Journal of Monetary Economics 112 (2020) 22–40

Contents lists available at ScienceDirect

Journal of Monetary Economics

journal homepage: www.elsevier.com/locate/jmoneco

The effects of quasi-random monetary experiments☆

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ARTICLE INFO

Article history:
Received 8 May 2018
Revised 18 January 2019
Accepted 30 January 2019
Available online 5 February 2019

JEL classification:
E01
E30
E32
E44
E47
E51
F33
F42
F44

Keywords:
Interest rates
Monetary experiments
Trilemma
Fixed exchange rates
Instrumental variables
Local projections
Local average treatment effect

ABSTRACT

The *trilemma* of international finance explains why interest rates in countries that fix their exchange rates and allow unfettered cross-border capital flows are outside the monetary authority's control. Based on this exogenous source of variation, we show that monetary interventions have large and significant effects using historical panel data since 1870. The causal effect of these interventions depends on whether: (1) the economy is above or below potential; (2) inflation is low; and (3) there is a credit boom in mortgage markets. Several novel control function adjustments account for potential spillover effects. The results have important implications for monetary policy.

Published by Elsevier B.V.

☆ Comments and suggestions from James Cloyne, Julian di Giovanni, Gernot Müller, Ricardo Reis, and Jón Steinsson have helped improve the paper. We are grateful to Helen Irvin for outstanding research assistance. We thank James Cloyne and Patrick Hürtgen for sharing their data with us. Seminar and conference participants at Ohio State University, U.C. Irvine, the Board of Governors of the Federal Reserve, the Federal Reserve Bank of Chicago, the Federal Reserve Bank of San Francisco, the Federal Reserve Bank of Cleveland, the Fourth CEPR Economic History Symposium, De Nederlandsche Bank, the Bank of England, the 23rd Dubrovnik Economic Conference, the 7th Joint Bank of Canada and European Central Bank Conference, the NBER Impulse and Propagation Mechanisms meeting, and the NBER Monetary Economics meeting provided useful feedback. All errors are ours. Generous support from the Institute for New Economic Thinking (INET), the Bundesministerium für Bildung und Forschung (BMBF), and the Volkswagen Foundation supported different parts of the data collection and analysis effort. We are grateful for their support. The views expressed in this paper are the sole responsibility of the authors and do not necessarily reflect the views of the Federal Reserve Bank of San Francisco or the Federal Reserve System.

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https://doi.org/10.1016/j.jmoneco.2019.01.021
0304-3932/Published by Elsevier B.V.

1. Introduction

The global financial crisis raised two separate yet equally important policy questions: Should central banks have been more aggressive slowing down the mortgage credit boom leading up to the crisis? And has monetary policy become largely ineffective in the aftermath as central banks struggle to hit their inflation and growth targets? We investigate such questions by examining whether or not monetary policy is less effective when the economy is below potential, or when inflation is low, or in periods of high credit growth.

It is largely an empirical matter to determine how useful a cyclical stabilizer monetary policy is. However, empirical measures of the effect of interest rates on macroeconomic outcomes are fraught. Macroeconomic aggregates and interest rates are jointly determined since monetary policy reflects the central bank's policy choices given the economic outlook. In the parlance of the policy-evaluation literature (see, e.g., Rubin, 2005), any measure of the treatment effect of policy is contaminated by confounders simultaneously correlated with the treatment assignment mechanism and the outcome.

Over time, several best-practice methods have emerged, but almost all are exclusively based on the Post-WW2 U.S. experience. One way is to control for information that might explain the policymaker's choices. Some of this control is explicit (along with additional exclusion restrictions) as with VAR approaches (e.g., Christiano et al., 1999). On another tack, Romer and Romer (1989) infer exogenous policy shifts from the narrative of the policy record. Continuing in this vein, Romer and Romer (2004), Cloyne and Hürtgen (2016), and Coibion et al. (2017) measure policy surprises by assuming that policymakers rely only on their staff's forecasts to choose policy. Taking a different approach, Kuttner (2001), Faust et al. (2004), Gürkaynak et al. (2005), Gertler and Karadi (2015), and Nakamura and Steinsson (2018) instead infer a surprise change in policy rates from high-frequency asset price reactions.

This paper proposes a new and different approach based on a quasi-natural experiment. Economic forces limit a country's policy choices with respect to the triad of capital mobility, exchange rates, and interest rates. The *trilemma* faced by policymakers says that they can have control over two out of the three policies, but not all three simultaneously. Articulating how and when the trilemma functions as a source of natural experiments in domestic monetary policy is one contribution of this paper.

The general empirical validity of the trilemma, in both recent times and in distant historical epochs, has been recognized for more than a decade: exogenous base country interest rate movements spill most strongly into local interest rates for open pegs (Obstfeld et al., 2004, 2005; Shambaugh, 2004).[1] Some important corollaries directly follow for empirical macroeconomics. A key contribution by di Giovanni et al. (2009) exploits the resulting identified local monetary policy shocks to estimate impulse response functions for other macroeconomic outcome variables of interest using standard VAR methods with instrumental variables.[2]

In this paper we take these ideas further. Variation in base rates can be used as a natural experiment, but only by appropriately sorting the different channels of trilemma transmission. We do this by extending the lessons from the literature on policy evaluation and identification with instrumental variables (IV) to a time series setting using local projections (Jordà, 2005).

Instrumental variable applications using local projections (LP-IV) have recently appeared in a variety of settings (see, e.g., Jordà et al., 2015; Jordà and Taylor, 2016; Ramey and Zubairy, 2018). Here we expand the sample from the commonly studied post-WW2 period in the U.S., and include now all of advanced economy macroeconomic history since 1870. Our results are thus based on a much larger cross-sectional sample spanning over a century—therefore bringing greater statistical power and generality relative to findings based hitherto on the U.S. post-WW2 data alone.

Our main findings are easily summarized. First, using the subpopulation of open pegs, 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. Second, we investigate the robustness of our new IV estimates for open pegs against the effects found by investigating the combined behavior of post-WW2 data from the U.S. and U.K. using the established approaches of Romer and Romer (2004) and Cloyne and Hürtgen (2016). Third, we take several steps to control for spillover confounding (or failure of the exclusion restriction), such as accounting for global business cycle effects, and working with base country policy surprises rather than directly with interest rates.

We find—echoing prior work—that one source of state dependence comes from how the economy responds to monetary policy in the boom versus the slump. Specifically, we find that stimulating a weak economy is much harder than reining in a strong one. Yet another source of state dependence comes from the level of inflation, however. Advanced economies have recently struggled with a low-growth, low-inflation environment, referred to by commentators as "lowflation." We find that monetary policy turns out to be rather ineffective in lowflation environments, thus revealing another hitherto unexamined dimension in which monetary policy is asymmetric. Perhaps this is not surprising. Episodes where inflation is very low are usually associated with nominal rates close to the zero lower bound, effectively limiting a central bank's room for maneuver.

Our empirical findings resonate with recent theoretical developments that examine the efficacy of monetary policy as a function of (household and/or firm) leverage. For example, Auclert (2017) shows that effectiveness depends on household

[1] Open pegs are countries that fix their exchange rate but allow relatively free movement of capital.

[2] In related work, di Giovanni and Shambaugh (2008) used the trilemma to investigate post-WW2 output volatility in fixed and floating regimes. Ilzetzki et al. (2013) partition countries by exchange rate regime to study the impact of a fiscal policy shock. In previous work (Jordà et al., 2015), we studied the link between financial conditions, mortgage credit, and house prices.

balance sheet exposure through the redistributive channels that interest rates can have. In Kaplan et al. (2018), the mechanism operates via the heterogeneity in marginal propensities to consume of households facing uninsurable income shocks in incomplete markets. Narrowing the focus, Iacioviello (2005) and Cloyne et al. (2015) argue that households' reactions to monetary policy shocks varies depending on variation in levels of mortgage indebtedness. We show that mortgage booms indeed have strong effects on the policy trade-off that are consistent with this literature. On the firm side, a venerable literature (Bernanke and Gertler, 1995; Kashyap et al., 1994; Kashyap and Stein, 1995) argued that credit constrained firms are more responsive to monetary policy. More recently, Ottonello and Winberry (2018) focus on firm (credit) heterogeneity to investigate asymmetry in the investment channel of monetary policy. In contrast to our earlier findings on mortgage credit, we find little evidence of asymmetries related to firm-side credit growth.

This paper thus makes a number of contributions to monetary economics and also to empirical macroeconomics broadly speaking. On average, monetary policy has strong and long-lasting effects, consistent with results from the recent literature. However, monetary policy effects can vary greatly with the state of the economy, and that state may depend on a rich set of characteristics, including the business cycle, inflation, and leverage. In terms of empirical methods, we are careful to spell out, in a dynamic local projection framework, the crucial IV identification assumptions and how they might be valid for some subpopulations but not for others. Moreover, we introduce methods to provide well-reasoned bounds on potential leftover biases coming from untestable failures of the IV exclusion restriction. All of these new developments should provide for a tighter link with long-standing policy evaluation methods in applied microeconomics and bring empirical macroeconomics closer to a unified protocol for data analysis.

2. The trilemma of international finance: A quasi-natural experiment

In open economies, exchange rates, capital flows, and monetary policy—and, thus, their management—are all intertwined. The observation that countries cannot simultaneously control all three of these policy components is known as the *trilemma* of international finance (see, e.g., Obstfeld et al., 2004, 2005; Obstfeld and Taylor, 1998, 2004; Shambaugh, 2004). In textbook theory, countries with credible fixed exchange rates and open to capital mobility must follow the interest rate of the base country they peg to.

Here, due to the broader and richer historical context we study, we must separately consider each of the three components of the trilemma to construct our instrument: (1) the choice of exchange rate regime; (2) the degree of capital mobility; and (3) the interest rate of the base country. Moreover, rather than directly using the base country interest rate, we first sterilize the predictable component that could be explained by economic conditions in the base country. The specific construction of the resulting instrumental variable, and the manner in which the analysis is designed around it, sets our paper apart from the literature. And unlike the majority of empirical papers in monetary economics limited to post-WW2 U.S. data, we rely on data for 17 advanced economies at annual frequency from 1870 to 2006.[3]

2.1. Defining the base country in fixed exchange rate regimes

The trilemma naturally breaks the sample into three subpopulations. First, there is the group of *base* countries whose currency serves as the focal anchor for pegging economies. The latter, the group denoted the *pegs*, then form the second of our subpopulations. The remaining economies, which allow their currency to be determined freely in the market, is the group we call the *floats*, the third subpopulation. This subsection discusses the construction of these subpopulations.

