No. 09-2021

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January 17, 2021

Abstract

This paper studies the effectiveness of micro- and macroprudential policy tools in the euro area. The established empirical literature on macroprudential policy generally considers panel estimations that suffer from two estimation biases, i.e., a selection bias and a time bias. We control for the former by a propensity score matching approach. Based on a logit model, we estimate the probability of a policy tightening for every country at each point in time. Matching procedures then find one or more matching partners for every tightening event with a similar likelihood of a tightening but no shift in the prudential policy stance. An iterative approach ensures that we offset the time bias, which exists if the estimation does not control for effects of preceding and subsequent prudential policy changes. We find that the announcement of a prudential policy tightening reduces credit growth significantly by about 1% on average. We further differentiate between effects along three dimensions. First, we observe that lending is more affected when policymakers have not communicated the implementation of measures before. Second, the effects are more substantial when EU/EA institutions are behind changes in the prudential policy stance. Third, microprudential policy measures have a bigger impact than macroprudential policies.

Keywords: Macroprudential policies, Financial cycles, Credit Growth, Propensity score matching

JEL classification: E44, E58, G18, G28

1 Introduction

The global financial crises of 2007/08 and the subsequent Great Recession were initially triggered by a burst of the US housing bubble. This underscores that distress in the financial markets can ultimately result in deep recessions. In highly interconnected financial markets, the stress in a subset of financial markets, such as the housing market, or stress of individual financial institutions, such as the Lehman Brothers' bankruptcy, can become systemic, leading to severe distortions of the entire financial system. Moreover, the crisis has proven that bank regulation on a microprudential level does not sufficiently limit systemic risk. In particular, market failures such as "too big" or "too connected" banks or the overstate of collaterals can lead to excessive, procyclical bank lending. Consequently, macroprudential policy measures have been introduced to tackle the weaknesses in the architecture with respect to banking. Among others, these measures include countercyclical capital buffers, liquidity ratios, and loan-loss provisions on the lender side. On the borrower side, measures such as limits on the loan-to-value (LTV) or the debt-to-income (DTI) ratio aim to avoid excessive leverage.

This paper empirically quantifies the effects of shifts in the prudential policy stance in euro area member states on credit growth and rents. The latter serves as a proxy for house prices. Overall, we find that the announcement of a prudential policy tightening reduces credit growth by about 1% on average. Our analysis is built on the data set provided by Budnik and Kleibl (2018), which lists all changes in prudential policy measures in EU member countries from 1999 to 2018. The data set includes information on which prudential measure was changed and whether the change can be interpreted as a tightening or an easing. Furthermore, information about the announcement and the implementation date for each measure is given and whether national and/or European institutions introduced the policy change. Since the changes' intensities are not always displayed, we focus on average treatment effects (ATE). The treatment group consists of all observations where policymakers tighten prudential policy measures.[1] Consequently, the control group lists all observations where no change in the prudential policy stance is observed.

In contrast to the bulk of the empirical literature, we rely on propensity score matching

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[1]Further differentiating between tightening and loosening is theoretically an option. However, the vast majority of policy changes are tightening events so that the sample size for loosening events becomes too small after all adjustments.

(PSM) approaches that are, according to Rosenbaum and Rubin (1985), designed to remove structural differences between treatment and control groups. These differences are present as prudential policies are a direct reaction to economic fundamentals. In our case, countries that change macroprudential policies might, for instance, face higher bank leverage or house prices than states that do not alter prudential policies. PSM requires two steps. In a first step, we estimate the likelihood of a policy tightening for each observation, the so-called propensity scores, via a logit estimation. In a second step, we then match for every tightening observation one or more observations that display almost identical propensity scores but no changes in the prudential policy. Differences in the dependent variable between the two matching partners finally allow us to estimate the ATE. An iterative algorithm further ensures that we correctly specify the effects of several measures that were conducted in a small time window. This way, we can generate impulse-responses, which is a major contribution to the empirical literature. Impulse-responses are, for instance, necessary to compare the outcome of structural New Keynesian models with empirical results.

Besides introducing an iterative PSM approach to the prudential policy literature, this paper has two further major contributions. First, we focus on the euro area (EA), which is of particular interest, as national authorities, as well as EA and EU institutions, are equipped with a macroprudential mandate. Hence, we can investigate whether measures conducted by national authorities are more effective than measures by EA/EU institutions. Measures initiated by national authorities might display more substantial effects as national institutions can target the domestic market more explicitly than EA or EU institutions. However, measures conduct by national authorities might be bypassed in an integrated European market. Which of these two opposing channels is more pronounced remains an empirical question. We observe that measures based on EU/EA legislation have stronger effects than measures conducted solely by national authorities. Furthermore, we find that primarily changes in EU/EA variables lead to changes in prudential policies. Moreover, we differentiate between announcement and implementation effects. This is relevant as the announcement effect of future prudential policy changes is unclear from a theoretical point of view. On the one hand, banks might reduce lending timely after the announcement of a prudential policy tightening. This way, they smoothly adjust to the new standards. On the other hand, bank lending might increase in the short run. If credit institutions and households anticipate tighter prudential policy in the future, they might move lending to a time before the tighter policies become binding. In this case, we would observe a J-curve where

the number of total credits increases in the short run, i.e., before the implementation and decreases after the implementation. However, we do not observe this J-curve empirically. Nevertheless, we find that the ATE is stronger when measures are not announced before, i.e., they are implemented right away.

Structural models show that macroprudential policies can influence bank lending and thereby systemic risk. In general, these dynamic stochastic general equilibrium (DSGE) models include collateral constraints, as in Iacoviello (2005). Macroprudential policy and in particular limits on the loan-to-value (LTV) ratio then influence this collateral constraint. Based on a news-driven model that incorporates the housing market, Lambertini, Mendicino and Punzi (2013) find that LTV measures reduce macroeconomic volatility when implemented countercyclically with respect to the credit cycle. Funke and Paetz (2012) study a model in which LTV ratios only adjust to excessive levels of house price inflation. They conclude that LTV measures can effectively dampen property price booms. Alpanda and Zubairy (2017) evaluate whether macroprudential, monetary or tax policies are best suited to reduce household debt. They find that tightening the LTV ratio and reducing the tax-deductibility of mortgage interest are the most effective and least costly policy instruments for reducing household debt. For the euro area, Quinta and Rabanal (2014) show that macroprudential policy can reduce macroeconomic volatility. This reduction is higher if nominal credit and not the credit-to-GDP gap is included in a macroprudential policy rule. Furthermore, they show that macroprudential policy accelerates the effectiveness of the monetary policy. Building on a model of two economies within a monetary union, Brzoza-Brzezina, Kolasa and Makarsk (2015) simulate imbalances that mainly hit the periphery countries in the euro area. They show that appropriate adjustments in the LTV ratio indeed reduce the volatility of credit and output in the periphery. However, this only holds when macroprudential policies are conducted decentralized.

Concerning the underlying data, the empirical literature can be split into two strands. While some studies focus on national data (e.g., Jimenez et al. (2017) and Ayiar, Calomiris and Wieladek (2014)), the majority of the empirical literature estimates the effectiveness of macroprudential policy on panel data series covering a multitude of countries. Building on the Spanish credit register, Jimenez et al. (2017) investigate the effect of dynamic provisioning that essentially works as a countercyclical bank capital buffer. They find that dynamic provisioning reduces the amplitudes of the credit cycle. In a similar man-

ner, Ayiar, Calomiris and Wieladek (2014) focus on capital requirements in the UK. They find that domestic banks lower credit supply in response to tighter capital requirements. However, the total amount of outstanding debt does not decrease because foreign banks increase lending. Using an IMF survey answered by national prudential policy authorities, Cerutti, Claessens and Laeven (2017) evaluate how the overall tightness of prudential regulation affects credit growth. More precisely, they construct an index that describes an economy's tightness by summing over all active measures. They then relate this index to credit growth changes and find that credit growth is lower when the prudential policy is tighter. In a similar vein, Akinci and Olmstead-Rumsey (2018) construct a macroprudential policy index that sums over all prudential policy changes relative to a base period. Their results indicate that tighter macroprudential policy leads to lower credit growth and house price inflation. Lim et al. (2011) further show that macroprudential policy can dampen credit growth's procyclicality. Zhang and Zoli (2014) find that primarily housing-related prudential policy measures reduce credit growth and house price inflation. Bruno, Shim and Shin (2017) show that macroprudential policies are more effective in Asia-Pacific economies if they complement monetary policy as they reinforce another.

