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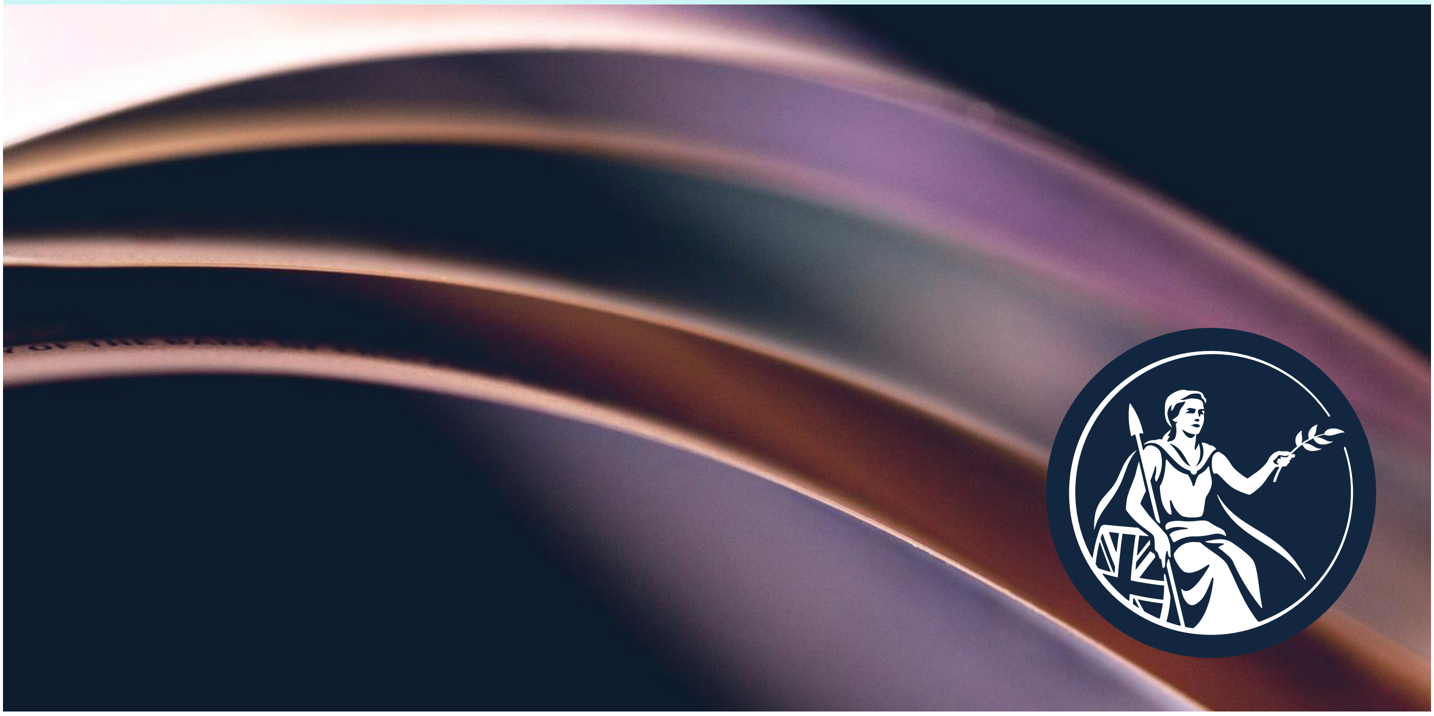

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Creditable capital: macroprudential regulation and bank lending in stress

Aakriti Mathur,[1] Matthew Naylor[2] and Aniruddha Rajan[3]

Abstract

Macroprudential policies enhance financial system resilience and dampen credit-led booms, but evidence of their effectiveness in stress is relatively scarce. We examine two such UK policies during Covid-19 – an exogenous shock to credit risk – combining mortgage register data, granular information on bank capital structures, and healthcare data on case rates. We find that expansionary macroprudential policy can mitigate credit crunch dynamics by alleviating capital constraints, supporting lending and risk-taking. However, policy design matters: policies, like cutting the countercyclical capital buffer (CCyB), which address numerous frictions leading to capital constraints (ie supervisory, regulatory and market signalling) are most effective.

Key words: Macroprudential policy, banks, capital constraints, lending, risk-taking.

JEL classification: E58, G21, G28, G32, G51.

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1 Introduction

Following the 2008 Global Financial Crisis, Basel III reforms redesigned capital regulation to enhance the effectiveness of macroprudential policy. In line with its objectives, contractionary macroprudential regulation has been shown to bolster resilience by increasing banks' capacity to absorb losses while remaining solvent (e.g. Benbouzid *et al.*, 2022; Meuleman and Vander Vennet, 2020; Altunbas *et al.*, 2018; Claessens *et al.*, 2013), as well as dampen credit-led booms (e.g. Tzur-Ilan, 2023; Acharya *et al.*, 2022; Galán, 2020; Jiménez *et al.*, 2017; Dell'Ariccia *et al.*, 2016). However, empirical evidence on expansionary macroprudential capital policies, targeted at banks to reduce the risk of credit crunches in downturns, remains relatively scarce. This is, in part, due to the limited case history of such actions - especially post-Basel III implementation. Even when deployed, causally studying the impact of these actions is challenging because of the endogenous nature of the stress in which they are often implemented, as well as a lack of detailed information on how policy actions transmit to different banks.[1]

We contribute to this discussion by investigaing the impact of two expansionary macroprudential policies on bank lending and risk-taking in the UK during Covid-19. The first policy is the regulatory capital buffer framework, wherein banks are required to finance themselves with additional capital - called buffers - on top of their minimum requirements. From a macroprudential perspective, buffers were intended to ensure banks maintained sufficient capital that could be drawn down (or, 'used') as needed to absorb unexpected losses and smooth credit-crunch dynamics without breaching the regulatory minimum. Although they were introduced as part of Basel III reforms, Covid-19 represented the first post-implementation test of whether the framework would work as intended. The Bank of England, European Central Bank, and Federal Reserve issued statements reinforcing the 'usability' of capital buffers at the onset of the pandemic, reflecting their macroprudential importance in supporting

[1]For instance, of all the macroprudential capital actions covered in Altunbas *et al.* (2018) for 61 countries between 1990-2012, only 17% have been expansionary. Additionally, most cross-country databases code macroprudential policy changes as binary, implicitly assuming that they affect all banks in the same manner (e.g. Alam *et al.*, 2019). See also Galati and Moessner (2018) for an overview.

lending in stress.[2] The second policy was a cut to the UK's countercyclical capital buffer (CCyB) rate from 1% to 0% (Bank of England, 2020a).[3] This tool was also introduced as part of the Basel III reforms to dampen procyclicality in the financial system (Financial Stability Forum, 2009), but was available to very few countries to utilise during Covid-19.[4] The UK therefore represents a useful case study for contrasting the efficacy of these two policies and comment on possible policy options to spur lending in stress.

We use detailed information on banks' capital structures and a novel methodology to disentangle banks' relative exposure to each policy. Then, to investigate the impact of these policies on lending, we exploit the UK mortgage credit register (Product Sales Database) using data between 2019 Q1 and 2020 Q4. This is advantageous as mortgages represented an economically important segment of UK household credit provision that was not *directly* impacted by government guarantees. Information on the flow of new lending, combined with a difference-in-differences specification and granular combinations of fixed effects, allow us to plausibly identify impacts to credit supply.

The Covid-19 crisis represented a large, exogenous shock to the macroeconomy, with banks exposed to a worsening economic outlook, volatility in financial markets, and severe uncertainty (e.g. Baker *et al.*, 2020; Bank of England, 2020c). Faced with the prospect of lower profitability and higher credit losses, impairments, and provisioning (e.g. BCBS, 2021; OECD, 2021), pre-existing capital constraints are likely to have played a more material role in determining banks' lending and risk-taking behaviour.[5] That is, at the onset of the

[2]The UK announcement by the Financial Policy Committee to use buffers was reinforced by the Prudential Regulatory Authority in April 2020 (Bank of England, 2020a,d), and supported by analysis that continuing to lend through the crisis would be net beneficial for banks (Bank of England, 2020b). The ECB and Federal Reserve also released statements on capital buffer use in March 2020 (European Central Bank, 2020; Federal Reserve Board, 2020). The usability of capital buffers and therefore its macroprudential objective is embedded in the rules (see, for example, Chapter RBC30, BCBS, 2021) and was emphasised by authorities even before the pandemic, such as in 2019 by the Basel Committee on Banking Supervision (BCBS) (BCBS, 2019).

[3]While all Basel III buffers are intended to be usable, only the CCyB can be explicitly cut, or 'released', in response to changing macroeconomic conditions. The released capital is then no longer subject to any regulatory consequences if it is drawn down or used. In the UK, conditions for CCyB changes are listed in the Financial Policy Committee's approach document (Bank of England, 2016).

[4]Only eight of the 27 BCBS member jurisdictions had a non-zero CCyB rate going into the Covid-19 crisis (BCBS, 2021).

[5]This hypothesis is in line with the finding by Acharya *et al.* (2021) that bank capital was perceived as

pandemic, more constrained banks are likely to have faced greater pressure to defend their capital positions to reduce the risk of future capital inadequacy; adverse supervisory reactions; market perceptions of material default risk; and higher funding costs (Stolz and Wedow, 2011; Lindquist, 2004; Gambacorta and Mistrulli, 2004; Van den Heuvel, 2002), especially in the face of higher than normal frictions to raising new equity (Gropp and Heider, 2010a; Behn *et al.*, 2016; Cornett and Tehranian, 1994; Myers and Majluf, 1984). Consequently, these banks may have reduced lending and risk-taking to manage these constraints, leading to procyclical outcomes and an amplification of adverse economic conditions caused by a collective action problem. The two policies of interest were intended to guard against precisely these dynamics, and we assess their effectiveness in doing so.

We first disentangle UK banks' exposures to the two policies. For the first policy, the regulatory capital buffer framework, we identify the extent to which banks were capital-constrained ahead of the onset of Covid-19. If effective, the usability of buffers – reinforced by the Bank of England statements – should have helped relax capital constraints for those banks for whom they were most binding, supporting their capacity to maintain lending. To measure capital constraints, we calculate banks' true excess capital (or "surpluses") held over and above their regulatory capital buffer requirements. This is a non-trivial exercise as banks are subject to several regulatory regimes that operate in parallel and interact with each other. Using granular data on UK banks' capital structures, we adjust banks' surpluses to account for interactions across three regulatory regimes (risk-weighted capital requirements, leverage requirements, and resolution or 'bail-in' requirements), as well as for differences in how banks meet quality standards for requirement-specific eligible capital instruments. As we show later, these nuances materially *increase* the degree to which banks are capital-constrained, relative to a more naïve assessment that does not take these factors into account.

Next, we turn to the cut to the CCyB. One of the key barriers to studying CCyB impacts is that changes apply to all banks simultaneously, with studies typically relying on either announcement effects (Couaillier and Henricot, 2023; Benbouzid *et al.*, 2022), pre-existing

the binding constraint at the onset of the pandemic, causing liquidity risk to negatively impact bank stock returns.

exposures to CCyB-affected sectors (Behncke, 2023; Auer *et al.*, 2022; Basten, 2020), or proxies for how this impact might vary across banks (Dursun-de Neef *et al.*, 2023). Instead, we directly observe the capital-relief to UK banks from a domestic CCyB change using confidential regulatory data. This relief is heterogeneous and varies across banks depending on their relative exposure to UK credit risk-weighted assets. We group banks based on the degree of capital-relief they received, i.e. the extent to which the CCyB cut quantitatively passed through into a release of capital requirements.

Overall, we find that the CCyB cut was successful in alleviating capital constraints and mitigating procyclicality in credit supply, but the ex-ante usability enshrined within the capital buffer framework was not. Using a difference-in-differences specification with data between 2019 Q1 and 2020 Q4, we find that, in aggregate, all UK banks increased their capital surpluses to a similar degree during the first year of the pandemic, perhaps reflecting a general desire to build precautionary surpluses due to the uncertain macroeconomic environment. However, when measured relative to starting levels, more capital-constrained banks increased their surpluses by significantly *more* than their peers. Importantly, this was at least partly achieved through a relative contraction in domestic credit supply, as interest rates on new mortgage loans increased and lending volumes fell.

We find contrasting results for the CCyB cut. Banks with higher capital-relief from the CCyB release defended their capital positions to a relatively lesser extent than their peers over the pandemic. Crucially, they were also able to maintain looser credit supply conditions, offering lower rates and higher lending volumes on new loans. Analysis of aggregate credit availability suggests that banks in general adjusted along the intensive, rather than the extensive, margins of credit supply.

Next, we turn to de-risking behaviour by banks during the crisis, relying on two complementary methodologies. First, we consider Covid-19 infection as a negative shock to borrower riskiness and match government collected Covid-19 case rate data at the local-area level with mortgage lending data. Covid-19 cases at the local level were correlated with regionally heterogeneous

UK government policies such as lockdown restrictions, in addition to adverse economic outcomes due to the spread of the disease such as higher sickness rates and unemployment.[6] Thus, they represent a shock to the household's cash-flows, which can significantly increase mortgage default probabilities (Ganong and Noel, 2022). Second, we use variations in conventional borrower risk measures, such as loan-to-value (LTV) and loan-to-income (LTI) ratios. This exploits the fact that high LTI and LTV loans have higher default likelihoods (Corbae and Quintin, 2015; Benetton *et al.*, 2018; Lazarov and Hinterschweiger, 2018) and are subject to additional regulatory constraints in the UK (Bank of England, 2014).

Across both risk metrics, we find evidence that corroborates the lending results above. The framework of usable buffers was not successful in inducing capital-constrained banks to maintain higher risk-bearing capacity: these banks maintained comparatively tighter terms on risky loans after the onset of Covid-19. In contrast, banks receiving higher capital-relief from the CCyB release maintained lower rates on risky loans and provided higher loan values, on average, than their peers.

One explanation for differences in efficacy between the two policies is related to how concretely they ease multifaceted capital constraints. Buffers may suffer from numerous frictions that inhibit their usability, including concerns around adverse supervisory reactions (BCBS, 2021), negative market stigma effects (Carvalho *et al.*, 2022; Borsuk *et al.*, 2020; Drehmann *et al.*, 2020), or the triggering of automatic restrictions on capital distributions.[7] Though public communications reinforcing usability in a stress may help to alleviate the risk of adverse supervisory reactions, they cannot alone mitigate other external barriers. However, an explicit cut to the CCyB that applies to all banks helps address all these barriers simultaneously and may therefore be more effective in supporting continued credit provision to the economy when unexpected shocks to credit risk arise.

