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Identifying structural VARs from sparse narrative instruments: dynamic effects of U.S. macroprudential policies

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Abstract

We study identification in Bayesian proxy VARs for instruments that consist of sparse qualitative observations indicating the signs of shocks in specific periods. We propose the Fisher discriminant regression and a non-parametric sign concordance criterion as two alternative methods for achieving correct inference in this case. The former represents a minor deviation from a standard proxy VAR, whereas the non-parametric approach builds on set identification. Our application to U.S. macroprudential policies finds persistent declines in credit volumes and house prices together with moderate declines in GDP and inflation and a widening of corporate bond spreads after a tightening of capital requirements or mortgage underwriting standards.

JEL classification: C32, E44, G38.

Keywords: Bayesian Proxy VAR, Discriminant Analysis, Sign Concordance, Capital Requirements, Mortgage Underwriting Standards.

Non-technical summary

Since the 2008 Global Financial Crisis policy-makers have developed new macroprudential regulatory policies, targeted at dampening cyclical fluctuations in credit and house prices. Studies assessing the effectiveness of the related policy instruments must rely on historical, qualitative data about the timing and direction of related supervisory interventions. Typically, these data are included as regressors in cross-country panel regressions to assess the effects of interventions on credit volumes and house prices. Studies thereby remain silent on issues such as transmission lags of policy interventions and their cost in terms of GDP and inflation.

In this paper, we explore the dynamic effects of macroprudential policy interventions from proxy vector autoregressions (VARs). Proxy VARs model the joint dynamics of the series of interest and identify the dynamic effects of policy interventions from using a respective indicator as an instrument. We provide a methodological contribution by adapting the proxy VAR approach to the case of sparse qualitative indicators, as faced with macroprudential policies, based on linear discriminant analysis and on the sign concordance of shocks with the indicators. A simulation study shows that the combination of the two criteria provides more accurate confidence bounds than existing versions of the proxy VAR approach and is more robust to observation errors.

We then study the effects of U.S. policy interventions related to capital requirements and mortgage underwriting standards over the period of 1956 to 2016. We find highly persistent effects of both types of policy interventions on credit volumes and less persistent and more moderate effects on GDP, inflation, and corporate bond spreads. Shocks to capital requirements impact on credit to non-financial corporations, while household credit and house prices remain unaffected, reflecting a shift towards lower risk weights in bank credit portfolios. By contrast, mortgage underwriting standards affect both types of credit and have a pronounced impact on house prices.

These results point to long lags in the transmission of macroprudential policies indicating a need for rule-based forward-looking policies. They also suggest that static panel regressions may underestimate the effects of macroprudential policies.

1 Introduction

Proxy variables and narrative data have been widely used in recent years for identifying policy innovations in vector autoregressive models (VARs). The common principle is to employ them as external information when extracting the innovations from the VAR forecast errors. Optimally, a quantitative proxy variable for the innovation is at hand. For instance, studies have identified monetary policy shocks from high-frequency financial market data indicating the news content of monetary policy communication (Gertler and Karadi, 2015). In other policy areas instruments yet often relate only to a small number of events or are of a qualitative nature. These limitations have been taken up by a narrative approach arguing that qualitative information on just a few events achieves robust identification. For U.S. monetary policy, Antolin-Diaz and Rubio-Ramirez (2018) show that restrictions on the sign and size of a single event in October 1979 add important information to identifying policy innovations in combination with sign restrictions on impulse responses.[1]

In this paper, we study the intermediate case of a sparse binary instrument with a possibly larger number of events. We aim to adapt existing Bayesian methods to this case and thereby to bridge the gap between proxy and narrative Bayesian VARs. Following Antolin-Diaz and Rubio-Ramirez (2018), we assume that the econometrician knows no more than the signs of innovations for a limited number of events. However, we also allow for errors in the econometrician's beliefs, a likely incident unless the number of events is very small. We therefore impose sign restrictions on the expected values of those innovations. We explore two estimation methods that provide correct inference for this type of restriction. First, we show that the Bayesian proxy VAR can be adapted to a binary instrument by using a discriminant (DC) regression at the identification step. Second, we augment the narrative sign restrictions of Antolin-Diaz and Rubio-Ramirez (2018) with a prior on the degree of sign concordance (SC) to cope with imperfect sign concordance between innovations and the instrument.[2]

[1]See also Caldara and Herbst (2019) and Jarocinski and Karadi (2019) for monetary policy and Mertens and Ravn (2013) and Mertens and Montiel Olea (2018) for fiscal policy applications of proxy VARs. Ludvigson, Ma, and Ng (2017) and Ben Zeev (2018) present other applications of the narrative approach.

[2]As discussed in section 2, estimation amounts to a purely binary classification problem. We therefore denote the instrument as binary, although it may take values of 0, +1, and -1.

These extensions may be useful in Bayesian applications where policy interventions are infrequent and difficult to quantify, as is the case with various types of regulatory, fiscal, and structural policies. While binary instruments may appear less than ideal, our Monte Carlo simulations show that actual efficiency losses are small when moving from quantitative to binary information. We also find that estimates from a standard Bayesian proxy VAR are similar to those from the DC regression, as the latter represents a minor modification of the standard approach. Hence, the standard approach appears to work fairly well in practice. The SC prior is less efficient than the DC regression in case of errors in the instrument but useful as a reliability prior in combination with the latter. Among frequentist methods, our Monte Carlo simulations find that available bootstrap methods for proxy VARs tend to overestimate uncertainty bands in the case of sparse binary instruments, whereas local projections, which use the instrument as a regressor, are rather inefficient.

In our empirical application, we study the effects of macroprudential policies in the post-war U.S. A large part of the macro-econometric literature on macroprudential policies relies on binary narrative indicators, as the high diversity of policy interventions impedes the construction of quantitative measures. Due to these limitations, studies typically focus on estimating the short-run responses of credit volumes and house prices from panel regressions (see Galati and Moessner, 2017). Our results add to a sporadic literature on the broader macroeconomic dynamics triggered by policy interventions. Building on a narrative dataset of Elliot, Feldberg, and Lehnert (2013), we focus on capital requirements and mortgage underwriting standards. For both types of policies, we find large and highly persistent declines in credit volumes and house prices after tightening measures, together with moderate declines in GDP and inflation and a temporary widening of corporate bond spreads. The long transmission lags suggest that panel regressions focusing on the short run may underestimate the total effects of these policies. Our findings also relate to the literature on credit supply shocks (Gilchrist and Zakrajsek, 2012) and government mortgage purchases (Fieldhouse, Mertens, and Ravn, 2018), and underpin the role of collateral constraints in generating the highly persistent leverage cycles found by Claessens, Kose, and Terrones (2012) and Rünstler and Vlekke (2018).

The remainder of the paper is organised as follows. Section 2 introduces our two identification schemes for binary instruments. Sections 3 and 4 present the Monte Carlo simulation exercise and the application to U.S. macroprudential policies, respectively. Section 5 concludes.

2 A Bayesian VAR for Sparse Binary Instruments

Consider the reduced-form VAR for $n \times 1$ vector x_t over periods $t = 1, \ldots, T$,

$$x_t = c + \sum_{s=1}^{r} B_s x_{t-s} + u_t, \tag{1}$$

where c is a constant term, and the VAR residuals u_t are independently distributed over time. The key assumption of the model is that residuals u_t have a structural representation that isolates a certain scalar innovation θ_t of interest as the first element of a vector of mutually independent innovations such that

$$A_0 u_t = \begin{pmatrix} \theta_t \\ \epsilon_t \end{pmatrix} \tag{2}$$

Denote with α^T the first row of matrix A_0, implying $\alpha^T u_t = \theta_t$. Once α is known, the dynamic response of x_t to innovation θ_t can be obtained. Proxy and narrative VARs aim at identifying α from outside information about realisations of θ_t. This information may take different forms requiring different statistical methods for estimating α. Proxy VARs, as introduced by Mertens and Ravn (2013), rely on a quantitative instrument for innovations θ_t, whereas narrative sign restrictions, as proposed by Antolin-Diaz and Rubio-Ramirez (2018), impose restrictions on the sign of θ_t for specific events.

In this paper, we consider a weaker version of narrative sign restrictions. Instead of imposing them directly on innovations θ_t, we apply them to the expected values of the latter. We thus assume that the econometrician knows about the presence of a mean shift in innovations θ_t for a set of $m < T$ specific events but is ignorant about its size. This assumption defines an instrument z_t that takes values of $z_t = \text{sign}(\mathbb{E}\,\theta_t)$ for events and $z_t = 0$ otherwise. We impose the moment conditions

$$\begin{aligned} \mathbb{E}(\theta_t|z_t) &= \gamma z_t \\ \mathbb{E}(\epsilon_t|z_t) &= 0. \end{aligned} \tag{3}$$

The conditions imply a shift of size γ in the expected value $\mathbb{E}(\theta_t|z_t)$ for observations with a non-zero value of z_t, whereas the remaining innovations ϵ_t are required to be independent of z_t. As a result, instrument z_t acts just like a treatment effect, identifying vector α from infrequent mean shifts in the conditional distribution of θ_t.

Conditions (3) are a straightforward adaptation of the proxy VAR moment conditions of Stock and Watson (2018) to the case of a sparse binary instrument (see section 2.4).[3] At the same time, they may be seen as a weak version of the narrative sign restrictions proposed by Antolin-Diaz and Rubio-Ramirez (2018), applied to the expected value of innovations. For an individual event, the sign restriction would therefore hold with a certain probability, depending on distributional assumptions and the value of γ.

Stock and Watson (2018) discuss the requirements for the identifiability of α in the general context of proxy VARs. First, the relevance condition $\gamma > 0$ ensures that instrument z_t picks up events that generate relevant innovations. Second, the exogeneity condition $\mathbb{E}(\epsilon_t|z_t) = 0$ requires that events included in z_t are independent of other contemporaneous shocks to the system. Third, while the exogeneity condition is considerably weaker than the requirement of lag exogeneity in regression-based approaches, the latter is replaced by an invertibility condition requiring that innovations θ_t are fully spanned by the VAR residuals, such that matrix A_0 is invertible. In practice, the validity of the invertibility condition is determined by the selection of variables x_t included in the VAR (Mertens and Montiel Olea, 2018).

The purpose of this section is to present two estimation methods that achieve correct Bayesian inference with a sparse binary instrument. As we will discuss in section 2.4, the infrequent shifts in the conditional mean of θ_t generate non-standard elements

[3] We differ from Stock and Watson (2018) and the previous proxy VAR literature by specifying conditions (3) in terms of conditional expectations rather than in terms of covariances. Annex B.2 shows that the two specifications are equivalent in the case of a binary instrument.

in either the conditional or the unconditional distribution of the VAR residuals and thereby violate the assumptions underlying the standard proxy VAR. Frequentist proxy VARs cope with such a feature by using bootstrap methods for inference. However, a Bayesian approach requires more fundamental adaptations. In particular, estimation of α requires classification methods that exploit the infrequent mean shifts.

Section 2.1 presents a discriminant (DC) regression, which is based on the assumption that the VAR residuals are normally distributed conditional on instrument z_t. Section 2.2 augments the narrative sign restrictions of Antolin-Diaz and Rubio-Ramirez (2018) with a prior on the degree of sign concordance (SC) to adapt the restrictions to conditions (3). The SC prior does not require specific distributional assumptions. Section 2.3 presents a Gibbs sampler to estimate the VAR. Section 2.4 compares these two methods with the standard approach. We denote the information set with $X = (x_t)_{t=1}^T$ and $Z = (z_t)_{t=1}^T$ and set $B_+ = (c, B_1, \ldots, B_p)$.

2.1 Fisher Discriminant Regression

One way to estimate α is the discriminant (DC) regression due to Fisher (1931), which is reviewed in Maddala (2013:18ff). The DC regression is designed to predict binary observations z_t from a set of explanatory variables, which are normally distributed conditional on z_t. We therefore combine equations (1) and (2) with the assumption that the structural innovations are normally distributed conditional on z_t with $n \times n$ identity covariance matrix I_n,

$$\begin{pmatrix} \theta_t \\ \epsilon_t \end{pmatrix} | z_t \sim N\left(\begin{pmatrix} \gamma z_t \\ 0 \end{pmatrix}, I_n \right). \tag{4}$$

Note that assumption (4) implies a non-standard unconditional distribution of the VAR residuals u_t, which emerges as a mixture of normal distributions with different means. We will address the implications of this feature for estimating the reduced form VAR in section 2.3. Here, we focus on estimating parameter vector α given the VAR residuals u_t.

Since the cases of $z_t = +1$ and $z_t = -1$ are symmetric, our classification problem can be transformed into a purely binary one by abstracting from the sign of z_t. Let $\delta_t = -1$ if $z_t = -1$ and $\delta_t = 1$ otherwise and define $z_t^* = \delta_t z_t$ and $u_t^* = \delta_t u_t$. Hence, z_t^* takes the values $z_t^* = 1$ for $z_t \neq 0$ and $z_t^* = 0$ otherwise. This implies

$$\begin{aligned} u_t^* &\sim N(\mu^*, \Sigma) \quad \text{for} \quad z_t^* = 1 \\ u_t^* &\sim N(\,0\,, \Sigma) \quad \text{for} \quad z_t^* = 0 \end{aligned} \tag{5}$$

Under equations (5), the DC regression

$$z_t^* = a_0 + a^T u_t^* + \xi_t \tag{6}$$

provides an efficient estimate of α to predict z_t^* from u_t^* based on the rule $\hat{z}_t^* = 1$ if $\alpha^T u_t^* > m/T$ and $\hat{z}_t^* = 0$ otherwise. Despite the non-standard distribution of ξ_t, the OLS estimate of a is subject to standard inference as the regression compares the means of two conditional normal distributions, maximising the squared mean difference between the two groups over the variance within groups (see Maddala, 2013:18ff). Assuming an uninformative prior for a and a Jeffrey prior for σ_ξ^2 gives $a|B_+, \sigma_\xi^2, X, Z \sim N(\hat{a}, \sigma_\xi^2 S_u^{-1})$ and $\sigma_\xi^2|B_+, X, Z \sim IG(\hat{\sigma}_\xi, T-n-1)$, where $\hat{a}$ and $\hat{\sigma}_\xi^2$ are the OLS estimates of equation (6) and $S_u = \Sigma_{t=1}^T u_t u_t^T$. Note that a determines α only up to scale. The rescaling step will be described in section 2.3.