Altogether, the empirical literature is still in its infancy and lags behind the theoretical considerations. For instance, structural models precisely show how changes in the macroprudential policy stance affect financial variables and the real economy over time. In contrast to that, the mainly applied policy indices do not directly measure the dynamic of a tightening or a loosening of a particular policy measure. The literature refrains mostly from analyzing impulse-responses because the already existent time bias would be amplified. This time bias is present when the endogenous variable responds with some delay. Distinguishing between the effects of prudential policy changes conducted in a small time window is then troublesome. Furthermore, a selection bias might be present because countries that change macroprudential policies might differ structurally from countries that do not alter prudential policies. Assuming that the economic fundamentals determine changes in the prudential policy stance, one would have to control for the economic fundamentals. However, precisely controlling for economic variables requires the knowledge of the underlying structural model. The empirical literature mainly focuses on linear models. In contrast to that, Funke and Paetz (2012) argue that macroprudential policies tightening primarily occur during excessive bank lending. According to Rosenbaum and Rubin (1985), the PSM approach used in this paper approach can solve this selection bias. Propensity

score matching approaches are relatively new to macroeconomics. Forbes, Fratzscher and Straub (2015) and Richter, Schularick and Shim (2019) are closest to our paper. Fratzscher and Straub (2015) analyze the effects of capital-flow management measures, which are only a subset of macroprudential policy measures. Since capital flows adjust timely, they do not face the time bias issue. The results indicate that some capital-flow management measures are capable of influencing capital flows. Building on that, Pandey et al. (2015) investigate capital controls in India. Forbes and Klein (2015) use PSM to evaluate how countries best respond to sudden stops in capital flows. Richter, Schularick and Shim (2019) rely on inverse propensity weights (IPW) to detect the impact of LTV measures for a panel of 56 countries and find that tighter LTV measures reduce credit and house prices. IPW and PSM are closely related. In fact, both require estimating propensity scores in a first step. In the second step, the IPW weights treated observations higher with a low probability of receiving treatment. The treated observations with a low propensity score are arguably closer to the control group. Put differently, while the PSM approach adjusts the control group to match the treatment group, the IPW also adjusts the treatment group to be closer to the control group. Austin and Stuart (2017) show that both methods yield similar results, but the PSM is preferable when the propensity score model is correctly specified.

The paper proceeds as follows: section two describes the institutional framework behind prudential policies in the EMU. In section three, we apply a standard panel estimation to our data set to bridge the gap between the PSM approach and those primarily applied in the current empirical literature on prudential policies. Afterward, we present empirical evidence from the PSM approach that removes the selection and the time bias. Section five concludes.

2   Prudential Policies in the Euro Area

The regulatory framework in the euro area is shaped by national authorities, EA and European Union (EU) institutions such as the European System of Financial Supervision (ESFS) and the Basel Accords. Prior to the global financial crisis of 2007/08 and the subsequent European debt crisis, prudential policies were primarily managed by national authorities. In the build-up to the crises, imbalances appeared on an international integrated financial market. Consequently, the ESFS has been introduced in 2011 with the main objective to monitor and harmonize prudential policies across member states. It

consists of the European Systemic Risk Board (ESRB) and the European Supervisory Authorities (ESAs)[2] . While the ESAs are responsible for microprudential policy, the ERSB's objective is to identify systemic stress in the EU and supervise the macroprudential regulation of national institutions. In general, national authorities are still responsible for the implementation of all kinds of prudential measures.

Since November 2014, the ECB functions as the direct prudential supervisor for the "systemically relevant banks" in the EA.[3] All banks with a value of its assets that is (i) above 30 billion or (ii) above 5 billion and exceeds 20% of national domestic GDP are categorized as a "systemically relevant bank". Furthermore, the ECB directly supervises prudential policies for banks that have applied for financial assistance under the European Stability Mechanism (ESM) or the European Financial Stability Facility (EFSF).

The Basel Accords set a more global framework for banking supervision. Since the Basel Committee is not endowed with a legislative mandate, the accords' enforcement is subject to national or EU-wide regulations. In the EU, the Basel Accords were implemented through the so-called Capital Requirements Regulations (CRR) and the Capital Requirement Directives (CRD). Both include measures that go beyond the scope of the Basel Accords. While the regulation is a binding legislative act, the directives are enforced through national law.

We analyze the role of prudential policy measures based on the data set provided by Budnik and Kleibl (2018) that also includes all CRR and CRD changes. On a quarterly frequency, their data contains information on all macroprudential policy measures and microprudential measures that are "likely to have a significant impact on the whole banking system" that were enforced in EU member states between 1995 and 2018. For each measure, the data set provides information on a large number of characteristics.[4] Most importantly, it states whether a measure is an easing or a tightening of prudential policies or it has an ambiguous objective. Additionally, information on whether the measure has been introduced by national authorities or is the result of EU/EA legislation is provided. Regarding

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[2]The ESAs consists of the European Banking Authority, the European Securities and Markets Authority and the European Insurance and Occupational Pensions Authority.

[3]While the participation of EA countries is obligatory, EU-members that are not part of the Euro-system are allowed to participate voluntarily.

[4]We solely focus on the characteristics that are relevant for our empirical analysis. A more detailed description of the data set is provided by Kleibl and Budnik (2018).

the time dimension, information on both the announcement as well as the implementation is given. Finally, the data set differentiates between eleven categories and 53 subcategories of regulation that can again be grouped into borrower- and lender-based measures as well as primarily micro- or macroprudential measures. The detailed information stems from questionnaires that were completed by national central banks and other supervisory authorities.

3   Evidence from Panel Analysis

The bulk of the empirical literature on macroprudential policy relies on panel estimation.[5] Consequently, panel analyses set a natural starting point for our investigation. Generally, these studies consider the overall macroprudential policy tightness of an economy and estimate its influence on credit growth and/or house price inflation. In contrast to that, our focus is on how the marginal effect of a particular policy tightening or easing evolves over time. Thus far, the empirical literature struggles to quantify these developments over time. Akinci and Olmstead-Rumsey (2018), for instance, justify their use of an index representing the overall macroprudential state of the economy with the argument that their empirical estimation is not capable of specifying the effect of a particular macroprudential loosening or tightening. More precisely, their approach would only allow them to estimate the impact in the quarter after a change in the macroprudential policy stance occurred. Furthermore, prudential policy measures conducted in consecutive quarters would introduce a bias on the estimation results when adjusting to prudential policy measures lasts longer than a quarter (time bias). Given that, in particular, (potential) borrowers are not expected to be informed about every change in the prudential policy stance, a full adjustment of the endogenous variable within a quarter is questionable. We propose an empirical method that is capable of dealing with these issues.

Put differently, this paper differs from the bulk of the literature by examining the role of flow variables rather than stock variables. To bridge the gap between our empirical approach and the one primarily observed in the literature, we nevertheless first apply the standard approach to our data set, where we look at the overall status of the prudential policy. Afterward, we estimate how the marginal effect of a prudential easing and tightening

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[5]These comprise, among others, Akinci and Olmstead-Rumsey (2018) and Cerutti, Claessens and Laeven (2017).

develops over time.

3.1 Estimation with a Prudential Policy Index

Our panel data set generally covers all 19 EMU member states from the date of accession to 2018:Q4 on a quarterly frequency. However, depending on the endogenous variables' data availability, the sample does not always cover every member state.

Following Akinci and Olmstead-Rumsey (2018), we introduce a Prudential Policy Index (PPI) that displays the tightness of the policy relative to a base period for every EA country. In our case, the establishment of the euro serves as the base date. Hence, the PPI is zero at the start of 1999:Q1.[6] In every period in which policymakers tighten (loosen) at least one measure, the PPI increases (decreases) by one. As in Akinci and Olmstead-Rumsey (2018) and Cerutti, Claessens and Laeven (2017), the data set does not allow us to take a stand on the intensity of a policy change. In principle, one could also sum up the number of measures that became tighter (looser) within a quarter. However, since most of these simultaneous changes in different measures are, in fact, a package of measures, and we do not capture the intensity of changes, we refrain from that. When in a given quarter, no changes in the prudential policy stance are conducted, the PPI series remains unchanged. This also holds for periods where loosening of some measures and tightening of other measures happened simultaneously.

As outlined above, we distinguish between announcement and implementation. Hence, we separately create the PPI series for a case where the announcement date and a case where the implementation date is decisive for constructing the PPI. Figure 1 displays the PPI according to the implementation date on a national basis. Overall, a strong tendency of a tightening over time is observed. Until 2018:Q4, Austria tightened the most relative to the base period with a PPI of 15, followed by Ireland and Latvia with a PPI of 13 and 12, respectively. On the lower end, Greece and Estonia appears to be the only countries with a somewhat looser prudential policy in 2018 in comparison to 1998:Q4 (net tightening of -2 and -1, respectively). The development of the EA mean PPI is outlined in Figure 2.[7] Hereby, we distinguish between the two PPI series. We again observe the tendency of a

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[6]For reasons of comparability, this also holds for countries that entered the EMU after 1999.

