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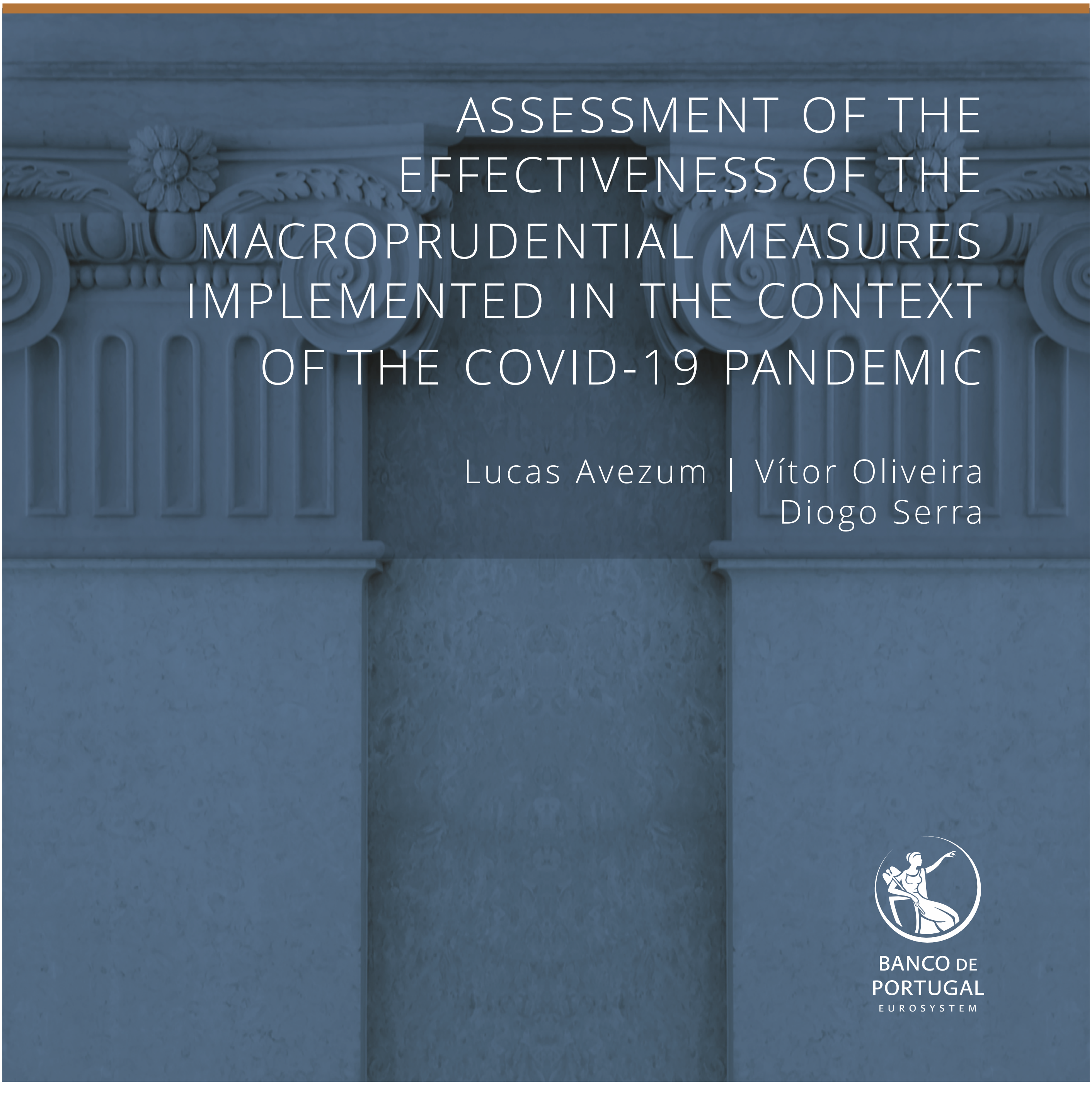

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Assessment of the effectiveness of the macroprudential measures implemented in the context of the Covid-19 pandemic

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Abstract
In this paper we assess the effectiveness of the macroprudential capital buffers' release on loans granted to households, implemented in the context of the Covid-19 pandemic. We obtain causal estimates by exploring differences in the availability of regulatory buffers prior to the pandemic shock among European countries and accounting for the time-varying effect of unobservable confounding variables with the synthetic control method. We find evidence that the buffers releases contributed, on average, to mitigate the procyclicality of credit to households, specifically for house purchase and for small businesses purposes. For the aggregate household lending, we find that the average treatment effect for both the release of the CCyB and that of the SyRB were positive. However, the results suggest that, for credit associated to small businesses purposes, only the release of the CCyB had an effect.

JEL: E51, G28, H12

Keywords: Macroprudential policy, capital buffer, Covid-19.

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1. Introduction

In 2020 we have witnessed an unprecedented exogenous shock that has disrupted social and economic life across the globe caused by a pandemic of a coronavirus (COVID-19). The containment measures that the great majority of countries across the globe has implemented in order to protect human lives and safeguard public health resulted in a synchronized global sudden stop in economic activity. This makes the global Covid-19 crisis unique, as it negatively impacted both supply and demand (Boissay and Rungcharoenkitkul 2020).

In addition to the aforementioned measures targeted at preserving public health, several authorities (governments, central banks, regulatory authorities) have put forward other type of policy measures, of a fiscal, monetary and regulatory nature, to mitigate the well-known long-lasting adverse effects the former ones and of the pandemic itself on the economy. This crisis has brought to light how fiscal, monetary and macro- and microprudential policies could act together and in a complementary way to address a sudden disruption in the economy, protecting financial stability.

The fiscal policy measures taken by the governments targeted the economic agents that were immediately and directly affected by the health measures such as: i. firms with the objective to minimize the destruction of productive capacity and job losses; and ii. households with the aim of minimizing the drop in income and its negative impact on consumption. This type of measures include direct loans and equity holdings in firms, cost sharing of furlough schemes, direct grants to certain types of firms and deferral/exemption of taxes and social contributions. Other type of measures with the aim of providing liquidity to help households and firms overcome the economic effects of the Covid-19 pandemic crisis call for the intermediation of financial system such as loans with public guarantee, debt moratoria and increased flexibility concerning the payment of insurance premiums (Haroutunian *et al.* 2021).

In addition to fiscal measures, some extraordinary monetary policy measures have been taken by central banks such as US Federal Reserve, Bank Japan, Bank of England and the European Central Bank, either by lowering interest rates or by increasing the scope of asset purchases. The European Central Bank has reduced the rate and raised the maximum amount of the existing monetary policy operations and launched a new and very relevant purchase operation for public and private debt, extending existing programs in this field coupled with a reduction of the discount applied to collateralized operations.

In what regards measures taken by the regulatory and supervisory authorities to safeguard financial stability, the materialization of risks caused by the pandemic prompted the implementation of several measures aimed at complementing the aforementioned fiscal and monetary policy responses, ensuring that the balance between preserving financial stability and sustaining economic activity is maintained. These measures include among others: i. the postponement of the adoption of final revisions to Basel III agreements, comprising the adjustment to the calculation of the leverage ratio, the introduction of a new leverage ratio buffer requirement applicable to global systemically important institutions, the revisions of the approaches for credit risk, operational risk and credit valuation adjustment and the introduction of the output floor; ii. The adoption of a regulation to amend the Capital Requirements Regulation, known as "CRR quick fix", which encompasses, among others, a transitional regime to reduce the impact of the adoption of IFRS 9, more favorable treatment of loans guaranteed by public authorities in the context of deduction from own funds related with Non-Performing Loans and the frontloading of certain prudential rules that were considered more favorable to financial institutions; and iii. The temporary flexibility in complying with part of the capital requirements and Pillar 2 guidance, with a view to encouraging institutions to make use of their capital buffers, and the release of some macroprudential buffers.[1] The principle of building up capital (and liquidity) buffers to deal with systemic risk materialisation underlies regulatory changes in the wake of the previous international financial crisis, with the purpose of preventing procyclical behavior of the financial system during troubled times, which could exacerbate the effects of an adverse shock.

In this paper, we assess the effectiveness of the macroprudential measures implemented in the context of the Covid-19 pandemic, focusing, in particular, on the impact of the macroprudential buffers' release on loans granted to households, following a synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie *et al.* (2010). We have narrowed the scope of the study to households in order to minimize the confounding effects that might arise from the overlap between the release of capital requirements and the use of governmental measures implemented in the context of Covid-19 pandemic, in particular State-guaranteed-loans, as the latter have widely supported firms as explained later in this paper.

Capital buffers are intended to increase the financial system's capacity to absorb unexpected losses in adverse periods, with the aim of preserving financial stability. The current EU legislation foresees five capital buffers, which, all together, result in the Combined Buffer Requirement (CBR): Capital Conservation Buffer (CCoB), Global Systemically Important Institutions (G-SII) and Other Systemically Important Institutions (O-SII), Countercyclical Capital Buffer (CCyB) and Systemic Risk Buffer (SyRB). Institutions that fail to meet the CBR are subject to automatic restrictions on dividends distributions, the payment of Additional Tier 1 (AT1) instruments and the repurchase of own shares until compliance is restored in accordance with a capital conservation plan duly approved by the microprudential supervisory authority.

