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\* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

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

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## Abstract

This paper examines the distributional implications of monetary policy from a long-run perspective with data spanning a century of modern economic history in 12 advanced economies between 1920 and 2015. We employ two complementary empirical methodologies for estimating the dynamic responses of the top 1% income share to a monetary policy shock: vector auto-regressions and local projections. We notably exploit the implications of the macroeconomic policy trilemma to identify exogenous variations in monetary conditions. The obtained results indicate that expansionary monetary policy strongly increases the share of national income held by the top one percent. Our findings also suggest that this effect is arguably driven by higher asset prices, and holds irrespective of the state of the economy.

JEL Codes: D63, E62, E64

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*"All public policy is distributional, be it monetary, fiscal, structural or social. The reason I know this is because redistribution is the way public policy works; it is what policy does for a living. Some policies redistribute resources between agents at a point in time. Others redistribute resources between agents over time. If policy is not working through one of these channels, it is not working."*

Haldane (2018)

## 1 Introduction

The last decades have been marked by a substantial rise in income and wealth inequality across the developed world. Low-income households in advanced economies have seen their wages stagnating, while wealth has never been so concentrated since the dawn of the 20th century. Such patterns have prompted a global narrative on inequality, which has spilled into policymaking circles, especially since uneven income and wealth distribution appear to support excessive household indebtedness (see, e.g., Kumhof et al. (2015) and Coibion et al. (2014)) and consequently fuel financial instability (Rajan, 2010).

The potential distributional effects of monetary policy have recently become an active topic in the inequality debate as a consequence of the unconventional instruments that central banks implemented following the financial crisis (see Colciago et al. (2019) for a complete survey). This was unusual, since it is widely accepted that central banks should not be concerned about inequality. In fact, they are independent of the political process, and therefore, dealing with distributional matters goes beyond their mandate. Nevertheless, the combination of an ultra-low interest rate environment and large asset purchase programs is suspected to have reduced interest income on modest households savings and driven up asset prices. Meanwhile, central bankers in the Eurozone and elsewhere, such as Draghi (2016) or Bernanke (2015), strongly believe that their non-standard monetary policies had modest distributional implications. However, they argue instead that the post-crisis monetary policy toolkit allowed for the restoration of growth and increased employment levels, which primarily favored low-income households. This debate underlines, in the spirit of Coibion et al. (2017), that the outcome of monetary policy on income inequality would be channeled through: (i) households' income composition (some rely primarily on labor income, while

others may receive other forms of revenue as rents or dividends), (ii) heterogeneous effects of business cycle fluctuations with respect to earnings (modest and low-skilled workers are generally the most exposed to unexpected shocks), and (iii) the distribution of assets and liabilities between households (financial assets are held primarily by rich households, which could be the first to benefit from higher asset prices).

In this context, macroeconomic research has been increasingly devoted to analyzing the collateral effects of monetary policy on the income and wealth distributions. As far as theoretical contributions are concerned, they have mainly built on New Keynesian frameworks. Dolado et al. (2018), for instance, emphasized two specific channels: (i) top-income households happen to be high-skilled and experience increasing wages as a consequence of a monetary expansion, since they benefit from lower matching frictions in labor markets; (ii) as these individuals present complementary features to capital, an increase in the demand for the latter only magnifies income inequality in comparison to poor, low-skilled workers. On the empirical side, numerous country-level studies suggest that conventional monetary tightening increases income inequality (see Coibion et al. (2017) for the U.S., Mumtaz and Theophilopoulou (2017) for the U.K., Samarina and Nguyen (2019) for the Euro Area, and Furceri et al. (2018) for a selection of advanced and emerging economies). In contrast, recent research on the distributional effects of unconventional monetary policy mostly shows that the relationship between monetary expansions and inequality is negative, although small in magnitude (see, e.g., Casiraghi et al. (2018), Guerello (2018), Frost and Saiki (2014)).

The existing empirical literature on the relationship between monetary policy and the income distribution focuses primarily on data on inequality over a short period of time. This can be problematic since inequality measures are perceived to be sticky in the short and medium run. Addressing this issue with short-term data also implies giving coverage to fewer macroeconomic events (such as recessions, financial crises or sovereign defaults). Such events result in exceptional monetary policy shocks, which can have an important impact on inequality. However, analyzing the distributional effects of monetary policy from a historical perspective poses two major challenges. The first one is to mobilize reliable cross-country inequality data and long macroeconomic series. The second has to do with the identification of exogenous monetary policy innovations. Deal-

ing with this point is particularly demanding, since the conduct of monetary policy in advanced economies experienced several changes throughout the 20th century. Such shifts relate, for instance, to the succession of different exchange rate regimes, the occurrence of many banking crises and the usage of multiple frameworks in monetary policy decisions (e.g., inflation targeting, the Taylor rule, exchange rate management). Our paper aims at addressing these challenges using a different setting and a novel approach, providing new evidence on the distributional consequences of monetary policy.

This paper analyzes the relationship between monetary policy and income inequality between 1920 and 2015 using annual data across 12 advanced economies: Australia, Canada, Italy, Germany, Denmark, France, U.K, Japan, the Netherlands, Norway, Sweden and the U.S. It was possible to conduct this historical analysis thanks to the combination of two datasets. We mobilize the World Inequality Database (WID), which offers open access to historical series of income and wealth inequality. The share of the national income held by the richest 1 percent is used as the main inequality indicator. The richest 1 percent receive a significant share of their total income in the form of dividends and capital gains while being almost untraceable in household income and wealth surveys. As an alternative inequality measure, we use in our analysis the top 10 percent share of the national income, which is believed to capture well-off households with heterogeneous income sources. Long series of macroeconomic variables are extracted from the Jordà-Schularick-Taylor Macrohistory Database, developed by Jordà et al. (2016). Using such historical macroeconomic data is of great interest because they offer a rich set of control variables that could enter as potential determinants of inequality. To the best of our knowledge, this is the first time that the WID and JST Macrohistory Database have been combined.

We employ two complementary empirical methodologies. The first consists in estimating a Panel VAR with 5 variables — including our inequality measure — to obtain the impulse response functions to an unexpected monetary policy shock. The second generates dynamic responses of inequality from local projections *à la* Jordà (2005). As noted by Barnichon and Brownlees (2018), the two methodologies complement each other. The Panel VAR model yields consistent results when it is correctly specified, but it becomes over-identified when using a large number of endogenous variables. Local Projections (LP) are admittedly

less efficient, but they remain robust to model misspecification. More important, since it does not impose specific dynamics in the equation system, the LP approach allows for greater flexibility in the estimations. We exploit these features for our research question in two ways: (i) we construct an instrumental variable, which leans on the well-known macroeconomic policy trilemma, in order to isolate exogenous fluctuations in the short-term interest rate, and (ii) since local projections accommodate non-linearities, we test our model in a regime-switching setting, where we allow the response of inequality to depend on the regime of a specific variable (i.e., a banking crisis, the inflation regime and the output gap).

Our evidence suggests that monetary policy has a significant impact on income inequality. Monetary loosening increases the share of national income held by the richest 1 percent, while restrictive monetary policy has the opposite effect. As far as the results from the Panel VAR and OLS local projection are concerned, a 100 b.p. decrease in the short-term interest rate implies a peak increase in the top income index of approximately 1 and 1.15 percentage points, respectively. We demonstrate that the effect of monetary policy on top-income households is arguably driven by higher stock prices, which is consistent with the income composition channel. The distributional effects of monetary policy in the instrumental variable setting are, however, more sizable. A perturbation to the domestic interest rate — via the external instrument — considerably widens inequality, with a peak increase of nearly 6 percentage points. The results are robust to a battery of robustness checks to the baseline model.

These findings support the theoretical predictions of Dolado et al. (2018), where the skill distribution across households plays an important role in shaping the effect of monetary policy on inequality. Our evidence is also in line with the empirical findings of Romer and Romer (1999). Although inequality is not the primary concern of central bankers, our results imply that it is a dimension that they should not overlook. This is especially true since the income distribution may affect the transmission mechanisms of monetary policy.

The remainder of the paper is organized as follows: Section 2 discusses the estimation methodology and the identification strategy. Section 3 thoroughly describes the data. The fourth section presents the Panel VAR and local projection results, while the fifth and final section concludes.

## 2 Estimation approach

The following section presents the two well-established empirical methodologies for estimating impulse responses: vector auto-regressions and local projections.

### 2.1 Panel VAR

Structural VARs are the traditional approach to identify structural monetary policy shocks and simultaneously trace out the corresponding impulse responses of macroeconomic variables. We begin here with a traditional small monetary VAR amended to take into consideration the panel nature of our data. This model is extended to include an inequality indicator as well as a dummy for an exogenous systemic banking crisis. Hence, the VAR contains five endogenous variables: the CPI, real GDP, the nominal short-term interest rate, stock prices and the top one percent's share of national income ( $P1$ ). These series enter the model as the log changes to the CPI ( $CPI_{i,t}$ ), real GDP ( $GDP_{i,t}$ ), stock prices ( $S_{i,t}$ ) and P1 ( $P1_{i,t}$ ), while nominal short-term interest rate enters in first differences ( $\Delta r_{i,t}$ ). Let  $X_{i,t} = (\pi_{i,t}, y_{i,t}, r_{i,t}, s_{i,t}, P1_{i,t})$  be a vector of the five endogenous variables in the VAR. The reduced form of the model can be represented as follows:

$$X_{i,t} = \mu_i + \sum_{l=1}^L \beta_l X_{i,t-l} + \kappa D_{i,t} + \nu_{i,t} \quad (1)$$

where the indices  $t$  and  $i$  relate to years and countries;  $\mu_i$  corresponds to country fixed effects;  $L$  represents the number of lags on the endogenous variables included in the model, set at two according to the Akaike information criterion;  $B_i$  are  $5 \times 5$  matrices of unrestricted coefficients;  $D_{i,t}$  is a banking crisis dummy that takes value 1 when there is a systemic banking crisis and 0 otherwise; and finally,  $\nu_{i,t}$  is a vector of unorthogonalized structural shocks.