[7]Note that the PPI series represents the mean of all observations, i.e. it is not a (GDP-)weighted average. However, an index that is based on a GDP-weighted average yields similar results.

tighter prudential policy over time for both series. In line with our expectations, the announcement PPI series is generally a leading indicator for the implementation PPI series. However, since 2016:Q2 the implementation index exceeds the announcement index. The reasoning behind this is that several measures were announced at the same point in time, but their implantation date varied.[8]

We follow the empirical literature and focus on the response of credit growth and house prices to prudential policy changes. Data on credit is available at the Bank for International Settlements (BIS). We consider the credit to the non-financial sector from all sectors at market value. We merge this data set with the ECB's data set on non-financial cooperation debt in order to reduce the number of missing values.[9] Data on housing prices are more difficult to find. Concerning the real estate type, the considered area (capital city or the whole country) and the frequency, the BIS' data set is not consistent across all EA countries. The ECB's data set does not capture observations prior to 2005. As the number of treatment events is limited, we do not want to lose observations by further cutting the sample. Therefore, we use actual rentals for housing from the HICP as a proxy for house prices as rents are a fundamental determinant of the value of housing, see e.g. Brunnermeier and Julliard (2008) and Plazzi, Torous and Valkanov (2010). Specifically, the present value of its future rents determines the price of a commercial property from an asset pricing perspective. Besides the fundamentals, a bubble term drives house prices. Hence, rents can be thought of as a proxy for the underlying fundamental value of house prices only. Empirically, Manganelli, Morano and Tajani (2014) find that house prices affect rents in Italy. For the US, Gallin (2008) showed that the house price-to-rent ratio is a reliable indicator of the valuation in the housing market. Additionally, we rely on a number of control variables. Namely, we include the national output gaps, the short-term money market rate and the CBOE Volatility Index (VIX) in the analysis below. The corresponding data sources are Eurostat and the Fred Database, respectively. The shadow rate by Wu and Xia (2016), which is available from 2003:Q3, allows us to deal with the zero lower bound (ZLB). From 1999:Q1 to 2003:Q3, the Eonia serves as our short term

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[8]To be more illustrative, suppose that one country announces the tightening of two measures simultaneously. The series for national PPI announcements increases by one as a tightening of at least one measure was announced. In contrast to that, the PPI series for implementation will increase by one for each implementation. Hence, the total increase in the PPI is two, if the implementation dates of the two measures differ.

[9]Priority is given to the BIS' data set.

interest rate, which we receive from Thomson Reuters Datastream.[10]

Following Akinci and Olmstead-Rumsey (2018), our estimation equation can be described by (1). The quarter-on-quarter credit or rents growth rate for country i at time t is the endogenous variable which is regressed on a country-specific constant, its own lagged values, a number of control variables and the PPI, which we are primarily interested in. The error term is described by $u_{i,t}$. We reduce endogeneity as much as possible by analyzing the lagged values of the PPI. In a similar manner, we generally consider lagged values of the control variables. In line with Akinci and Olmstead-Rumsey (2018), we include three lags of the endogenous variable into our regression./footnoteThe results are not sensitive to other lag lengths for the endogenous variable. The (log) VIX is the only variable that is allowed to have a contemporaneous influence on credit growth since its value is determined by the US market. However, lagging the VIX does not substantially alter the results. In contrast to Akinci and Olmstead-Rumsey (2018), we consider GDP gaps rather than GDP growth rates because GDP gaps are better suited to capture the business cycle's current state. Finally, our last control variable is the (lagged) change in the short term interest rate with respect to the quarter one year ago.

$$Y_{i,t} = c_i + \sum_{j=1}^{3} \rho_j \cdot Y_{i,t-j} + \alpha \cdot VIX_{i,t} + \beta \cdot GDP_{t-1} + \delta \cdot interest_{i,t-1} + \gamma \cdot PPI_{i,t-1} + u_{i,t} \quad (1)$$

To further reduce the endogeneity of PPI in equation (1), Akinci and Olmstead-Rumsey (2018) apply an Arellano-Bond[11] (AB) General Methods of Moments (GMM) estimator. Yet, this estimator is only unbiased when the number of countries (N) exceeds the time dimension (T), which is not the case here. A feasible alternative is a bias-corrected Least Square Dummy Variable (LSDV) estimator.[12] We adopt the bias-correction by Bruno (2005), which can also be applied to unbalanced panels.

The results of the bias-corrected LSDV estimator are displayed in Table 2. For the sake of comparison, we also present evidence for the Blundell-Bond estimator[13] (Table 3) and

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[10]A complete list of all variables and their sources can be found in Table 1.

[11]The dynamic panel estimator was introduced by Arellano and Bond (1991).

[12]In fact, Monte Carlo simulations by Judson and Owen (1999) indicate that the bias-corrected LSDV estimator is preferable to GMM estimators when N is relatively small.

[13]The Blundell-Bond estimator is an extension of the AB estimator that performs better under a limited

for the non-adjusted LSDV estimator (Table 4). The latter's results match those of the bias-corrected estimator quite well, indicating that the correction is not substantial. Credit growth is positively dependent on the previous two periods' credit growth, while all other control variables have no significant impact. These findings hold for all three models. Our variable of interest, namely the PPI, display the expected negative sign and is statistically significant on a confidence interval of 99%. An increase of the PPI Implementation series, for instance, reduces credit growth by 0.146% on average, indicating that tighter prudential policy regimes reduce credit growth in general. In line with our expectations, the impact of the PPI announcement series is of similar magnitude.

A somewhat different picture arises for rents. Growth of rents is positively influenced by its lagged value of orders one and three. Increases in global uncertainty, measured by the VIX, and changes in the short term money market rate, do not significantly affect rents. Higher output levels are associated with higher rents, as indicated by the significant GDP gap coefficient. This finding describes demand side effects on the housing market. The effect of the PPI on rents is less pronounced than its effect on credit growth. Although the expected negative sign is observed, its impact is insignificant. A plausible explanation for this finding is that bank lending is only one of many determinants of house prices and rents. Therefore, a reduction of credit growth due to tighter prudential policies does not necessarily decrease rents one-to-one. Put differently, prudential policy makers have better control over credit growth than over rents. All results hold for both equations, i.e. regardless of whether the implementation or the announcement PPI is implemented in the model.

The interpretation of the results presented above is only valid when the endogenous variable fully adjusts within one quarter after the change. To see that, consider a case where a country tightens monetary policy but then eases twice in the subsequent two quarters. If the endogenous variable takes some time to adjust, the tightening effect would be attributed to a lower PPI. Hence, the estimated results of equation (1) are biased. As we find no substantial difference between the announcement and the implementation index, it is tempting to conclude that the effects already appear after the announcement of prudential policies. However, this interpretation is misleading because we estimate the role of the prudential policy stance, which is by nature similar to announcement and implementation dates, and do not estimate effects after a change in the policy.

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sample size, see Blundell and Bond (1998).

3.2 Impulse Responses to a Prudential Tightening/Loosening

In this section, we outline a way to correct for the time bias in equation (1). This method further allows us to see how the endogenous variables respond to a prudential tightening or loosening over time. Yet, the method cannot solve the selection bias, which will be done in the following section.

It is common practice in the field of counterfactual analysis to make use of regression results to offset the effects of endogenous variables, see, e.g., Taylor (2007) and Mohaddes and Pesaran (2016). Building on that, we propose an iterative approach that disentangles the effects of a particular change in a prudential policy measure of the impact of preceding and subsequent policy changes. We proceed as follows. First, we introduce the underlying estimation equation. Afterward, we describe how we adjust results by the iterative approach.

In line with our research question, we want to assess how a prudential tightening (loosening) influences credit growth and rents over time. Hence, the left-hand-side of our estimation equation (2) is given by the percentage change of the endogenous variable from before a shift in prudential policies until q quarters after that shift. The explanatory variables we incorporate are country-fixed effects, a linear time-trend, a set of control variables and two dummy variables that indicate policy tightening and loosening events, respectively. More precisely, $D_{i,t}^T$ ($D_{i,t}^L$) equals one whenever country i announces the tightening (loosening) of at least one measure at time t. Since we correct for the time bias, lagging the dummy variables is not necessary. For the moment, we assume that the announcement and not the implementation of prudential policies move the endogenous variable. In principle, one could separately add dummies for the implementation. Yet, the iterative algorithm outlined below does not converge towards a local minimum under these circumstances. In section 4, we further disentangle the announcement from the implementation effect. The set of (lagged) control variables $X_{i,t-1}$ contains the change in the (shadow) short rate, output gap, year-on-year inflation, and the credit-to-GDP gap. All changes are expressed relative to the previous year. The former two variables are identical to those from section 3.1. Inflation is taken from Eurostat. The credit-to-GDP gap series is derived from the BIS. Gaps in the time series are filled by the ECB's data set on "non-financial cooperation outstanding debt to GDP". Analogously to the BIS data, we receive gaps by applying an HP-filter with a λ of 400,000.[14]

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[14]See Table 1 for an overview of all variables used throughout this paper and their sources.

$$\frac{Y_{i,t+q} - Y_{i,t-1}}{Y_{i,t-1}} = c_i + \gamma_{1,q} \cdot D_{i,t}^T + \gamma_{2,q} \cdot D_{i,t}^L + \beta \cdot X_{i,t-1} + \delta \cdot t + u_{i,t} \tag{2}$$

The iterative approach proceeds as follows. First, we estimate equation (2) using the biased-corrected LSDV estimator. In a local projections[15] style, we vary the time horizon of the change in the dependent variable. We assume that the endogenous variable fully adjusts within four quarters. Hence, we separately estimate equation (2) for every possible $q \in \{0, 1, 2, 3, 4\}$ and always save the coefficients $\gamma_{1,q}$ and $\gamma_{2,q}$.[16] With those estimates at hand, we can calculate hypothetical values of the endogenous variables under the assumption that a particular shift in prudential policies had not happened. Thus, we can discern between two changes in the prudential policy stance conducted in a short period of time. We then manipulate the endogenous variable to offset the effects of prudential policy measures conducted earlier or thereafter. In an iterative process, we rerun all the regressions and readjust the endogenous variable until the adjustment is negligible. Our algorithm stops when the change in $\gamma_{1,q}$ and $\gamma_{2,q}$ for $q \in \{0, 1, 2, 3, 4\}$ is below .0001 for every parameter.