[6] In the UK, mobility restrictions were placed in areas based on a set of epidemiological indicators through Covid-19. The same indicators also determined the stringency of the restrictions; see, for example, https://www.gov.uk/government/speeches/returning-to-a-regional-tiered-approach. In the time series, there is a statistically significant correlation (0.7) between case rates and the unemployment rate.

[7] Alongside the regulatory capital buffer framework, Basel III introduced automatic restrictions that limit the size of capital distributions a bank can make (i.e. dividends, AT1 coupon payments and variable remuneration to employees). These limits come into force once a bank's capital ratio falls within its regulatory buffers and become increasingly stringent as capital buffers are depleted.

Our identification relies on the onset of Covid-19 as an exogenous shock to credit risk, which was unrelated to pre-pandemic bank capital constraints. Identifying the link between capital constraints, CCyB pass-through rates, and lending is challenging for at least two reasons. First, it is important to disentangle shifts in credit demand from shifts in loan supply. In addition, for the Covid-19 period, this also involves considering the impacts of concurrently introduced fiscal support policies. To overcome these challenges, we zoom in on mortgage lending, as the majority of fiscal support for lending in the UK was directed at corporates rather than households. And wherever support schemes were targeted at households, these were generally targeted at existing borrowers that formed the *stock* of banks' lending rather than directly impacting the *flow* of new lending - our focus being on the latter. We also exploit the richness of our loan-level dataset by relying on granular combinations of fixed effects, such as local area-time fixed effects (similar to Peydró *et al.*, 2023). This helps better account for indirect impacts of Covid-19 schemes and time-varying shocks to credit demand and borrower riskiness at a reasonably small geographical level. Thus, we compare the pricing and volume on new loans extended by a more capital-constrained bank (or alternatively, a high capital-relief bank) to equally risky borrowers in the same local area in the same quarter, relative to those extended by their peers. Overall, the opposing directions of the loan rate (price) and loan value (quantities) responses by the affected banks supports the interpretation that we identify credit supply, rather than demand, effects (Juelsrud and Wold, 2020).

The second challenge is to ensure that banks are not systematically different across our comparison groups in ways that could explain differences in credit supply responses during the pandemic. To address this, we include a host of lagged bank balance sheet controls in all our specifications that could drive both capital choices as well as lending, such as bank size, liquidity, funding, profitability, share of household lending, and measures of riskiness, among others. In our loan-level specifications, we also explicitly allow the relationship between borrower characteristics and loan pricing and quantity to vary with the pandemic, accounting therefore for any changes in banks' risk perceptions. These specifications also include bank-local area fixed effects that control for any systematic differences in geographical exposure, competition, pricing, or lending volumes across our different comparison groups. In our risk-taking specifications, we include bank-time fixed effects to account for any time-varying bank unobservables that may drive differences in lending conditions. We regard these as our most conservative specifications. In each case, we test that lending outcomes were similar across these banks prior to the onset of the pandemic, and consistently find the parallel trends hypothesis to be supported by the data.

Our results have important implications for policy. Barriers to the usability of capital buffers have been expressly acknowledged (e.g. Garcia Pascual and Abad, 2022; Behn *et al.*, 2016) by industry practitioners and policy-makers, and a range of policy interventions early on in the pandemic highlights regulators' concerns regarding this issue. Our results provide support to the validity of policymakers' concerns around the usability of buffers during Covid-19. Furthermore, it is plausible that these effects could become even more acute in other stresses where extraordinary monetary and fiscal measures are not taken. The CCyB results show that expansionary macroprudential policy can be deployed to offset procyclical bank lending and risk-taking dynamics that can arise from buffer usability issues. This then points to one possible policy solution: making a greater proportion of Basel III capital buffers releasable in stress. This would be effective because releasing buffers can simultaneously address numerous potential frictions that underpin capital constraints, i.e. supervisory, regulatory, and market signalling.

2 Related literature

Our paper contributes to three strands of the banking literature. First, it provides evidence regarding the functioning of bank-based expansionary macroprudential policies – especially the CCyB – during periods of stress. For the Covid-19 period specifically, Bergant and Forbes (2021) have shown that countries which eased their CCyB rates experienced lower rates of economic and financial stress. Couaillier *et al.* (2022b) use loan-level corporate data to find that only permanent capital relief measures - which included cuts to both the CCyB and the systemic risk buffer - helped support Euro Area banks' capacity to supply credit during the pandemic. However, government lending schemes during Covid-19 have been shown to lead to substitution between different categories of corporate credit (Altavilla *et al.*, 2021; Minoiu *et al.*, 2021). Therefore, changes in banks' incentives to participate in corporate lending markets may affect estimates focusing on corporate credit during this period. Dursun-de Neef *et al.* (2023) use branch presence for European banks as a proxy for the benefit from the CCyB cut during Covid-19 and find that benefitting banks expanded lending overall. Avezum *et al.* (2021) draw similar conclusions on credit supply to European households using country-level data. Using proxies for banks' benefit from changes in the CCyB and using country-level lending data can conflate domestic versus foreign lending, which have been shown to have different dynamics during crises (e.g. Cetorelli and Goldberg, 2012). As a result, we focus on domestic provision of household credit and isolate bank-specific impacts stemming from the cut to the CCyB, utilising detailed information on banks' credit risk exposures in the UK, and combining this with loan-level mortgage data that is not directly affected by government guarantees.

Prior to the pandemic, empirical evidence on the effectiveness of expansionary macroprudential policies was somewhat limited and typically drawn from regulatory changes that were similar in spirit to the CCyB.[8] This included dynamic provisioning in Spain (Jiménez *et al.*, 2017), unexpected capital releases in Slovenia (Sivec *et al.*, 2018), and bank-specific capital requirements in the UK (Aiyar *et al.*, 2014).[9] The key takeaway is that these policies can help smooth credit cycles and expand credit supply during bad times.[10]

[8] The CCyB has been shown theoretically to reduce downside risks to GDP growth and the volatility of credit in negative shocks and improve financial stability by dampening credit cycles, such as by Ampudia *et al.*, 2021; Galán, 2020; Faria-e Castro, 2019; Gersbach and Rochet, 2017; Karmakar, 2016; Tayler and Zilberman, 2016; Benes and Kumhof, 2015; Drehmann and Gambacorta, 2012, among others, and making the banking system safer by, for example, reducing default probabilities especially for weaker banks (Couaillier and Henricot, 2023) and limiting system-wide losses (Bui *et al.*, 2017).

[9] Some studies find evidence of asymmetry in the effectiveness of expansionary and contractionary macroprudential actions, with the former being less effective than the latter, such as Valencia *et al.*, 2020; Cantú *et al.*, 2020; Altunbas *et al.*, 2018; Cerutti *et al.*, 2017; Aiyar *et al.*, 2014. However, these studies also note the limited history of expansionary actions, and the fact that they are typically introduced in response to endogenous financial crises.

[10] There is also a literature on the impact of higher CCyB rates outside of financial system stress, where it has been unambiguously shown to tighten credit supply. For example, the activation of the sectoral CCyB in Switzerland led to a portfolio reallocation away from residential mortgages (Auer *et al.*, 2022), an increase in mortgage rates (Basten, 2020), and a decrease in risk-taking (Behncke, 2023). These works measure exposure to the CCyB change using pre-existing share of residential loans on a bank's balance sheet.

The second strand of the literature focuses on the role played by banks' capital constraints in amplifying shocks. Banks' reactions to output shocks depends crucially on how much capital they hold *in excess* of regulatory requirements, a distinction that relying on capitalisation *levels* can obscure (Gambacorta and Mistrulli, 2004).[11] Analysis undertaken during the pandemic broadly confirms that capital-constrained banks (i.e. those with smaller surpluses over their capital buffer requirements) had lower capacity to support lending than their less capital-constrained peers.[12] In addition to our use of mortgage loan-level data and its advantages mentioned earlier, we also contribute methodologically to the definition and measurement of capital constraints, by making use of regulatory data and detailed information on banks' capital structures. This is important because accurately measuring surplus capital has become a complicated task after the implementation of Basel III, where banks have to meet several frameworks of requirements in parallel, such as on different qualities of capital, on a risk-weighted as well as unweighted (i.e. leverage) basis. As we show later, not accounting for these multiple regulatory requirements can lead to material over-estimation of loss absorbing capacity, and therefore mismeasurement of capital constraints. To the best of our knowledge, this has not been done previously in the literature.

Finally, we contribute more broadly to the literature on procyclicality of lending. Instead of investigating the risk-sensitivity of Basel III capital requirements, as done by Behn *et al.* (2016) and Repullo and Suarez (2012), we show how macroprudential policy design (even if risk-based) can nevertheless help to mitigate these procylical lending incentives. This paper also relates to the broader work on bank capital and lending (Favara *et al.*, 2021; Mendicino *et al.*, 2020; De Jonghe *et al.*, 2020; Fraisse *et al.*, 2020; Gropp *et al.*, 2019; Berrospide and Edge, 2019; Greenwood *et al.*, 2017; Baker and Wurgler, 2015; Admati *et al.*, 2014; Carlson *et al.*, 2013; Acharya *et al.*, 2012; Kashyap *et al.*, 2010, to name a few). We show how loosening macroprudential requirements can spur lending in a stress, but only if it

[11] More generally, bank capital also acts as a conduit for the efficacy and transmission of other policies, such as macroprudential and monetary policies (e.g. Altunbas *et al.*, 2018; Jiménez *et al.*, 2014, 2012; Kishan and Opiela, 2000).

[12] See, for example BCBS (2021) which documents effects for a global sample of banks using aggregate balance sheet data, Couaillier *et al.* (2022a) and Avezum *et al.* (2023) who use corporate loan-level data for the Euro Area and for Portugal respectively, and Berrospide *et al.* (2021) using loan-level information on small and medium enterprises in the US.

simultaneously relaxes numerous capital constraints.

3 The UK regulatory capital framework

Capital requirements in the UK are an interaction of three different regimes, with differing eligibility of specific capital instruments within each regime (see Figure 2). These requirements are calibrated to capture a range of micro and macroprudential risks that banks are exposed to.

The first is the risk-weighted capital requirements regime, wherein capital resources are measured as a percentage of risk-weighted assets (RWAs). The second is the leverage capital requirements regime, wherein capital resources are measures as a percentage of leverage exposures.[13] There are two components to each regime: minimum requirements and buffer requirements. Banks are required to satisfy minimum capital requirements at all times.[14] These are intended to ensure that banks can continue to operate, even after a stress, with an adequate layer of capital to protect depositors, maintain the confidence of markets, and enable orderly bank failure without losses to the taxpayer. Minimum capital requirements can be met with a mix of different types of regulatory capital instruments, with a minimum proportion that must be met with high quality capital instruments (CET1, Tier 1, and Tier 2 as the case may be). Regulatory buffers sit on top of these requirements, and must be met with CET1 - the highest quality of regulatory capital instrument. They are intended to be drawn down to absorb unexpected losses while allowing banks to continue to operate without cutting the provision of critical financial services to the real economy - to that extent, they serve a crucial macroprudential purpose especially in stress (see Chapter RBC30, BCBS, 2021, and BCBS, 2019).

The third regime, which applies to only major UK banks, is the minimum requirement

[13] The leverage ratio applies to UK banks with retail deposits in excess of GBP50 billion or foreign assets greater than or equal to GBP10 billion. The leverage exposure measure is comprised of a bank's assets as well as a range of off-balance sheet and other items along with some exceptions - most notably the exclusion of central bank reserves. For more detail, see the UK leverage ratio framework.

[14] In the UK, minimum capital requirements are comprised of a Pillar 1 risk-weighted requirement as well as a UK-specific Pillar 2A add-on that varies across banks and is set by supervisors through regular reviews.

for own funds and eligible liabilities regime (MREL), which boosts banks' recapitalisation capacity for resolution purposes.[15] This recapitalisation element can be met with regulatory capital instruments or 'eligible liabilities' (long-term, unsecured and subordinated debt, often referred to more loosely as 'bail-in' bonds).[16] A major contribution of this paper - which we detail in Section 4.2 - is that we combine granular information on banks' capital requirements and resources with the features of the UK capital framework, and use this to disentangle the effects of the two macroprudential policies during Covid-19 on UK banks' lending and risk-taking.

4 Data and disentangling banks' relative policy exposures

4.1 Data

We collect data for 159 UK banks at their highest level of consolidation on a quarterly basis over Q1 2019 - Q4 2020. Data on banks' balance sheets comes from different confidential regulatory returns collected by the PRA. These include, amongst other information, the granular breakdown of their capital requirements and resources. Summary statistics for key balance sheet variables used in our analysis are in Table 1.

We also make use of loan-level data on the universe of newly issued residential mortgages from the Product Sales Database (PSD001), which is updated quarterly by the Financial Conduct Authority (FCA). As mentioned in Section 1, we focus on mortgage lending as a key segment of UK credit provision that was not directly impacted by government guaranteed lending schemes. Mortgages are one of the largest asset classes on UK banks' balance sheets and the largest liability on the household sector balance sheet. Understanding the impact of expansionary macroprudential policies on this asset class therefore provides insight into important credit supply responses and financial stability implications during a large

[15] MREL is imposed by the Bank of England as the Resolution Authority. For more details, see the "Purple Book".

[16] A list of banks subject to these requirements is available here. For more information on MREL policy, see Bank of England (2021).

unexpected shock such as the pandemic.[17]

The dataset provides information for individual loans on a host of product characteristics such as loan value, interest rate, property value, loan-to-income (LTI), loan-to-value (LTV), term, type of repayment and property location at a granular three-digit postcode level (hereafter 'local area'). We also have information on borrower characteristics such as gross income, impairment status, employment status, income verification status and borrower type (home-mover, first-time buyer, or re-financer). After undertaking a few standard steps to prepare the dataset, we are left with 90% of the original 1.8 million loans and 69 of the initial 88 banks in our loan-level dataset. Summary statistics for the loan-level data are provided in Table 2.[18]

We observe *completed* mortgages in the loan-level dataset. We do not observe mortgages that were applied for but rejected. Given that this is the case for all banks in our data, we rely on our difference-in-differences approach and granular combinations of fixed-effects to provide confidence that we are nonetheless estimating the average causal effect from our parameters of interest. But, consequently, to discuss dynamics regarding *offered* lending conditions in aggregate for, for example, interest rates by different product types, we rely on data from *Moneyfacts*; a dataset detailing the range of mortgage products offered by lenders (for more details, see Rajan and Willison, 2018).

