DISCUSSION PAPER SERIES

DP14797
(v. 4)

LEANING AGAINST THE WIND AND CRISIS RISK

Moritz Schularick, Lucas Ter Steege and Felix Ward

ECONOMIC HISTORY

FINANCIAL ECONOMICS

INTERNATIONAL MACROECONOMICS AND FINANCE

MONETARY ECONOMICS AND FLUCTUATIONS

CE PR

LEANING AGAINST THE WIND AND CRISIS RISK

Moritz Schularick, Lucas Ter Steege and Felix Ward

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- Economic History
- Financial Economics
- International Macroeconomics and Finance
- Monetary Economics and Fluctuations

Any opinions expressed here are those of the author(s) and not those of the Centre for Economic Policy Research. Research disseminated by CEPR may include views on policy, but the Centre itself takes no institutional policy positions.

The Centre for Economic Policy Research was established in 1983 as an educational charity, to promote independent analysis and public discussion of open economies and the relations among them. It is pluralist and non-partisan, bringing economic research to bear on the analysis of medium- and long-run policy questions.

These Discussion Papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character.

LEANING AGAINST THE WIND AND CRISIS RISK

Abstract

Can central banks defuse rising stability risks in financial booms by leaning against the wind with higher interest rates? This paper studies the state-dependent effects of monetary policy on financial crisis risk. Based on the near-universe of advanced economy financial cycles since the 19th century, we show that discretionary leaning against the wind policies during credit and asset price booms are more likely to trigger crises than prevent them.

Moritz Schularick - moritz.schularick@uni-bonn.de
University of Bonn, Macrofinance Lab and CEPR

Lucas Ter Steege - s6luters@uni-bonn.de
University of Bonn

Felix Ward - ward@ese.eur.nl
Erasmus University Rotterdam, Erasmus School of Economics

Leaning against the wind and crisis risk[*]

Moritz Schularick[†]   Lucas ter Steege [‡]   Felix Ward[§]

September 14, 2020

Abstract

Can central banks defuse rising stability risks in financial booms by leaning against the wind with higher interest rates? This paper studies the state-dependent effects of monetary policy on financial crisis risk. Based on the near-universe of advanced economy financial cycles since the 19th century, we show that discretionary leaning against the wind policies during credit and asset price booms are more likely to trigger crises than prevent them.

Keywords: financial stability, monetary policy, local projections.

JEL Codes: E44, E50, G01, G15, N10

How should a central bank react when it observes that a potentially dangerous credit and asset price boom is under way? Can policymakers defuse rising financial stability risks by leaning against the wind and increasing interest rates?

Two prominent historical episodes delineate the issue that our paper speaks to. Consider the U.S. economy in 1928. Concerned about booming stock prices, a frenzy in commercial real estate markets, and substantial lending against both, the Federal Reserve increased policy rates from 3.5% to 6% between January 1928 and August 1929, surprising market participants. Most economic historians today think that these policy decisions, instead of bringing financial markets and credit growth back to more sustainable levels, played an important role in triggering the Great Depression (Eichengreen, 1992; Bernanke, 2002). Could the economy have avoided the financial crash had policy makers not raised interest rates to discourage what they perceived as rampant speculation in the stock market? Fast-forward 75 years. In the 2000s, U.S. policymakers decided to not lean against booming credit and housing markets. Instead, they stuck to a policy that was, by and large, consistent with flexible inflation targeting without taking financial stability considerations on board (Bernanke, 2010). Financial imbalances continued to grow and erupted in the 2008 global financial crisis. What would have happened had the Federal Reserve raised interest rates to lean against the credit boom? Could the crash and the Great Recession have been avoided?

The two biggest financial crises in the past 100 years come with conflicting messages regarding the effectiveness of leaning against the wind (LAW) policies in safeguarding financial stability. Notwithstanding, current debates on the financial stability mandate of central banks often invoke one or the other episode to argue for or against leaning against the wind. Yet, which historical lesson is actually representative? The issue looms large for current thinking about monetary policy (Stein, 2013; Svensson, 2017; Adrian and Liang, 2018). It has become exceedingly clear how large the economic costs of financial crises are (Cerra and Saxena, 2008; Jordà, Schularick and Taylor, 2013). Moreover, recent research suggests that such financial boom states are detectable in real-time using quantity and price indicators (Richter, Schularick and Wachtel, forthcoming) so that policymakers have the chance to intervene. Yet, what do we know about the effects of monetary policy changes on financial stability during financial booms? The answer so far is not much, other than inconclusive anecdotal evidence.

This paper aims to close this gap. We systematically study the available evidence for the state-dependent effects of monetary policy on financial stability. The state we condition on is a financial boom, defined as a large and sustained deviation of credit growth and real asset prices from trend. Conditional on being in such an (observable) boom state, we estimate how a monetary policy shock affects financial crisis probability and severity. We do so based on the near-universe of advanced economy financial cycles

and crises since the 19th century.

Note that the question we are interested in is not how systematic LAW policy rules (S-LAW) affect financial stability and the macroeconomy. Our focus is squarely on discretionary LAW policy actions (D-LAW) that take place in a financial boom state, and our identification strategy speaks to those cases only. Our empirical analysis is based on a local projection instrumental variable (LP-IV) strategy that has recently been introduced by Jordà, Schularick and Taylor (2019). The IV exploits a type of monetary policy variation that is not itself influenced by local economic conditions, namely, policy rate changes in small open economies with fixed exchange rates that are induced by the base economy.

For instance, in the early 1990s, Sweden witnessed a credit and house price boom. When the German Bundesbank surprised markets in December 1991 and raised its Lombard rate to 9.75% in response to inflationary pressures following German reunification, under the prevailing fixed exchange rate regime, it forced the hand of the Swedish central bank too. At the time, the *New York Times (1991)* quoted a market economist: "This is the Bundesbank's way of showing they will use their power and independence without regard to the economic conditions in the rest of Europe." The Riksbank had to defend the exchange rate of the Swedish Krona vis-à-vis the German Mark. Following the Bundesbank, the Riksbank also increased its policy rate at a time when credit and housing markets in Sweden were booming. This episode provides us with a quasi-experiment for an exogenous change in monetary conditions at a time when credit and housing markets in Sweden were in a financial boom.[1]

We bring this identification strategy to bear on a long-run dataset that spans 150 years and covers most advanced economies, including dates of systemic financial crises. The dataset contains 1,525 country-year observations of countries whose currency is pegged to a base country's currency. Among those, we observe more than 170 credit boom episodes, of which 98 coincide with exogenous increases in base country policy rates. This rich dataset and the IV identification strategy allow us, for the first time, to zoom in on the causal effects of D-LAW policy—*increases* in policy rates *during booms* in credit and asset prices.

Our results are unambiguous in the sense that the estimates suggest that the effect of D-LAW policy on crisis risk has the opposite sign from what is often assumed. We show that a 1 percentage point (ppt) policy rate change during a financial boom *increases* the risk of a financial crisis by about 10 ppts over a one-year horizon. Crisis risk remains elevated for about two years after the monetary shock before subsiding to its long-run average level. However, at no point in the five years following the policy rate increase do

---

[1]For this reason, the identification strategy does not speak directly to the two introductory examples of monetary policy (in-)action during financial booms in the U.S. The U.S. was a base country, not a pegged economy, in both instances.

we find evidence for a reduction in crisis risk. The empirical evidence thus lends support to some of the worst fears about D-LAW policy—that it is more likely to trigger crises than prevent them (Bernanke and Gertler, 2000; Bernanke, 2002).

Although D-LAW heightens crisis risk in the near term, it could still be beneficial if it limits the economic costs of the crisis. We compare real GDP losses across financial crises that were preceded by different degrees of D-LAW prior to the start of the crisis, instrumenting the central bank's pre-crisis monetary policy stance. Our findings suggest that D-LAW policy does not systematically reduce crisis severity. In the five years after a financial crisis, real GDP falls by around 8% below trend, regardless of whether pre-crisis monetary policy was taking a leaning stance.

We corroborate these findings through a series of robustness checks. In particular, we examine alternative financial boom definitions, threats to the exclusion restriction, alternative financial crisis definitions, and differences between D-LAW interventions that take place early on versus late during financial booms. Throughout, the crisis trigger effect of D-LAW policy emerges as a robust feature of the data, whereas evidence for the crisis severity reduction effect remains elusive.

The empirical evidence brought together in this paper substantiates concerns that have been voiced by the opponents of leaning policies (Bernanke and Gertler, 2001; Gilchrist and Leahy, 2002; Svensson, 2017): contractionary monetary policy at best appears ineffective at addressing financial instability risks and at worst appears outright harmful (Bernanke and Gertler, 2000; Bernanke, 2002). Most existing studies of D-LAW policy focus on how monetary policy affects financial crisis risk and severity through its effect on credit growth (Bauer and Granziera, 2017; Svensson, 2017). This "credit-only" approach suggests that D-LAW policy decreases crisis risk and ameliorates crisis severity to the extent that it reins in pre-crisis credit growth. However, it is plausible that monetary policy affects financial stability also through other channels, e.g., through its effect on debt servicing costs, asset prices, expectations, and incomes (Korinek and Simsek, 2016; Alpanda and Zubairy, 2017). Our paper provides a direct causal estimate of the effects of D-LAW on financial stability that is agnostic with respect to the channels at work. The findings we obtain on the basis of this approach contradict the notion that D-LAW can lower crisis risk, notwithstanding its effect on credit growth.[2]

Our findings represent a stronger indictment of D-LAW policy than is implied by the canonical cost-benefit analysis of Svensson (2017). In Svensson's work, the main cost of D-LAW comes from the adverse impact of a discretionary monetary tightening on unemployment, while its benefit comes from a lower crisis probability and a reduced crisis severity. By contrast, the results in our paper indicate that D-LAW policy neither lowers

---

[2]Note that the "credit-only" approach has also found its way into analyses of S-LAW policy, through semi-structural models that make crisis probability a function of credit growth only (Gerdrup et al., 2017; Gourio, Kashyap and Sim, 2018; Ajello et al., 2019).

crisis probability, nor reduces crisis severity.

Theoretical studies have focused on monetary policy rules that incorporate S-LAW elements (Woodford, 2012; Filardo and Rungcharoenkitkul, 2016; Gourio, Kashyap and Sim, 2018). Such rules require the central bank to react to financial booms in a rule-based way. Currently, most central banks do not follow an explicit S-LAW policy rule. Any policy change in that direction would thus initially resemble a discretionary policy change until the commitment to the new policy regime has been credibly established (Svensson, 2016).[3] So while our paper speaks to the effects of state-dependent discretionary changes in monetary policy and not to the effects of S-LAW, it can also inform the debates about the design and transition to S-LAW policies.

The remainder of this paper is structured as follows. Section I introduces the data, and section II describes our empirical strategy. The results are presented in section III. Section IV concludes.

I. Data

Our main data source is the JST Macrohistory Database (Jordà, Schularick and Taylor, 2017, http://www.macrohistory.net/data/). It provides annual data on the real economy and the financial sector for 17 developed countries since 1870. The countries included in the sample are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

To analyze how monetary policy affects financial crisis risk, we use the systemic financial crisis dummy defined by Schularick and Taylor (2012). This binary indicator is a narrative crisis measure that takes the value 1 in years in which a country experienced bank runs, bank defaults, forced mergers, or major public interventions in the financial sector. As a robustness check, we also consider the banking crisis dummies defined by Reinhart and Rogoff (2011) and Baron, Verner and Xiong (2018) (see online Appendix B). Regardless of the crisis indicator chosen, we obtain very similar results.

Our main explanatory variable of interest is the stance of monetary policy, which we measure as the change in nominal short-term interest rates. The short-term risk-free nominal rates most consistently available across our long-run sample are the yields on 3-month government securities and money market interest rates. Other variables that enter our analysis also come from the JST Macrohistory Database.

---

[3]The relative infrequency of financial booms raises further questions about the extent to which central banks are able to credibly commit to a S-LAW policy rule, as well as the private sector's ability to systematically incorporate such a rule in its decision making.

I.A.   Exchange rate regime, base countries, and capital controls

To construct the Trilemma IV for nominal short-term interest rate changes, we combine data on a country's exchange rate regime and capital account openness with data on interest rate changes in important base countries (see section II). Our long-run exchange rate regime indicator comes from Jordà, Schularick and Taylor (2019), relying on the work of Ilzetzki, Reinhart and Rogoff (2019). We use a binary variable that classifies country-year observations as a peg (=1) if the exchange rate is fixed, or a float (=0) otherwise.

Besides knowing whether a country entertains a fixed or floating exchange rate regime, our empirical strategy requires that we define a base country with respect to which the exchange rate is fixed. This base country's interest rate—the base rate—is the primary source of variation in the Trilemma IV. In the definition of base countries, we follow Jordà, Schularick and Taylor (2019). The U.K. is the base country prior to 1914. After 1945, the base is generally the U.S., with the exception of the ERM/EMS/Eurozone countries, for which Germany is treated as the base country. In the interwar years, we define a "gold rate," which is an average of U.K., U.S., and French short-term rates. Of the three countries, only those on gold are included in the average in any given year (see Obstfeld and Taylor, 2004).[4] To capture the degree to which local interest rates are insulated from base country rates through capital controls, we make use of the capital mobility indicator by Quinn, Schindler and Toyoda (2011). Their index ranges from 0 to 100, with 0 indicating a low degree of capital mobility and 100 a high degree. We rescale this indicator to the 0-1 interval.

I.B.   Financial boom indicators

To analyze the effect of D-LAW policy in the context in which it is usually considered (i.e. periods of rapidly expanding credit), we construct a binary indicator for credit booms. This boom indicator, $B_{i,t}$, takes a value of 1 when log real credit, $y_{i,t}$, is above its trend level, $\bar{y}_{i,t}$, and growing:

$$(1) \qquad\qquad B_{i,t} = I(y_{i,t} > \bar{y}_{i,t} \wedge \Delta y_{i,t} > 0).$$

To obtain the cyclical component, we use a one-sided HP-filter with a smoothing parameter, λ, equal to 100 (Hodrick and Prescott, 1997). As a robustness check, we also consider the Christiano-Fitzgerald bandpass filter (Christiano and Fitzgerald, 2003), isolating fluc-

---

[4]Our measure for the nominal short-term interest rate in the base country is the money market rate in the case of France and Germany, and the Treasury bill yield in the case of the U.K. and U.S. after 1918 and 1930, respectively. Before that, our indicator for the short-term nominal rate in the U.K. is the yield on bills of exchange, and in the U.S. it is the yield on commercial paper.

tuations in the 2- to 16-year period range, as well as the novel non-parametric filtering method that has recently been proposed by Hamilton (2018).[5] Results based on these alternative filtering methods are very similar to the baseline results we report in the main text (see online Appendix A). In all cases, the detrending is conducted in a one-sided fashion, so the results are relevant to policymakers who have to evaluate whether the economy is in a boom state or not in real time.

We also consider combined booms in credit and house prices, as well as in credit and stock prices. These two combined boom indicators take a value of 1 when credit and the respective asset price both fulfill condition 1. Finally, as a robustness check, we also partition boom episodes into early and late boom stages, in order to evaluate the claim that early D-LAW interventions are more effective at diffusing crisis risk. For this, the early boom stage is defined variably as either the first half of a boom episode, the first two years of a boom episode, or only the very first year of a boom episode.

Figure 1 looks at six historical time frames that are commonly associated with financial market booms. The solid blue lines depict the log of the financial variable of interest. The solid and hollow circles highlight years that the above-described procedure isolates as asset price booms and credit booms, respectively. Consistent with more general appraisals of financial market conditions, the 1880s in Australia, the 1980s in Sweden, and the 2000s in Spain are all identified as house price booms. The 1920s stock price boom in the Netherlands, the 1980s boom in Japan, as well as the 1990s dot-com boom in Italy are similarly well captured. In most years, these asset price booms were also underpinned by booms in credit.

Our sample contains a total of 255 credit booms. Of these, 142 coincided with booms in house prices and 168 with booms in stock prices. Importantly for our identification strategy, our sample contains 171 credit boom episodes in countries with a fixed exchange rate. The respective numbers for the joint credit+house price, and credit+stock price booms are 100 and 113. During 98 of the pegs' credit boom episodes, the pegged country was exposed to a policy rate hike in the base country and thus participated in the quasi-experiment. The respective numbers for the joint credit+house price, and credit+stock price booms are 67 and 75.

---

[5]Recent simulation evidence presented by Hodrick (2020) indicates that while the Hamilton filter outperforms the HP and Baxter-King filters in classical times series environments with constant parameters (random walks and ARIMAs) it performs worse in more complex environments that are characterized by changes in trends and parameters. The credit, stock price, and house price data we detrend are characterized by abrupt changes in growth rates that can persist for several decades. These changes in time series behavior have been linked to structural changes in the underlying economies that are more naturally accounted for by time series models that allow for trend changes and time-varying parameters. Our baseline results are thus based on the HP-filtered series.

Figure 1: Asset prices and boom periods
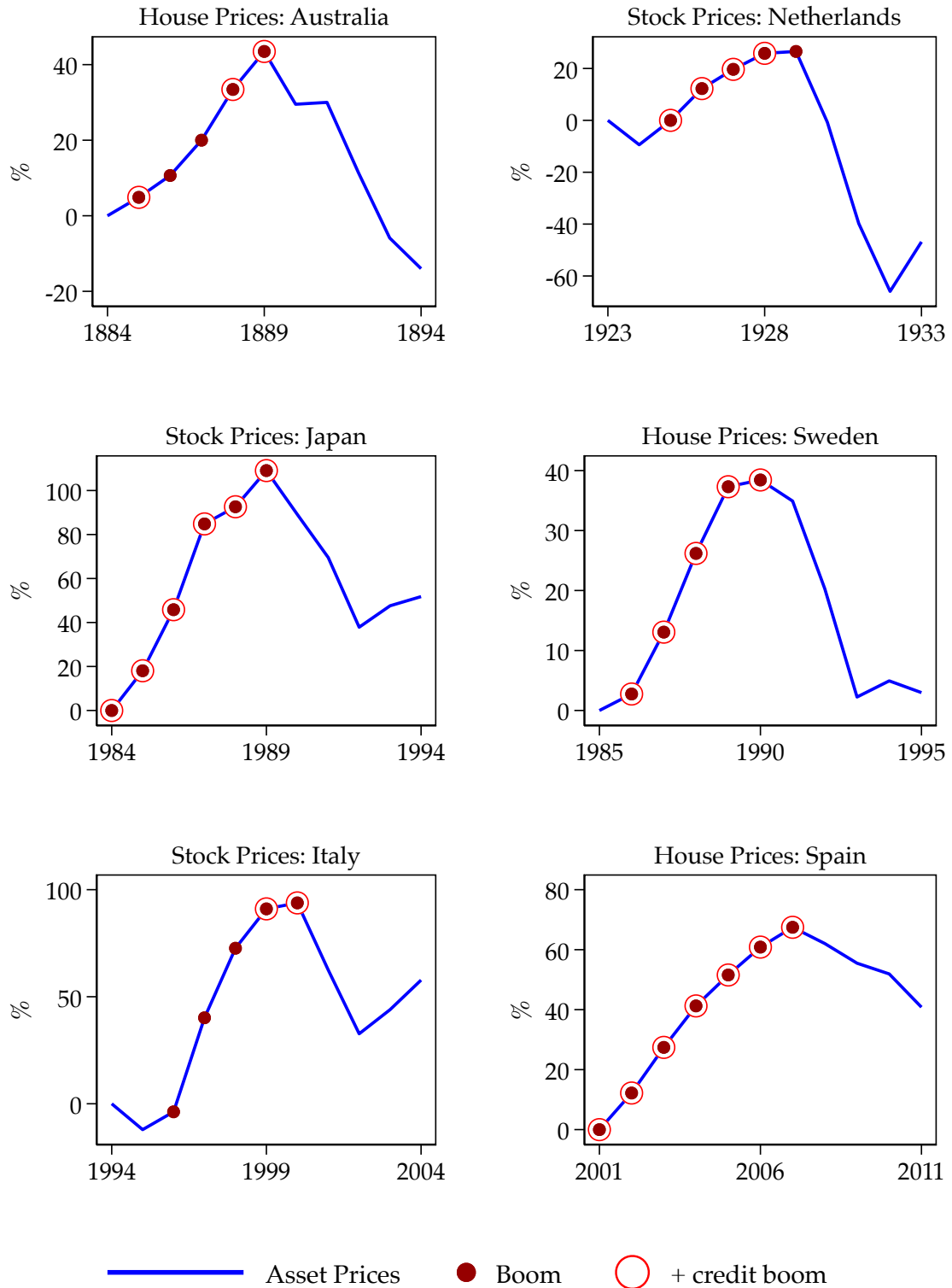
Notes: The figure shows log real asset price deviations from their level at the beginning of the time frame. Solid circles indicate asset price booms, and hollow circles indicate accompanying credit booms. The boom state is defined as the conjunction of growth and an above trend level (equation 1). Trend levels are based on a one-sided HP filter with smoothing parameter equal to 100.

II. Empirical strategy

To identify the effects of monetary policy on crisis risk, we apply the Trilemma instrumental variable (IV) strategy pioneered by Jordà, Schularick and Taylor (2019). The reasoning behind the Trilemma IV strategy is as follows. When a country pegs its exchange rate to a base country's currency, the local interest rate from then on has to match that of the base country. To see why, consider the peg country setting its interest rate below that of the base country. This will lead to unsustainable capital outflows, as capital seeks to obtain the highest return. Vice versa, if the peg sets its interest rate too high, this results in unsustainable capital inflows. Hence, under perfect capital mobility, peg country interest rates have to move in sync with base country interest rates. Furthermore, because base country interest rate changes are determined only by base country economic conditions, their variation is exogenous to the economic conditions in the peg countries.