This study analyses, in particular, the release of the CCyB, O-SII and SyRB buffers. The CCyB aims that banks build up an additional capital buffer in periods when cyclical systemic risk increases due to excessive credit growth. It

[1] For a detailed description please see the Special Feature "Policy measures in response to the COVID-19 pandemic of relevance to financial stability" on Banco de Portugal (2020a).

is defined based on the analysis of a set of macroeconomic and financial indicators, which provide information on cyclical systemic risk developments. Whenever risks materialize or decrease, this capital buffer ensures that the banking sector is better equipped to absorb losses and remain solvent, without disrupting lending to the economy. The O-SII buffer intends to mitigate the structural systemic risk underlying this type of institution, reducing externalities stemming from excessive risk taken by systematically important institutions and the associated moral hazard (also known as "too big to fail"). The SyRB is used to mitigate sources of systemic risk not covered by the remaining buffers. According to the Capital Requirements Directive (CRD IV), this buffer may apply to all exposures or in a targeted manner to either domestic or foreign exposures.[2]

In a synchronized response to the Covid-19 shock, a number of macroprudential authorities in the European Economic Area (EEA) countries at that date decided to release mainly the three aforementioned buffers (CCyB, O-SII and SyRB): in March of 2020, macroprudential authorities in seven countries decided to fully release the CCyB (Denmark, France, Iceland, Ireland, Lithuania, Sweden and United Kingdom), while three other macroprudential authorities decided to release this buffer only partially (Norway, Czech Republic and Slovakia); the macroprudential authorities in Estonia, Finland and Poland have fully released the SyRB for all institutions, while the Dutch macroprudential authority has partially released this buffer; finally, the Finnish and the Dutch macroprudential authorities have also decided to lower the O-SII buffer rate applied to some institutions, due to the existing linkages between this buffer and the systemic risk buffer, thus ensuring an actual easing in capital requirements.

The aforementioned decisions regarding the release of several buffers may mitigate the impact of the shock on the flow of credit to the economy. Nonetheless, the additional credit actually granted by the institutions also depends on their own decisions regarding the management of the buffers coupled with the stigma effect associated with the use of capital buffers, the demand for credit and other measures targeted directly to the private non-financial sector already mentioned.

Using a sample of 14 countries, we find evidence that macroprudential buffers releases contributed, on average, to mitigate the procyclicality of credit to households. Compared to countries that did not release buffers, credit growth to households was 0.99 percentage point higher in countries where there was a buffer release. Our results suggest that the release of capital buffers contributed, first, to support the credit supply for housing, and, second, to provide liquidity to households for business purposes. However, the effect on consumption was muted. In addition, for aggregate household lending, we find that the average treatment effect for both the release of the CCyB and that of the SyRB were positive. However, the results suggest that for credit associated to small businesses purposes, only the release

[2] For a detailed description of these buffers and how they interact please see the Special Feature "Interaction between regulatory minimum requirements and capital buffers" on Banco de Portugal (2020a).

of the CCyB had an effect. The results are robust to the choice of estimation window, the exclusion of treated units and controlling for monetary policy and microprudential policies effects.

The literature concerning the impact of the policy measures already in place (of a fiscal, monetary, supervisory and regulatory nature) as a response to the shock prompted by Covid-19 is so far in its infancy, taking into account that the majority of the effects have not been observable yet.

A first strand of studies such as the one carried out by Boissay and Rungcharoenkitkul (2020), try to draw lessons from other crises in order to be applied to the current shock. This study in particular analyzes the impact of other pandemics such as the influenza pandemic (1918), SARS (2003), H5N1 avian influenza (2003-2019) and Ebola (2014-2016) on the economy of the most affected countries. They have concluded that this type of pandemics and the respective containment measures have resulted in long-lasting adverse effects on the economy. However they signal that the high output losses from global efforts to contain the Covid-19 pandemic are unprecedented and incomparable with other more recent pandemics.

A second strand of studies try to shed some light about the potential effectiveness of the fiscal, monetary and supervisory and regulatory policy measures implemented by governments, monetary policy authorities and micro- and macroprudential authorities respectively.

Lewrick *et al.* (2020) assesses how much lending bank capital buffers can support, taking into consideration Covid-19 induced losses. The study starts by documenting the amount of banks' Common Equity Tier 1 (CET 1) capital above the minimum regulatory requirements and assesses how much of that amount banks would be willing to use designated as "potential buffers" (13% for Systemically Important Institutions and 10% for other banks). In a second step the authors compute the amount of these potential buffers that may be eroded if an adverse macroeconomic scenario takes place. Finally, the amount that banks could expand lending is estimated taking into account how much of their usable buffers they allocate to loans. According to the study and taking into account the capital level observed at the end of 2019, usable buffers alone might not be sufficient to bolster lending should the crisis deepen to a scale comparable of the Great Financial Crisis (GFC), thus other measures, notably to decrease the risks weights, are needed such as public guarantees.

Altavilla *et al.* (2020) carried out the first empirical study that analyzes the impact of the monetary policy measures taken after the pandemic outbreak in the euro area. In this vein they study how the policy response has been effective in protecting banks' intermediation capacity and if the coordination between monetary policy measures and supervisory and regulatory measures taken by either microprudential or macroprudential authorities have functioned. In the absence of sufficient data post-crisis to carry out an econometric analysis on the effects of the policy measures, they opt for studying the impact of similar measures adopted in the past. In this vein they have used a sample of 305 banks from

August 2007 to April 2020, including some crisis periods. They have concluded that the cumulative impact on loan growth of non-orthodox monetary policy measures (Target longer-term refinancing operations - TLTRO III) is relevant. Non negligible is the complementarity underlying the measures taken by micro- and macroprudential authorities. The effect on firms' behavior was also studied to show that firms more exposed to monetary policy measures and capital relief measures tend to increase their employment levels significantly.

A Box published Banco de Portugal (2020b) entitled "The banking system as economic stabilizer of the pandemic shock: a simulation of micro- and macroprudential policies" simulates the effects on credit, investment and output of using the flexibility measure of banks' Pillar 2 Guidance (P2G) and CBR. The study considers two scenarios: one where the measure is not implemented and another where the measure is in place and financial institutions can use this flexibility. The study, using a dynamic general equilibrium model based on Clerc *et al.* (2015) and calibrated for the Portuguese economy, shows that under the scenario where the flexibility is not available the exogenous shock results in a sharp fall of GDP in the second quarter of 2020 and a gradual recovery towards steady-state values after 12 quarters. When banks are allowed to manage the buffers with flexibility the impact of the shock on credit to households and firms is mitigated. This is more pronounced in corporate loans, as this segment has higher risk weights.

Another study on the impact of macroprudential measures on mitigating the adverse effects of Covid-19 on the economy was developed by Banco de España (2020) which tries to measure the impact of the pandemic on Growth-at-Risk (GaR) considering the mitigating impact of the macroprudential measures adopted. The study concludes that the group of countries that have been able to fully or partly release their macroprudential buffers seems to have contained the adverse impact on the GaR better than those that do not have the necessary macroprudential space to do so. However, the study was not able to isolate other measures that could have also contributed to mitigate the impact of the pandemic on the GaR, such as the fiscal and monetary ones.

Against this background, our paper contributes to a growing, albeit scarce, empirical literature on the effect of macroprudential capital buffers during the downturn of the financial cycle by assessing the impact of the macroprudential measures implemented in the context of the Covid-19 pandemic, at the European level. The majority of the studies so far have been analyzing the importance of macroprudential buffers with data from other crises as a proxy for the Covid-19 one. Others do not control for the confounding effects of the fiscal, monetary and supervisory/regulatory measures when studying the impact of one type of these measures in mitigating the impact of the pandemic on the economy.

The main contributions of this paper are twofold. Firstly, to the best of our knowledge, this paper configures the first empirical study that handle this topic with the most recent data, as it takes into account the macroprudential measures implemented in the context of the Covid-19 pandemic trying to control for other measures such as fiscal and monetary ones. Secondly, this study follows a method that addresses the pre-treatment differences in trends between countries where the measures were adopted and those where they were not adopted, overcoming some issues that characterize Difference-in-Difference estimators, as explained further below.

Notwithstanding this analysis is not free of challenges, in particular the fact that a number of countries has implemented several measures with a different nature (fiscal, monetary and supervisory/regulatory) but we are only interested in one type of measure. The potential resulting confounding effects are properly addressed by selecting a sample of countries with a similar profile in terms of adoption of other type of measures compared with those we are interested in, as well as a similar degree of stringency in what regards containment measures.

The remainder of the paper is organized as follows: the next section describes the methodology followed. Section 3 describes the data and the sample used. Section 4 reports the results of the analysis. Section 5 presents robustness checks and extensions. Section 6 concludes.

2. Methodology

We follow the SCM to assess the abovementioned effect of the release of macroprudential capital buffers on the growth of loans granted to households, including for each loan purpose within this institutional sector. This approach, initially developed by Abadie and Gardeazabal (2003), has been implemented for comparative case studies used to estimate the impact of aggregate interventions on a small number of large units.

In this vein, the proposed assessment aims to compare the growth of loans between each country whose macroprudential authority has announced a release of a macroprudential capital buffer (henceforth, treated country) and the countries which have not (henceforth, control countries), after the intervention has taken place.

Under this method, the control countries are weightily combined to obtain a synthetic version of the treated country (henceforth, synthetic control) prior to the intervention. In other words, the synthetic control is estimated to resemble as much as possible the pre-intervention characteristics of the treated country, assuming that the resemblance can often be more accurately achieved with a combination of untreated units than by any single untreated unit (Abadie *et al.* 2015). Accordingly, this method addresses the pre-intervention differences in trends between the countries concerned. Moreover, the SCM also ends up estimating the counterfactual of the treated country, reflected by the outcome trend of the synthetic control in the post-intervention period.[3]

[3]As further described in Section 3, the pre-intervention characteristics include determinants of loan growth based on the evidence given by the literature, and also pre-intervention values of the outcome variable.