Finally, we merge the lending data with granular information on Covid-19 case rates, defined as new cases per 100,000 people from the UK's official Covid-19 reporting dashboard.[19]

[17] Lending to households accounts for about half of all credit to private non-financial sector by UK banks, while mortgages (as of 2017) account for about 80% of the total stock of household debt (Peydró *et al.*, 2023). Approximately 30% of UK households have a mortgage (Ministry of Housing, Communities, & Local Government, 2020).

[18] To prepare the dataset, we restrict ourselves to fixed-rate mortgages, which are directly comparable products and make up the overwhelming majority of the market. We also focus on employed and self-employed borrowers, filtering out, for instance, retirees, who make up a small, niche, segment of the market. Other steps include restricting the dataset to: loans for which the postcode is available (as this forms a key part of our identification strategy); loans for which the income verification is available; and mortgage types with 1000 loans or more (thereby filtering out buy-to-let, business loan and high net worth mortgages). Finally, we winsorize the LTI and LTV observations at the 0.001% tails, and restrict the sample to loans with interest rates greater than 0.1%, to eliminate obvious miscoding errors in the data.

[19] This is available at `https://coronavirus.data.gov.uk/`. We use averages to convert weekly data to monthly. This data is available at the Middle Layer Super Output Area (MSOA) level, which are comparable

Summary statistics on case rates disaggregated by UK regions (which are larger than local areas) are shown in Table 3, and the geographic distribution shown in panel (c) of Figure 11. We rely on this geographic heterogeneity in our analysis, which is discussed in greater detail in Section 5.

4.2 Banks' exposure to Covid-19 macroprudential policies

We now discuss the methodology we employ to disentangle banks' relative exposure to each of the two policies we are interested in: the operation of the usable capital buffer framework at the onset of the stress; and the explicit cut of the UK CCyB rate from 1% to 0%. We do this by measuring the degree to which banks were capital-constrained ahead of the pandemic and the degree to which they received capital relief from the CCyB cut, respectively.

Capital-constrained banks

To identify the relative bindingness of capital constraints across banks, we measure a bank's voluntary excess CET1 capital, or surplus, on top of its regulatory buffer requirements.[20] However, this is a non-trivial exercise due to the post-Basel III regulatory framework and its UK implementation, discussed in Section 3. The challenge is that calculating the bank-specific CET1 surplus in a 'simple' manner - i.e. typically calculated in the literature as the distance between a bank's risk-weighted CET1 ratio and its risk-weighted regulatory requirement - overstates the bank's true loss absorbing capacity. It conflates the true voluntary CET1 surplus that banks maintain, amongst other reasons, to avoid breaching thresholds for regulatory intervention with CET1 that banks maintain for two other purposes: to meet other regulatory requirements (leverage and MREL); and to circumvent frictions in accessing other qualities of capital (additional Tier 1, Tier 2, and eligible liabilities) that could otherwise be used to

to UK council districts, and have at least 5000 residents per the 2011 Census (e.g. Fetzer, 2022).

[20] We narrowly focus on a CET1-based measure of surplus for two reasons. First, regulatory capital buffers can only be met using CET1 capital resources. Focusing on CET1 therefore provides the cleanest read of the likelihood a bank faces of using regulatory buffers in the face of shocks. Second, it enables us to account for the approaches of different types of banks in meeting their regulatory requirements. For example, smaller UK banks and building societies tend not to issue lower quality capital instruments due to more limited access to public funding markets. Consequently, they use CET1 to meet their requirements even where lower quality capital instruments are permitted.

meet these requirements. These interactions, once taken into account, represent a material drag on a bank's simple surplus.

In practice, as detailed in Figure 3, this implies first calculating a simple CET1 risk-based surplus for each bank, and then subtracting any shortfall in additional Tier 1 (AT1), Tier 2 and eligible liabilities resources. The same exercise is then repeated for the leverage requirement separately. Then, we define the 'effective' CET1 surplus as the minimum of the quality-of-capital risk-based or quality-of-capital leverage-based surplus calculations.[21] This requires a detailed knowledge of the various requirements applied to the bank (as shown in figure 2), and information on the banks' capital structure, allowing for any of the different requirements to be the relevant binding constraint.

The relevance of these detailed calculations is shown in Figure 4. The effective surplus distribution lies to the left of the simple surplus distribution, with half the median (4.3% vs. 8.1% in panel (b)), supporting our conclusion that simple CET1 surpluses are an overestimate of a bank's true loss absorbing capacity and, therefore, an underestimate of its true degree of capital constraints.

We use our measure of effective CET1 surpluses to separate banks into those who are more or less capital-constrained. A more capital-constrained bank is defined as one with less than 2 percentage points (pp) average effective surplus relative to RWAs in 2019, whilst a less capital-constrained bank is defined as one above this cut-off.[22] This cut-off represents 45% of the minimum CET1 requirement excluding buffers, and about a quarter of the minimum CET1 requirement including buffers in the sample. Emphasising the relevance of our effective

[21]By design we exclude the firm-specific PRA buffer from our measurement of capital constraints. Since it is neither disclosed publicly nor subject to any automatic restrictions on distributions, there are limited constraints by design in banks' use of the PRA buffer.

[22]The cut-off is chosen in line with other existing work on usable buffers, internal data collections by the PRA on banks' capital targets relative to requirements and UK-specific evidence on average capital surpluses over a longer time period. In their sample of UK banks, Bridges *et al.* (2014) use a cutoff of 1.5% to separate large and small buffer banks, while Eckley *et al.* (2022) use a host of definitions to define "dangerzone" banks, i.e. UK banks particularly close to regulatory minimum capital requirements, and find that these banks maintained surpluses of between 2-3pp of RWAs between 1989–2013. Couaillier *et al.* (2022a) use a bottom quartile definition with a cutoff at 2.6%, while Berrospide *et al.* (2021) use a below median defintion with a cutoff at 2.13%.

surplus calculation, there would be only two banks considered as capital-constrained using the simple CET1 surplus. In our baseline specification, we exclude banks in the top quartile of the pre-pandemic surplus distribution as there is a long right tail in the distribution (Figure 4). This exclusion ensures that the results are based on banks with more comparable business models. However, we show later that our results are robust to the inclusion of this group.

For robustness, we also confirm that our main results hold with two alternate versions of the capital constraint definition: one where capital-constrained banks are identified as those in the bottom quartile of the cross-sectional 2019 effective surplus distribution, and one where we use a continuous measure of the effective surplus. To ensure that our results are not driven by changes in sample composition, we use the same categorisation of banks for regressions we run using the mortgage loan-level dataset which is comprised of a smaller sample of banks.

Capital-relief banks

To analyse the impact of the second policy experiment on bank outcomes, we rely on cross-sectional bank-specific variation in the degree to which the CCyB cut 'passed-through' to actual capital relief. Under its regulatory definition, the UK CCyB rate is set as a percentage of a bank's UK credit RWAs. The degree of pass-through is therefore defined as $\frac{UK\,Credit\,RWAs}{Total\,RWAs}$ and is bounded between 0 and 1. Banks with higher pass-through rates are more exposed to UK credit markets and are affected to a greater extent by changes in the UK CCyB rate. We exploit this fact to assess whether the CCyB cut affected bank responses differently depending on their bank-specific capital relief, while controlling for potential differences in their underlying business models. We define a high capital-relief bank as one that had a pre-pandemic (2019) pass-through rate of more than 50%. This heterogeneity in the degree to which UK banks were affected by the CCyB cut is a significant advantage of focusing on the UK.

5 Empirical strategy: Isolating credit supply

5.1 Evolution in capital surpluses

We first test the impact of each expansionary policy on the pace of banks' capital building. To do so, we employ a standard difference-in-differences methodology, comparing the evolution of banks' CET1 effective surpluses across the respective groups of interest, pre- and post-Covid. Specifically, to test the impact of the usable capital buffer framework, we compare surplus building by banks after the onset of the pandemic between more versus less capital-constrained banks. To test the impact of the CCyB cut, we compare the capital behaviour of more versus less capital-relief banks.

For bank b in each quarter t, we regress the dependent variable of interest, CET1 effective surplus, expressed in percentage points, (and alternatively *log* CET1 effective surplus) on the double interaction of Post-Covid$_t$ and either More capital-constrained$_b$ (equations 1 and 2) or High capital-relief$_b$ (equations 3 and 4). The Post-Covid$_t$ dummy takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. To identify *causal* effects of these macroprudential policy actions on bank behaviour, we must ensure that the *only* source of variation between the banks in our two respective comparison groups are their capital constraints and extent of capital relief from the CCyB cut, respectively. To achieve this, across these specifications, we account for unobservable time-invariant heterogeneity across banks with bank fixed effects (f_b), as well as time-varying shocks common to all banks, such as regulatory changes or changes in macroeconomic conditions, with time (quarterly) fixed effects (f_t).

We also include a set of time-varying balance sheet control variables that account for differences in business models across the two comparison groups, lagged by one quarter ($X_{b,t-1}$). These include banks' lending (total loans), liquidity (cash), funding (deposits), and profitability (retained earnings), all expressed as a share of total assets (e.g. Gropp *et al.*, 2019; Cohen and Scatigna, 2016; Aiyar *et al.*, 2014; Gropp and Heider, 2010b; Francis and Osborne, 2010;

Berger *et al.*, 2008; Ayuso *et al.*, 2004).[23] We also include the contemporaneous capital requirement to strip out the mechanical impact of changes in banks' capital requirements on their surpluses. This also allows the interpretation to be driven by concerns around buffer usability frictions rather than around breaching minimum requirements. This ensures that any impact on surpluses we measure is a *behavioural* response by banks via changes to their capital ratios. Additionally, we account for the expectation communicated by the PRA in March 2020 that major UK banks suspend capital distributions in the form of dividends and bonuses until the end of 2020 (PRA, 2020), by including the interaction $\text{Post-Covid}_t \times \text{Distribution restriction}_b$, where the latter is a dummy variable that is equal to 1 if the bank was subject to this expectation and 0 otherwise.

The coefficient β_1 in equation 1 reflects the evolution of capital for more capital-constrained banks, relative to less capital-constrained peers, after the onset of the pandemic. When the dependent variable is *log* capital surplus in equation 2, the coefficient β_2 reflects the change in capital for more capital-constrained banks, relative to their starting point.

$$\text{Capital Surplus}_{b,t} = \boldsymbol{\beta_1}\text{More capital-constrained}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_b + f_t + \epsilon_{b,t} \tag{1}$$

$$log\ \text{Capital Surplus}_{b,t} = \boldsymbol{\beta_2}\text{More capital-constrained}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_b + f_t + \epsilon_{b,t} \tag{2}$$

Meanwhile, the coefficient β_1 in equation 3 reflects the evolution of capital surpluses for banks that received greater capital relief from the BoE's cut to the CCyB, relative to peers. When the dependent variable is *log* capital surplus in equation 4, the coefficient β_2 reflects the change in capital for high CCyB pass-through rate banks relative to their starting point. If

[23] At the onset of the pandemic, authorities extended transitional relief to offset the impact of increasing IFRS9 provisions on capital (BCBS, 2020). This was due to concerns about procyclical effects arising from a large ramp-up in IFRS9 provisions that would have drawn down on banks' capital ratios, caused by the high economic uncertainty (Saporta, 2021). Along with other Covid-19 measures, this supported an increase in banks' capital ratios over the pandemic (Figure 1). To further account for any residual impact of increased IFRS9 provisions during the pandemic on banks' behaviour, we also do a robustness check where we include provisions as a control in Table 11. The results remain robust to this inclusion.

releasing the CCyB successfully alleviated capital constraints, we would observe negative β_3 and β_4 coefficients.

$$\text{Capital Surplus}_{b,t} = \boldsymbol{\beta_3}\text{High Capital-Relief}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_b + f_t + \epsilon_{b,t} \tag{3}$$

$$log\ \text{Capital Surplus}_{b,t} = \boldsymbol{\beta_4}\text{High Capital-Relief}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_b + f_t + \epsilon_{b,t} \tag{4}$$

Across these equations, the coefficients of interest β_1 and β_2 and β_3 and β_4 would be negative if the policies encouraging use of capital buffers and releasing the CCyB, respectively, were successful in materially relaxing banks' capital constraints, necessitating fewer defensive actions by banks to support their capital positions after the pandemic.

5.2 Evolution in lending

Next, we compare the impact of expansionary macroprudential policies on banks' lending behaviour using the UK mortgage register.

In equation 5, we test whether a new loan issued to borrower i in local area l by bank b in quarter t was priced differently after the onset of the pandemic by more capital-constrained (γ_1) or high capital-relief banks (ϕ_1), relative to their respective peers. Similarly, in equation 6, we test whether more capital-constrained (γ_2) or high capital-relief banks (ϕ_2) altered their average issued loan value after the onset of the pandemic, relative to peers. If the two policy experiments successfully alleviated capital constraints, then conditional on the parallel trends assumption being satisfied, more capital-constrained and high capital-relief banks would have both had capacity to support lending by lowering rates and increasing loan values (i.e. $\gamma_1, \phi_1 < 0$ and $\gamma_2, \phi_2 > 0$).

$$
\begin{aligned}
log \text{ Interest}_{i,l,b,t} = \; & \boldsymbol{\gamma_1} \text{More capital-constrained}_b \times \text{Post-Covid}_t \qquad (5) \\
& + \boldsymbol{\phi_1} \text{High capital-relief}_b \times \text{Post-Covid}_t + \delta_1 \text{Post-Covid}_t \times Z_{i,l,b,t} \\
& + \delta_2 X_{b,t-1} + f_{l,t} + f_{l,b} + \epsilon_{i,l,b,t} \\
log \text{ Loan Value}_{i,l,b,t} = \; & \boldsymbol{\gamma_2} \text{More capital-constrained}_b \times \text{Post-Covid}_t \qquad (6) \\
& + \boldsymbol{\phi_2} \text{High capital-relief}_b \times \text{Post-Covid}_t + \delta_1 \text{Post-Covid}_t \times Z_{i,l,b,t} \\
& + \delta_2 X_{b,t-1} + f_{l,t} + f_{l,b} + \epsilon_{i,l,b,t}
\end{aligned}
$$

Identification. To isolate the causal effects of the two policies on credit supply, we must account for any other factors that may vary systematically between the two comparison groups of interest, impacting lending outcomes. This includes the unprecedented scale of support announced by the UK government during the pandemic. The provision of government guaranteed schemes and their regulatory treatment would have acted to offset the capital cost and higher risk associated with lending to specific segments of the market during the pandemic, acting as a clear confounding factor for our empirical strategy. This would have been particularly acute for corporate lending, large portions of which were guaranteed under various Covid-19 government schemes.[24]

We tackle this challenge in a couple of different ways. First, we restrict our analysis to household mortgage lending, which is helpful as the majority of fiscal support for lending in the UK was directed at corporate, rather than household, lending. Where support schemes were introduced for households, specifically mortgage payment holidays and the Coronavirus Job Retention Scheme (CJRS), these were primarily targeted at existing borrowers that formed the *stock* of banks' balance sheets rather than directly enabling the *flow* of new lending - our focus being on the latter.[25] These measures were intended to support household

[24] These included the Bounce Bank Loan Scheme (BBLS) and the Coronavirus (Large) Business Interruption Loan Scheme (CBILS and CLBILS) (for more details, see Browning, 2022).