In order to disentangle the causal chain of events and identify structural shocks of interest, we consider timing assumptions based on contemporaneous restrictions among the exogenous shocks in the VAR. Specifically, our approach departs from the recent literature on the macroeconomic effects of monetary policy. In fact, the latter has relied primarily on the narrative approach of Romer and Romer (2004) to identify innovations to monetary policy. This is notably the approach adopted, for instance, by Coibion et al. (2017) and Furceri et al. (2018) to study the distributional effects of monetary policy. Nonetheless, this

identification strategy is not tractable in our case since it requires (at least) forecasts of short-term interest rates, inflation and GDP growth, which are not available over the long run. Moreover, our interest in the effects of monetary policy on inequality as well as the use of annual data for this purpose reduce the problems raised by monetary policy shocks derived from a VAR. Indeed, it is less likely that the short-term interest rates move endogenously with changes in inequality than with changes in output. Further, anticipatory movements in our monetary policy measure are less plausible with annual than with quarterly data.

However, our identification strategy is not without problems. As a matter of fact, our simple recursive identification scheme generally delivers puzzling dynamic responses of inflation to monetary policy innovations (the so-called “price puzzle”). This counter-intuitive result questions the validity of the estimates of structural monetary policy shocks and, therefore, the accuracy of the derived inequality impulse response function. As a result, the first step consists in addressing the price puzzle.

Numerous solutions have been proposed in the literature, mostly based on the inclusion of additional variables into the VAR. However, these alternatives are not well suited to our context, which is characterized by the use of historical data and the limitation of available time-series. In dealing with the “price puzzle”, the proposal of Estrella (2015) is very attractive because it makes the model simpler and less data demanding. This proposal actually incorporates a theoretically motivated exclusion constraint in the VAR. The constraint is based on the insights of Friedman (1958, 1961) and the empirical observations of Bernanke et al. (1999) and Batini and Nelson (2001), which point to the existence of lags between monetary policy actions and their influence on prices. This suggests imposing a single zero restriction on the coefficients matrix for the first lags of the short-term interest rate in the CPI equation. By doing so, in addition to the traditional short-run restrictions — given by constraining the matrix  $A_0$  to be lower triangular — the dynamic responses of key macroeconomic variables to monetary policy are no longer a puzzle.<sup>12</sup> As a result, the structural form of the estimated model is given by:

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<sup>1</sup>See Table A3 in the appendix for a comparison of the IRFs.

<sup>2</sup>We test other specifications of the model in which we add another zero restriction to account for the fact that changes in monetary policy rates are likely to be independent of changes in inequality.

$$\begin{aligned}
& \overbrace{\begin{pmatrix} 1 & 0 & 0 & 0 & 0 & 0 \\ \alpha_0^{21} & 1 & 0 & 0 & 0 & 0 \\ \alpha_0^{31} & \alpha_0^{32} & 1 & 0 & 0 & 0 \\ \alpha_0^{41} & \alpha_0^{42} & \alpha_0^{43} & 1 & 0 & 0 \\ \alpha_0^{51} & \alpha_0^{52} & \alpha_0^{53} & \alpha_0^{54} & 1 & 0 \end{pmatrix}}^{A_0} \begin{pmatrix} CPI_{i,t} \\ GDP_{i,t} \\ \Delta r_{i,t} \\ S_{i,t} \\ P1_{i,t} \end{pmatrix} \\
& = \mu_i + \sum_{l=1}^L \begin{pmatrix} \alpha_l^{11} & \alpha_l^{12} & 0 & \alpha_l^{14} & \alpha_l^{15} \\ \alpha_l^{21} & \alpha_l^{22} & \alpha_l^{23} & \alpha_l^{24} & \alpha_l^{25} \\ \alpha_l^{31} & \alpha_l^{32} & \alpha_l^{33} & \alpha_l^{34} & \alpha_l^{35} \\ \alpha_l^{41} & \alpha_l^{42} & \alpha_l^{43} & \alpha_l^{44} & \alpha_{l,i}^{45} \\ \alpha_l^{51} & \alpha_l^{52} & \alpha_l^{53} & \alpha_l^{54} & \alpha_l^{55} \end{pmatrix} \begin{pmatrix} CPI_{i,t-l} \\ GDP_{i,t-l} \\ \Delta r_{i,t-l} \\ S_{i,t} \\ P1_{i,t-l} \end{pmatrix} + \kappa D_{i,t} + \varepsilon_{i,t} \quad (2)
\end{aligned}$$

where  $\alpha_l$  are unrestricted structural parameters that are allowed to differ for each country, and  $\varepsilon_{i,t}$  is a vector of uncorrelated iid shocks. As noted above, the impact matrix  $A_0$  is the lower triangular. The variable ordering of this recursive identification scheme implies that inequality contemporaneously reacts to innovations in key macroeconomic variables.<sup>3</sup>

The Panel VAR model is estimated equation-by-equation by a fixed effect estimator. We acknowledge that this method has some flaws, as it can yield biased estimates. In fact, demeaning in a dynamic panel model results in correlation between the error terms and regressors. However, as shown by Nickell (1981), the size of the fixed effects bias decreases as the length of the sample increases. Hence, the importance of this bias in our analysis is small because the time dimension is long and much longer than the country dimension<sup>4</sup>.

## 2.2 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 also estimating impulse response

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<sup>3</sup>At best, this ordering choice is questionable. Hence, we also adopt a more conservative ordering by setting the inequality variable first in the VAR. The result are in this case highly consistent and available upon request.

<sup>4</sup>We also estimate our model by a mean group type estimator. The method consists in allowing all the coefficients to vary by country and using OLS to estimate the model. By doing so, we capture cross-sectional dynamic heterogeneity. The results of these estimates are reported in the appendix and are very consistent with our baseline results.

functions (IRF) from local projections. In its basic form, local projection consists of a sequence of regressions of the endogenous variable shifted several steps ahead. As a result, the approach consists in estimating the following equation<sup>5</sup>:

$$\Delta_h y_{i,t-1} = \alpha_i^k + \beta^k \Delta r_{i,t-l} + \theta^k X_{i,t} + \varepsilon_{i,t}^k \quad (3)$$

where  $\Delta_h y_{i,t-1} = y_{i,t+h} - y_{i,t-1}$  and corresponds to change in the growth rate of the top income share from the base year  $t-1$  up to year  $t+h$ , with  $h = 0, 1, \dots, H$ ;  $\Delta r_{i,t}$  denotes the change in the short-term interest rate; and  $X_{i,t}$  refers to a vector containing the control variables. The control variables include the lags of  $\Delta_h y_{i,t-1}$  and  $\Delta r_{i,t}$  and a rich set of additional controls that theoretically explain inequality and can be correlated with the monetary policy shock<sup>6</sup>.

It is important to note that each step of the accumulated IRF is obtained from a different equation and that it directly corresponds to the estimates of  $\beta^k$ . This means that unlike the VAR approach, the estimated coefficients contained in  $\theta^k$  are not used to build the IRF. They only serve as controls and cleanse the  $\beta^k$  from the effects of past inequality and monetary policy changes, in addition to contemporaneous and past changes in other macroeconomic variables (output and CPI, for instance). In other words, the LP approach does not impose any particular dynamics on the variables in the system. As shown by Jordà (2005), this 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. However, the VAR approach is more efficient when the model is well specified. Hence, the local projection method has complementarity features with the VAR approach to obtain IRs. In what follows, we describe the benefits of local projections with respect to our research question.

First of all, the local projections allow us to control in our results for numerous factors that may influence inequality and be, at the same time, correlated with monetary policy actions. The  $X_{i,t}$  vector includes numerous supplementary variables compared to the VAR: stock price growth, house price growth, the change in financial depth, the change in trade openness, government expenditure growth and U.S. patent number growth as a proxy for technological progress.

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<sup>5</sup>The index  $l$  refers to the lag length, which is set to 2.

<sup>6</sup>Time-fixed effects are not included as it requires estimating hundred additional parameters.

The second benefit of local projection is that it offers an original identification strategy to estimate dynamic causal effects. To build shock series, our strategy relies on external instruments. These variables are 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, which borrow from microeconomics, have recently attracted growing interest in applied macroeconomics (Jordà et al., 2015, 2017; Jordà and Taylor, 2016; Ramey and Zubairy, 2018; Stock and Watson, 2018). To be more specific, our approach tackles the issue of joint determination of economic policies and macroeconomic outcomes that tends to bias estimates of the effects of economic policies. Regarding our research question, we note that monetary policy is not likely to be driven by inequality and, therefore, that the dynamic causal effect is clear (no simultaneity bias). In such a case, using external instruments would be worthless. However, even if inequality is not a target of central banks, both inequality 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 calls for the use of exogenous (to domestic economic conditions) monetary policy shocks rather than short-term interest rates. As is widely agreed in the literature, the major challenge — even for a macro issue like ours — is to find external factors that would make the monetary policy shock a random treatment.