For each release announcement (r), let I countries be composed by J control countries and one treated country. The sample is divided into a pre-intervention period T_0 and a post-intervention period T_1 starting at t_0+1. To define the length of T_0, we seek to balance, on the one hand, the availability of pre-intervention information and on the other hand, the potential confounding factors regarding changes in capital requirements that may have been observed before the intervention. Furthermore, the panel is balanced, implying that each country i is observed in the same time periods, $t = 1, \ldots, t_0, t_0+1, \ldots, T$.[4]

Let X_0 be a $(K \times J)$ matrix containing the values of the K pre-intervention characteristics for the control countries, and X_1 the analogous $(k \times 1)$ vector for the treated country. The variables considered as pre-intervention characteristics are averaged over T_0. For $k = 1, ..., K$, let X_{0k} be a $1 \times J$ vector containing the values of the k^{th} variable for the control countries, and X_{1k} the value of the k^{th} variable for the treated country.

To construct the synthetic control, define $W = (w_1, \ldots, w_J)'$ as the $(J \times 1)$ vector of non-negative weights that will be assigned to each control country j, constrained by $w_1 + ... + w_J = 1$. Following Abadie *et al.* (2015), the W^* is estimated to minimise

$$\sum_{k=1}^{K} v_k (X_{1k} - X_{0k})^2, \tag{1}$$

where v_k represents the non-negative weights that reflect the relative relevance of each pre-intervention characteristic, in terms of its contribution to construct the synthetic control.

Accordingly, the solution to (1), W^*, depends on the diagonal $(K \times K)$ matrix V, whose elements are $v_k = v_1, \ldots, v_K$. V is estimated in such a way the pre-intervention loan growth of the treated country is best reproduced by the synthetic control estimated by $W^*(V)$:

$$V^* = \underset{V \in \mathcal{V}}{\arg\min} (Y_1^{pre} - Y_0^{pre} W^*(V))'(Y_1^{pre} - Y_0^{pre} W^*(V)), \tag{2}$$

where Y_1^{pre} is the $(T_0 \times 1)$ vector of the values of loan growth for the treated country in the years prior to the intervention, and Y_0^{pre} is the analogous $(T_0 \times J)$ matrix for the control countries (Abadie and Gardeazabal 2003). We choose V among positive definite and diagonal matrices $\mathcal{V}$ such that the mean squared prediction error (MSPE) of the outcome variable is minimized for the pre-intervention periods, imposing $\sum_{k=1}^{K} v_k = 1$. In this sense, the pre-intervention period ends up being a validation period as the estimation of V is conditional on the minimization of the prediction error of the outcome over this period (Billmeier

[4]Abadie *et al.* (2011) show that, under certain conditions, the bias of the synthetic control estimator is bounded by a function that goes to zero as the length of T_0 increases.

and Nannicini 2013). Therefore, the weights for the synthetic control are given by $W^*(V^*)$.

After the intervention, the treatment effect (TE) is given by $Y_1 - Y_1^*$, i.e. the difference between the loan growth to the treated country (Y_1) and that for the synthetic control $(Y_1^* = Y_0 W^*)$. As stated above, the post-intervention Y_1^* reflects simultaneously the counterfactual of the treated country, in absence of treatment. An average treatment effect on the treated (ATT) for each treated unit can be computed as the time average of the treatment effect for each treated country.

In what regards statistical inference, we follow Abadie *et al.* (2010) to conduct placebo tests to evaluate the statistical significance of our estimates, assessing whether the gap in loan growth between the treated country and its synthetic control may have been influenced by other factors than the release.[5] The placebo tests consist in estimating a synthetic control for each country where a release was not observed (placebos), including the country which actually announced a release in the control group. Hence, we compare the gap for the (actual) treated country with that estimated for each placebo. Complementarily, we also present the ratios of post- and pre-intervention MSPE for the treated country and each placebo, which mean the difference between the loan growth of a unit and its synthetic control, prior and after the release announcements.[6]

Moreover, as indicated earlier, the synthetic control method is estimated for R release announcements $(r = 1, \dots, R)$. In this vein, we also estimate the average treatment effect (ATE) of releasing a macroprudential capital buffer, given by:

$$\text{ATE} = \sum_{r=1}^{R} \frac{\text{TE}_r}{\text{RMSPE}_r} \Big/ \frac{1}{\text{RMSPE}_r}, \tag{3}$$

where TE_r is the treatment effect associated to the r^{th} release announcement, and RMSPE_r the respective root mean square prediction error for the pre-intervention period. The ATE is weighted by the inverse of the RMSPE, favouring the treated countries that better fit their respective synthetic controls (Berger *et al.* 2021). The ATE is also estimated for the release of each of the two macroprudential capital buffers considered, i.e. the CCyB and the SyRB.

To evaluate the significance of the ATE, we combine the methods proposed by Acemoglu *et al.* (2016) and Berger *et al.* (2021). These authors estimate confidence intervals by bootstrapping the distribution of placebos. If the estimated ATE is larger than a given percentile of this distribution, then the ATE is statistically significant at this level. Acemoglu *et al.* (2016) obtain placebos by estimating the SCM for control units, similar to Abadie *et al.* (2010). Berger *et al.* (2021), obtain

[5] As subsequently explained in the following sections, we consider further criteria to mitigate potential confounding effects that may arise from the simultaneous application of different measures implemented as a response to the Covid-19 pandemic.

[6] A higher ratio is indicative of good fit of the synthetic control (low pre-intervention MSPE) and/or of a noticeable gap in the outcome after the treatment (high post-intervention MSPE).

placebos by estimating the SCM for the treated units but assuming the treatment took place in a different point in time. We follow Acemoglu *et al.* (2016) and use the control units to estimate the placebos, but, given our small sample size, we also assign P different placebo treatment dates. Therefore, we increase the pool of placebos to draw from the number of control countries (J) to the product $J \times P$. From this pool, we draw a number of placebos equal to the number used to calculate the ATE. We calculate the ATE among these placebos. We repeat this 5,000 times to obtain a distribution of the ATE. We use this distribution to calculate confidence intervals to test whether the ATE is significantly different from zero. The ATE is statistically significant at x% level, if it is greater than the $(1-x/2)$% percentile of the placebos distribution. Lastly, we conduct additional robustness analysis, first, on the initial date for the pre-intervention period. Secondly, we also estimate the method for the euro area countries, to assess whether the differences in monetary policy between the ECB and the remaining EU countries interact with the effect of release announcements on loan growth.

3. Data

As response to the Covid-19 shock, 14 macroprudential authorities in the EEA decided to release, completely or partially, either the CCyB, which, in this paper, is assumed equivalent to the aggregated institution-specific CCyB,[7] or the SyRB, which, in some countries, incorporates the announced release of the O-SII buffer, given the interaction between the two buffers in the CBR under the CRD.[8] For the sake of simplicity, henceforth we refer to this latter buffer release as SyRB release. Moreover, from this pool of potential treated units, we consider four countries that released the CCyB (Denmark, Lithuania, Slovakia, and Sweden) and three that released the SyRB (Estonia, the Netherlands, and Poland). We then take 7, from the remaining 16, as potential control units (Austria, Belgium, Germany, Greece, Latvia, Luxembourg, and Slovenia). To mitigate potential confounding effects, we exclude countries that largely used other policies in the context of the Covid-19 pandemic that could have an impact on credit to households: (i) because of the impact on loan amortization, we exclude countries with a ratio of loans to household under moratoria to total loans to households exceeding the 90th percentile of the

[7]Since the CCyB rate implemented in a given country applies to credit exposures to the domestic private non-financial sector, the institution-specific countercyclical buffer rate consists of a weighted average of the buffer rates applicable in the countries where the relevant credit exposures of the institution are located.

[8]Under the CRD, the SyRB was cumulative with the O-SII buffer when it applied to domestic exposures, but the higher of the buffers applied when the SyRB is implemented to all exposures. In this latter case, releasing solely the SyRB would not result in a decrease of the CBR.

respective distribution (Cyprus, Hungary, and Portugal);[9] and (ii) we also exclude Italy and Spain, the only two countries with significant state-guaranteed-loans to households. Because of the impact on credit demand, we exclude countries where the stringency of containment measures for the second quarter of 2020 is above the 90th percentile (Ireland, Cyprus, and France) and below the 10th percentile (Finland, Czech Republic, and Bulgaria) of the respective distribution.[10] Finally, data is missing for Croatia, Iceland, Malta, Norway, and Romania.

We assume the policy intervention occurred in March 2020, taking into account the ECB press release on 12 March 2020.[11] Although, at the national level, buffers were released at different dates, none was released before the ECB announcement, and for all but Slovakia the announcement of the release occurred in March. As signs of a second wave of Covid-19 started to show up in autumn, new policy measures were taken by that time, with a potential impact on the credit market to households. Therefore, the post-treatment period goes from March 2020 to August 2020. As to the pre-treatment period, we have considered July 2019 to February 2020. This choice of pre-treatment estimation window is relevant for the robustness of the results. On the one hand, as Abadie *et al.* (2010) shows, the longer the pre-treatment estimation window, the smaller is the bias from the SCM. On the other hand, as we increase the pre-treatment period, the probability to observe other shocks or policy measures that affect the credit market also increases. We choose the estimation window that minimizes the pre-treatment MSPE of the ATE. In section 5, we check the robustness of this choice by varying the initial period from September 2017 to September 2019.