[25] The Coronavirus Job Retention Scheme (CJRS), launched on 20 March 2020, allowed employers to furlough workers while the government paid up to 80% of their salaries. Mortgage payment holidays, also announced in March 2020, allowed existing mortgagors who struggled to keep up with mortgage payments due to the pandemic to defer payments.

incomes and reduced the likelihood that household debt would amplify the pandemic-driven recession (Franklin *et al.*, 2021).

Second, we exploit the richness of our loan-level dataset, which contains granular information about the geographic location of the property for which each mortgage is issued. This location is at the 'local area' (i.e. three-digit postcode) l level.[26] With this precise geographic information, we are able to account for time-varying shocks to credit demand, such as indirectly through Covid-19 schemes and other housing market policy interventions, at a very granular level through local area × time fixed effects ($f_{l,t}$).[27] As there is no way in our dataset to identify new borrowers that were supported by income support measures during Covid-19, we rely on the fact that the scale and take-up of household support schemes was correlated with incidence of Covid-19 disease and, thus, varied across different administrative regions of the UK (Adams-Prassl *et al.*, 2020).[28] We are also able to account for any systematic differences in geographical exposure (e.g. branch presence), competition, pricing, or lending volumes across our different bank comparison groups, through local area × bank fixed-effects ($f_{l,b}$).

We also need to account for systematic differences in borrower risk perceptions across bank groups as a result of the pandemic. To do this, we rely on information on borrower characteristics such as loan-to-income (LTI) ratio, loan-to-value (LTV) ratio, history of credit impairment, first-time borrower status, self-employment, income verification by the lender, and interest-only repayments. We allow the relationship of these risk characteristics with the dependent variables to vary with the onset of Covid-19, Post-Covid$_t$ × $Z_{i,l,b,t}$.

[26] An average UK 'local area' or 3-digit postcode contains 24,000 people based on the 2011 census, and is approximately a quarter the size of the average US county. There are 2780 local areas in our dataset with an average of 625 newly issued loans. For context, this is more granular than the analysis done by (Peydró *et al.*, 2023), who use a Local Administrative Unit Level 1 (LAU1).

[27] One such scheme that may have directly affected household loan demand was the temporary reduction in Stamp Duty Land Tax (SDLT) announced on 8 July 2020, which increased the threshold above which SDLT is paid from property values of GBP125,000 to GBP500,000. Note however that this policy would not be able to explain differences in lending outcomes in Q1 & Q2 2020.

[28] Our local area × time fixed-effects in loan-level mortgage lending data are analogous to the Khwaja and Mian (2008) firm × time fixed effects used in analyses of corporate lending data. The difference is that instead of looking at one firm borrowing from several banks, we rely on mortgages issued by different banks in a sufficiently granular geographic local area.

Finally, we continue to account for systematic differences in business models across our comparison groups through the inclusion of time-varying bank controls ($X_{b,t-1}$). These are similar to the controls we include in equations 1 and 2 but we also add banks' size (log total assets), density ratio (or average risk-weight), and the average total capital of the bank in 2019 interacted with the Post-Covid$_t$ dummy as well (similar for example to Auer *et al.*, 2022; Peydró *et al.*, 2023; De Jonghe *et al.*, 2020; Jiménez *et al.*, 2017).[29]

6 Results: Impact on capital and lending

6.1 Capital surpluses

Unconditionally, both more and less capital-constrained banks increased their CET1 surpluses by around 1.5pp of RWAs after the onset of the pandemic, as shown in panel (a) Figure 5. The average CET1 surplus increased from 1.3pp to 2.6pp for more capital-constrained banks, and from 5.4pp to 7.1pp for less capital-constrained banks from Q4 2019 to Q4 2020. However, as shown in panel (b) of Figure 5, relative to the levels with which these banks entered the pandemic, the increase was much larger for more capital-constrained banks despite exhibiting similar pre-pandemic trends. Conversely, panel (a) of Figure 6 demonstrates that high capital-relief banks maintained more stable capital surpluses after the onset of the pandemic. Specifically, the increase in capital surplus was from 5.4pp to 7.4pp for low capital-relief banks, representing an increase of 37%, and from 5.2pp to 5.8pp for high capital-relief banks, representing an increase of only 12%.

The coefficients reported in Table 4 accord well with the unconditional trends discussed above. Columns (1) and (2) present estimates of the coefficient β_1 described for more capital-constrained banks in equation 1 and for high capital-relief banks in equation 3, using bank-level balance sheet data. After controlling for potential confounding observable and unobservable factors, the coefficient in column (1) is insignificant and indicates that there was

[29]The results are robust to the inclusion of an expanded set of borrower, loan, and bank characteristics as shown in appendix Table 19. In an unreported exercise, we also include banks' share of non-government guaranteed lending, intending to capture spillovers, if any, from participation in Covid-19 schemes, but find that the results remain similar.

no statistically significant difference between more or less capital-constrained banks in how their surpluses increased after the onset of the pandemic. This behaviour is likely reflective of a precautionary motive across all UK banks at the onset of the pandemic due to the heightened macroeconomic uncertainty, increase in credit risk, and likelihood of unexpected losses due to borrower defaults. The coefficient in column (2) is negative and statistically significant at the 5% level, providing evidence for the hypothesis that high capital-relief rate banks increased their capital surpluses by 1.3pp of RWAs *less* than low capital-relief banks.

Columns (3) and (4) of Table 4 present estimates of the coefficient β_2 for the *log* specifications for more capital-constrained banks in equation 1 and for high capital-relief banks in equation 3. Column (3) suggests that as a proportion of their starting point, more capital-constrained banks grew their surpluses by approximately 43% more than less capital-constrained banks, and this is statistically significant at the 1% level. Column (4) shows a qualitatively consistent result - that high capital-relief banks grew surpluses by approximately 13% less than low capital-relief banks, relative to their respective starting points, though this is not statistically significant at conventional levels (p-value = 0.15). These results suggest that the first policy experiment of having usable capital buffers was not successful in preventing banks' from taking defensive actions at the time of the pandemic, but the second policy experiment cutting the CCyB was.

Figures 7 and 8 plot the coefficients obtained by regressing the dependent variables on the dummies for capital-constrained and capital-relief banks respectively, interacted with a full set of time-dummies. The reference period is Q1 2019 throughout, and the models are as described in equations 1 and 3. The coefficients in pre-Covid periods are not quantitatively or statistically different from 0, providing support for the parallel-trends assumption. However, it is interesting to note that the time profiles of the results, which get stronger over the course of 2020, are suggestive of a lagged transmission mechanism.

We check these results are robust to using alternate definitions of more capital-constrained banks, specifically, banks in the bottom quartile of the cross-sectional surplus distribution in 2019, as well as using the continuous versions of the 2019 effective surplus and CCyB pass-through rate variables. Table 10 shows that the results are not sensitive to the precise definition. We also use an expanded set of balance sheet controls and control alternately for size (log total assets) and the density ratio in Table 11, and show that the results are not sensitive to the precise set of control variables included.

6.2 Impact on lending

Figure 12 shows the quarterly time series of offered interest rates by product types, i.e. for different combinations of fixed terms and LTVs, from *Moneyfacts* between Q1 2019 and Q4 2020. There was an increase in loan rates offered by lenders across the board during the pandemic, but this increase was particularly acute for higher LTV mortgages ($> 90\%$). This is indicative of the increase in risk-aversion and tightening of lending conditions that occurred at the onset of the pandemic, which we will discuss in Section 7.

The baseline results using loan-level data are presented in columns (1) and (2) of Table 5. Column (1) presents estimates of the coefficients γ_1 and ϕ_1 described in equation 5. The coefficient on the double interaction of Post-Covid$_t$ $\times$ More capital-constrained$_b$ is positive and statistically significant at the 1% level. It supports the unconditional trends observed in the data, and shows that more capital-constrained banks maintained loan rates that were 3.7% higher during the pandemic as compared to low capital-constrained banks. For a more capital-constrained bank with an average loan rate of 2.0pp in Q4 2019, the coefficient implies an increase of 7.5 basis points relative to peers. In contrast, the coefficient on the double interaction Post-Covid$_t$ $\times$ High capital-relief$_b$ is negative and also statistically significant at the 1% level, reflecting that high capital-relief banks decreased loan rates by 3.7% during the 2020 pandemic. For a high capital-relief bank with an average loan rate of 2.0pp in Q4 2019, these results reflect a decline of about 7.4 basis points. This implies that the cut to the UK CCyB rate was helpful in supporting lending during the stress, likely because it fundamentally relaxed banks' capital constraints.

Column (2) presents estimates of the coefficient γ_2 and ϕ_2 described in equation 6. The coefficient on the double interaction of Post-Covid$_t$ $\times$ More capital-constrained$_b$ is negative and statistically significant at the 1% level, indicating that more capital-constrained banks maintained loan values that were approximately 2.2% lower after the onset of the pandemic as compared to their peers. For a more capital-constrained bank, this represented an average loan value that was approximately GBP 4800 lower. The results on the double interaction Post-Covid$_t$ $\times$ High capital-relief$_b$ show that high capital-relief banks were able to maintain loan values that were approximately 2.3% higher, translating to about GBP 4500 for an average bank.

Figure 9 shows that there are no systematic differences in loan rates across the two groups of banks prior to the onset of the pandemic – for more capital-constrained banks in panel (a) and high capital-relief banks in panel (b) – as the coefficients in pre-Covid periods are not quantitatively or statistically different from 0. This is also true for loan values in Figure 10. These results are therefore consistent with the parallel-trends assumption.

We subject our results to a battery of robustness checks. First, similar to before, we vary the definition of more capital-constrained banks, using an alternate definition of banks in the bottom quartile of the cross-sectional surplus distribution in 2019, or the continuous 2019 effective surplus variable as another alternative. For the high capital-relief banks, we also replace the dummy by the continuous version of the variable.

Table 12 shows that the results are remarkably consistent across the various specifications, even if the statistical significance varies. When we use the bottom quartile definition in columns (2) and (5), we see that the results are in line (with varying significance) with those obtained in the baseline in columns (1) and (4), i.e. that more capital-constrained banks maintained higher loan rates and lower loan values after the onset of the stress, while high capital-relief banks maintained lower loan rates and higher loan values. When we use the continuous versions of both explanatory variables, we expect the sign on the more capital-constrained variable to be reversed to be consistent with our hypotheses, as an increase in the variable now reflects an increase in surplus. The expected sign on the CCyB pass-through variable remains the same as before - positive for loan rates and negative for loan values. These results are in columns (3) and (6). Another way to test the sensitivity of the results to our definitional choices is to change the definition of high surplus banks, by crowding in the top quartile of the distribution which we had dropped so far. As shown in columns (1) and (2) of Table 17, we see that the results are consistent even when the sample is expanded.

We also address any concerns regarding the timing of the two policies and our assumption around how quickly banks reacted. While Covid-19 emerged as a global concern over Q1 2020, the UK CCyB rate was decreased to 0% only in March 2020. Using Post-Covid$_t$ assumes that high capital-relief banks started adjusting immediately. If they did not, then our baseline results would be downward biased. Therefore, we check whether our results are maintained when Q1 2020 is dropped from the sample (which reduces our sample size by 200,000 observations). Columns (1) and (2) of Table 18 support this hypothesis and indeed become stronger. They indicate that more capital-constrained banks increased interest rates by 5.5% and decreased loan values by 2.7%, while high capital-relief banks decreased rates by 4.2% and 3%. However, as there is no way to know for sure when banks started responding to the crisis, given their global presence and the global reach of the pandemic, we continue to prefer the more conservative baseline estimates. Finally, we check whether the results are robust to the inclusion of an expanded set of borrower, loan, and bank balance sheet controls. Columns (1) and (2) of Table 19 show that the results are robust to the inclusion of these controls.

The moderating effect of explicitly releasing capital on buffer usability frictions. Overall, the results for high capital-relief banks closely mirror, in reverse, those for more capital-constrained banks, indirectly reflecting the benefits of releasing, i.e. cutting the CCyB, over the policy encouraging use of buffers. All else equal, a release of the CCyB should be of greater value to a bank that is *close* to its regulatory buffers to begin with. We now test whether this moderating effect exists by running extensions of equations 5 and 6, where we include a triple interaction of Post-Covid$_t$ × Capital-constrained$_b$ × Pass-through rate$_b$, in addition to both base interactions. To avoid running into empirical issues stemming from an uneven split of banks across categories, we use the continuous version of the CCyB pass-through rate rather than the dummy of high capital-relief. If releasability helps reduce capital constraints, then we would expect more capital-constrained banks with higher CCyB pass-through rates to have had greater capacity to maintain credit provision as compared to more capital-constrained banks with low CCyB pass-through rates. That is, the coefficient on the triple interaction coefficient should be negative when the dependent variable is loan rate and positive when the dependent variable is loan value.

Column (3) of Table 5 confirms the above results that more capital-constrained banks with low CCyB pass-through rates maintained materially higher loan rates (significant at the 10% level), while less capital-constrained banks with high CCyB pass-through rates decreased them (though this is not statistically significant). The coefficient on the triple interaction term, while not significant, indicates that each additional percentage point of CCyB pass-through rate acts as a moderating factor on higher interest rates on loans supplied by more capital-constrained banks. Similarly, from column (4), we see that more capital-constrained banks with low CCyB pass-through rates materially reduced loan values, but each additional percentage point increase in their CCyB pass-through rate helped to partially offset this contraction of credit supply. These results are consistent with regulatory buffers behaving as more of a binding constraint. Though we consider these results as descriptive, they are nevertheless interesting for policy: they are suggestive that releases of regulatory buffers (such as the CCyB) can act to dampen unintended consequences associated with potential buffer usability frictions.