The DC regression is a special case of discriminant analysis and a workhorse classification method for binary dependent variables. It is an efficient solution to our classification problem under an intuitive loss function. Specifically, under assumptions (4), the above classification rule implied by the DC regression minimizes the loss function $mC_1 = (T-m)C_0$, where C_i is the cost of misclassifying an observation with $z_t^* = i$. Hence, the cost of misclassification is inversely proportional to the number of observations in each category, imposing a high cost of misclassifying non-zero z_t under small m, which we regard as a desired feature.[4]

[4]Discriminant analysis refers to a general theory of classifying categorial observations from quantitative variables based on certain loss functions, see Annex B.1 for a brief review.

2.2 Sign Concordance Prior

A useful statistic for the relevance of instrument z_t is the sign concordance (SC) criterion φ, defined as the share of instances for which the signs of innovations θ_t coincide with z_t,

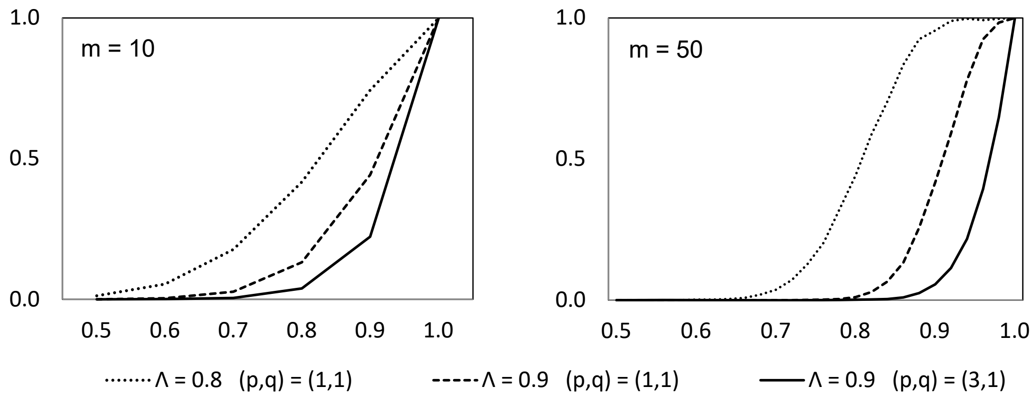
$$\varphi(B_+, \alpha, X, Z) = m^{-1} \sum_{z_t \neq 0} \mathbf{I}(\theta_t z_t > 0), \tag{7}$$

where $\mathbf{I}()$ denotes the indicator function. Given the independence of θ_t over time, the number of correct signs follows a binomial distribution,

$$p(m\varphi|\alpha, \lambda, B_+, X, Z) = f_z(m\varphi; m, \lambda), \tag{8}$$

where λ is the unknown probability of the correct classification of a single event.

Figure 1: Beta-Binomial Priors for Sign Concordance



The graphs show densities $f(\varphi; m, \Lambda, p, q)$ of beta-binomial distributions with the number of events set to $m = 10$ and $m = 50$. The beta distribution is defined over support $[\Lambda, 1]$ with parameters p and q.

In combination with an appropriate prior on λ that supports the acceptance of high values of φ, the SC statistics can be used as a non-parametric alternative to the DC regression for estimating α based on set identification. The principle is to obtain uninformative draws of α and to accept them with a certain probability depending on the value of φ. Clearly, the prior for λ should be chosen such that these probabilities increase with φ. A suitable option is a beta-distribution $\lambda \sim \beta(p, 1)$ over support $[\Lambda, 1]$ with $\Lambda > 0.5$. Figure 1 shows examples of the resulting beta-binomial prior density $f(\varphi; m, \Lambda, p, q)$ for φ, which defines the acceptance probabilities. The benchmark case of a uniform distribution, $p = q = 1$, creates acceptance weights with a smooth threshold at Λ. For values of $p > 1$ the prior becomes tighter.

One may generate more complex priors that allow the probability of correct classification λ to differ across individual events from a Poisson binomial distribution. The computational complexity of obtaining the density of this distribution increases yet rapidly with m. Efficient methods are provided by Chen and Liu (1997).

2.3 Sampling

The distributional assumptions (4) underlying the *DC* regression imply a non-standard unconditional distribution of the VAR residuals u_t in equation (1). Normality can yet be established conditional on mean shift γ by adding the impact of γ on the residuals as a deterministic term to the VAR. This allows for estimating the model via a Gibbs sampler that iterates between the reduced form VAR and the *DC* regression. We set a standard Normal-Wishart prior for parameters B_+ and Σ and uninformative priors for α and γ. Sampling proceeds as follows:

(1) Given B_+ draw from the conditional posterior $a|B_+, X, Z$ as described in section 2.1. Construct matrix A_0 such that $A_0 A_0^T = \Sigma$ and the first row of A_0 contains α^T. This can be achieved from a Gram-Schmidt orthogonalisation as in Arias et al. (2018). Let $A_0^T = A_* Q$, where A_* is the Choleski decomposition of Σ and $Q = (q_1, \ldots, q_n)$ is orthogonal, $QQ^T = I_n$. The first column q_1 of Q is found as $q_1 = A_*^{-1} a / ||A_*^{-1} a||$. The remaining columns $q_2, \ldots, q_n$ are constructed without further restrictions.

(2) Draw from the posteriors of $\gamma | B_+, \alpha, \sigma_\gamma^2 \sim N(\hat{\gamma}, m^{-1}\sigma_\gamma^2)$ and $\sigma_\gamma^2 | B_+, \alpha \sim IG(\hat{\sigma}_\gamma^2, m - 1)$, where $\hat{\gamma} = m^{-1} \sum_{t=1}^{T} \theta_t z_t$ is the sample mean of sign-adjusted innovations $\theta_t z_t$ and $\hat{\sigma}_\gamma^2$ is the corresponding sample variance. Obtain the impact of mean

shift γ on the VAR residuals as

$$\Gamma z_t = A_0^{-1} \begin{pmatrix} \gamma \\ 0 \end{pmatrix} z_t.$$

(3) Define $x_{Z,t} = x_t - \Gamma z_t$ and draw from the posterior of $B_+, \Sigma|\alpha, \gamma, X, Z$ based on the regression

$$x_{Z,t} = c + \sum_{s=1}^{r} B_s x_{t-s} + u_{Z,t}, \qquad u_{Z,t} \sim N(0, \Sigma)$$

Obtain the residuals $u_t = u_{Z,t} + \Gamma z_t$ of the original VAR (1).

In case of *SC* prior we follow Antolin-Diaz and Rubio-Ramirez (2018) in drawing from the posterior of a by rejection sampling. Step (1) from above is replaced as follows.

(1') Obtain an uninformative draw of α. Following Arias et al. (2018) we specify $\alpha = A_* q_1$, where q_1 is a draw from the Haar measure of orthogonal matrices. This is obtained as $q_1 = v/||v||$ from a random draw of vector $v \sim N(0, I_n)$. Construct the remaining columns $q_2, \ldots, q_n$ from a Gram-Schmidt orthogonalisation as in Arias et al. (2018).[5]

(1") Draw from the prior of λ and accept the draw with probability $f_z(m\varphi; m, \lambda)$.

Since the SC prior does not require specific distributional assumptions, it may be used with either assumption (4) or with an unconditional normal distribution of residuals, $u_t \sim N(0, \Sigma)$. In the latter case, mean adjustment Γz_t is ignored and the Gibbs sampler collapses to direct sampling of $B_+, \Sigma|X$. Finally, the *DC* regression may be combined with the *SC* prior by drawing α from the former and adding the rejection sampling step (1") after step (1).

[5] The Haar measure has been subject to controversy. Giacomini et al. (2021) propose robust priors as an alternative. Inoue and Kilian (2020) yet argue that concerns about the Haar measure have been overstated.

2.4 Comparison with Existing Approaches

We regard the *DC* regression as an adaption of a standard proxy VAR to the case of a sparse binary instrument. Proxy VARs are defined as in equations (1) and (2) with standard distributional assumptions on the VAR residuals. They achieve identification from the orthogonality conditions $\mathbb{E}\epsilon_t z_t = 0$ together with the relevance condition $\mathbb{E}\theta_t z_t > 0$. An estimate of α is obtained from the proxy regression $z_t = a^T u_t + \xi_t$. From rewriting the *DC* regression (6) as

$$z_t = a_0 \delta_t + a^T u_t + \delta_t \xi_t$$

it is apparent that *DC* and proxy regressions algebraically differ only by the deterministic term $a_0 \delta_t$, which corrects for the mean shifts in conditional distributions.[6]

The key difference between the two regressions is the distributional assumptions on residual ξ_t. Bayesian proxy VARs so far have maintained that ξ_t follows a standard normal distribution (Caldara and Herbst, 2019; Giacomini et al. 2021).[7] Clearly, a binary dependent variable creates a fundamental departure from this assumption, which invalidates inference (see e.g. Maddala, 2013). Under conditional normality the *DC* regression (6) provides correct inference.

The *SC* prior is a generalisation of the narrative sign restrictions used in earlier studies (Antolin-Diaz and Rubio-Ramirez, 2018; Ludvigson et al., 2017; Ben Zeev, 2018). These papers assume perfect sign concordance. Their restrictions are therefore a special case of the *SC* prior with $\lambda = 1$. Once the number of events increases beyond what has been used in these studies, the assumption of $\lambda = 1$ may yet turn overly tight. By allowing for errors in the econometrician's beliefs the *SC* prior makes this type of restriction suitable for larger numbers. As a non-parametric method, the *SC* prior does not require any distributional assumptions and is based on set identification (see Arias et al., 2018). Hence, it may be combined with other types of restrictions on innovation θ_t, such as sign restrictions on impulse responses as in Antolin-Diaz and Rubio-Ramirez (2018).[8]

[6] The alternative expression is obtained by multiplying equation (6) with δ_t, noting that $\delta_t^2 = 1$.

[7] Arias et al. (2021) take a different route implementing the moment conditions as deterministic restrictions.

[8] Technical details are discussed in Annex B.2. Since the *SC* criterion is non-parametric, it may

The DC regression and the SC prior may also be combined with each other. Caldara and Herbst (2019) highlight that in a Bayesian approach the instrument is informative for the posterior of the reduced form VAR, because it carries information about the VAR residuals in specific periods. They impose a reliability prior on the correlation of the instrument with innovations θ_t to use this feature for sharpening the reduced form VAR posterior. Similarly, the SC prior attains an interpretation as reliability prior when applied to draws of α from the DC regression, giving higher weight to draws that result in high values of sign concordance.

As shown in section 2.3, the assumption of conditional normality allows for estimating the VAR from a Gibbs sampler based on the conditional distribution. Alternatively, one may assume unconditional normality and use a classification method that does not require specific assumptions on the conditional distributions. One option is the non-parametric SC prior. Another, parametric, alternative to the DC regression is logistic regression. The latter has however several drawbacks in the application with proxy VARs. In particular, the efficiency of logistic regression is known to be impaired by a strongly imbalanced dependent variable, as it gives equal weight to both types of misclassification. This may, in extreme cases, lead to convergence issues with uninformative priors.[9]

Frequentist proxy VARs typically use bootstrap methods for estimating confidence bands. These methods should, in principle, be able to cope with a non-standard distribution of the proxy regression residual ξ_t. However, with a sparse instrument some care must be taken with choosing an appropriate bootstrap method. Jentsch and Lunsford (2016) show that standard bootstraps grossly underestimate confidence bands if instruments are sparse. They propose a modified block bootstrap for this case, whereas Montiel Olea, Stock, and Watson (2021) present bootstraps that are also suitable for weak instruments.

also be applied to other types of binary restrictions, such as those on relative magnitudes in the historical VAR decomposition used by Antolin-Diaz and Rubio-Ramirez (2018).

[9]The key difference between logistic and DC regression is that the former does not require distributional assumptions for the classifiers, whereas the latter assumes conditional normality (Efron, 1975). It is precisely this feature that makes the DC regression more efficient with imbalanced classifiers. A recent study on imbalanced classifiers is Li, Belotti, and Adams (2019). Albert and Anderson (1984), Allison (2008), and Gosh, Li, and Mitra (2018) study convergence issues.

Sparse quantitative instruments have been extensively used with frequentist proxy VARs. They are typically regarded as censored variables, limited to indicating large realisations of normally distributed innovations (see Mertens and Montiel Olea, 2018). Our assumption of infrequent mean shifts in the VAR residuals together with conditional normality is a departure from this view. This has raised the question whether the sparse instrument should be interpreted as signifying specific rare events. One may view the DC regression this way. We believe that it is also a valid way of looking at the data in applications related to rare economic events such as major regulatory changes or fiscal reforms. It is consistent with recent literature that models the residuals of macroeconomic VARs as mixtures of normal distributions and finds modestly heavy tails in the data, see e.g. Karlsson, Mazur, and Ngyuen (2021). In the end, however, the issue appears to make little difference for estimation. Our Monte Carlo simulations discussed in section 3, at least, show only small differences in outcomes between a standard Bayesian proxy VAR, which assumes unconditional normality, and the DC regression based conditional normality.