We collect monthly country-level data on loans and advances to households from the ECB Statistical Data Warehouse (SDW). We focus on lending to households in order to minimize the confounding effects that might arise from the overlap between the release of capital requirements and the use of state-guaranteed-loans, which are mostly intended to support firms.

The dependent variable is an index of the stock of loans to households which takes the value of 100 in June 2019, i.e., we study the cumulative growth rate of the stock of loans to households relative to the level observed in June 2019. In section 4.2, we also analyze the components of credit to households: housing, consumption, and other lending.

We follow the literature on the determinants of credit to households to define the covariates in our analysis.[12] From the SDW we collect country-level information

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[9]Information on moratoria is taken from the EBA (2020). Figure B.1 of the appendix shows the distribution of the ratio of loans to household under moratoria to total loans to households across European countries.

[10]Information on containment stringency comes from Hale *et al.* (2020). Figure B.2 of the appendix shows the distribution of the index for containment stringency across European countries.

[11]ECB Banking Supervision provides temporary capital and operational relief in reaction to coronavirus.

[12]See, e.g., Calza *et al.* (2003), Castro and Santos (2010), and Behrendt (2016).

on the credit market. These are: MFIs' total loans over MFIs' total assets (loan ratio), MFIs' total deposits over MFIs' total liabilities (deposit ratio), the logarithm of MFIs' total assets as a measure of the size of the banking system, and the average interest rate on new business to households. We also collect information on the countries' economic environment from the OECD data. These are: the one year lag of the logarithm of real GDP, the consumer price index (CPI) annual growth rate, and the one year lag of the logarithm of the index of real house prices. Lastly, following Abadie *et al.* (2010), we add three lags of the dependent variable, January 2020, October 2019, and July 2019. Table A.1 of the appendix A describes all the variables and data sources.

4. Results

Using the SCM, we estimate, separately, the counterfactual for each treated country as a linear combination of the outcomes of the control countries. Consequently, for each treated country, we obtain different optimal weights for the same control countries. Table 1 shows that the estimated weights vary considerably across treated units. For instance, the synthetic Sweden is 78% Germany and 18% Latvia while the synthetic Netherlands is a combination of 51% Austria, 16% Latvia, 15% Greece, 12% Belgium, and 5% for the remaining countries.

Figure 1 shows the stock of loans time series for the treated countries in solid lines and for their synthetic control in dashed lines. The vertical dashed line separates the pre- from the post-treatment periods (February 2020).

The model fit varies considerably among the treated countries. On the one hand, for countries with a relatively stable series, such as, of Denmark, Estonia and Lithuania, the model performs quite well. On the other hand, in part due to the small pool of control units, the model struggles to replicate relatively volatile series, such as, of Sweden and Poland. Table 1 shows that outcome with the worst fit, Sweden, is an order of magnitude larger than the outcome with the best fit, Denmark. Nevertheless, the SCM improves considerably the fit of controls to the treated unit in relation to a naïve non-weighted average comparison. Figure B.3 in Appendix B shows a figure similar to Figure 1 but where the synthetic control series are replaced by the arithmetic average of the controls. Besides displaying the poor fit of the average, Figure B.3 shows that the outcome variable for the control and treated units do not follow a common trend for the pre-treatment period, corroborating our choice of estimation method.

For most countries, the estimated treatment effect, i.e., the difference between the actual and the synthetic series, is positive for the entire post-treatment period. The exceptions are Sweden early in the period and the Netherlands by the end of the period. The last row of Table 1 reports the average treatment effect on the treated (ATT) for each treated unit, i.e., the time average of the treatment effect for each treated country. ATT varies from 0.34 percentage points for the Netherlands to 3.23 percentage points for Poland. However, we cannot compare directly the treatment effect on Poland to the treatment effect on the Netherlands as the quality of the model fit for the latter is three and a half times larger than the former.

Figures 2 and 3 report the results of the procedures proposed by Abadie *et al.* (2010) to assess statistical significance of the treatment effect, as explained in Section 2. Both the placebo tests in Figure 2 and the ratio of post- to pre-treatment MSPE in Figure 3 show little support for statistical significance of the estimated effects. Among the placebos tests, only the effects on Lithuania and on Poland are larger than the "effect" on the placebos. For the ratio of post- to pre-treatment MSPE, none of the ratios for the treated countries are above the ratios of the placebos. However, the reduced number of control units in our analysis hampers any conclusion using the methods proposed by Abadie *et al.* (2010). The best we can obtain with 7 placebos is a p-value of 14.3%. In most cases the maximum possible p-value is even larger as we have to exclude some of the placebos due to poor model fit.

Because of the drawbacks described in the last two paragraphs, the ATTs not being comparable and the poor quality of inference, we compute an average treatment effect (ATE) across treated units for each time period weighted by the inverse of the RMSPE. The advantage of this approach is twofold. First, the RMSPE weighting results in a more robust estimate of the average treatment effect as it gives more weight to the estimates with better model fit. Second, we can run bootstraps to estimate a confidence interval for the ATE à la Acemoglu *et al.* (2016) and Berger *et al.* (2021).

Figure 4 shows that the average effect of the buffers release on household lending is positive and statistical significant. On average, between March and August 2020, credit to households grew 0.99 percentage points more in countries where capital buffers were released compared to the counterfactual with no release. The confidence intervals suggest that the effect is statistical significant at the 5% level, for the months of April, May, June, and at the 10% for the months of March and July. As discussed in Section 3, the treatment period considered starts in March 2020, taking into account the announcement of the ECB and anticipation concerns. However, effectively, most national authorities have only announced the release of capital later on the month. Combined with the time that banks had to implement the changes in capital requirements, it is expected to observe no effect already in March. Also, our effort to eliminate confounding effects relies mostly on data up to the second quarter of 2020. Given the amount of economic and financial policy measures implemented (or relaxed) in 2020, it is expected that, at some point in time, the uncertainty on the estimated effect increases. Therefore, we consider the estimated effects during the months of April, May and June the most reliable of the impact of buffer releases on lending.

4.1. CCyB vs SyRB

In this section, we evaluate the impact of capital buffer release on lending by type of buffer: cyclical or structural. Given the uncertainties on both internal and external capital generation associated with a stressed period, the time to replenish capital ratios in the future may play an important role on banks' current lending decisions. Hence, it is crucial, from a policy perspective, to evaluate if the impact on lending of the CCyB and the SyRB releases are different. Since only the CCyB was conceptually designed to be released, banks may expect authorities to require the rebuild of the structural buffers in a shorter period than the released CCyB.

Figure 5 reports the average effect for the countries that released the cyclical buffer, the CCyB, and for the countries that released the SyRB. The former are Denmark, Lithuania, Slovakia and Sweden and the latter are Estonia, the Netherlands and Poland. On average, between March and August 2020, credit to households grew 0.90 and 1.11 percentage points more in countries where, respectively, cyclical and structural buffers were released compared to the counterfactuals without capital release. Interestingly, while the effect of the SyRB appears to be slightly larger, the effect of the CCyB seems to be more persistent. The estimated confidence intervals suggest that the effect is statistically significant only in April for the CCyB and in April and May for the SyRB. Although the magnitude of the effects is very close to the effect in Figure 4, they are not statistical significance due to the reduced number of observations and, consequently, to wider confidence intervals.

4.2. The effect on the different components of loans to households

Figure 6 shows that the average effect of the buffers release is positive and statistical significant for lending for house purchase. On average, between March and August 2020, lending for house purchase grew 0.46 percentage points more in countries where capital buffers were released compared to the counterfactual with no release. The estimated confidence intervals suggest that the effects are statistical significant at the 10% level only in April and May. Figure B.4 in the Appendix B show that the effects are stronger and more persistent among the countries where the CCyB was released, but marginally significant different from zero.

The macroprudential capital buffers release appears to have no effect on credit for consumption. Figure 7 shows that the estimated effect is negative but not statistically different from zero. As Figure B.5 in the Appendix B reports, the result is strongly driven by the countries where the SyRB was released, but still not statistically significant. The statistically insignificant effect for consumption lending may reflect the impact that the containment measures and the uncertainty with respect to the development of the crisis had on households' consumption. Therefore, a softening of capital buffer requirements is expected to have little to no effect if the demand for credit decreases.

Lastly, Figure 8 shows that the average effect of the buffers release is positive and statistical significant for other lending, which includes loans granted to households for small business and debt consolidation.[13] On average, between March and August 2020, other lending grew 2.85 percentage points more in countries where capital buffers were released compared to the counterfactual with no release. The estimated confidence intervals suggest that the effect is statistical significant at the 10% for the entire post-treatment period and at 1% level in April, June, and July. Figure B.6 in the Appendix B show that the effect is only relevant among the countries where the CCyB was released. In these countries, the estimated impact on lending is 4.45 percentage points and statistically significant at the 1%.

The evidence in this section suggests that the effect of the release of capital buffers on lending to households was driven by house purchase and other lending. Given that lending to house purchase represents the largest share in household segment across the countries under analysis (79%, on average[14]), together with the results shown in Figure 6, we conclude that the effect observed on the aggregate lending is strongly driven by lending to house purchase. Therefore, the buffer release supported the supply of credit for housing, contributing to mitigate the impact that the containment measures and the uncertainty with respect to the development of the crisis had on households' investment in real estate decisions. Furthermore, the strong result on other lending suggests that the release of capital buffers allowed banks to provide the liquidity demanded in that period, in the form of loans granted to households for business, commonly with a small size, and debt consolidation.