We also subtract predicted base country interest rate changes, $\Delta \hat{r}_b$, from actual rate changes, Δr_b, to isolate unpredictable movements in base country interest rates. The Trilemma IV is constructed using this unpredictable component to prevent the IV from conveying information about base rate changes that could have been anticipated by the peg's households and firms. As in Jordà, Schularick and Taylor (2019), we use the first lags of the following base country variables to predict base country interest rate changes: the growth rates of GDP, consumption, investment, stock prices, and credit (all CPI deflated), as well as changes in nominal long-term interest rates, nominal short-term interest rates, the CPI inflation rate, and the current account-to-GDP ratio.

The Trilemma IV, z, for local policy rate changes, Δr, is thus defined as

$$(2) \qquad z_{i,t} \equiv \left(\Delta r_{b(i,t),t} - \Delta \hat{r}_{b(i,t),t} \right) \times PEG_{i,t} \times PEG_{i,t-1} \times KOPEN_{i,t}.$$

where i and t are the country and year indices, $b(i,t)$ denotes country i's base country in year t, and $PEG_{i,t}$ is the exchange rate regime dummy that indicates whether a country's exchange rate is fixed or floating with respect to the base b. The lagged dummy ensures that the instrument includes only well-established pegs that have lasted for at least two years, excluding the most fleeting and possibly incidental single-year pegs. $KOPEN_{i,t}$ is the rescaled financial openness indicator. Jordà, Schularick and Taylor (2019) show that the Trilemma IV, $z_{i,t}$, is closely aligned with changes in pegs' domestic short-term rates and is thus clearly relevant. In our sample, the instrument exhibits a highly significant slope coefficient of 0.59 over the full sample (SE = 0.10) and 0.73 for the post-World War II sample (SE = 0.13).

To trace the effect of a +1 ppt increase in policy rates on crisis risk, we estimate impulse response functions (IRFs) through local projections. More particularly, the sequence of fixed effects models we estimate represent a sequence of linear crisis probability models

through which we can assess how monetary policy affects crisis risk over a five-year horizon, $h = 0, ..., 5$:

$$(3) \qquad C_{i,t+h} = \alpha_{i,h} + \beta_h^{IV} \Delta r_{i,t} + \sum_{l=0}^{L} \mathbf{\Gamma}_{h,l} \mathbf{X}_{i,t-l} + \epsilon_{i,t+h},$$

where $\alpha_{i,h}$ denote country fixed effects, $\Delta r_{i,t}$ is instrumented by $z_{i,t}$, and $\mathbf{X}_{i,t}$ contains additional control variables.[6] The dependent variable is a dummy, $C_{i,t}$, that takes the value 1 if a financial crisis occurs in country i in year t or in any of the following two years, $t+1$, $t+2$. This definition reflects that while it is notoriously hard to predict the exact crisis year, it is possible to predict whether an economy enters a *danger zone* in which financial crises are more likely to occur (e.g., Kaminsky and Reinhart, 1999; Ward, 2017). The coeffcients $\{\beta_h^{IV}\}_{h=0}^{H}$ trace out the response of crisis risk to a $+1$ ppt increase in monetary policy rates. We translate the three-year crisis probability IRFs into annual crisis probability IRFs using $\widehat{P}^{\text{annual}} = 1 - (1 - \widehat{P}^{\text{three years}})^3$.

We include a rich set of control variables, $\mathbf{X}_{i,t}$. In particular, we include four lags of the following variables: per capita GDP growth, consumption growth, investment growth (all in real terms), CPI inflation, a measure of world GDP growth as in Jordà, Schularick and Taylor (2019), changes in short-term and long-term interest rates, growth rates of real stock prices, real house prices, real bank loans, the current account-to-GDP ratio, and the binary crisis dummy on which $C_{i,t}$ is based. Note that, except for the crisis dummy, we include the time t realizations of all control variables. Thus, we take a conservative stance with respect to the contemporaneous response of the dependent variable to monetary policy, effectively attributing as much as possible of that response to contemporaneous variation in the control variables and not the policy rate change. As a robustness check, we also apply the spillover correction proposed by Jordà, Schularick and Taylor (2019), which immunizes our results against potential violations of the exclusion restriction brought about by international goods and financial market spillovers (see online Appendix C).

III. RESULTS

Can contractionary monetary policy diffuse crisis risk? This section presents the answer provided by our empirical results. We begin by reporting the full sample results and then

---

[6]Linear probability models (LPM) yield very similar results to logit models in our application. However, the LPM allows us to saturate our specification with a rich set of control variables. In an LP-IV context this can help safeguard correct identification and improve estimator precision (Stock and Watson, 2018). Appendix C provides a detailed comparison of our LPM and logit results and discusses the reasons for their similarity.

narrow down to D-LAW policy as conventionally defined—as policy rate hikes against the backdrop of financial booms. In a second step, we investigate the effects of D-LAW on crisis severity.

## III.A.   The effect of D-LAW on crisis probability

The full sample results in the top left panel of Figure 2 suggest that interest rate hikes increase crisis risk in the near term. More precisely, a +1 ppt policy rate hike increases crisis risk by 2 ppts on impact, as well as in the following year. The size of this effect is substantial, given that average annual crisis risk in the full sample is 3.4%.

Next, we consider the effect of contractionary monetary policy for subsamples of financial booms. Can D-LAW policy rein in crisis risk against the backdrop of soaring credit aggregates and asset prices? The top right panel in Figure 2 shows our credit boom subsample results, which suggest that an interest rate hike during credit booms has a particularly adverse effect on crisis risk. Crisis risk increases by 4 ppts on impact, as well as in the year following the D-LAW policy. Taking into account that crisis risk is already elevated during credit booms, a +1 ppt interest rate increase raises annual crisis risk from 4.8% to around 10%.

The subsample results for combined booms in credit and asset prices point in the same direction. A discretionary +1 ppt increase in interest rates, aimed at reining in equity or house price booms, increases crisis risk by 6 to 8 ppts for up to two years. Given that average crisis risk is already 5.2% in the credit + house price boom subsample and 4.7% in the credit + stock price boom subsample, the D-LAW policy raises crisis risk above 10% in the short term.

These findings lend empirical substance to the concern that D-LAW policies might provoke financial crises rather than prevent them. We find little evidence to support the notion that D-LAW policy may pay off in the form of lower crisis risk in the medium term. The only significantly negative effect of D-LAW policy on crisis risk that we can document occurs in year 4 after the interest rate hike in the combined credit + stock price boom subsample.

Figure 2: Financial crisis risk responses
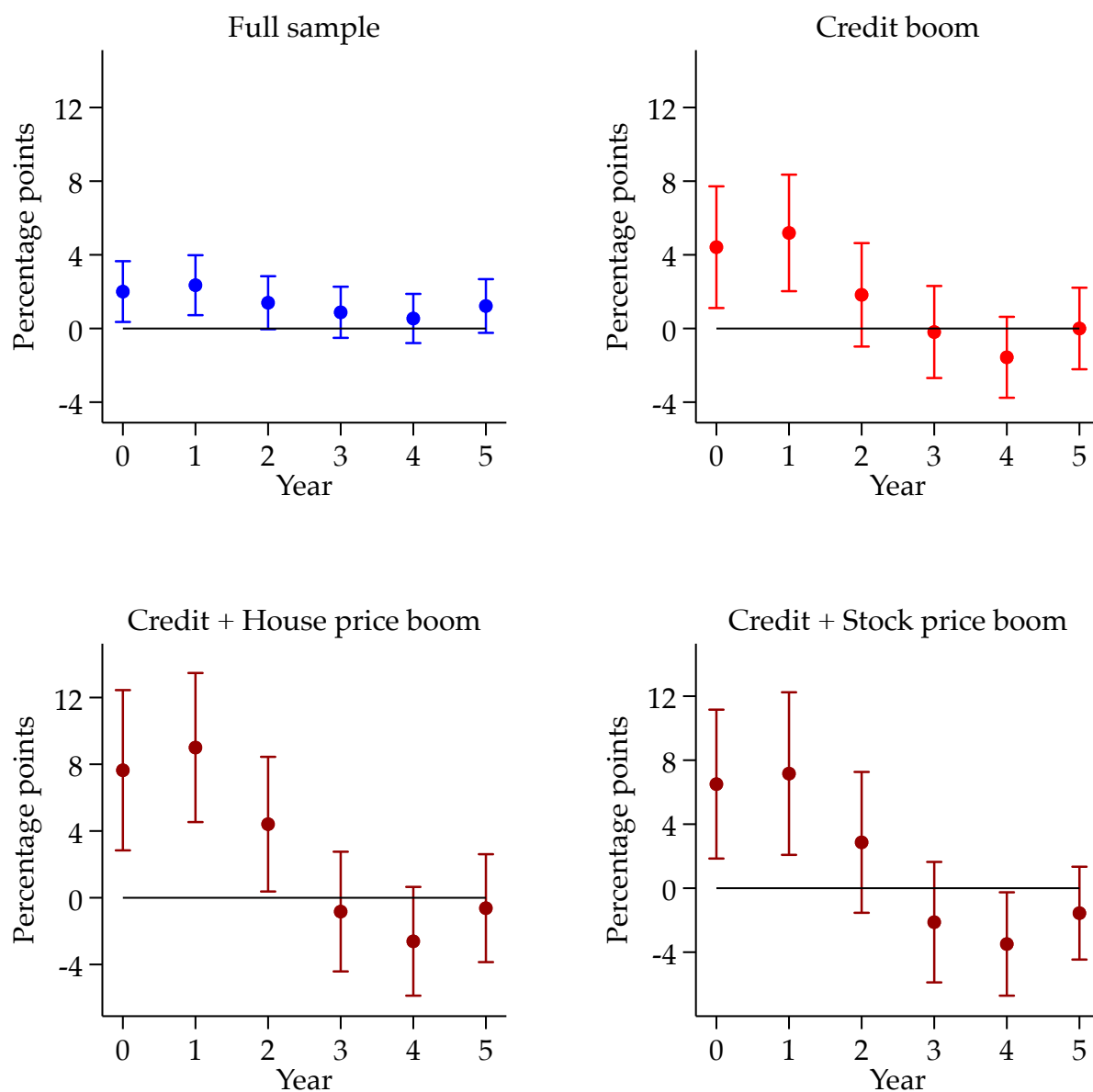
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100.

Policy rate hikes versus cuts

Recent research suggests that policy rate increases have stronger effects on the economy than policy rate decreases (Tenreyro and Thwaites, 2016; Angrist, Jordà and Kuersteiner, 2017). This finding is relevant for D-LAW policy, which is commonly defined

asymmetrically—as policy rate hikes during booms. Does crisis risk respond differently to policy rate hikes and cuts?

To answer this question, we augment our baseline specification (eq. 3) with an interaction term that separates positive changes in the instrumented policy rate from negative ones,

$$(4) \qquad C_{i,t+h} = \alpha_{i,h} + \beta_h^{IV} \Delta r_{i,t} + \gamma_h^{IV} \Delta r_{i,t} \cdot hike_{i,t} + \sum_{l=0}^{L} \mathbf{\Gamma}_{h,l} \mathbf{X}_{i,t-l} + \epsilon_{i,t+h},$$

where $hike_{i,t}$ is a dummy that takes the value 1 for policy rate hikes and 0 otherwise. This specification allows us to search for asymmetries in the response of crisis risk: $\{\beta_h^{IV} + \gamma_h^{IV}\}_{h=0}^{H}$ traces out the crisis risk response to policy rate increases, whereas $\{\beta_h^{IV}\}_{h=0}^{H}$ shows the same response for policy rate decreases.

Figure 3 shows the asymmetry results for the full sample, as well as the three financial boom subsamples. The immediate increase in crisis risk after a policy rate hike stands out, regardless of subsample. For the full sample, a +1 ppt rate hike increases short-term crisis risk by 3.6 ppts—almost two times the effect size of the symmetric specification. For the financial boom subsamples, a full percentage point rate hike increases annual crisis risk by 8 to 14 ppts. Evidence for medium-term crisis risk reduction again is scant. Only for the credit and the credit + house price boom subsamples do we find isolated coefficient estimates that are in line with a medium-term crisis risk reduction effect. The absolute size of these negative coefficients, however, is small compared to the initial crisis trigger effect.

Policy rate cuts tend to be followed by decreases in crisis risk. However, this crisis risk reduction effect tends to be less immediate than in the case of contractionary rate hikes. Only with a lag of one to two years does crisis risk decline significantly. For the full sample, the crisis risk reduction effect amounts to 2.7 ppts after two years. In the joint boom subsamples, crisis risk falls more substantially in a shorter amount of time.[7] A (pointwise) Wald test for equality of the rate hike and cut responses, however, indicates that the above-mentioned asymmetries in crisis risk responses are rarely statistically significant.[8]

The finding that policy rate hikes give rise to especially large increases in financial crisis risk strengthens the earlier contraindication result against D-LAW policy. This appears particularly pertinent against the backdrop of financial booms—precisely when D-LAW policy moves are usually considered.

---

[7]Additional results reported in online Appendix A confirm the robustness of these findings using the Christiano-Fitzgerald bandpass filter (Christiano and Fitzgerald, 2003), and the Hamilton filter (Hamilton, 2018) to define financial boom episodes.

[8]Only sporadically, in the credit boom and credit + stock price boom subsamples, does the Wald test reject equality of the rate hike and rate cut responses in the short run (90% confidence level).

Figure 3: Rate hikes versus rate cuts and crisis risk
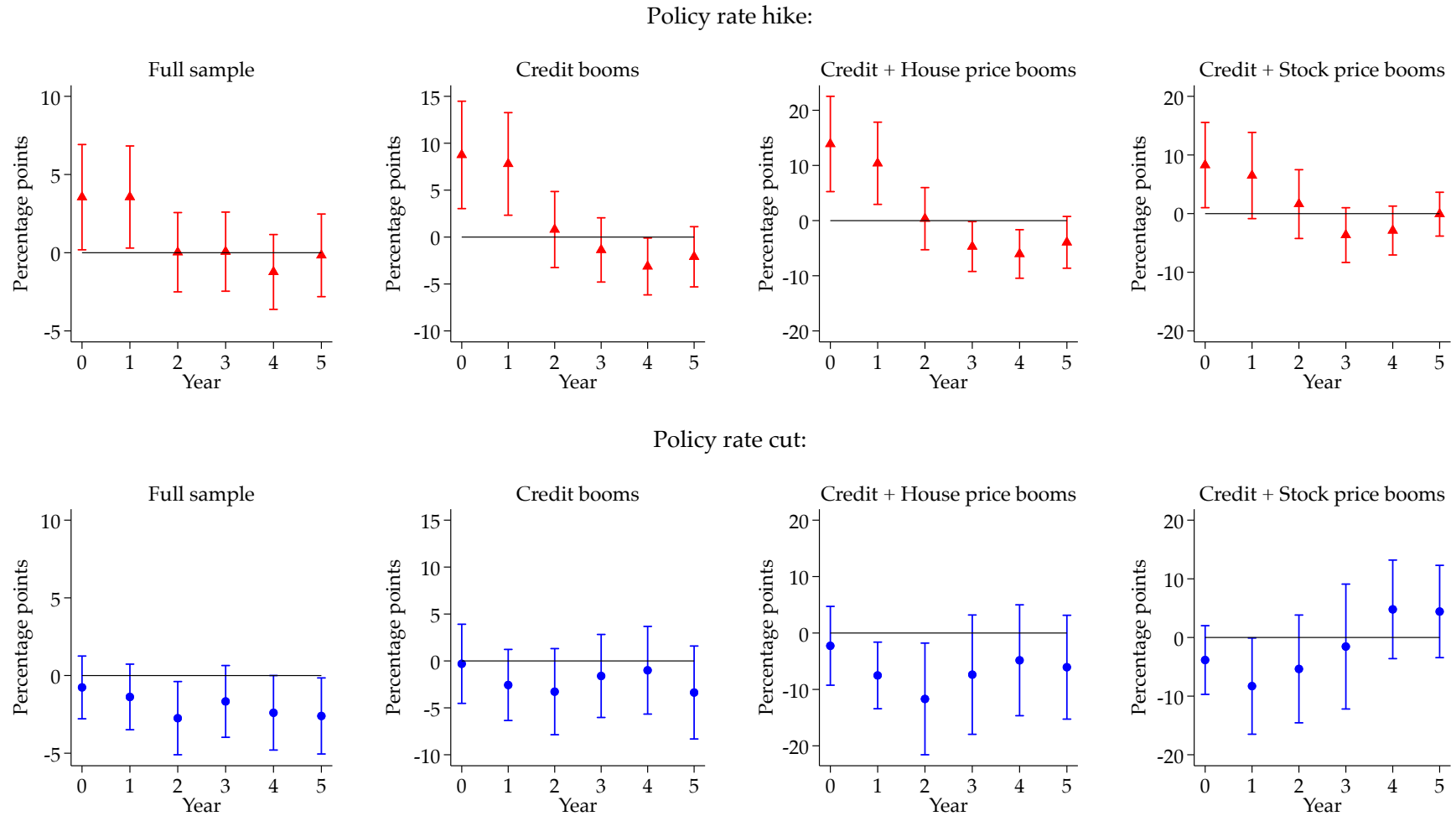
Notes: The figure shows the asymmetric annual crisis probability effects of a 1 ppt policy rate hike (top panels) and a 1 ppt policy rate cut (bottom panels). Solid bars depict pointwise 95% confidence intervals. The "Full sample" panels depict the non-state-dependent effects. The other panels depict the state-dependent effects during credit and asset price booms.

Early versus late interventions

Maybe rate hikes trigger financial crises only when they are administered too late in the boom. By contrast, the same rate hike might diffuse crisis risk when administered early on in the boom.[9]

To empirically test this idea, we extend our baseline specification by an interaction term that allows the effects of D-LAW policy to differ for early and late interventions:

$$(5) \qquad C_{i,t+h} = \alpha_{i,h} + \beta_h^{IV} \Delta r_{i,t} + \gamma_h^{IV} \Delta r_{i,t} \cdot early_{i,t} + \sum_{l=0}^{L} \mathbf{\Gamma}_{h,l} \mathbf{X}_{i,t-l} + \epsilon_{i,t+h},$$

where all terms are defined as before, the policy rate changes $\Delta r_{i,t}$ are again instrumented by the Trilemma IV, and $early_{i,t}$ is a dummy variable that takes the value 1 in the first year of a boom episode. We also considered other definitions of "early," such as the first two years of a boom or the first half of a boom. The results for these alternative partitions between early and late boom interventions are very similar to the baseline results reported here (see online Appendix C).

Figure 4 shows how crisis risk responds to a 1 ppt increase in policy rates, early and late during a financial boom. In no case do we find evidence for the notion that early interventions can lower financial crisis risk. For the credit boom subsample, the early and late intervention IRFs both exhibit a crisis trigger effect, though the mean estimate suggests that it is smaller for early interventions. A Wald test for equality of the late and early intervention IRFs, however, cannot reject the null hypothesis that both IRFs are equal.

For the credit + house price subsample early and late interventions have very similar effects throughout. Only for the credit + stock price subsample do we find evidence that early interventions are significantly less harmful than late interventions. Even in that case, however, rate hikes do not lower crisis risk—they just do not appear to trigger crises.

In sum, while early interventions may be somewhat less harmful than late interventions, they do not appear to systematically lower crisis risk. At best, early interventions leave crisis risk unaffected. At worst, early boom interventions appear to be just as potent in triggering financial crises as late boom interventions are.

---

[9]The limit cycle framework by Beaudry, Galizia and Portier (2015) allows for a formalization of this notion.