3 Sparse Policy Interventions: a Monte Carlo Study

This section presents Monte Carlo simulations to compare DC and SC restrictions with standard Bayesian and frequentist proxy VARs. We inspect the bias and uncertainty in the estimates of impulse responses (IRFs) together with the accuracy of uncertainty bounds.

The simulations extend upon the econometric framework of section 2 by assuming that innovations θ_t include a sparse set of policy interventions ζ_t, from which we generate the instrument. We thereby aim to replicate the dynamics of regulatory policies in a basic way. Our design also allows us to study cases where z_t is a contaminated measure of actual policy interventions with either irrelevant events being added to the instrument or relevant events missing from the latter. Such a trade-off is important in practice as the scope of events to be included in a sparse instrument is not well-defined: for instance, individual interventions may have been not binding or largely explained by past developments. One, therefore, faces a choice between including many events, some of which may be of limited relevance, or focusing on a small set of major events at the risk of missing relevant information. Mertens and Montiel Olea (2018) advocate the latter strategy.

We use the data generating process (DGP)

$$\begin{pmatrix} x_{t,1} \\ x_{t,2} \end{pmatrix} = B_1 \begin{pmatrix} x_{t-1,1} \\ x_{t-1,2} \end{pmatrix} + A_0^{-1} \begin{pmatrix} \eta_t + \zeta_t \\ \epsilon_t \end{pmatrix}, \tag{9}$$

where innovations $\theta_t = \eta_t + \zeta_t$ are the sum of regular innovations η_t and independent sparse policy interventions ζ_t. We let $(\eta_t, \epsilon_t)^T \sim N(0, 10^{-2} I_2)$. Sparse policy interventions ζ_t are generated by the policy rule

$$\begin{aligned} \zeta_t^* &= \omega x_{t-1,2} + \nu_t \\ \zeta_t &= -\mathbf{I}(\zeta_t^* \geq \bar{\zeta})\zeta_t^+. \end{aligned}$$

Interventions ζ_t arise both from exogenous shocks ν_t and the dependency of the policy target ζ_t^* on the past state of the system. The policy-maker intervenes only once ζ_t^* exceeds a certain threshold $\bar{\zeta}$. The absolute size ζ_t^+ of interventions is drawn from a lognormal distribution $\ln(\zeta_t^+/\bar{\zeta}) \sim N(-\sigma_\zeta^2/2, \sigma_\zeta^2)$ such that $\mathbb{E}\zeta_t^+ = \bar{\zeta}$ and $\mathrm{var}(\zeta_t^+) = \exp(\sigma_\zeta^2) - 1$. We set $T = 200$ and calibrate $\bar{\zeta}$ to achieve a number of interventions of either $m = 10$ or $m = 20$, while setting the dispersion of interventions to $\sigma_\zeta = 0.005$ or $\sigma_\zeta = 0.01$. We choose matrix B_1 to generate cyclical fluctuations with a length of 32 quarters in x_t and matrix A_0 to achieve a correlation of 0.3 among the VAR residuals together with a large initial response of $x_{1,t}$ to θ_t.[10]

We compare seven models. Two standard Bayesian proxy VARs serve as benchmarks. Model BV_ζ uses the true policy interventions ζ_t as an instrument, whereas model BV, like all remaining models, uses the qualitative instrument z_t. Both models apply a standard proxy regression, which assumes a normal distribution of residual ξ_t as described in section 2.4. We then consider the DC regression, a uniform SC prior for λ over interval $[0.9, 1]$, and the combination of the DC regression with the SC prior

[10] More precisely, we set $\bar{\zeta}$ to values of 0.0164 and 0.0128 for $m = 10$ and $m = 20$, respectively, and discard draws that that do not deliver the desired number of events. See Annex B.3 for further explanations.

(model DSC). In all these cases, we use an uninformative prior for the reduced-form VAR. Among frequentist models, we include a proxy VAR (pV) with uncertainty bands obtained from the bootstrap of Montiel Olea et al. (2021). Moreover, we inspect the widely used local projections (LP), which employ z_t directly as a regressor rather than as an instrument, with a standard bootstrap for uncertainty bands.[11]

The first four simulations shown in Table 1 ignore contamination issues in assuming that the econometrician observes $z_t = \text{sign}(\zeta_t)$. Note that the instrument nevertheless might differ from $\text{sign}(\theta_t)$ in certain periods due to innovations η_t. The table shows statistics on the standardised IRF of $x_{t,1}$ including the root mean squared error (RMSE) and the bias of the central estimate, the [0.1, 0.9] interquantile difference of its distribution as a measure of its true uncertainty, and the width of the corresponding estimated uncertainty bands. Moreover, Table 1 shows coverage ratios, defined as the share of draws where the true IRF lies within the estimated uncertainty bands. Annex C.1 plots the IRFs with true and estimated uncertainty bands.

We find, first, that the DC regression and the standard proxy VAR BV yield similar outcomes. Hence, assumption (4) of conditional normality underlying the DC regression has only modest effects, and the standard proxy VAR appears to work reasonably well in practice despite the violation of its distributional assumptions. The DC regression turns out slightly more efficient in all simulations. Both models modestly overestimate the width of the true uncertainty bands, while coverage ratios are fairly accurate. Second, the comparison of models BV_ζ and BV yields only modest efficiency losses from using z_t in place of the true policy intervention ζ_t. Losses are negligible for a value of $\sigma_\zeta = 0.005$, while they increase for a higher value of $\sigma_\zeta = 0.01$ in simulation (2) due to a higher share of small policy innovations ζ_t that are confounded by innovations η_t.

Third, the efficiency of the SC prior falls short of the DC regression, whereas the overestimation of uncertainty bands is more pronounced. The combination of the two methods, model DSC, tends to underestimate the width of uncertainty bands at horizon 0. At horizon 4, however, model DSC is as efficient as model DC and

[11] The models are described in more detail in Annex B.3. For models pV and LP, we build on the replication files of Mertens and Montiel Olea (2018).

Table 1: Monte Carlo Simulations

	m	100*σ_ζ		BV_ζ	BV	DC	DSC	SC	pV	LP	DC	DSC
							$h=0$					$h=4$
(1)	Baseline											
	10	0.5	RMSE	.13	.13	.13	.15	.19	.14	.30	.13	.13
			Bias	-.05	-.03	-.06	-.07	-.14	-.03	.01	-.10	-.10
			IQD	.40	.45	.43	.47	.57	.45	.96	.35	.35
			UB	.44	.51	.51	.39	.73	.72	1.13	.41	.34
			CR	.78	.77	.83	.74	.90	.86	.85	.72	.67
(2)	Baseline											
	10	1.0	RMSE	.14	.18	.17	.21	.27	.18	.35	.16	.17
			Bias	-.01	-.06	-.08	-.10	-.20	-.04	.01	-.12	-.13
			IQD	.41	.55	.52	.65	.81	.54	1.11	.41	.42
			UB	.41	.60	.60	.44	.80	.95	1.51	.48	.40
			CR	.73	.79	.81	.67	.83	.88	.87	.69	.63
(3)	Baseline											
	20	0.5	RMSE	.10	.11	.10	.11	.15	.11	.20	.10	.10
			Bias	.03	.02	-.02	-.03	-.08	.02	.02	-.06	-.06
			IQD	.30	.34	.31	.35	.46	.33	.65	.28	.30
			UB	.28	.33	.33	.28	.56	.41	.84	.31	.29
			CR	.74	.78	.82	.72	.90	.88	.88	.77	.75
(4)	Lagged dependency											
	10	0.5	RMSE	.14	.15	.15	.17	.21	.16	.31	.16	.16
			Bias	-.05	-.05	-.09	-.09	-.17	-.05	-.01	-.13	-.12
			IQD	.42	.47	.46	.52	.64	.48	.99	.35	.37
			UB	.52	.61	.56	.42	.76	.77	1.16	.45	.39
			CR	.84	.84	.83	.73	.88	.85	.84	.67	.69
(5)	Redundant events											
	10	0.5	RMSE			.23	.24	.27	.21	.46	.20	.20
			Bias			-.18	-.17	-.23	-.10	-.45	-.19	-.18
			IQD			.67	.75	.84	.62	.62	.45	.46
			UB			.86	.57	.88	1.03	.84	.63	.46
			CR			.85	.72	.85	.88	.47	.63	.61
(6)	Unobserved events											
	20	0.5	RMSE			.16	.17	.20	.17	.31	.14	.14
			Bias			-.07	-.07	-.14	-.04	-.07	-.11	-.10
			IQD			.50	.51	.60	.50	.95	.35	.36
			UB			.59	.44	.85	.80	1.13	.47	.38
			CR			.85	.74	.93	.89	.84	.75	.76

The table shows statistics of standardized IRFs at horizons $h = 0$ and $h = 4$. m is the number of interventions, while σ_ζ is the dispersion of policy shocks and ω is the weight of the lagged policy target in the policy rule (see equation (9)). $RMSE$ and $Bias$ are the root mean squared error of the estimate and its difference to the true IRF, respectively. IQD is the [0.1, 0.9] interquantile difference of the distribution of the central estimate measuring of its true uncertainty, while UB is the corresponding estimated uncertainty bands. CR stands for the coverage ratio, the share of draws where the true IRF lies within the estimated bands with a correct value of .80. The models and further simulation details, in particular simulations (5) and (6), are explained in the main text. We take 1000 draws.

uniformly provides more accurate estimates of uncertainty bands, as the information from the SC posterior feeds back into the posterior reduced-form VAR coefficients.

Fourth, the Bayesian approach outperforms frequentist models. While the central estimates from the frequentist proxy VAR are very similar to those from the DC regression by construction, the bootstrap of Montiel Olea et al. (2021) clearly overestimates uncertainty bands. We found this outcome also for the bootstrap of Jentsch and Lunsford (2016). Local projections, which include z_t directly as a regressor, are clearly less efficient than any of the above models. We note that our simulations ignore invertibility issues, which are an advantage of local projection methods compared to proxy VARs (Stock and Watson, 2018). The efficiency losses are yet to be considered when comparing the various methods.[12]

The final two simulations deviate from the specification $z_t = \text{sign}(\zeta_t)$ to study the implications of observation errors in instrument z_t. We consider the trade-off between the two possibilities that the econometrician either misses relevant interventions in instrument z_t or mistakenly includes redundant interventions. We take the perspective of an econometrician who faces 20 potential policy events but is ignorant about their relevance. Simulation (5) assumes that the econometrician mistakenly adds 10 redundant events to z_t, which do not correspond to policy shocks ζ_t: we generate $m = 10$ true events ζ_t and add another 10 random non-zero observations to z_t. Simulation (6) studies the case that the econometrician misses 10 relevant events: we generate $m = 20$ events ζ_t, but remove 10 of those events from z_t.

We find that redundant events create clearly larger distortions than missed events. The removal of 10 relevant events in simulation (6) creates only a modest increase in the RMSE compared to simulations (1) and (3). By contrast, keeping 10 irrelevant events as in simulation (5) almost doubles the RMSE, partly because of downward biases in the estimates. The SC prior improves somewhat relative to the DC regression in both cases. Model DSC turns out best, as it produces the most accurate estimates of uncertainty bands. Overall, the results suggest a conservative approach to constructing sparse instruments, while combining the DC regression with an SC prior seems to provide some insurance against observation errors.

[12]See Kilian and Kim (2011) and for a related study.

Annex B.3 shows further simulation results based on an alternative DGP that assumes unconditional normality of the VAR residuals. We set $\zeta_t \equiv 0$ and select the instrument among the larger realisations of η_t. The simulation design implies perfect sign concordance between the realisations of z_t and θ_t, and thereby generates a stronger instrument. We find the above results confirmed, yet with two exceptions. First, under perfect sign concordance, model SC based on a value of $\lambda = 1$ turns out even more efficient than the DC regression. However, uncertainty bands remain overestimated. Model DSC clearly performs best in this case, as it combines the higher efficiency of the SC prior with accurate uncertainty bands. Second, the bootstrap of Montiel Olea et al. (2021) now provides fairly accurate uncertainty bands for the frequentist proxy VAR.

4 Macroprudential Policy Interventions in the U.S.

We apply our approach to estimating the effects of policy interventions related to bank capital requirements and underwriting standards on mortgage credit in the postwar U.S. Capital requirements and borrower-based measures (of which underwriting standards are an important category) represent the most widely used macroprudential policy instruments since the 2008 Global Financial Crisis. The impact assessment of these policies is still impeded by the poor quality of data on policy interventions. Macroprudential policies have been carved out from supervisory and regulatory policies only after 2008. The historical interventions are scattered across time and cover highly diverse instruments. Accordingly, historical databases differ substantially in their coverage of policy interventions reflecting a fundamental ambiguity in the ex-post classification of individual interventions as being of a macroprudential nature (Budnik and Kleibl, 2018). Altogether, these difficulties inhibit the construction of quantitative time series of the policy stance.