Here, we use the local projection-instrumental variable (LP-IV) method proposed by Jordà et al. (2015), Ramey (2016) and Jordà et al. (2017). We couple this method with the identification strategy of external variations in monetary conditions based on Jordà et al. (2015) and Jordà et al. (2017). The purpose here is to use the macroeconomic policy trilemma to find external variations in monetary policy conditions. The macroeconomic trilemma states that a country cannot simultaneously achieve free capital mobility, a fixed exchange rate and independent monetary policy. By pursuing any two of the goals, it is necessary to abandon the third. Building on the trilemma framework (Obstfeld and Taylor, 1998, 2004 ; Shambaugh, 2004), we trace out episodes where external conditions can generate exogenous perturbations of the short-term interest rate. The latter

are considered to be unrelated because the base country — for example, the U.S. in the Bretton Woods era — does not internalize the externalities of its own policy choices on partner countries. The trilemma links the domestic interest rate with the base country interest rate. A simple algebraic expression is given by:

$$\Delta r_{i,t} = a + b[PEG_{i,t} * KOPEN_{i,t} * \Delta r_{i,t}^{base}] + \Theta X_{i,t} + \mu_{i,t} \quad (4)$$

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, and  $X_{i,t}$  is a vector of macroeconomic controls in country  $i$  at time  $t$ .

According to equation 4, variations in  $\Delta r_{i,t}$  are related to external conditions (the base country) when there is perfect mobility of capital and a fixed exchange rate regime. Given this natural pseudo-experiment, it appears that the term  $z_{i,t} = PEG_{i,t} * KOPEN_{i,t} * \Delta r_{i,t}^{base}$  also has an exogenous influence on local monetary policy conditions. It therefore provides a source of variation in short-term interest rates that is exogenous to domestic conditions in terms of inequality. As a result,  $z_{i,t}$  constitutes a theoretically good external instrument. In what follows, as in Jordà et al. (2015), we use  $z_{i,t}$  as an IV for the change in the interest rate to check the consistency of our baseline OLS estimates from equation 3.

The third motivation for using local projection is that it easily accommodates non-linearities.<sup>7</sup> This makes it possible to enrich our analysis by checking whether the impulse responses of inequality to a monetary policy innovation are state dependent. This is of great interest since we use historical data that cover different monetary policy regimes. This also follows many studies, which highlight that the effects of monetary policy vary over the business cycle. In practice, we extend equation 3 by conditioning the effects of the interest rates on inequality by a state variable:

$$\Delta_h y_{i,t-1} = \alpha_i^k + \beta^k \Delta r_{i,t} + \kappa^k \Delta r_{i,t} * State_{i,t} + \theta^k X_{i,t} + \vartheta^k State_{i,t} + \varepsilon_{i,t}^k \quad (5)$$

where  $State_{i,t}$  is a dummy variable indicating the state or regime.

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<sup>7</sup>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 IRs, which often make the estimation intractable in practice, if we are outside the baseline framework.

## 3 Data description

### 3.1 Inequality

The Gini coefficient has long been used to analyze income inequality, in that it illustrates the degree to which a variable is equally distributed across its population. However, the Gini index assigns relatively greater weight to observations in the middle of the distribution than to those located at the tails. This flaw hampers efforts to account for aspects of concentration, which are at the very heart of the inequality issue. That is why a sound alternative would be to consider instead measures that exclusively focus on the tails of the distribution. Such indicators take the shape of decile ratios or the shares of national income received by the 5, 1 or 0.1 percent of individuals with the highest market incomes. As shown by ?, these measures strongly correlate with the Gini index and therefore do not alter the broad picture of inequality in an important way.

In this paper, top income data are extracted from the World Inequality Database (WID, 2017). Specifically, income inequality is operationalized by the top 1 percent’s pre-tax national income share (P1) in 12 advanced economies over the period 1920-2015. The countries considered include Australia, Canada, Italy, Germany, Denmark, France, U.K, Japan, the Netherlands, Norway, Sweden and the U.S. We also conduct our empirical analysis by excluding WWII from the sample.<sup>8</sup> As a robustness check, we test our model on the top 10 percent’s pre-tax national income share (P10). In fact, as emphasized by Roine et al. (2009), P1 and P10 are quite different: while the first concentrates on individuals receiving important shares of capital income, the second contains more high income earners. Hence, P10 would be considered more heterogeneous than P1 in that it gathers “rich” individuals who differ substantially in terms of their income sources. Figure A1 in the appendix plots for each country P1 and P10 over the studied period. The dynamics of top income shares can be classified into three stages: high top income shares on the eve of the two world wars before these gains were then eliminated; a post-war characterized by a declining accumulation of private wealth; and then increasing top income shares at the beginning of the 1980s, which largely favored top-income households.

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<sup>8</sup>Table A1 traces out in detail the data sources and their availability, for each country during both periods.

## 3.2 Macroeconomic variables

We exploit the Jordà-Schularick-Taylor Macrohistory Database, which provides us with long series of macroeconomic data.<sup>9</sup> In this database, information on several macroeconomic variables are available from 1870 to 2016 and cover approximately 17 developed economies.<sup>10</sup>

For the baseline Panel VAR framework, we mobilized the following macroeconomic aggregates: real GDP per capita (index, 2005=100), consumer prices (index, 1990=100) and the short-term interest rate. As mentioned in section 3.1, to ensure the stationarity of the series, real GDP per capita and the consumer price index are considered in terms of growth rates (logs), while the first difference is taken for the short-term interest rate. In addressing the question of monetary policy and inequality, our paper also departs from the existing literature by building on several macroeconomic controls. The choice of these variables fits with the literature on the determinants of inequality. The set of control variables used for both the local projections and the instrumental variable are summarized in Table A2 (see the appendix).

Our battery of control variables can be divided into four categories: financial development, globalization, public debt and technological progress. The way in which financial development shapes income inequality remains an open question in the literature. While it was widely believed that financial development would reduce inequality through better access to credit for low-income households, recent findings (De Haan and Sturm (2017) provide a complete survey on this question) argue, on the contrary, that more finance primarily favors top income shares. 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 inequality net of housing expenditures. That is why, to control for this factor, we add a real house price index. Regarding globalization, Jaumotte et al. (2013) demonstrate, for a panel of 51 countries, that its effect on inequality has two offsetting tendencies. In fact, while trade globalization is associated with a reduction in inequality, financial globalization is associated with an increase.

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<sup>9</sup>All variables are winsorized in order to eliminate outliers.

<sup>10</sup>Our sample is only restricted because of the limited availability of income inequality data.

Regarding government debt, it is proxied in our analysis by the standard public debt-to-GDP ratio. From the outset, it is not easy to establish on the basis of this indicator the relationship between public debt and inequality. However, 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 debt when inequality increases. Moreover, technological change has been repeatedly identified in the literature as playing a potent role in widening wage inequalities (see Acemoglu (1998), Card and DiNardo (2002), and Jau-motte et al. (2013) among others). One way to control for this factor consists in mobilizing data on patents. The use of such data would make it possible to measure the number of inventions, some of which are likely to become marketable. To that end, we rely on a novel dataset, which tracks patent and grant activity in the U.S. since 1790. Specifically, we include data on utility patent applications, as these primarily concern “useful” inventions. The choice of focusing on the U.S. stems from the fact that it is usually the country where the most breakthrough innovations have emerged.

### 3.3 External instruments

As discussed above, the instrumental variable  $z_{i,t}$  is the product of changes in the base country’s short-term interest rate ( $\Delta r_{i,t}^{base}$ ), the exchange rate regime ( $PEG_{i,t}$ ) and the degree of capital control ( $KOPEN_{i,t}$ ).<sup>11</sup> Following Jordà et al. (2015), the definitions of pegs prior to WWII are extracted from Obstfeld et al. (2004, 2005). After WWII, data on exchange rate regimes are completed using data in Ilzetzi et al. (2017). Table A3 in the appendix lists for each period and country of our sample the applicable exchange rate regime. Similarly, the indicator for capital mobility status builds on the index (which ranges from 0 to 100) initially introduced by Chinn and Ito (2006). As in Jordà et al. (2015), we use this index rescaled to the unit interval, with 0 meaning fully closed and being 1 fully open. Figure 1 below plots, for our panel, changes in home interest rate  $\Delta r_{i,t}$  against the constructed LP-IV.<sup>12</sup>

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<sup>11</sup>The U.S. are treated as the base country. We replicate our results using the U.K. as the base between 1920 and 1938, and the U.S. after 1945. Our results are not sensitive to this choice.

<sup>12</sup>Given the irrelevance of the short-term interest rate in the context of Zero Lower Bound (ZLB), we conduct a pre-crisis analysis (until 2007). The results are consistent with our baseline findings.