Figure 4: Early versus late interventions and crisis risk
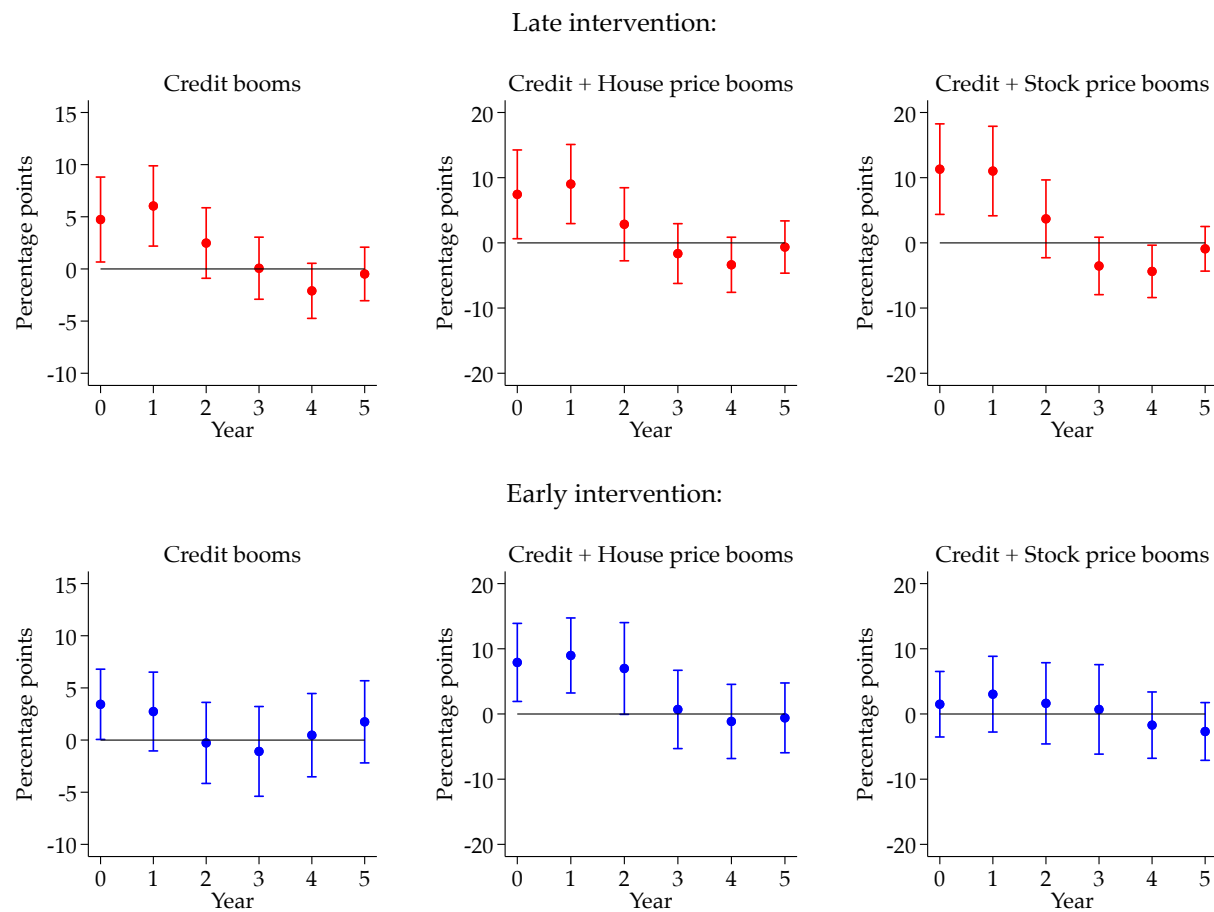
Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first year of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms.

III.B. The effect of D-LAW on crisis severity

In spite of the crisis trigger effect, D-LAW policy could still be beneficial if, by causing a small crisis now, it prevents a much bigger crisis later on. In other words, by hindering booms from proceeding unchecked, D-LAW policy might limit the fallout from the subsequent bust.

We investigate this hypothesis by looking at whether D-LAW reduces the real GDP loss associated with financial crises. To do this, we first characterize the degree of leaning over a one-year, three-year, and five-year horizon as the cumulative sum of policy rate changes over the same time period, $\Delta^K r_{i,t-1} \equiv \sum_{k=1}^{K} \Delta r_{i,t-k}, K = 1, 3, 5$. How leaning affects financial crisis severity is then estimated on the basis of the following local projections:

$$(6) \qquad \Delta^h y_{i,t+h} = \alpha_{i,h} + \beta_h C_{i,t} + \gamma_h^{IV} C_{i,t} * \Delta^K r_{i,t-1} + \sum_{l=0}^{L} \mathbf{\Gamma}_{h,l} \mathbf{X}_{i,t-l} + \epsilon_{i,t+h},$$

where $\Delta^h y_{i,t+h}$ denotes the cumulative h-year change in real GDP, $\Delta^K r_{i,t-1}$ denotes the central bank's leaning stance in the years leading up to the crisis, and all other terms are defined as before.[10] The $\Delta^K r_{i,t-1}$ are again instrumented by the equivalent expression based on the Trilemma IV, and local projections are estimated separately for each of the three leaning periods, $K = 1, 3, 5$. $\{\beta_h\}_{h=0}^{5}$ describes how the real GDP path after a financial crisis deviates from its usual path after non-crisis years, and $\{\gamma_h^{IV}\}_{h=0}^{5}$ reveals how the real GDP path after a financial crisis is affected by leaning. If a leaning policy systematically lowers crisis severity, this should be indicated by estimates of γ_h^{IV} that are larger than zero. As a consequence, the path traced out by $\{\beta_h + \gamma_h^{IV}\}_{h=0}^{5}$ should lie above the path described by $\{\beta_h\}_{h=0}^{5}$.

Figure 5 shows the results for the full sample and the three boom subsamples. Our findings do not lend support to the idea that a leaning policy lowers crisis severity. Real GDP falls by around 8% regardless of whether monetary policy took a leaning stance or not. Only in the joint credit + house price boom subsample does the mean estimate indicate that crisis severity might be lower following a one-year leaning period. Large standard errors, however, render this difference statistically insignificant. We confirm the finding that D-LAW policy cannot be relied upon to lessen crisis severity on the basis of the banking crisis dummies defined by Reinhart and Rogoff (2011) and Baron, Verner and Xiong (2018) (see online Appendix B).

---

[10]Note that the control vector $\mathbf{X}_{i,t}$ includes contemporaneous interest rate changes, which in contrast to the earlier specifications no longer receives separate mentioning here. $\mathbf{X}_{i,t}$ also contains the non-interacted leaning term $\Delta^K r_{i,t-1}$ to control for the effects of the central bank's policy stance in non-crisis years.

Figure 5: D-LAW and crisis severity
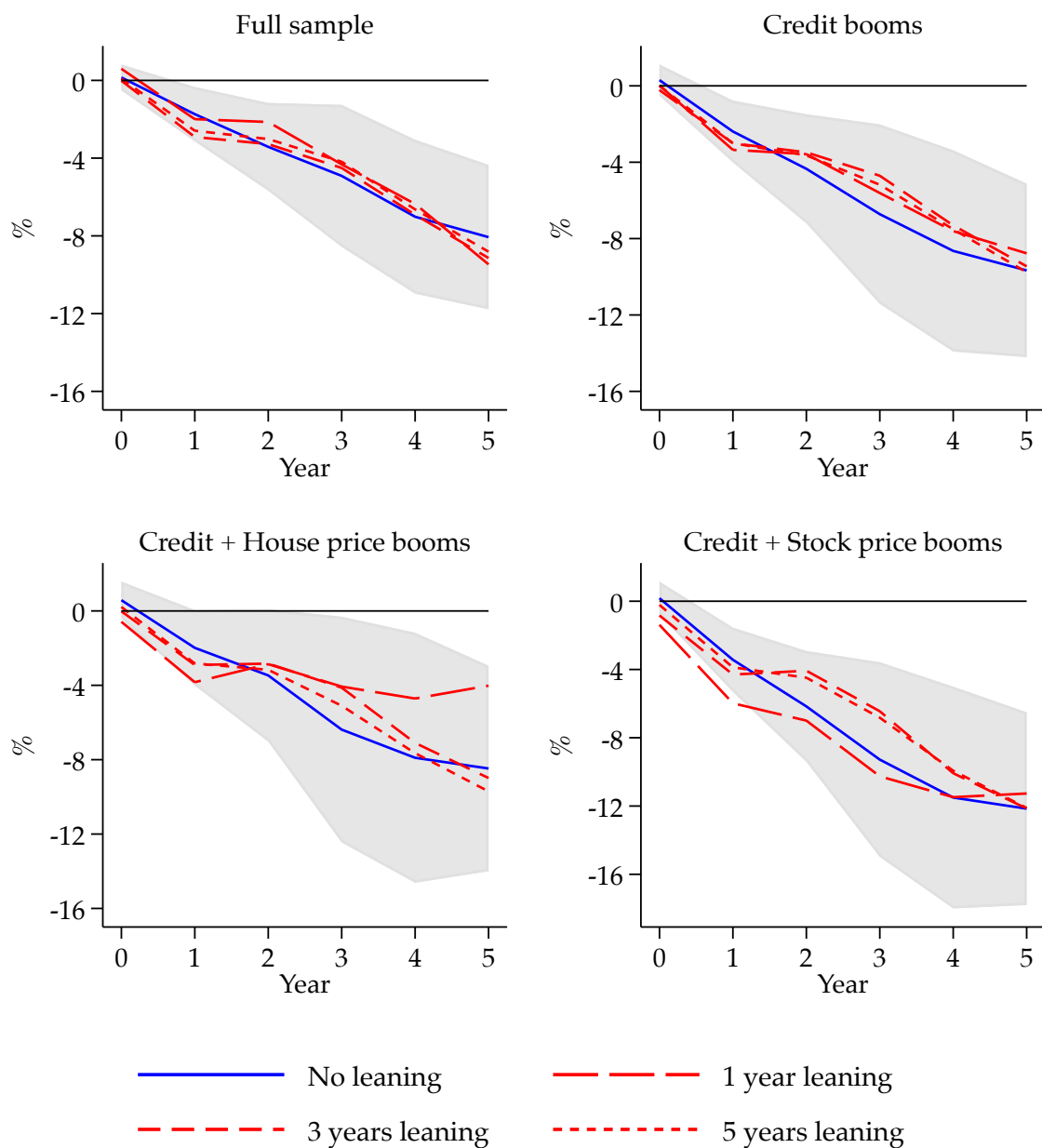
Notes: The figure shows how real GDP falls below trend following a financial crisis. Dashed lines indicate the same GDP loss conditional on monetary policy following a +1 ppt leaning stance before the crisis. The leaning stance is measured as the cumulative sum of policy rate changes over a one-year, three-year, and five-year pre-crisis horizon. Shaded areas depict pointwise 95% confidence bands.

IV. CONCLUSION

Whether conventional monetary policy should be applied to address financial stability risks is a long-standing question in macroeconomics. In this paper, we present the most

comprehensive empirical analysis of D-LAW episodes in modern economic history. Our findings lend support to the concern that contractionary monetary policy increases financial crisis risk rather than reducing it. A policy rate hike increases crisis risk in the short run, with little evidence that this short-term effect is compensated by either lower crisis risk in the medium-term or a reduction in crisis severity.

Our results add a new perspective to the current debate about whether macroprudential policy or monetary policy is better suited to address the buildup of financial fragilities. While monetary policy "gets into all the cracks" (Stein, 2013; Adrian and Liang, 2018), the empirical evidence points to severe side effects of D-LAW policies.

REFERENCES

Adrian, Tobias, and Nellie Liang. 2018. "Monetary policy, financial conditions, and financial stability." *International Journal of Central Banking*, 14(1): 73–131.

Ajello, Andrea, Thomas Laubach, David López-Salido, Taisuke Nakata, et al. 2019. "Financial Stability and Optimal Interest Rate Policy." *International Journal of Central Banking*, 15(1): 279–326.

Alpanda, Sami, and Sarah Zubairy. 2017. "Addressing household indebtedness: Monetary, fiscal or macroprudential policy?" *European Economic Review*, 92: 47–73.

Angrist, Joshua D., Òscar Jordà, and Guido M. Kuersteiner. 2017. "Semiparametric estimates of monetary policy effects: String theory revisited." *Journal of Business & Economic Statistics*, 1–17.

Baron, Matthew, Emil Verner, and Wei Xiong. 2018. "Identifying Banking Crises." Working Paper.

Bauer, Gregory H., and Eleonora Granziera. 2017. "Monetary policy, private debt, and financial stability risks." *International Journal of Central Banking*, 13(3): 337–373.

Beaudry, Paul, Dana Galizia, and Franck Portier. 2015. "Reviving the limit cycle view of macroeconomic fluctuations." NBER Working Paper 21241.

Bernanke, Ben S. 2002. "Asset-Price "Bubbles" and Monetary Policy. Speech before the New York Chapter of the National Association for Business Economics, New York, NY."

Bernanke, Ben S. 2010. "Monetary policy and the housing bubble: A speech at the Annual Meeting of the American Economic Association, Atlanta, Georgia, January 3, 2010."

Bernanke, Ben S., and Mark Gertler. 2000. "Monetary policy and asset price volatility." NBER Working Paper 7559.

Bernanke, Ben S., and Mark Gertler. 2001. "Should central banks respond to movements in asset prices?" *American Economic Review*, 91(2): 253–257.

Cerra, Valerie, and Sweta Chaman Saxena. 2008. "Growth dynamics: The myth of economic recovery." *American Economic Review*, 98(1): 439–57.

Christiano, Lawrence J., and Terry J. Fitzgerald. 2003. "The band pass filter." *International Economic Review*, 44(2): 435–465.

Eichengreen, Barry. 1992. *Golden fetters: The Gold Standard and the Great Depression, 1919-1939.* Oxford University Press.

Filardo, Andrew J., and Phurichai Rungcharoenkitkul. 2016. "A quantitative case for leaning against the wind." BIS Working Paper 594.

Gerdrup, Karsten R., Frank Hansen, Tord Krogh, and Junior Maih. 2017. "Leaning against the wind when credit bites back." *International Journal of Central Banking*, 13(3): 287–320.

Gilchrist, Simon, and John V. Leahy. 2002. "Monetary policy and asset prices." *Journal of Monetary Economics*, 49(1): 75–97.

Gourio, François, Anil K. Kashyap, and Jae W. Sim. 2018. "The trade offs in leaning against the wind." *IMF Economic Review*, 66(1): 70–115.

Hamilton, James D. 2018. "Why you should never use the Hodrick-Prescott filter." *Review of Economics and Statistics*, 100(5): 831–843.

Hodrick, Robert J. 2020. "An exploration of trend-cycle decomposition methodologies in simulated data." NBER Working Paper 26750.

Hodrick, Robert J., and Edward C. Prescott. 1997. "Postwar US business cycles: An empirical investigation." *Journal of Money, Credit, and Banking*, 1–16.

Ilzetzki, Ethan, Carmen M. Reinhart, and Kenneth S. Rogoff. 2019. "Exchange arrangements entering the twenty-first century: Which anchor will hold?*." *The Quarterly Journal of Economics*, 134(2): 599–646.

Jordà, Òscar, Moritz Schularick, and Alan M. Taylor. 2013. "When credit bites back." *Journal of Money, Credit and Banking*, 45: 3–28.

Jordà, Òscar, Moritz Schularick, and Alan M. Taylor. 2017. "Macrofinancial history and the new business cycle facts." In *NBER Macroeconomics Annual 2016*. Vol. 31, , ed. Martin Eichenbaum and Jonathan A. Parker. University of Chicago Press.

Jordà, Òscar, Moritz Schularick, and Alan M. Taylor. 2019. "The effects of quasi-random monetary experiments." *Journal of Monetary Economics*, 112: 22–40.

Kaminsky, Graciela L., and Carmen M. Reinhart. 1999. "The twin crises: The causes of banking and balance-of-payments problems." *American Economic Review*, 89(3): 473–500.

Korinek, Anton, and Alp Simsek. 2016. "Liquidity trap and excessive leverage." *American Economic Review*, 106(3): 699–738.

New York Times. 1991. "Bundesbank increases rates to highest level since 1948."

Obstfeld, Maurice, and Alan M. Taylor. 2004. *Global capital markets: Integration, crisis, and growth.* Cambridge University Press.

Quinn, Dennis, Martin Schindler, and A. Maria Toyoda. 2011. "Assessing measures of financial openness and integration." *IMF Economic Review*, 59(3): 488–522.

Reinhart, Carmen M., and Kenneth S. Rogoff. 2011. "From financial crash to debt crisis." *American Economic Review*, 101(5): 1676–1706.

Richter, Björn, Moritz Schularick, and Paul Wachtel. forthcoming. "When to Lean Against the Wind." Journal of Money, Credit and Banking.

Schularick, Moritz, and Alan M. Taylor. 2012. "Credit booms gone bust: Monetary policy, leverage cycles and financial crises, 1870–2008." *American Economic Review*, 102(2): 1029–1061.

Stein, Jeremy C. 2013. "Overheating in credit markets: origins, measurement, and policy responses. Speech at the "Restoring Household Financial Stability after the Great Recession: Why Household Balance Sheets Matter" research symposium sponsored by the Federal Reserve Bank of St. Louis, St. Louis, Missouri."

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

Svensson, Lars E. O. 2016. "Cost-benefit analysis of leaning against the wind: Are costs larger also with less effective macroprudential policy?" IMF Working Paper 16/3.

Svensson, Lars E. O. 2017. "Cost-benefit analysis of leaning against the wind." *Journal of Monetary Economics*, 90: 193–213.

Tenreyro, Silvana, and Gregory Thwaites. 2016. "Pushing on a string: US monetary policy is less powerful in recessions." *American Economic Journal: Macroeconomics*, 8(4): 43–74.

Ward, Felix. 2017. "Spotting the danger zone: forecasting financial crises with classification

tree ensembles and many predictors." *Journal of Applied Econometrics*, 32(2): 359–378.

Woodford, Michael. 2012. "Inflation targeting and financial stability." NBER Working Paper 17967.

Online Appendix

to *Leaning against the wind and crisis risk*

Moritz Schularick[*]   Lucas ter Steege [†]   Felix Ward[‡]

A.   ALTERNATIVE TIME SERIES FILTERS

The main results in the paper use the one-sided HP filter to obtain cyclical components of credit and asset price time series to date boom periods. Here we investigate the robustness of our results with regard to that choice by using two alternative time series filters. Specifically, as the first alternative, we use the Christiano-Fitzgerald bandpass filter (Christiano and Fitzgerald, 2003), which we specify such that fluctuations in the 2- to 16-year period range are isolated. The results for the boom phases are shown in Figure A.1, while the crisis risk responses and the asymmetric responses are shown in Figure A.3, Figure A.5, and Figure A.7.

As the second alternative, we employ the novel non-parametric filtering method that has recently been proposed by Hamilton (2018), in which the cyclical component of a time series is defined as the residuals from an OLS regression of future values of the time series on a constant and its own lags. Results using this filter for boom phases are shown in Figure A.2, and the results for the crisis risk responses and the asymmetric responses are shown in Figure A.4, Figure A.6, and Figure A.8. For both alternative filters, we find very similar classifications of boom phases and also similar results for the baseline empirical specification as well as for the asymmetric specifications.

[*]Federal Reserve Bank of New York and Department of Economics, University of Bonn; and CEPR (schularick@uni-bonn.de).

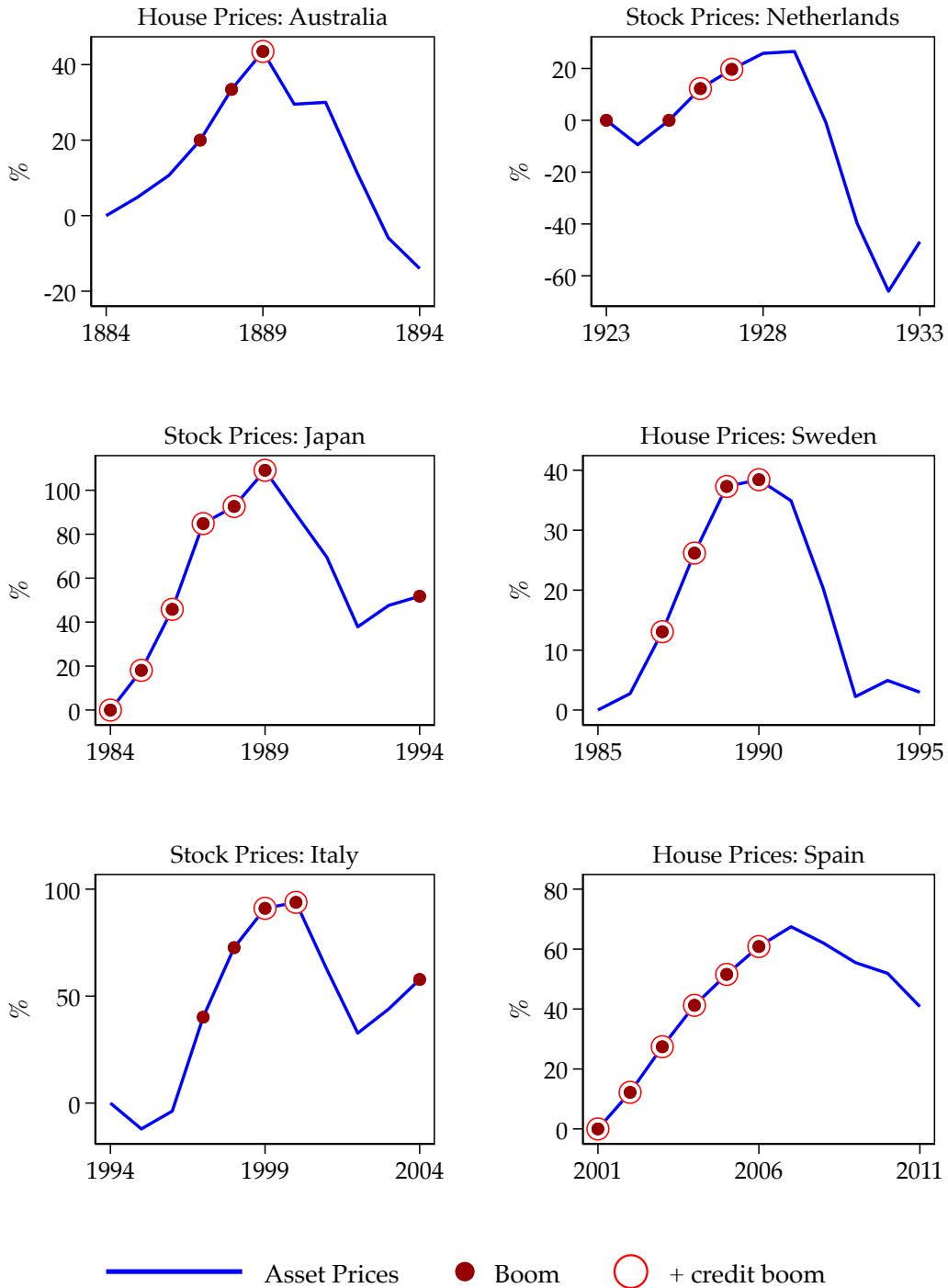[†]Department of Economics, University of Bonn; (s6luters@uni-bonn.de).