The majority of studies on the macroeconomic effects of macroprudential policies, therefore, use binary indicators in cross-country panel regressions to assess the effects of interventions on credit volumes and house prices.[13] Galati and Moessner (2017)

[13] See, e.g. Vandenbussche et al. (2015), Cerutti, Claessens and Laeven (2017), and Budnik (2020).

note that these studies thereby remain silent on issues such as the persistence of these effects and transmission lags. In particular, given that macroprudential tightening reduces credit, it may have macroeconomic effects in terms of lower output and higher interest rates. Balancing these effects against financial stability benefits is key to the design of optimal policies and the coordination of macroprudential with monetary policies (Van der Ghote, 2021). The macroeconomic dynamics triggered by policy interventions has yet been addressed only by a few empirical studies. Richter, Schularick, and Shim (2019) and Pogoshyan (2020) use panel local projection methods to estimate the effects of borrower-based measures. These studies report declines in credit and house prices over horizons of four years. Richter et al. (2019) also study GDP and inflation. They find a decline in GDP after a tightening of loan-to-value ratios for emerging economies but no effect for advanced economies. At the same time, inflation tends to increase in both cases. Kim and Mehrotra (2017, 2018) include the count of policy events as an endogenous variable in a structural panel VAR. They identify macroprudential policy shocks from a Choleski decomposition assuming a zero contemporaneous response of these shocks to all other innovations apart from monetary policy. For a panel of 17 emerging and advanced economies, they find small negative effects on both GDP and inflation.

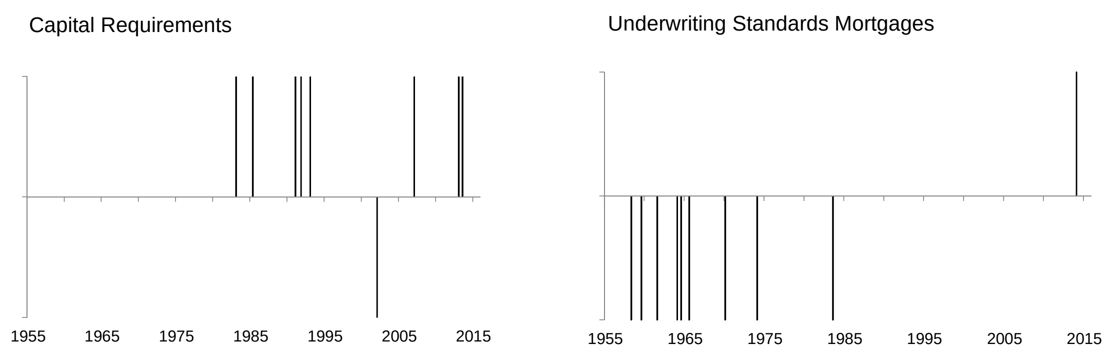
We study the macroeconomic effects of macroprudential policies for the U.S over the period of 1958Q1 to 2016Q4. Our VAR includes seven series: real GDP (y_t), consumer prices (p_t), the effective Federal Funds Rate (r_t), the spread between the rate of return on BAA corporate bonds and the 10-year Treasury Bond (r_t^C), real total credit to the non-financial corporate sector (c_t^P) and the household sector (c_t^H), and real residential property prices (h_t). With the exception of interest rates, the series enter the VAR as quarterly log-differences. The data are taken from the FRED database. For residential property prices, we use the Shiller U.S. national home price index. The credit data are from the BIS long credit statistics.[14]

[14] See https://fred.stlouisfed.org/ and https://www.bis.org/statistics/totcredit.htm.

4.1 The Narrative Instruments

The primary source of our information on capital requirements and mortgage underwriting standards is the database of Elliott, Feldberg, and Lehnert (2013), which contains a wide range of policy interventions addressing macro-financial risks in the U.S. between 1914 and the early 1990s. We augment the information provided by Elliot et al. (2013) until the end of 2016, adding interventions related to capital requirements introduced after the Basel Accords and Agreements based on various sources. For both types of policies, we define instrument z_t such that $z_t = -1$ in case an expansionary measure was set in period t, $z_t = 1$ in case of a contractionary measure, and $z_t = 0$ otherwise. This results in 10 events each for capital requirements and underwriting standards. The events are listed in Annex A.

Figure 2: The Narrative Indicators



Positive values indicate policy tightenings, while negative values indicate easings.

Although these policy interventions aimed at controlling macro-financial risks, they are still likely to represent contemporaneously exogenous shocks. As argued by Richter et al. (2019), a response of macroprudential authorities to macro-financial shocks within the same quarter is rather unlikely. Instead, authorities would respond at a certain point in time to imbalances that have accumulated over the past. Such delayed response is reinforced by the specificities of the U.S. institutional framework, as the responsibility for policy interventions has been distributed over different agencies, including the U.S. Congress. Policy actions typically required multiple consultations rendering the exact timing of policy actions less predictable (Elliot et al., 2013). Hence, condition $\mathbb{E}(\epsilon_t|z_t) = 0$ in equations (3) is very likely satisfied.

We also examine lagged dependencies of the indicators on the endogenous variables x_t included in the VAR from ordered probit regressions. Table 2 shows the results from likelihood ratio tests of the joint significance of coefficients related to each series. The regressions indicate lagged dependencies of the indicators on their respective main target variables only at higher lags. When including four lags, we find some predictive power of credit to households for capital requirements and the corporate bond spread and house prices for underwriting standards. However, these effects vanish if only the first two lags of x_t are considered. We remove one event from the capital requirements indicator that is correctly classified by the probit.

Table 2: Lagged Dependencies of the Instruments

Capital requirements	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
p = 2	.84	1.69	0.84	1.62	1.12	4.96	4.08
p = 4	4.88	6.02	5.38	6.93	7.16	**13.78	4.20
Underwriting standards	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
p = 2	1.00	1.65	*7.15	3.56	.02	1.70	1.66
p = 4	3.05	2.09	7.89	*9.51	.84	3.18	*10.03

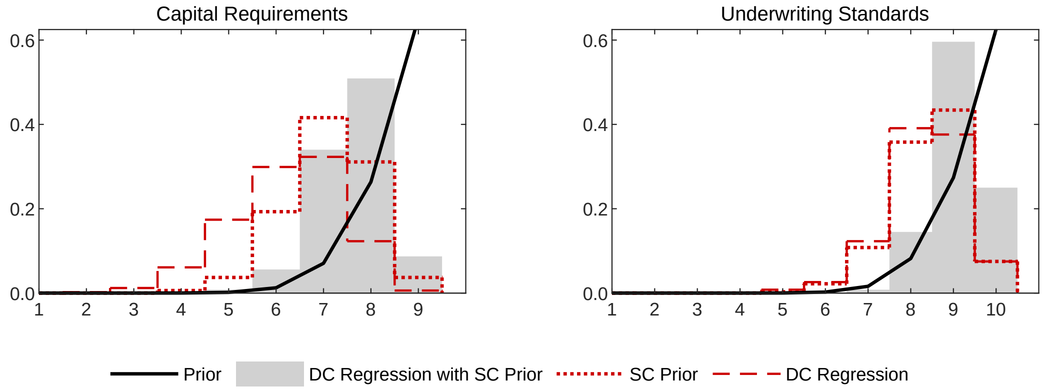
The table shows the LR statistics of $\beta_{j,1} = \ldots = \beta_{j,p} = 0$ from ordered probits regressing instrument z_t on the variables included in the VAR at lags 1 to p. Lags are set to either $p = 2$ or $p = 4$. The statistics are χ^2-distributed with 2 and 4 df, respectively. '*' and '**' indicate significance at 5% and 1% levels, respectively.

4.2 Impulse Responses to Policy Shocks

We turn to estimates of the impulse responses (IRFs) to macroprudential policy innovations from the narrative VAR. We consider three models, i.e. the *DC* regression, the *SC* prior, and the combination of the two criteria in model *DSC*, which gives the *SC* criterion an interpretation as reliability prior. We specify the prior for λ as a uniform distribution over support $[0.9, 1]$. We use the seven variables described above, include eight lags and impose a standard Minnesota prior on the reduced form VAR based on a standard Normal-Wishart prior for B_+ as described by Karlsson (2013).[15]

Figure 2 shows the sign concordance posteriors from the three models. For underwriting standards, the number of correctly classified events peaks at a value of 9 out of 10 events with little difference between DC and SC restrictions. For capital requirements, the DC restriction gives rise to a substantial share of draws with a low sign concordance of $\varphi < 0.5$ resulting in a median value of the SC posterior of below 0.7. In both cases, the combination of the DC regression with the SC prior acts to reduce the weight of draws with low φ shifting the SC posterior to the right compared to both the DC regression and SC prior used in isolation.

Figure 3: Sign Concordance Posterior Densities



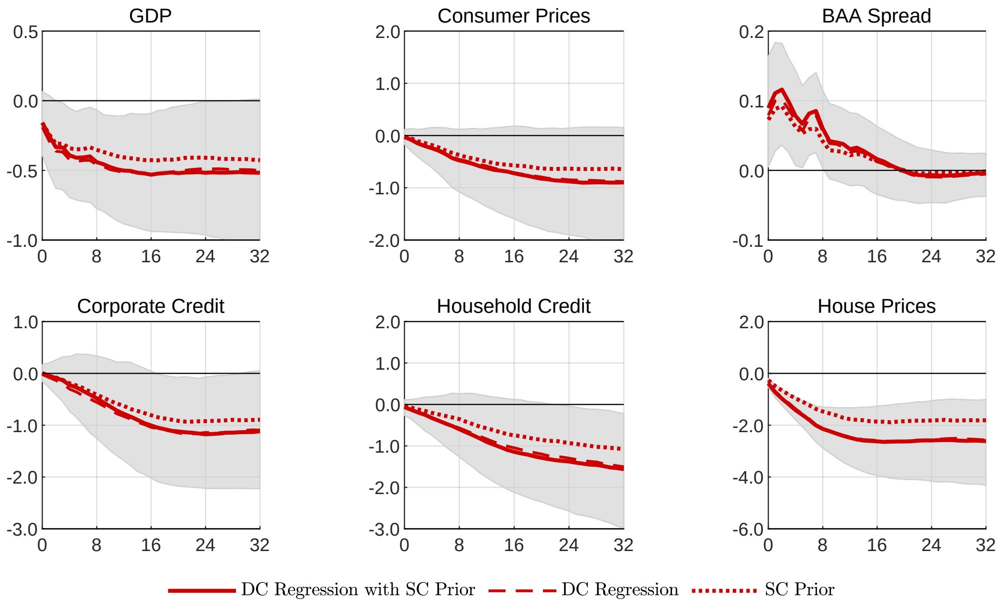
The shaded area shows the posterior density of the sign concordance statistics $m\varphi$ for model DSC. The lines show the same posterior density for models DC and SC and the sign concordance prior.

The impulse responses (IRFs) to a policy tightening in capital requirements and underwriting standards are shown in Figures 3 and 4. They are standardized to give the response to a shock of size $\theta_t = 1$. We show results for nominal residential property prices p_t^H. The IRF estimates turn out very similar across the three models. In line with the simulation results from section 3, estimates based on the SC prior

[15] We specify the prior variance of coefficient $B_{s,ij}$ as $\tau_{s,ij} = (\pi_0 * s_i^{(-\pi_3)})^2 s_j$, where s_i is the residual variance of an univariate autoregressions of series $y_{i,t}$. We set overall tightness $\pi_0 = 0.2$, lag decay $\pi_3 = 0.5$, and use a mean value of $B_{1,ii} = 0.3$ for the first own lag. For Σ, we use an inverse Wishart prior $IW(S, n + 2)$, where S is a diagonal matrix with elements s_i on the main diagonal. We take 1000 draws.

Figure 4: Standardized IRFs Capital Requirements

Figure 5: Standardized IRFs Underwriting Standards



The graphs show the median estimates of IRFs to a shock of 1 % from models *DSC*, *DC*, and *SC*, together with [0.10, 0.90] quantiles for model *DSC*.

tend to deliver somewhat smaller median responses and larger confidence bounds compared to models DC and DSC (Fig. Annex C.1).

For both measures, a policy tightening induces a persistent decline in credit, while the corporate bond spread is subject to a small but significant increase. At the same time, economic activity and inflation decline. The effects of the two types of policy measures differ in two ways. First, the impact of a change in capital requirements is concentrated on corporate credit while leaving household credit and house prices unaffected. By contrast, a change in underwriting standards affects both credit categories and results in a pronounced decline in house prices. Second, the impact of a change in capital requirements is less persistent. The response of corporate credit peaks after about two years, while the effect on economic activity reverses after four quarters. For underwriting standards, the responses of both series stabilize only after about 4 years and are highly persistent.

Table 3 presents the average effect of a policy intervention on the series included in the VAR as estimated from model DSC. For each draw, we multiply the IRF with the corresponding estimate of γ, which is obtained as the average of sign-adjusted innovations, $\hat{\gamma} = m^{-1} \sum_{t=1}^{T} \theta_t z_t$. We find a larger impact of underwriting standards compared to capital requirements. Interventions on underwriting standards on average, resulted in declines of corporate and household credit of 0.8% and 1.2%, respectively, after 32 quarters, while house prices dropped by close to 2.2%. Interventions on capital requirement induced a decline in corporate credit of 0.7% but had little effect on household credit and house prices. In both cases, GDP declined by about 0.3% after a year, while the corporate bond spread increased by close to 10 basis points.

Our estimates of the short-term response of credit and house prices are in line with the literature based on cross-country panel regressions. In a meta-analysis of these studies, Gadea-Rivas, Bräuer and Perez-Quiros (2019) find an average response of credit volumes of about 0.5% in advanced economies after a year. Pogoshyan (2020) reports similar outcomes for credit and house prices in the euro area based on local projection methods. Medium-term effects have so far only been addressed by Kim and Mehrotra (2017, 2018) for emerging economies. Our estimates for the U.S. indicate long transmission lags in the responses to borrower-based measures and moderate but significant declines in economic activity. Moreover, the weak response of household credit and housing prices to a change in capital requirements suggests that a shift towards a credit portfolio subject to lower risk weights is an important element in banks' responses to these policies.