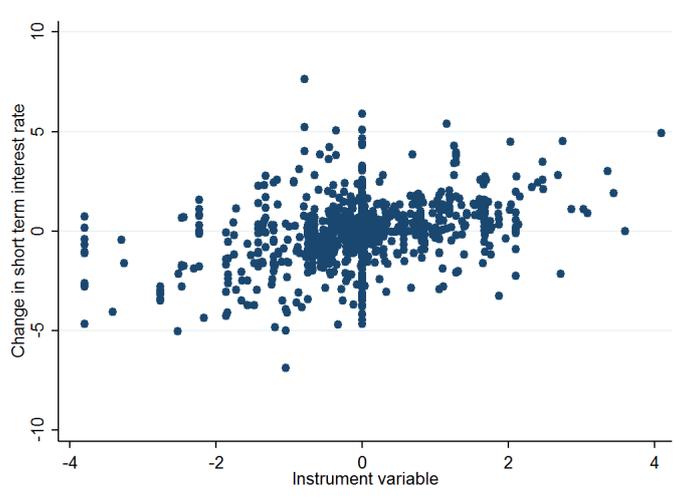


Figure 1: Jorda, Schularick and Taylor based IV: change in short-term interest rate in home and base countries

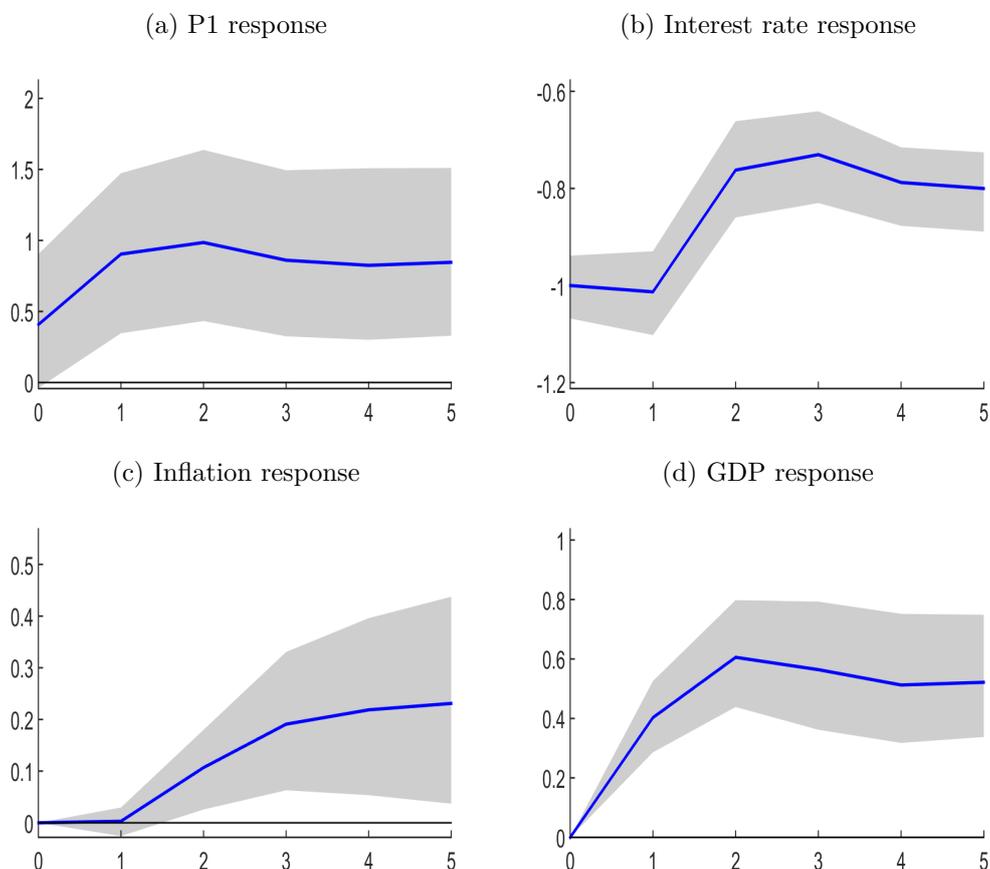
## 4 Results

This section reports the impulse response functions of inequality to a monetary policy shock both from vector auto-regression and local projection.

### 4.1 Panel VAR results

The Panel VAR described in the previous section is estimated by OLS and used to compute impulse response functions. Figure 2 displays impulse responses to orthogonalized 100 b.p. negative interest rate shocks in a 90% confidence band. As we have previously noted, a typical concern in the monetary VAR literature is the finding of a price puzzle, i.e., the fact that unexpected monetary tightening leads to increased inflation. Hence, a preliminary requirement is to check that the dynamic restriction that we imposed in our VAR following the proposal of Estrella (2015) is well suited to eliminate the price puzzle. The estimated shape of the inflation response to a monetary policy shock does not exhibit counter-intuitive effects. The dynamic effect of an interest shock on real GDP is also fairly standard. All of this supports the choice of our identification structure and allows us to reasonably analyze the effects of our structural monetary policy shock on inequality.

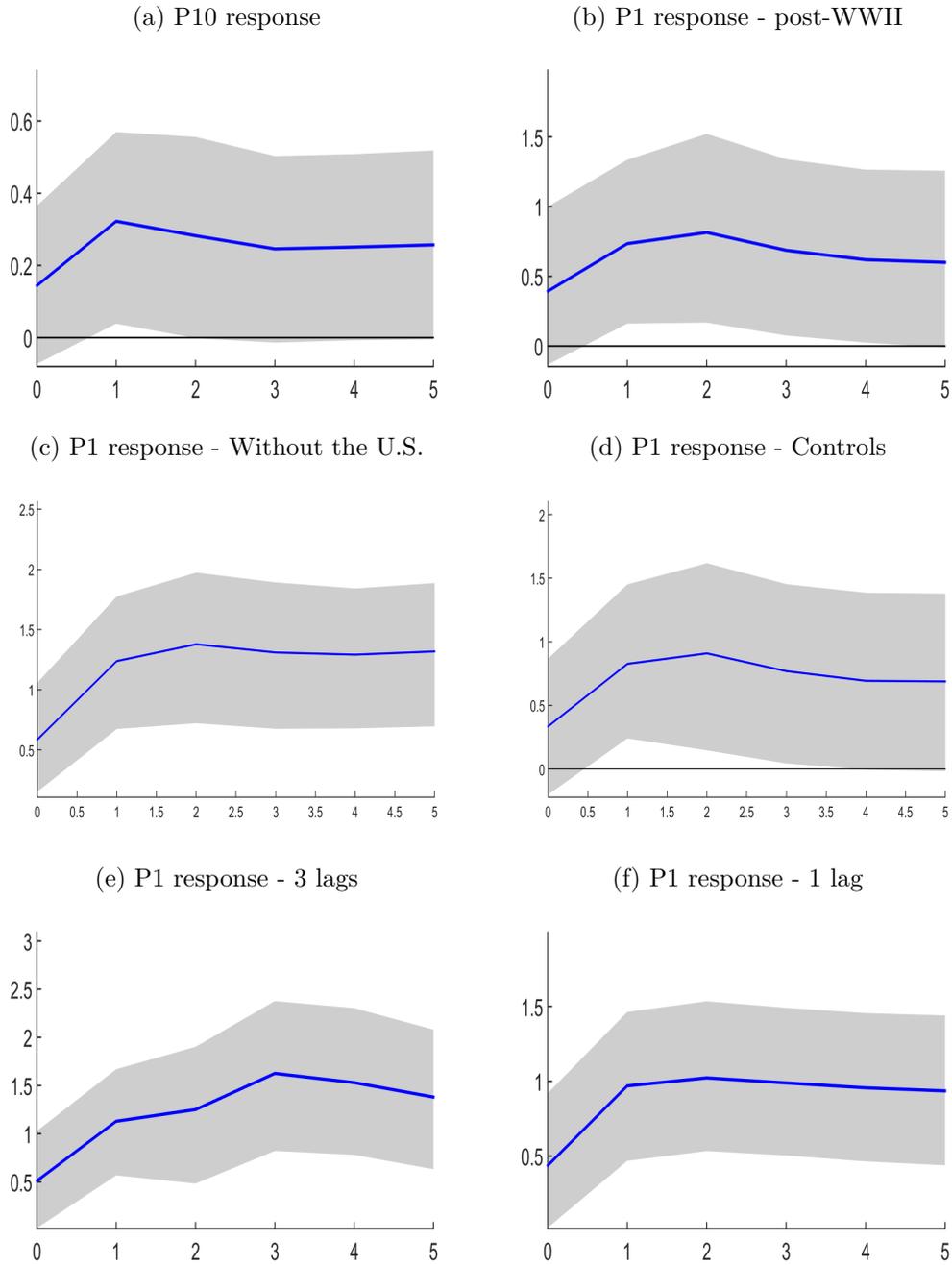
Figure 2: Inequality PVAR responses to a negative short-term interest rate shock: Cumulated effects



Note: The figure shows cumulated impulse responses of inequality to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

The impulse response function of the top 1 percent pre-tax national income share (P1) reported in the top-left corner of Figure 2 indicates that an unanticipated monetary policy shock leads to an increase in income inequality. This means that monetary policy loosening increases inequality. We can see that the monetary policy shock has significant and medium-term effects on inequality. A 100 b.p. decrease in the short-term interest rate increases the top income index by approximately 1 percentage point two years after the shock. Our results are in line with the recent theoretical predictions of Dolado et al. (2018) and the empirical results documented by Romer and Romer (1999). However, they contradict the empirical findings of Coibion et al. (2017) and Furceri et al. (2018).