[‡]Erasmus School of Economics, Erasmus University Rotterdam; and Tinbergen Institute (ward@ese.eur.nl).

Figure A.1: Asset prices and boom periodsstiano-Fitzgerald filter
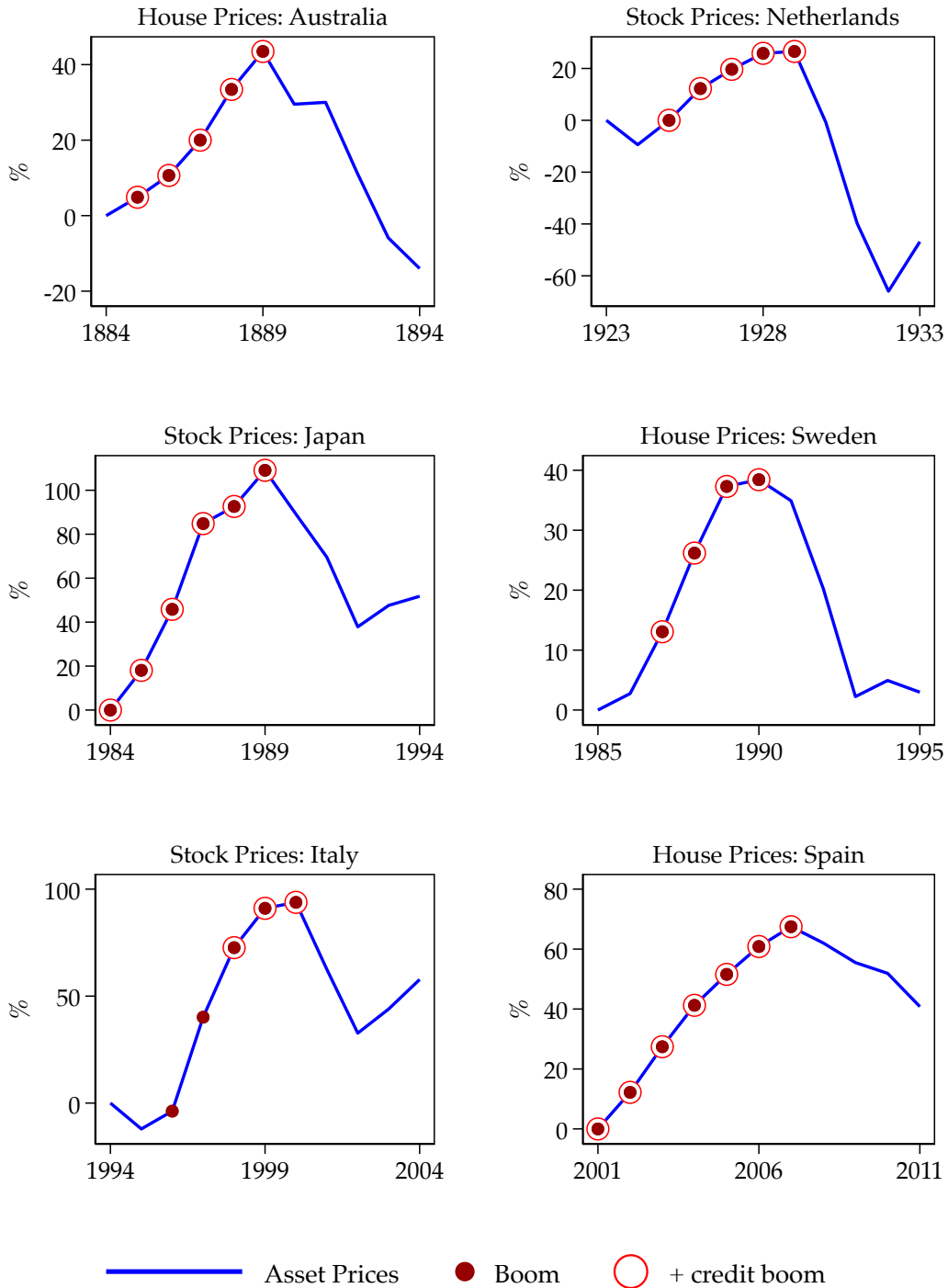
Notes: The figure shows log real asset price deviations from their level at the beginning of the time frame. Solid circles indicate asset price booms, and hollow circles indicate accompanying credit booms. The boom state is defined as the conjunction of growth and an above trend level (equation 1). Trend levels are based on a one-sided CF filter (2-year to 16-year period range).

Figure A.2: Asset prices and boom periods—Hamilton filter

House Prices: Australia

Stock Prices: Netherlands

Stock Prices: Japan

House Prices: Sweden

Stock Prices: Italy

House Prices: Spain

——— Asset Prices   ● Boom   ○ + credit boom

Notes: The figure shows log real asset price deviations from their level at the beginning of the time frame. Solid circles indicate asset price booms, and hollow circles indicate accompanying credit booms. The boom state is defined as the conjunction of growth and an above trend level (equation 1). Trend levels are based on a one-sided Hamilton filter (lags 3 to 6).

Figure A.3: Crisis risk response—Christiano-Fitzgerald filter
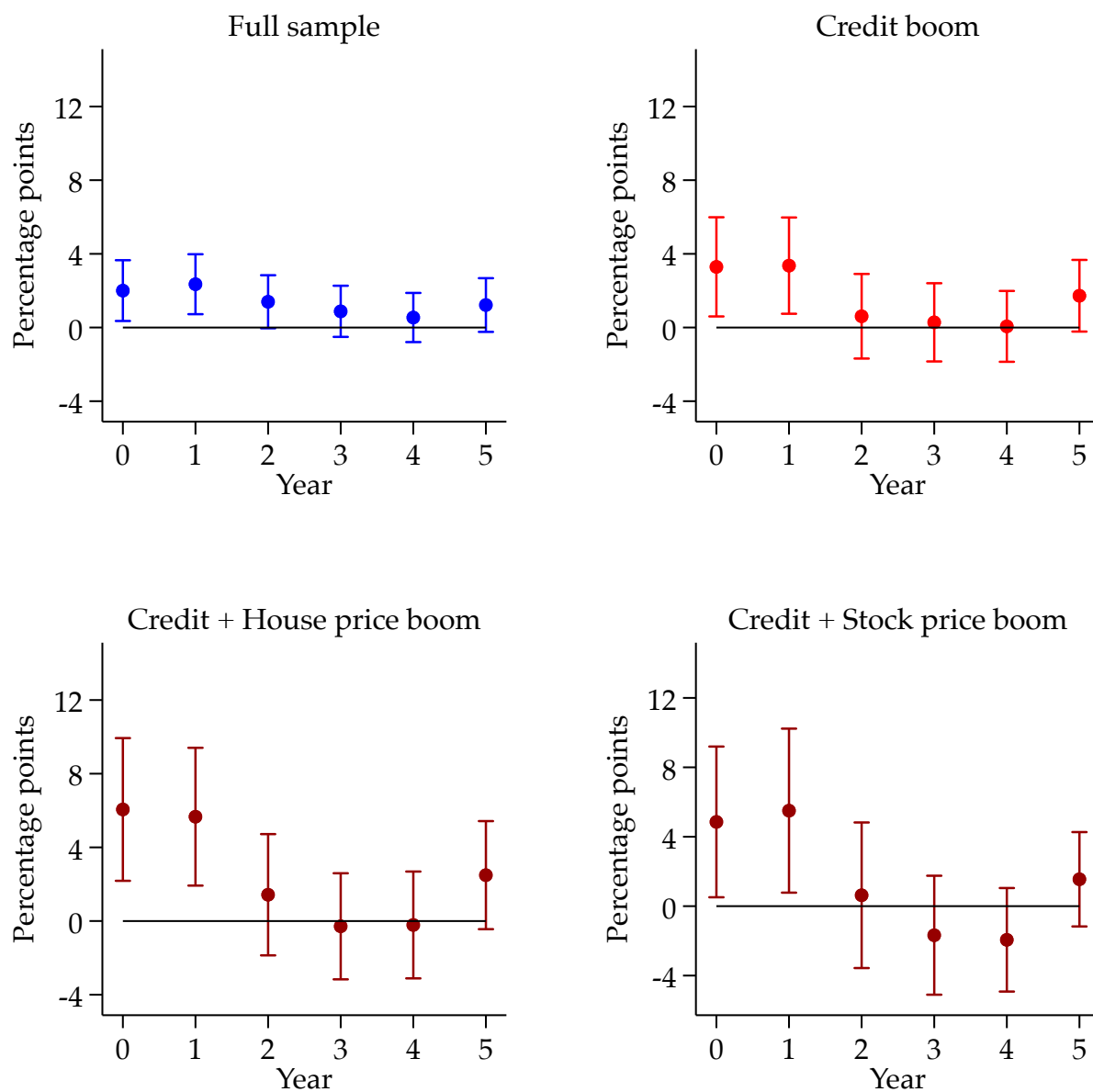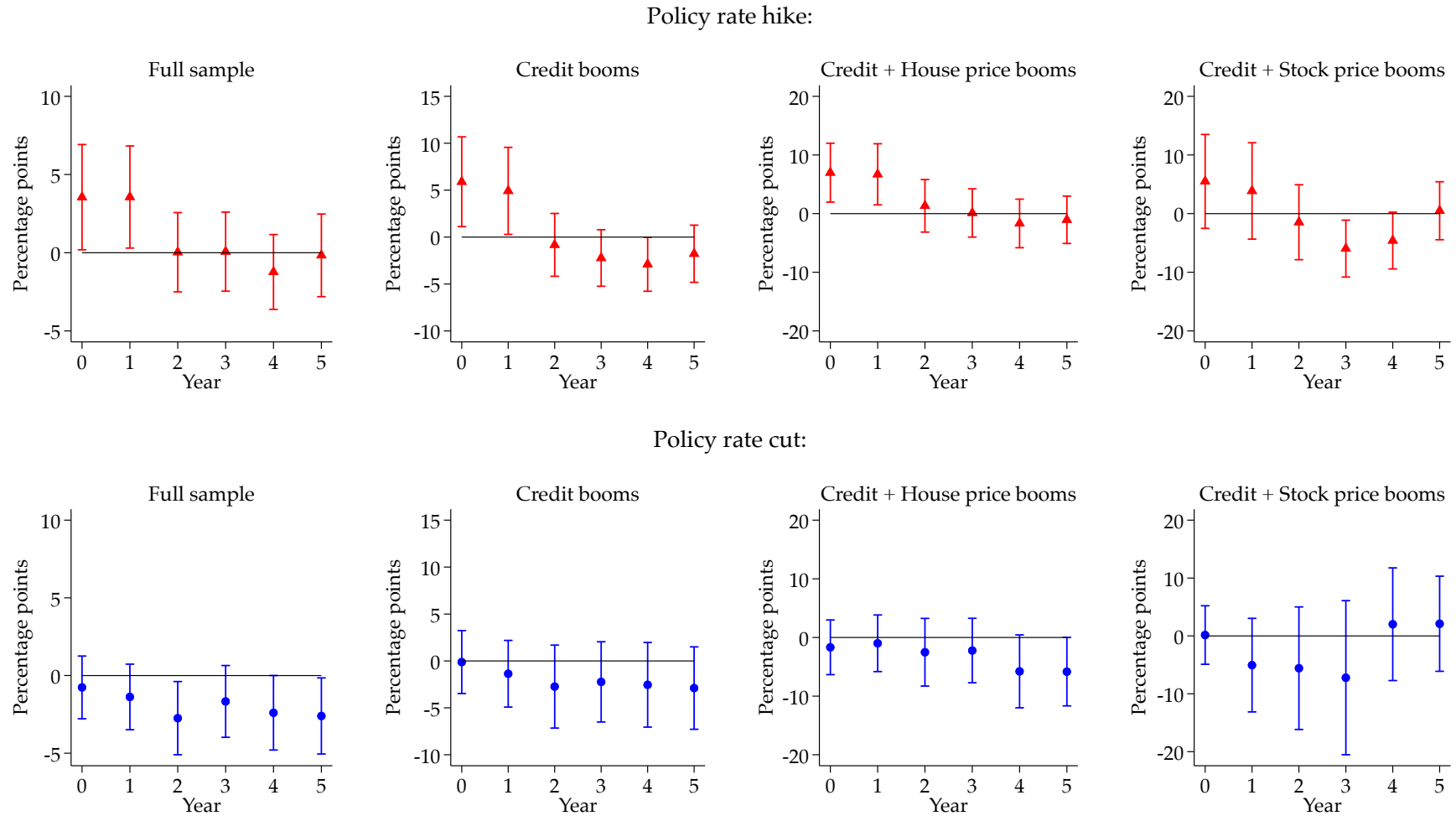
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided CF filter (2-year to 16-year period range).

Figure A.4: Crisis risk response—Hamilton filter
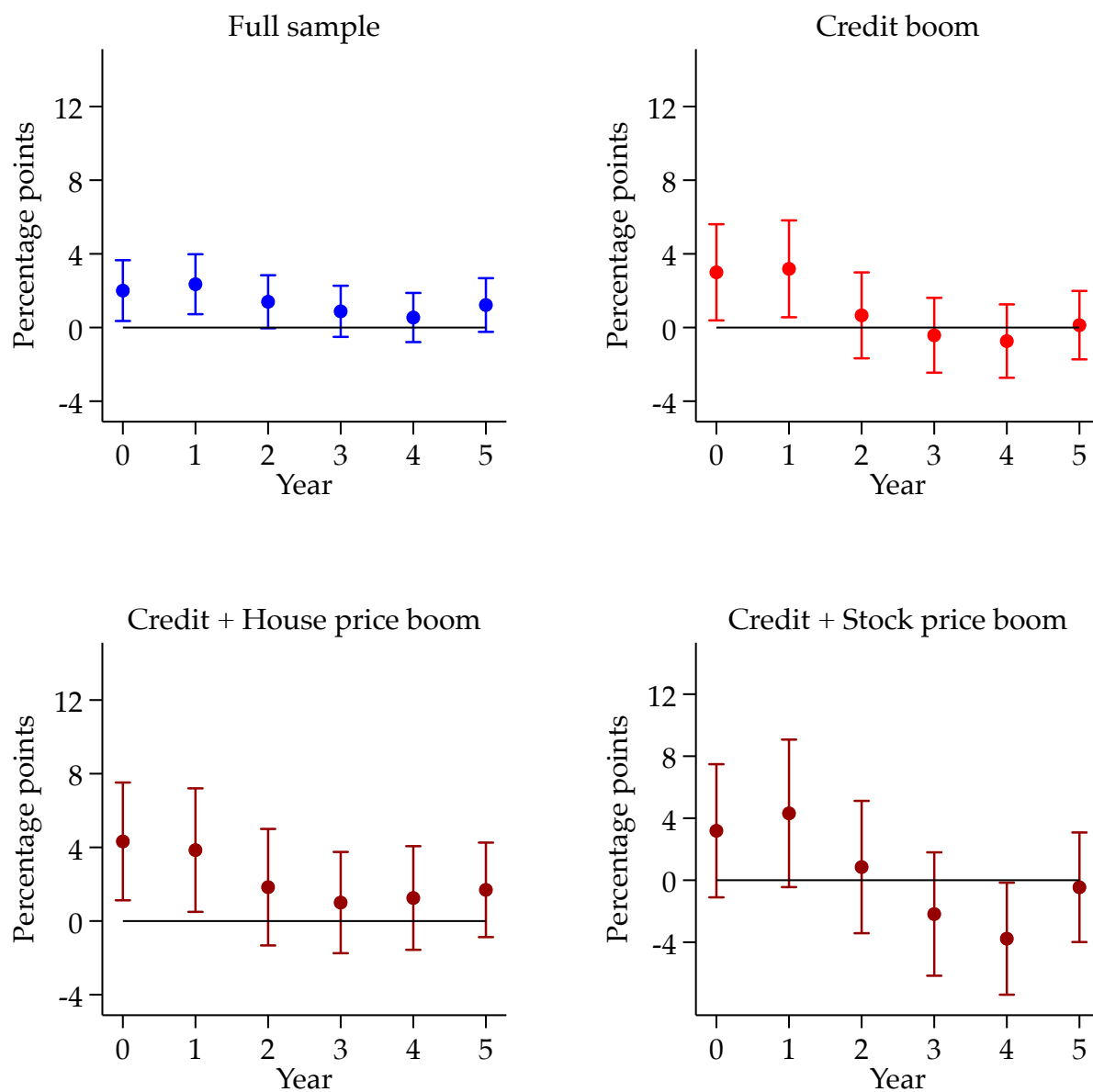
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided Hamilton filter (lags 3 to 6).

Figure A.5: Rate hikes versus rate cuts and crisis risk—Christiano-Fitzgerald filter
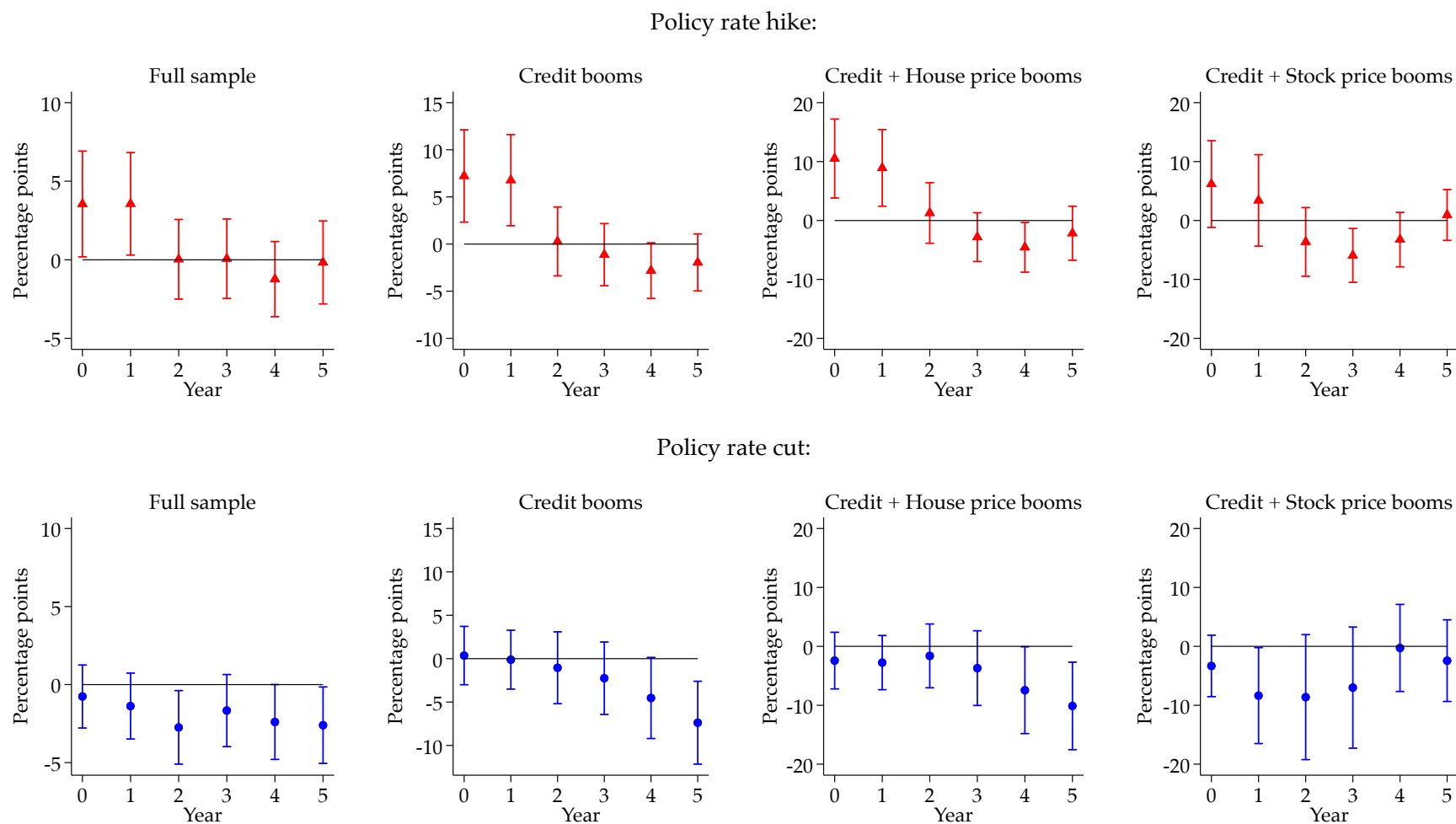
Policy rate hike:

Policy rate cut:

Notes: The figure shows the asymmetric annual crisis probability effects of a 1 ppt policy rate hike (top panels) and a 1 ppt policy rate cut (bottom panels). Solid bars depict pointwise 95% confidence intervals. The "Full sample" panels depict the non-state-dependent effects. The other panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided CF filter (2-year to 16-year period range).