Table 3: Average Impact of Policy Innovations

Capital requirements	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
h	4	4	4	1	32	32	32
0.1	- .47	-.37	-.37	.01	-1.31	-.96	-1.31
0.5	-.24	-.12	-.18	.06	-.58	-.12	-.16
0.9	-.02	.14	.01	.13	.06	.83	1.02
Underwriting standards	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
h	4	4	4	1	32	32	32
0.1	-.62	-.54	-.42	.02	-1.97	-2.58	- 3.91
0.5	-.33	-.20	-.19	.09	-.93	-1.31	-2.21
0.9	-.04	.12	.04	.17	.03	-.17	-.74

The table shows the median and 0.1 and 0.9 quantiles of responses to the average policy shock from model DSC at different horizons (quarters), as indicated in row h.

4.3 Robustness

Our first concern is the validity of the invertibility condition discussed in section 2. In the main estimates, the instrument is set to non-zero values in the periods when policy interventions entered into force. However, it cannot be ruled out that the effects of these interventions are only partly spanned by the VAR forecast errors in these periods. As discussed by Stock and Watson (2018), any effect of policy interventions not spanned by the forecast errors at implementation would necessarily materialise in forecast errors in subsequent periods.

With a binary instrument, the validity of the invertibility condition can therefore be explored by projecting the instrument on the VAR forecast errors subsequent to the period of implementation.[16] Table 4 reports the outcome of a related exercise based on

[16] The impact of an event Θ_t can be expressed as $\mathbb{P}(x_{t+h} - x_t|\Theta_t) = \sum_{s=0}^{h} C_i \mathbb{P}(u_{t+h-s}|\Theta_t)$, where $\mathbb{P}$ is the linear projections operator and $x_t = \sum_{s=0}^{\infty} C_s u_{t-s}$ is the moving average representation of the VAR. The invertibility condition $\mathbb{P}(\Theta_t|u_t) = \theta_t$ implies $\mathbb{P}(u_{t+h-i}|\theta_t) = 0$ for $i \neq h$.

model DC, where we set the instrument to non-zero values simultaneously for lags 1 to 4 after the implementation date. We thereby integrate over potential effects across the individual lags. We find minor further negative effects on household credit and house prices for both policy measures, but none of these would approach significance or alter our conclusions. We also experimented with setting the instrument to non-zero values at individual lags, using the SC prior as a selection criterion to attach larger weights to lags that generate further effects. This gave similar results. The IRFs from these exercises are shown in Figure C.4 in Annex C.

Table 4: Robustness Check against Lagged Impacts

Capital requirements	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
h	4	4	4	1	32	32	32
0.1	- .11	-.14	-.10	-.05	-.21	-.78	- .93
0.5	.00	-.01	-.00	-.00	.09	-.25	-.25
0.9	.12	.11	.09	.04	.48	.09	.27
Underwriting standards	y_t	p_t	r_t	s_t	c_t^P	c_t^H	p_t^H
h	4	4	4	1	32	32	32
0.1	-.24	-.14	-.10	-.02	-.47	-.68	-.74
0.5	-.08	-.01	-.01	.01	-.08	-.16	-.09
0.9	.02	.13	.10	.04	.21	.17	.43

The table shows the median and 0.1 and 0.9 quantiles of lagged responses to policy interventions from model DSC at different horizons (quarters), as indicated in row h.

The results of further robustness checks are shown in Figures C.5 and C.6. We consider estimates based on an uninformative prior for the reduced-form VAR coefficients using 4 lags and add a banking deregulation index as an exogenous variable to the baseline VAR to control for the deregulation of the U.S. banking sector in the 1980s (Mian, Sufi, and Verner 2017). Finally, estimates from the standard Bayesian and frequentist proxy VARs again are very similar to those from model DSC.

5 Conclusions

This paper studied estimation methods for Bayesian structural VARs that are identified from sparse narrative information, which may be subject to errors. First, we showed that a discriminant (DC) regression, based on the assumption of conditional normality, represents a minor adaptation of the standard Bayesian proxy VAR. Consequently, the standard approach turns out to perform fairly well in practice. Second, a non-parametric sign concordance (SC) criterion tends to be less efficient than the DC regression but proves useful as a reliability prior in combination with the latter.

These methods should be useful for assessing the effects of policies in areas where interventions are infrequent and difficult to quantify. Our Monte Carlo simulations indicate that efficiency losses from using binary instruments remain contained, while Bayesian VARs tend to outperform frequentist approaches. In a recent paper, Giacomini et al. (2022) make a strong case for a Bayesian approach to sparse narrative proxies by showing that the assumptions required for the asymptotic validity of the Montiel Olea et al. (2021) bootstrap are violated for weak sparse instruments. Moreover, applications may benefit from Bayesian approaches to estimating large VARs. For instance, studies of regulatory policies typically use cross-country panel data since interventions in individual countries are rare. This suggests using a panel proxy VAR, with clear benefits from Bayesian estimation. Such an approach is pursued by Rünstler (2021) in a study on the effects of labour market reforms in the euro area.

Our application to the effects of macroprudential policies in the postwar U.S. indicates long transmission lags in the response of credit and house prices, in particular for borrower-based measures. Studies based on cross-country panel regressions typically inspect rather short horizons and may therefore understate the effects of these policy measures. We also found moderate but significant declines in economic activity and a widening of corporate bond spreads after a policy tightening, pointing to benefits from a coordination of macroprudential with monetary policies. Our findings are informative about the impact of general shifts in credit supply and in household collateral constraints and underpin the high persistence of leverage cycles documented, for instance, by Claessens et al. (2012) and Rünstler and Vlekke (2018). Similarly, Fieldhouse et al. (2018) have stressed that the easing of borrowing constraints due to financial innovation has materialized in house prices only with long lags.

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Annexes

Annex A: The Narrative Indicators

Capital Requirements

1981/15/12 Tightening

The Federal Reserve Board and the Office of the Comptroller of the Currency introduce capital standards common to all banks. The standards employ a leverage ratio of primary capital (which consisted mainly of equity and loan loss reserves) to average total assets. Standards differ slightly by type of institution, with a value of 6 % for community banks and 5 % for large regional institutions. Source: Federal Deposit Insurance Corporation (FDIC).

1983/03/01 Tightening

Congress passes the International Lending Supervision Act (ILSA). This statute directs the banking regulators to "achieve and maintain adequate capital by establishing minimum levels of capital" for banks subject to regulation. The ILSA was enacted in response to the Latin American debt crisis, which revealed a high risk of the foreign sovereign debt exposure of some U.S. banks. The law also put on firmer footing the regulators' authority to issue capital adequacy rules. Source: Federal Register.

1985/15/06 Tightening

Regulators abolish the differences in bank leverage by type of bank as established in the 1981/15/12 Act in favour of a uniform standard of 5.5 %. Banks with less than 3% of primary-capital-to-total assets are declared to be "operating in unsafe condition" and are made subject to enforcement actions. Source: FDIC.

1990/31/12 Tightening

The first stage of the Basel I rules is enacted by U.S. regulators imposing two requirements on capital ratios, related to Tier 1 and Tier 2 capital. First, Basel I calls for a minimum ratio of total (Tier 1 plus Tier2) capital to risk-weighted assets (RWA) of 8 %, and of Tier 1 capital to risk-weighted assets of 4 %. The first stage requires respective ratios of 7.25% and 3%, while the full are phased in until the end of 1992. Source: Posner (2014).[17]

1991/19/12 Tightening

The Federal Deposit Insurance Corporation Improvement Act categorises institutions according to their capital ratios. Other than "well capitalised" banks (at least 10 % total risk-based, 6 % Tier 1 risk-based, and 5% leverage capital ratios) face restrictions on certain activities and are subject to mandatory or discretionary supervisory actions. Source: Government Publishing Office (GPO).

1992/31/12 Tightening

The final implementation stage of the Basel I rules is enacted by U.S. regulators with the own funds ratio set to 8%, and the leverage ratio set to 4%. Source: Posner (2014).

[17] Posner, E. (2014). How do bank regulators determine capital adequacy requirements? Coase-Sandor Institute for Law & Economics Working Paper 698.

2002/01/01 Easing

The Recourse Rule reduces risk weights for AAA- and A.A.- rated "private-label" mortgage-backed securities (MBS) and collateralised debt obligation (CDO) tranches originated by large banks to 0.2 in line with government-sponsored enterprise (GSE)–originated MBS. For A-rated tranches, the risk weights are set to 0.5, while lower-rated tranches are assigned higher risk weights. The rule is designed to encourage securitisation without encouraging risk-taking, while risk weights are kept close to 2004 Basel II risk weights. Source: Posner (2014).

2006/31/12 Tightening

The Tier 1 leverage ratio is increased to 4 %. Source: Posner (2014).

2013/01/01 Tightening

The Federal Reserve Board approves a final rule to implement changes to the market risk capital rule, which requires banking organisations with significant trading activities to adjust their capital requirements to better account for the market risks of those activities (Basel II.5). The adoption of Basel II.5, also known as the market capital risk rule, has been issued by the U.S. federal banking regulators on June 7, 2012. Source: Federal Reserve Board (FRB).

2013/30/07 Tightening

The Federal Reserve Board (FRB) introduces a Annexary leverage ratio requirement of 3% for banks using the advanced approach for RWA calculation. An additional 2% buffer requirement has been proposed for G-SIBs. Further, IRB banks are required to apply the lower of capital ratios calculated under the standardised and IRB approaches. Source: FRB.

Mortgage Underwriting Standards

1958/01/04 Easing

Changes to requirements on loans insured by the Veteran Administration. Removal of 2% down payment requirement on insured loans. Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first $ 13,500 of value plus 85% of next USD 2,500 plus 70% of value in excess of $ 16,000 to maximum mortgage of USD 20,000. (ii) LTV for existing construction, 90% of first US$D 13,500 of value plus 85% of next $ 2,500 plus 70% of value in excess of $ 16,000 to maximum mortgage of $ 20,000. Source: Elliot et al. (2013).

1959/23/09 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first $ 13,500 of value plus 90% of next $4,500 plus 70% of value in excess of $18,000 to maximum mortgage of $ 22,500. (ii) LTV for existing construction, 90% of first $18,000 of value plus 70% of value in excess of $18,000 to maximum mortgage of $ 22,500. Source: Elliot et al. (2013).

1961/30/06 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction set to 97% of first $15,000 of value plus 90% of next $5,000 plus 75% of value in excess of $20,000 to maximum mortgage of $25,000. (ii) LTV for existing construction, 90% of first $20,000 of value plus 75% of value in excess of $20,000 to maximum mortgage of $25,000. (iii) Easing of maturity standards for new construction, maximum mortgage term raised from 30 to 35 years or 3/4 of the remaining life of improvements, whichever is less; existing construction still 30 years. Source: Elliot et al. (2013).

1964/01/01 Easing

National banks are allowed to extend real estate loans with 25-year terms and 80% LTV if fully amortised. Source: Elliot et al. (2013).

1964/02/09 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first $15,000 of value plus 90% of next $5,000 plus 75% of value in excess of $20,000 to maximum mortgage of $30,000. (ii) LTV for existing construction, 90% of first $20,000 of value plus 75% of value in excess of $20,000 to maximum mortgage of $30,000. Source: Elliot et al. (2014).

1965/10/08 Easing

Act of Congress changes requirements on loans insured by the Federal Housing Administration. (i) LTV for new construction, 97% of first $15,000 of value plus 90% of next $5,000 plus 80% of value in excess of $20,000 to maximum mortgage of $30,000. (ii) LTV for existing construction, 90% of first $20,000 of value plus 80% of value in excess of $20,000 to maximum mortgage of $30,000. Source: Elliot et al. (2013)

1970/01/01 Easing

National banks are allowed to extend real estate loans with 30-year terms and 90% LTV if fully amortised. Source: Elliot et al. (2013).

1974/01/01 Easing

National banks are allowed to extend real estate loans with 30-year terms and 90% LTV if 75% amortised. Source: Elliot et al. (2013).

1983/01/09 Easing

LTV limits are removed for all bank mortgage loans (Garn-St Germain). Source: Elliot et al. (2013).

2014/30/01 Tightening

A New Ability to Repay (ATR) and Qualified Mortgage (Q.M.) Rule by Consumer Financial Protection Bureau (CFPB) establishes a minimum set of underwriting standards in the mortgage market. For qualified mortgages, the borrower must prove a debt service-to- income ratio no greater than 43%. Source: CFPB.

Annex B.1: Linear Discriminant Analysis

This Annex outlines the relation of the DC regression to discriminant analysis, following Maddala (2013).

Consider a dichotomous variable z_t that takes the value $z_t^* = 1$ for m observations and $z_t^* = 0$ for the remaining $T - m$ observations. The objective of discriminant analysis is to estimate function $\psi(x_t)$ to predict z_t^* from a set of random variables $x_t = (x_{1,t}, \ldots, x_{n,t})$ based on the rule $\hat{z_t^*} = 1$ if $\psi(x_t) > 0$ and $\hat{z_t^*} = 0$ otherwise (e.g. Maddala, 2013: 79ff). $\psi(x_t)$ is chosen to minimise the objective function

$$C = C_1 \int_{R_1} f_1(x_t)dx + C_0 \int_{R_0} f_0(x_t)dx,$$

where $f_k(x_t)$ denote the conditional distributions of $x_t|z_t^* = k$. R_1 defines the region such that $\psi(x_t) > 0$ if $x_t \in R_1$ and R_0 is the complement of R_1. C_k is the cost of misclassifying a member of group G_k.