Part of the data construction effort consists of defining pegs, and also the base countries for pegs, across different eras. This is described in detail in online Appendix A and summarized in Table 1. The possible base country interest rates used at different times in the history of exchange rate regimes correspond to the four rows in the table. The four major eras correspond to the four columns in the table. The table cells indicate which pegging countries correspond to each base in each era.

Central banks of base countries, like the U.S. Federal Reserve in the Bretton Woods era, generally have paid little to no attention to economic conditions in other countries when making policy choices. Such behavior finds ample support in the historical record, as discussed in Jordà et al. (2015). Thus, to peg is to sacrifice monetary policy autonomy, at least to some degree.

If a country is currently in a peg *and* it was in a peg the previous year then we define the exchange rate regime indicator $q_{i,t} = 1$, otherwise $q_{i,t} = 0$. This definition makes a more conservative choice to ensure that the peg is well-established and somewhat credible before including a particular country-time pair $\{i, t\}$ when constructing the subpopulation of pegs. That is, we want to eliminate opportunistic pegging motivated by conditions that could be related to a country's monetary conditions.

[3] These advanced economies are: Australia, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, U.K., and U.S. The data, most current coverage, and detailed descriptions are available online at http://www.macrohistory.net/data. See Jordà Schularick and Taylor (2017). We limit the sample to 2006 to avoid contamination from the Global Financial Crisis.

Table 1
Selection of base country short-term interest rate for pegged exchange rates by era.

Base country interest rate	Home country interest rates associated with the base country			
	Pre-WW1	Interwar	Bretton Woods	Post-BW
UK (Gold standard/BW base)	All countries		Sterling bloc: AUS*	
UK/USA/France composite (Gold standard base)		All countries		
USA (BW/Post-BW base)			All other countries	Dollar bloc: AUS, CAN, CHE, JPN, NOR
Germany (EMS/ERM/Eurozone base)				All other countries
Float observations	170	126	97	220
Peg observations	493	95	345	341
Total observations	663	221	442	561

* We treat Australia as moving to a U.S. dollar peg in 1967.
Notes: See text, Jordà et al. (2015), Obstfeld et al. (2004), Obstfeld et al. (2005), and Ilzetzki et al. (2017). Pre-WW1: 1870–1914; Interwar: 1920–1938; Bretton Woods: 1948–1973; Post-BW: 1974–2006.

2.2. Constructing the instrument: The first stage

We have nearly all the elements in place to construct our instrumental variable. Specifically, let $\Delta r_{i,t}$ denote the change in short-term nominal interest rates in country i at time t, and let $\Delta r_{b(i,t),t}$ denote the short-term nominal interest in country i's base country b at time t, which can differ across i and over time—hence the notation $b(i, t)$. Both $\Delta r_{i,t}$, and $\Delta r_{b(i,t),t}$ are three-month short-term government bill or private market interest rates, the closest measure of monetary conditions that we were able to obtain consistently for our long and wide panel of historical data.[4]

Next, define the variable $k_{i,t} \in [0, 1]$ which indicates whether country i is open to international markets or not. We base this capital mobility indicator on the index (from 0 to 100) in Quinn et al. (2011). We use a continuous version of their index rescaled to the unit interval, with 0 meaning fully closed and 1 fully open. Over time, in the advanced economies we study, full international capital mobility has been notably interrupted by the two world wars. Resumption of mobility was nearly immediate after WW1. It was not so after WW2, in large part due to the tight constraints on capital movements that were central to the Bretton Woods regime. Nowadays, capital mobility is commonplace in all advanced economies.

More specifically, we find that in our data the average value of k for the pegs in the full sample is 0.87 (with a standard deviation of 0.21) versus 0.70 (0.31) for floats. In the post-WW2 era, these averages are virtually indistinguishable from one another, with values of 0.76 (0.24) for pegs and 0.74 (0.30) for floats. Thus, it cannot be said that pegs on average used more restrictions on capital to regain control over monetary policy than floats, and the subsamples are balanced on this dimension. Moreover, note that it is clear that restrictions on capital mobility have not been used as a high-frequency policy tool by pegging economies since the index is very slow moving in advanced economies, unlike interest rate policy settings.

Finally, we denote with $\Delta\widehat{r}_{b(i,t),t}$ movements in base country $b(i, t)$ rates explained by observable controls for that base, denoted $\boldsymbol{x}_{b(i,t),t}$. Similarly, denote with $\boldsymbol{x}_{i,t}$ a broad set of domestic macroeconomic controls in country i. Such controls include current and lagged values of macroeconomic aggregates, and lagged values of the policy variable. Putting all these elements together, our instrument is constructed to equal $z_{i,t} \equiv k_{i,t}(\Delta r_{b(i,t),t} - \Delta\widehat{r}_{b(i,t),t})$ if $q_{i,t} = 1$, and to equal $z_{i,t} = 0$ if $q_{i,t} = 0$.

Notice three features of how the instrument is constructed. First is our focus on isolating *unpredictable* movements in base country interest rates.[5] The extent to which there are external factors affecting base and pegging economies will tend to contaminate the raw measure, but not its unpredictable component. Second, notice that we observe an important prescription of the trilemma by modulating the strength of the instrument according to the degree of capital openness, $k_{i,t}$. Third, the instrumental variable is allowed to operate only for pegs, as the trilemma dictates. These are all important innovations with respect to what has been previously attempted in the literature.

An assumption that will play a key role later in our analysis is that of monotonicity, formally:

Assumption 1 (Monotonicity)**.** Let Δr denote the indicator of domestic monetary conditions, $\boldsymbol{x}$ the vector of domestic controls, including lags of Δr, and let z denote the instrument for Δr. We omit subindices for simplicity. Then we assume that in population:

$$\frac{\partial E(\Delta r\,|\,\boldsymbol{x})}{\partial z} \geq 0.$$

[4] Swanson and Williams (2014) and Gertler and Karadi (2015) are two recent examples of papers that step back from using typical interbank overnight rates and instead measure monetary policy with government rates for bonds at a duration of up to 2-years in some cases.

[5] A lower standard of proof would be to use $\Delta r_{b(i,t),t}$ directly. Experiments with such an instrument produced similar results to those reported below and are available upon request.

Table 2
First-stage relationship between change in short-rates for pegs and the trilemma instrument.

Dependent variable: $\Delta r_{i,t}$	No controls			With controls		
	(1) All years	(2) Pre-WW2	(3) Post-WW2	(4) All years	(5) Pre-WW2	(6) Post-WW2
Pegs: $q = 1$						
$z_{i,t}$ (instrument)	0.57***	0.40***	0.65***	0.55***	0.31**	0.62***
	(0.09)	(0.09)	(0.10)	(0.08)	(0.12)	(0.06)
t-statistic	[6.55]	[4.26]	[6.43]	[6.87]	[2.64]	[9.68]
Observations	1072	438	634	727	192	535

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Full sample: 1870–2006 excluding 1914–1919 and 1939–1947. Pre-WW2 sample: 1870–1938 (excluding 1914–1919). Post-WW2 sample: 1948–2006. Country fixed effects included in the regressions for columns 4–6. These regressions also include up to two lags of the first difference in log real GDP, log real consumption, investment to GDP ratio, credit to GDP, short and long-term government rates, log real house prices, log real stock prices, and CPI inflation. In addition we include world GDP growth to capture global cycles. See text.

Basically, following the trilemma we assume that if the base country raises its interest rate, then on average the home country will do so too. A similar monotonicity assumption can be found, e.g., in Angrist and Imbens (1994).

Table 2 reports first-stage regression results of the endogenous variable, $\Delta r_{i,t}$ on the instrument $z_{i,t}$, without controls in columns 1–3, and more formally with controls, in columns 4–6. We do this for the subpopulation of pegs as the trilemma suggests.[6] The table clearly shows that $z_{i,t}$ is not a weak instrument. Columns 4–6 refer to the formal first-stage regression with controls, country fixed effects and robust (clustered) standard errors (the regression also allows the coefficients of the controls to differ for the period 1973–80 to account for instability as we discuss below). The t-statistic on $z_{i,t}$ is well above 3 for the full and post-WW2 samples. Moreover, notice that the slope estimates in columns 4–6 are similar to those in columns 1–3, which suggests that the instrument is relevant in that contains information that is quite different from that contained in the controls.[7]

3. Methods

The central goal of our paper is to evaluate the effects of a domestic monetary policy intervention on domestic macroeconomic outcomes. Identification of the causal effect is based on an external instrumental variables (IV) approach using the trilemma discussed in the previous section. Identification depends on the exclusion restriction, in this case the assumption that base country interest rates affect peg economies only through the interest rate channel.

When the model is just identified—as ours is—the exclusion restriction is untestable. Based on how the instrument is constructed and the economics of the trilemma, we have solid justification for why the exclusion restriction holds, at least approximately. However, we also discuss a method to provide interpretable bounds on potential spillover effects even if the exclusion restriction were to fail—another innovation of this paper relative to the literature.

We propose a method to calculate these bounds using a control function approach by taking advantage of the float subpopulation for which the trilemma instrument is known to be invalid. The way we calculate such bounds is to assume the worst case scenario, that is, that any effect of base country interest rates on floating economies operates *entirely* through spillover effects and not through interest rate channels.

Define the random variable $y(h)$ in reference to a macroeconomic outcome of interest observed at a horizon h periods from today for $h = 0, 1, \ldots, H-1$. For example, we may be interested in the growth outcome given by 100 times the log difference between real GDP in $t+h$ relative to t when computing a cumulative impulse response resulting from a monetary policy intervention. We can collect all such random variables into an $H \times 1$ vector $\boldsymbol{y} = (y(0), y(1), \ldots, y(h), \ldots, y(H-1))$.

In reporting the average effects of a policy intervention over time, we will adopt the convention of normalizing the intervention to a 1 percentage point (100 bps) increase in interest rates ($\Delta r = 1$) compared to a counterfactual of leaving interest rates unchanged ($\Delta r = 0$). Further, we denote as $\boldsymbol{x}$ the vector of controls which include lags of the outcome, lags of the intervention variable, and any other exogenous or predetermined variables. Moreover, we assume that the controls affect the outcome linearly for the moment.

Like the *average treatment effect* in applied microeconomics, we are interested in the impulse response given by the forecast path for the outcome variable and its counterfactual, that is

$$\mathcal{R}_{ATE} \equiv E(\boldsymbol{y} \mid \Delta r = 1; \boldsymbol{x}) - E(\boldsymbol{y} \mid \Delta r = 0; \boldsymbol{x}), \tag{1}$$

[6] Similar estimates for the float subpopulation are consistent with a nearly zero and insignificant pass-through of base rates to domestic rates, as the trilemma suggests. They are available upon request.