Figure A.6: Rate hikes versus rate cuts and crisis risk—Hamilton filter

Policy rate hike:

Policy rate cut:

Notes: The figure shows the asymmetric annual crisis probability effects of a 1 ppt policy rate hike (top panels) and a 1 ppt policy rate cut (bottom panels). Solid bars depict pointwise 95% confidence intervals. The "Full sample" panels depict the non-state-dependent effects. The other panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided Hamilton filter (lags 3 to 6).

Figure A.7: Early versus late interventions and crisis risk—Christiano-Fitzgerald filter
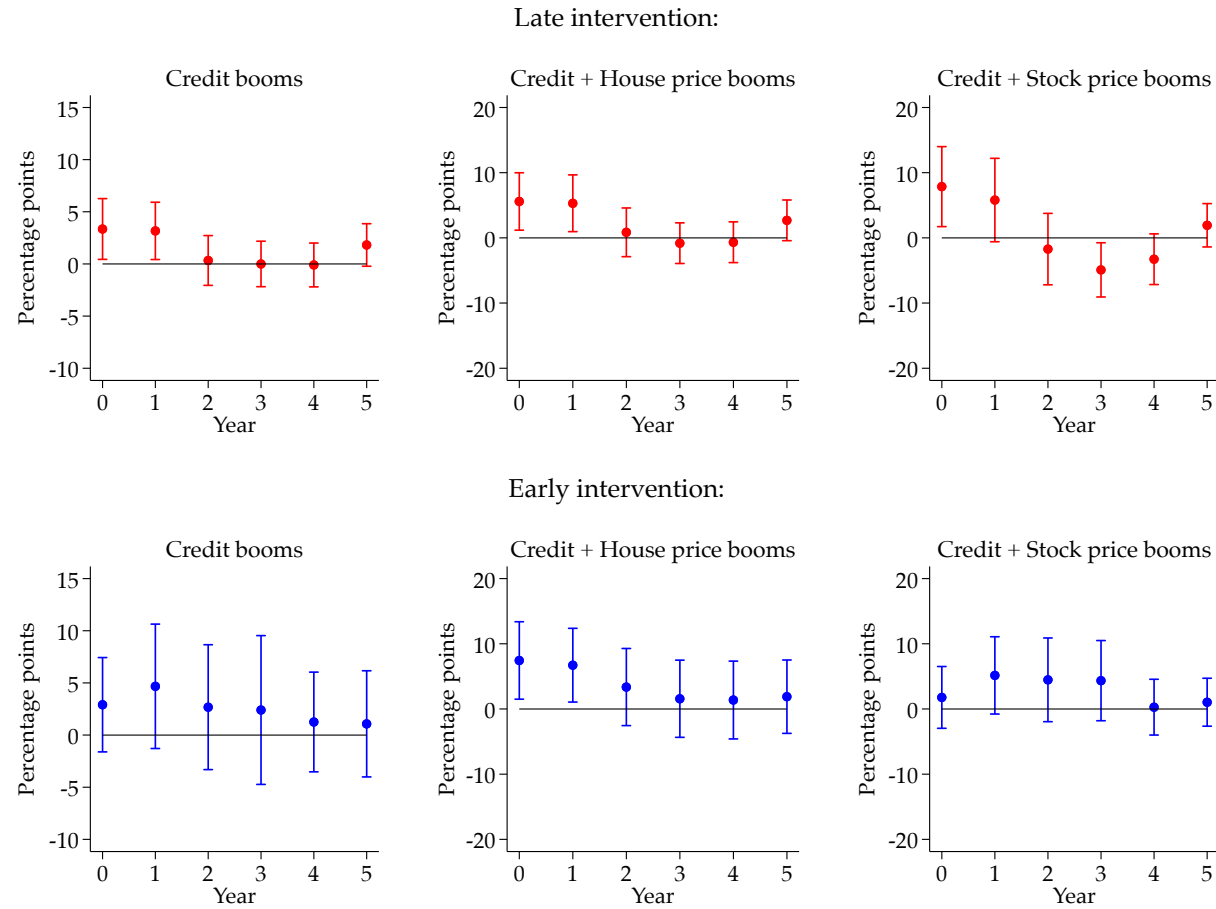
Late intervention:

Early intervention:

∞

Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first year of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms. Boom episodes are defined on the basis of a one-sided CF filter (2-year to 16-year period range).

Figure A.8: Early versus late interventions and crisis risk—Hamilton filter
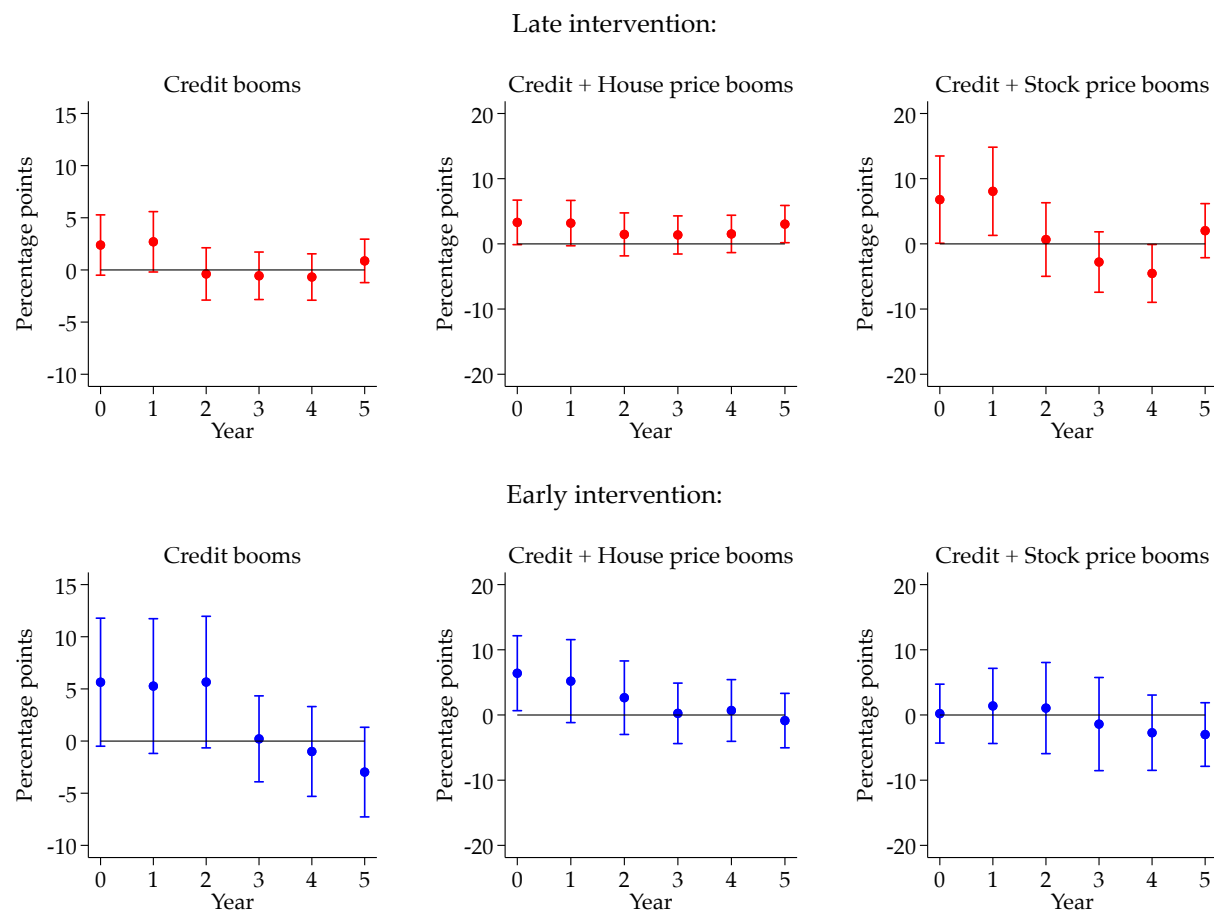
Late intervention:

Early intervention:

Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first year of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms. Boom episodes are defined on the basis of a one-sided Hamilton filter (lags 3 to 6).

B. Alternative financial crisis indicators

The way to date financial crises is always subject to debate, and different indicators of financial crises have been proposed in the literature. Our main results use the systemic financial crisis dummy from Schularick and Taylor (2012). One possible alternative to this indicator is the banking crisis dummy defined by Reinhart and Rogoff (2011). Their indicator marks a given year in a country as a banking crisis if either bank runs occurred that led to the closure, merging, or takeover by the public sector of one or more financial institutions, or, absent bank runs, if the closure, merging, takeover, or large-scale government assistance of one or more important financial institutions marked the beginning of similar outcomes for other financial institutions.

As a second robustness check, we also estimate all models using the dummy defined by Baron, Verner and Xiong (2018). Their approach to date banking crises creates a joint list of crisis dates from several studies and refines this list using data on bank equity price declines. More precisely, the joint list is refined by adding episodes in which both the bank equity index declines by 30% or more and the narrative record shows substantial evidence of widespread banking failures or bank runs. These two criteria are also used to remove crisis dates from the joint list if neither condition is met. Thus, in contrast to the crisis dummy used in the main text as well as the crisis dummy defined by Reinhart and Rogoff (2011), this approach goes beyond the narrative identification by adding additional quantitative requirements to date financial crises. Figures B.1-B.4 show that both the crisis risk responses and the crisis severity results from the main text are robust to using either indicator.

Figure B.1: Crisis risk response—Reinhart and Rogoff crisis dummies
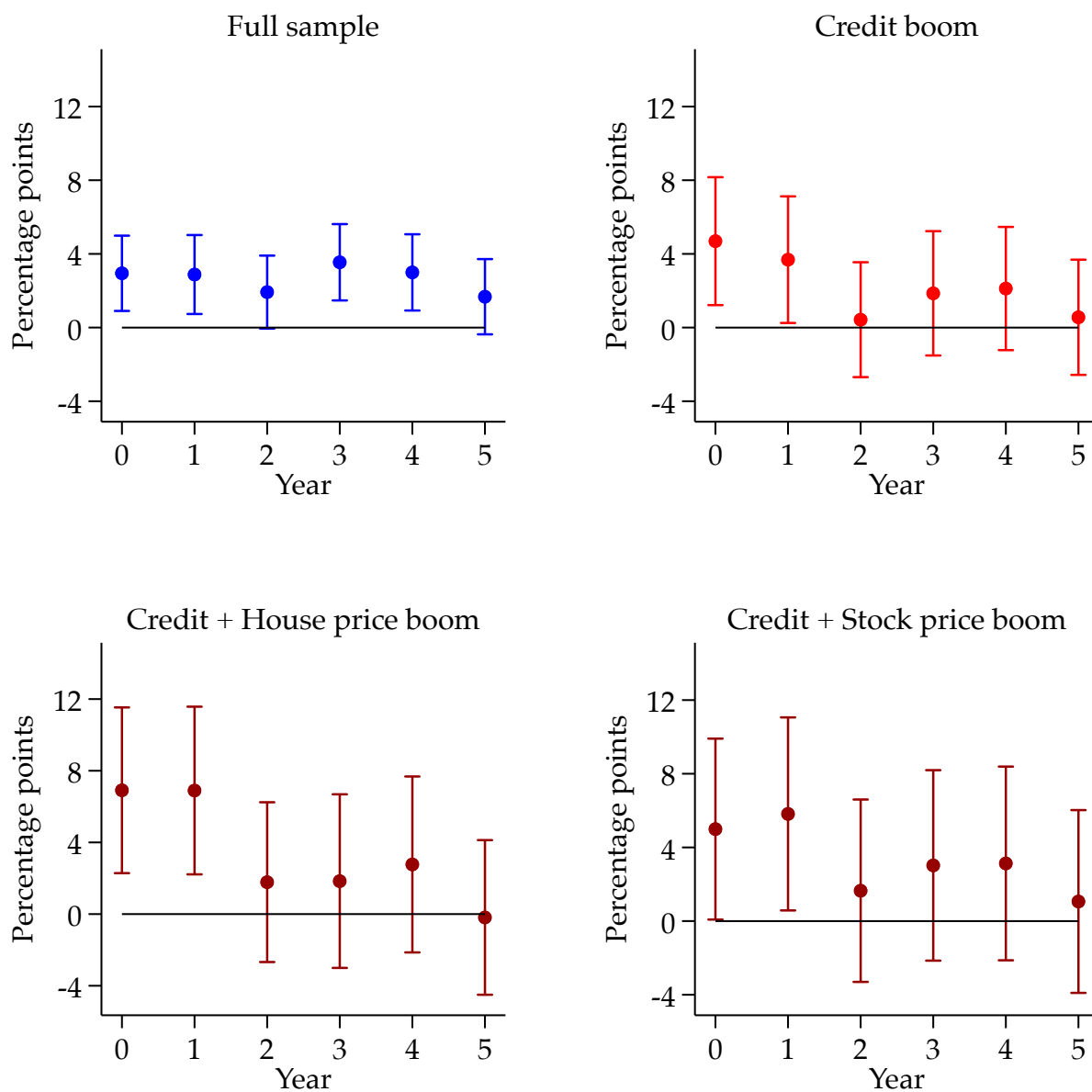
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. Banking crisis dummy from Reinhart and Rogoff (2011).

Figure B.2: Crisis risk response—Baron, Verner & Xiong crisis dummies
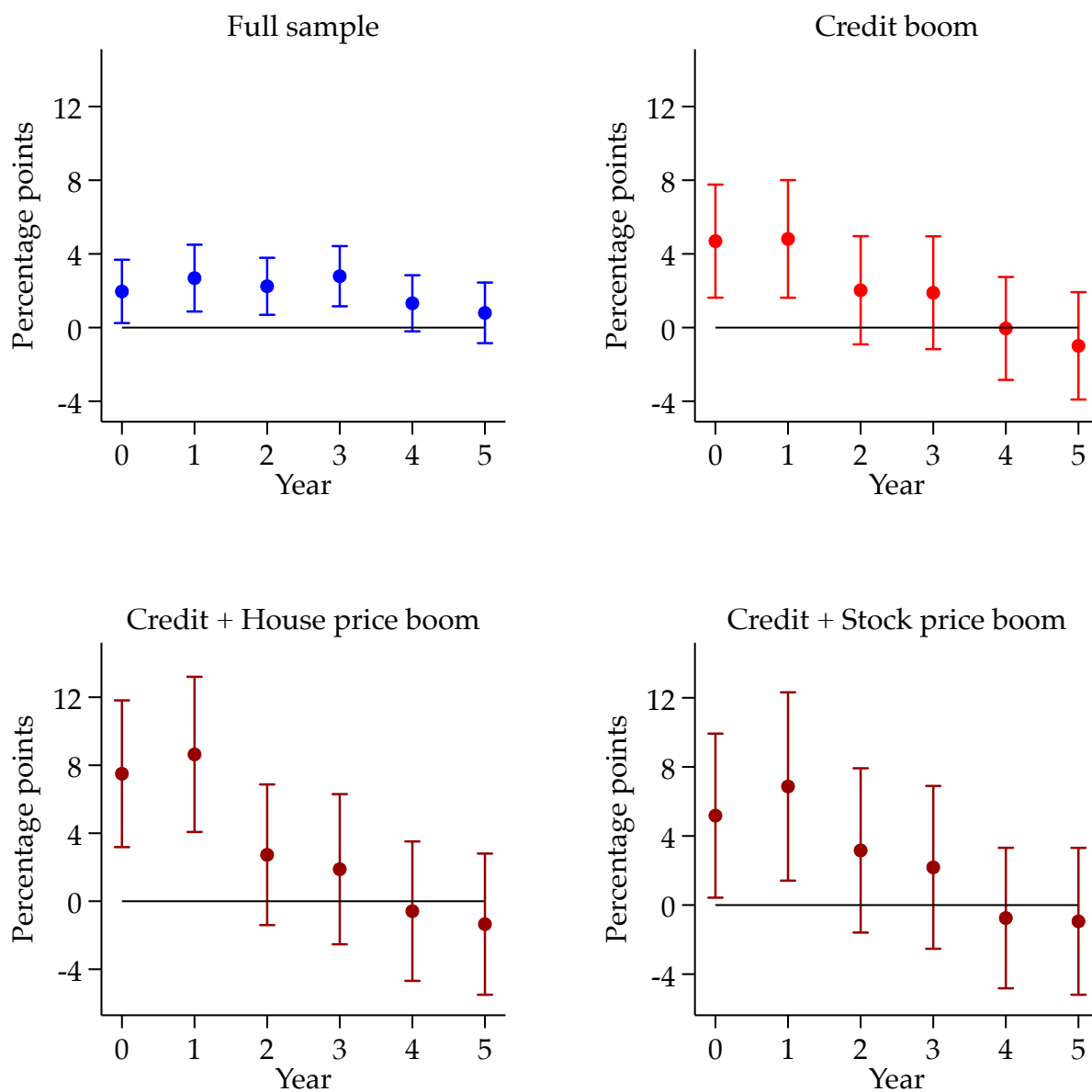
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. Banking crisis dummy from Baron, Verner and Xiong (2018).

Figure B.3: D-LAW and crisis severity—Reinhart and Rogoff crisis dummies
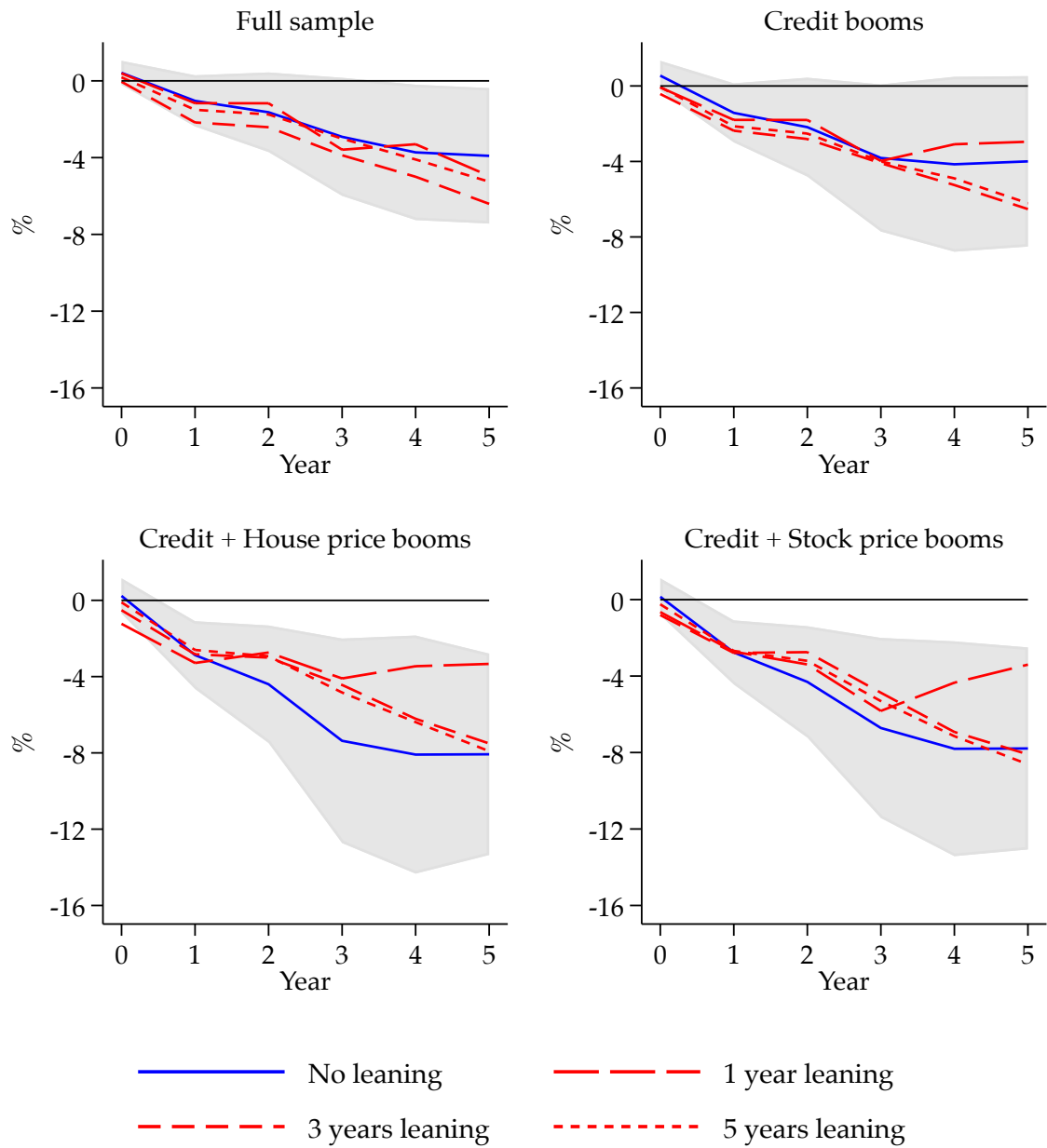
Notes: The figure shows how real GDP falls below trend following a financial crisis. Dashed lines indicate the same GDP loss conditional on monetary policy following a +1 ppt leaning stance before the crisis. The leaning stance is measured as the cumulative sum of policy rate changes over a one-year, three-year, and five-year pre-crisis horizon. Shaded areas depict pointwise 95% confidence bands. Banking crisis dummy from Reinhart and Rogoff (2011).