Under the assumption that $x_t|z_t^* = 1 \sim N(\mu_1, \Sigma)$ and $x_t|z_t^* = 0 \sim N(\mu_0, \Sigma)$, the optimal discriminant function is linear, $\psi(x_t) = \psi_1^T x_t$. Under the specific loss function $mC_1 = (T - m)C_0$, the maximum likelihood estimate of parameter vector ψ_1 maximizes the ratio of the squared difference in means between groups and the variance within groups, $(\psi_1^T \Sigma \psi_1)^{-1} \left[\psi_1^T(\mu_1 - \mu_0)\right]^2$. This is equivalent up to scale to estimating a via OLS from the regression $z_t^* = a_0 + a^T x_t + \xi_t$, where $z_t^* = z_t - m/T$ (Maddala, 2013:18ff).

Annex B.2: Further Details on the Structural VAR

Identifying Restrictions

Section 2 specifies the identifying conditions (3) in terms of conditional expectations, whereas the literature typically defines them in terms of covariances (e.g. Stock and Watson, 2018) as

$$\begin{aligned} \mathrm{cov}(\theta_t, z_t) &> 0 \\ \mathrm{cov}(\epsilon_t, z_t) &= 0. \end{aligned} \tag{10}$$

This section shows that the two specifications are equivalent in the case of a binary instrument. To see this, note that the covariance between a continuous random variable η_t and a binary random variable z_t can be expressed in terms of conditional expectations. For symmetry reasons, it again suffices to show this for a purely binary instrument. Assume, therefore, that z_t is a random variable that takes a value of one with probability $0 < \lambda < 1$ and is zero otherwise. The covariance $\mathrm{cov}(\eta_t, z_t)$ then can be expressed as

$$
\begin{aligned}
\operatorname{cov}(\eta_t, z_t) &= \mathbb{E}(\eta_t z_t) - \mathbb{E}\eta_t \mathbb{E}z_t \\
&= \lambda\mathbb{E}(1\eta_t|z_t = 1) + (1-\lambda)\mathbb{E}(0\eta_t|z_t = 0) - \lambda\mathbb{E}\eta_t \\
&= \lambda\mathbb{E}(\eta_t|z_t = 1) - \lambda\left(\lambda\mathbb{E}(\eta_t|z_t = 1) + (1-\lambda)\mathbb{E}(\eta_t|z_t = 0)\right) \\
&= \lambda(1-\lambda)\left[\mathbb{E}(\eta_t|z_t = 1) - \mathbb{E}(\eta_t|z_t = 0)\right]
\end{aligned}
$$

It follows immediately that conditions (3) imply conditions (10), as $\operatorname{cov}(\theta_t, z_t) = \lambda(1-\lambda)\gamma$ and $\operatorname{cov}(\epsilon_t, z_t) = 0$.

Vice versa, we combine conditions (10) with the assumption that $z_t = 0$ does not convey information about the VAR residuals, $\mathbb{E}(\theta_t|z_t = 0) = \mathbb{E}(\epsilon_t|z_t = 0) = 0$. Hence, $\mathbb{E}(\theta_t|z_t = 1) = [\lambda(1-\lambda)]^{-1}\operatorname{cov}(\theta_t, z_t) > 0$ and $\mathbb{E}(\epsilon_t|z_t = 1) = 0$. Conditions (10) therefore imply conditions (3) under the additional assumption. From equation (2), the assumption $\mathbb{E}(\theta_t|z_t = 0) = \mathbb{E}(\epsilon_t|z_t = 0) = 0$ is equivalent to $\mathbb{E}(u_t|z_t = 0) = 0$, which replaces the standard condition $\mathbb{E}u_t = 0$ in our model. It is, therefore, required as a normalisation condition for identifying the constant term c of the VAR (1), as is obvious from section 2.3 on estimation. Hence, the two specifications are equivalent. Note that conditions (3) do actually not require z_t to be a random variable.

Constructing matrix A_0

Consider the moving average representation of equation (2)

$$
x_t = \Big(\sum_{s=0}^{\infty}\Psi_s\Big)A_0^{-1}c + \sum_{s=0}^{\infty}\Psi_s A_0^{-1}\varepsilon_{t-s}^{+}
$$

where $(\varepsilon_{t-s}^{+})^T = (\theta_t, \varepsilon_t^T)^T$ and) matrices Ψ_s are the elements of lag polynomial $\Psi(L) = B^{-1}(L)$ with $B(L) = I_n - \sum_{s=1}^{p} B_s L^s$. $\Psi(L)$ defines the IRF of SVAR given by equations (1) and (2). Since $\Psi_0 = I_n$, matrix A_0^{-1} gives the contemporaneous impact of the structural innovations on the VAR series.

We first review the construction of matrix A_0 as, e.g. set out in Arias et al. (2018). The condition $u_t = A_0^{-1}\epsilon_t^{+}$, together with $\mathbb{E}u_t u_t^T = \Sigma$ and $\mathbb{E}\epsilon_t^{+}(\epsilon_t^{+})^T = I_n$, implies $\Sigma^{-1} = A_0^T A_0$. Further, matrix A_0 can be expressed as $A_0^T = A_* Q$, where A_* is a unique lower triangular matrix derived from the Choleski decomposition $\Sigma^{-1} = A_* A_*^T$ and $Q = (q_1, \ldots, q_n)$ is an arbitrary orthogonal matrix, $Q^T = Q^{-1}$, that is constructed such that A_0 satisfies certain restrictions. Arias et al. (2018) show how random draws of Q that satisfy deterministic restrictions may be constructed in a recursive way from a Gram-Schmidt orthogonalisation: column q_j is obtained by drawing an $n \times 1$ vector $x_j \sim N(0, I_n)$ and deriving q_j such that q_j is orthogonal to $(q_1, \ldots, q_{j-1})$ and satisfies further deterministic restrictions specific to innovations $\epsilon_{t,j}^{+}$.

In case of the *DC* restriction, vector α defines the first column of A_0, which implies $q_1 = A_*^{-1}\alpha$.

The reverse expression $\alpha = A_* q_1$ is used in case of the SC restriction. An uninformative random draw of α is obtained by drawing q_1 from the Haar measure of orthogonal matrices as $q_1 = v/||v||$, with random draw $v \sim N(0, I_n)$. In both cases, the remaining columns of matrix Q are irrelevant and are constructed without further restrictions, as explained in Arias et al. (2018). Note that q_1 suffices for defining the contemporaneous impact of θ_t, as $A_0^{-1} = (A_*^T)^{-1}Q$ and the first column of A_0^{-1} is therefore well-defined and independent of all q_j with $j > 1$.

Combination with Sign and Zero Restrictions

DC and SC restrictions may also be embedded in the approach of Arias et al. (2018) and thereby be combined with zero and sign restrictions on IRFs. Define $g(A_0, \Psi(L)) = \left[\Psi_0^T, \Psi_1^T, \ldots, \Psi_s^T\right]^T A_0^{-1}$. Express zero and sign restrictions on column j of $\Psi(L)$, i.e. the IRFs to shock $\varepsilon_{j,t}$ as

$$Z_j g(A_0, \Psi(L)) e_j = 0$$
$$S_j g(A_0, \Psi(L)) e_j > 0$$

with appropriate selection matrices Z_j and S_j. Vector e_j denotes column j of identity matrix I_n.

The algorithm of Arias et al. (2018) to generate posterior draws of $\Psi(L)A_0^{-1}$ under this type of restrictions proceeds by (i) drawing from the posterior $(B(L), \Sigma)$ to obtain $\Psi(L)$ and A_*; (ii) obtaining uninformative draws of Q that satisfy the zero restrictions $Z_j g(A_* \Psi(L))$; and (iii) applying an importance sampling step to account for volume changes due to zero restrictions; and (iv) inspecting the validity of sign restrictions.

With the DC regression, the draw of α uniquely defines $q_1 = A_*^{-1}\alpha$, while the remaining columns of Q remain unspecified. The DC restriction may therefore be combined with zero and sign restrictions on shocks $\epsilon_{t,j}$ for $j > 1$. Note that we draw α from a non-degenerate distribution. Hence, there is no volume reduction, and the importance sampling step by Arias et al. (2018) is not required. The SC posterior on shock θ_t is implemented from a rejection sampling step. Hence, it may be combined with sign restrictions on shocks $\epsilon_{t,j}$ for all j and zero restrictions for $j > 1$.

Annex B.3: Monte Carlo Simulations

For the simulations presented in section 3 of the main text, we set

$$B_1 = \rho \begin{bmatrix} \cos(\omega) & \sin(\psi) \\ -\sin(\psi) & \cos(\psi) \end{bmatrix} \qquad A_0^{-1} = \begin{bmatrix} 1.0 & 0.3 \\ 0.3 & 1.0 \end{bmatrix}^{1/2} \begin{bmatrix} \cos(a) & \sin(a) \\ -\sin(a) & \cos(a) \end{bmatrix}$$

with $\rho = 0.9$, $\psi = 0.2$, and $a = \pi/4$. Matrix B_1 is subject to complex conjugate roots and generates cyclical fluctuations of length of $2\pi/\psi = 32$ quarters. Matrix A_0^{-1} is constructed from the Choleski decomposition times a rotation matrix, $A_0^T = A^* Q$, such that residuals $u_t = A_0^{-1}\varepsilon_t^+$, are subject to a correlation of 0.3 for zero ζ_t, while the rotation matrix ensures that the initial response of $x_{t,1}$ to θ_t is sufficiently large.

To calibrate the number of policy shocks m, we let $\sigma_\nu = 0.01$ and calibrate the expected value of the size of the policy innovation, $\bar{\zeta}$, to achieve the desired expected number of policy interventions m. This give values of $\bar{\zeta} = 1.64$ for $m = 10$ and $\bar{\zeta} = 1.28$ for $m = 20$. As regards simulation (4), since $\operatorname{var} x_{t,i}) = (1 - \rho^2)^{-1}\Sigma_\varepsilon$, with $\omega = 0.5$, the lagged term $x_{2,t-1}$ explains about 70% of the total variance of ζ_t^*. The results presented are based on 1000 draws of the DGP (9) and, for each draw of the DGP, 200 draws of the posterior or bootstrap confidence bounds, respectively. The number of observations is set to $T = 200$.

The Bayesian VARs are explained in the main text. In all cases, we employ an uninformative Jeffrey prior for the reduced form VAR and assume 1 lag in estimation, mirroring the data generating process. For models BV_ζ and BV we skip mean adjustment Γz_t and draw from the proxy regression $\zeta_t = a^T u_t + \xi_t$ using an uninformative Normal-Gamma prior and assuming a normal distribution of residual ξ_t. In implementing the frequentist proxy VAR, we rely on the code of Mertens and Montiel Olea (2018), which offers the bootstraps of Jentsch and Lunsford (2016) and Montiel Olea et al. (2021) for proxy VARs with sparse instruments. The two bootstraps give very similar results, and we report only the latter. For local projections we estimate the equation $x_{1,t+h} = a^T x_t + \gamma_h z_t + u_{1,t}$ and obtain the impulse response to the policy innovation at horizon h directly from coefficient γ_h. We obtain uncertainty bands from a standard bootstrap.

Table B.1 shows the results for an alternative data generating process, which assumes unconditional normality of the generated innovations. In terms of equations (9), this is achieved by setting $\zeta_t \equiv 0$. For strong identification we construct instrument z_t by randomly selecting $m = 10$ observations from the largest 40 realisations of innovations η_t out of $T = 200$ observations. For weak identification, we select them from the $m = 100$ largest realisations. This implies that innovations associated with the instrument are, on average smaller in absolute size in the second case. In either case, however, instrument z_t displays perfect sign concordance with the true innovations η_t for the m selected periods.

The results differ in two important ways from those presented in the main text. First, models DSC and SC using the sign concordance prior with $\lambda = 1$ now provide the most efficient estimates. This suggests that the SC prior is highly efficient under perfect sign concordance. However, model SC continues over-estimating the width of uncertainty bands as it is based on set identification only, whereas model DSC maintains a weak tendency of underestimating them. Second, the Montiel Olea et al. (2021) bootstrap for the frequentist proxy VAR now provides accurate confidence bounds.

Table B.1: Monte Carlo Simulations (alternative DGP)

			BV_ζ	BV	DC	DSC	SC	pV	LP
(1)	Weak	RMSE	.12	.13	.13	.10	.09	.15	.21
		Bias	-.04	-.06	-.06	-.01	-.01	-.05	.05
		IQD	.39	.42	.41	.32	.27	.44	.67
		UB	.41	.52	.51	.23	.25	.55	.97
		Coverage	.79	.85	.84	.65	.71	.85	.89
(2)	Strong	RMSE	.09	.09	.09	.09	.07	.09	.12
		Bias	-.02	-.02	-.04	-.04	-.02	-.03	.06
		IQD	.29	.29	.29	.27	.22	.30	.35
		UB	.26	.27	.28	.29	.53	.32	.48
		Coverage	.72	.74	.75	.76	.98	.78	.85

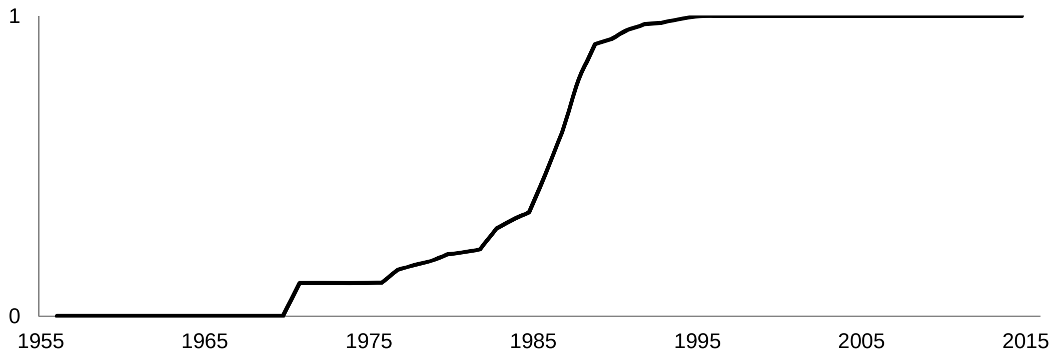
The table shows statistics of standardised IRFs at horizons $h = 0$ for the alternative data generating process assuming unconditional normality of the residuals as described in the current section of the Supplement. The models are as in the main text. $RMSE$ and $Bias$ are the root mean squared error of the estimate and its difference to the true IRF, respectively. IQD is the [0.1, 0.9] interquantile difference of the distribution of the central estimate measuring its true uncertainty, while UB is the corresponding estimated uncertainty bands. $Coverage$ stands for the share of draws where the true IRF lies within the estimated bands with a correct value of .80.