To be more illustrative, consider the following example. Austria announces a tightening of one measure in 2010:Q1 and again in 2010:Q4. In the first round, equation (2) is separately estimated for all permissible q. In that estimation, we refrain from these two tightening events' interaction effect on the endogenous variable. Obviously, this estimation is biased as, for instance, the movement in the endogenous variable in 2010:Q4 is, in fact, a combination of the responses to both tightening events. If the effects of these two tightenings exactly equal the average tightening effect and no white noise are present, the increase in the endogenous variable $\frac{Y_{i,t} - Y_{i,t-1}}{Y_{i,t-1}}$ in 2010:Q4 is given by $\gamma_{1,0} + \frac{\gamma_{1,3} - \gamma_{1,2}}{1 + \gamma_{1,2}}$. The latter term describes the effect that the announcement in 2010:Q1 had on the credit growth between 2010:Q3 and 2010:Q4. Vice versa, the effect of the 2010:Q1 and the 2010:Q4 tightening in 2010:Q4 is $\frac{Y_{i,t} - Y_{i,t-1}}{Y_{i,t-1}} - \frac{\gamma_{1,3} - \gamma_{1,2}}{1 + \gamma_{1,2}}$ and $\frac{Y_{i,t} - Y_{i,t-1}}{Y_{i,t-1}} - \gamma_{1,0}$. Hence, we ultimately will have an unbiased estimator, when we subtract the effects of previous and preceding measures. For the deduction process, we first consider the estimates for $\gamma_{1,q}$ and $\gamma_{2,q}$ from the initial estimations. We then iteratively reestimate equation (2) and adjust the subtraction parameters until the convergence condition is met.

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[15]Local projections were introduced by Jorda et al. (2005). Jorda, Schularick and Taylor apply this estimation technique to a panel data set.

[16]Note that, in contrast to local projections, the variable of interest is not a purely exogenous shock.

Figure (3) plots the mean estimator along with their 90% confidence bands for $\gamma_{1,q}$ and $\gamma_{2,q}$ as a function of q. The upper panel displays the average response of credit growth to shifts in the prudential policy stance. In line with our expectations, a tighter (looser) prudential policy stance decreases (increases) credit growth. The size of the effect is similar for tightening and loosening. It is only significant in the first period. The lower panel of Figure (3) shows the responses of rents to a change in prudential policy. In line with the estimations from section 3.1, we observe no significant impact of prudential policies on rents. However, one has to be very cautious with the interpretation here as the LSDV approach might still suffer from the selection bias.

4   A Propensity Score Matching Approach

As described above, the LSDV panel analysis only mitigates the endogeneity issue. Least squares estimation might still suffer from a selection bias for two reasons. First, the least-squares estimator requires a linear relationship between the endogenous and exogenous variables with a known functional form, e.g., lag-structure. Biased estimates occur whenever the regression is based on an incorrect functional form. In contrast to that, the propensity score matching (PSM) approach used below does not require a precise functional form, which is of special interest in the field of prudential policies where the empirical literature is still in its infancy. Second, the least-squares estimator does not put high weights on those observations with similar economic fundamentals. It weighs observations higher that have a more equal distribution between receiving and not receiving the treatment, i.e., tightening and non-tightening of a prudential policy measure. As opposed to that, the PSM approach puts the highest emphasis on those observations with a high treatment probability that do not receive the treatment. This way, structural differences between observations with and without treatment are minimized.

We start by summarizing the methodology. Afterward, we estimate which macroeconomic variables influence the likelihood of a prudential tightening/loosening, i.e., the first stage of the PSM approach. Finally, we show how the endogenous variables (credit growth and house prices) react to a change in the prudential policy stance, i.e., the second stage of the PSM approach.

4.1 Methodology

Propensity score matching estimates the effect of a binary treatment on an endogenous variable, the so-called average treatment effect (ATE). In our case, we set up a binary variable for the tightening of prudential policy measures. Intuitively, we find a matching partner for every tightening event that has the same probability of tightening but does not alter its policy stance. The difference of the endogenous variable gives the impact of the tightening. Taking the average over all events results in the ATE. The binary variable $D_{i,t} = \{0,1\}$ defines whether the observation of country i at time t belongs to the treatment ($D_{i,t} = 1$) or the control group ($D_{i,t} = 0$). Now let $Z_{0,i,t}$ be the outcome of the endogenous variable if country i decides not to carry out any prudential policy action in t and $Z_{1,i,t}$ be the outcome if policy makers tighten at least one measure. Apparently, we only observe one of these outcomes in every period, namely $Z_{1,i,t}$ for the treatment and $Z_{0,i,t}$ for the control group. Put differently, $Z_{1,i,t}|D_{i,t} = 1$ and $Z_{0,i,t}|D_{i,t} = 0$ are known, while $Z_{1,i,t}|D_{i,t} = 0$ and $Z_{0,i,t}|D_{i,t} = 1$ are unknown. Consequently, we are able to identify differences between the two observable variables, the left-hand-side of equation (3).

$$E[Z_{1,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0] = E[Z_{1,i,t} - Z_{0,i,t}|D_{i,t} = 1]$$
$$+ [E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]] \tag{3}$$

The right-hand side of equation (3) consists of the ATE, ($E[Z_{1,i,t} - Z_{0,i,t}|D_{i,t} = 1]$), and a selection bias, ($[E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]]$). The former term describes the expected value of the differences between the observed outcome of the endogenous variable and the hypothetical outcome if the tightening had not occurred for each tightening event. Hence, this term measures the average effect of a policy tightening on the endogenous variable. The selection bias measures the part of $[E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]$ that stems from structural differences between the treatment and the control group. It is zero only if the sample is free of pre-treatment differences between the two groups. However, this is very unlikely in the case of macroeconomic variables as policy changes happen for a reason. For instance, a prudential tightening is expected to occur primarily when excessive bank lending or a mortgage boom is present. Not accounting for these circumstances leads to biased estimation results.

According to Rosenbaum and Rubin (1985), the selection bias is removed if treated variables are matched with control variables with the same probability of receiving the treat-

ment. This exactly describes the intuition behind the PSM methodology. Suppose that a matrix of exogenous variables $X_{i,t-1}$ exists that determines whether a prudential tightening occurs. In our case, the exogenous variables could be changes in interest rates or the credit-to-GDP ratio. Estimating the likelihood of a tightening, i.e., the propensity scores can then be achieved by a logit model according to equation (4). The influence of the exogenous variables are captured in Ψ and α is a constant. However, changes in the prudential policy stance might again have an effect on $X_{i,t}$. In order to overcome this endogeneity issue in the first stage of the PSM, we consider pre-treatment variables, i.e., we lag the exogenous variables by one quarter.

$$ln\left(\frac{Pr[D_{i,t} = 1|X_{i,t-1}]}{Pr[D_{i,t} = 0|X_{i,t-1}]}\right) = \alpha + \Psi \cdot X_{i,t-1} + \epsilon_{i,t} \tag{4}$$

The ATE is then given by $E[Z_{1,i,t} - Z_{0,i*,t*}|Pr[D_{i,t} = 1] \approx Pr[D_{i*,t*} = 1]]$ where $i*$ and $t*$ display country and time of the matching partner within the control group. Matching algorithms, such as nearest neighbor matching, ensure that the difference in the treatment probability between the treated variables and their matching partner is minimized. As we are interested in the development of the endogenous variable's response to a policy change over time, we estimate the ATE for different time horizons. Thus, we introduce the time horizon $q \in \{0, 1, 2, 3, 4\}$ into the endogenous variables $Z_{1,i,t}^q$ which is now given by equation (5). In line with section 3.2, $Y_{i,t}$ describes credit to the nonfictional sector or house prices for country i at time t.

$$Z_{1,i,t}^q = \frac{Y_{i,t+q} - Y_{i,t-1}}{Y_{i,t-1}} \tag{5}$$

Finally, we control for the time bias. For this task, we draw on the iterative approach described in section 3.2. We escape from the time bias in the logit estimation by including the number of tightening and loosening events in the previous year as exogenous variables. Hence, the time bias-correction only considers the second stage of the PSM approach. This procedure's advantage is that we do not have to limit the sample size in the first step already. As before, we first run the PSM estimation without any adjustments. For each change in the prudential policy, the estimated coefficients allow us to control for other policy measures that also might affect the endogenous variable. We iteratively reestimate all equations and then update the coefficients we apply to control for other policy changes. We assume that convergence is achieved when the change of every coefficient between

iterations is below .0001.