Figure 3: Inequality PVAR responses to a negative short-term interest rate shock:  
Robustness check



Note: The figure shows cumulated impulse responses of inequality to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

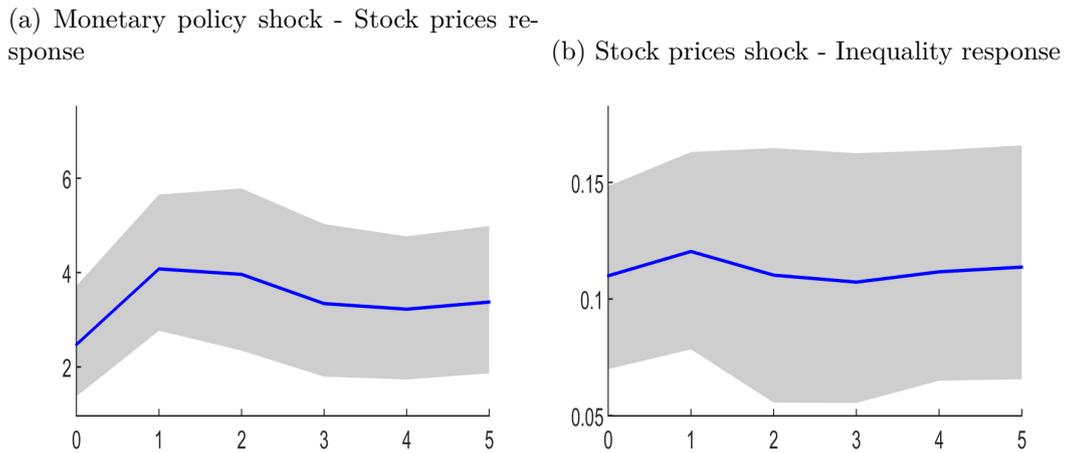
This obviously raises the question of the sensitivity of these findings to our model and sample choices. Figure 3 displays the impulse responses to a variety of robustness checks. The first impulse response (graph (a)) reports results with our alternative measure of inequality. We find evidence of a positive reaction of the top 10 percent’s pre-tax national income share (P10) to an unexpected negative monetary policy shock. While the finding is consistent with our baseline result, note that the reaction is less intense and less significant. This is in line with our expectations. Indeed, monetary policy shocks produce a more potent impact in the tails of the income distribution. Therefore, we can infer that the response of the top 0.1 percent would be larger than that in our baseline results.

We next examine the effect of our particular sample choice in two ways. First, we exclude the pre-WWII period. By doing so, we check that our results are not driven by what may be considered an anachronistic monetary regime. This is also a way to be more closely aligned with the literature, which has conducted empirical investigations with samples starting in the early 1980s. Second, we exclude the U.S. from our sample. There are indeed good reasons to believe that the U.S. is characterized by specific behaviors due to its historical international monetary role. This could create heterogeneity bias because our empirical model controls for unobserved level heterogeneity — by demeaning the data — but not for unobserved dynamic heterogeneity. The results reported in Figure 3, panels (b) and (c) confirm our previous conclusions. Moreover, excluding the pre-WWII period and the U.S. does not alter the response of inequality to an unanticipated negative monetary policy shock.

Finally, to assess the robustness of our results, we estimate different specifications of the Panel VAR. First, we include variables that reflect the dynamics of asset prices. This provides a more complete representation of the macro-level dynamics, in response to the fact that there are linkages among monetary policy, asset prices and inequality. In practice, we estimate a 6-dimensional Panel VAR model that enriches our baseline VAR with a measure of the house price cycle. Graph (d) in Figure 3 demonstrates the consistency of our results to the extension of the Panel VAR model. Second, we check the sensitivity of our results to the number of lags included in the model. The evidence displayed in panels (e) and (f) reveals that the response of inequality to a negative monetary policy shock remains unchanged with respect to the findings in our baseline model.

Our PVAR approach also offers the advantage of providing some insights into the channels through which monetary policy favors the rich. Our previous results could support the income composition channel. To empirically test the latter, we need to formally demonstrate that an unexpected negative change in interest rates positively affects asset prices and, in turn, that an unexpected rise in asset prices increases the relative income of rich households. By doing so, we link the increase in inequality following monetary loosening to the heterogeneity of income sources across households. This documents the fact that the richer households are, the higher the shares of their income from financial markets.

Figure 4: Inequality Panel VAR responses to a negative short-term interest rate shock: Insights on the income composition channel



Note: The figure depicts the cumulated impulse responses of stock prices and inequality to an unexpected 100 b.p. increase in the short-term interest rate and a 100 b.p. increase in stock prices, respectively. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

Figure 4 reports the impulse response function of stock prices to an unexpected monetary policy shock and the response of inequality to an unexpected stock price shock. The former shows that an unexpected decrease in the short-term interest rate increases stock prices, i.e., expected future income, while the latter indicates that a negative shock to stock prices increases the incomes of the top 1 percent. Therefore, this shows that the effect of monetary policy on the richest 1 percent is likely to be channeled through higher asset prices.

## 4.2 Local projection results

Our Panel VAR approach is supplemented by a local projections estimation along with a novel identification of monetary policy shocks. The first step is to assess the strength of our instrumental variable. To do so, we estimate, in the context of equation 3, a first-stage regression of 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 3 and underline the soundness of our instrumental variable. The coefficient estimates of the instrument  $z_{i,t}$  remain statistically significant and range between 0.45 and 0.48 from year 0 (when the shock is felt) to year 4, while the F statistics feature high values across samples. Thus, we can now proceed to analyze the local projection responses of inequality to monetary policy shocks.

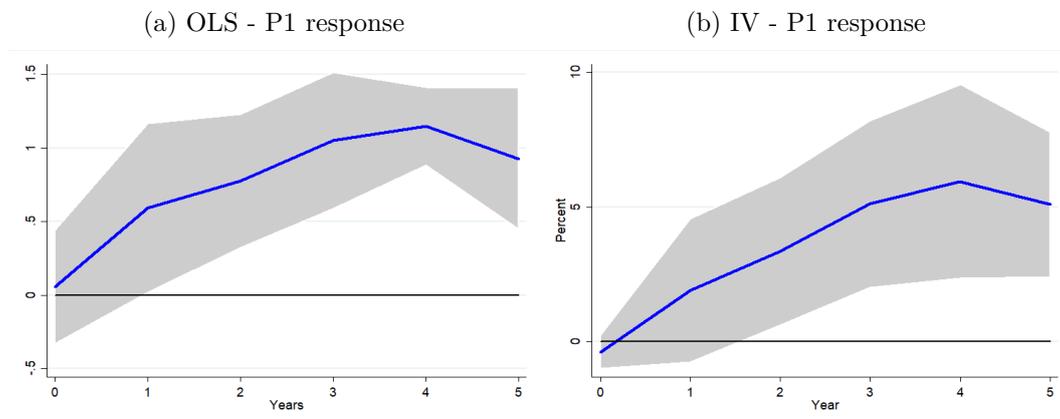
Table 1: Local projection-IV: First-stage results

$\Delta$ <i>Short-term interest rate</i>	Year 0	Year 1	Year 2	Year 3	Year 4
IV	0.47*** (0.06)	0.47*** (0.06)	0.48*** (0.06)	0.46*** (0.06)	0.45*** (0.06)
Observations	659	646	633	619	604
F	60	60.84	61.34	55.05	52.46

Note: \*, \*\* and \*\*\* indicate statistical significance at the 10%, 5% and 1% levels, respectively.

The results obtained from the estimation of equation 3 by local projections are presented in Figure 5. The two graphs illustrate the impulse response functions of inequality to an unexpected negative monetary policy shock with the associated confidence bands, using both OLS and the instrumental variable. The initial glance at the IRs seems to confirm what has been documented in the previous section, that is, monetary easing significantly and durably increases income inequality. Precisely, an unanticipated decrease of 100 b.p. in the short-term interest rate (graph (a) on the right) increases the share of the top 1% by approximately 1.15 percentage point four years after the shock. This impact on inequality is quite similar to what our Panel VAR produced. Nonetheless, the effects on inequality are, interestingly, more pronounced when we instead use the instrumental variable. Indeed, a perturbation to the domestic interest rate  $r_{i,t}$  via the instrument  $z_{i,t}$  (graph (b)) increases P1 by 3.35, 5.11 and 5.95 percentage points two, three and four years following the shock, respectively.

Figure 5: Inequality local projection responses to a negative short-term interest rate shock



Note: The figure shows cumulated impulse responses of inequality to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

These differences are clearer in Table 2, which jointly reports coefficient estimates of OLS and the 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 note that the impulse responses obtained under both methods exhibit a relatively similar pattern. However, the coefficient estimates obtained via OLS are moderately lower than those produced by the IV, although the signs are correct. For example, a monetary policy shock reduces P1 in year 3 after the shock by 1.05% using OLS and by 5.11% in the IV estimation. Note that Jordà et al. (2015) — who investigate the effect of monetary policy on house prices in the very long run — document the same observation in a more or less similar magnitude. How should we account for this contrast between the OLS and LP-IV coefficient estimates? The answer essentially lies in how monetary policy shocks are captured by the instrumental variable. To be more specific, the way the instrument  $z_{i,t}$  is built makes it more likely to be driven by exogenous perturbations in the world economy, in comparison to fluctuations captured by a simple OLS regression. As a result, the domestic interest rate in the IV estimation is highly sensitive to changes in external financial and monetary conditions.

Ensuring the reliability of the local projection results also requires conducting a variety of alternative estimations. As noted in Figure 6, we replicate in the OLS and LP-IV estimation methods the analysis conducted in the Panel VAR model. The robustness checks begin with our alternative measure of inequality, i.e., P10.