Extensive margin analysis. So far, our loan-level analysis has focused on intensive margins of credit supply adjustment by banks: did lenders tighten terms on the loans they offered? We now also consider impacts on more aggregate measures of the provision of credit - i.e. the extensive margins of loan supply: did lenders reduce the number or volume of loans that they offered, or their likelihood of offering a risky loan? This takes us a step closer to assessing effects on the aggregate availability of credit, which is of particular concern to regulators during episodes of stress. To do so, we first aggregate our loan-level dataset to bank-region-quarter level, and use specifications set out in equation 7 and equation 8. The dependent variables we use are the log of number of loans and log of total volume of loans. Aggregating at the region level r allows us to still account for credit demand – albeit at a less granular level than before – by including region $\times$ time fixed effects ($f_{r,t}$), and differences in bank geographic exposure and competition, by including region $\times$ bank fixed effects ($f_{b,r}$). If more capital-constrained banks contracted credit provision in aggregate, we would expect to see a significant decline in number and total volume of loans: $\tau_1 < 0$ and $\tau_2 < 0$. Similarly, if high capital-relief banks maintained higher credit provision in aggregate, then we would expect to observe an increase in number and total volume of loans: $\theta_1 > 0$ and $\theta_2 > 0$.

$$\begin{aligned} log \text{ Number of Loans}_{b,r,t} =& \boldsymbol{\tau_1}\text{More capital-constrained}_b \times \text{Post-Covid}_t \qquad (7) \\ &+ \boldsymbol{\theta_1}\text{High capital-relief}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_{b,r} + f_{r,t} + \epsilon_{b,t} \end{aligned}$$

$$\begin{aligned} log \text{ Volume of Loans}_{b,r,t} =& \boldsymbol{\tau_2}\text{More capital-constrained}_b \times \text{Post-Covid}_t \qquad (8) \\ &+ \boldsymbol{\theta_2}\text{High capital-relief}_b \times \text{Post-Covid}_t + \delta X_{b,t-1} + f_{b,r} + f_{r,t} + \epsilon_{b,t} \end{aligned}$$

These results are shown in Table 8, with log number of loans and log volume of loans as the dependent variables in columns (1) and (2) respectively, based on equations 7 and 8. The results indicate that neither more capital-constrained banks nor high capital-relief banks changed either the number or volume of loans they supplied in a significantly different manner to their peers. Given the unprecedented scale of fiscal, monetary, and prudential support, it is not entirely surprising that these banks did not withdraw from the mortgage lending market overall, instead choosing to adjust along intensive margins of credit supply.

7 Results: Risk-taking

Having examined the consequences of the two policy experiments on overall credit supply, we now turn to their potential impacts on banks' risk-bearing capacity. We approach this question by defining 'risky loans' in two different ways: a Covid-specific measure of borrower risk using healthcare data on local area Covid-19 case rates, and a conventional measure of borrower risk, based on LTI and LTV ratios. In the latter case we also go one step further by zooming in on outcomes for a particularly vulnerable set of borrowers - first-time buyers.

7.1 Covid-specific borrower risk measure

First, we focus on exogenous local shocks to borrower risk by calculating average Covid-19 case rates (per 100,000 persons) between March and December 2020 at local area l level.[30] Then, we classify areas as "high-risk" if they are above the $75th$ percentile of the cross-sectional distribution of case rates.[31] This makes use of the fact that local case rates are correlated with the imposition of various government pandemic policies, such as local restrictions, which are found to be associated with higher unemployment, a higher rate of bankruptcies, and a higher risk of loan defaults (Barkit *et al.* (2020); Hasan *et al.* (2021)). Indeed, Temesvary and Wei (2021) tie this together by explicitly finding that US banks' exposure to countries with high case rates was associated with decreased lending.[32] The increase in cash-flow constraints and possibility of negative house equity is likelu to have increased household default probabilities (see Ganong and Noel, 2022), representing an exogenous increase in borrower risk, above and beyond classic risk characteristics that we already account for.

We employ a similar specification as earlier, as shown in equations 9 and 10, except we now use a triple interaction to compare the interest rate and loan value provided by the same bank in the same quarter in a "high-risk" local area, compared to a "low-risk" local area. The

[30] We merge the Covid-19 case rate data with our loan-level data at the local area (three-digit) postcode level using ONS geographic conversion codes. We are able to match 2100 local areas out of 2780 and use the average case rate over the course of 2020 due to incomplete data and coverage issues in the initial period of the pandemic (Fetzer, 2022).

[31] As a robustness check, we show that the results are very similar when we use the median as an alternate cut-off for the high case-rate category.

[32] In addition, for the UK there is a statistically significant correlation (0.7) between the time series of Covid-19 case rates and unemployment between March and December 2020. This test uses data on seasonally adjusted unemployment rates for population aged 16 and over from the ONS (available here).

coefficients of interest are γ_1 and ϕ_1 on the triple interactions More capital-constrained$_b$ × Post-Covid$_t$ × High-Risk Area$_{l,t}$ and High capital-relief$_b$ × Post-Covid$_t$ × High-risk area$_{l,t}$ respectively. The respective policies would be considered successful if the coefficients were negative for prices, and positive for loan values. However, we expect that the earlier results will be reflected here in that more capital-constrained banks would have had limited risk-taking capacity compared to high capital-relief banks.

Utilising an additional dimension of variation allows us to replace the time-varying bank-level balance sheet controls with bank × time fixed effects, $f_{b,t}$, stripping out *all* time-varying bank unobservables that may drive differences in lending conditions (for example, in response to other central bank or government policies). We also rely on our fixed effects and the triple interaction setup to account for confounding factors that may be related to both Covid-19 case rates as well as lending outcomes, especially loan values. For example, it might be that high-risk local areas l witnessed a reduction in housing demand and therefore house prices, which may have depressed loan values mechanically. These types of factors are accounted for in two ways. First, through the local area × time fixed effects ($f_{l,t}$), which capture time-varying unobservables at the local area level, including but not limited to macroeconomic conditions and, housing demand and prices. Second, local area conditions like house price dynamics are common to the two groups of banks, and cannot explain *differential* responses of one set of banks over another.

$$
\begin{aligned}
&log\ \text{Interest}_{i,l,b,t} \\
&\quad = \boldsymbol{\gamma_1}\text{More capital-constrained}_b \times \text{Post-Covid}_t \times \textbf{High-risk area}_{l,t} \\
&\quad + \boldsymbol{\phi_1}\text{High capital-relief}_b \times \text{Post-Covid}_t \times \textbf{High-risk area}_{l,t} \\
&\quad + \delta_1\text{Post-Covid}_t \times X_{i,l,b,t} + f_{l,t} + f_{b,t} + f_{l,b} + \epsilon_{i,l,b,t} \qquad (9)
\end{aligned}
$$

$$
\begin{aligned}
&log\ \text{Loan Value}_{i,l,b,t} \\
&\quad = \boldsymbol{\gamma_1}\text{More capital-constrained}_b \times \text{Post-Covid}_t \times \textbf{High-risk area}_{l,t} \\
&\quad + \boldsymbol{\phi_1}\text{High Capital-Relief}_b \times \text{Post-Covid}_t \times \textbf{High-risk area}_{l,t} \\
&\quad + \delta_1\text{Post-Covid}_t \times X_{i,l,b,t} + f_{l,t} + f_{b,t} + f_{l,b} + \epsilon_{i,l,b,t} \qquad (10)
\end{aligned}
$$

The results for loan rates and loan values are reported in column (1) and (2) of Table 6 respectively. The interest rate coefficient for more capital-constrained banks in column (1) is positive, but not statistically significant, while the coefficient for loan value is negative and significant. That is, while more capital-constrained banks maintained higher loan rates on average (as shown in our baseline lending specifications), these were not materially higher for local areas which were more affected by Covid-19. However, the loan values they offered were significantly lower in these areas, by around 1.4% on average. This indicates that more capital-constrained banks exhibited higher risk aversion and greater sensitivity to borrower risk during the pandemic, and this was reflected in the terms they offered on new lending, implying that the policy encouraging buffer use did not prevent these banks from de-risking to defend their capital positions.

On the other hand, high capital-relief banks continued to support lending in areas with elevated pandemic-related borrower risk, as they maintained loan rates that were 1.5% lower and loan values that were 0.6% higher in these areas, as compared to peers. Both these results are significant at the 1% and 10% levels respectively. This shows that cutting the CCyB rate was successful in enabling banks to maintain additional risk-taking capacity during the stress and suggests that buffer releases can help mitigate against buffer usability frictions.

In Table 13, we repeat the same exercise using an alternate definition of a high-risk local area, this time using the cross-sectional median as a cut-off rather than the 75^{th} percentile. The results are very similar to those reported above.

7.2 Conventional borrower risk measures

While the above approach relies on unobservable Covid-19 related risk characteristics of borrowers in a particular local area l, we can also explore changes in lenders' sensitivity to conventional risk measures such as LTV and LTIs. Therefore, in a second approach, we consider particularly risky loans on the basis of these historically conventional measures. A 'risky loan' is defined as a loan which is LTV $\geq$ 90% and LTI $\geq$ 4.5. In the baseline, we compare the loan rate and loan value outcomes for this 'risky' category of borrowers relative to all loans with LTV $\leq$ 90% and LTI $\leq$ 4.5. We propose one alternate definition of a risky loan (LTV $\geq$ 85% and LTI $\geq$ 4.5) and two alternate, lower risk, groups for comparison: LTV $\leq$ 75% and LTI $\leq$ 4.5 and LTV $\leq$ 60% and LTI $\leq$ 4.5. The results are consistent across all definitions.

Here we exploit the fact that these risky loans reflect higher likelihoods of default, in addition to being subject to higher risk-weights and other regulatory constraints in the UK. Higher LTIs and LTVs attract riskier borrowers, are associated with higher default probabilities (Corbae and Quintin, 2015; Benetton *et al.*, 2018; Lazarov and Hinterschweiger, 2018), and represent higher expected losses in case of default for lenders.[33] We follow Campbell and Cocco (2015) who argue that regulators should think about combinations of LTV and LTI ratios rather than controlling for these parameters in isolation. The authors also show that household default probabilities become particularly large as LTV ratios increase, especially as they exceed 90%. Mortgages above 90% LTV are also costlier in capital terms as they are faced with a significantly higher risk-weight compared to mortgages below 90% (see Tables A1 and A2 in PRA, 2021). As LTIs increase, cash-flow shocks can be particularly material due to lower mortgage affordability and tighter borrowing constraints. In the UK, a regulatory limit on the proportion of mortgage lending banks could do above LTI ratios $\geq$ 4.5 was introduced in June 2014, which has been shown to be a relevant regulatory constraint for bank lending (Peydró *et al.*, 2023).[34] Therefore, we expect that more capital-constrained banks would have been less able to support lending at high LTV and high LTI ratios after the onset of the pandemic.

The specification is similar to those for the Covid-specific borrower risk measure (equations 9 and 10), except we now replace the high-risk area dummy with a risky-loan dummy, and

[33] As a result, LTV and LTI limits are the most commonly used macro-prudential regulations around the world (Acharya *et al.*, 2022; Alam *et al.*, 2019).

[34] In June 2014, the FPC recommended to the PRA and FCA to ensure that mortgage lenders did not extend more than 15% of their total number of new residential mortgages with LTI ratios at or greater than 4.5 (Bank of England, 2014).

modify the borrower and loan risk controls in $X_{i,l,b,t}$, replacing LTI and LTV with log gross income and log property value instead. This is due to the fact that risky loans are now constructed on the basis of LTV and LTI ratios.

Figure 12 uses data from *Moneyfacts* to show that there was a material decline in number of products with higher LTVs offered by banks after the onset of the pandemic. While there was almost no change in the number of products offered with LTVs less than 60%, products with LTVs between 60% and 90% declined significantly, and products in the $> 90\%$ market disappeared almost completely. In line with this, our loan-level dataset shows that there was an overall decline of 14% in the number of loans provided in the non-risky loan category, compared to a much larger decline of 60% in the number of risky loans provided.

Columns (1) and (3) of Table 7 show that the results from re-running equations 9 and 10 based on this conventional measure of risky loans are in line with expectations. More capital-constrained banks reduced risk-taking during the pandemic to a greater extent than their peers, while high capital-relief banks maintained relatively looser terms on risk-taking activity during the pandemic. Column (1) shows that more capital-constrained banks maintained loan rates that were 7% higher on average for risky loans, and that this effect was statistically significant at the 5% level. The coefficient implies an increase of 20 basis points for an average risky loan for a more capital-constrained bank. On the other hand, high capital-relief banks were able to supply risky loans at interest rates that were 9% lower than their peers, also significant at the 1% level. For an average risky loan provided by a high capital-relief bank, these results reflect a relative decline of about 20 basis points.

The results for loan values are set out in column (3) of Table 7. More capital-constrained banks provided, on average, 2% lower loan values for risky loans, though this is not statistically significant. This is a relative decline of approximately GBP 21000. Meanwhile, high capital-relief banks maintained loan values that were 9% higher, amounting to a roughly GBP 23,280 relative increase for an average risky loan provided by this bank.

First-time buyers. Finally, we zoom in on first-time buyers as a particularly risky category of borrowers.[35] These borrowers typically face significant financing constraints in the form of down payments, have different demographic profiles relative to other classes of borrowers (e.g. remortgagors), and lack a long credit history. In periods of stress and elevated macroeconomic uncertainty, capital-constrained banks are likely to differentiate across this class of borrowers due to the lack of a pre-existing lending relationship, while prioritising existing borrowers. The rationale for zooming in to risky loans to first-time buyers is that the default risk is higher for these loans (as shown by Kelly *et al.* (2015) for Ireland), and thus, this class of loans are likely to be shelved first.

We test whether more affected banks offered tighter terms on risky loans to first-time buyers by re-running the specification used for the traditional risk measures for the sub-sample of first-time buyers. We expect that more capital-constrained banks, due to their higher sensitivity to increased borrower risk, would have maintained higher loan rates and loan values to these borrowers, with the opposite results expected for high capital-relief banks.

Across these risky lending specifications, our results strengthen when we focus on first-time buyers. As discussed above, these borrowers would have been considered even riskier by banks, all else equal. First-time buyers were the primary recipients of 'risky loans', with approximately 80% of these loans going to these borrowers. From 2019 to 2020, there was a decline of 4.2% in the number of less risky loans provided to first-time buyers, i.e. those defined as LTV $\leq$ 90% and LTI $\leq$ 4.5, while the number of risky loans overall declined by 46%.

Columns (2) and (4) of Table 7 set out these results. The results strengthen both in statistical significance and economic magnitude across our more capital-constrained and high capital-relief coefficients. In particular, our loan value result for more capital-constrained banks triples in magnitude and becomes statistically significant at the 1% level.