4.2 Logit Estimation

Since calculating reasonable propensity scores is crucial for the correct estimation of the ATE, the logit model deserves some special attention. The binary variable $D_{i,t} = \{0, 1\}$ in equation (4) is one (zero) whenever a tightening of at least one measure and no loosening of any other measure was announced (whenever neither a tightening nor a loosening of any prudential policy measure was announced). Furthermore, we have to discard entries that would lead to biased estimation. We drop observations whenever the implementation of a before announced policy change occurs.

Via $X_{i,t-1}$ on the right-hand side of equation (4), we estimate the influence of macroeconomic variables on the likelihood of a policy tightening. The list of exogenous variables covers the shadow (short) rate, GDP gap, headline inflation, credit-to-GDP gap and changes in rents. As before, changes refer to the previous year. These variables have already been introduced in the previous section. Prudential policies are conducted by national authorities and EA/EU institutions. Hence, we always separately include national and EA-wide variables. Furthermore, systemic risk lets policymakers change the prudential policy stance. For the EA, we include changes in the Composite Indicator of Systemic Stress (CISS). On a national basis, the CISS is not available for every country. We overcome this issue by relying on the Country-Level Index of Financial Stress (CLIFS). We receive both indices from the ECB's statistical data warehouse. As described above, we lag all exogenous variables by one lag to minimize possible endogeneity issues. Finally, we incorporate a linear time trend as well as the number of tightenings and the number of loosenings in prudential policies over the previous year.

The results of this exercise are outlined in Table (5). We find that the prudential policy primarily reacts to EA-wide developments. On a 10% significance level, the probability of a policy tightening raises with a higher EA credit-to-GDP gap and increasing systemic stress as indicated by the CISS. Moreover, prudential policy is empirically a complement of monetary policy, as both tend to tighten simultaneously. Finally, the likelihood of a policy tightening increases with the number of tightening events in the previous year. All these results are plausible. As all national variables do not significantly alter the likelihood of a policy tightening, it is tempting to conclude that solely EA variables lead to changes

in prudential policy measures. However, this is too short-sighted as the national credit-to-GDP gap is just not significant.

4.3   Average Treatment Effect

With the propensity scores at hand, we estimate the ATE. However, we first have to discard some observations. For the logit estimation, we identified 109 announcements of a prudential policy tightening across EA member states. Since we are not able to estimate an ATE for loosening in the prudential policy, we exclude all periods when a loosening of at least one measure is present. Throughout the paper, we assume that the full adjustment to a prudential policy change happens within a year.[17] Therefore, we have to exclude the four periods after each loosening event as well. As the model does not allow us to specify the implementation effect of a measure announced before, we also exclude the four periods following these implementations. This exclusion further reduces the amount of policy tightening events to a total number of 45. Additionally, the matching approach requires that all observations must have a treatment probability in the interval $[0, 1]$. Since the probabilities are within 0.006 and 0.250, this requirement is met without any adjustment.

An observation is placed in the control group whenever neither a tightening nor a loosening has been announced or implemented in the respective quarter or the four quarters before. We count 911 observations in the control group. Finally, we set up an exclusion period for every treatment observation. More precisely, an observation in the treatment group can only be matched with an observation of the same country if they differ by at least one year. This is necessary as some exogenous variables in the first stage of the regression refer to changes over the previous year. Hence, two observations of a country within a year are also likely to have a similar treatment probability. Estimating the treatment effect out of these two observations is troublesome when the endogenous variable does not fully adjust within a quarter.

We present evidence based on three different matching approaches, i.e., the nearest neighbor, radius and kernel matching. For each of these identification strategies, we show that the results are robust to variations in the number of matching partners considered. For every observation in the treatment group, the nearest neighbor approach considers the n

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[17]As outlined below, this assumption is in line with our estimation results.

observations in the control group with the smallest difference in the treatment probability. The radius matching considers all observations that are within a given radius around each treated variable. For these two matching approaches, each observation's treatment effect is then given by the difference in the endogenous variable between the treatment variable and the average of the matching partners. Put differently, each identified matching partner receives an equal weight, while all other observations in the control group receive a weight of zero. In contrast to that, the kernel approach assigns positive weights to all observations. In this approach, variables in the control group that are more similar to the treated variable receive higher weights. As the names suggest, the weighting is achieved through a kernel function. We consider an Epanechnikov Kernel[18] as outlined by equation (6), where s is the adjusted difference in the propensity score between the treatment and the control variable. The adjustment is achieved by multiplying the difference with $\frac{1}{bw}$, where bw is a pre-specified bandwidth. Our sample's highest probability of a policy tightening is 0.25, which serves as our benchmark bandwidth. We check the robustness of this bandwidth by considering lower and higher values, i.e., 0.05 and 0.5. Finally, all weights are rebased via the rule of proportion so that they sum up to one.

$$K(s) = \frac{3}{4}(1-s)^2 \tag{6}$$

The ATE is less sensitive to effects stemming from the matching partner by considering a higher number of matching partners, a wider radius or a larger bandwidth. On the other hand, relying on matching partners that have more different propensity scores also increases the likelihood that observations with structural differences are matched. For all matching methods, the ATE is then given by the average differences between the treated variables and the (weighted) average of their matching partner.

Before we turn to the estimation results, we first evaluate whether the matching approaches were actually able to remove substantial differences between the treatment and the control group. We present evidence based on the same variables that were included in the logit estimation.[19] Table 6 displays mean values for the treatment group and compares them with the mean of various control groups.[20] To save space, we only show results of one

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[18]Other Kernel functions lead to similar results.

[19]We refrain from the number of loosening events in the previous year as we exclude all observations one year after a loosening in the policy stance.

[20]For the nearest neighbor and the radius matching, the control group consists of all observations that serve at least once as a matching partner. As outlined above, the kernel matching approach puts a

set-up for each matching method, i.e., we set the "number of nearest neighbors" n to five, the radius to 0.01 and the bandwidth to 0.25. These are the median values of the models outlined below. Other set-ups yield similar results. Before any matching, the untreated observations differ significantly from the treatment group among seven of 13 variables on a 5% significance level, see Table 6. The matching approaches were able to remove some of these differences. The radius matching and the kernel matching algorithm are the most successful. Only two variables differ significantly across the two groups. The nearest neighbor matching still exhibits structural differences among four variables. Although the matching approach reduces overall differences between control and treatment groups, some differences remain.

We now evaluate the impact of a tightening in the prudential policy stance. The ATE as a function of q and the corresponding 90% confidence bands are plotted in Figure 4. The confidence bands are generated via bootstrapping. The upper panel displays the effects on credit growth. We find that a tightening of the prudential policy stance reduces credit growth on average. The size of the effect varies with the matching method, but it is around 1% on impact for the majority of estimations. We see that credit adjusts within one quarter. In line with our expectations, we do not observe a hump-shaped response indicating that the effect does not vanish over time. Furthermore, only the initial impact is statistically different from zero in two of three considered matching methods. The lower panel of Figure (4 depicts the response of rents. Again we find that rents tend to decrease when prudential policy tightens. This finding is not significant on a 10% level. Compared to credit growth, the impact on rents is, additionally, of a smaller magnitude, i.e., around 0.25%. This is not surprising since house prices are also determined by other factors and adjust sluggishly.

Figures 5 and 6 allow us to take a stand on whether the announcement or the implementation of a prudential policy measure is decisive. Therefore, we calculate two separate ATEs based on the timing of the implementation. While the first only considers events characterized by an instant implementation, i.e., announcement and implementation occur in the same quarter, the second focuses on those announcements accompanied by an implementation in the following periods (delayed implementation). For the latter group, the implementation lagged on average 3.3 quarter behind. We count 24 instant implementation and 21 delayed implementation events. Due to the smaller sample size, estimation uncer-

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different emphasis on all untreated observations. The mean of the untreated is then calculated via the rebased weights.

tainty increases and error bands widen. Note that we do not alter the logit estimation. Thus, we implicitly assume that the macroeconomic circumstances influence the likelihood that a prudential policy tightening is announced but has no substantial impact on whether the implementation of the measure happens right away or with some delay. In any case, the instant implementation should reduce credit growth in the short to medium run, no matter whether the announcement or the implementation is decisive. From a theoretical point of view, the effect of a delayed implementation is unclear. On the one hand, banks might reduce lending at the time of the announcement so that they meet the regulatory criteria once they become binding. On the other hand, banks might increase lending in response to the announcement because they anticipate tighter policy in the future and thus move lending activities from the near future into the present. The latter channel is arguably in particular relevant for borrower based measures. Which of these two effects predominates is an empirical question in the end. According to Figure 5, there is indeed a tendency that instant implementations (upper panel) have stronger effects than delayed implementations (lower panel). This underpins that the implementation of a prudential tightening primarily moves credit growth. However, the differences between delayed and instant implementation are not statistically significant, possibly reflecting the small sample sizes. The fact that delayed implementations have smaller effects on credit growth on average might also indicate that the announcement of some measures, e.g., borrower based measures, lead to increases in credit growth in the short-run. If policymakers aim for the highest impact, they should not announce the implementation of future policy measures beforehand. However, this interpretation leaves out the fact that prudential policy makers, households and financial intermediaries are not playing a one-shot game. Similar to monetary policy, communication is potentially preferred when announcements reduce market uncertainty. According to Figure 6, the different effects on credit growth also tend to translate into different responses of rents. However, the responses of rents are again not significant.

