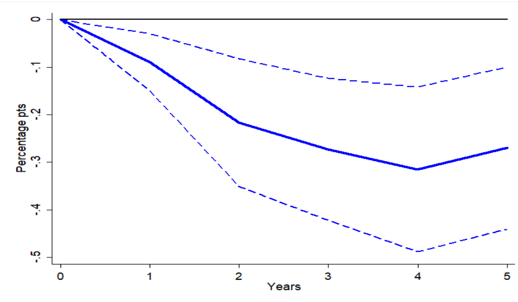
(f) Post-WWII - P1 average



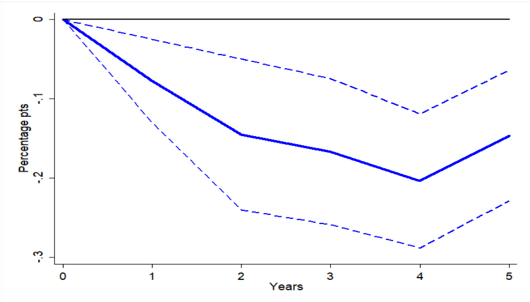
Notes: The figures depict the responses (in percentage points) of P1 – relative to its initial value in year 0 – to a normalized +100 b.p. increase in the short-term interest rate via the instrument. We report LP-IV results for the full sample along with the post-WWII period. In figures (a) and (b), the short-term interest rate is instrumented by the contemporaneous and lagged values of our IV. In figures (c) and (d), the instrumental variable is constructed using the base country definitions adopted by Ilzetzki et al. (2019). Finally, figures (e) and (f) report the jackknifed average of the top one percent's income share to control for potential spillover effects. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure 4: Sensitivity tests

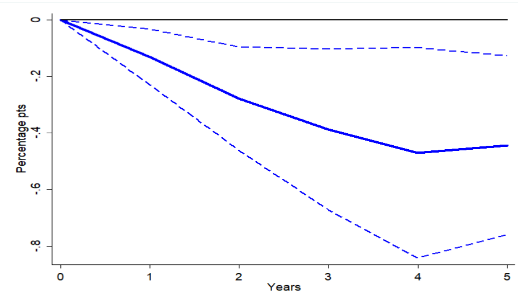
(a) Full sample - No control



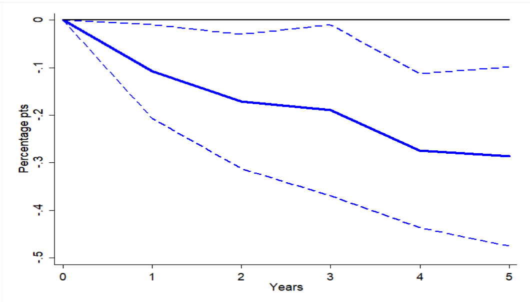
(b) Post-WWII - No control



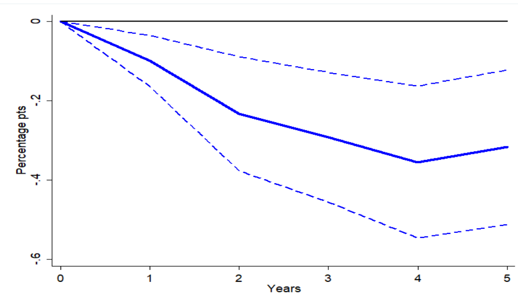
(c) Full sample - Fewer parameters



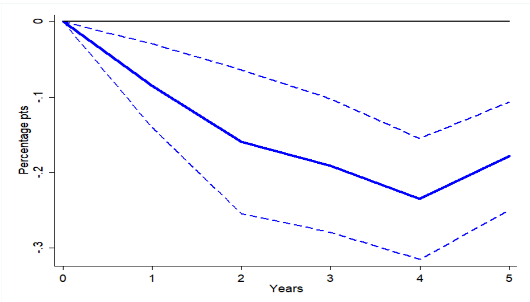
(d) Post-WWII - Fewer parameters



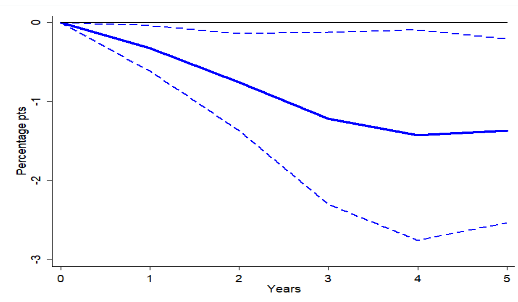
(e) Full sample - Peg subpopulation only