[7] The control list includes up to two lags of the first difference in log real GDP, log real consumption, investment to GDP ratio, credit to GDP, short and long-term government rates, log real house prices, log real stock prices, and CPI inflation.

where we note that $\mathcal{R}_{ATE}$ is a vector of dimension $H \times 1$. Notice that $\mathcal{R}_{ATE}$ could be estimated by using a vector autoregression (or VAR). For reasons that will become apparent shortly, we prefer to approximate the conditional expectations in (1) using local projections (Jordà, 2005).

In particular, consider the following set of panel local projections,

$$y_{i,t+h} = \alpha_{i,h} + \Delta r_{i,t}\beta_h + \boldsymbol{x}_{i,t}\boldsymbol{\gamma}_h + v_{i,t+h}\,; \qquad \text{for } h = 0, \ldots, H-1, \tag{2}$$

using a longitudinal sample where $i = 1, \ldots, N$; and $t = 1, \ldots, T$. Clearly, $\mathcal{R}_{ATE} = \boldsymbol{\beta} = (\beta_0, \ldots, \beta_{H-1})'$. Expression (2) could be naïvely estimated by standard OLS panel methods, which we refer to as the LP-OLS estimate. Notice that $\alpha_{i,h}$ is a fixed effect. Also note for later reference that we will include a global real GDP growth variable to parsimoniously remove global business cycle effects.[8]

What about causality? Under OLS, the identification of causal effects for the coefficient β_h in expression (2) relies, roughly speaking, on Δr being randomly assigned given the controls included in the regression. One way to express the conditions under which such a regression would be causally interpretable is to use the potential outcomes approach (see Rubin, 1974). Using standard notation, and in a linear setup such as ours, it would suffice to assume the following:

Assumption 2 (Conditional mean independence)**.** Let $\boldsymbol{y}_1$ denote the random variable drawn from the treated subpopulation distribution of potential outcomes, that is when $\Delta r = 1$. Similarly, let $\boldsymbol{y}_0$ refer to the random variable drawn from the control subpopulation distribution of potential outcomes, that is, when $\Delta r = 0$. We assume that

$$E(\boldsymbol{y}_1 \,|\, \Delta r = 1, \boldsymbol{x}) = E(\boldsymbol{y}_1 \,|\, \boldsymbol{x}) \quad \text{and} \quad E(\boldsymbol{y}_0 \,|\, \Delta r = 0, \boldsymbol{x}) = E(\boldsymbol{y}_0 \,|\, \boldsymbol{x}), \tag{3}$$

that is, conditional on the controls, $\boldsymbol{x}$, there is no selection mechanism that explains differences in the conditional means of the potential outcomes $\boldsymbol{y}_1$ and $\boldsymbol{y}_0$.

Put differently, given $\boldsymbol{x}$, Δr is as good as if it were randomly assigned. Parenthetically, this type of assumption permeates the VAR literature, although it is rarely stated like this. For example, identification based on zero short-run restrictions and Cholesky ordering is a type of conditional mean assumption. The Cholesky ordering is equivalent to calculating the conditional mean $E(\boldsymbol{y} \,|\, \Delta r, \boldsymbol{x})$ by including in the control vector $\boldsymbol{x}$ those variables ordered first in the assumed ordering dated contemporaneously with the treatment variable Δr in addition to their lags. However, as we show below, the data *strongly reject* this conditional mean assumption.

3.1. Identification with external instruments

Fluctuations in the unpredictable component of the base country policy rate, modulated according to the mobility of capital across borders, induce exogenous fluctuations in the policy rate of countries that peg their exchange rate to it. Hence, the correlation between the trilemma instrument and the policy intervention variable can be used to calculate a *local average treatment effect* as in Angrist and Imbens (1994): because the instrument does not operate for floats, causal effects can only be formally estimated for the subpopulation of pegs. In terms of the conditions needed for our instrument to be valid in this sense, we make the usual assumptions, that is:

Assumption 3 (Relevance and exogeneity)**.** We assume

$$\begin{aligned} &\text{Relevance:} && L(\Delta r \,|\, \boldsymbol{x}, z; q = 1) \neq L(\Delta \,|\, \boldsymbol{x}; q = 1), \\ &\text{Exogeneity:} && L(\boldsymbol{y}_j \,|\, \boldsymbol{x}, \Delta r, z; q = 1) = L(\boldsymbol{y}_j \,|\, \boldsymbol{x}, \Delta r; q = 1) \text{ for } j = 0, 1, \end{aligned} \tag{4}$$

where, for example, $L(\Delta r \,|\, \boldsymbol{x}, z)$ refers to the linear projection of Δr on $\boldsymbol{x}$ and z.

A few comments are in order. First, Assumption 3 is milder than making assumptions on conditional expectations since identification is based on the usual covariance between instrument and policy intervention. Note also that we explicitly condition only on $q = 1$ to emphasize that in our trilemma-based application, these assumptions only need to hold for the subpopulation of pegs. Later we evaluate the robustness of the results to failure of the exclusion restriction in expression (4).

We now have the ingredients required to estimate the causal effects of a policy intervention for the subpopulation of pegs. Using Assumptions 1 and 3, and given a sample of panel data, we can now estimate the following set of local projections using instrumental variables (LP-IV),

$$y_{i,t+h} = \alpha_{i,h} + \boldsymbol{x}_{i,t}\boldsymbol{\gamma}_h + \Delta\widehat{r}_{i,t}\beta_h + v_{i,t+h}\,; \qquad \text{for } h = 0, \ldots, H-1, \tag{5}$$

which can be compared to the LP-OLS form at (2), and where the estimates of $\Delta\widehat{r}_{i,t}$ come from the first stage regression

$$\Delta r_{i,t} = a_i + \boldsymbol{x}_{i,t} g + z_{i,t} b + \eta_{i,t}\,. \tag{6}$$

The impulse response can therefore be expressed as

$$\mathcal{R}_{LATE} = E(\boldsymbol{y}_1 - \boldsymbol{y}_0 \,|\, \Delta r, \boldsymbol{x}, z; q = 1) = \boldsymbol{\beta} = (\beta_0, \ldots, \beta_{H-1})', \tag{7}$$

[8] Time fixed effects would require, literally, over a hundred additional parameter estimates.

which can be estimated from the sequence of equations in expression (5).

Several remarks are worth making. First, the control vector $\boldsymbol{x}$ will include contemporaneous values of all the variables except the outcome variable (since we begin at $h = 0$ to avoid singularity). This is to provide insurance against variation in the policy intervention that could have been explained by information observed concurrently with the policy treatment. Second, expressions (5) and (6) include fixed effects and global GDP to capture global business cycle effects. Third, standard errors are estimated using a clustered-robust covariance matrix estimator, which conveniently removes serial correlation induced by the local projection setup nonparametrically.

3.2. Checking for spillovers: Robustness of the exclusion restriction

Economically speaking, a violation of the exclusion restriction could occur if base rates affect home outcomes through channels other than movements in home rates. Additional influences via such channels are sometimes referred to as *spillover effects*. These could occur if base rates proxy for factors common to all countries. That said, these factors would have to persist despite having included global real GDP growth to soak up such business cycle variation, and despite controlling for base and home country economic conditions—a tall order. Our problem, specifically the break down into different subpopulations, offers a unique opportunity to assess such spillover effects more formally than is customarily possible.

Consider a simple example to present the basic idea (online Appendix B contains more formal derivations). Let y be a univariate outcome variable, Δr the intervention, and z the instrument. We abstract from the constant term, controls, state dependence, and any other complication for now. The standard IV setup consists of the first and second stage regressions given by

$$\begin{aligned} \Delta r &= zb + \eta\,, \\ y &= \Delta\hat{r}\,\beta + z\,\phi + \nu\,. \end{aligned} \tag{8}$$

Typically we assume $E(\Delta r\,\nu) \neq 0$, but $E(z\,\nu) = 0$. The exclusion restriction refers to the assumption that $\phi = 0$. If this restriction were not to hold, it is easy to see that

$$\hat{\beta}_{IV} \xrightarrow{p} \beta + \frac{\phi}{b}\,.$$

This last expression is both simple and intuitive: the bias induced by the failure of the exclusion restriction depends on both the size of the failure, ϕ, *and* the strength of the instrument, b. Weaker instruments will tend to make the bias worse. This point was made in, for example, Conley et al. (2012).

The float subpopulation ($q = 0$) contains useful information that we now exploit. Continue to assume that $E(z\,\nu) = 0$. We think this is justified since large economies (typically bases) with monetary policy autonomy are unlikely to consider the macroeconomic outlook of smaller countries (typically non-base floats) when setting rates. Hence, consider estimating (8) using OLS when $q = 0$. Estimates of the intervention effect β and the spillover effect ϕ will be biased as long as $E(\Delta r \nu) \neq 0$ and $b \neq 0$. However, it is easy to show (under standard regularity conditions) that the equivalent OLS estimates of expression (8) are such that

$$\left.\begin{aligned} \hat{\beta}_{OLS} &\xrightarrow{p} \beta - \theta \\ \hat{\phi}_{OLS} &\xrightarrow{p} \phi + b\theta \end{aligned}\right\} \quad \text{with a bias term} \quad \theta = \frac{E(\Delta r \nu)\,E(z^2)}{E(z\Delta r)^2 - E(z^2)\,E(\Delta r^2)}\,. \tag{9}$$

Expression (9) is intuitive. If $E(\Delta r \nu) > 0$, then $\theta > 0$ and the effect of domestic interest rates on outcomes, $\hat{\beta}_{OLS}$, will be *attenuated* by the bias term θ. Similarly, the spillover effect, $\hat{\phi}_{OLS}$, will be *amplified* by an amount $b\theta$. This amplification will be larger the stronger the correlation between Δr and z, as measured by the pseudo first-stage coefficient b.

Later we show in Table 4 that the difference between OLS and IV estimates suggests that there is considerable attenuation bias in $\hat{\beta}$. The implication is that simple OLS will tend to make the spillover effect seem *larger* than it really is, and the interest rate response *smaller* than it really is. (Of course, if $E(\Delta r \nu) < 0$, then $\theta < 0$, and the sign of the biases would be reversed. A priori the direction of the bias is ambiguous, as we cautioned earlier.)