Figures 7 and 8 consider the collaboration between national authorities and EU/EA institutions. Hence, we compare the ATE of measures that were conducted solely by national authorities (32 observations) and the ATE for measures where both, EU/EA institutions and national authorities are decision-makers (13 observations). Again, the outcome is not clear-cut from a theoretical viewpoint. National authorities might know their economy better and therefore pick prudential policy measures that are better suited for the domes-

tic market. However, missing international collaboration might open up the opportunity for globally active credit institutions to bypass tighter measures. Empirically, we find that credit growth responses are more pronounced when international institutions are behind these measures. Again for rents, the differences are less clear-cut, see Figure 8.

Finally, we differentiate between micro- (25 observations) and macroprudential policy measures (16 observations). In principle, we would expect that macroprudential policy has a greater impact since it is targeted towards the entire financial system. According to the Figures 9 and 10, we observe the opposite. Only microprudential measures are capable of reducing credit growth and rents. However, one has to keep in mind that the survey by Budnik and Kleibl (2018) only includes microprudential measures that are "likely to have a significant impact on the whole banking system".

5 Conclusions

In this paper, we study the role of prudential policies on bank lending and rents in the EA from an empirical point of view. We, therefore, draw on the data set provided by Budnik and Kleibl (2018). For each member state, this data set lists all dates of announcement and implementation for every macroprudential measure as well as for microprudential measures that are "likely to have a significant impact on the whole banking system".

Building on the established empirical literature of macroprudential policies, we first analyze how the economy's overall prudential stance influences rents and credit growth via panel estimation. We evaluate the tightness of the prudential policies compared to a base period by summing up over policy changes.[21] Our results suggest that tighter prudential policy reduces credit growth significantly. The effect on rents also displays the expected negative sign but is insignificant. However, one has to be cautious since the estimation is subject to a time bias and a selection bias.

We then subsequently remove the time bias and selection bias. The correction of the time bias allows us to evaluate how a prudential tightening or loosening affects the endogenous variable's development over time. Leaving aside the selection bias correction, we observe plausible negative mean responses of credit growth and rents to a prudential tightening.

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[21]More precisely, we sum over all prudential tightening events and deduct all loosening events.

Finally, we build on a propensity score matching approach to remove the selection bias. The propensity scores for prudential policy changes are based on a logit model. We include 14 exogenous variables that potentially determine the likelihood of a policy tightening, which consists of national and EA aggregate data. According to the estimation results, primarily the EA variables determine the prudential policy stance. More precisely, the credit-to-GDP gap, as well as changes in the CISS and the policy rate, are all positively related to the likelihood of tighter prudential policy. Based on these propensity scores, matching approaches find for each tightening event one or more partners from the control group that have a similar probability of tightening. We present evidence that matching approaches are indeed capable of reducing the selection bias. The propensity score matching's estimation results show that a prudential policy tightening significantly decreases credit growth by approximately 1% on average. The adjustment happens right away. Although rents also tend to decrease after a prudential tightening, we find no significant relationship.

In line with our expectations, we find that the effects are stronger when policy measures are directly implemented and not communicated before. We further observe that measures that rest on an EA/EU basis but were implemented nationally have a more substantial impact on credit growth than measures conducted solely on a national mandate. This is a plausible result in highly integrated European markets. Finally, microprudential policy measures that are "likely to have a significant impact on the whole banking system" display somewhat more pronounced effects on credit growth than macroprudential policy measures.

This paper's lessons for policymakers are manifold. First, policymakers are capable of altering bank lending. Second, policymakers have to internalize that the market adjusts fast, i.e., within one quarter, to changes in the prudential policy stance. Third, if policymakers aim for the biggest impact, they should not communicate policy changes before. However, this interpretation leaves out the fact that policymakers, financial intermediaries and agents are not playing a one-shot game. In fact, the announcement of measures might be welfare maximizing if communication reduces market uncertainty and volatility. Fourth, as measures based on an EA/EU mandate have a stronger impact, international cooperation is important.

Several expansions to this paper would be fruitful but are currently not feasible. In par-

ticular, one would prefer to further disentangle measures initiated on an EU-level from measures initiated on an EA-level. However, doing so would require us to incorporate aggregate data on EU and EMU levels into the logit model, which would lead to almost perfect collinearity. Moreover, we are not able to quantify measure-specific effects due to limited data availability. A starting point would be to decompose borrower-based from lender-based measures. However, the subset of borrower-based measures is too small to calculate any effect robustly. In fact, only 9 of the 59 observations describe borrower-based measures. For the same reason, we cannot discern between tightening and loosening of prudential policy measures.

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A   Data Sources and Definitions

Table 1: Data Sources

Data Series	Source	Description
Bank Assets to GDP	ECB	
CBOE Volatility Index VIX	FRED St. Louis	
Credit	BIS	Credit to the non-financial sector
Credit	ECB	Non financial cooperation debt
Credit-to-GDP	BIS	Credit to the non-financial sector
Credit-to-GDP	ECB	Non financial cooperation debt to GDP
Eonia	Datastream	Euro Over Night Index Average
GDP GAP	Eurostat	
House Prices	BIS	
Inflation	Eurostat	
Real Effective Exchange Rate	BIS	Broad Index
Shadow (Short) Rate	Wu and Xia (2016)	

B   Figures and Tables

Figure 1: Prudential Policy Index across countries
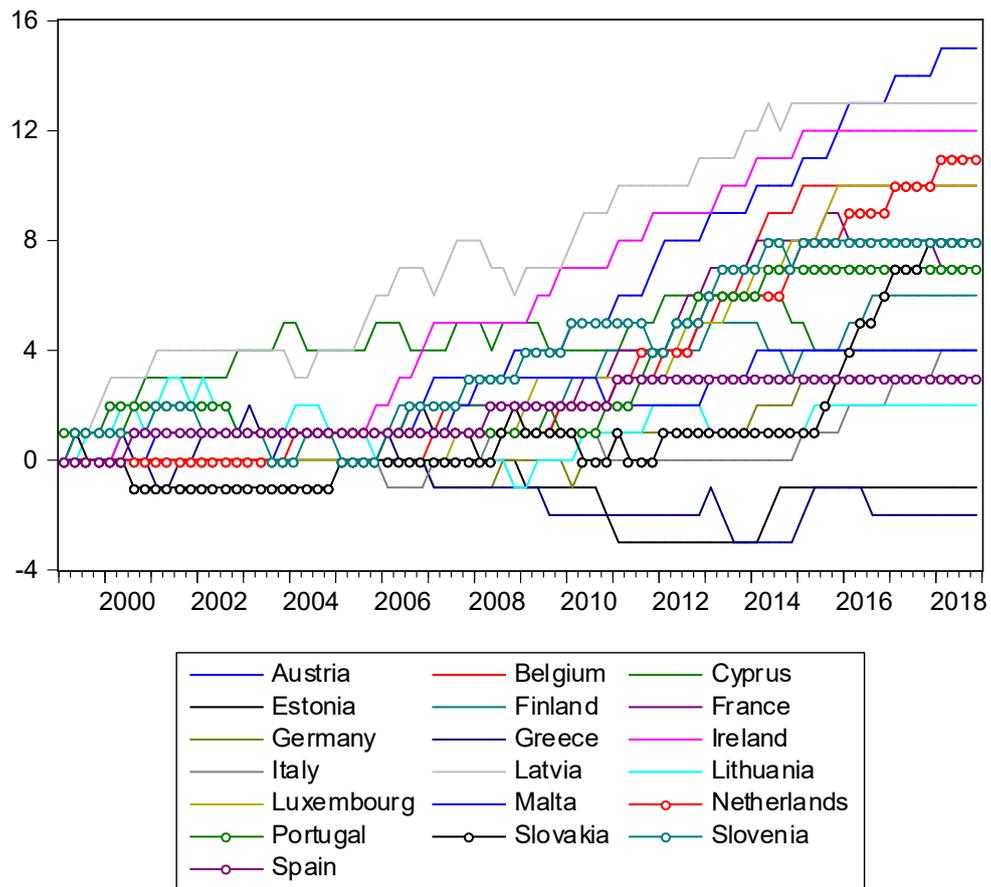
Figure 2: Prudential Policy Indices: comparison of means
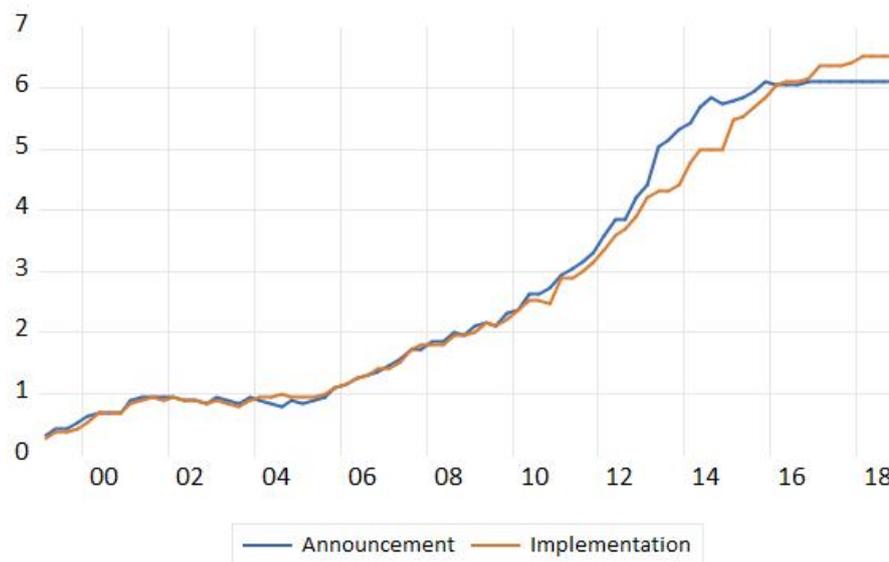
Table 2: Panel estimation results for PPI (bias-corrected LSDV estimator)