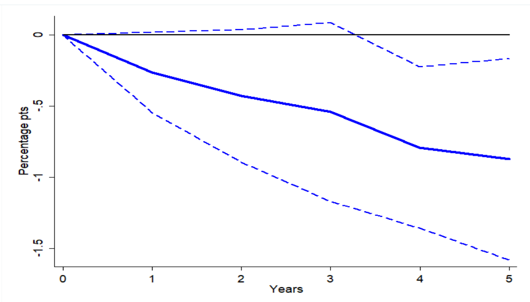
(f) Post-WWII - Peg subpopolution only



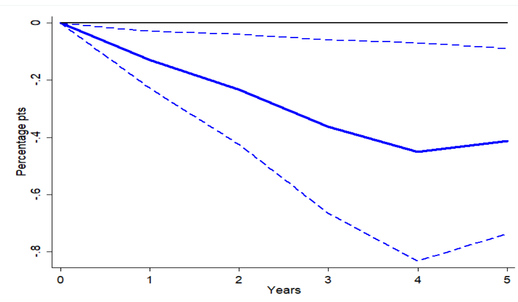
(g) Full sample - LTR



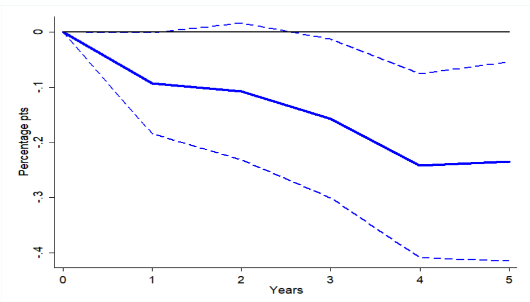
(h) Post-WWII sample - LTR



(i) Full sample - Pre-2007



(j) Post-WWII sample - Pre-2007

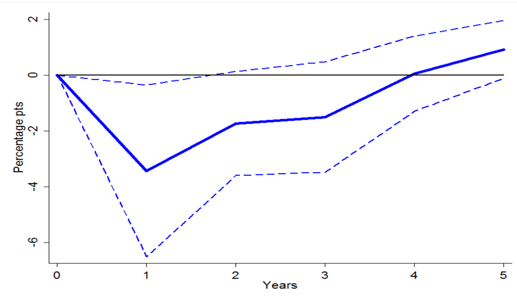


Notes: The figures show the responses (in percentage points) of P1 – relative to its initial value in year 0 – to a +100 b.p. increase in the short-term interest rate. We report LP-IV results for the full sample along with the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

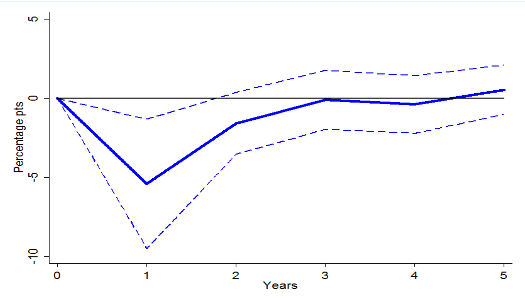
Figure 5: Insights on the *income composition channel*

Total return on wealth

(a) Full sample

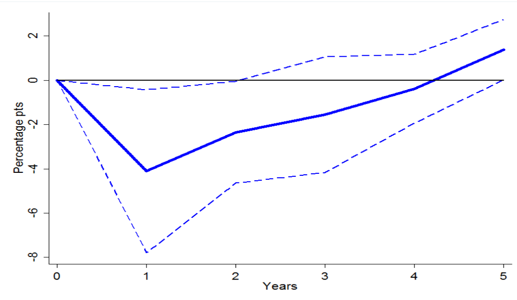


(b) Post-WWII

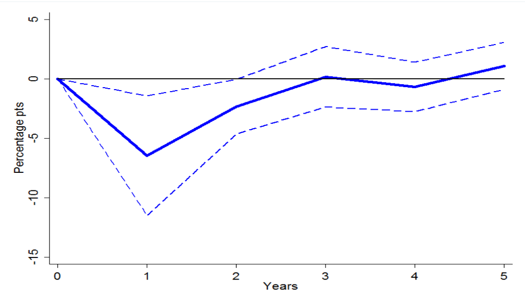


Total return on risky assets

(c) Full sample



(d) Post-WWII

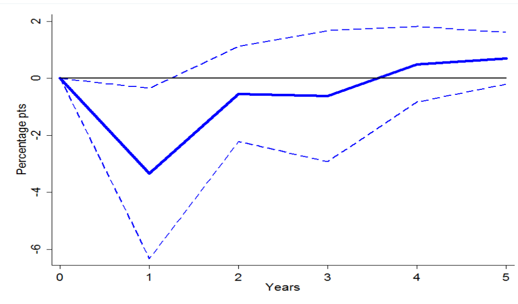


Notes: The figures show non-cumulated IRFs of (real) returns on wealth and risky assets to an unexpected +100 b.p. increase in the short-term interest rate via the instrument. We report LP-IV results for the full sample during the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