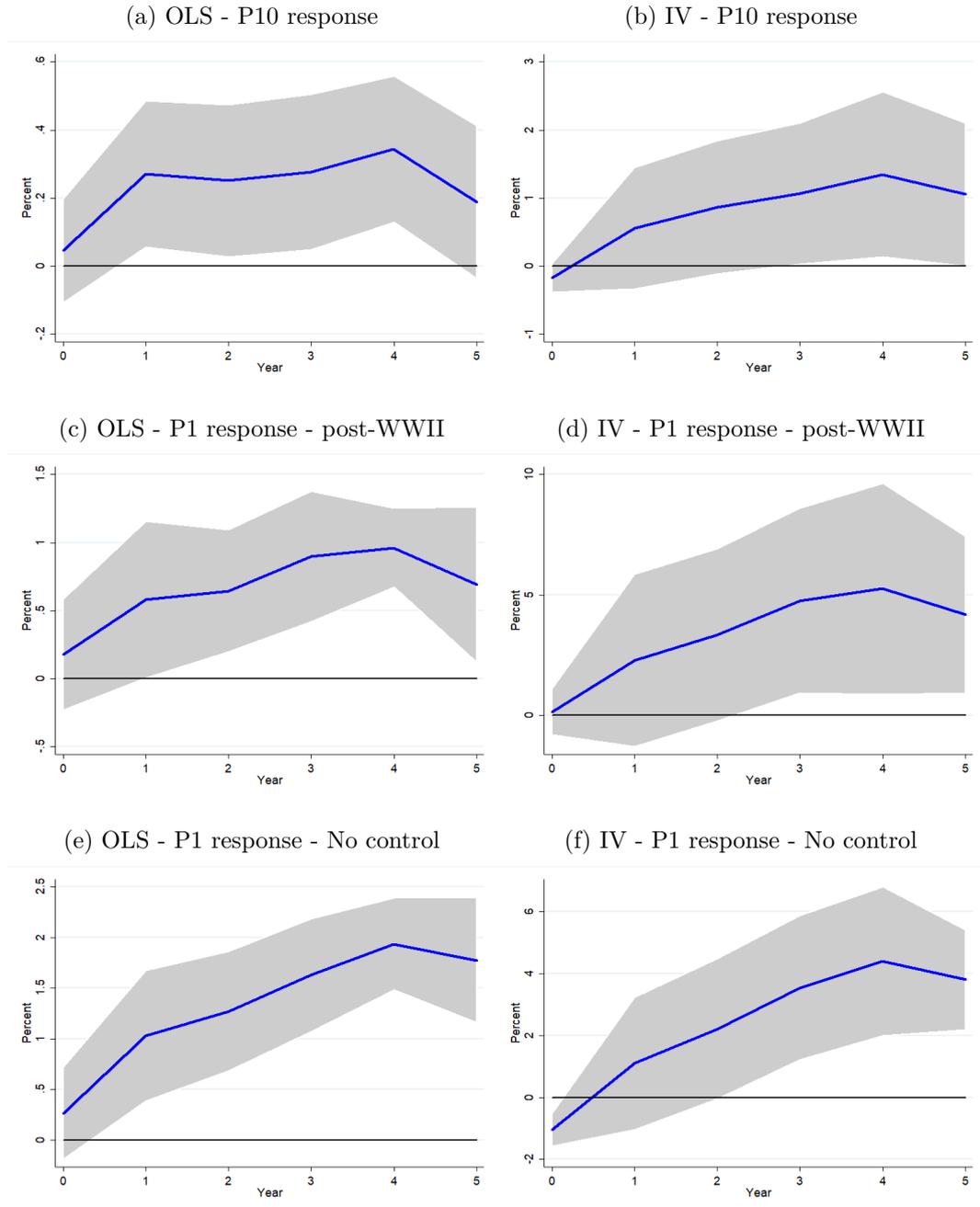
Table 2: Local projection: OLS and IV estimation results

<b>OLS estimates - P1</b>						
	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
$\Delta$ <i>Short-term interest rate</i>	-0.06 (0.23)	-0.59* (0.35)	-0.78** (0.28)	-1.05*** (0.28)	-1.15*** (0.16)	-0.928*** (0.29)
$R^2$	0.205	0.195	0.16	0.154	0.15	0.15
Observations	659	646	633	619	604	591
<b>IV estimates - P1</b>						
$\Delta$ <i>Short-term interest rate</i>	0.39 (0.38)	-1.91 (1.62)	-3.35** (1.67)	-5.11*** (1.89)	-5.95*** (2.19)	-5.10*** (1.86)
$R^2$	0.201	0.170	0.095	0.034	0.08	0.06
Kleibergen-Paap	6.66	6.64	6.85	6.71	6.43	48.04
Observations	659	646	633	619	604	591

Note: Country-based cluster-robust standard errors are reported in parentheses below the coefficient estimates. Coefficient estimates of controls and fixed effects are not reported. The controls include the twice-lagged terms of (i) the change in the short-term interest rate; (ii) the change in inequality; 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 U.S. GDP growth; (vii) the level of financial development; (viii) the level of commercial openness; (ix) house price growth; (x) government spending; and (xi) patent activity. We report the Kleibergen and Paap (2006) statistic for weak instruments. \*, \*\* and \*\*\* indicate statistical significance at the 10%, 5% and 1% levels, respectively.

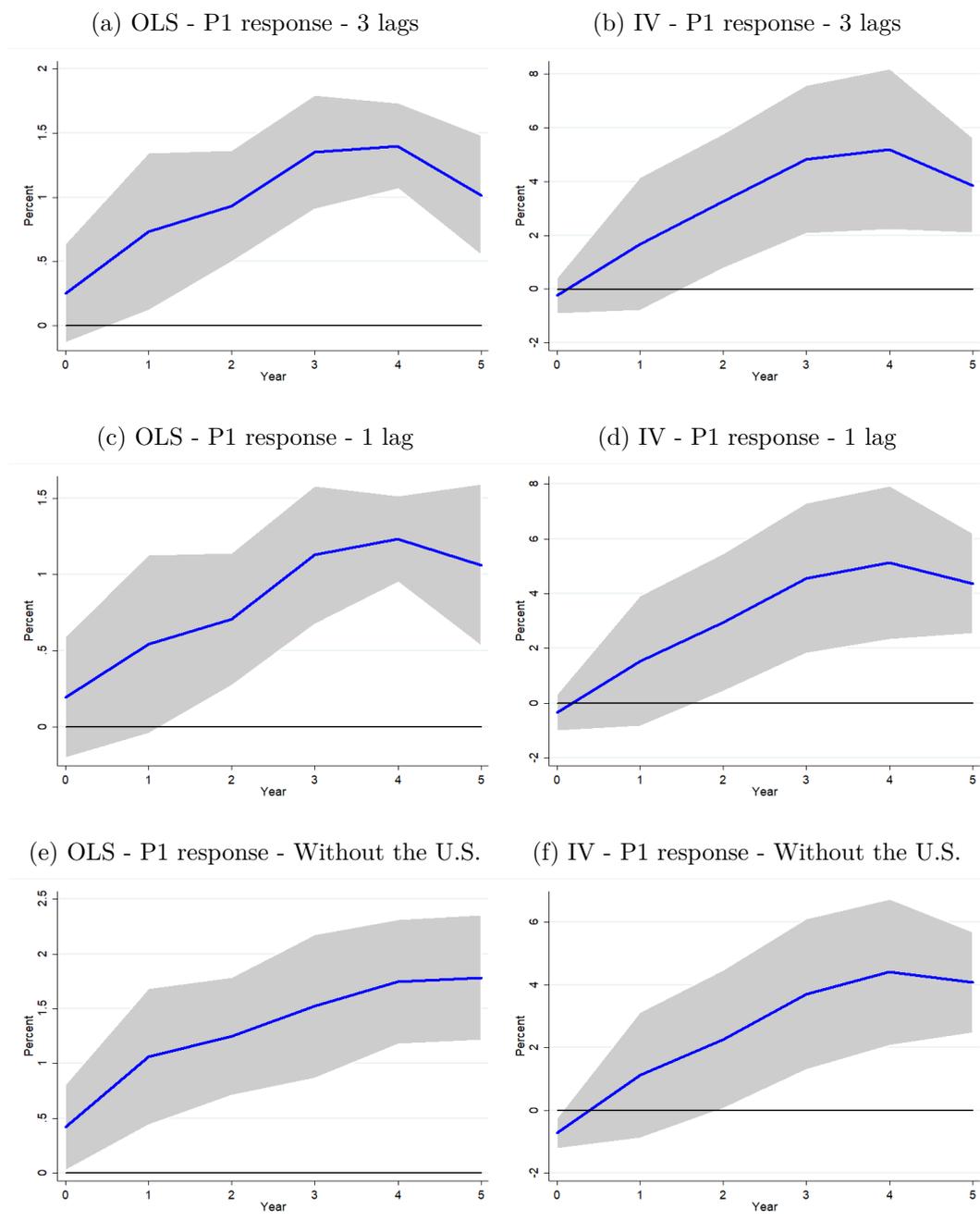
The impulse responses documented in graphs (a) and (b) affirm as before that the responses of P10 to an unexpected monetary policy shock are lower than those of P1. Two further observations regarding the responses of P10 are worth discussing: (i) the coefficient estimates obtained from OLS remain smaller than their IV counterparts, and (ii) the OLS estimates become statistically significant earlier (as of year 1 in contrast to year 3 for IV). The same pattern can be observed for the local projection estimates starting from the post-WWII period (see graphs (b) and (c)). Furthermore, the post-war coefficient estimates are very similar to those obtained in the benchmark LP estimation, for both OLS and the IV estimation. Therefore, this does not contradict our main findings and continues to confirm the stability of our estimates. An additional robustness check consists of estimating equation 3 with country fixed effects while omitting the rich set of control variables. This exercise is valuable because it assesses whether the IV exclusion restrictions are not violated. As a matter of fact, a correctly specified instrument would be sufficient to avoid potential endogeneity bias. The evidence depicted in graphs (e) and (f) does not contradict our main results. This sheds light on the reliability of the IV and OLS regressions in the context of the local projections framework.