4.3. Robustness checks and extensions

In this section, we present some robustness checks to our estimation. As described in section 3, the choice of estimation window is relevant. We choose as benchmark the estimation window from July 2019 to February 2020. Figure 9 shows the results from estimations where we change the initial period of the estimation window, iteratively, from September 2017 to September 2019. The fit of the model deteriorates but the magnitude of the estimated average effect remains. The ATE ranges from 0.86 to 1.71 percentage points with an average ATE weighted by the pre-treatment RMSPE of 1.13 percentage points and weighted standard deviation of 0.28 percentage points.

In Figure 10, we test if the results are driven by a few of the treated units. In each panel of Figure 10 we estimate the ATE and confidence intervals by removing

[13] According to the SDW's manual, "Other lending refers to loans other than for consumption and house purchase, and includes loans granted to households for business, debt consolidation, education purposes, etc. Loans to sole proprietors/unincorporated partnerships without legal status predominantly for business purposes (as opposed to personal) are separately identified, unless the conditions for reduced reporting apply. Loans included in this category may or may not be collateralised."

[14] Other lending represents 10%, on average.

one of the treated units. The average ATE ranges from 0.86 (when excluding Poland) to 1.30 (when excluding Denmark) percentage points and the effect is statistically significant at 5% during the months of April, May and June for all the estimations.

Finally, we assess whether the differences in monetary policy between the ECB and the remaining EU countries interact with the effect of release announcements on loan growth. Moreover, given the geographical coincidence, this robustness analysis ends up taking into account the loosening microprudential measures adopted under the SSM.

Figure 11 shows the results if we consider only countries belonging to the euro area, i.e., we excluded from the ATE calculation, Denmark, Sweden, and Poland. The result remains positive and statistically significant at the 5% level.

5. Conclusions

In this paper we assess the effectiveness of the macroprudential measures implemented in the context of the Covid-19 pandemic, in particular the impact of the capital buffers' release on loans granted to households.

We find evidence that macroprudential buffers releases contributed, on average, to mitigate the procyclicality of credit to households. Compared to countries that did not release buffers, credit growth to households was 0.99 percentage point higher in countries where there was a buffer release. Our results suggest that the release of capital buffers contributed, first, to support the credit supply for housing, and, second, to provide liquidity to households for business purposes, but had a muted effect on loans for consumption. In addition, for aggregate household lending, we find that the average treatment effect was positive for both the countries where the CCyB was released and for the countries where the SyRB was released. However, the results suggest that for credit associated to households' business purposes only the release of the CCyB had an effect.

Our paper is the first, to the best of our knowledge, to find a difference in the effect of the release of cyclical and structural buffers on lending. The uncertainties related to capital generation in stressed period and the expectation banks may have that supervisors will require structural buffers to be build up in a shorter period than the CCyB may explain the different effects. Hence, taking into account the policy concern of limiting the procyclical nature of lending and capital requirements, the evidence provided in this paper points to the need to further discuss an adjustment of the capital regulation framework to allow for more releasable capital buffers.

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Tables

	Denmark	Estonia	Lithuania	Netherlands	Poland	Slovakia	Sweden
Austria	0.02	0.01	0.00	0.51	0.00	0.02	0.00
Belgium	0.00	0.06	0.42	0.12	0.00	0.35	0.00
Germany	0.58	0.00	0.00	0.03	0.01	0.01	0.78
Greece	0.16	0.00	0.00	0.15	0.08	0.00	0.03
Latvia	0.10	0.03	0.01	0.16	0.10	0.02	0.18
Luxembourg	0.04	0.22	0.00	0.02	0.00	0.15	0.00
Slovenia	0.10	0.67	0.56	0.02	0.81	0.46	0.00
RMSPE	0.08	0.16	0.25	0.15	0.52	0.33	1.00
ATT (p.p.)	0.45	1.29	1.82	0.34	3.23	1.25	1.89

Table 1. This table provides the weights of control countries (rows) for each of the treated countries (columns) in our sample. The synthetic unit for each treated country is the linear combinations of the controls countries weighted by the respective weights, estimated separately for each treated country. The covariates that determine the match are the loans ratio, deposit ratio, size of the banking system, interest rate to households, lag of real GDP, inflation rate, and lag of house prices. The weights are rounded to the nearest second decimal place. The synthetic controls method minimizes the root mean-squared prediction error (RMSPE) between the actual series of the treated unit and the synthetic unit during the pre-treatment window. The average treatment on treated (ATT) is the average of the difference between the actual series of the treated unit and the synthetic unit during the post-treatment period.

Figures

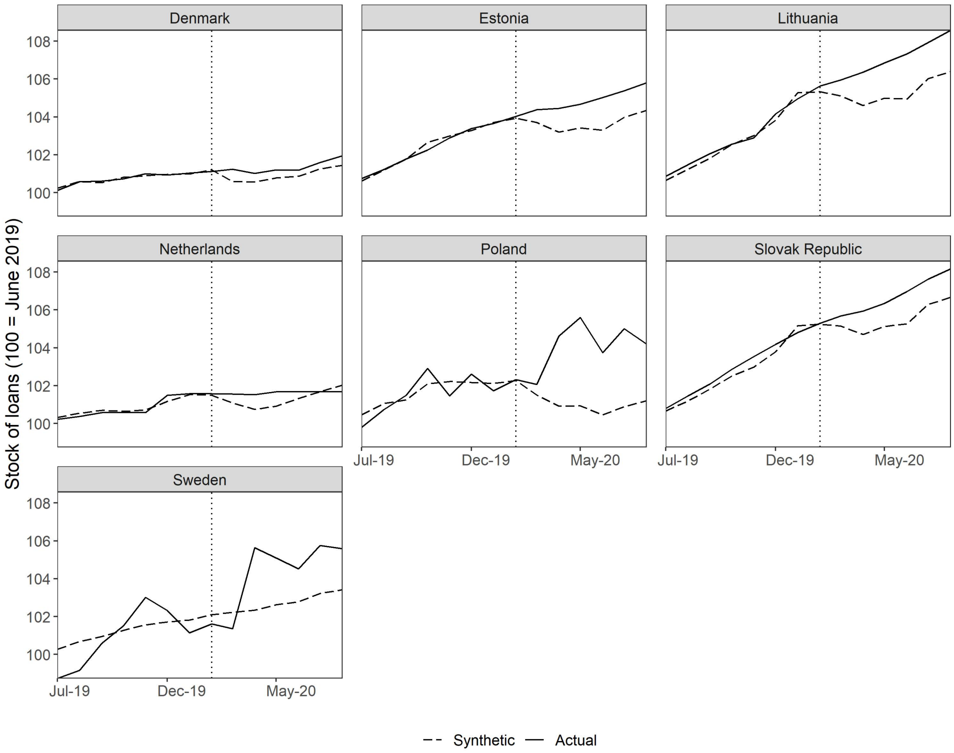

Figure 1: **The effect of capital buffer's release on household credit.** Figure 1 shows the stock of loans time series for the treated countries in solid lines and for their synthetic control in dashed lines. The vertical dashed line separates pre- from the post-treatment periods (February 2020).

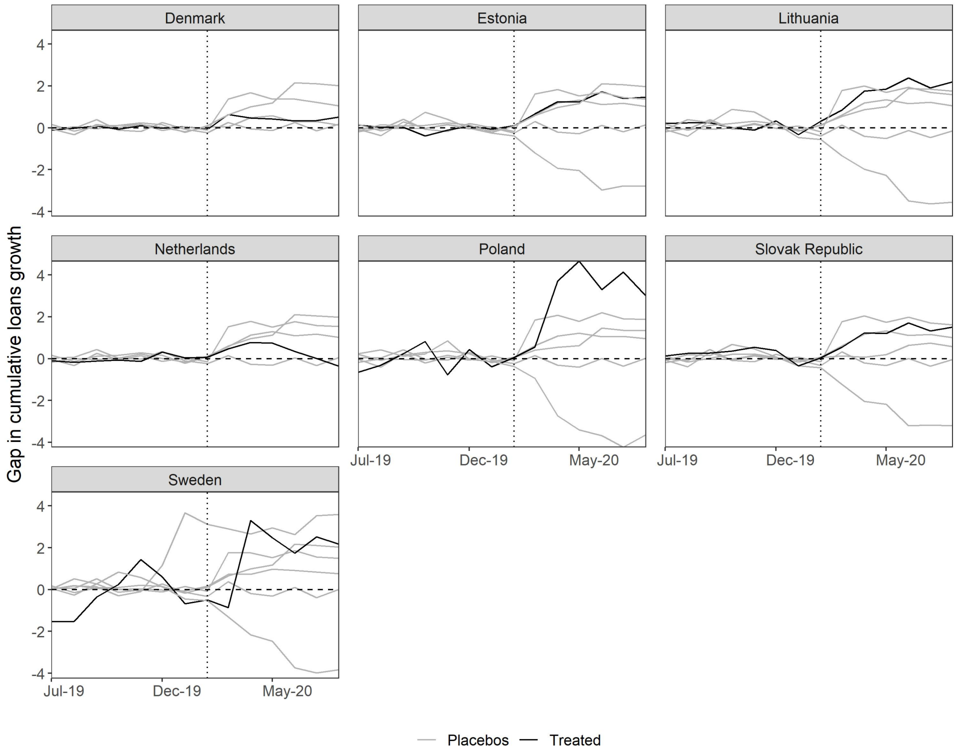

Figure 2: **Placebos test.** Each panel in Figure 2 shows the treatment effect of the capital buffer's release on household credit in black lines and the placebos in grey. The vertical dashed line separates pre- from the post-treatment periods (February 2020).