Without loss of generality, suppose that $\beta = \lambda\phi$, that is, the true domestic interest rate effect on outcomes is a scaled version of the spillover effect from the foreign interest rate. In this case,

$$\hat{\phi}(\lambda) = \frac{\left(\hat{\phi}_{OLS} + \hat{b}\,\hat{\beta}_{OLS}\right)}{1 + \lambda\,\hat{b}} \xrightarrow{p} \beta(\lambda)\,. \tag{10}$$

Taking λ as given, we can use a control function approach (Wooldridge 2015) to correct our LP-IV estimates of $\mathcal{R}_{LATE}$ for biases due to potential spillover effects. Expression (8) can then be rewritten as

$$\left(y - z\,\hat{\phi}(\lambda)\right) = \Delta r\,\beta + \nu + z\left(\hat{\phi}(\lambda) - \phi(\lambda)\right).$$

Moreover, the usual moment conditions imply that

$$E\left(z\left(y - z\hat{\phi}(\lambda)\right)\right) = E(z\Delta r)\,\beta + E(z\nu) + E\left(z^2\left(\hat{\phi}(\lambda) - \phi(\lambda)\right)\right),$$

$$\text{with} \quad E(z\nu) = 0\,, \quad \text{and} \quad \left(\hat{\phi}(\lambda) - \phi(\lambda)\right)\frac{1}{N_p}\sum_j^{N_p} z_j^2 \ \xrightarrow{p}\ 0\,,$$

as long as

$$\frac{1}{N_p}\sum_j^{N_p} z_j^2 \ \xrightarrow{p}\ Q_z < \infty\,, \quad \text{and} \quad N_f \longrightarrow \infty \quad \text{as} \quad N_p \longrightarrow \infty\,,$$

with N_f and N_p denoting the sizes of the subpopulations of floats and pegs respectively.

From this, we can now present a extension of our IV estimator *corrected for potential spillover effects*, where this new variant is constructed by subtracting the spillover term from the outcome variable in the standard IV coefficient estimator, that is

$$\hat{\beta}(\lambda) \equiv \frac{\frac{1}{N_p}\sum z_j\left(y_j - z_j\hat{\phi}(\lambda)\right)}{\frac{1}{N_p}\sum z_j \Delta r_j} \ \xrightarrow{p}\ \beta(\lambda)\,.$$

We have assumed that the sample sizes of both float and peg subpopulations tend to infinity. In practice, λ is unknown. We proceed below by using economic arguments to provide an interval of plausible values $\lambda \in [\underline{\lambda}, \overline{\lambda}]$ over which we compute $\hat{\beta}(\lambda)$. This interval provides a sense of the sensitivity of our benchmark LP-IV estimates of $\mathcal{R}_{LATE}$ to potential spillover contamination.

4. Monetary policy interventions: Understanding the subpopulations

The next sections apply the methods just described to investigate the effects of a monetary policy intervention on a wide variety of outcomes central to monetary economics. Throughout its history, a country can fall into any of the following bins defined earlier: *pegs*, *floats*, and *bases*. For example, during Bretton Woods, Germany was in a *peg* to the dollar. With the end of Bretton Woods, and later the introduction of the European Monetary System, we consider Germany to become a *base* for many European economies. And there are other periods where we classify Germany as a *float*, as was the case for much of the interwar period.

When presenting results, we will always measure and display the outcome variable in deviations relative to its initial value in year 0, with units shown in percent of the initial year value (computed as log change times 100), except in the case of interest rates where the response will be measured in units of percentage points. The policy intervention variable will be defined as the one-year change in the short-term interest rate in year 0, and normalized in all cases to a 1 percentage point, or 100 basis points (bps) increase.

The vector of explanatory variables includes a rich set of macroeconomic controls consisting of the first-difference of the contemporaneous values of all variables (excluding the response or outcome variable), and up to 2 lags of the first-difference of all variables, including the response variable. The list of macroeconomic controls is: log real GDP per capita; log real consumption per capita; log real investment per capita; log consumer prices; short-term interest rate (usually a 3-month government security); long-term interest rate (usually a 5-year government security); log real house prices; log real stock prices; and the credit to GDP ratio.[9]

In almost all respects, we found that this estimation setup produced stable outcomes. However, in line with the well-known "price puzzle" literature (e.g., Eichenbaum, 1992; Hanson, 2004; Sims, 1992), we found that there was substantial instability in the coefficients of the control variables, and that this finding was driven by the postwar high-inflation period of the 1970s. The traditional resolution of this puzzle has been to include commodity prices as a way to control for oil shocks. Given the constraints of our data, we addressed this issue by allowing the controls to take on a potentially different coefficient for the subsample period of years from 1973 to 1980 inclusive, thus bracketing the volatile period of the two oil crises.

4.1. LP-OLS: Subpopulation $\mathcal{R}_{ATE}$ under conditional mean independence

We begin our analysis by following the older selection-on-observables tradition. We naïvely estimate via LP-OLS the effect of an interest rate intervention on output levels (measured by log real GDP per capita) and prices levels (measured by log CPI)—two variables that commonly feature in many central bank mandates. These results are provided in Table 3 and are based on a panel regression that allows the relevant coefficient estimates to vary for each of the three subpopulations that we consider: pegs, floats and bases.

These estimates are a natural benchmark: *if* regression control is sufficient to achieve identification, then we could quite easily obtain estimates of $\mathcal{R}_{ATE}$ simply by averaging standard panel-based estimates across subpopulations. Hence the table

[9] The data are described in more detail in Jordà Schularick and Taylor (2017), and its online appendix.

Table 3
LP-OLS. Real GDP per capita and CPI price responses to interest rates.

Responses at years 0 to 4 (100 × log change from year 0 baseline).								
(a) Full sample	*Output response*			*P=F=B*	*Price response*			*P=F=B*
Year	Pegs (1)	Floats (2)	Bases (3)	*p*-value (4)	Pegs (5)	Floats (6)	Bases (7)	*p*-value (8)
$h=0$	0.06**	0.02	0.21***	0.01	0.13*	0.30***	0.05	0.13
	(0.02)	(0.08)	(0.04)		(0.07)	(0.11)	(0.07)	
$h=1$	−0.27***	−0.26	−0.36***	0.72	0.18	0.63***	0.09	0.10
	(0.10)	(0.18)	(0.07)		(0.14)	(0.24)	(0.15)	
$h=2$	−0.36**	-0.47**	−0.63***	0.28	0.03	0.60	−0.11	0.17
	(0.16)	(0.24)	(0.10)		(0.22)	(0.37)	(0.21)	
$h=3$	−0.43**	−0.43*	−0.52***	0.89	−0.20	0.36	−0.64**	0.12
	(0.18)	(0.23)	(0.13)		(0.32)	(0.47)	(0.25)	
$h=4$	−0.43*	−0.42	−0.51***	0.94	−0.36	0.26	−1.11***	0.07
	(0.23)	(0.27)	(0.14)		(0.42)	(0.56)	(0.30)	
$H_0: subATE=0$	(0.00)	(0.04)	(0.01)		(0.02)	(0.00)	(0.27)	
Observations		949				949		
(b) Post-WW2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$h=0$	0.03	−0.01	0.14***	0.02	0.12**	0.18*	0.07	0.56
	(0.02)	(0.07)	(0.03)		(0.06)	(0.10)	(0.06)	
$h=1$	−0.23**	−0.28*	−0.41***	0.15	0.18	0.41*	0.14	0.44
	(0.10)	(0.17)	(0.07)		(0.13)	(0.22)	(0.11)	
$h=2$	−0.26*	−0.43**	−0.75***	0.00	0.08	0.37	−0.02	0.52
	(0.15)	(0.21)	(0.10)		(0.20)	(0.34)	(0.16)	
$h=3$	−0.29*	−0.35*	−0.65***	0.08	−0.10	0.11	−0.47**	0.39
	(0.17)	(0.20)	(0.13)		(0.28)	(0.41)	(0.21)	
$h=4$	−0.25	−0.31	−0.63***	0.10	−0.23	−0.01	−0.81***	0.25
	(0.20)	(0.23)	(0.14)		(0.37)	(0.48)	(0.26)	
H_0: *LATE* = 0	0.00	0.01	0.00		0.01	0.00	0.18	
Observations		761				761		

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *P* denotes pegs, *F* floats, *B* bases. Cluster robust standard errors in parentheses. Full sample: 1870–2006 excluding WW1: 1914–1919 and WW2: 1939–1947. PostWW2 sample: 1948–2006. The column *P=F=B* displays the *p*-value of the null that for a given horizon *h*, estimates of the corresponding elasticity are equal across subpopulations. $H_0: subATE=0$ refers to the null that the coefficients for $h=0,\ldots,4$ are jointly zero for a given subpopulation. See text.

evaluates whether estimates across subpopulations are statistically different from one another. In addition, we also provide a joint test that, over the 5 horizons considered, the effect of interest rates on output and prices is zero. The analysis is conducted over the full and the post-WW2 samples.

Consider the output responses first, reported in columns 1–3. Full sample results indicate some minor differences across subpopulations. The *p*-values of the null that the coefficients are equal is reported in column 4. The differences are economically minor, however. The post-WW2 results in column 4 suggest that if anything, the differences are even less important over this sample. Generally speaking, the coefficient estimates have the expected signs. An increase in interest rates causes output to decline. Note that in all cases the effect is statistically different from zero as reported in the rows labeled $H_0: subATE=0$, by which we denote "subpopulation ATE."

The price responses reported in columns 5–7 fit intuition less neatly. The overall effect of an interest rate increase on prices in the full sample is essentially null for pegs and floats (columns 5 and 6 respectively), but negative for bases with a -1.11 significant response in year $h=4$. The picture changes somewhat for the post-WW2 subsample. Responses are essentially zero for $h=0,1,$ and 2. Negative signs appear for $h=3,$ and 4, but the responses are generally not very different from zero in the statistical sense (except for bases, again).

What are the main takeaways from the naïve LP-OLS estimates Table 3? On first glance there is little evidence that anything is amiss. Output and price responses across subpopulations are similar, have the expected signs, and are statistically significant (although for prices only after year $h=3$). On average across subpopulations, post-WW2 results indicate that a one percent increase in interest rates would reduce output and price levels roughly between one quarter to half a percentage point over 5 years, at best about a tenth of a percent in annual rate of decline. The price responses offer a less reassuring picture, in large part because the responses are generally insignificant and often have the "wrong" sign early on. The next step is to examine the estimates of the local average treatment effect for the pegs and for bases. Here, any departure from the parameter estimates just reported above would be indicative of a violation of the conditional mean independence assumption.