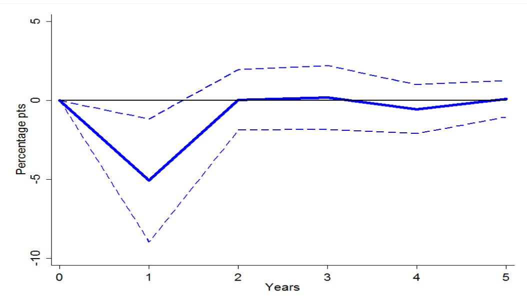
Figure 6: *r* - *g* evidence

Total wealth

(a) Full sample

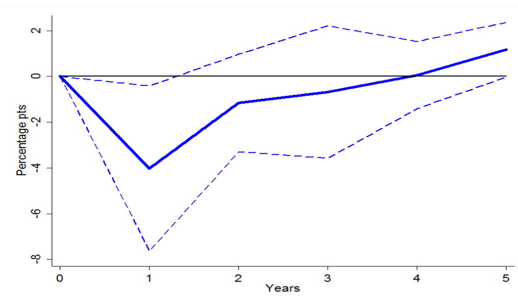


(b) Post-WWII

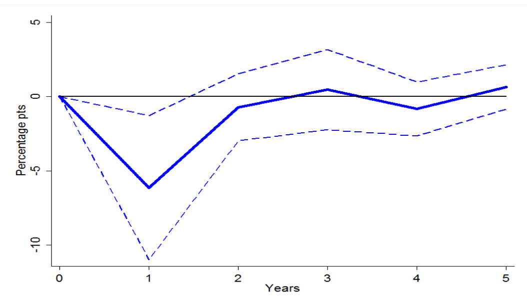


Risky assets

(c) Full sample



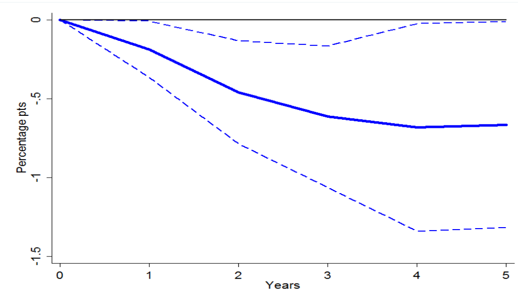
(d) Post-WWII



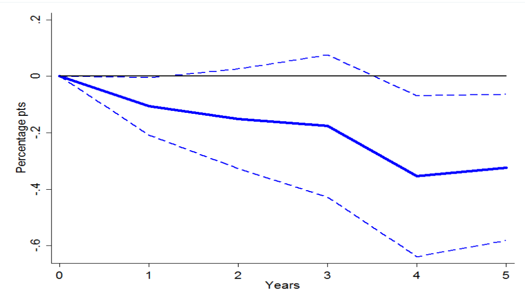
Notes: The figures show non-cumulated IRFs of *r* - *g* (based on wealth and risky asset returns) to an unexpected +100 b.p. increase in the short-term interest rate via the instrument. We report LP-IV results for the full sample during the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure 7: Top one percent LP responses to a positive short-term interest rate shock: state-dependent effects

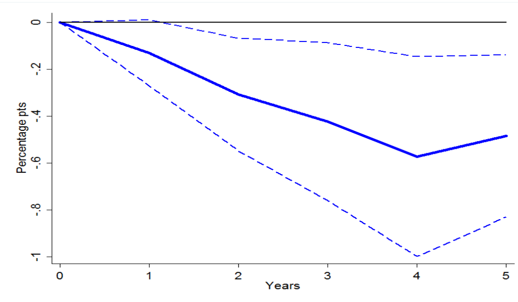
(a) Business cycle boom



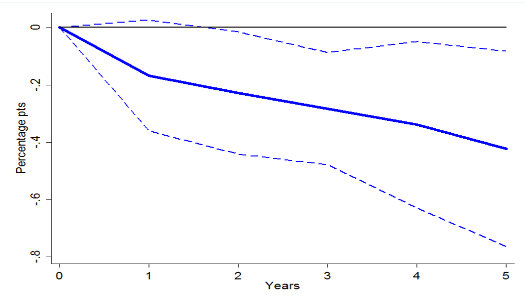
(b) Business cycle slump



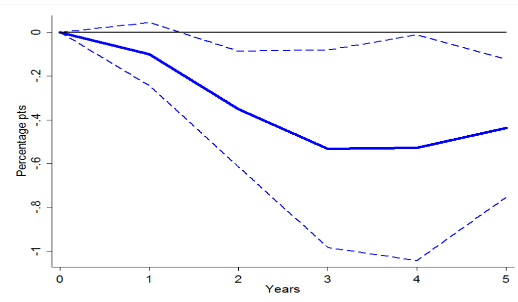
(c) Inflation > to the country's fourth quartile



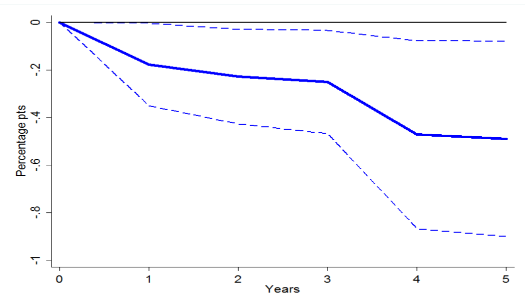
(d) Inflation < to the country's first quartile



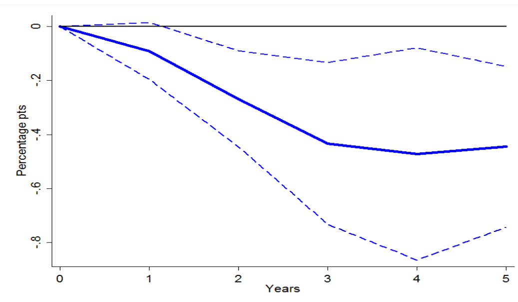
(e) Credit boom



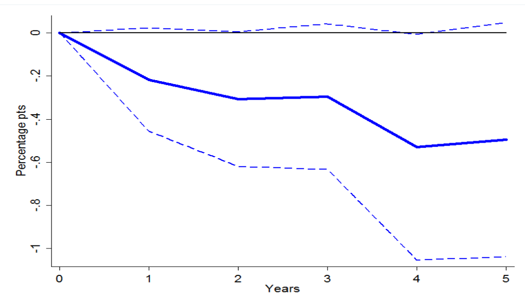
(f) Credit slump



(g) Stock prices boom



(h) Stock prices slump



Notes: The figures show, under several regimes, the responses (in percentage points) of P1 – relative to its initial value in year 0 – to an unexpected +100 b.p. increase in the short-term interest rate via the instrument. The dashed lines represent 90% country-based cluster-robust confidence bands.

References

Acemoglu, D. (1998). Why do new technologies complement skills? directed technical change and wage inequality. *The Quarterly Journal of Economics*, 113(4):1055–1089.

Acemoglu, D. and Robinson, J. A. (2015). The rise and decline of general laws of capitalism. *Journal of Economic Perspectives*, 29(1):3–28.

Alvaredo, F., Atkinson, A. B., Piketty, T., and Saez, E. (2013). The top 1 percent in international and historical perspective. *Journal of Economic Perspectives*, 27(3):3–20.

Atkinson, A. B. and Piketty, T. (2014). *Top Incomes: A Global Perspective*. Oxford University Press.

Atkinson, A. B., Piketty, T., and Saez, E. (2011). Top incomes in the long run of history. *NBER Working Papers 15408*.

Azzimonti, M., de Francisco, E., and Quadrini, V. (2014). Financial globalization, inequality, and the rising public debt. *American Economic Review*, 104(8):2267–2302.