	$Y_t =$ Credit Growth		$Y_t =$ Rents Growth	
Y, t-1	0.146***	0.147***	0.147***	0,147
Y, t-2	0.090***	0.091***	-0,031	-0,031
Y, t-3	-0,013	-0,013	0.155***	0.155***
VIX (log)	-0,013	0,018	-0,149	-0,143
GDP Gap, t-1	-0,042	-0,048	0.069***	0.069**
Pol. Rate chg., t-1	0,035	0,037	-0,015	-0,015
PPI Impl., t-1	-0.129***		-0,003	
PPI Announc., t-1		-0.122***		-0,001

Notes: The panel estimation relies on the bias-corrected LSDV estimator by Bruno (2005). We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

Table 3: Panel estimation results for PPI (Blundell-Bond estimator)

	Y_t = Credit Growth		Y_t = Rents Growth	
Y, t-1	0,108	0,110	0,136	0,137
Y, t-2	0,071	0,072	-0,023	-0,023
Y, t-3	0,004	0,004	0.167***	0.167***
VIX (log)	0,079	0,107	-0,235	-0,229
GDP Gap, t-1	-0,031	-0,038	0.079**	0.079**
Pol. Rate chg., t-1	0,020	0,022	-0,020	-0,020
PPI Impl., t-1	-0.148***		-0,005	
PPI Announc., t-1		-0.143***		-0,003

Notes: The panel estimation relies on the estimator by Blundell and Bond (1998). We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

Table 4: Panel estimation results for PPI (LSDV estimator without bias-correction)

	Y_t = Credit Growth		Y_t = Rents Growth	
Y, t-1	0.146***	0.129***	0.129***	0.129***
Y, t-2	0.091***	0.092***	-0,032	-0,032
Y, t-3	-0.013	-0,010	0.155***	0.155***
VIX (log)	-0,013	0,021	-0,146	-0,140
GDP Gap, t-1	-0,042	-0,049	0.071***	0.070***
Pol. Rate chg., t-1	0,035	0,038	-0,015	-0,015
PPI Impl., t-1	-0.129***		-0,003	
PPI Announc., t-1		-0.123***		-0,001

Notes: The panel estimation relies on an LSDV estimator. We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

Figure 3: Panel estimation results: responses to changes in the prudential policy
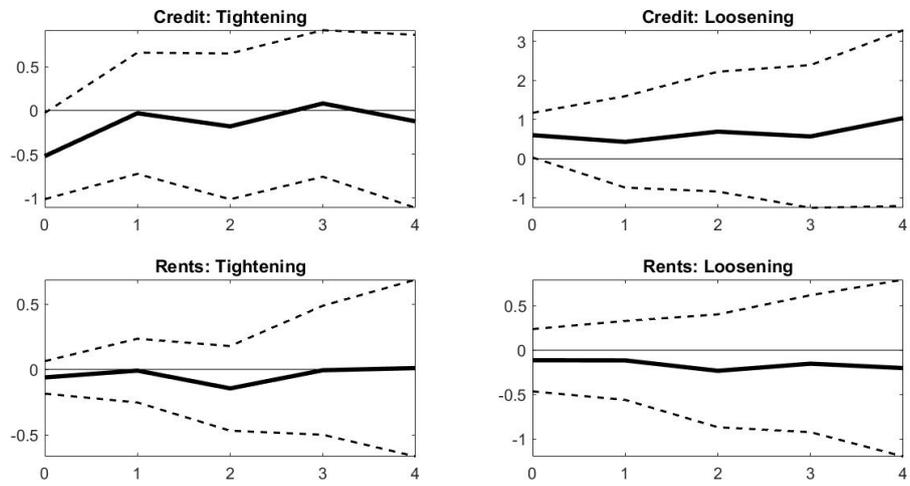
Notes: The panel estimation relies on the bias-corrected LSDV estimator by Bruno (2005). We include country-fixed effects and consider robust standard errors clustered by country. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines.

Table 5: Logit Estimation

Variable	Coef.	p-value
Const.	-4.4508	0.0007
Pol. Rate Change	0.3027	0.0073
Nat GDP Gap	-0.0115	0.8827
EMU GDP Gap	-0.2537	0.1281
Nat. Inflation	-0.1111	0.3874
EMU Inflation	0.2572	0.2602
Nat. Cr/GDP Gap	0.0223	0.1014
EMU Cr/GDP Gap	0.1821	0.0000
Nat. Rents Change	-0.0368	0.3973
EMU Rents Change	0.4103	0.4603
Nat. CLIFS Change	-0.3568	07181
EMU CISS Change	1.2023	0.0635
Lin. Trend	0.0291	0.0103
# Loosening prev. Year	-0.1185	0.5390
# Tightening prev. Year	0.2690	0.0397

Notes: $D_{i,t} = \{0,1\}$ is the dependent variable. It is one whenever a tightening of at least one measure and no loosening of any other measure was announced and zero otherwise. After all adjustments, we are left with 109 observations for $D_{i,t} = 1$ and 911 observations for $D_{i,t} = 0$.

Table 6: Differences between treatment and control group

|	Prior to Matching		Control Group After Matching		
	Treated	Untreated	Near. Neighb.	Radius	Kernel
Pol. Rate Change	-0,1451	-0,6283**	-0,6391**	-0,5606*	-0,5696*
Nat GDP Gap	-0,4382	0,0671**	-0,2574	-0,2775	0,0236
EMU GDP Gap	-0,2166	0,0079	-0,1089	-0,1321	-0,0157
Nat. Inflation	1,8190	1,8354	2,0149	2,0328	1,9025
EMU Inflation	1,9049	1,6898**	1,8116	1,9288	1,7747
Nat. Cr/GDP Gap	6,0560	1,6290***	2,4212**	3,2993	2,1003**
EMU Cr/GDP Gap	2,1345	-0,4187***	-0,3812***	-0,3284***	-0,2085***
Nat. Rents Change	1,2926	2,5485**	2,3873*	2,4023*	1,9684
EMU Rents Change	1,6097	1,5344	1,6341	1,6737	1,5657
Nat. CLIFS Change	-0,0035	-0,0011	-0,0045	-0,0054	-0,0023
EMU CISS Change	-0,0553	0,0047*	-0,0079	-0,0011	-0,0039
Lin. Trend	41,9333	23,8982***	37,2959	34,6233**	42,5138
# Tightening prev. Year	0,5111	0,5	0,2544**	0,3151*	0,4713

Notes: Comparison of structural variables between the treatment group and control groups. ***, ** and * display significant differences among the two groups on a 1%, 5% and 10% significance level, respectively.