[35] Cuts to house purchase stamp duty (SDLT) during Covid-19 were likely to have had a more material impact on first-time buyers. To avoid the effects of these policies from confounding our results, we control for property values (allowing the relationship with our variables of interest to vary with the onset of the pandemic). In addition, there is likely to be a large geographical dimension to the increased credit demand, which should be further captured by our granular local area-time fixed effects.

Robustness checks. As discussed previously, we do a few key checks to ensure that our results are not sensitive to the precise definition of the risky loan variable. We have two approaches. First, we vary the comparison group for the risky loan, i.e. by looking at LTV $\geq$ 90% and LTI $\geq$ 4.5 against LTV $<$ 90%, 75%, and 60%, while maintaining the LTI condition. The results for this check are reported for the whole sample in Table 14. Columns (1)-(3) show that more capital-constrained banks maintained higher interest rates on these loans across all definitions, with the coefficient varying between 7.1% to 9.3%, and columns (4)-(6) show that they maintained lower loan values by between 0.5% to 5.3% although these coefficients are all quite imprecise. Similarly, high CCyB pass-through banks were able to maintain interest rates that were between 8.7% and 10.3% lower and loan values that were between 5.4% and 8.6% higher. As with the baseline, the results only get stronger in terms of both statistical significance and economic magnitudes when we restrict attention to first-time buyers in Table 15.

In our second test, we change the threshold for risky loan from 90% to 85%. The results are shown in Table 16 for the entire sample in columns (1) and (3), and for the sub-sample of first-time buyers in columns (2) and (4). We see that the conclusions from the previous exercises are not sensitive to the precise definition of the risky loan variable.

Extensive margin analysis. Finally, we turn to changes in the probability of lenders' issuing a risky loan (equation 11). The dependent variable is now a dummy that takes value one if the loan is 'risky'. The rest of the linear probability model is similar to before, with one exception: we now additionally account for interest rate and loan value on the right-hand side, also interacted with Post-Covid$_t$, for consistency. Our hypothesis is that more capital-constrained banks would have had more limited capacity to issue risky loans after the onset of Covid-19 ($\tau_3 < 0$), while high capital-relief banks would have been able to maintain a greater degree of risk-taking ($\theta_3 > 0$).

$$
\begin{aligned}
\mathbb{1}[\text{Risky loan}]_{i,l,b,t} = & \boldsymbol{\tau_3}\text{Capital-constrained}_b \times \text{Post-Covid}_t \\
& + \boldsymbol{\theta_3}\text{Capital-relief}_b \times \text{Post-Covid}_t + \delta_1 \text{Post-Covid}_t \times X_{i,l,b,t} \\
& + \delta_2 X_{b,t-1} + f_{b,r} + f_{r,t} + \epsilon_{b,t}
\end{aligned}
\tag{11}
$$

These are shown in Table 9. Across the definitions of risky loans, and after controlling for potential confounding factors, we find that there was also no significant relative change in the probability of issuing risky loans by more capital-constrained banks. However, high capital-relief banks did maintain significantly higher probabilities of issuing risky loans after the onset of the pandemic relative to peers.

8 Conclusions and policy implications

Macroprudential policies have become an integral part of policymakers' toolkits around the world. Though there is significant evidence that *tightening* these policies works in line with their objectives, empirical evidence on *expansionary* policies has remained relatively scarce. This has been driven in part by a limited case history of such actions, especially post-Basel III. But even when they have been deployed, causally studying the impact of these policy actions has been challenging because of the endogenous nature of the stress in which they are implemented, and a lack of detailed information on how the policy transmits to different banks.

In this paper, we zoom in on two expansionary macroprudential actions in the UK, and investigate their impact on bank lending and risk-taking during a recent stress, the Covid-19 pandemic. The first policy is the capital buffer framework, which requires banks to finance themselves with additional capital on top of their minimum requirements. These buffers are meant to be drawn-down, or usable, as needed in stress to support lending. Reflecting their macroprudential importance, public authorities issued statements at the onset of the pandemic reinforcing the usability of capital buffers. The effectiveness of the framework was tested for the first time post-implementation during Covid-19, even though they were introduced as part of the Basel III reform package. The second policy was the cut to the UK's CCyB rate at the onset of the pandemic. Focusing on the UK allows us to contrast these two policies and comment on possible policy options to spur lending in a stress.

Each policy was designed to alleviate capital constraints and mitigate procyclical credit dynamics. We use detailed information on bank capital structures and a novel methodology to disentangle banks' relative exposure to each policy. For the first policy (the usability of capital buffers), we identify the extent to which banks were capital-constrained ahead of the pandemic. For the second policy (the cut to the CCyB), we use confidential data to calculate UK banks' exposure to the capital release, or the pass-through rate. This measures the varying degree to which the CCyB cut 'passed through' to a fall in banks' capital requirements.

Using data from the UK mortgage credit register, and a difference-in-differences specification, we show that expansionary macroprudential policy can mitigate procyclical lending dynamics. However, policy design matters: while the CCyB cut worked as intended, the usable regulatory capital buffers did not. High capital-relief banks, i.e. those that received greater capital relief from the UK cut to the CCyB, defended their capital positions to a lesser extent than their peers, maintaining looser credit supply conditions. They also exhibited greater capacity for risk-taking. In contrast, more capital-constrained banks contracted domestic credit supply by tightening interest rates and lending volumes on new mortgage loans. They also exhibited lower risk-taking capacity by extending credit at tighter terms to particularly risky areas and borrowers.

One explanation for these differences in efficacy across the two policies relates to how they ease capital constraints. Frictions to drawing down capital to support lending during shocks can exist for numerous reasons, such as adverse supervisory reactions, negative market stigma effects, or the triggering of automatic restrictions on capital distributions. Statements reinforcing the usability of capital buffers in a stress can help to alleviate some, but not all, of these frictions. However, an explicit cut to the CCyB that applies to all banks helps address all these barriers *simultaneously* and may therefore be more effective in supporting continued credit provision to the economy when unexpected shocks to credit risk arise. While we provide empirical evidence showing the presence of buffer usability frictions, and point to one possible policy solution, the causes of these frictions are outside the scope of our paper. We leave the assessment of these and subsequent work on optimal policy design as questions for future research.

The banking sector has remained resilient during the Covid-19 period, maintaining capital ratios well above minimum and buffers requirements. This has been partly due to more resilient bank balance sheets relative to before the Global Financial Crisis, but also rapid responses by central banks in reducing requirements, releasing buffers, restricting capital distributions where appropriate, and unprecedented monetary and fiscal support during the stress. The fact that we nonetheless find evidence that capital constraints affected the intensive margins of credit supply gives credence to the presence of usability frictions. This is striking in and of itself, suggesting that these constraints might become more binding as government support is withdrawn and subsequent shocks arise. The evidence from this paper, consistent with similar recent studies undertaken in other jurisdictions, indicates that buffer usability frictions warrant continued monitoring and policy discussion.

These results have important implications for macropudential policy design in stress. Our analysis on the UK CCyB cut indicates that releasable buffers might be a necessary precondition for practical usability. More concerningly, it may also imply that other non-releasable regulatory buffers are indeed unusable in practice, as suggested by Saporta (2021). The evidence also provides support to suggestions, such as those by Restoy (2021), that a large non-zero buffer built up during good times, which can be easily released during periods of stress, has macroprudential benefits.

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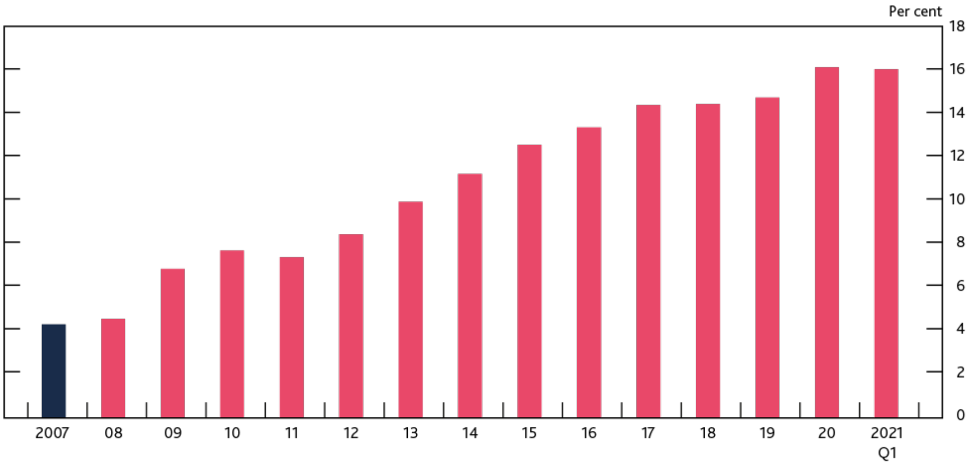
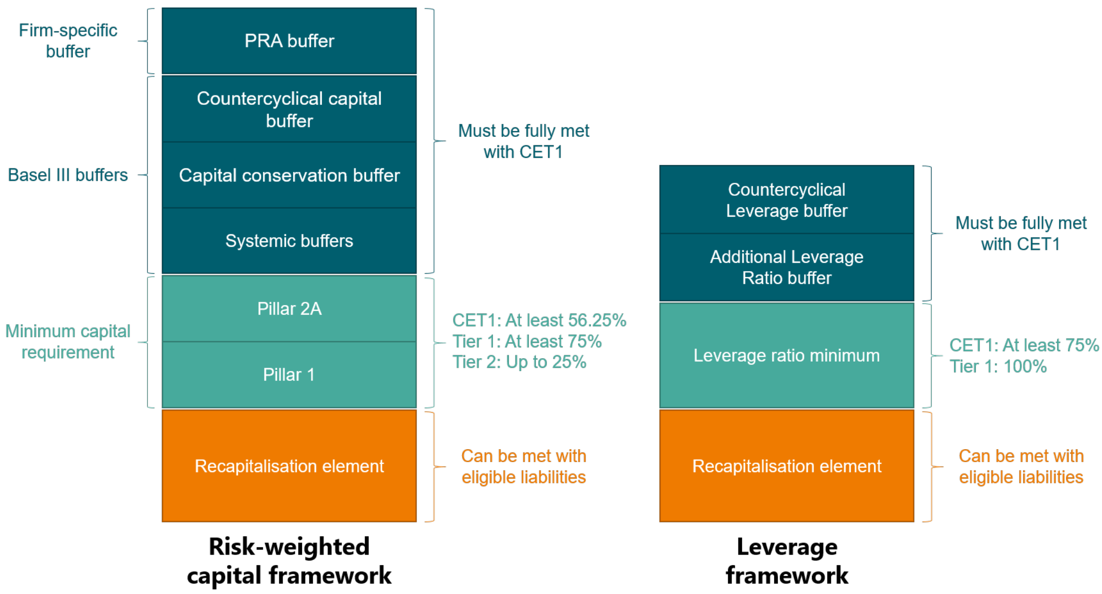
Figures

Figure 1: **Average CET1 capital ratio**



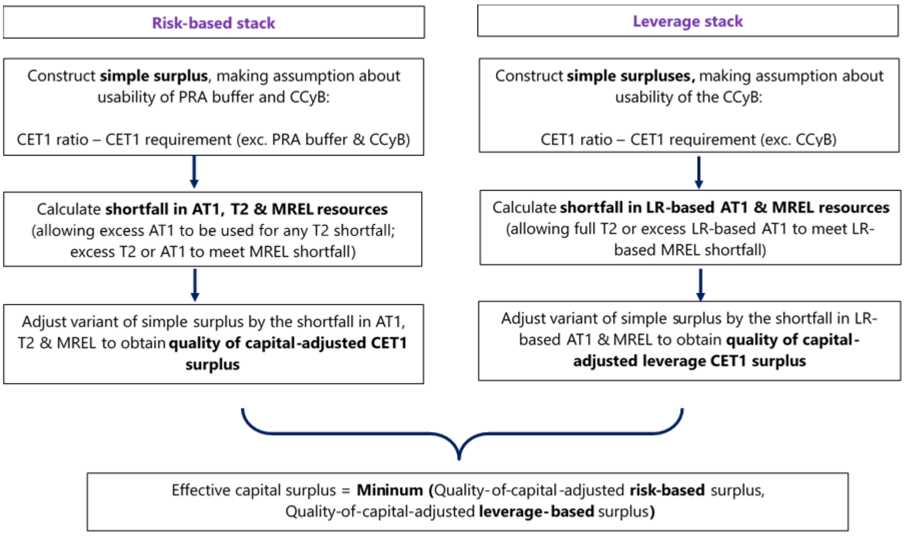
Notes: The CET1 capital ratio is defined as CET1 capital as a percentage of risk-weighted assets. Major UK banks are Barclays, HSBC, Lloyds Banking Group, Nationwide, NatWest Group, Santander, Standard Chartered and, from end-2020, Virgin Money. Prior to 2011, the chart shows Bank of England estimates of banks' CET1 ratios. Capital figures are year-end, except 2021 Q1.

Figure 2: **The UK risk-weighted capital and leverage ratio frameworks**



Notes: The figure shows risk-based and leverage based capital frameworks that apply in the UK as of 2022.

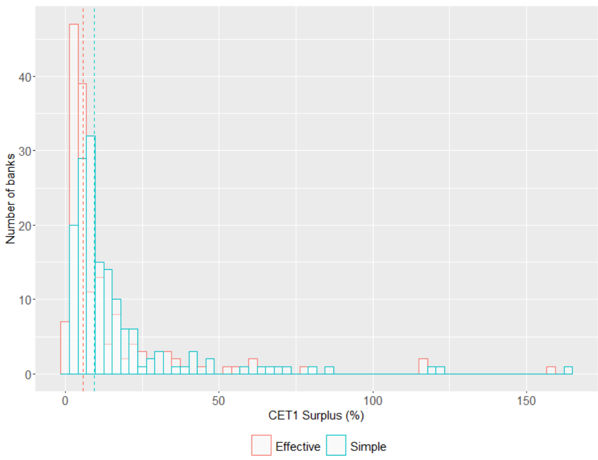
Figure 3: **The effective surplus calculation**



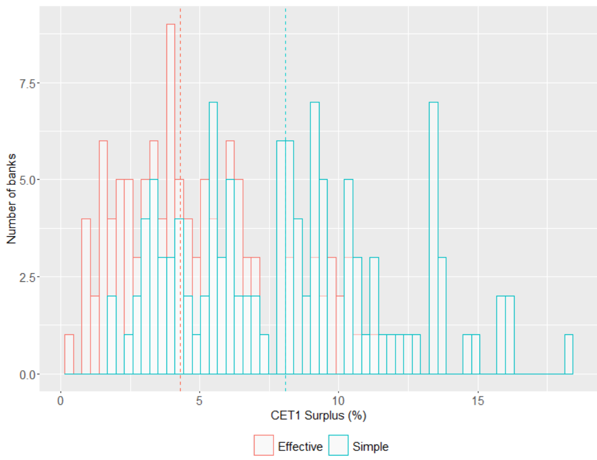
Notes: The flow chart shows the calculations done to convert simple CET1 surpluses to *effective* capital surpluses. See Sections 3 and 4.2 for more information on the institutional framework and the surplus calculations, respectively.