Bergeaud, A., Cette, G., and Lecat, R. (2016). Productivity trends in advanced countries between 1890 and 2012. *Review of Income and Wealth*, 62(3):420–444.

Burkhauser, R., Feng, S., Jenkins, S., and Larrimore, J. (2012). Recent trends in top income shares in the united states: Reconciling estimates from march cps and irs tax return data. *The Review of Economics and Statistics*, 94(2):371–388.

Card, D. and DiNardo, J. E. (2002). Skill-biased technological change and rising wage inequality: Some problems and puzzles. *Journal of Labor Economics*, 20(4):733–783.

Casiraghi, M., Gaiotti, E., Rodano, L., and Secchi, A. (2018). A "reverse robin hood"? the distributional implications of non-standard monetary policy for italian households. *Journal of International Money and Finance*, 85:215–235.

Cloyne, J., Surico, P., and Ferreira, C. (2020). Monetary policy when households have debt: New evidence on the transmission mechanism. *Review of Economic Studies*, 87(1):102–129.

Coibion, O., Gorodnichenko, Y., Kueng, L., and Silvia, J. (2017). Innocent bystanders? monetary policy and inequality. *Journal of Monetary Economics*, 88:70–89.

De Haan, J. and Sturm, J.-E. (2017). Finance and income inequality: A review and new evidence. *European Journal of Political Economy*, 50(December):171–195.

Dustmann, C., Fitzenberger, B., and Zimmermann, M. (2018). Housing expenditures and income inequality. *ZEW-Centre for European Economic Research Discussion Paper*, (48).

Furceri, D., Loungani, P., and Zdzienicka, A. (2018). The effects of monetary policy shocks on inequality. *Journal of International Money and Finance*, 85:168–186.

Guerello, C. (2018). Conventional and unconventional monetary policy and households income distribution. an empirical analysis for the euro area. *Journal of International Money and Finance*, 85(July):187–214.

Herbst, E. and Johannsen, B. K. (2020). Bias in local projections. *FEDS Working Paper No. 2020-010.*

Ilzetzki, E., Reinhart, C. M., and Rogoff, K. S. (2019). Exchange arrangements entering the 21st century: Which anchor will hold? *Quarterly Journal of Economics*, 134(2):599–646.

Inui, M., Sudo, N., and Yamada, T. (2017). Effects of monetary policy shocks on inequality in japan. *Bank of Japan Working Paper Series 17-E-3.*

Jaumotte, F., Lall, S., and Papageorgiou, C. (2013). Rising income inequality: Technology, or trade and financial globalization? *IMF Economic Review*, 61(2):271–309.

Jordà, O., Singh, S. R., and Taylor, A. M. (2019). The long-run effects of monetary policy. *Working Paper WP 2020-09.*

Jordà, Ò. (2005). Estimation and inference of impulse responses by local projections. *American economic review*, 95(1):161–182.

Jordà, Ò., Knoll, K., Moritz, S., and Taylor, A. M. (2019). The rate of return on everything, 1870–2015. *Quarterly Journal of Economics*, 134(3):1225–1298.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2015). Betting the house. *Journal of International Economics*, 96:S2–S18.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2016). Macrofinancial history and the new business cycle facts. *NBER Macroeconomics Annual 2016*, 31(27).

Jordà, Ò., Schularick, M., and Taylor, A. M. (2020). The effects of quasi-random monetary experiments. *Journal of Monetary Economics*, 112(June):22–40.

Jordà, Ò. and Taylor, A. M. (2016). The time for austerity: estimating the average treatment effect of fiscal policy. *The Economic Journal*, 126(590):219–255.

Kuhn, M., Schularick, M., and Steins, U. (2019). Income and wealth inequality in america, 1949-2016. *Opportunity and Inclusive Growth Institute WP n° 9, Federal Reserve Bank of Minneapolis.*

Leigh, A. (2011). Top incomes. In *The Oxford Handbook of Economic Inequality.* Oxford Handbooks Online.

Mumtaz, H. and Theophilopoulou, A. (2017). The impact of monetary policy on inequality in the UK. An empirical analysis. *European Economic Review*, 98:410–423.

Obstfeld, M., Shambaugh, J. C., and Taylor, A. M. (2004). Monetary sovereignty, exchange rates, and capital controls: the trilemma in the interwar period. *IMf staff papers*, 51(1):75–108.

Obstfeld, M., Shambaugh, J. C., and Taylor, A. M. (2005). The trilemma in history: tradeoffs among exchange rates, monetary policies, and capital mobility. *Review of Economics and Statistics*, 87(3):423–438.

Paarlberg, D. (1993). *An Analysis and History of Inflation.* Praeger.

Piketty, T. (2014). *Capital in the 21st Century.* Harvard University Press.

Quinn, D., Schindler, M., and Toyoda, M. (2011). Assessing measures offinancial openness and integration. *IMF Economic Review*, 59(3):488–522.

Ramey, V. A. (2016). Macroeconomic shocks and their propagation. In *Handbook of Macroeconomics*, volume 2, pages 71–162. Elsevier.

Ramey, V. A. and Zubairy, S. (2018). Government spending multipliers in good times and in bad: evidence from us historical data. *Journal of Political Economy*, 126(2):850–901.

Roine, J., Vlachos, J., and Waldenström, D. (2009). The long-run determinants of inequality: What can we learn from top income data? *Journal of Public Economics*, 93(7-8):974–988.

Roine, J. and Waldenström, D. (2015). Long-run trends in the distribution of income and wealth. In *Handbook of Income Distribution*, volume 2, pages 469–592. Elsevier.

Saez, E. and Zucman, G. (2016). Wealth inequality in the United States since 1913: Evidence from capitalized income tax data. *Quarterly Journal of Economics*, 131(2):519–578.