Table 4
LP-OLS vs. LP-IV. Attenuation bias of real GDP per capita and CPI price responses to interest rates. Trilemma instrument. Matched samples.

Responses at years 0 to 4 ($100\times$ log change from year 0 baseline).						
(a) Full sample	*Output response*		*OLS=IV*	*Price response*		*OLS=IV*
Year	LP-OLS (1)	LP-IV (2)	p-value (3)	LP-OLS (4)	LP-IV (5)	p-value (6)
$h=0$	0.09***	−0.08	0.07	0.09	−0.03	0.53
	(0.03)	(0.09)		(0.07)	(0.20)	
$h=1$	−0.19*	−0.63***	0.01	0.18	−0.47	0.08
	(0.10)	(0.17)		(0.14)	(0.38)	
$h=2$	−0.28*	−1.70***	0.00	0.09	−1.23**	0.01
	(0.17)	(0.33)		(0.25)	(0.53)	
$h=3$	−0.29	−1.85***	0.00	−0.12	−2.27***	0.01
	(0.20)	(0.41)		(0.37)	(0.79)	
$h=4$	−0.20	−2.57***	0.00	−0.26	−3.18***	0.00
	(0.25)	(0.62)		(0.51)	(1.02)	
KP weak IV		88.19			63.13	
$H_0: subATE=0$	0.00	0.00		0.06	0.01	
Observations	639	639		639	639	
(b) Post-WW2	(1)	(2)	(3)	(4)	(5)	(6)
$h=0$	0.05**	−0.06	0.11	0.08	0.17	0.51
	(0.02)	(0.07)		(0.06)	(0.13)	
$h=1$	−0.15	−0.75***	0.01	0.13	0.07	0.76
	(0.10)	(0.23)		(0.12)	(0.21)	
$h=2$	−0.21	−1.44***	0.00	0.00	−0.42*	0.09
	(0.15)	(0.32)		(0.21)	(0.25)	
$h=3$	−0.20	−1.38***	0.00	−0.24	−1.03***	0.01
	(0.17)	(0.35)		(0.30)	(0.32)	
$h=4$	−0.10	−1.73***	0.00	−0.45	−1.50***	0.01
	(0.21)	(0.46)		(0.42)	(0.41)	
KP weak IV		111.42			86.69	
$H_0: subATE=0$	0.00	0.00		0.00	0.01	
Observations	497	497		497	497	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Cluster robust standard errors in parentheses. Full sample: 1870–2006 excluding WW1: 1914–1919 and WW2: 1939–1947. PostWW2 sample: 1948–2006. *Matched sample* indicates LP-OLS sample matches the sample used to obtain LP-IV estimates. *KP weak IV* refers to the Kleibergen-Paap test for weak instruments. $H_0: subATE=0$ refers to the p-value of the test of the null hypothesis that the coefficients for $h=0,\dots,4$ are jointly zero for a given subpopulation. *OLS=IV* shows the p-value for the Hausman test of the null that OLS estimates equal IV estimates. See text.

4.2. LP-IV $\mathcal{R}_{LATE}$: Two instruments, two subpopulations

We now compare LP-OLS estimates with LP-IV estimates based on our trilemma instrument for the subpopulation of pegs by matching the samples. This will generate small differences between the LP-OLS estimates in Table 4 and those in columns 1 and 5 in Table 3. We calculate the $\mathcal{R}_{LATE}$ of an interest rate intervention and evaluate any attenuation bias from violations of conditional mean independence using a Hausman test. Table 4 summarizes the main results.

The table is organized as follows. The output and price responses in columns 1 and 4 are LP-OLS estimates over the same sample as the LP-IV estimates reported in columns 2 and 5. Column 3 reports the p-value of the Hausman test of the null that the estimate in column 1 is equal to that in column 2; likewise, column 6 reports the Hausman test for columns 4 and 5. We check (again) whether the trilemma instrument is weak with Kleibergen-Paap tests. Finally, we test the null that all ATE coefficients are jointly zero, reporting the p-value of the test in the row labeled $H_0: subATE=0$.

The first task is to compare the LP-OLS responses reported in columns 1 and 4 here, with those reported in Table 3 in columns 1 and 5. Recall that in Table 3 we estimate the model using all observations, but allow coefficients to vary by subpopulation. The differences are relatively minor, owing to slight differences in the sample used given the availability of the instrument.

The important result of Table 4 is the size of the attenuation bias in the case of LP-OLS compared to LP-IV. The differences are economically sizable and statistically significant as indicated by the Hausman tests of columns 3 and 6. Conditional mean independence clearly fails. Using LP-OLS estimates (column 1) and the full sample, output would be estimated to be about 0.2% lower four years after an increase in interest rates of 1%. In contrast, the LP-IV effect is measured to be about a 2.6% decline, or about an 0.5% annualized rate of lower growth. A similar pattern is observable for the price response. Full sample

Table 5
LP-OLS vs. LP-IV. Attenuation bias of real GDP per capita and CPI price responses to interest rates. U.S. and U.K. using RRCH instrument.

Responses at years 0 to 4 (100 × log change from year 0 baseline).						
RRCH IV	*Output response*		*OLS=IV*	*Price response*		*OLS=IV*
Year	LP-OLS (1)	LP-IV (2)	*p*-value (3)	LP-OLS (4)	LP-IV (5)	*p*-value (6)
$h=0$	0.38***	0.15	0.88	0.36***	−0.50	0.11
	(0.10)	(0.22)		(0.10)	(0.48)	
$h=1$	0.01	−0.46	0.27	1.03***	0.21	0.61
	(0.19)	(0.41)		(0.25)	(0.62)	
$h=2$	−0.31	−1.25**	0.07	1.23***	−0.14	0.51
	(0.27)	(0.62)		(0.41)	(0.98)	
$h=3$	−0.12	−0.85	0.23	1.00*	−1.94	0.22
	(0.31)	(0.60)		(0.56)	(1.86)	
$h=4$	0.12	0.25	0.64	0.71	−3.78	0.15
	(0.34)	(0.57)		(0.71)	(2.79)	
KP weak IV		13.41			5.29	
$H_0: subATE=0$	0.00	0.50		0.00	0.30	
Observations	61	61		61	61	

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Cluster robust standard errors in parentheses. RRCH refers to the Romer and Romer (2004) and Cloyne and Hürtgen (2016) IV. U.S. sample: 1969–2007. U.K. sample: 1976–2007. *KP weak IV* refers to the Kleibergen-Paap test for weak instruments. $H_0: subATE=0$ refers to the *p*-value of the test of the null hypothesis that the coefficients for $h=0,\dots,4$ are jointly zero for a given subpopulation. *OLS=IV* shows the *p*-value for the Hausman test of the null that OLS estimates equal IV estimates. See text.

LP-OLS estimates are largely insignificant and often have the wrong sign. LP-IV estimates are sizable, significant, and have the right sign.

Comparing the full sample results with the post-WW2 results we find differences in the output response to be relatively minor. The price response, however, becomes somewhat delayed or stickier after WW2. The LP-IV response suggests that on impact and the year after, the price response is essentially zero although by year 4, prices are expected to be about 1.5% lower than they were four years earlier. Tests for weak instruments suggest the trilemma instrument is relevant and tests of the null show that the $\mathcal{R}_{LATE}$ estimated with LP-IV is statistically different from zero. Interest rates do indeed have a strong causal effect on output and prices for the subpopulation of pegs.

Next, Table 5 compares these results with estimates based on a different IV and subpopulation. We turn to the Romer and Romer (2004) instrument for the U.S., as updated and extended to the U.K. by Cloyne and Hürtgen (2016). We shall henceforth refer to this instrument as *RRCH*. Both the U.S. and the U.K. can be thought of as belonging to the subpopulation of bases and thus provide the best approximation of the $\mathcal{R}_{LATE}$ results for this group, where we should note that a similar IV for Germany as a base is not (yet) available.

The results in Table 5 are organized in a manner similar to Table 4. The table reports the output and price responses estimated by LP-OLS and LP-IV using the RRCH instrument. We note that the RRCH instrument is available only from 1969 to 2007 for the U.S. and 1976 to 2007 for the U.K. Because of the abbreviated sample, we limit the control set to save on degrees of freedom. We allow up to 3 lags of interest rates, output and inflation, but omit all other controls. This parsimonious specification still allows coefficients to vary over the oil crisis period of 1973–1980, as before. In terms of a formal model, one can think of this specification as the empirical counterpart to a three-variable New Keynesian VAR specification.

We can see that the LP-OLS and LP-IV estimates of the output response appear similar from the statistical perspective of the Hausman test reported in column 3. However, the sample is rather limited. Only 71 observations taxed by 11 regressors (a different constant for U.S. and U.K. observations and nine other regressors). Economically speaking, there is a fair amount of attenuation bias. By year $h=2$ the LP-OLS response is −0.3% compared to −1.25% for LP-IV. That said, both methods generally deliver the correct sign and the responses have a similar shape. The differences are more apparent for the price response. The LP-OLS response of prices to an interest rate intervention (column 4) is economically and statistically small, with coefficients that have the wrong sign. The LP-IV response has a similarly muted response initially but it becomes increasingly negative. By year 4, prices are expected to be about 3.8% lower than they would otherwise would be, a response for bases that is similar to that for pegs based on the trilemma IV as reported previously in Table 4.

How do these results compare to estimates found elsewhere in the literature? Romer and Romer (2004) find that in response to a shock in the funds rate of 1%, industrial production declines by about 4% by year 2 and is still down by 2% by year 4 (p. 1069, and Fig. 2). A version of their results based on a VAR delivers similar effects, though the response is slightly more muted (from 4% to 3% by year 2). Gertler and Karadi (2015) use high frequency identification methods on a more recent sample. The main results from Fig. 1 in their paper indicate that in response to an increase in the funds rate of 25 bps, industrial production declines by 0.4% in year 2 (or scaling the response up to 1% increase in the funds rate, a 1.6%

Table 6
LP-OLS. Real GDP per capita and CPI price responses to domestic and base-country interest rates. Full and post-WW2 samples for subpopulation of exchange rate float economies.