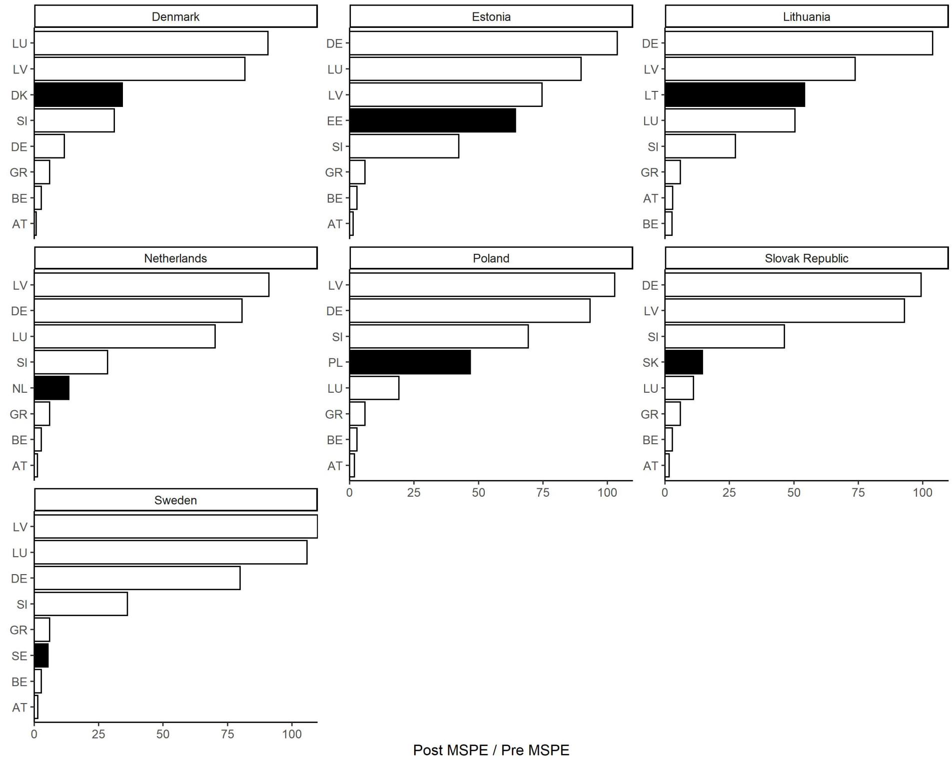

Figure 3: **Post-/pre-treatment MSPE ratio test.** Each panel in Figure 3 shows the ratio of the post-/pre-treatment mean squared predicted error for the treated unit in black and for the placebos in white.

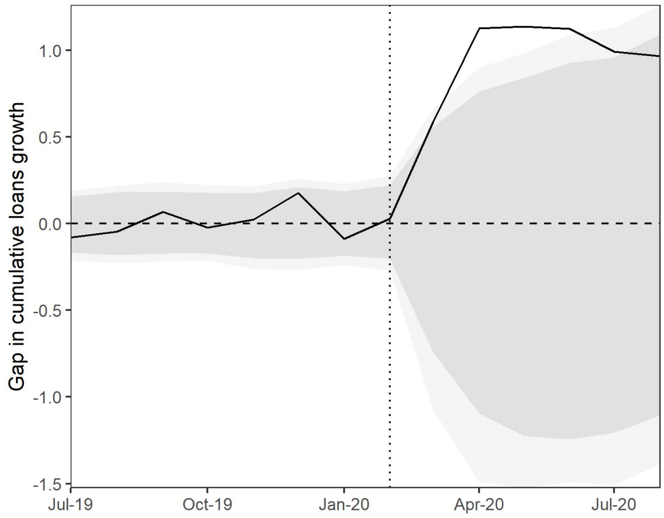

Figure 4: **Average treatment effect.** Figure 4 shows the average treatment effect of the capital buffer's release on household lending. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The dark and light shaded area contain the confidence interval at 10% and 5% level, respectively.

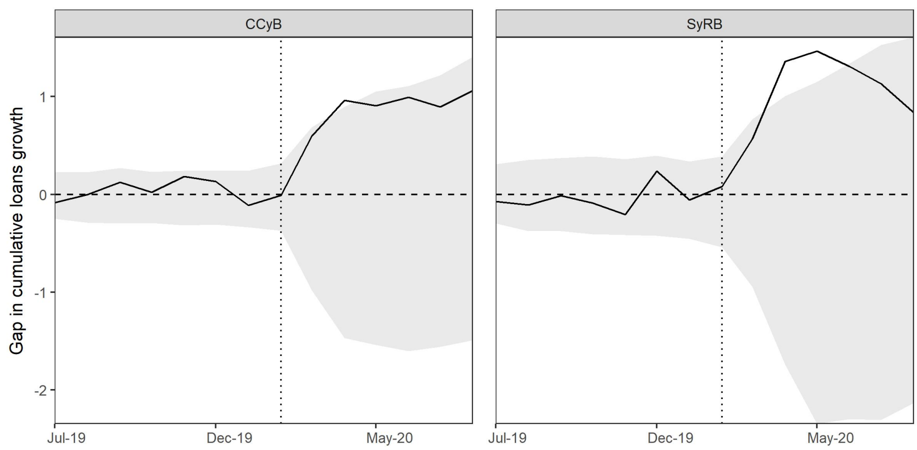

Figure 5: **ATE for CCyB and for SyRB.** Figure 5 shows the average treatment effect on household lending of the countercyclical buffer release and the systemic buffer release. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

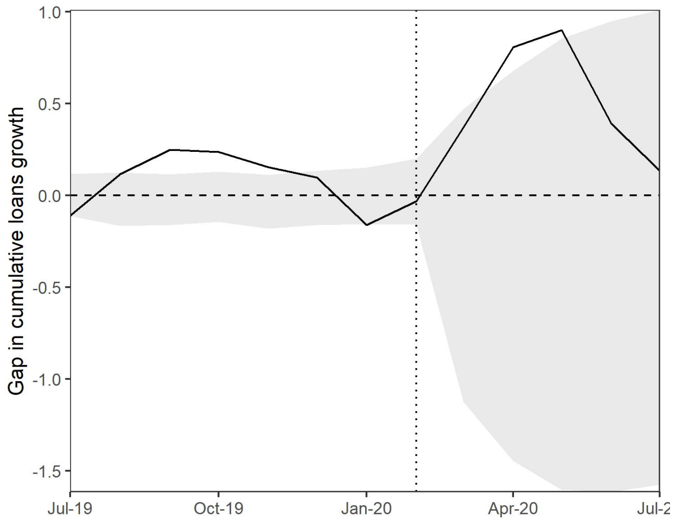

Figure 6: **ATE on lending for house purchase.** Figure 6 shows the average treatment effect of the capital buffer's release on household lending for house purchase. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

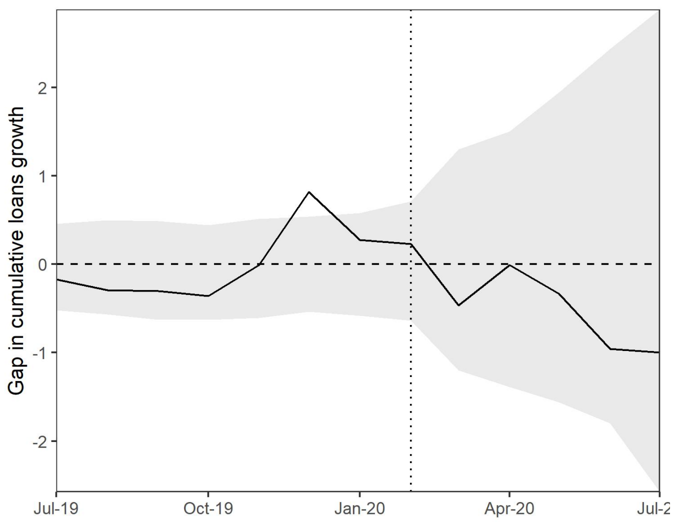

Figure 7: **ATE on lending for consumption.** Figure 7 shows the average treatment effect of the capital buffer's release on household lending for consumption. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

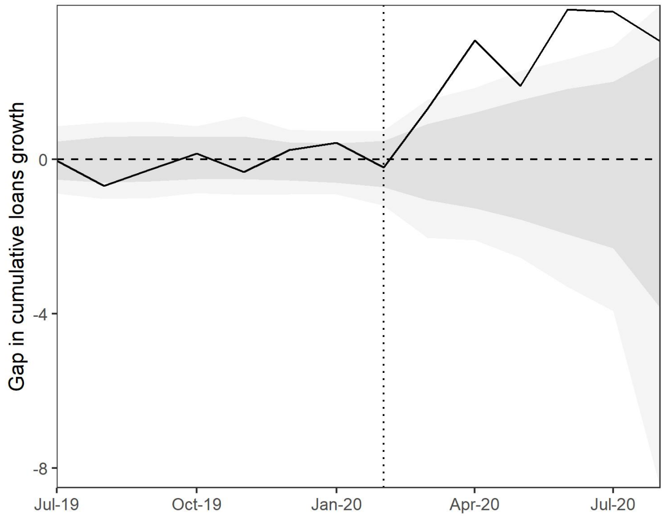

Figure 8: **ATE on lending for other lending.** Figure 8 shows the average treatment effect of the capital buffer's release on household for lending other than for house purchase and consumption. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The dark and light shaded area contain the confidence interval at 10% and 1% level, respectively.