Figure B.4: D-LAW and crisis severity—Baron, Verner & Xiong crisis dummies
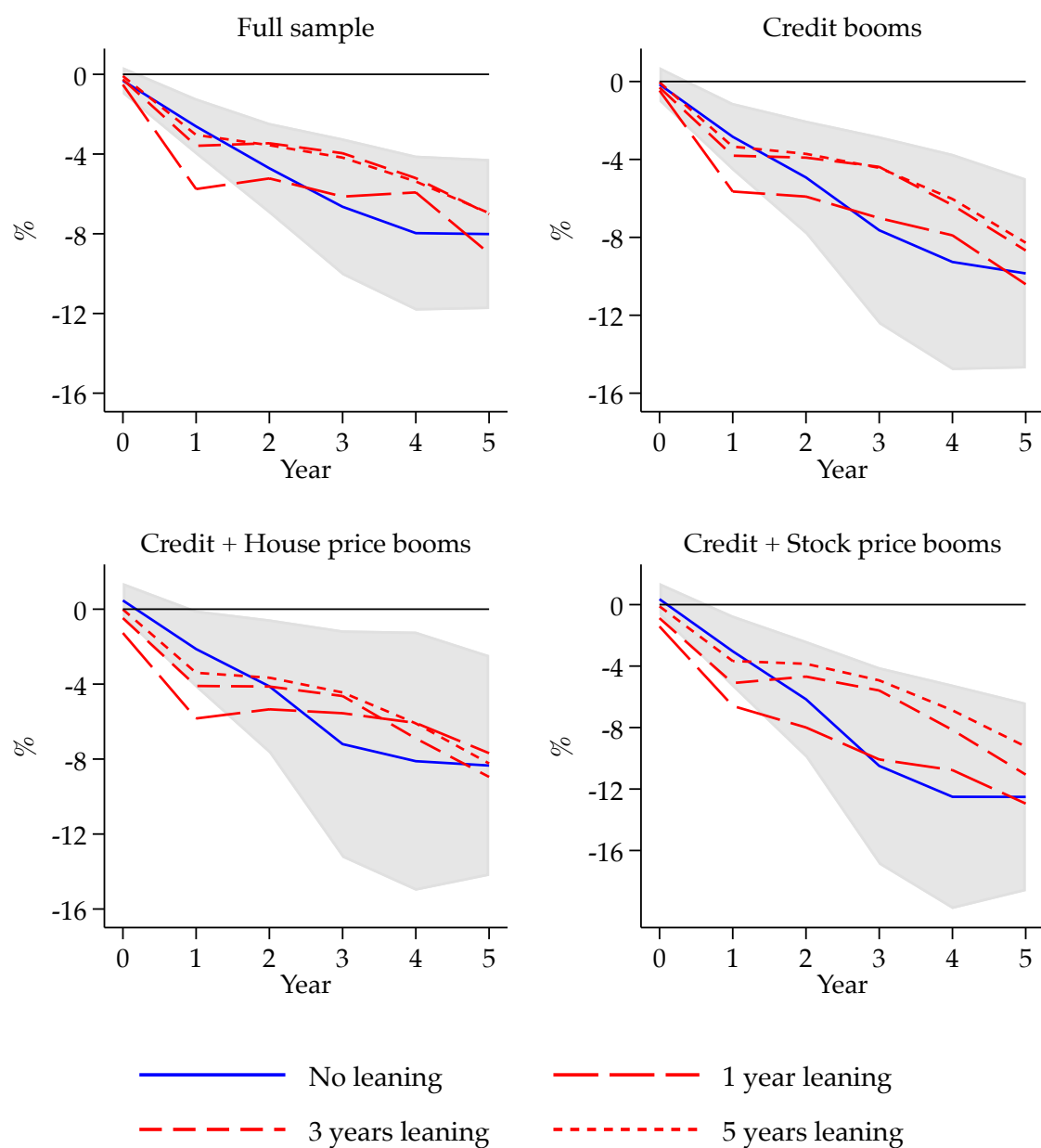
Notes: The figure shows how real GDP falls below trend following a financial crisis. Dashed lines indicate the same GDP loss conditional on monetary policy following a +1 ppt leaning stance before the crisis. The leaning stance is measured as the cumulative sum of policy rate changes over a one-year, three-year, and five-year pre-crisis horizon. Shaded areas depict pointwise 95% confidence bands. Banking crisis dummy from Baron, Verner and Xiong (2018).

C. Model specification and subsample analysis

This section's robustness checks are concerned with the model specification, temporal subsamples (pre-World War II, post-World War II, post-Bretton Woods), the definition of early and late interventions in section 4.1 of the main text, and potential spillover effects.

First, Figures C.1 to C.3 show that logit models yield results that are very similar to the baseline linear probability model (LPM). An advantage of the LPM model is that it allows us to use a rich set of controls, as is common in IRF analysis through local projections (Stock and Watson, 2018; Jordà, Schularick and Taylor, 2019; Jordà et al., 2019). By contrast, to prevent convergence failures, our logit models require a more parsimonious set of control variables. We include only up to one lag of each control variable, and we exclude consumption growth, investment growth, long-term interest rate changes, stock price growth, house price growth, and the current account-to-GDP ratio from the baseline control set. This results in the following parsimonious set of control variables for the logit models: the crisis dummy, the first difference in short-term interest rates, the growth rates of real credit, real GDP, consumer prices, and global GDP. [1]

Figures C.4 to C.7 compare the average predicted crisis probability of the LPM and logit models.[2] These figures explain why the LPM model results align closely with the logit model results. First, the models' crisis probability predictions are restricted to a relatively narrow range, from around 0 to 0.2. It is well-known that within such narrow ranges the LPM can give a good approximation of a non-linear model's prediction. Second, for the most part the LPM models' prediction stays within the 0-1 probability range. Only for the most negative interest rate innovations in our sample does the LPM begin to predict negative values. For an increase in Δr from 0 to 1 – the policy experiment we are most interested – crisis probability predictions are positive and below 1. Third, for the -1 to $+1$ ppt range, the logit and LPM models mostly make statistically indistinguishable crisis probability predictions. The only exceptions to this are horizon 0 and horizon 1 predictions in the house price and stock price boom subsamples. Here, however, the LPM is the more conservative choice, predicting lower crisis probabilities than the logit model.

Figures C.8, C.9, and C.10 show how our baseline crisis risk response looks like in restricted sample periods. Overall, the subsample responses keep exhibiting a short-run crisis trigger pattern. For the pre-WW2 subsample we use the more parsimonious set of

---

[1]The more parsimonious set of controls also reduces the risk of obtaining biased maximum likelihood-estimates in settings where the number of regressors becomes non-negligible relative to the number of observations (Sur and Candès, 2019).

[2]For this comparison we estimate the LPM with the same parsimonious set of control variables as the logit model.

logit controls described above to not overburden the more limited data. The pre-WW2 estimates are less precise, and statistically significant crisis trigger effects are restricted to the credit boom and credit + house price boom subsamples. However, initial mean crisis probability estimates are positive also for the other two subsample. Evidence for a crisis risk reduction due to D-LAW is restricted to horizons four and five during credit booms. Quantitatively, however, this reduction barely offsets the initial crisis triggering effect. For the post-WW2 subsample the responses are very similar to our baseline responses, though the credit + house price boom response is somewhat smaller in size. For the smaller post-Bretton Woods sample we again use the more parsimonious set of control variables. The responses we obtain are very similar to our baseline results, with the exception that the crisis trigger effect in the credit + stock price boom is partially offset by a crisis risk reduction effect in years three and four after the D-LAW intervention. However, even for that subsample, the overall response is not supportive of a net benefit in terms of crisis probability due to the large initial triggering effect.

Third, regarding early versus late interventions, the main text defines an intervention as early if it occurs in the first year of a boom period. Figure C.11 and Figure C.12 show, respectively, that our main results are robust to changing this window to include the first two years of a boom phase or to include the first half. Expectedly, the early and late intervention results become more similar when the early intervention time window is extended to two years or to the whole first boom half.

Last, we run the spillover-correction check from Jordà, Schularick and Taylor (2019) to check whether non-policy rate spillover effects drive our results. For details, we refer to their paper, but the intuition is as follows. The instrumental variable approach assumes that the instrument affects the outcome only through its effect on the policy rate. This assumption may plausibly be violated because of trade and financial market linkages between the countries in our sample. The idea is to estimate this spillover effect from the sample of floating exchange rate countries and with this estimate correct the IV estimates. Figure C.13 shows that our baseline results are not driven by any potential bias arising from a failure of the exclusion restriction due to spillovers.

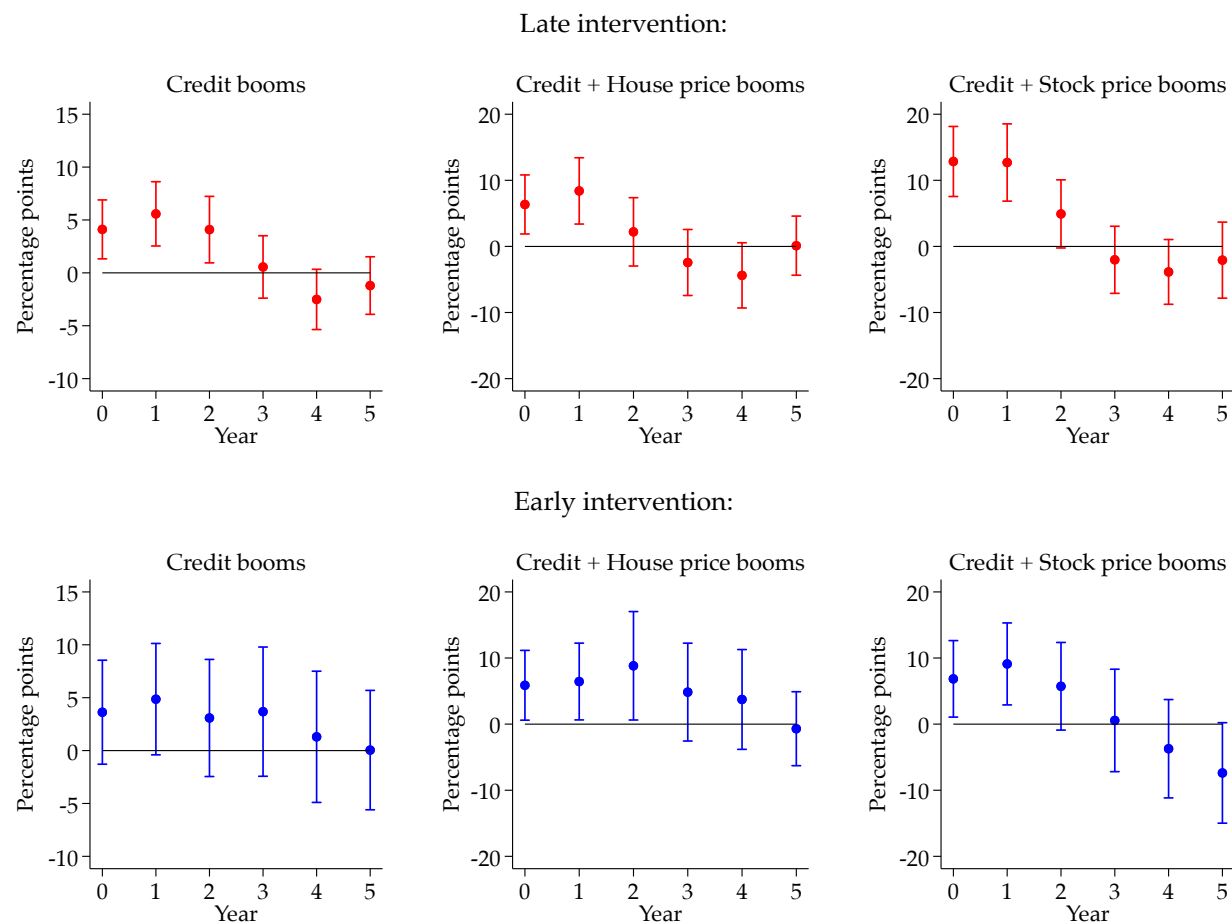Figure C.1: Crisis risk response—Logit model
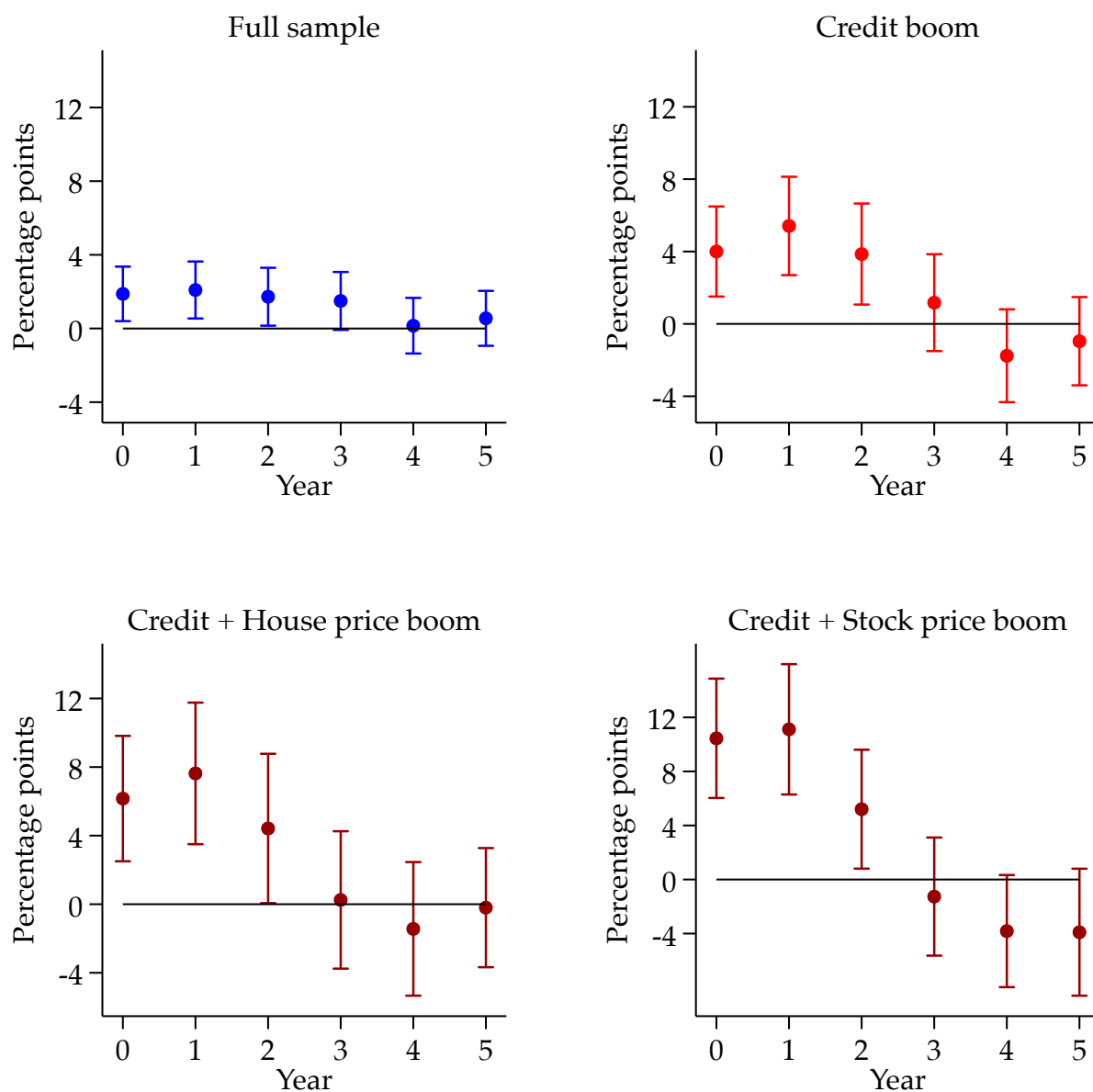
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100.

Figure C.2: Rate hikes versus rate cuts and crisis risk—Logit model
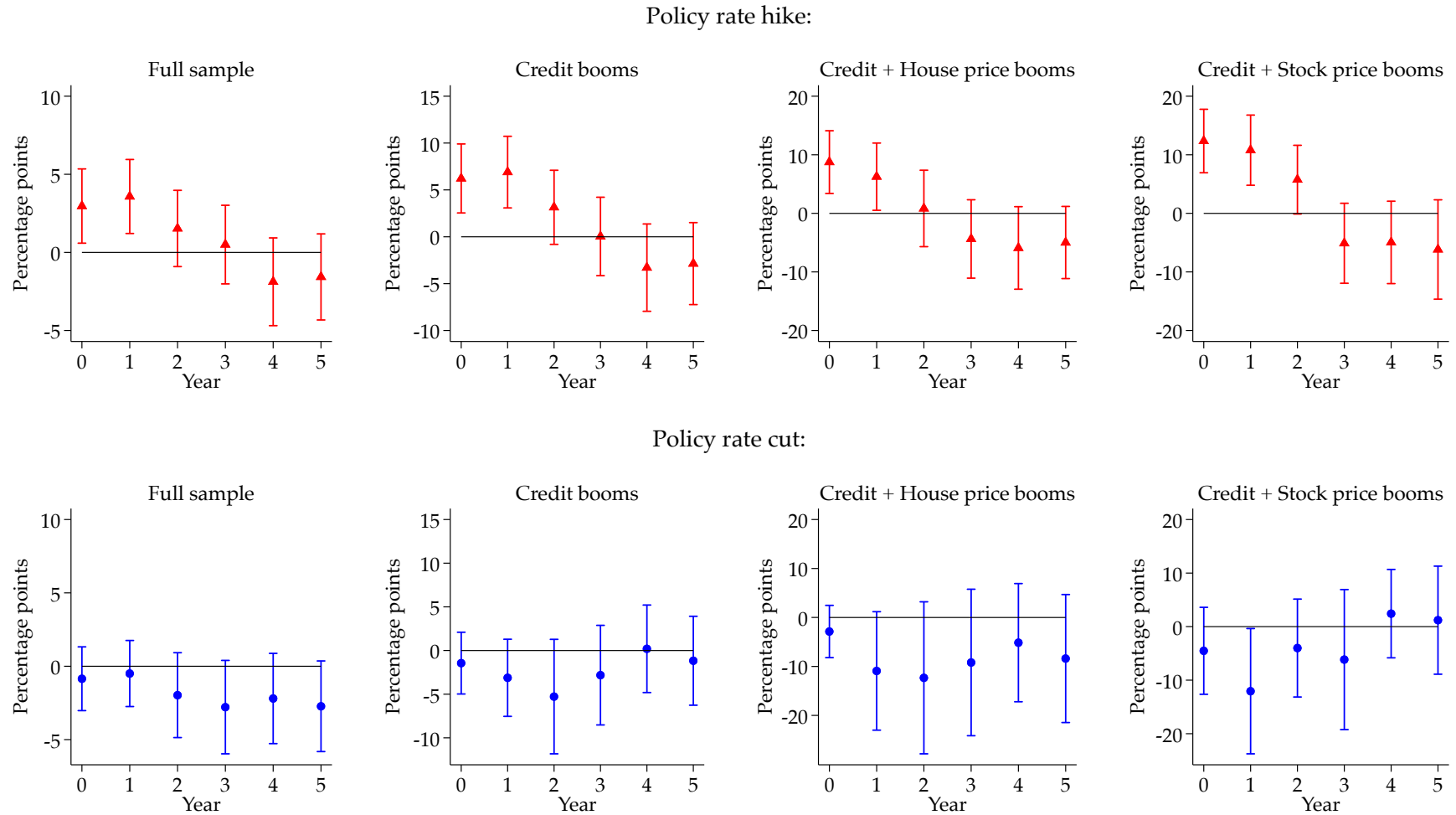
Policy rate hike:

Policy rate cut:

Notes: The figure shows the asymmetric annual crisis probability effects of a 1 ppt policy rate hike (top panels) and a 1 ppt policy rate cut (bottom panels). Solid bars depict pointwise 95% confidence intervals. The "Full sample" panels depict the non-state-dependent effects. The other panels depict the state-dependent effects during credit and asset price booms.

Figure C.3: Early versus late interventions and crisis risk—Logit model

Late intervention:

Early intervention:

Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first year of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms.