Annex B.4: Banking Deregulation Index

Our banking deregulation index is an unweighted average of two sub-indices related to inter-state and intra-state deregulation. Each sub-index takes values of zero (full regulation) to one (no regulation) with intermittent values equal to the GDP shares (as of 1980) of states which had introduced respective deregulation. Hence, the index equals zero before 1970, the beginning of deregulation, and one after 1996.

As discussed by Kroszner and Strahan (1999, 2014), deregulation was a gradual process that consolidated the fragmented banking system in multiple ways. States differed in the timing of when they allowed banks from other states to operate in their jurisdiction and in how many other states were given access. Another source of variation was the timing of the removal of intra-state branching restrictions that prohibited banks from expanding their branch network within a state.[18]

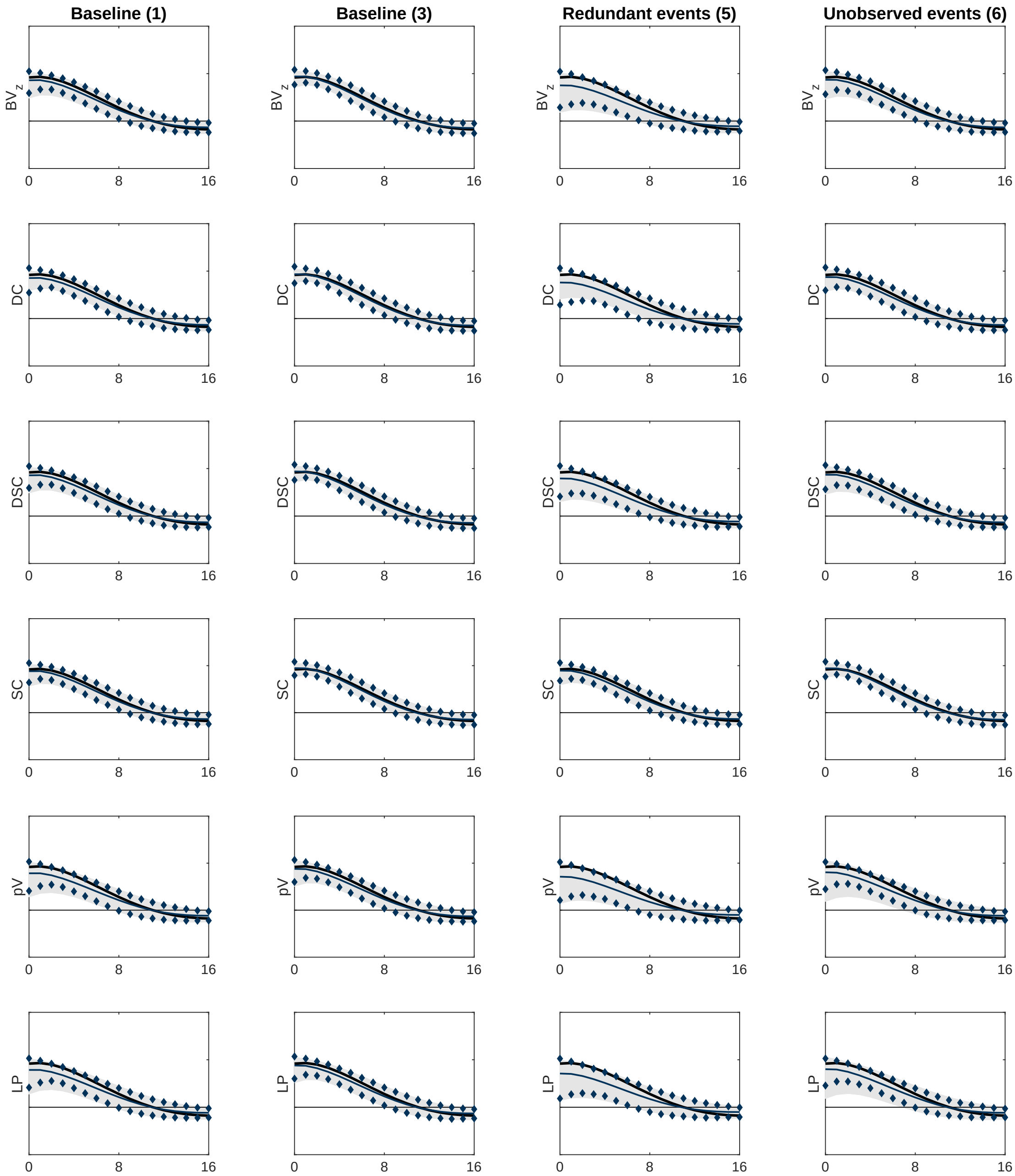
Figure B.1: Banking Deregulation Index



We use the indices provided by Mian et al. (2017), which reflect the start of the deregulation process. For example, the year of inter-state banking deregulation is defined as the first year in which a state allowed out-of-state banks to open a branch. These decisions were based on bilateral arrangements between states until the Riegle-Neal Act of 1994 resulted in the general deregulation of U.S. interstate banking. Kroszner and Strahan (1999, 2014) conclude that the process of deregulation was largely exogenous to macroeconomic conditions as it was driven by a combination of technological change and shifts in private and public interest. For instance, the speed of deregulation is highly correlated with republican versus democratic state government.

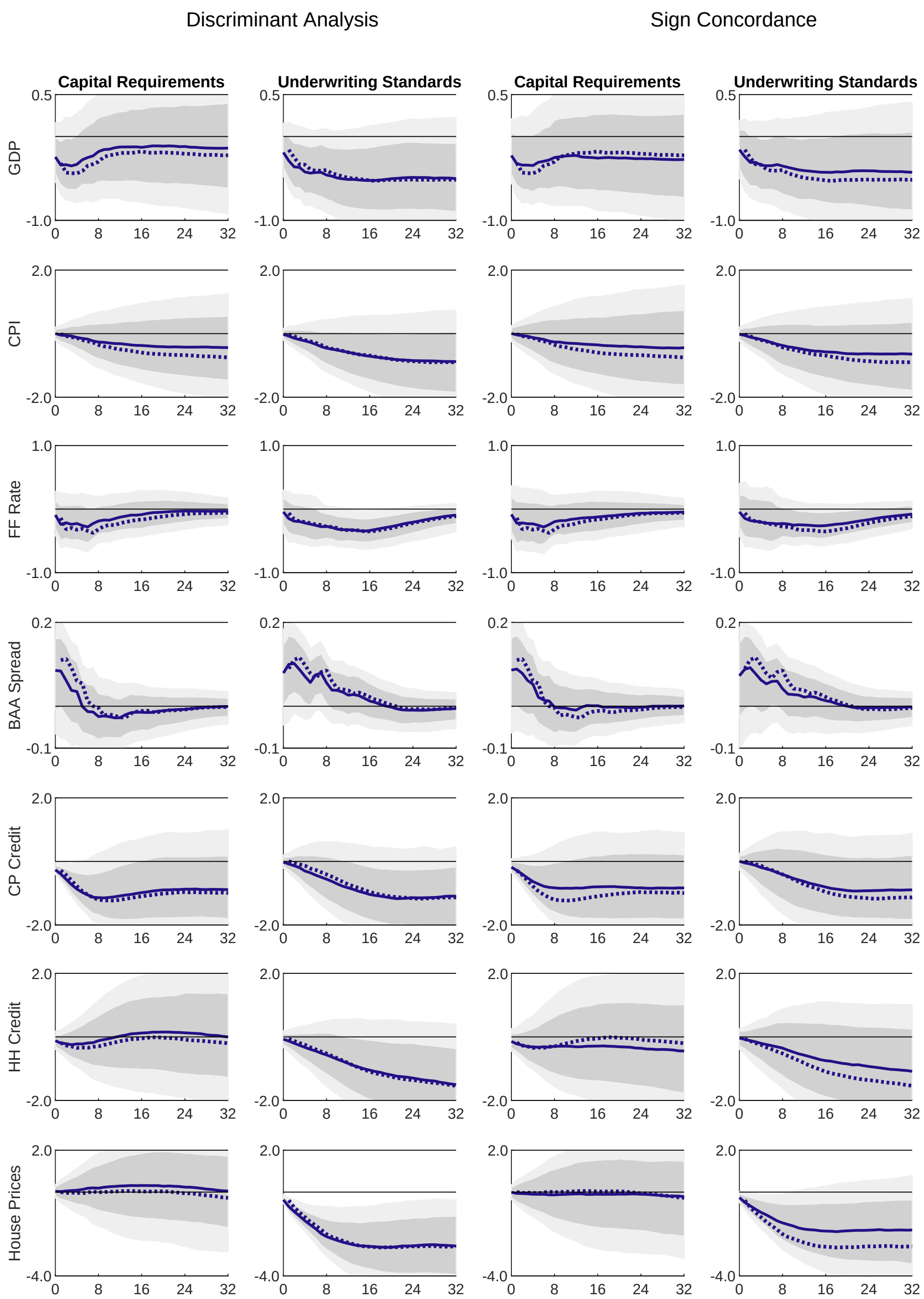
[18]See Kroszner, R.S. and P. E. Strahan. (1999). What Drives Deregulation? Economics and Politics of the Relaxation of Bank Branching Restrictions. The Quarterly Journal of Economics 114(4):1452-1467 and Kroszner, R.S. and P. E. Strahan. (2014). Regulation and Deregulation of the U.S. Banking Industry: Causes, Consequences, and Implications for the Future. In N. L. Rose (ed.). NBER Book Economic Regulation and Its Reform: What Have We Learned?: 485-543. University of Chicago Press.

Figure C.1: Monte Carlo Simulations



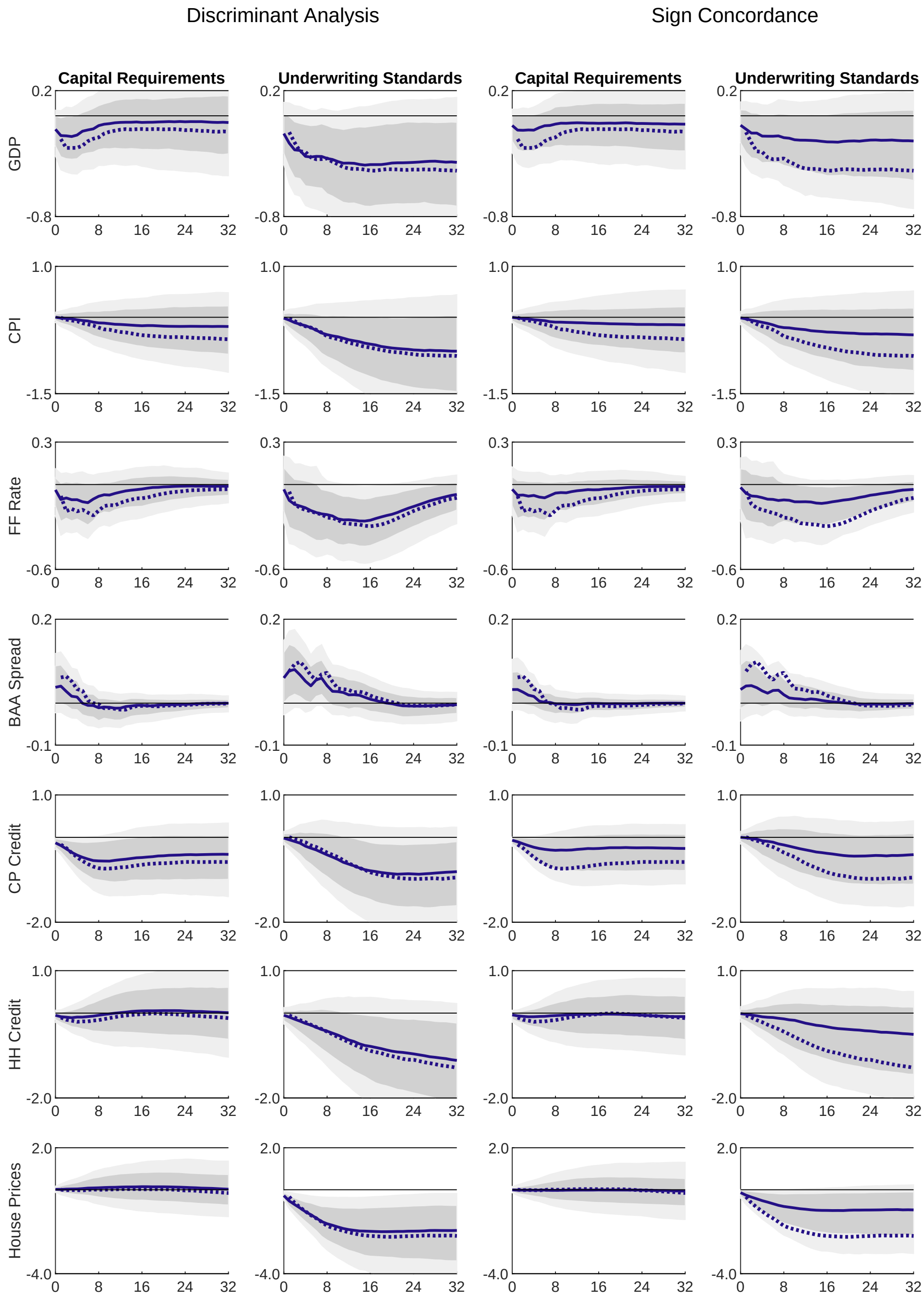
The black solid line shows the true IRF. The blue solid and dotted lines show the central estimate and its [.10, .90] quantiles as provided by the various methods. The shaded area shows the [.10, .90] quantiles of confidence bounds. See Table 1 for the definition of the simulations. The models and the calculation of central estimates and confidence bounds are explained in the main text.