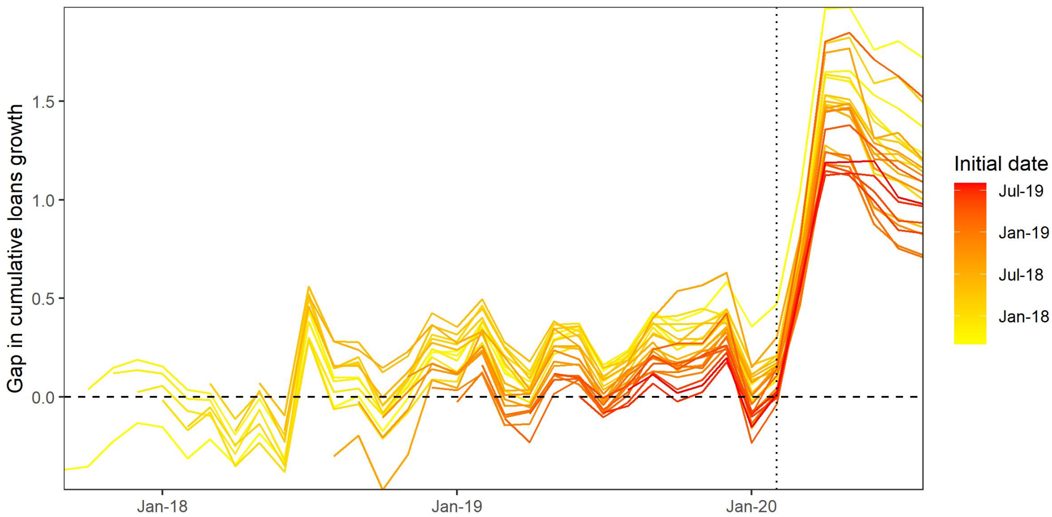

Figure 9: **Different estimation windows.** Each line in Figure 9 is the average treatment effect estimated with a different initial date. The vertical dashed line separates pre- from the post-treatment periods (February 2020).

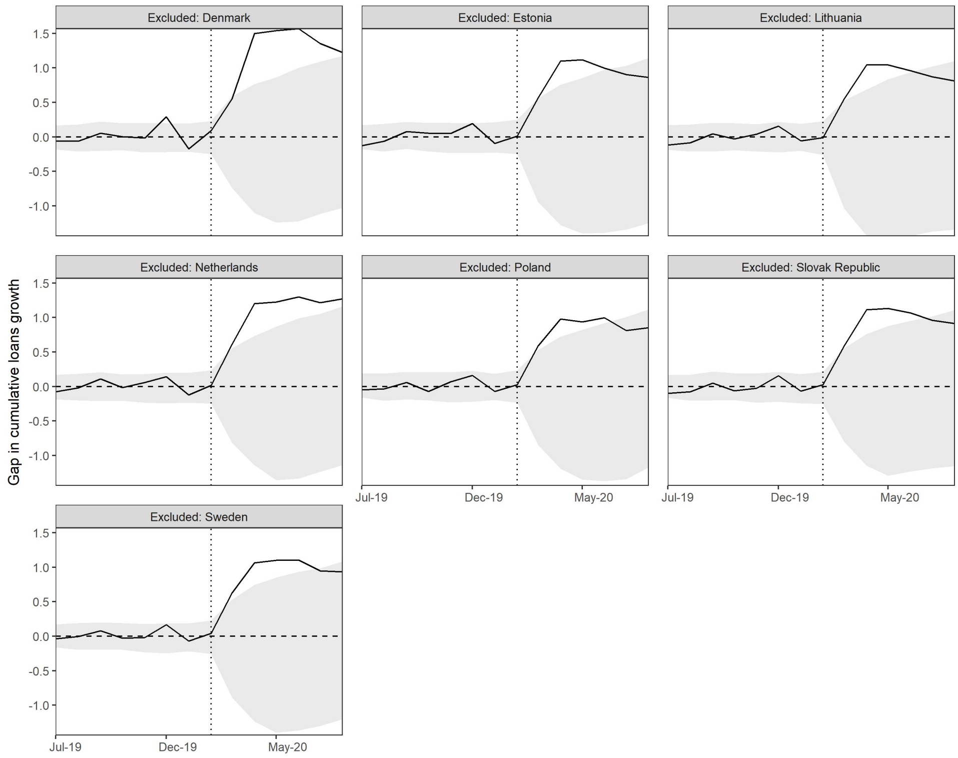

Figure 10: **Leave-one-out ATEs.** Each panel in Figure 10 is the average treatment effect estimated without one of the treated units. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

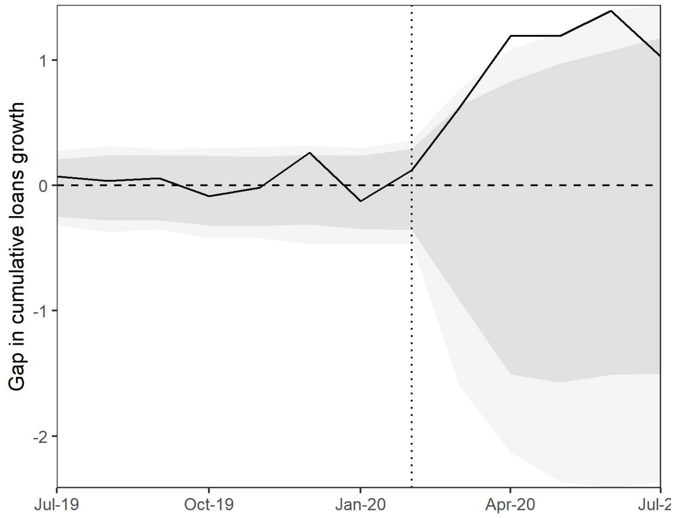

Figure 11: **Controlling for monetary policy and supervision.** Figure 11 shows the average treatment effect of the capital buffer's release on household lending considering only the countries from the Euro area. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The dark and light shaded area contain the confidence interval at 10% and 5% level, respectively.

Appendix A:

Variables	Description	Source
Dependent variables		
Index household loans	Index of MFI's domestic loans to households (stock), in local currency (100 =August 2019)	SDW
Index housing loans	Index of MFI's domestic loans to households for house purchase (stock), in local currency (100 =August 2019)	SDW
Index consumption loans	Index of MFI's domestic loans to households for consumption (stock), in local currency (100 =August 2019)	SDW
Index other loans	Index of MFI's domestic loans to households for other purposes (stock), in local currency (100 =August 2019)	SDW
Credit market characteristics		
Loan ratio	MFIs' total loans over MFIs' total assets	SDW
Deposit ratio	MFIs' total deposits over MFIs' total liabilities	SDW
Size of the banking system	The logarithm of MFIs' total assets as a measure of the size of the banking system	SDW
Interest rate to households	Average of the interest rates on new business to households by purpose (housing and consumption), weighted by the volume of purpose	SDW
Interest rate to households for housing	The interest rates on new business to households for purchase of house	SDW
Interest rate to households for consumption	The interest rates on new business to households for consumption	SDW
Country characteristics		
Lag of real GDP	One year lag of the logarithm of real GDP (volume index)	OECD
Inflation	CPI annual growth rate	OECD
Lag of house prices	One year lag of the logarithm of the index of real house prices (100=2015)	OECD

Table A.1. **Variables Description and Sources**

Appendix B:

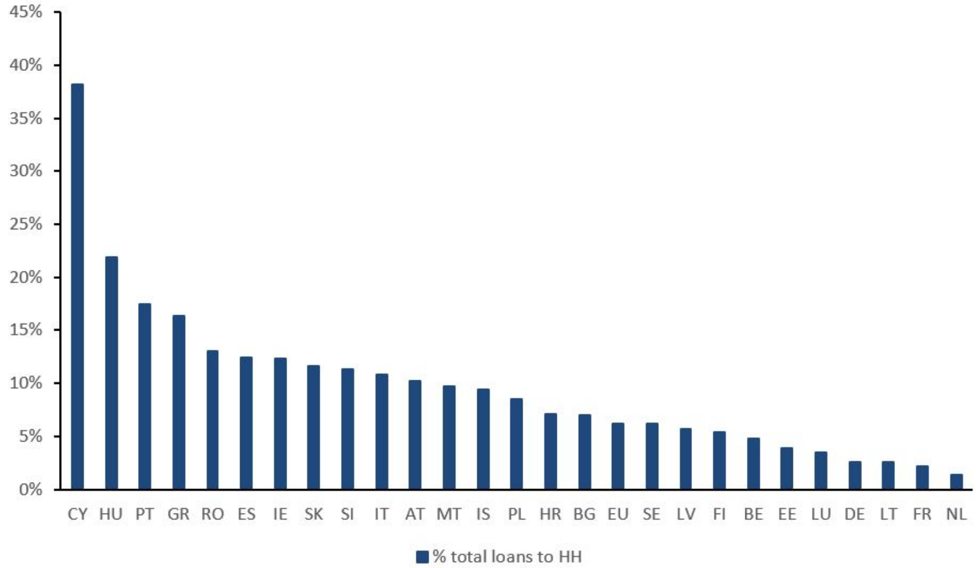

Figure B.1: **Moratoria during the Covid-19 pandemic.** Figure B.1 shows loans to HH granted moratoria as a percentage of total loans to HH by country – June 2020. According to the EBA Thematic note at stake, moratoria-related data has been reported by 132 banks in total (100 at the highest consolidation level), and thereby these exposures include loans to counterparties of all regions that are granted moratoria and, therefore, for some countries, these exposures may be particularly driven by their banks' presence in other countries (including non-European Economic Area countries) through their subsidiaries.