Samarina, A. and Nguyen, A. D. M. (2019). Does monetary policy affect income inequality in the euro area? *Bank of Lithuania WP Series 61.*

Shambaugh, J. (2004). The effect of fixed exchange rates on monetary policy. *The Quarterly Journal of Economics*, 119(1):301–352.

Solt, F. (2020). Measuring income inequality across countries and over time: The standardized world income inequality database.

Stock, J. H. and Watson, M. W. (2018). Identification and estimation of dynamic causal effects in macroeconomics using external instruments. *The Economic Journal*, 128(610):917–948.

Stock, J. H., Wright, J. H., and Yogo, M. (2002). A survey of weak instruments and weak identification in generalized method of moments. *Journal of Business & Economic Statistics*, 20(4):518–529.

Appendix

Table A1: Data sources and periods of top income shares

Country	Period	Details
Australia	1921-2015	WID (2019)
Canada	1920-2010	WID (2019)
Germany	1925-2013	WID (2019)
Denmark	1920-2016	WID (2019)
France	1920-2014	WID (2019)
U.K.	1951-2014	WID (2019)
Italy	1974-2009	WID (2019)
Japan	1920-2010	WID (2019)
Netherlands	1920-2012	WID (2019)
Norway	1948-2011	WID (2019)
Sweden	1943-2013	WID (2019)
U.S.	1920-2016	WID (2019), Atkinson et al. (2015)

Notes: There are some years with missing values in each subperiod.

Table A2: Control variable definitions

Variable	Variable definition	Source
Hpnom	House price growth (real index, 1990=100)	Macrohistory Database JST
Stocks	Stock price index growth (real index)	Macrohistory Database JST
CPI	Consumer Price Index year-over-year growth	Macrohistory Database JST
Tloans	Ratio of total loans to non-financial private sector to GDP	Macrohistory Database JST, own calculations
Com_open	Ratio of imports and exports to GDP	Macrohistory Database JST, own calculations
gdp_pc	country Real GDP per capita (index, 2005=100)	Macrohistory Database JST
cons_pc	country Real consumption per capita (index, 2006=100)	Macrohistory Database JST
Invest	country Real investment growth	Macrohistory Database JST, own calculations
expenditure	government expenditure-to-GDP ratio	Macrohistory Database JST
World_gdp	World real GDP growth	Macrohistory Database JST
TFP	Total Factor Productivity growth	Long-Term Productivity Database
Gini_mkt	Gini index, market income	SWIID (2020)
Gini_disp	Gini index, disposable income	SWIID (2020)
risky	Tot. rtn. on risky assets, Wtd. avg. of housing and equity	Macrohistory Database JST
capital_tr	Tot. rtn. on wealth, Wtd. avg. of housing, equity, bonds and bills	Macrohistory Database JST
eq_tr	Equity total return	Macrohistory Database JST

Notes: This set of control variables has been used in the context of local projections. To ensure stationarity, real indexes are obtained by dividing the variables by CPI, and growth rates are computed in logs.

Table A3: Base countries for the 12 economies

Country	Interwar	Bretton Woods	Post-BW
Australia*	Hybrid	U.K.	USA*
Canada	Hybrid	USA	USA
Germany	Hybrid	USA	Germany
Denmark	Hybrid	USA	Germany
France	Hybrid	USA	Germany
U.K.	Hybrid	USA	Germany
Italy	Hybrid	USA	Germany
Japan	Hybrid	USA	USA
Netherlands	Hybrid	USA	Germany
Norway	Hybrid	USA	USA
Sweden	Hybrid	USA	Germany
U.S.	USA	USA	USA

* Following Jordà et al. (2020), we treat Australia as moving to a U.S. dollar peg in 1967.
Notes: Hybrid refers to the gold standard base, which is a combination of US and French rates. Interwar: 1920–1938; Bretton Woods: 1948–1973; Post-BW: 1974–2016.

Table A4: Exchange rate regimes

Country	Fixed	Floating
Australia	1920-1938, 1946-1983	1939-1945, 1984-2015
Canada	1920-1938, 1946-2015	1939-1945
Germany	1920-1938, 1946-1972, 1999-2014	1939-1945, 1973-1998
Denmark	1920-1938, 1946-2016	1939-1945
France	1920-1938, 1949-2014	1939-1948
U.K.	1920-1938, 1946-2008	1939-1945, 2009-2015
Italy	1920-1938, 1949-2014	1939-1948
Japan	1920-1938, 1948-1977	1939-1947, 1978-2015
Netherlands	1920-1938, 1946-2014	1939-1945
Norway	1920-1938, 1946-2014	1939-1945
Sweden	1920-1938, 1946-2014	1939-1945
U.S.	1920-1938	1939-2016