Figure 6: Inequality local projection responses to a negative short-term interest rate shock: Robustness check



Note: The figure shows cumulated impulse responses of inequality to to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

Figure 7: Inequality local projection responses to a negative short-term interest rate shock: Robustness check



Note: The figure shows cumulated impulse responses of inequality to to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

In a second step of our sensitivity analysis, we test the robustness of our baseline model to different lag numbers, for both estimation methods. Figure 7 exhibits the additional modifications made to the empirical analysis. To begin with, graphs (a), (b), (c) and (d) suggest that the LP framework remains robust to different lags and does not depart from results documented by the Panel VAR. Examining the corresponding impulse responses, we notice that the signs do not change, while statistical significance and coefficient estimates remain stable. Finally, we assess whether the effects of monetary policy on inequality are robust when excluding the U.S. from the sample. Once again, the results in graphs (e) and (f) are very similar to those obtained in the baseline estimation, except that the response of P1 under OLS becomes statistically significant immediately in year 0. This further validates that monetary policy shocks are well identified in our empirical analysis.

### 4.3 Regime switching

The results we have reported in the previous sections suggest that monetary policy easing increases inequality irrespective of the state of the economy. 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 method easily accommodates non-linearities. Therefore, it is convenient to develop a regime-switching version of the previous model. That means that we allow the impact of monetary policy on inequality to depend upon the regime of another variable (see equation 5). In this way, we can compute conditional impulse responses in a particular regime. In this context, we consider three factors potentially leading to different impulse responses of monetary policy: the occurrence of a systemic banking crisis, the inflation regime and the state of the economy over the business cycle. For each of these variables, we define a binary variable taking value one when there is a banking crisis or the inflation rate exceeds the median of its sample distribution (2.58%) and 0 otherwise. We also do the same when the output gap — computed with the regression filter proposed by Hamilton (2018) — is positive.

Figure 8: Inequality local projection responses to a negative short-term interest rate shock: Regime switching

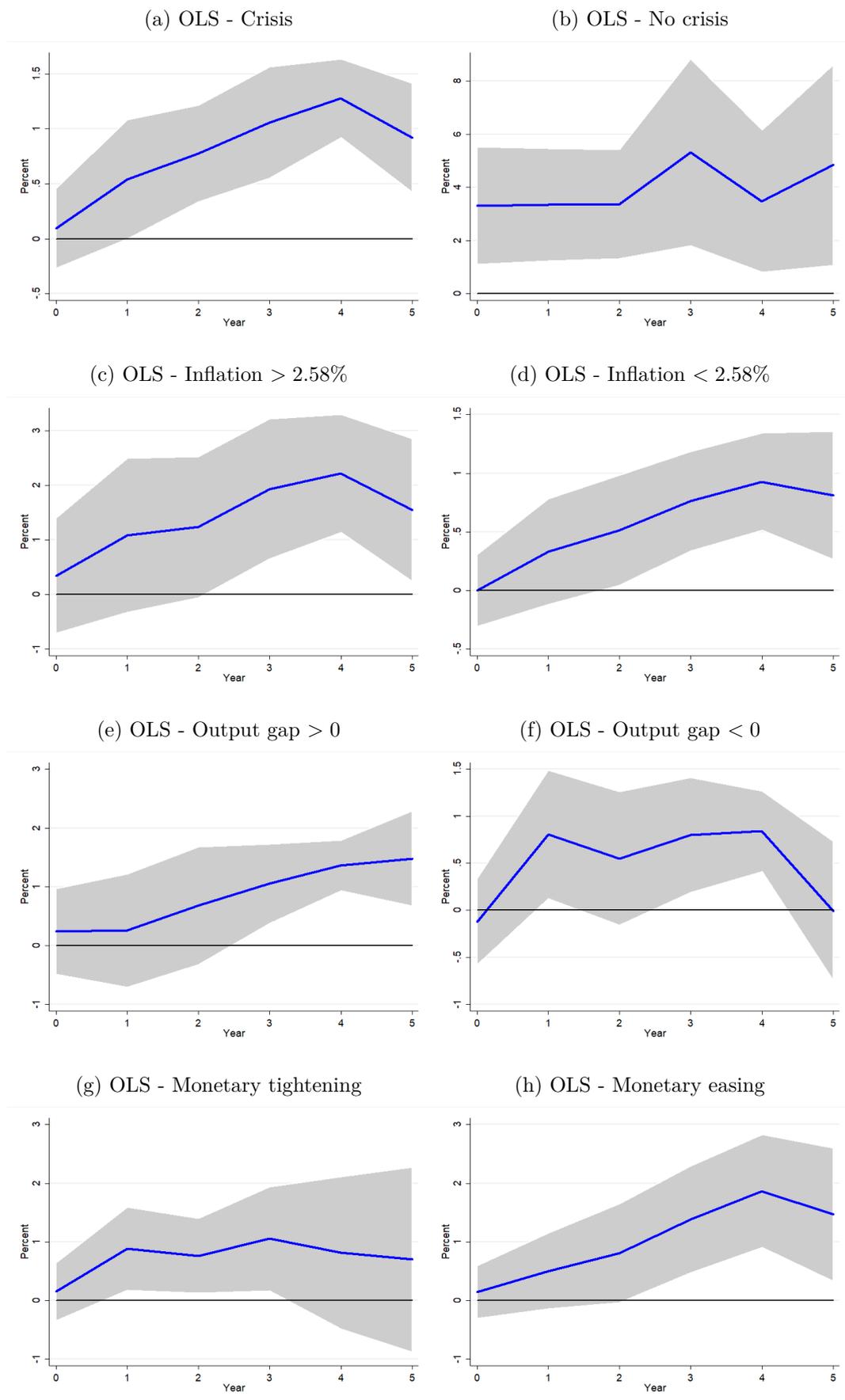


Figure 8 reports the impulse responses estimated with the regime-switching model (equation 5) for the 3 factors previously described. Overall, the impulse responses displayed do not conflict with the previous results. We find that monetary policy loosening has considerably larger effects outside banking crisis periods and when inflation is high. 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, decreasing the value of their incomes"*. The effects of monetary policy also differ according the state of the economy. We find that a monetary policy shock has more immediate effects on inequality during recessions than during expansions. However, there is no a significant difference regarding its effect on the medium run. Interestingly, the same empirical strategy could be used to check for asymmetries between expansionary (negative variations in interest rates) and contractionary (positive variations in interest rates) monetary policies. The impulse responses reported in graphs (g) and (h) of Figure 8 document that neither of these monetary policies presents striking differences in terms of its impact on inequality.

## 5 Conclusion

This paper sought to investigate the distributional consequences of monetary policy between 1920 and 2015. The central idea that guided this paper's argument is that the existing literature considers the distributional effects of monetary policy using data on inequality over a short period of time. However, inequalities tend to vary more in the medium-to-long run. We address this shortcoming by studying how changes in monetary policy stance over a century impacted the income distribution while controlling for the determinants of inequality. To do so, we combined two large datasets: (i) the World Inequality Database (WID) to extract data on the share of national income held by the richest 1% and the Jordà-Schularick-Taylor Macrohistory Database, which allows us to access large series of macroeconomic and financial variables.

Our empirical strategy considers two complementary approaches that conciliate between consistency and flexibility. The first approach involves a Panel VAR framework, which delivers the orthogonalized impulse response function of inequality to a monetary policy shock. The second generates dynamic responses

by inequality in a Local Projections (LP) setting, wherein a rich set of macroeconomic controls is included. This framework allows us to consider a natural experiment, where exogenous perturbations in monetary policy are driven by factors unrelated to domestic economic conditions. Such exogenous perturbations enter as an instrumental variable, which traces out the impulse responses of inequality.

The results obtained from both empirical methods indicate that *loose monetary conditions strongly increase the top one percent's income and vice versa*. In fact, following an expansionary monetary policy shock, the share of national income held by the richest 1 percent increases by approximately 1 to 6 percentage points, according to estimates from the Panel VAR and Local Projections (LP). This effect is statistically significant in the medium run and economically considerable. We also demonstrate that the increase in top 1 percent's share is arguably the result of higher asset prices. The baseline results hold under a battery of robustness checks, which (i) consider an alternative inequality measure, (ii) exclude the U.S. economy from the sample, (iii) specifically focus on the post-WWII period, (iv) remove control variables and (v) test different lag numbers. Furthermore, the regime-switching version of our model indicates that our conclusions are robust, regardless of the state of the economy.

For future research, we would like to test the effects of monetary policy on different inequality measures, which exclusively focus on “rich” households (i.e., the top 5% or top 0.1% with the highest market incomes). In the same perspective, are the obtained results also valid for wealth inequality? This aspect is important because wealth is more unevenly distributed than incomes. Moreover, given that we use pre-tax data, policymakers may be interested in the effects of monetary policy on inequality, net of the contribution of fiscal policy. Finally, the empirical approach adopted in this paper only considers — in the spirit of the corresponding literature — the global effects of monetary policy on the income distribution. That is, we do not identify all the transmission channels through which distributional effects of monetary policy operate. That said, what are the policy implications we can draw from these findings for the ongoing debate on monetary policy and inequality? Central bankers need to be attentive not only to the aggregate consequences of monetary policy but also to their side effects.