Figure 4: Average Treatment Effect
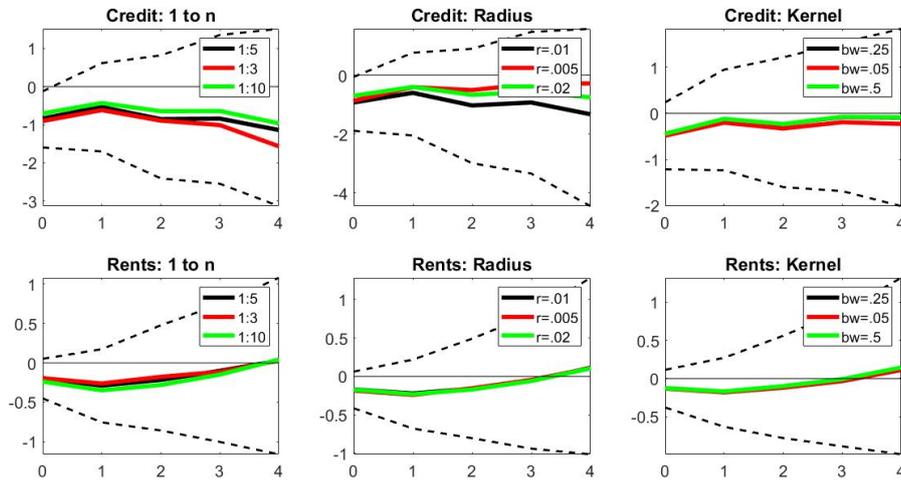
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of credit (rents) growth.

Figure 5: Credit: Instant vs. Delayed Implementation
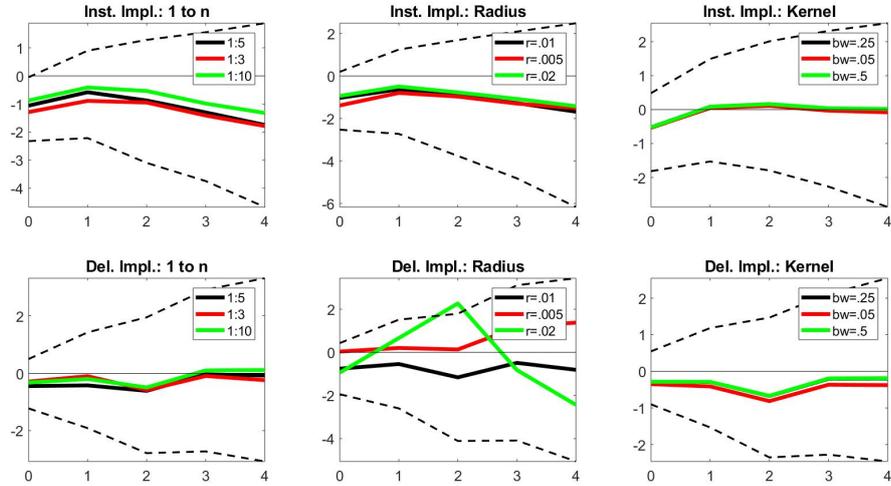
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of instant (delayed) implementation.

Figure 6: Rents: Instant vs. Delayed Implementation
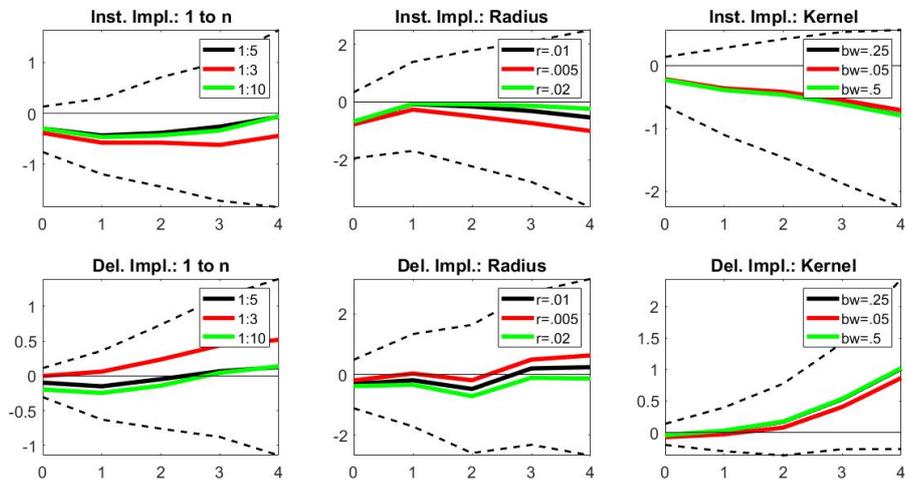
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of instant (delayed) implementation.

Figure 7: Credit: National vs. EU/EA Authorities
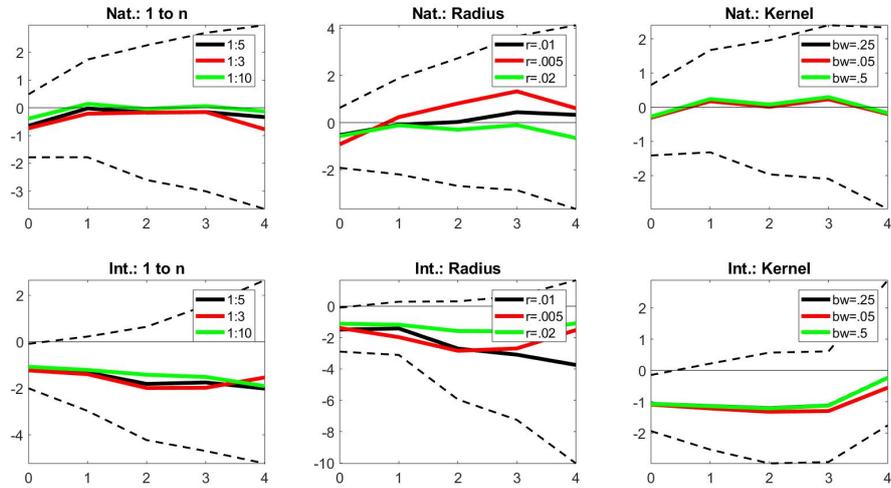
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from national authorities only (a cooperation of national and EU/EA authorities).

Figure 8: Rents: National vs. EU/EA Authorities
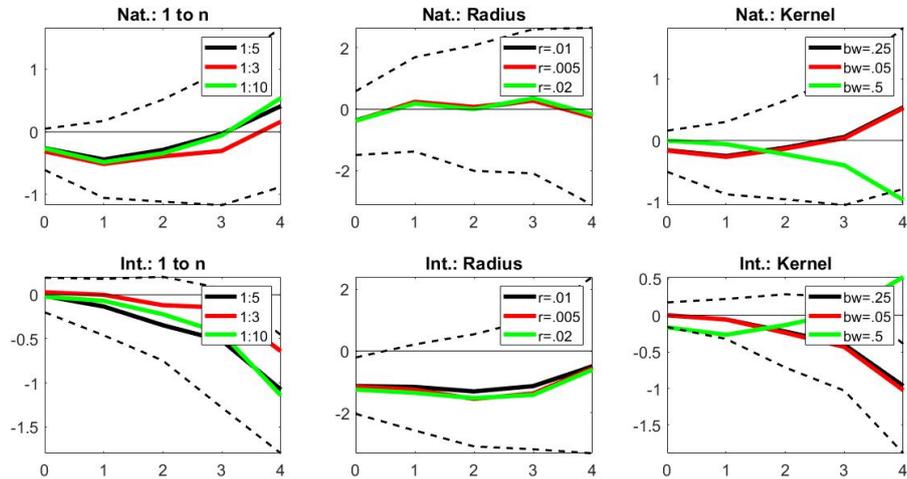
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from national authorities only (a cooperation of national and EU/EA authorities).

Figure 9: Credit: Micro- vs. Macroprudential Policy
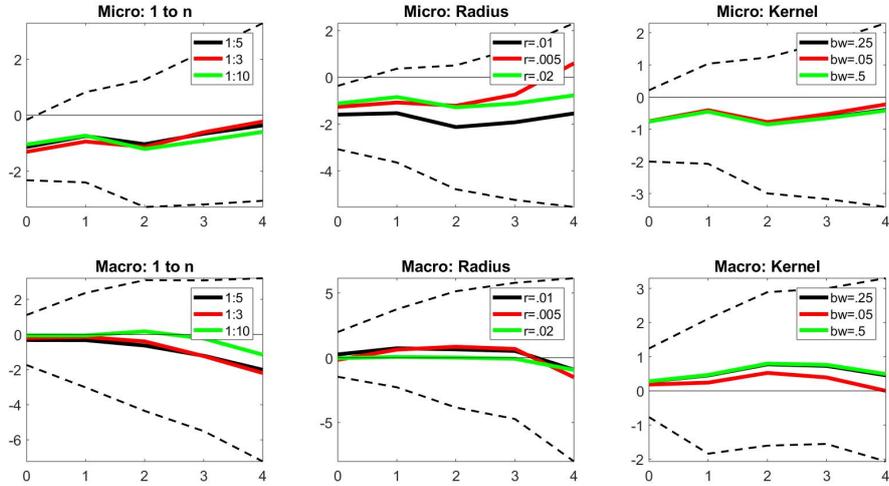
Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from microprudential (macroprudential) policy measures.

Figure 10: Rents: Micro- vs. Macroprudential Policy

Notes: The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from microprudential (macroprudential) policy measures.