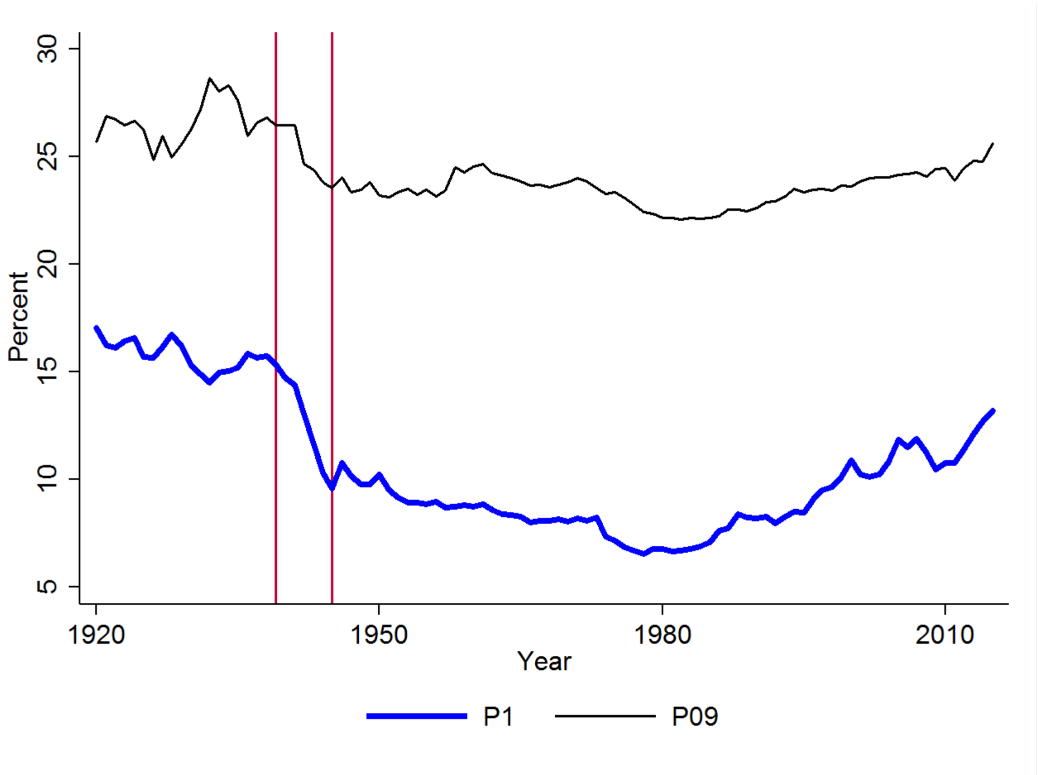


Figure A1: Sample mean of top income shares (P1 and P09)