19

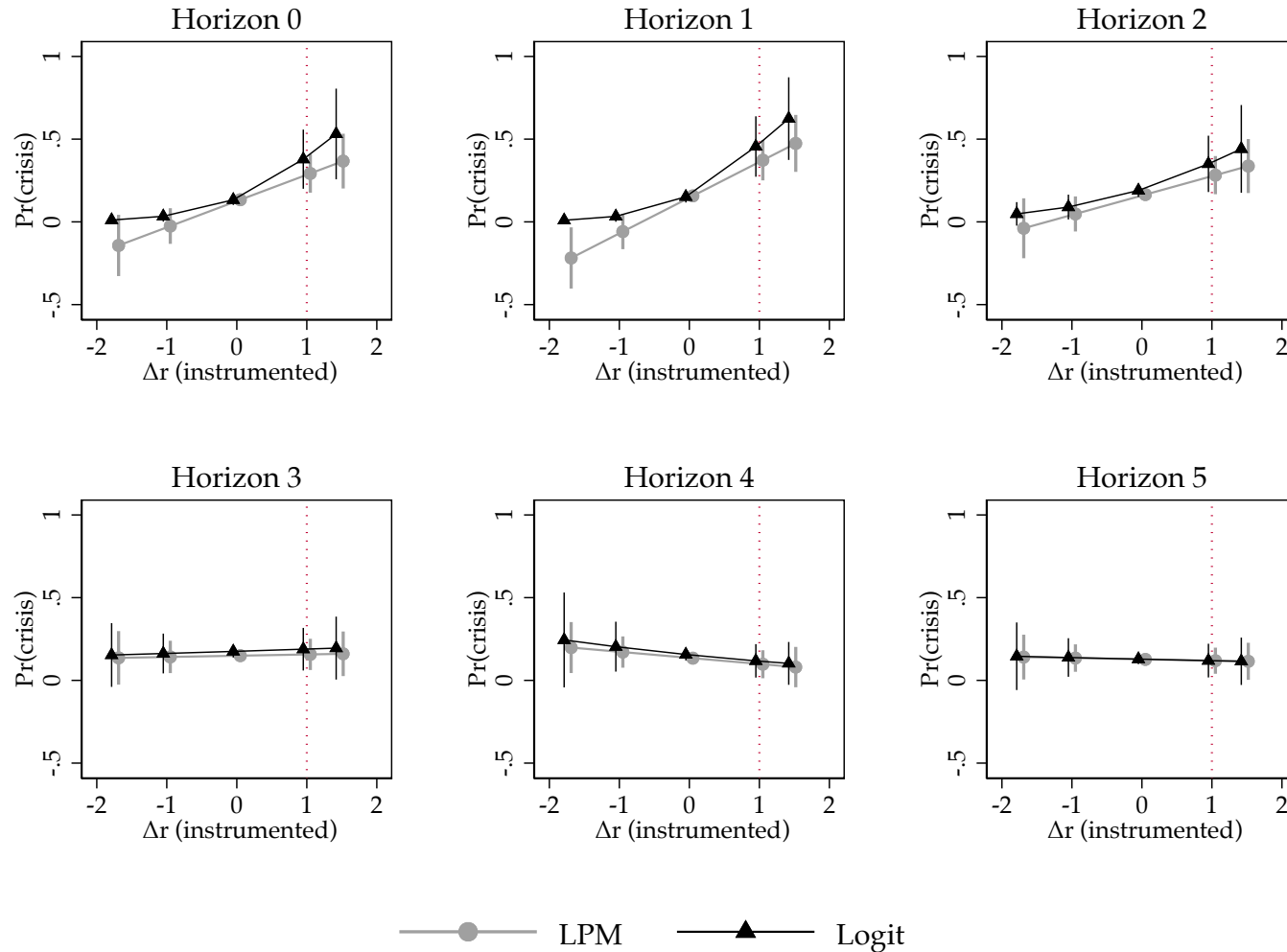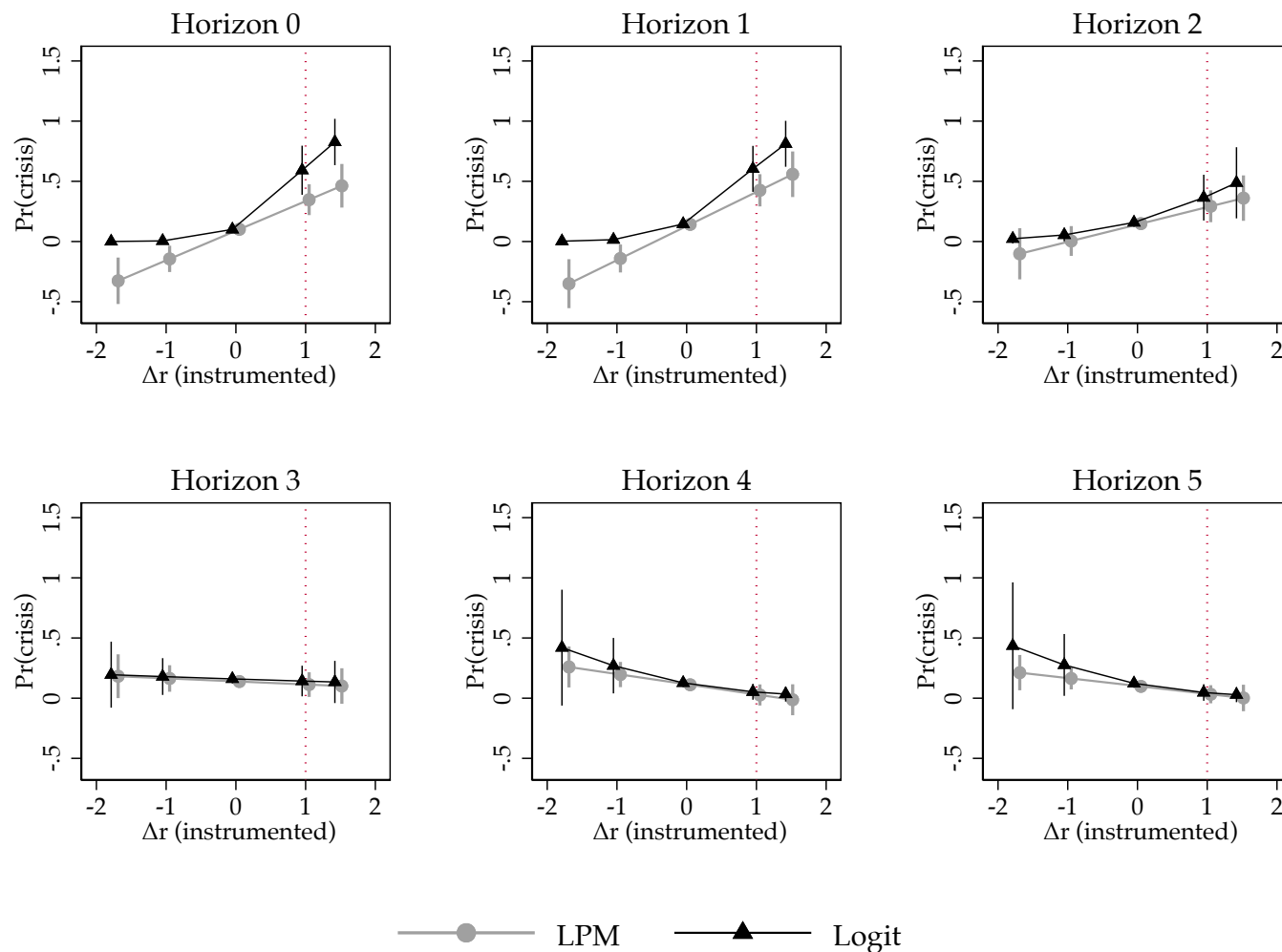Figure C.4: Predictive margins—Full sample
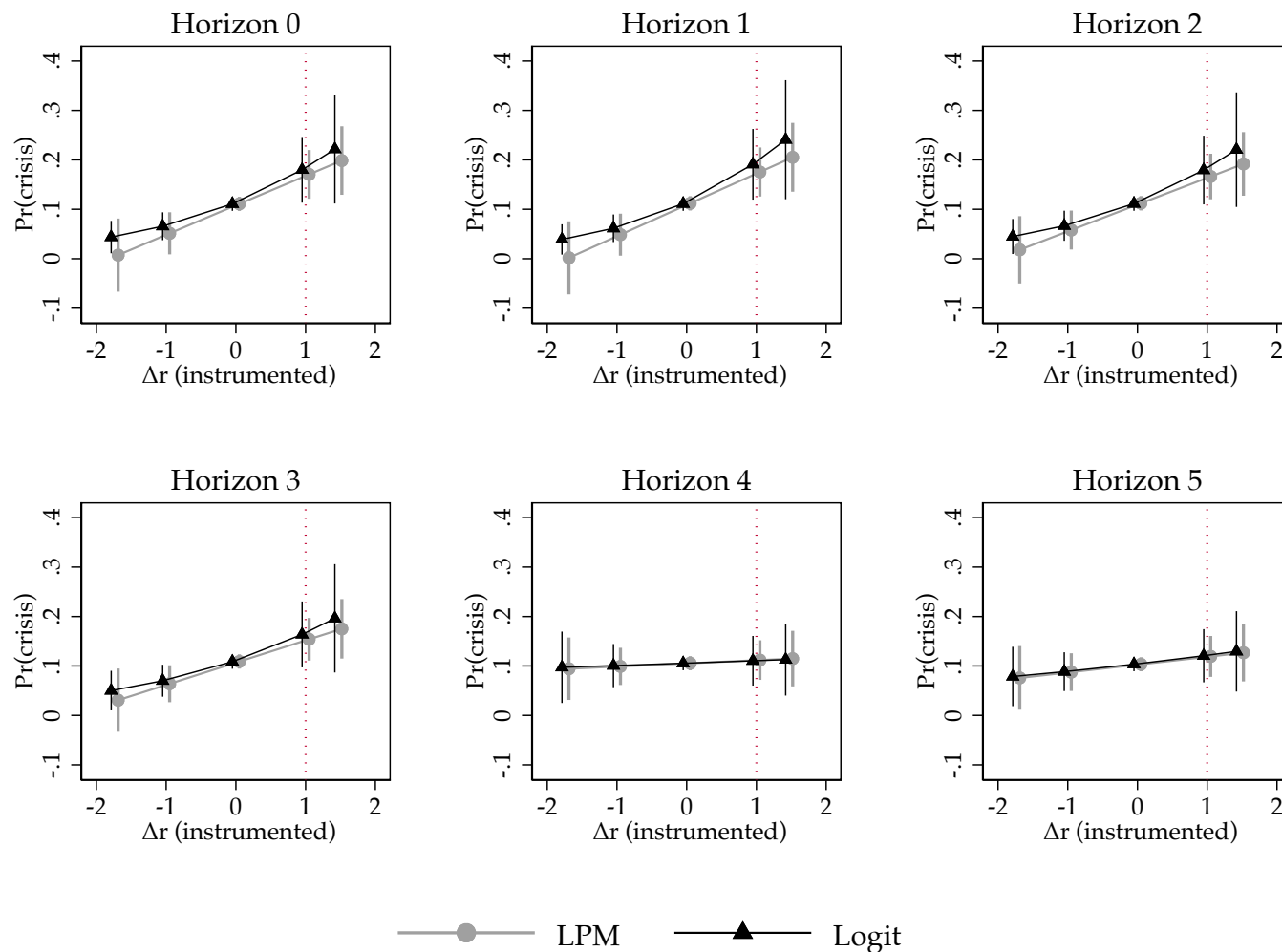
LPM — Logit

Notes: The figure shows the average predicted crisis probability of the LPM and logit models for each local projection horizon, and over the full range of instrumented policy rate changes ($min = -1.74$ ppt; $max = 1.47$ ppt). The vertical dotted line highlights the $+1$ ppt policy rate hike results. Solid bars depict 95% confidence intervals. The y-axis depicts the raw three-year crisis probability predictions not yet annualized using $\widehat{P}^{\text{annual}} = 1 - (1 - \widehat{P}^{\text{three years}})^3$.

Figure C.5: Predictive margins—Credit booms
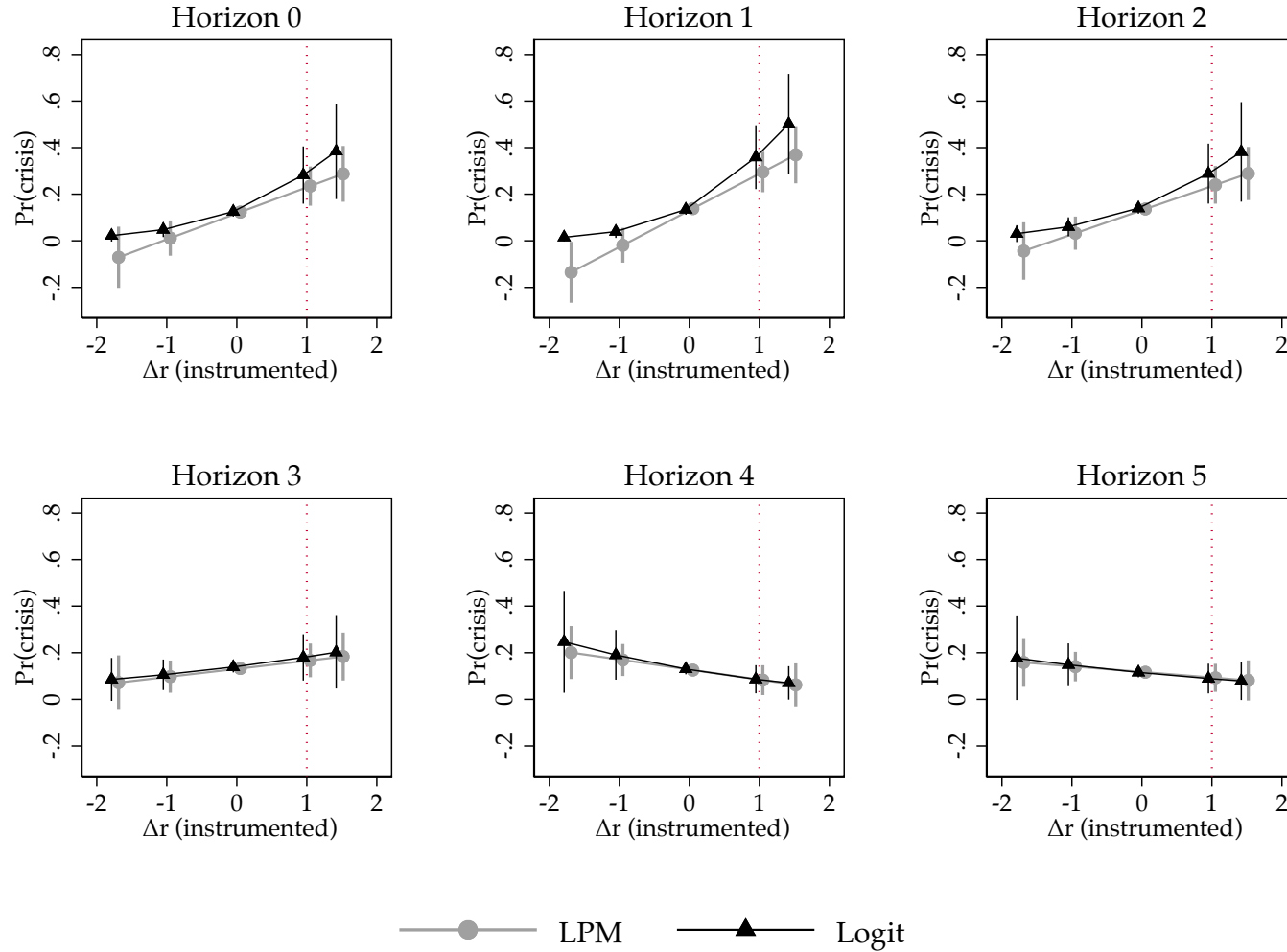
Notes: The figure shows the average predicted crisis probability of the LPM and logit models for each local projection horizon, and over the full range of instrumented policy rate changes ($min = -1.74$ ppt; $max = 1.47$ ppt). The vertical dotted line highlights the +1 ppt policy rate hike results. Solid bars depict 95% confidence intervals. The y-axis depicts the raw three-year crisis probability predictions not yet annualized using $\widehat{P}^{\text{annual}} = 1 - (1 - \widehat{P}^{\text{three years}})^3$.

Figure C.6: Predictive margins—Credit + house price booms

Notes: The figure shows the average predicted crisis probability of the LPM and logit models for each local projection horizon, and over the full range of instrumented policy rate changes ($min = -1.74$ ppt; $max = 1.47$ ppt). The vertical dotted line highlights the $+1$ ppt policy rate hike results. Solid bars depict 95% confidence intervals. The y-axis depicts the raw three-year crisis probability predictions not yet annualized using $\widehat{P}^{\text{annual}} = 1 - (1 - \widehat{P}^{\text{three years}})^3$.

Figure C.7: Predictive margins—Credit + stock price booms

Horizon 0 · Horizon 1 · Horizon 2 · Horizon 3 · Horizon 4 · Horizon 5

y-axis: Pr(crisis) · *x-axis:* Δr (instrumented)

Legend: LPM — Logit

Notes: The figure shows the average predicted crisis probability of the LPM and logit models for each local projection horizon, and over the full range of instrumented policy rate changes ($min = -1.74$ ppt; $max = 1.47$ ppt). The vertical dotted line highlights the $+1$ ppt policy rate hike results. Solid bars depict 95% confidence intervals. The y-axis depicts the raw three-year crisis probability predictions not yet annualized using $\widehat{P}^{\text{annual}} = 1 - (1 - \widehat{P}^{\text{three years}})^3$.

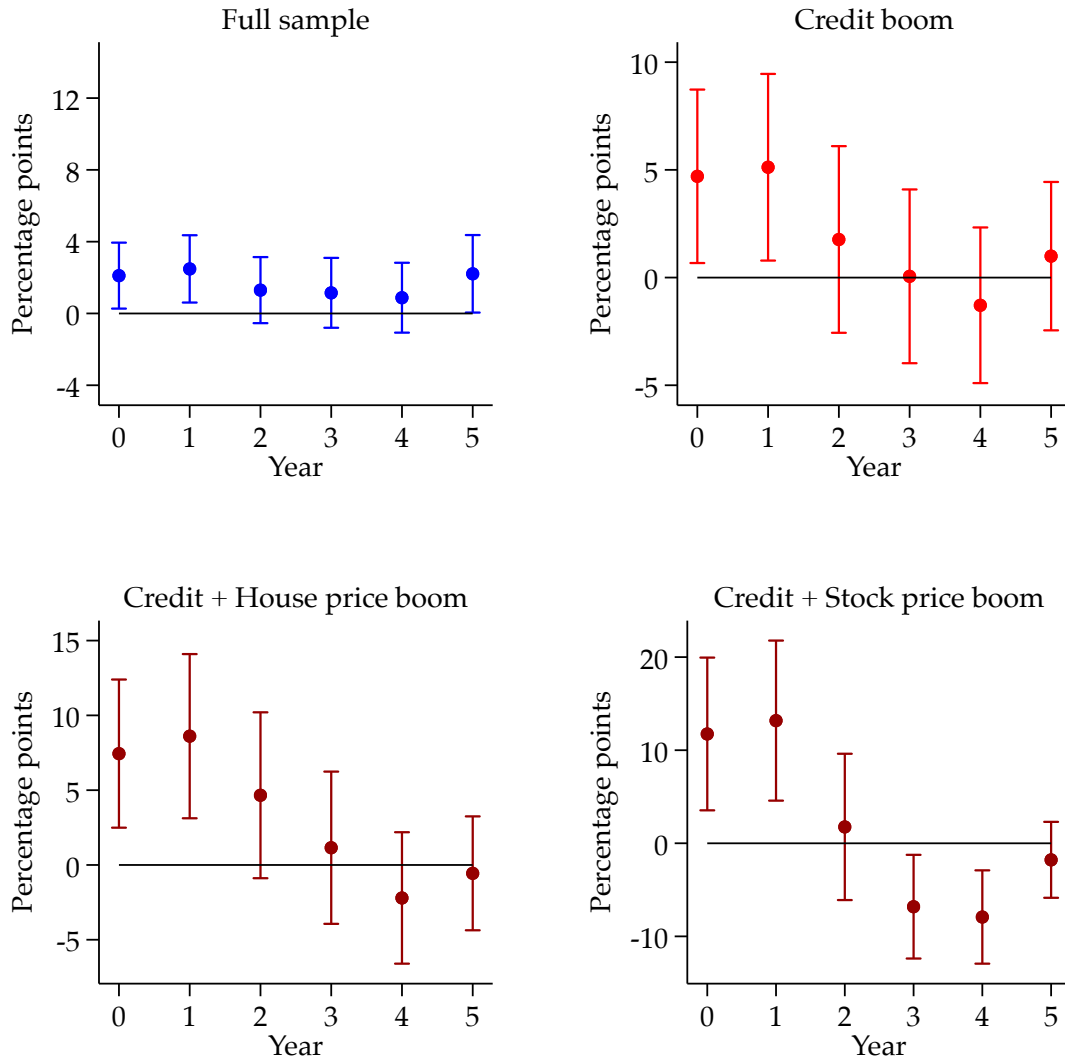Figure C.8: Crisis risk response—Pre-World War II subsample
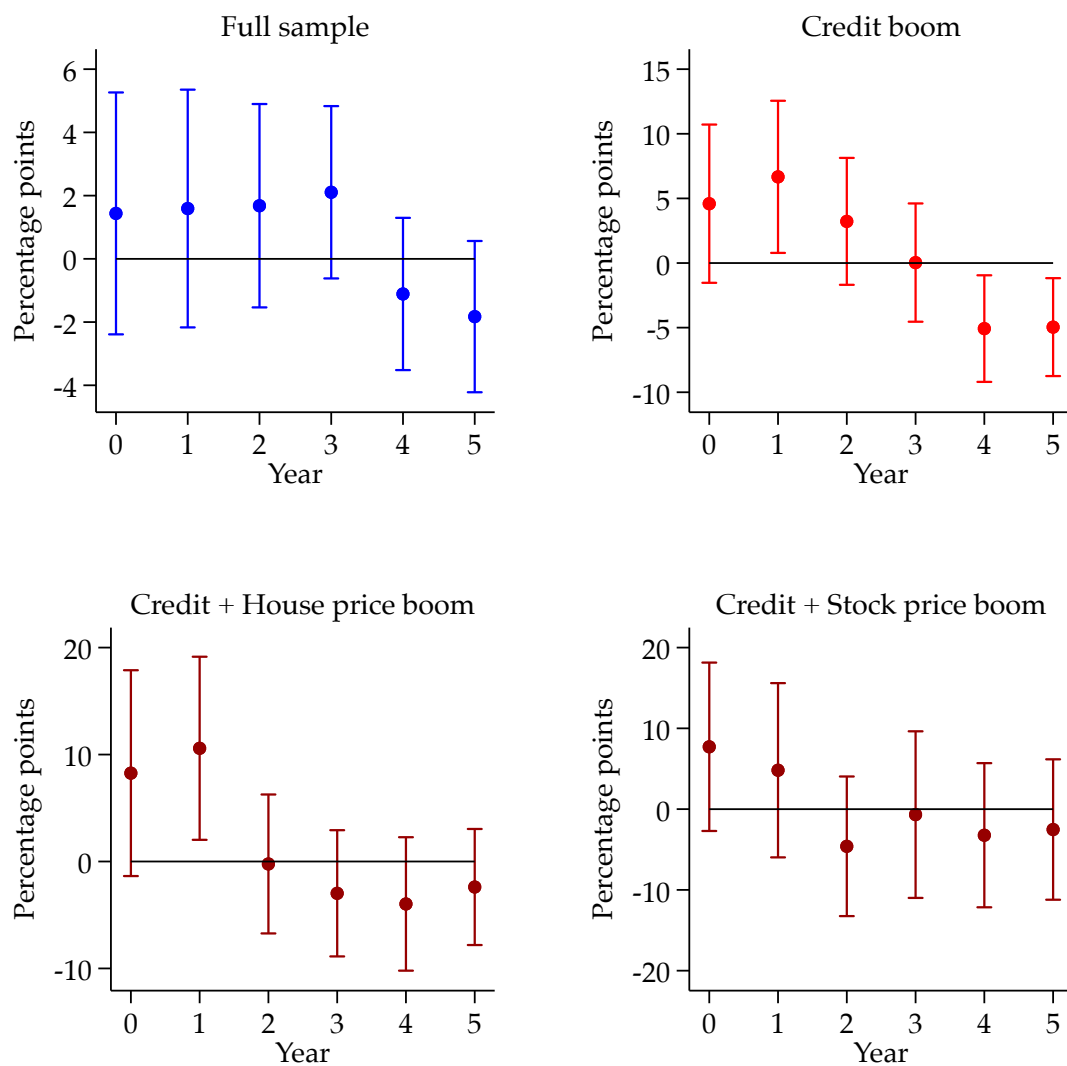
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. The sample includes data from 1870 to 1939.