Figure 4: **Histogram of simple and effective CET1 surpluses**

Panel (a): All banks



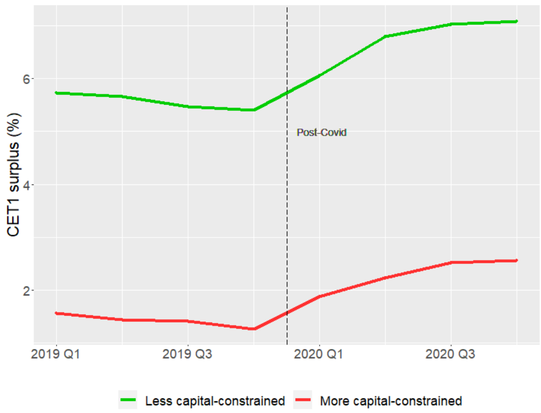
Panel (b): Top quartile dropped



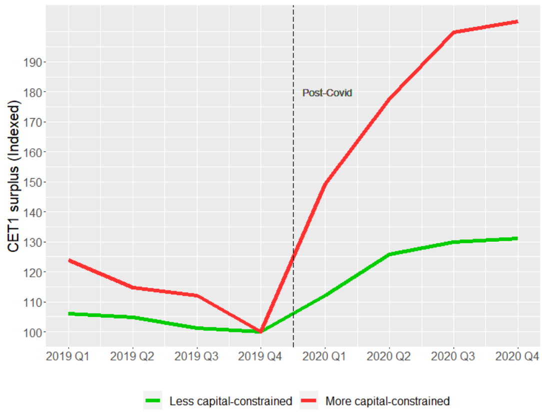
Notes: The figure plots the distribution of simple CET1 surpluses (in blue) and effective CET1 surpluses (in red) based on 2019 averages. The data are sourced from PRA regulatory returns and based on authors' calculations. The full sample in Panel (a) includes 159 banks, while the sample based on dropping the top quartile in Panel (b) has 118 banks. The fourth quartile is excluded from our baseline analysis for comparability.

Figure 5: **Evolution of capital surpluses based on capital-constrainedness**

Panel (a): Raw trends



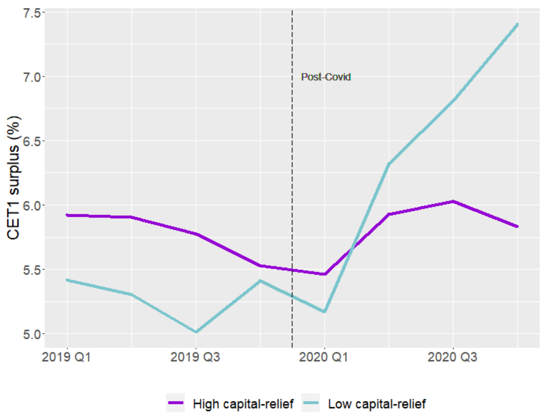
Panel (B): Relative trends



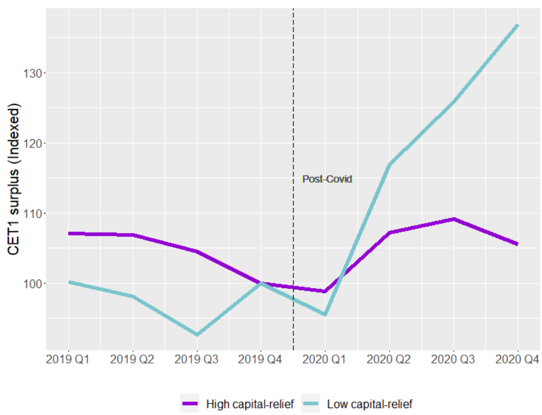
Notes: The figure shows the evolution of capital surpluses for banks with more and less capital-constraints, categorised based on their effective surpluses. Panel (a) shows the evolution of surpluses in levels, as percentage points of risk-weighted assets. Panel (b) shows the evolution of surpluses in relative terms, indexed to 100 at 2019 Q4. *More capital-constrained banks* in red are defined as those with less than 2pp average effective surplus in 2019, while *less capital-constrained* are those above 2pp. We drop the top quartile for comparability purposes. The Post-Covid period covers 2020 Q1-2020 Q4.

Figure 6: **Evolution of capital surplus based on CCyB pass-through rates**

Panel (a): Raw trends



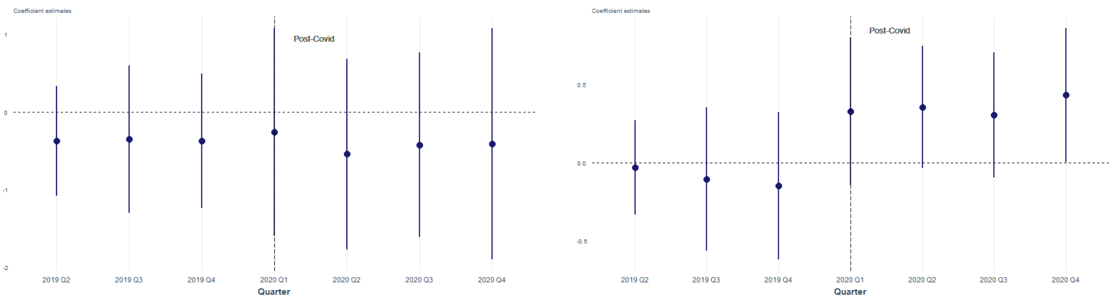
Panel (B): Relative trends



Notes: The figure shows the evolution of capital surpluses for banks with high and low capital-relief, categorised based on their CCyB pass-through rates. Panel (a) shows the evolution of surpluses in levels, as percentage points of risk-weighted assets. Panel (b) shows the evolution of surpluses in relative terms, indexed to 100 at 2019 Q4. *Low capital-relief banks* in blue are defined as those with less than 50% pass-through of the UK countercyclical capital buffer in 2019, while *high capital-relief banks* in purple are those with more than 50%. The Post-Covid period covers 2020 Q1-2020 Q4.

Figure 7: **Capital regressions for capital-constrained banks: Parallel trends**

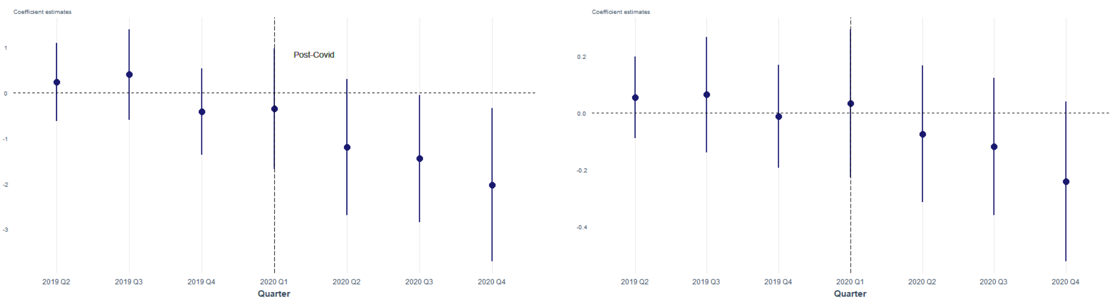
Panel (a): CET1 surplus regression Panel (b): log CET1 surplus regression



Notes: Panels (a) and (b) represent the conditional parallel trends for the CET1 surplus regressions shown in columns (1) and (3) of Table 4 respectively. The figure plots coefficients from a modified version of equations 1 and 2 where we replace the Post-Covid dummy with a full set of time dummies. 2019 Q1 is the base period throughout.

Figure 8: **Capital regressions for capital-relief banks: Parallel trends**

Panel (a): CET1 surplus regression Panel (b): log CET1 surplus regression

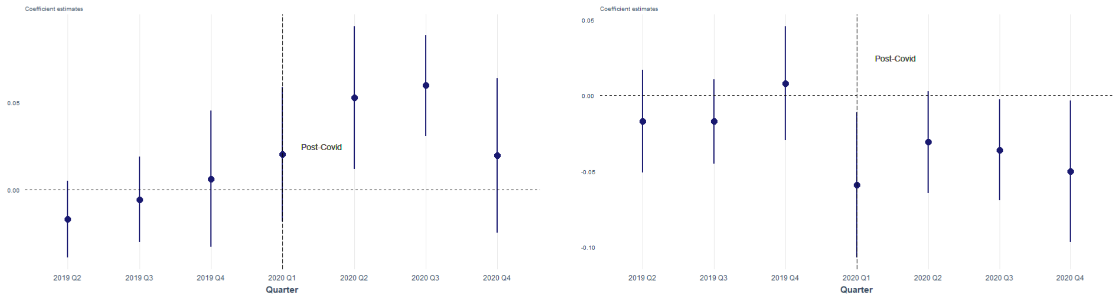


Notes: Panels (a) and (b) represent the conditional parallel trends for the CET1 regressions on releasable buffers shown in columns (2) and (4) of Table 4 respectively. The figure plots coefficients from a modified version of equations 3 and 4 where we replace the Post-Covid dummy with a full set of time dummies. 2019 Q1 is the base period throughout.

Figure 9: **Credit supply regression (log interest rate): Parallel trends**

Panel (a): More capital-constrained

Panel (b): High capital-relief

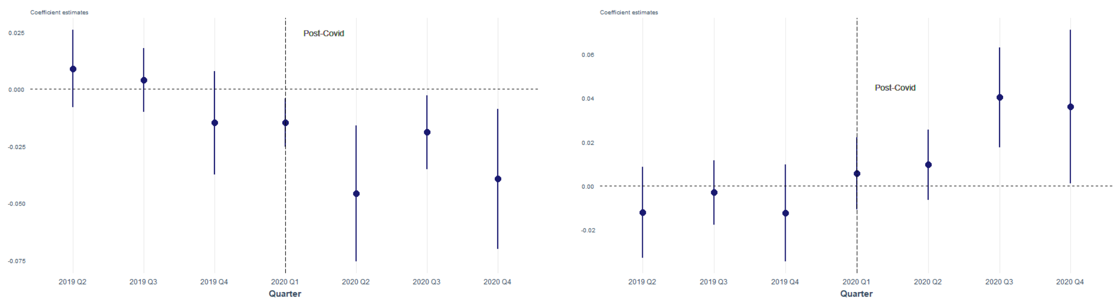


Notes: Panels (a) and (b) represent the conditional parallel trends for the credit supply regression with log interest rates as the dependent variable, using mortgage loan-level data, as shown in column (1) of Table 5. The figure plots coefficients from a modified version of equation 5 where we replace the Post-Covid dummy with a full set of time dummies. 2019 Q1 is the base period throughout.

Figure 10: **Credit supply regression (log loan value): Parallel trends**

Panel (a): More capital-constrained

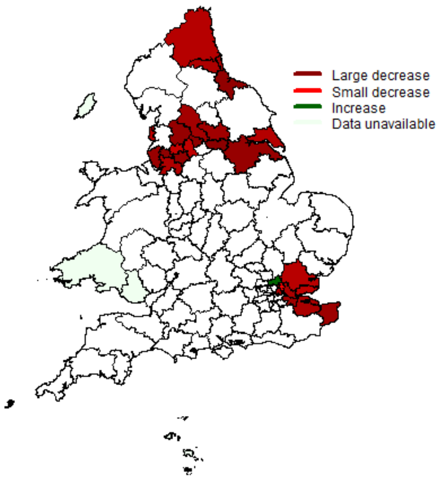
Panel (b): High capital-relief



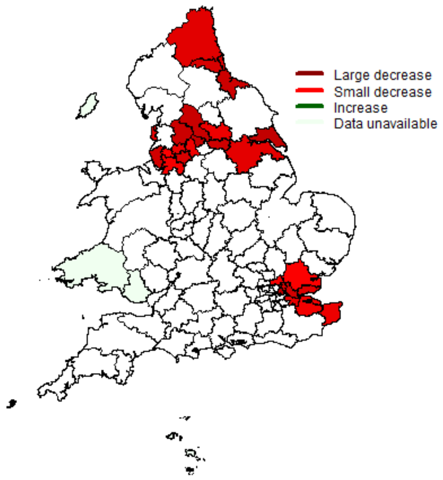
Notes: Panels (a) and (b) represent the conditional parallel trends for the credit supply regression with log interest rates as the dependent variable, using mortgage loan-level data, as shown in column (2) of Table 5. The figure plots coefficients from a modified version of equation 6 where we replace the Post-Covid dummy with a full set of time dummies. 2019 Q1 is the base period throughout.

Figure 11: **Share of mortgages by more and less capital constrained banks and geographic distribution of Covid-19 cases**

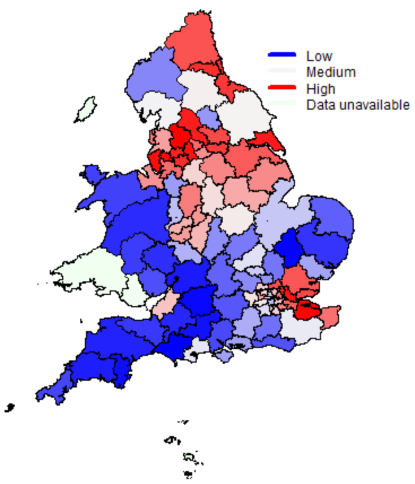
(a) Change in mortgage volume by more capital-constrained banks



(b) Change in mortgage volume by less capital-constrained banks



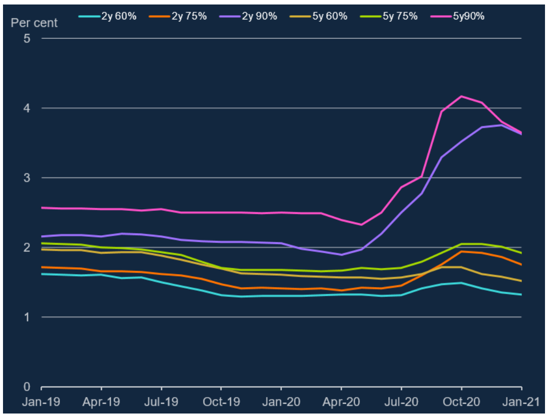
(c) Average Covid-19 case rate in 20203



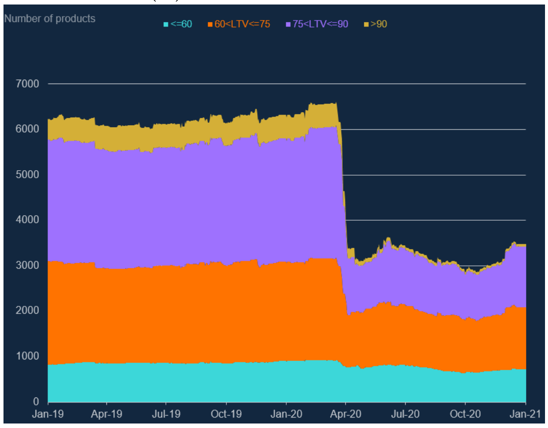
Notes: The maps show the change in share of mortgages in high Covid-19 risk areas for more capital-constrained banks in Panel (a), and for low capital-constrained banks in Panel (b). Change in share of mortgages is calculated for 2020 relative to the 2019 averages, using loan-level data from PSD. High-risk areas are defined as those with average Covid-19 case rates above the 75^{th} percentile in 2020. Areas in white are designated as low-risk based on this cut-off. Panel (c) shows the geographical heterogeneity in average Covid-19 case rates per 100,000 people. Data on Covid-19 case rates is sourced from the government dashboard available at https://coronavirus.data.gov.uk/.