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# Appendix

Table A1: Data sources and periods of inequality measures

Country	Period P1	Period P2	Details
Australia	1921-1938	1946-2015	WID (2017)
Canada	1920-1938	1946-2010	WID (2017)
Germany	1925-1938	2001-2013	WID (2017)
Denmark	1920-1938	1946-2010	WID (2017)
France	1920-1938	1946-2014	WID (2017)
U.K.	-	1951-2014	WID (2017)
Italy	-	1974-2009	WID (2017)
Japan	1920-1938	1947-2010	WID (2017)
Netherlands	1920-1938	1952-2012	WID (2017)
Norway	-	1948-2011	WID (2017)
Sweden	1920-1935	1946-2013	WID (2017)
U.S.	1920-1938	1946-2015	WID (2017), Atkinson et al. (2015)

Note: There are years with missing values in each subperiod.

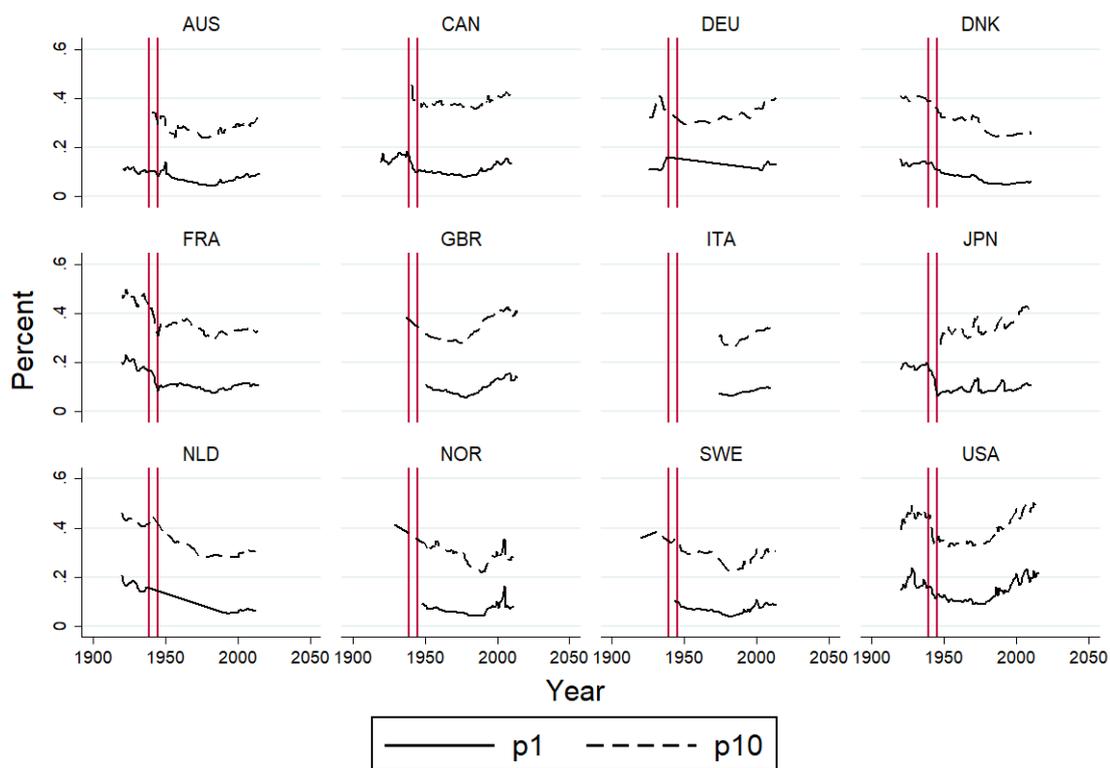


Figure A1: Inequality over time: 12 countries

Table A2: Control variables definition

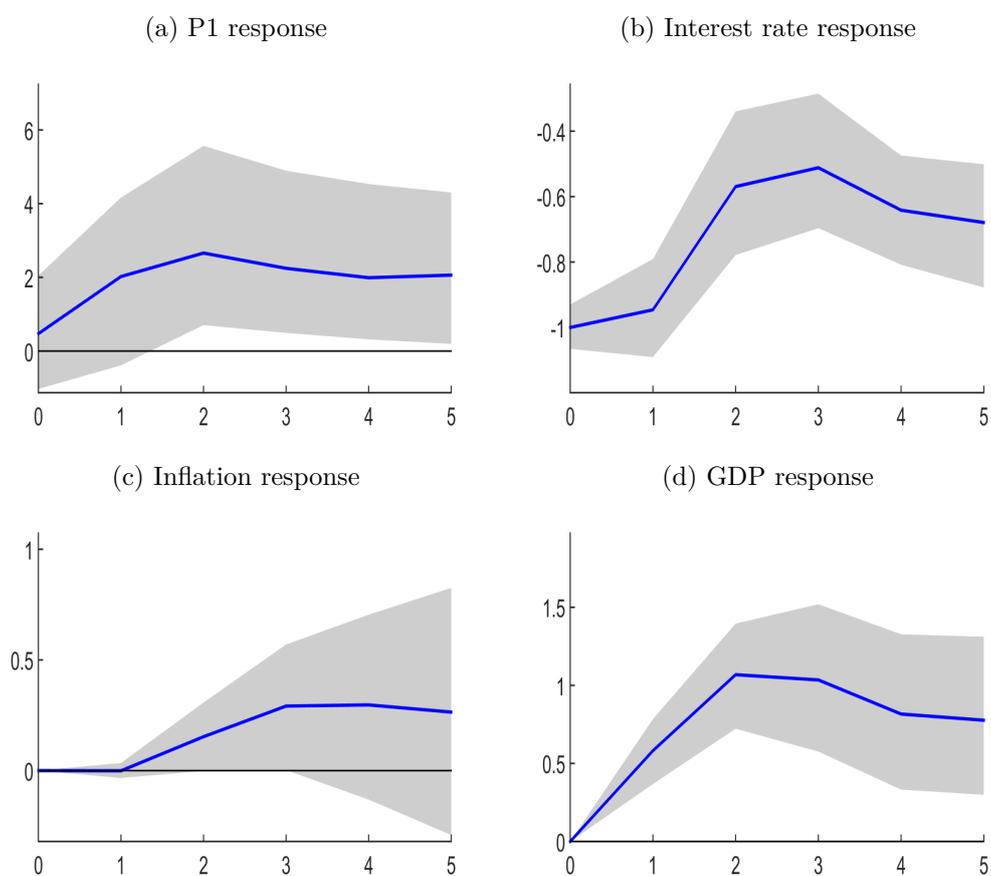
Variable	Variable definition	Source
Hpnom	House prices growth (real index, 1990=100)	Macrohistory Database JST
Stocks	Stock prices index growth (real index)	Macrohistory Database JST
Tloans	Ratio of total loans to non-financial private sector to GDP	Macrohistory Database JST, own calculations
Com_open	Ratio of imports and exports to GDP	Macrohistory Database JST, own calculations
Debt_gdp	Public debt-to-GDP ratio (in log-level)	Macrohistory Database JST
US_gdp	GDP growth of the U.S.	Macrohistory Database JST
US_patents	Growth rate of utility patents applications	United States Patent and Trademark Office

Note: Real indexes are obtained by dividing the variables by CPI, and growth rates are computed in logs.

Table A3: Exchange rate regimes

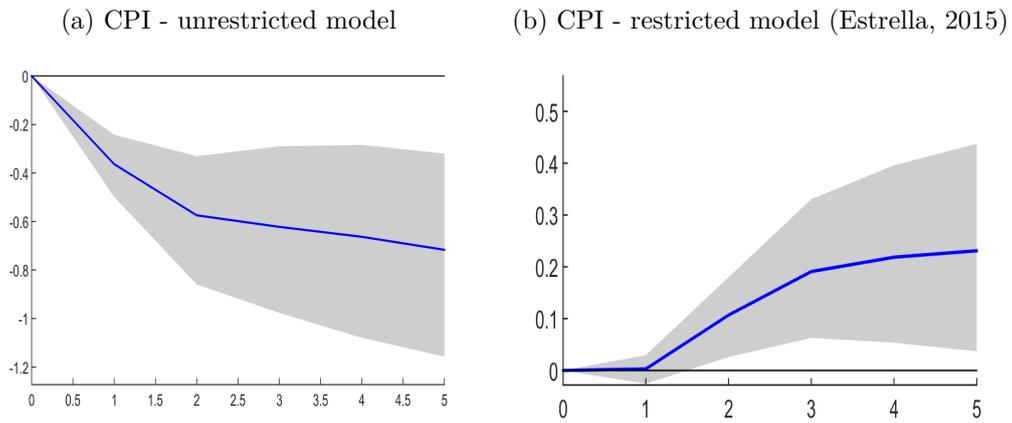
Country	Fixed	Floating
Australia	1920-1938, 1946-1983	1939-1945, 1984-2015
Canada	1920-1938, 1946-2015	1939-1945
Germany	1920-1938, 1946-1972, 1999-2014	1939-1945, 1973-1998
Denmark	1920-1938, 1946-2014	1939-1945
France	1920-1938, 1949-2014	1939-1948
U.K.	1920-1938, 1946-2008	1939-1945, 2009-2015
Italy	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	1939-2015

Figure A3: Inequality PVAR responses to a short-term interest rate shock: Cumulated effects estimated from a mean group type estimator



Note: The figure shows cumulated impulse responses of inequality to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

Figure A3: Comparison of CPI Impulse response functions



Note: The figure shows cumulated impulse responses of CPI to an unexpected 100 b.p. decrease in the short-term interest rate. The colored bands represent 90% confidence bands generated by bootstrapping (1000 draws).

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