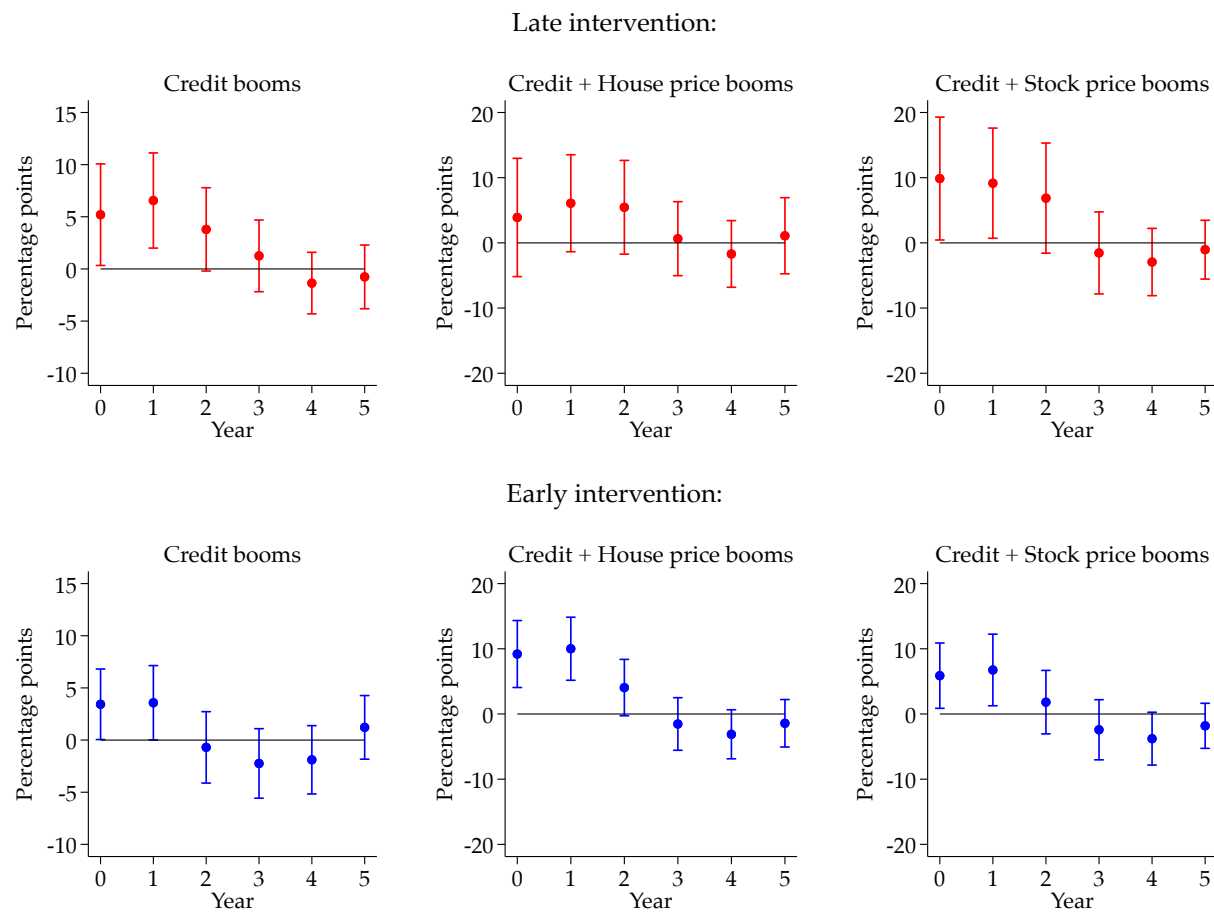Figure C.9: Crisis risk response—Post-World War II subsample
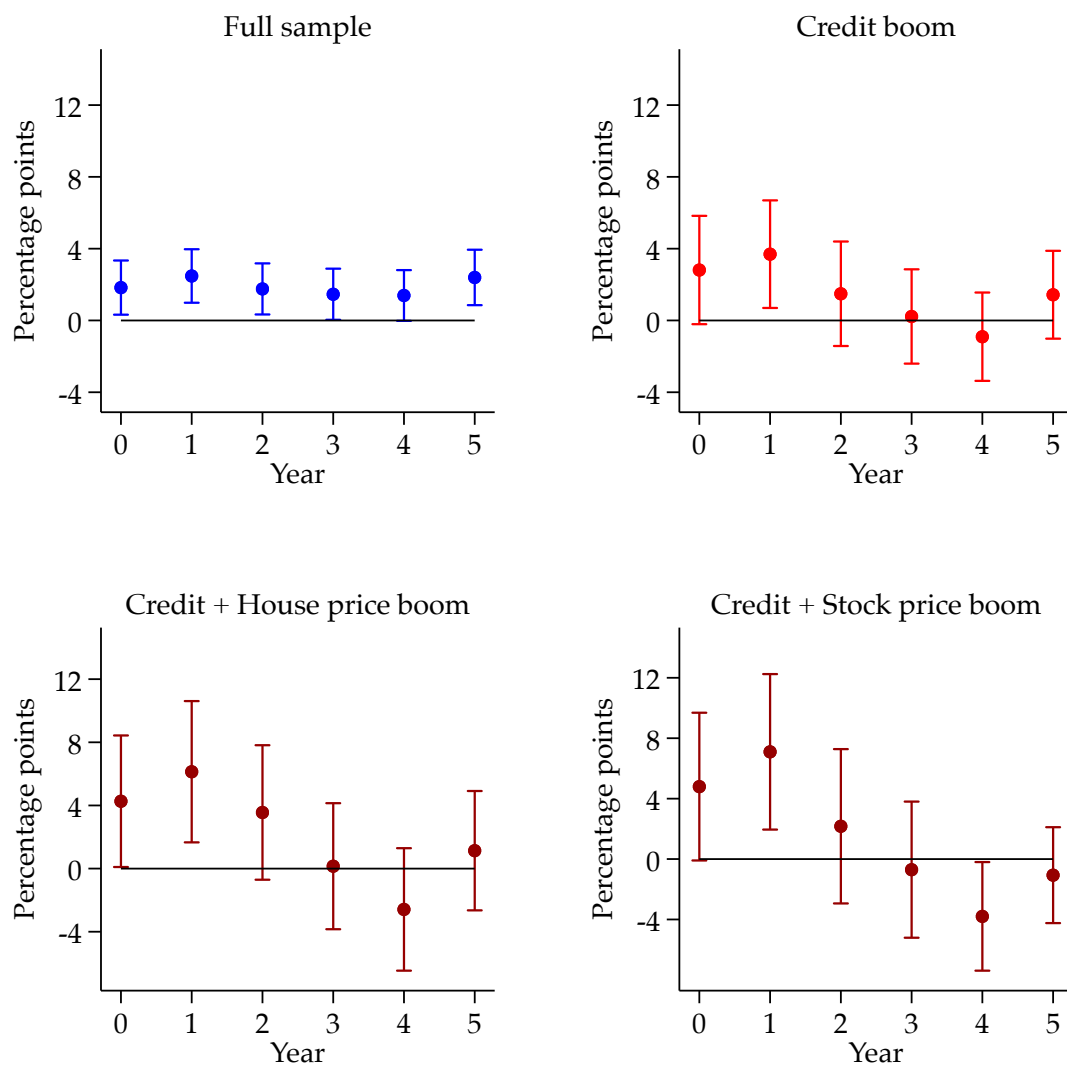
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. The sample includes data from 1947 to 2016.

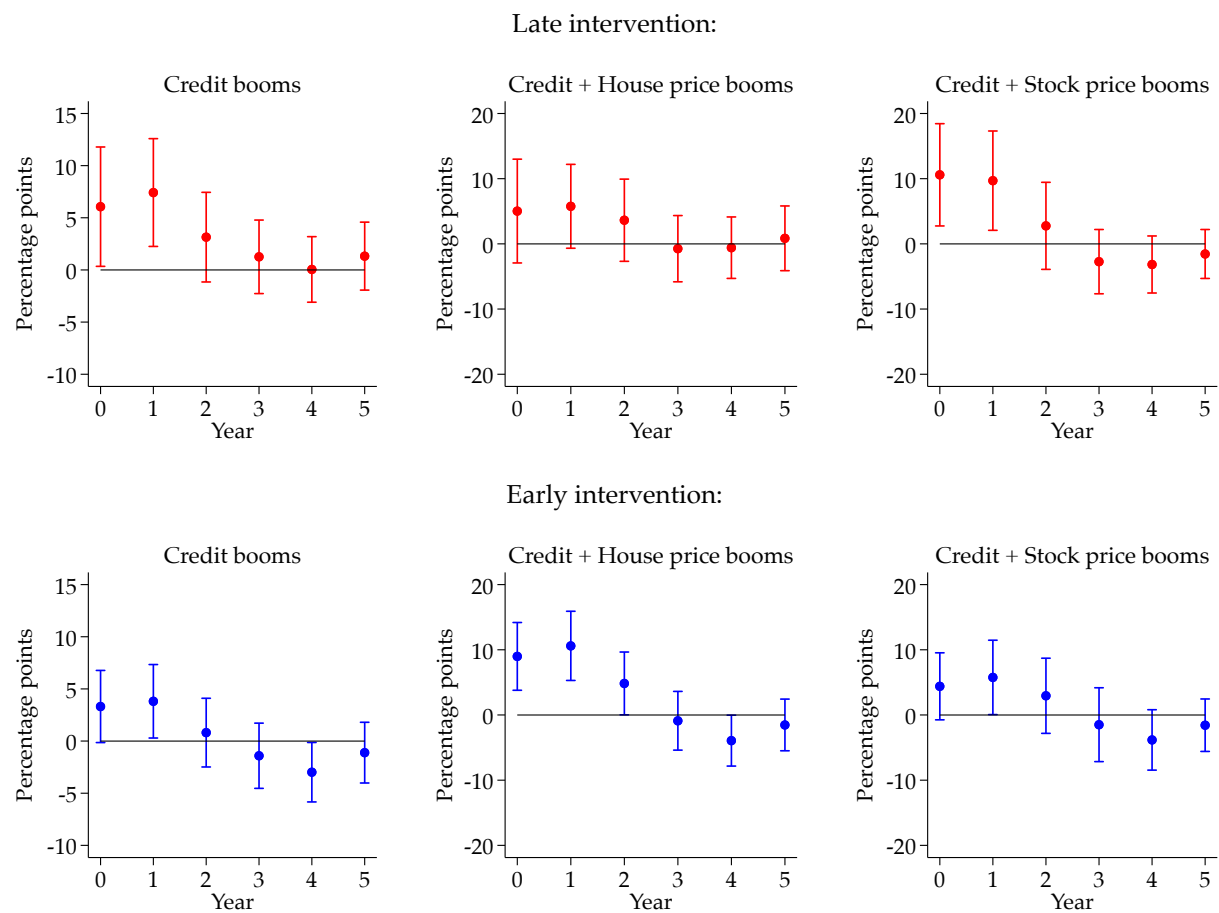Figure C.10: Crisis risk response—Post-Bretton Woods subsample

Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. The sample includes data from 1973 to 2016.

Figure C.11: Early versus late interventions and crisis risk—First two boom years

Late intervention:

Early intervention:

Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first two years of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms.

Figure C.12: Early versus late interventions and crisis risk—First versus second boom halves

Late intervention:

Early intervention:

Notes: The figure shows the annual crisis probability effects of a 1 ppt policy rate increase that occurs late during a financial boom (top panels) and early during a financial boom (bottom panels). An early intervention is defined as occurring during the first half of a boom. A late intervention is defined as occurring during any of the remaining boom years. Solid bars depict pointwise 95% confidence intervals. All panels depict state-dependent effects during different credit and asset price booms.

Figure C.13: Crisis risk response—Spillover correction
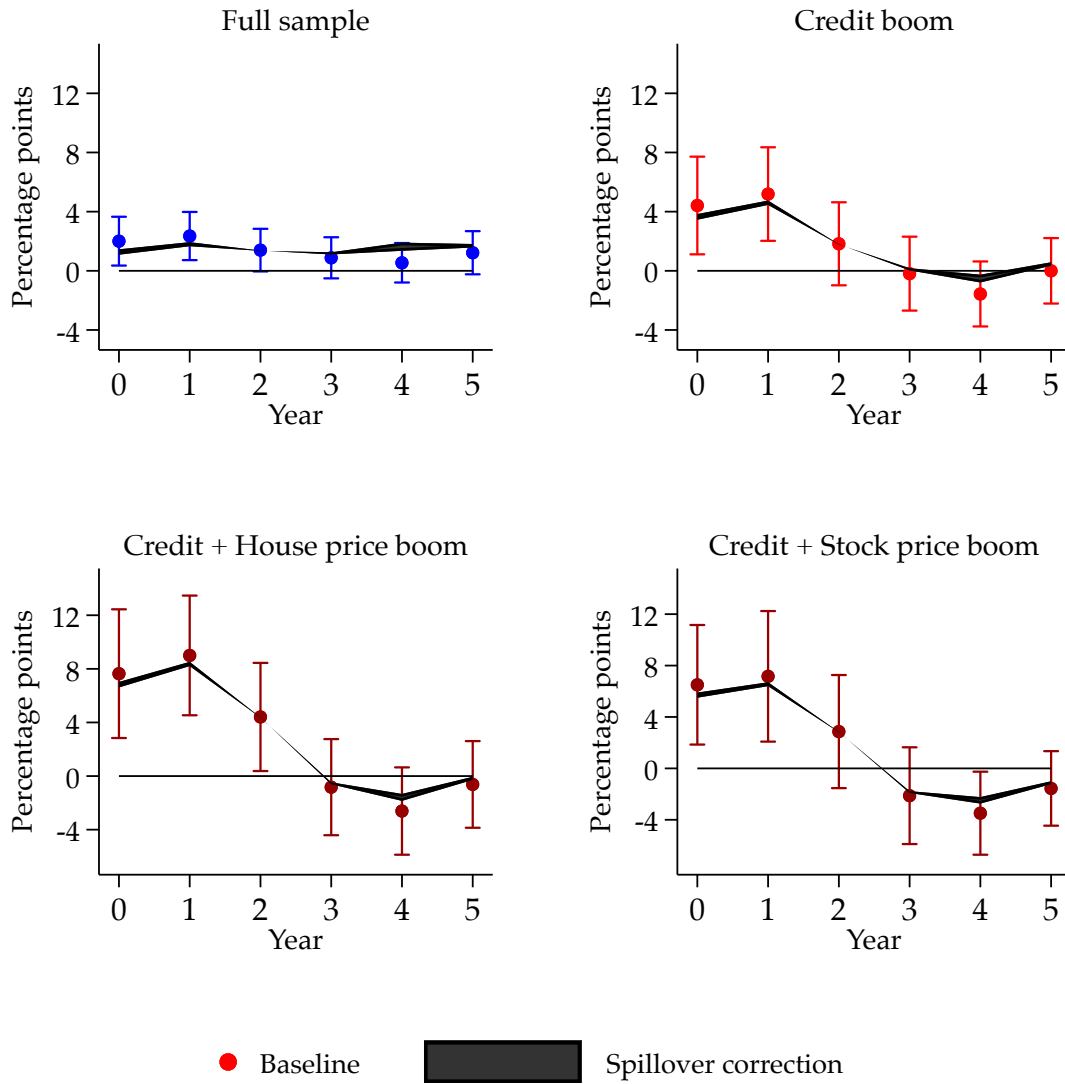
Notes: The figure shows the annual crisis probability effect of a 1 ppt policy rate increase. Solid bars depict pointwise 95% confidence intervals. The "Full sample" panel depicts the non-state-dependent effect. The other three panels depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100. Black area indicates range for spillover-corrected mean IRF estimates.

D. ADDITIONAL RESULTS

Figure D.1 displays the own response of the short term interest rate to a +1ppt interest rate innovation. The interest rate shock that drives our main results exhibits a hump-shaped pattern that peaks one year after the innovation occurs. After that, the shock slowly decays. The interest rate shocks that occur during credit and asset price booms exhibit less persistence, which makes their larger effect on crisis risk even more notable.

Figure D.1: Interest rate own-response
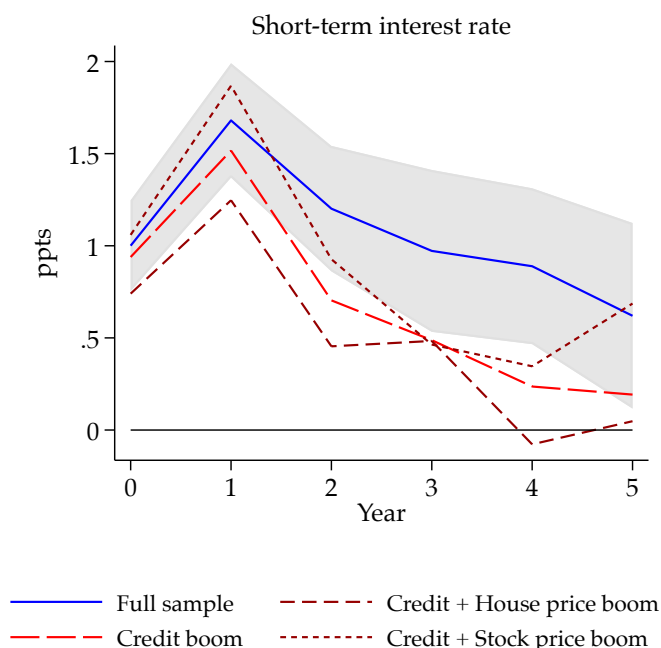
Notes: The figure shows the own-response of the short-term interest rate to a 1 ppt policy rate innovation. The grey area depicts pointwise 95% confidence bands. The "Full sample" line depicts the non-state-dependent effect. The three dashed lines depict the state-dependent effects during credit and asset price booms. Boom episodes are defined on the basis of a one-sided HP filter with smoothing parameter equal to 100.series (λ=100).

REFERENCES

Baron, Matthew, Emil Verner, and Wei Xiong. 2018. "Identifying Banking Crises." Working Paper.

Christiano, Lawrence J., and Terry J. Fitzgerald. 2003. "The band pass filter." *International Economic Review*, 44(2): 435–465.

Hamilton, James D. 2018. "Why you should never use the Hodrick-Prescott filter." *Review of Economics and Statistics*, 100(5): 831–843.

Jordà, Òscar, Moritz Schularick, Alan M. Taylor, and Felix Ward. 2019. "Global financial cycles and risk premiums." *IMF Economic Review*, 67(1): 109–150.

Jordà, Òscar, Moritz Schularick, and Alan M. Taylor. 2019. "The effects of quasi-random monetary experiments." *Journal of Monetary Economics*, 112: 22–40.

Reinhart, Carmen M., and Kenneth S. Rogoff. 2011. "From financial crash to debt crisis." *American Economic Review*, 101(5): 1676–1706.

Schularick, Moritz, and Alan M. Taylor. 2012. "Credit booms gone bust: Monetary policy, leverage cycles and financial crises, 1870–2008." *American Economic Review*, 102(2): 1029–1061.

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

Sur, Pragya, and Emmanuel J Candès. 2019. "A modern maximum-likelihood theory for high-dimensional logistic regression." *Proceedings of the National Academy of Sciences*, 116(29): 14516–14525.