Responses at years 0 to 4 (100 × log change from year 0 baseline).						
(a) Full sample	*Output response to*		$\Delta r = z$	*Price response to*		$\Delta r = z$
	Δr	z	p-value	Δr	z	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
$h = 0$	0.03	0.15	0.25	0.33	−0.33*	0.15
	(0.14)	(0.09)		(0.32)	(0.17)	
$h = 1$	−0.13	−0.07	0.81	0.49	−0.34	0.37
	(0.26)	(0.17)		(0.53)	(0.44)	
$h = 2$	−0.07	−0.65**	0.29	0.50	−0.46	0.44
	(0.40)	(0.22)		(0.68)	(0.64)	
$h = 3$	0.01	−0.89***	0.02	0.35	−0.90	0.43
	(0.29)	(0.20)		(0.85)	(0.85)	
$h = 4$	−0.02	−0.93***	0.01	0.43	−1.45	0.32
	(0.30)	(0.25)		(1.01)	(0.95)	
Δr on z	0.15			0.15		
first stage estimate	(0.11)			(0.11)		
Observations	204			204		
(b) Post-WW2	(1)	(2)	(3)	(4)	(5)	(6)
$h = 0$	−0.01	0.13	0.16	0.18	−0.23	0.23
	(0.09)	(0.09)		(0.22)	(0.15)	
$h = 1$	−0.15	−0.08	0.78	0.19	−0.36	0.34
	(0.27)	(0.16)		(0.30)	(0.35)	
$h = 2$	−0.11	−0.61**	0.33	0.10	−0.39	0.52
	(0.38)	(0.25)		(0.35)	(0.48)	
$h = 3$	−0.07	−0.92***	0.01	−0.04	−0.67	0.52
	(0.29)	(0.22)		(0.42)	(0.64)	
$h = 4$	−0.17	−0.90***	0.03	0.08	−1.02	0.32
	(0.31)	(0.28)		(0.45)	(0.78)	
Δr on z	0.15			0.15		
first stage estimate	(0.12)			(0.11)		
Observations	174			174		

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Cluster robust standard errors in parentheses. Δr is the short-term interest rate of the domestic economy considered and indexed by i; $z_{i,t}$ refers to the base country short-term interest rate times the openness index. We use the U.K. as the base before 1939; we use the U.S. for the Bretton-Woods era (1946–1971) for all countries, and for Australia, Canada, Japan, and the U.K. after Bretton-Woods (1972 onward); and we use Germany for all remaining economies (all in Europe) in the post Bretton-Woods era (1972 onward). $\Delta r = z$ refers to the test of the null that the coefficient for the float rate is equal to that for the instrument and report the p-value of the test. Δr on z refers to the coefficient of the regression of the float rate on the instrument (called b in expression (8)). See text.

decline). Again, this is very similar to Romer and Romer (2004) with our results falling somewhere in the middle of these two well cited studies.

Turning to inflation, we find a decline in the CPI level of 4% by year 4 using the full sample (about 2% if using the post-WW2 sample). Romer and Romer (2004) find a decline in the producer price index of 6% by year 4 following the same 1% increase in the funds rate. The same results using the CPI less shelter and the PCEPI are closer to 3% instead. Gertler and Karadi (2015) find a response closer to a -0.60% decline in the CPI for a more recent sample. Again, we think our results nicely straddle these two landmark studies and should offer readers some reassurance.

4.3. *Checking for spillovers*

Table 6 reports OLS estimates of expression (8) based on the float subpopulation (with the usual control set). We employ the same trilemma instrument, but now utilize it in the *float* subpopulation to operationalize our control function approach. The left-hand side variables are log real GDP per capita and log CPI price level. Table 6 also reports the coefficient associated with the pseudo-first stage regression of Δr on z. This provides an estimate of the parameter b in expression (9).

Table 6 makes clear the intuition behind expression (9). The interest rate responses of real GDP per capita reported in column 1 are economically small. They are mostly statistically significant (usually at the 95% confidence level) in the full

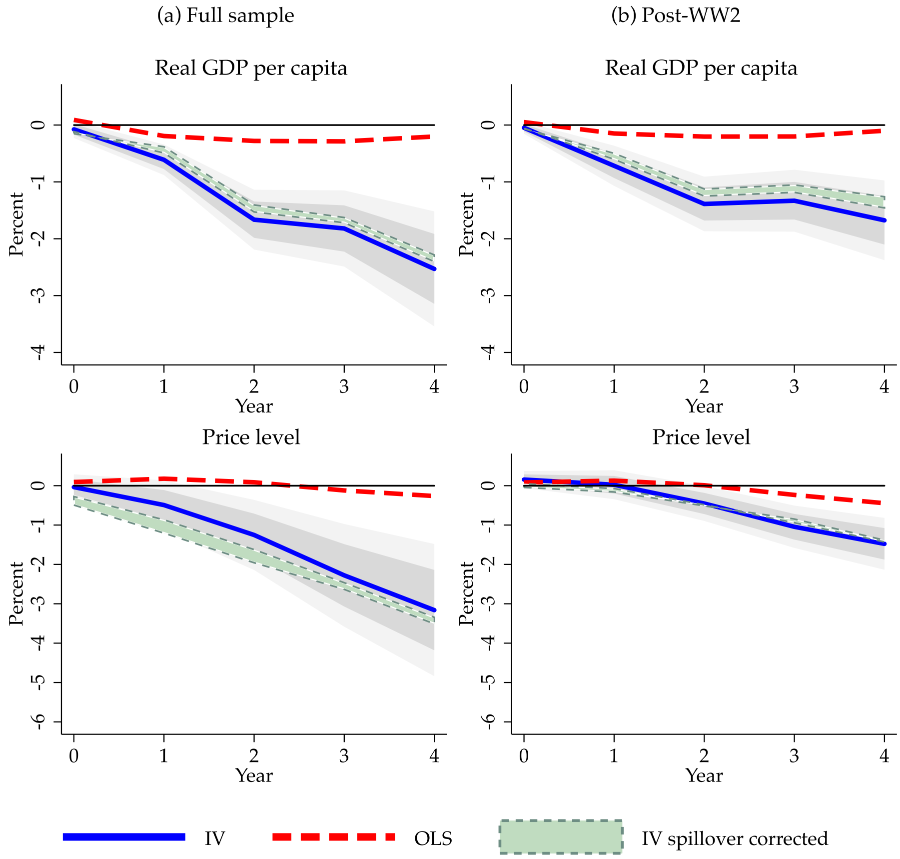

Fig. 1. Real GDP per capital and CPI price responses to a 1 percent increase in interest rates. LP-OLS, LP-IV, and spillover corrected LP-IV.
Notes: Full sample: 1870–2006 excluding WW1: 1914–1919 and WW2: 1939–1947. LP-OLS estimates displayed as a dashed red line, LP-IV estimates displayed as a solid blue line and 1 S.D. and 90% confidence bands, LP-IV spillover corrected estimates displayed as a light green shaded area with dashed border, using $\lambda \in [1, 8]$. See text. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

sample estimates reported in panel (a) of the table, and in the Post-WW2 sample reported in panel (b). In contrast, the response to the instrument (think of it as a shock to the base country interest rate) is much larger and significant. Price responses follow a different pattern, with responses to the own interest rate shock of the wrong sign, but responses to the base country interest rate (measured by the instrument) of the correct sign. This is a feature we will return to in the results reported below.[10]

Lastly, to make progress we need an auxiliary assumptions on λ, which cannot be determined from data. We assert that it is natural to assume that $\lambda \geq 1$. That is, home rates affect outcomes at least as strongly as rates in the foreign base country. In order to provide bounds, we use a range of values of λ between 1 and 8. In other words, the home rate effect is assumed to be 1 to 8 times larger than the base rate effect.

Using these assumptions and our spillover correction, Fig. 1 shows our adjusted estimates of $\mathcal{R}_{LATE}$. The figure displays responses to a 100 bps increase in home rates for real GDP per capita and the price level. The red dashed lines show the responses reported in Table 4, columns 1 and 4, based on LP-OLS and the rich control set described above. The responses clearly suffer from attenuation bias as described in expression (9). Next, the solid blue lines with associated point-wise error bands in grey show the LP-IV estimates reported in Table 4, columns 2 and 5. As we noted, the responses are considerably larger, both statistically (the null $H_0 : subATE = 0$ is rejected at the 1% level) and economically. Finally, the light green shaded region with a dashed border displays the range of responses that would result from our spillover correction using $\lambda \in [1, 8]$.

Several results deserve comment. First, the correction for spillover effects tends to attenuate the output responses as compared to our preferred LP-IV estimates. The correction suggests that the output response could be about 0.5 to 1 percentage points less negative by year 4 than that reported using LP-IV alone. In the post-WW2 era this means that the cumulative change in output is probably closer to −1% (about −0.2% per annum) than to −2% (about −0.4% per annum).

[10] We note that the regression of the domestic interest rate on the instrument and the control set is generally non-zero, but about half to one third the magnitude of the coefficient estimated in the first stage regression for the peg subpopulation and reported in Table 2. Compare 0.20 for the floats with 0.40 for the pegs (using full sample estimates in the case for output). These results are consistent with those reported in Obstfeld et al. (2005).