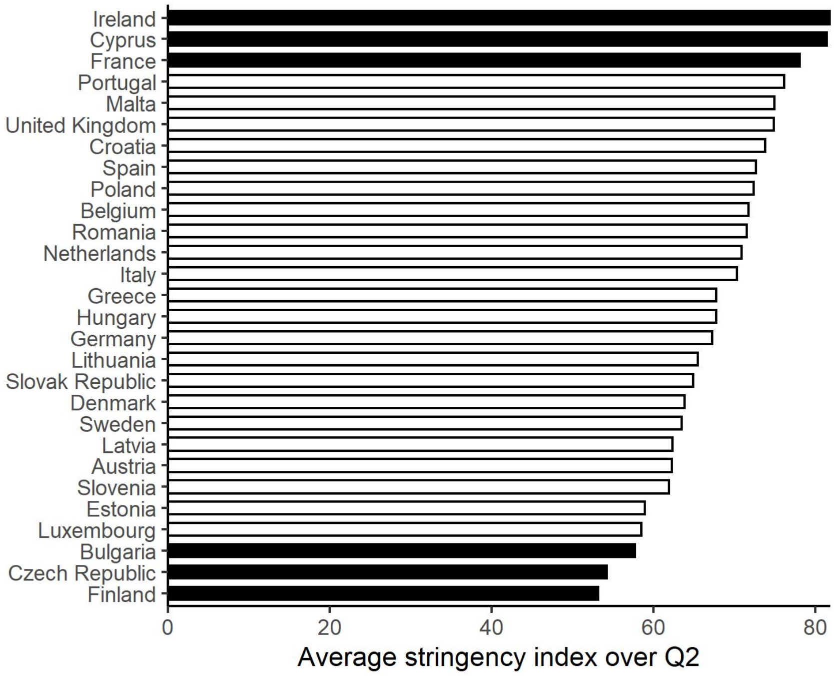

Figure B.2: **Containment stringency.** The figure shows the index of eight indicators for the stringency of the containment measures implemented during the covid-19 pandemic.

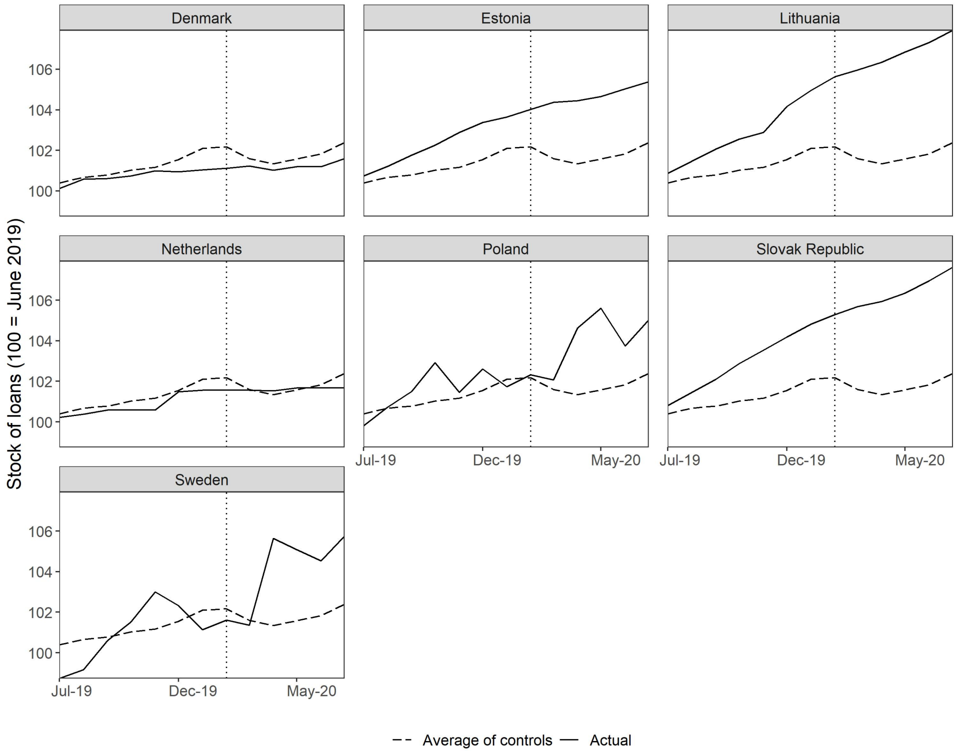

Figure B.3: **The average of controls as counterfactual.** The figure shows the stock of loans time series for the treated countries in solid lines and arithmetic average of the control in dashed lines. The vertical dashed line separates pre- from the post-treatment periods (February 2020).

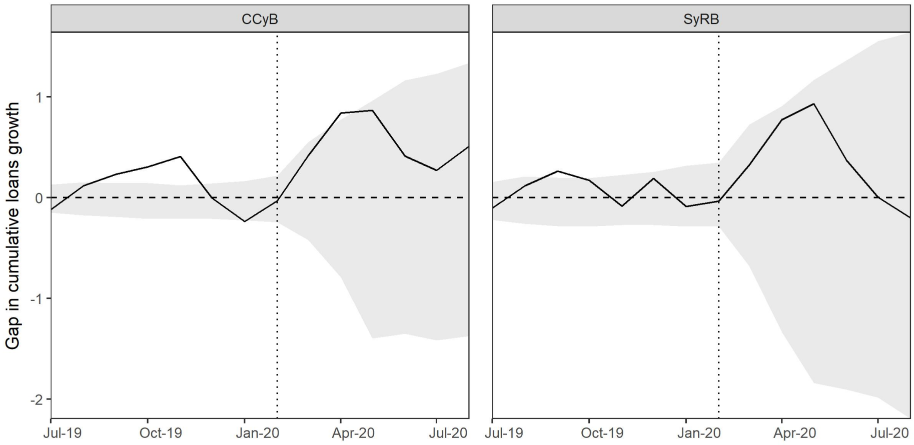

Figure B.4: **CCyB and SyRB effect on lending for house purchase.** The figure shows the average treatment effect on household lending for house purchase of the countercyclical buffer release and the systemic buffer release. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

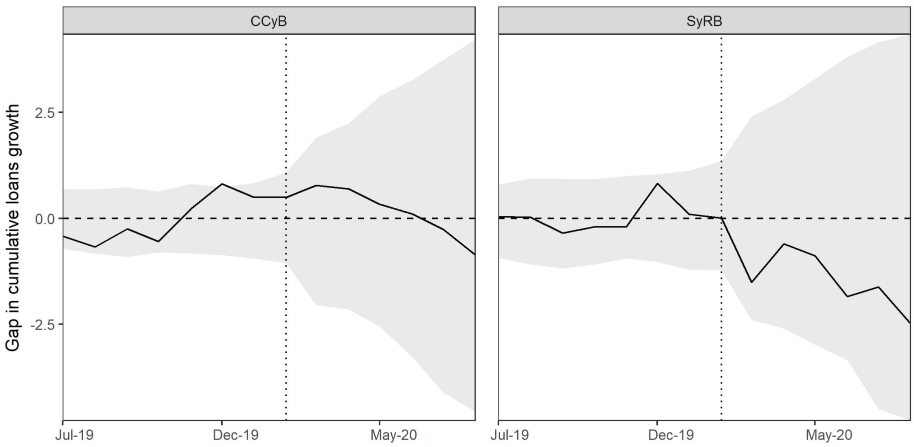

Figure B.5: **CCyB and SyRB effect on lending for consumption.** The figure shows the average treatment effect on household lending for consumption of the countercyclical buffer release and the systemic buffer release. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The shaded area contain the confidence interval at 10% level.

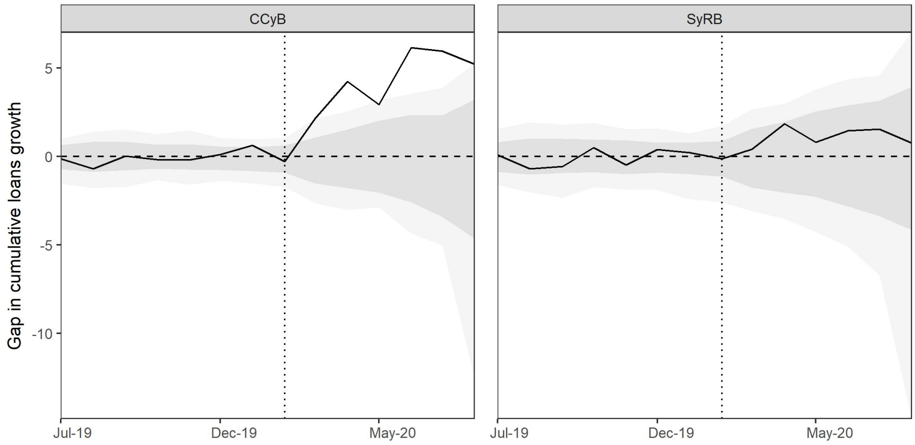

Figure B.6: **CCyB and SyRB effect on other lending.** The figure shows the average treatment effect on household lending other than for house purchase or consumption of the countercyclical buffer release and the systemic buffer release. The vertical dashed line separates pre- from the post-treatment periods (February 2020). The dark and light shaded area contain the confidence interval at 10% and 1% level, respectively.

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