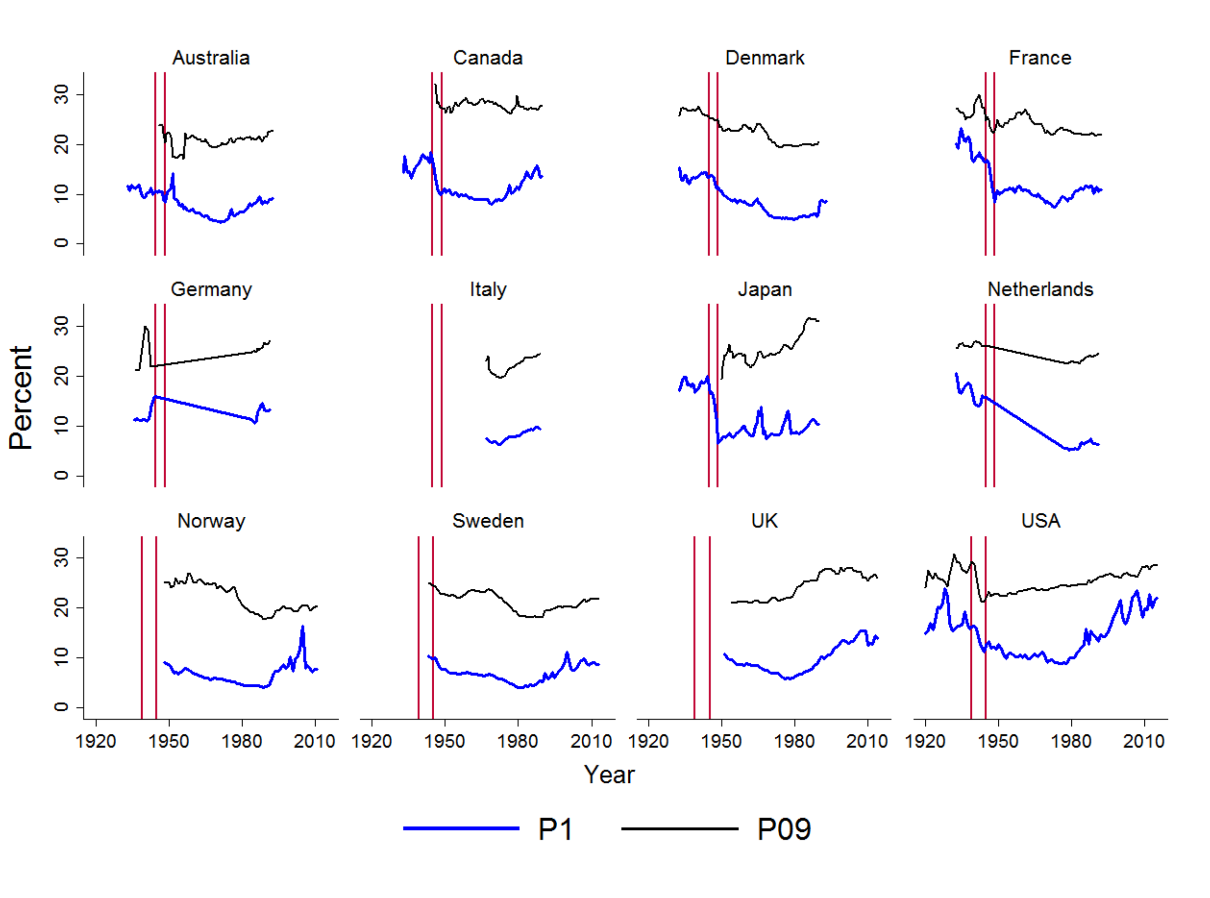


Figure A2: Top income shares over time – 12 countries

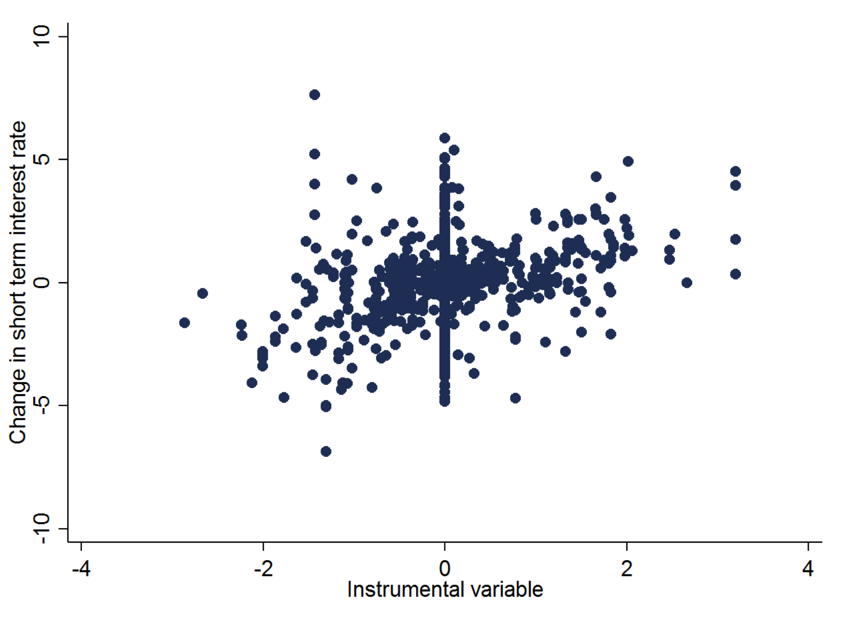
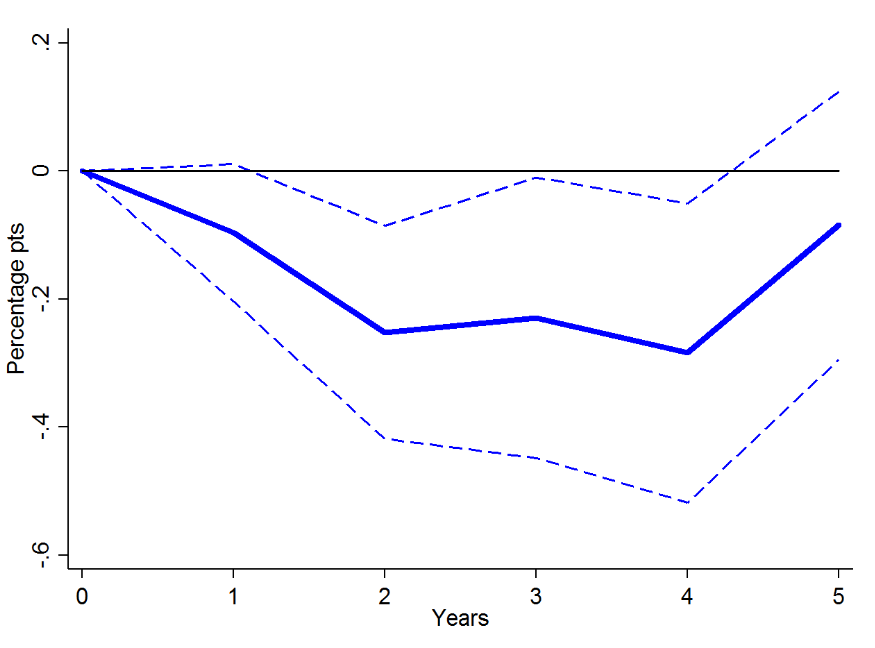


Figure A3: Jorda, Schularick and Taylor-based IV: change in short-term interest rate in home and base countries

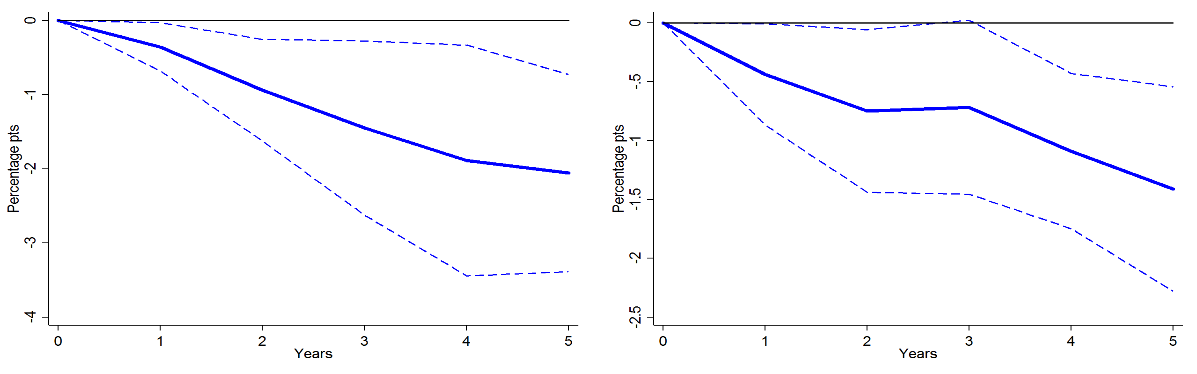
Figure A4: LPs to a positive short-term interest rate shock: post-1980



Notes: The figures show the IRF (in percentage points) of P1 – relative to its initial value in year 0 – to a +100 b.p. increase in the short-term interest rate via the instrument. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure A5: LPs to a positive short-term interest rate shock: P1/P09 ratio