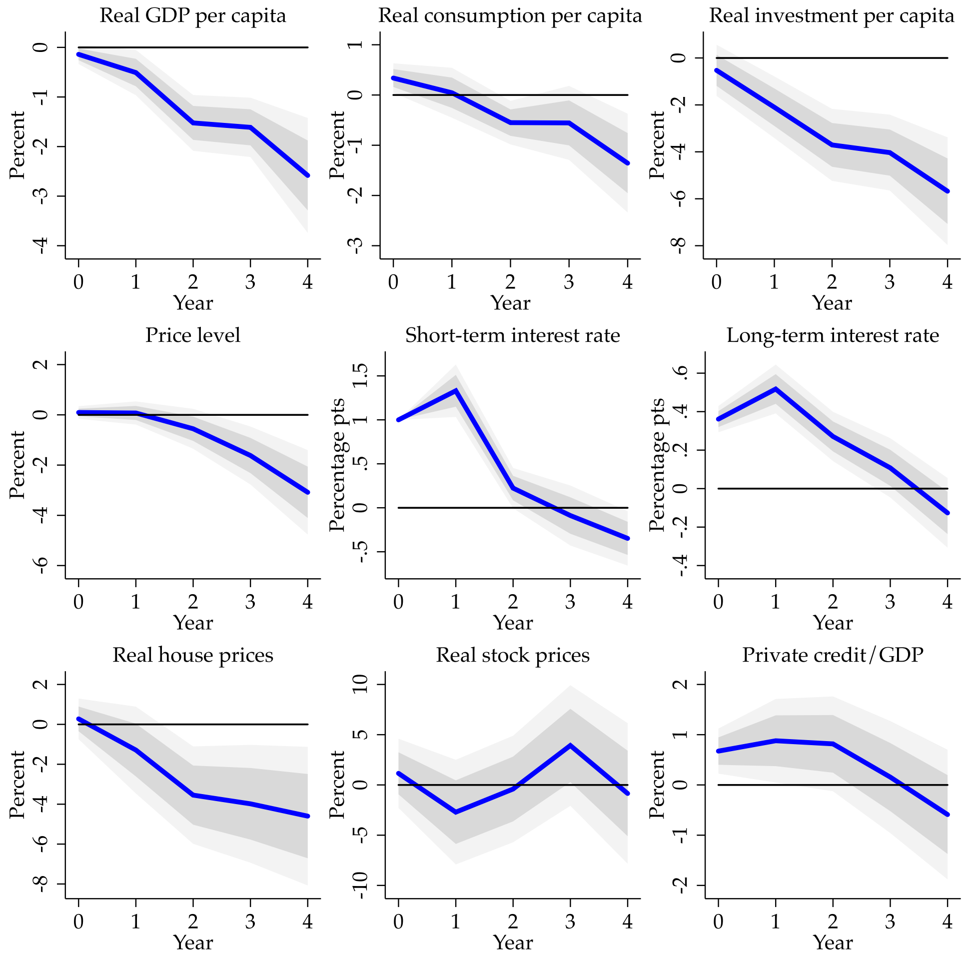

Fig. 2. Full baseline results. Full sample.
Notes: Full sample: 1870–2006 excluding WW1: 1914–1919 and WW2: 1939–1947. LP-IV estimates displayed with a solid blue line and 1 S.D. and 90% confidence bands. See text. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Interestingly, the response of prices is amplified. The reason is easy to see in Table 6. The LP-OLS estimate of the effect of domestic interest rates on prices is positive rather than negative. Meanwhile, the effect of base country interest rates is strongly negative. Thus, the correction makes the LP-IV price response more sensitive to interest rates, especially on impact. This feature has often been an achilles heel of the post-WW2 U.S. VAR literature, where findings often have a price response with the wrong sign or a price response that remains largely muted for a prolonged period of time even as the response of output shows a more immediate response. These results are reassuring. Our LP-IV estimates are reasonably robust to potential spillover effects under economically meaningful scenarios. Even if the spillover effects were large, the true estimate would be still be much closer to the LP-IV estimates that we report, than to the LP-OLS estimates commonly found in the literature.

5. The causal effects of interest rates on the macroeconomy

In this section we briefly present a comprehensive study of the causal response of a wider array of macroeconomic outcomes to a short-term interest rate increase of +100 bps. Fig. 2 summarizes the responses for the full sample (the equivalent figure using the Post-WW2 sample only is virtually identical and provided in online Appendix C for completeness as Fig. C1).

Starting at the top left chart of Fig. 2, we see that a +100 bps increase in the short rate *causally* leads to a 2.5% decline in real GDP per capita (or about −0.5% per annum), a 1.5% decline in real consumption per capita, a 6% decline in real investment per capita, and a 3.5% decline in the price level (in the second row, first column), where all effects are relative to the no-change policy counterfactual and the measurements are cumulative over the horizon of 4 years.

Moving along the second row of charts in Fig. 2, we look first at the own response of short-term interest rates to a +100 bps rate rise in year 0 (row 2, column 2). This path reflects the intrinsic persistence of changes in interest rates. In this case, short-term interest rates increase by +125 bps in year 1, drop back to +50 bps in year 2, and then decline to effectively zero by year 3. The next chart (row 2, column 3) shows the response of long-term interest rates, which are, as is well-known, more subdued in amplitude than short rates; the long-term interest rate moves about half as much. A +100 bps rise in the short rate causally leads to the long rate rising +40 bps in year 1, rising to nearly +60 bps in year 2, and then falling back towards zero by year 3 as well.

Proceeding to the last row of charts in Fig. 2 (columns 1 and 2), we examine the responses of two key asset prices: a +100 bps rise in the short rate causally leads to a cumulative 5% decline in real house prices and a cumulative 1.5% decline in real stock prices after the first year, again as compared to the no-change policy counterfactual. Although these real responses may appear quite small, bear in mind that the nominal responses are far larger in the negative direction, as reported in Fig. C2 in online Appendix C. That is, nominal asset prices drop strongly and quickly in response to an interest rate hike. Overall, these asset price responses are consistent with a significant wealth-effect channel for monetary policy, working alongside the more often noted income-effect channel visible in the path of real GDP.

Finally, in the last row and column of Fig. 2, we display the causal response of aggregate credit (bank lending to the nonfinancial sector relative to GDP). This chart shows that a +100 bps rise in the short rate has a relatively small effect on the ratio of bank loans to GDP cumulated over four years, although this effect is in the end consistent with models where contractionary monetary policy leads to less demand and/or supply of credit. If anything, the effect is slightly more pronounced when using the Post-WW2 sample although the responses are very similar. Bauer and Granziera (2017) have reported similar patterns based on post-1970 OECD data. Loan contracts are difficult to undo in the short-run relative to the decline of GDP. Therefore, although nominal private credit declines on impact (not shown), the private credit to GDP ratio may take a bit longer to decline.

The takeaway from these impulse responses is clear. An exogenous shock to interest rates has sizable effects on real variables (somewhat larger than those measured using conventional VARs), but along the lines predicted by most monetary models with rigidities or frictions. Term structure responses conform very well with standard results in the literature. Nominal variables decline strongly. Perhaps the only variable that appears to be somewhat unresponsive to interest rates is the credit to GDP variable. Loans decline in response to a shock to interest rates, but in the short run their rate of decline matches closely the rate of decline in real economic activity.[11]

6. State dependence

In this section we address the possible state dependence of the impulse-response functions by stratifying our analysis. We use indicators for whether the economy is in a boom or slump, in low or high inflation, or in a credit surge or slowdown, to address big issues from the research literature and current policy debates. Results are present in figures showing estimated local projections by state. Implied path multipliers and inference across states is discussed in Appendix D.

6.1. State dependence in boom and slump episodes

In the first set of nonlinear experiments shown in panel A of Fig. 3, we set the state variable equal to 1 in booms and 0 in slumps. Booms (slumps) are years when log real GDP per capita is above (respectively, below) its long-run country-specific trend component, as measured by an HP filtered series with a very low-pass setting ($\lambda_{HP} = 100$, annual). The boom/slump stratification delivers a state-dependent analysis that echoes the analysis in Auerbach and Gorodnichenko (2013) and Jordà and Taylor (2016) for fiscal policy

These results are strongly indicative of an asymmetric macroeconomic response to interest rates. The experiment is always normalized to be a +100 bps increase in the short-term rate. This is done to facilitate impulse-response comparisons across states even though, of course, tight policy in a slump is unlikely. The response of real GDP per capita to monetary policy appears to be quite strong in booms (about −2.5% by year 4, full sample), but considerably weaker in slumps (less than −0.5% by year 4, full sample).[12] Evidence of asymmetry is less clear for the inflation response, however. Differences using the full sample are very minor and not very different from the average response. Formal tests reported in online Appendix Table D1.

To sum up, a central bank rate tightening of +100 bps in a boom would have causal effects on output that are strongly contractionary on output going forward. In contrast, a central bank rate loosening of −100 bps in a slump would have causal effects on output that are, proportionately, only weakly expansionary. However, the effects on prices would be roughly symmetric across the two states. Thus, the “sacrifice ratios” for monetary policy are markedly different, and so are the incentives to pursue pre-emptive monetary policy, in one state versus the other.

[11] For further reference, Figs. C3 and C4 in online Appendix C report the full set of impulse responses out to year 10 after impact for the full and post-WW2 samples respectively. These figures are provided to investigate the neutrality of money in the long-run. We are able to confirm that neutrality cannot be rejected, although the time frame over which neutrality reasserts itself is perhaps quite a bit longer than one might presume. Investment, the more volatile of the components of output, is nearly back to its starting value by year 10 only.

[12] This difference of nearly 2% (closer to 1% Post-WW2) is broadly consistent with Angrist et al. (2018); Tenreyro and Thwaites (2016), and Barnichon and Matthes (2016).