Figure C.2: Standardized IRFs for DC and SC Restrictions



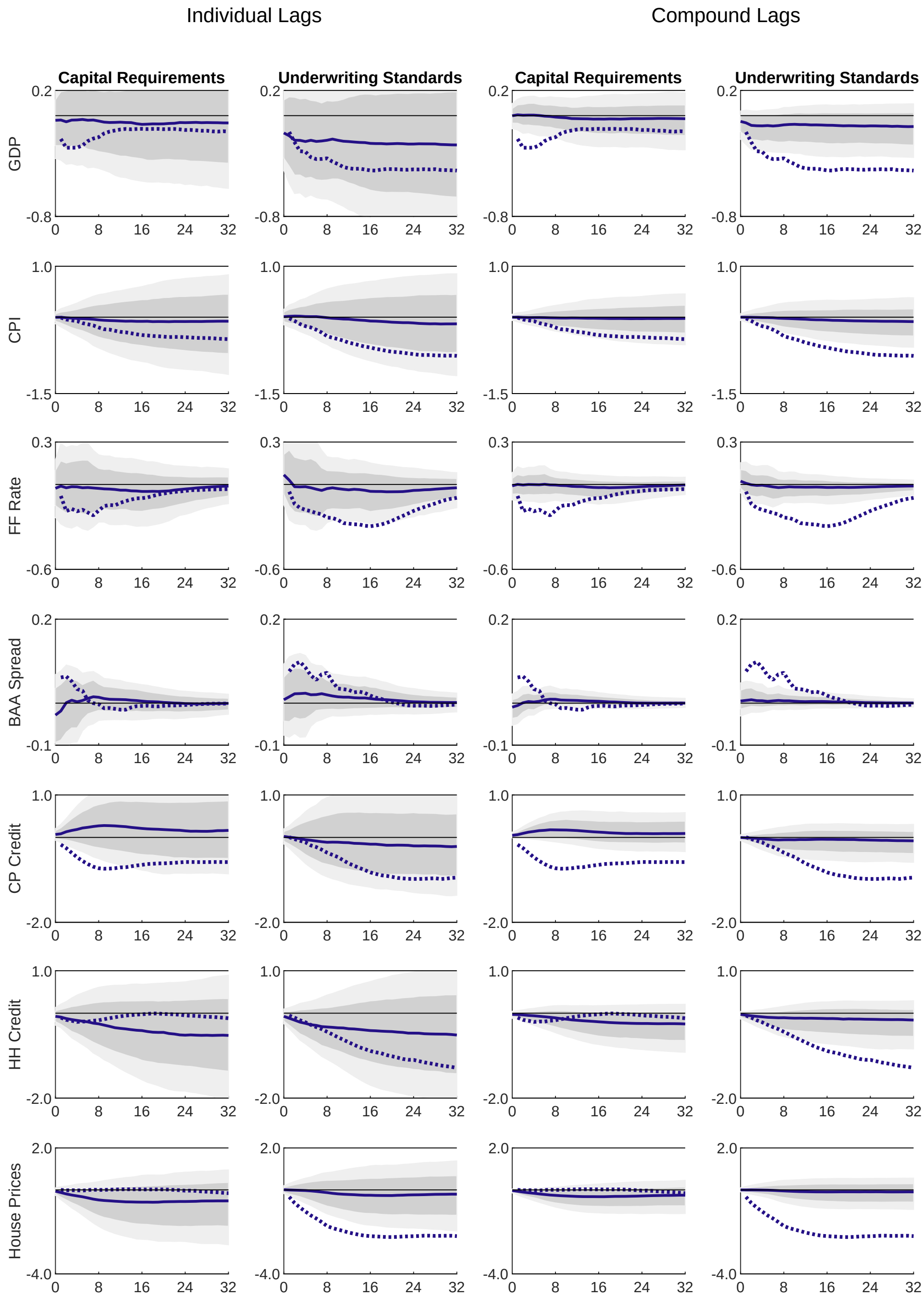
The graphs show the impulse responses to a 1% shock based on either *DC* or *SC* restrictions. The solid line shows the median and bounds show [0.05; 0.95] and [0.16; 0.84] quantiles of IRFs. The dotted line shows the main estimate from the *DSC* restriction.

Figure C.3: IRFs Scaled by the Average Policy Impact



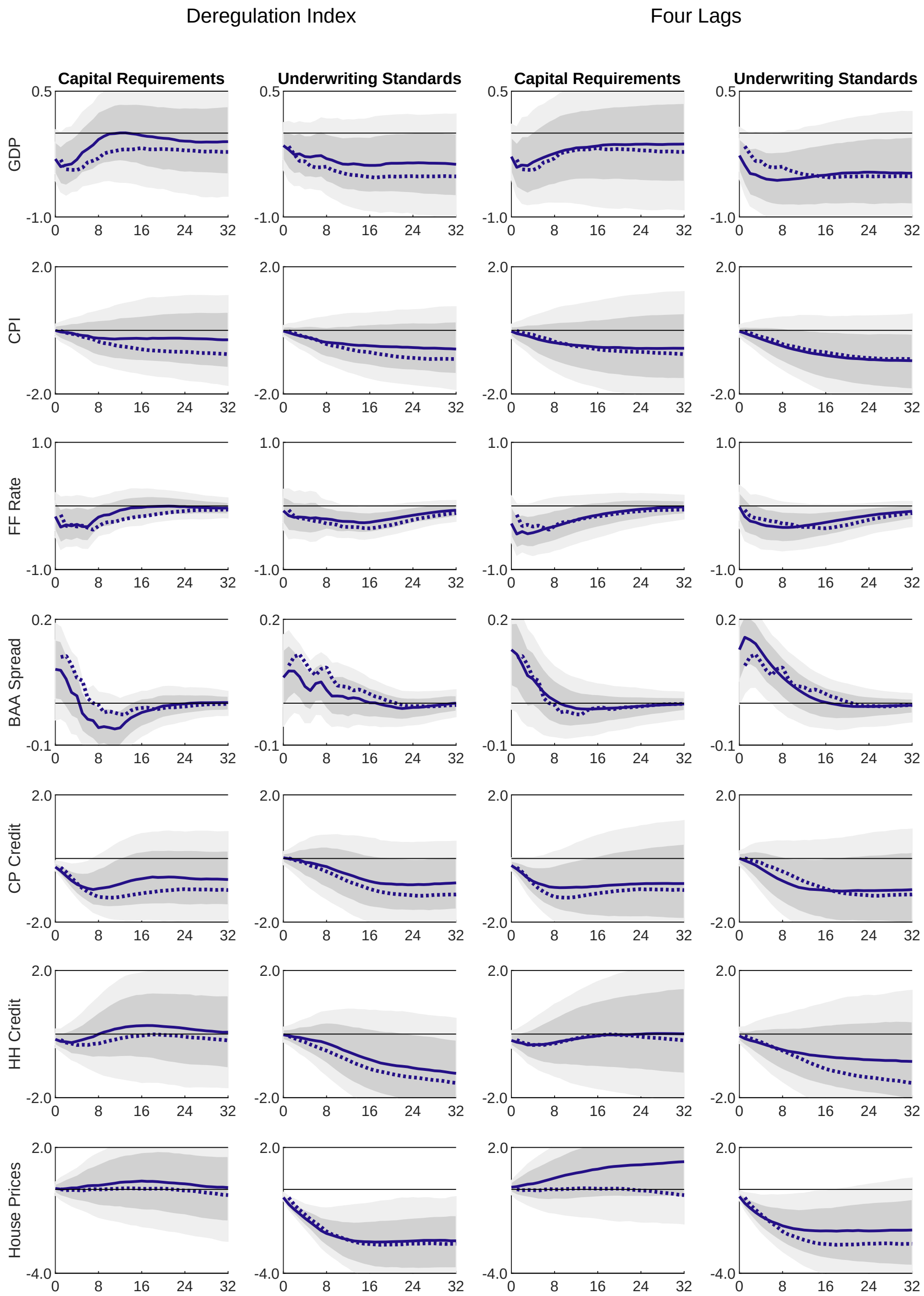
The graphs show the IRFs scaled the impact of average policy shock of size based on either DC or SC restrictions. See Figure C.3 for further explanations.

Figure C.4: Robustness Check against Lagged Impacts



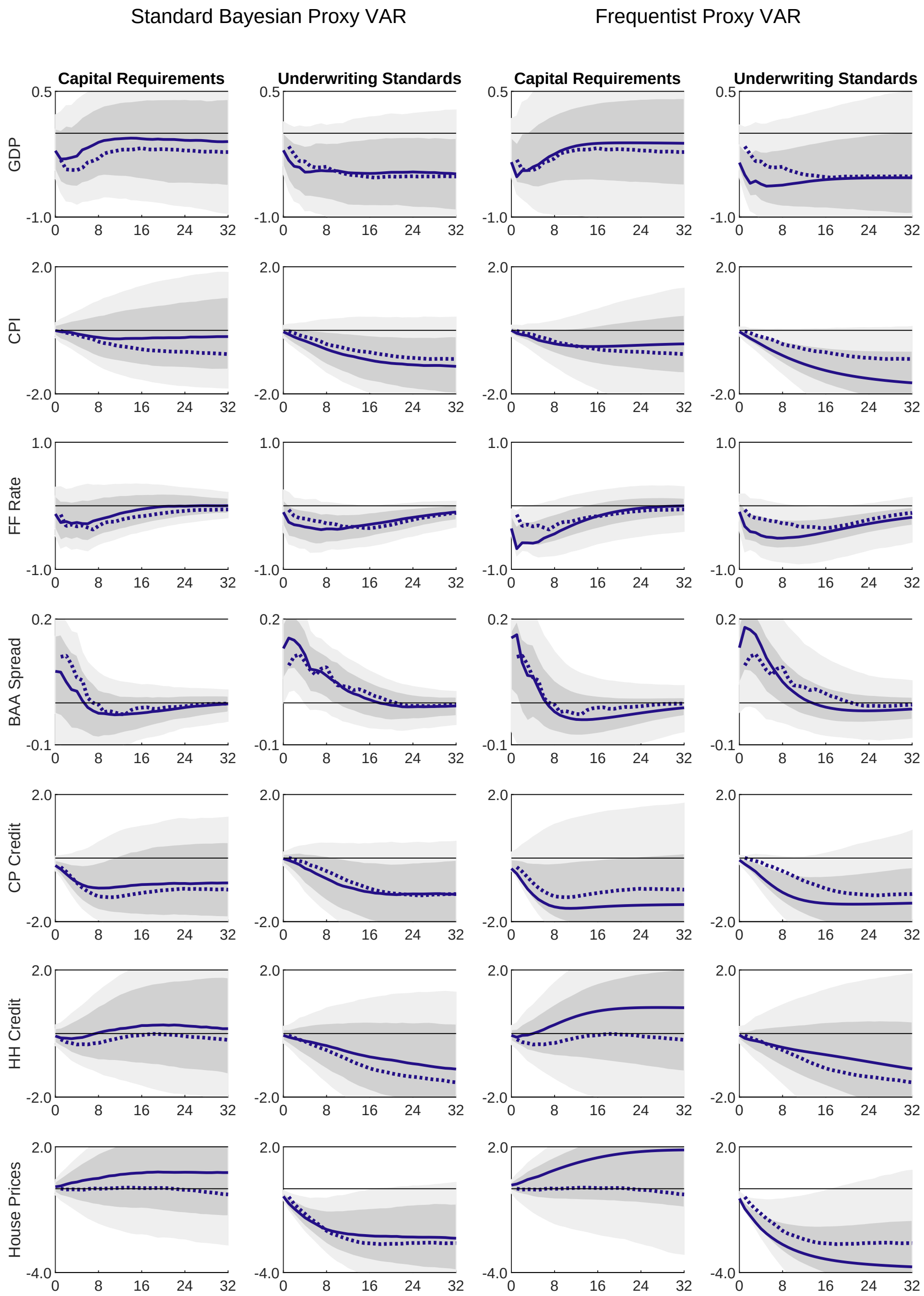
The graphs show standardised IRFs from the robustness checks against lagged innovations discussed in section 4.3. The left hand graph shows estimates with lags drawn for individual periods from a uniform distribution (model *DSC*) and using the *SC* prior, the right hand one for fixed lags from 1 to 4 (model *DC*). The latter corresponds to the results shown in Table 4. See Figure C.3 for further explanations.

Figure C.5: Standardized IRFs from Alternative Estimates



The left hand graph shows estimates including the deregulation index. The right hand graph shows estimates from a VAR including 4 lags. See Figure C.3 for further explanations.

Figure C.6: Standardised IRFs from Standard Proxy VARs



The graphs show IRFs from a standard Bayesian proxy VAR and from a frequentist proxy VAR with confidence bands based on the bootstrap by Montiel Olea et al. (2021). See Figure C.3 for further explanations.

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The views expressed in this paper are those of the authors and do not necessarily reflect the views of the European Central Bank.

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