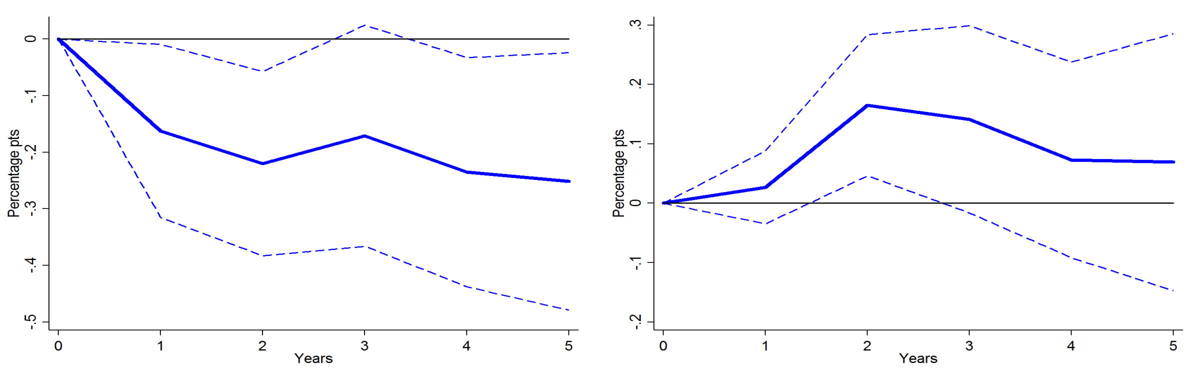
(a) Full sample (b) Post-WWII



Notes: The figures show the responses (in percentage points) of the P1/P09 ratio – relative to its initial value in year 0 – to a +100 b.p. increase in the short-term interest rate via the instrument. We report LP-IV results for the full sample along with the post-WWII period. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure A6: LPs to a positive short-term interest rate shock: Post Korean-war sample

(a) P1 (b) P09

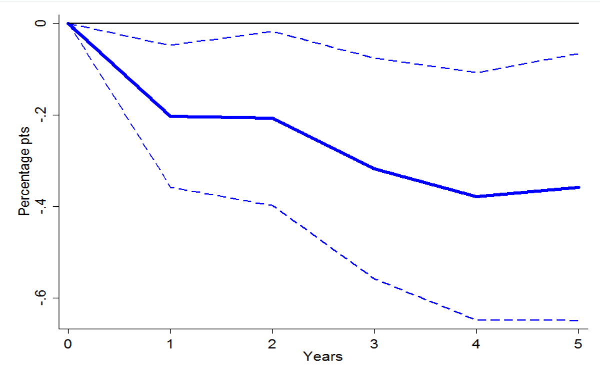


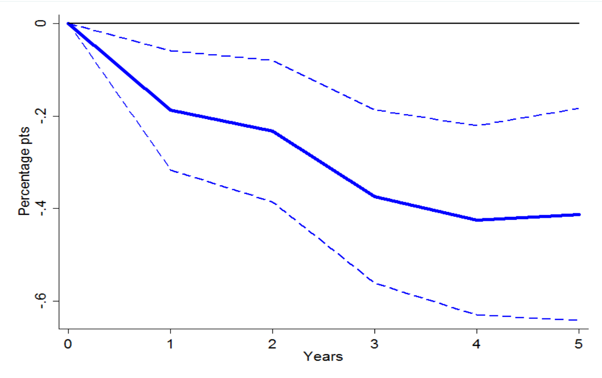
Notes: The figures show the responses (in percentage points) of P1 and P09 – relative to their initial values in year 0 – to a +100 b.p. increase in the short-term interest rate via the instrument. The dashed lines represent 90% country-based cluster-robust confidence bands.

Figure A7: LP state-dependent effects: monetary policy stance

(a) Monetary tightening

(b) Monetary easing





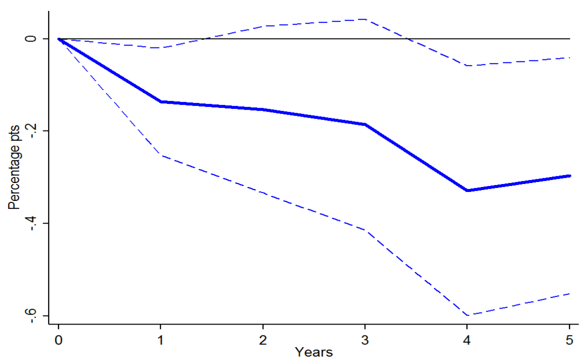
Notes: The figures show the responses (in percentage points) of P1 – relative to their initial values in year 0 – to a +100 b.p. increase in the short-term interest rate via the instrument. The dashed lines represent 90% country-based cluster-robust confidence bands.

Table A5: Wald Chi-squared test of the difference in the cumulated effect of the interest rate shock between the two states

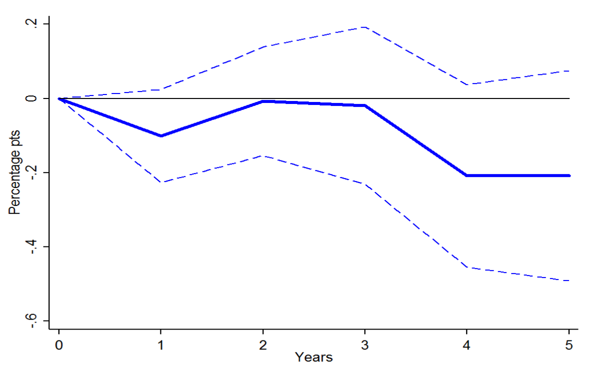
State:	Business cycle	Inflation high/low	Credit boom/slump	Stock prices boom/slump
chi2 Year 5	0.67	0.24	0.09	0.03
Prob Year 5	0.41	0.62	0.76	0.86

Figure A8: LPs to a positive short-term interest rate shock: P10 income share response

(a) Full sample



(b) Post-WWII



Notes: The figures show the responses (in percentage points) of the top 10% income share – relative to their initial values in year 0 – to a +100 b.p. increase in the short-term interest rate via the instrument. The dashed lines represent 90% country-based cluster-robust confidence bands.