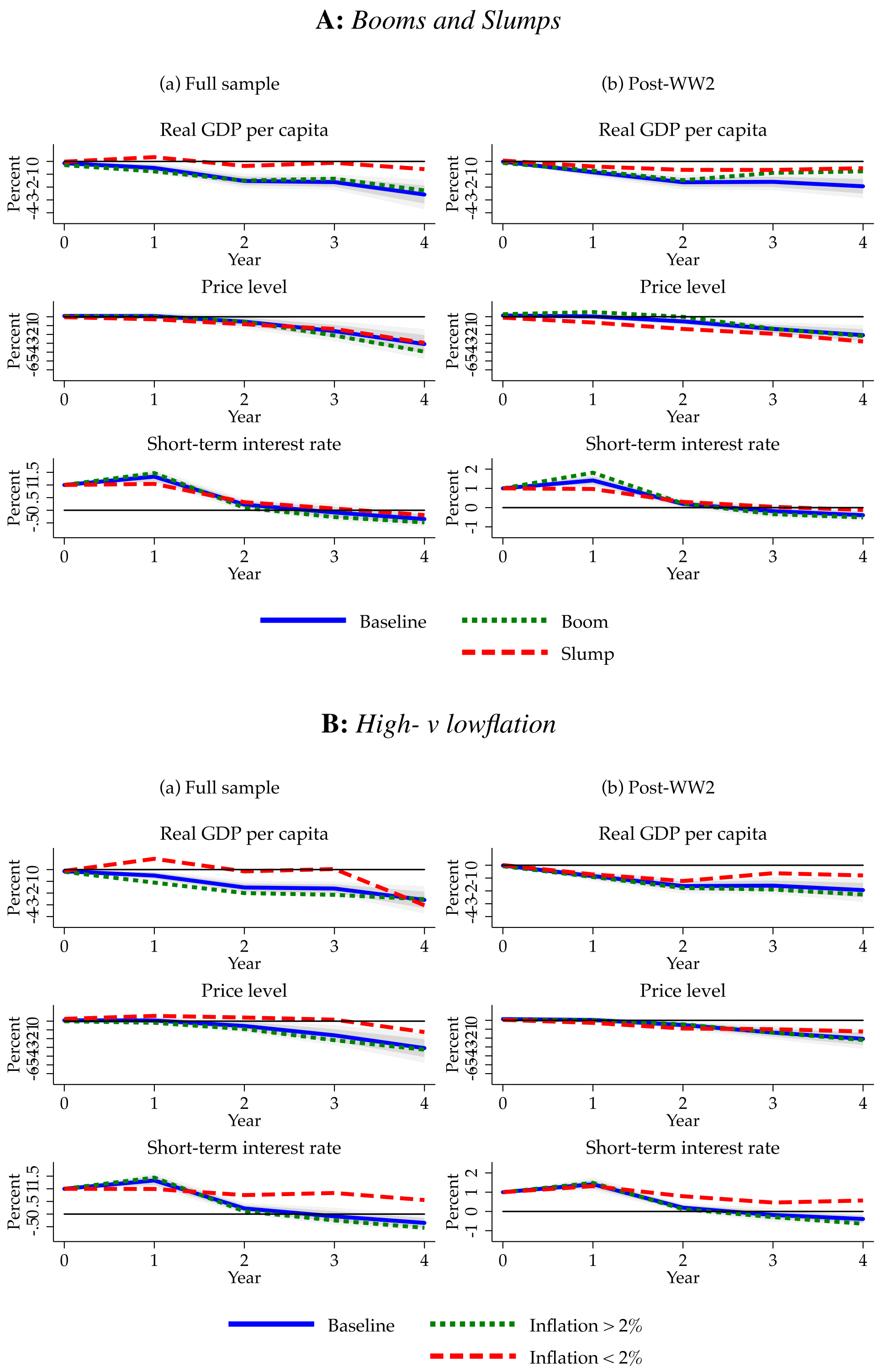

Fig. 3. State dependence: boom v slump and high- v. lowflation.
Notes: Full sample: 1870–2006 excluding world wars (1914–1919 and 1939–1947). Post-WW2 sample: 1948–2006. Linear LP-IV estimates displayed with a solid blue line and 1 S.D. and 90% confidence bands. Panel A stratified by the boom displayed with a green dotted line whereas estimates in the slumps are displayed with a red dashed line. See text. Panel B stratified by the lowflation regime displayed with a red dashed line whereas estimates when inflation is above 2% are displayed with a green dotted line. See text. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

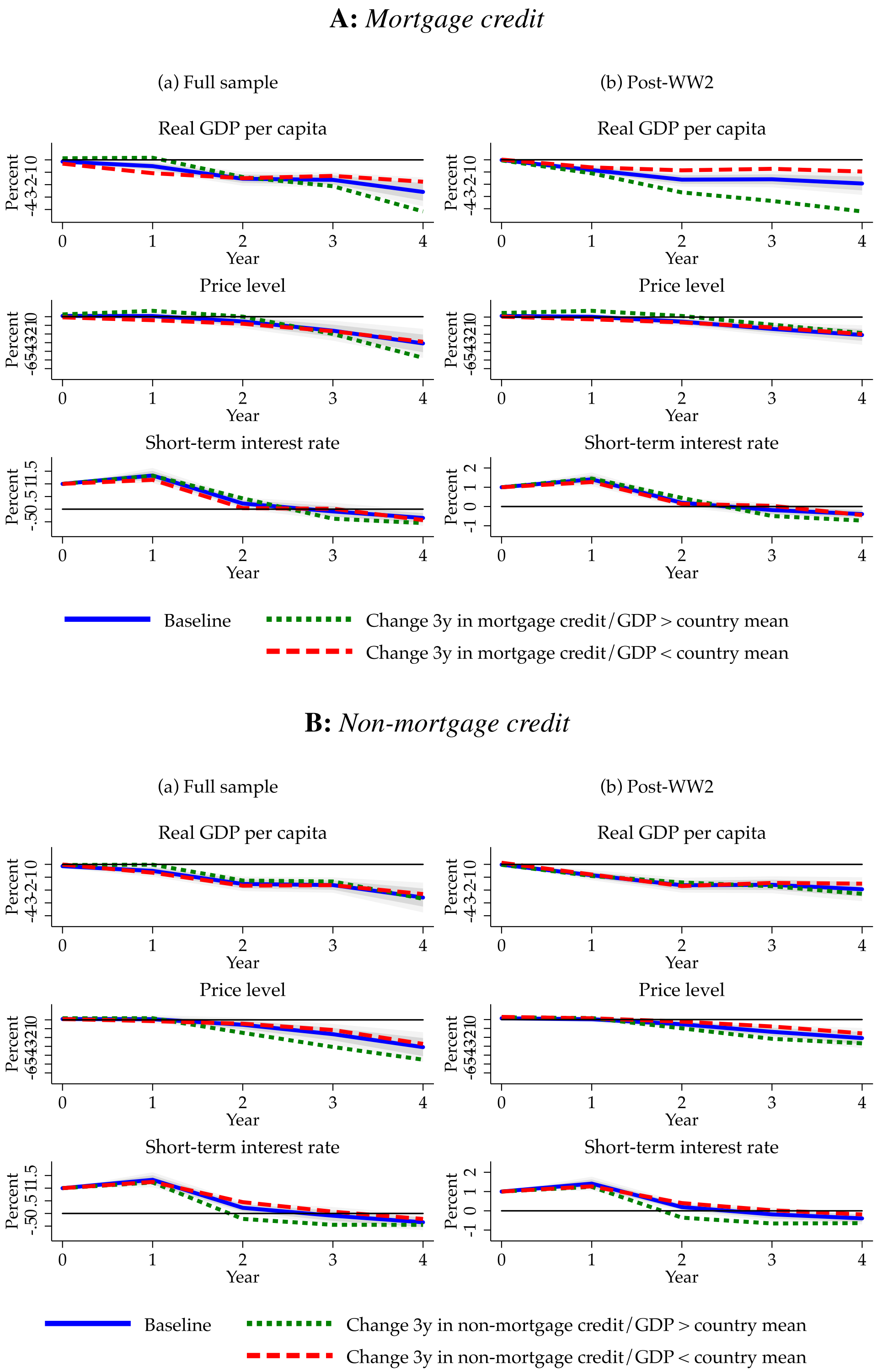

Fig. 4. State dependence: monetary policy and mortgage v. non-mortgage credit.

Notes: Full sample: 1870–2006 excluding world wars (1914–1919 and 1939–1947). Post-WW2 sample: 1948–2006. Linear LP-IV estimates displayed with a solid blue line and 1 S.D. and 90% confidence bands. For each type of credit, estimates stratified with below country-average (3y) credit over GDP growth regime displayed with a red dashed line, and above country-average (3y) credit over GDP growth regime displayed with a green dotted line. See text. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

6.2. State dependence in "lowflation" episodes

Our next set of nonlinear experiments, in panel B of Fig. 3, concerns the hypothesis that monetary policy may have different effects in times of low inflation, a topic of rising interest since the advanced economies entered an era of "lowflation" as the Great Recession wore on after 2008. To examine this nonlinearity we chose to set an indicator state variable equal to 1 when inflation is low (at or below 2%) and 0 otherwise.

Again we normalize the responses to a +100 bps rate increase, while recognizing rates are unlikely to go up when there is lowflation. The response of real GDP to monetary policy appears to be quite strong when inflation is above 2% (−3% by year 4). However, monetary policy loses most of its traction when the inflation rate dips below the 2% threshold and economies tip into a lowflation state, especially in the post-WW2 sample (see online Appendix Table D2 for formal tests).

6.3. State dependence in credit boom and bust episodes

The financial crisis and theories that try to explain its aftermath often build on what happens when a credit boom goes bust, as we discussed in the introduction (see, e.g., Schularick and Taylor 2012). Thus, the natural next step is to stratify the responses to monetary policy according to credit.

Our final set of experiments is presented in Fig. 4. The results presented in panel A refer to above/below country-specific 3-year lagged mean changes in mortgage credit over GDP, whereas panel B focuses instead on changes in non-mortgage credit over GDP.

In previous research (Jordà et al., 2016) we have found this distinction to matter and in the context of the theoretical literature, and the limitations of the data, it is the closest we can get to the evaluating household versus firm leverage. Historically, most mortgage credit refers to household rather than commercial real estate borrowing, a trend that has been increasing of late (e.g., the commercial share of mortgages in the U.S. has fallen in the past 40 years from about 40% to 25%). Non-mortgage credit on the other hand, is usually unsecured loans issued to firms rather than households. Hence one can interpret panel A as suggestive that household leverage affects the efficacy of monetary policy in a way that lending to firms does not, as shown in panel B.

In particular, panel A shows that in a period of rapid recent growth in mortgage credit, a 1% shock in interest rates can depress output by about 5 percentage points more than after a period of below average credit growth, 4 years after intervention. Meanwhile, prices respond much more to interest rates during periods of low recent growth in mortgage credit, relative to other times. In response to the same 1% interest rate shock discussed earlier, the decline in prices can be about 3 percentage points higher 4 years after intervention.

Meanwhile, panel B shows that there is essentially no difference in the response to monetary policy whether or not non-mortgage credit grows above or below average in the preceding 3 years. Thus, theories that stress heterogeneity coming from consumer leverage appear to find stronger support in our data than theories that rely on firm leverage instead. Furthermore, there seems to be little difference in results including or excluding pre-WW2 data. The effects that we report are fairly stable across eras despite considerable evolution in the financial sector.

7. Conclusion

The effectiveness of monetary stabilization policy is not only a major policy concern but also an important matter of ongoing controversy among both theoretical and empirical macroeconomists. This paper argues that interest rates can have a considerable effect on macroeconomic outcomes. The source of the attenuation bias that we report suggests that common assumptions, often based on regression control arguments (conditional mean independence), do not provide an adequate basis for identification. Using a quasi-natural experiment in international finance and novel empirical methods, we show why this bias occurs and how to resolve it.

Monetary stabilization policy turns out to be state-dependent, a critical observation in the context of the Great Recession and its aftermath. Policymakers have faced a situation of consistent undershoot relative to their stated objectives and forecasts. A slower than expected growth trajectory has been seen in the U.S. economy, and worse yet in the U.K., Europe, and Japan; and we have seen persistent sub-2%-target inflation afflicting all of these economies. Amid worries of deflation risk and secular stagnation, the extensive and unconventional application of monetary policy tools has failed to shunt the macroeconomic locomotive onto a faster track. Debate centers on why the central banks got so derailed, and the subsequent lack of policy traction.

Empirical evidence from the most comprehensive advanced economy macroeconomic dataset assembled for this study shows that these travails are, in a sense, nothing new. In conditions where output is weak, inflation is low, or credit is stagnant, the task of stimulating the economy out of its torpor is much more difficult. These challenging conditions have been seen in many western economies for almost a decade, but they were also present in the distant past with similar consequences. Our results therefore have profound implications for how today's monetary models are formulated and applied.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jmoneco.2019.01.021.

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