Figure 12: **Trends in offered rates and available mortgage products**

Panel (a): Offered interest rates



Panel (b): Product availabilities



Notes: Panels (a) and (b) represent offered interest rates (in percent) and number of available products by LTV buckets, respectively, between 2019 Q1 and 2020 Q4, which is the sample period of study. The data is sourced from Moneyfacts.

Tables

Table 1: **Descriptive statistics for aggregate dataset**

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
CET1 ratio (% RWAs)	814	17.92	4.94	9.43	14.55	20.32	58.83
CET1 effective requirement (excl. PRA buffer, % RWAs)	810	12.55	2.67	7.81	10.72	13.85	26.51
CET1 simple surplus (excl. PRA buffer, % RWAs)	814	8.64	4.55	1.25	5.50	10.92	42.83
CET1 effective surplus (excl. PRA buffer, % RWAs)	810	5.40	3.56	−1.29	2.93	7.04	32.32
CCyB pass-through rate	814	0.72	0.36	0.00	0.41	1.00	1.00
log Total assets	814	22.08	2.30	17.86	20.33	23.20	28.46
Cash/ assets (%)	814	14.49	11.93	0.00	6.86	18.16	72.65
Deposits/ assets (%)	814	78.54	19.16	2.52	75.47	91.43	96.60
Provisions/ assets (%)	814	0.33	1.69	0.00	0.01	0.21	22.47
Retained profits/ assets (%)	810	3.89	6.35	−27.25	1.82	6.25	64.07
Loans/ assets (%)	814	67.04	20.46	5.07	54.74	82.27	97.39
Mortgage lending/ assets (%)	410	25.29	30.14	0.00	0.001	55.04	84.32

Notes: The table shows the summary statistics for our bank-level panel dataset based on PRA regulatory returns and author calculations, excluding the top quartile of the average 2019 effective surplus distribution. There are two key variables interest. First, the *CET1 effective surplus* used to measure capital constraints and study the impact of the first policy usable regulatory capital buffers in March 2020. Second, the *CCyB pass-through rate* used to identify capital-relief banks and study the impact of the second policy announcement reducing the domestic CCyB rate for all UK banks in March 2020. For more details on how we construct and use each measure to disentangle the effects of the two policies, see Section 4.2.

Table 2: **Descriptive statistics for loan-level dataset**

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Loan-to-income (ratio)	1,734,984	3.200	1.074	0.114	2.447	4.066	14.637
Loan-to-value (pp)	1,734,984	67.354	21.442	2	53.6	85	100
Property value (GBP)	1,734,984	313,593	267,629	19,000	170,000	375,000	26,250,000
Loan value (GBP)	1,734,984	197,262	155,691	4,331	106,800	244,335	15,275,000
Gross income (GBP)	1,734,984	65,445	82,843	1,782	37,000	75,000	28,693,979
Interest rate (pp)	1,734,984	2.068	0.571	0.740	1.690	2.290	19.400
Age (years)	1,734,984	37.818	9.655	18	30	45	85
First-time buyer (dummy)	1,734,984	0.318	0.466	0	0	1	1
Self-employed (dummy)	1,734,984	0.109	0.311	0	0	0	1
Impaired borrower (dummy)	1,734,984	0.002	0.048	0	0	0	1
Income verification (dummy)	1,734,984	1.851	0.357	0	1	1	1
Interest only mortgage (dummy)	1,734,984	0.019	0.135	0	0	0	1

Notes: The table shows the summary statistics for the loan-level mortgage dataset Product Sales Database, after excluding the top quartile of banks (identified on the basis of the aggregate sample to avoid sample composition driving changes in results).

Table 3: **Descriptive statistics for Covid-19 case rate dataset**

Statistic	No. of local areas	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
East Anglia	119	100.616	77.010	19.800	46.172	125.992	660.790
East Midlands	92	127.552	114.141	21.300	46.656	202.529	1,490.795
London	283	154.642	194.285	12.368	42.820	147.776	995.394
North	148	162.283	128.745	15.248	58.657	248.862	1,389.307
North West	235	163.199	141.735	18.100	54.940	230.842	1,038.273
Scotland	5	117.344	60.759	38.300	64.650	143.725	237.300
South East	483	132.173	161.508	12.800	43.153	134.734	1,128.984
South West	260	105.480	82.651	16.900	46.081	146.814	901.207
Wales	15	93.829	55.864	24.800	56.823	134.475	543.175
West Midlands	211	131.203	109.911	16.547	48.074	209.307	591.660
Yorkshire & Humber	230	156.518	135.879	18.688	55.361	223.229	1,120.574
Overall	2081	139.307	145.309	12.368	47.077	182.225	1,490.795

Notes: The table shows the summary statistics for the merged in Covid-19 case dataset between March and December 2020. The column "No. of local areas" refers to the number of local areas or postcodes within each region, as our loan-level regressions use *local area* × *time* and *local area* × *bank* fixed effects. Our analysis uses the number of new cases per 100,000 people calculated over a 7-day rolling period. The data is obtained from the UK's official Covid-19 reporting dashboard available on `https://coronavirus.data.gov.uk/`. The information is available at a very granular Middle Layer Super Output Area (MSOA) level, which are comparable to UK council districts, and have an average of 8288 residents per the 2011 Census (Fetzer, 2022). MSOAs are more smaller than local areas, the latter being the geographical unit of our lending analysis. On average, there are 7 MSOAs within a local area.

Table 4: **Capital behaviour of capital-constrained and capital-relief banks**

	CET1 surplus (%)		CET1 surplus (Logs)	
	(1)	(2)	(3)	(4)
Post-Covid x More capital-constrained bank$_b$	-0.14		0.43^{***}	
	(0.41)		(0.11)	
Post-Covid x High capital-relief bank$_b$		-1.30^{**}		-0.13
		(0.63)		(0.09)
No. of observations	890	891	886	887
R^2 (within)	0.10	0.12	0.08	0.04
Bank controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
No. of groups: Banks	116	116	116	116
No. of groups: Time	8	8	8	8

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank level are reported in brackets.

Notes: The table reports results of the regressions in equations 1 and 3. The dependent variable in columns (1) and (3) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and its log respectively. The dependent variable in columns (2) and (4) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and the CCyB and its log respectively. *Post-Covid*$_t$ is a dummy variable that takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. *More capital-constrained bank*$_b$ is a dummy variable that takes the value 1 if the bank has an average 2019 effective surplus of less than 2pp, and *High capital-relief bank*$_b$ is a dummy variable that takes the value 1 if the bank has an average 2019 CCyB pass-through rate of more than 50%. Bank controls included are loans to assets, cash to assets, retained earnings to assets, deposits to assets, all lagged by one quarter; contemporaneous capital requirement (appropriately calculated); and Post-Covid$_t$ interacted with a dummy for whether the bank was subject to distribution restrictions in March 2020.

Table 5: **Lending behaviour of capital-constrained and capital-relief banks**

	Interest rate (logs) (1)	Loan value (logs) (2)	Interest rate (logs) (3)	Loan value (logs) (4)
Post-Covid$_t$ x More capital-constrained$_b$	0.037***	−0.022***	0.143*	−0.214***
	(0.009)	(0.004)	(0.075)	(0.030)
Post-Covid$_t$ x High capital-relief$_b$	−0.037***	0.023***		
	(0.013)	(0.005)		
Post-Covid$_t$ x CCyB pass-through (2019)$_{b,t}$			−0.000	0.000***
			(0.000)	(0.000)
Post-Covid$_t$ x More capital-constrained$_b$ x CCyB pass-through (2019)$_{b,t}$			−0.002	0.004***
			(0.001)	(0.001)
Pass-through variable	Dummy	Dummy	Continuous	Continuous
No. of observations	1602650	1602650	1602650	1602650
R^2 (within)	0.207	0.469	0.207	0.469
Bank controls	Yes	Yes	Yes	Yes
Borrower risk controls	Yes	Yes	Yes	Yes
Bank × Postcode FE	Yes	Yes	Yes	Yes
Postcode × Time FE	Yes	Yes	Yes	Yes
No. of groups: Bank-Postcode	38916	38916	38916	38916
No. of groups: Postcode-Time	21570	21570	21570	21570

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode level are reported in brackets.

Notes: The table reports results of the regressions in equations 5 and 6 using loan-level data in columns (1) and (2). In columns (3) and (4) we additionally include a triple interaction of Post-Covid$_t$ × More capital-constrained$_b$ × CCyB pass-through (2019)$_b$. The dependent variable in columns (1) and (3) is the interest rate (in pp), and in columns (2) and (4) is the log loan value in GBP. *Post-Covid$_t$* is a dummy variable that takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. *More capital-constrained bank$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 effective surplus of less than 2pp, and *High capital-relief$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 CCyB pass-through rate of more than 50%, and CCyB pass-through (2019)$_{b,t}$ is the continuous pass-through rate. Bank controls included are cash to assets, retained earnings to assets, deposits to assets, log size, density ratio, mortgage loans to assets, all lagged by one quarter, plus two interactions of the Post-Covid dummy with: average total capital in 2019 and a dummy for whether the bank was subject to distribution restrictions in March 2020.

Table 6: **Risk-taking behaviour: Using Covid-19 specific shock to borrower risk**

	Interest rate (logs)	Loan value (logs)
	(1)	(2)
Post-Covid$_t$ x More capital-constrained$_b$ x High-risk area$_l$	0.002	-0.014^{***}
	(0.002)	(0.003)
Post-Covid$_t$ x High capital-relief$_b$ x High-risk area$_l$	-0.015^{***}	0.006^{*}
	(0.002)	(0.003)
No. of observations	1368512	1368512
R^2 (within)	0.190	0.480
Borrower risk controls	Yes	Yes
Bank $\times$ Postcode FE	Yes	Yes
Bank $\times$ Time FE	Yes	Yes
Postcode $\times$ Time FE	Yes	Yes
No. of groups: Bank-Postcode	32075	32075
No. of groups: Bank-Time	186	186
No. of groups: Postcode-Time	16498	16498

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table reports results of the regressions in equations 9 and 10 using loan-level data. The dependent variable in column (1) is the log interest rate (in pp), and in column (2) is the log loan value (in GBP). *Post-Covid$_t$* is a dummy variable that takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. *More capital-constrained bank$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 effective surplus of less than 2pp, and *High capital-relief bank$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 CCyB pass-through rate of more than 50%. *High-risk area$_l$* is a dummy that takes value 1 when the local area l has an average Covid-19 case rate between March and December 2020 above the *75th* percentile. Borrower risk controls interacted with Post-Covid$_t$ are included as in the baseline. We drop bank balance sheet controls due to the inclusion of bank $\times$ time fixed effects.

Table 7: **Risk-taking behaviour: Using conventional measures of borrower risk**

	Interest rate (logs)		Loan value (logs)	
	(1)	(2)	(3)	(4)
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$	0.071^{**}	0.117^{***}	-0.020	-0.062^{***}
	(0.030)	(0.021)	(0.021)	(0.013)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$	-0.087^{***}	-0.092^{***}	0.086^{***}	0.051^{***}
	(0.029)	(0.014)	(0.018)	(0.012)
No. of observations	1272317	319075	1272317	319075
R^2 (within)	0.121	0.083	0.552	0.672
Borrower type	All	First-time buyers	All	First-time buyers
Risky loan	LTV $\geq$ 90% & LTI $\geq$ 4.5			
Borrower risk controls	Yes	Yes	Yes	Yes
Bank × Postcode FE	Yes	Yes	Yes	Yes
Bank × Time FE	Yes	Yes	Yes	Yes
Postcode × Time FE	Yes	Yes	Yes	Yes
No. of groups: Bank-Postcode	46367	27281	46367	27281
No. of groups: Bank-Time	414	348	414	348
No. of groups: Postcode-Time	21479	19981	21479	19981

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Robust standard errors clustered at bank and postcode level are reported in brackets.

Notes: The table reports results of the risk-taking regressions using conventional, i.e. LTV and LTI-based, measures of risky loans. This is done for all borrowers in columns (1) and (3), and only first-time buyers in columns (2) and (4). The dependent variable in columns (1) and (2) is the log interest rate (in pp), and in columns (3) and (4) is the log loan value. *Post-Covid$_t$* is a dummy variable that takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. *More capital-constrained bank$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 effective surplus of less than 2pp, and *High capital-relief bank$_b$* is a dummy variable that takes the value 1 if the bank has an average 2019 CCyB pass-through rate of more than 50%. *Risky loan$_i$* is a dummy that takes value 1 when loan has an LTV $\geq$ 90% and an LTI $\geq$ 4.5. Borrower risk controls interacted with Post-Covid$_t$ are included; see Section 5 for more details. We drop bank balance sheet controls due to the inclusion of bank × time fixed effects.

Table 8: **Extensive margin: Number and volume of loans**

	Log number of loans	Log volume of loans
	(1)	(2)
Post-Covid$_t$ x More capital-constrained$_b$	0.258	0.323
	(0.329)	(0.372)
Post-Covid$_t$ x High capital-relief$_b$	-0.099	-0.102
	(0.219)	(0.188)
No. of observations	1871	1871
R^2 (within)	0.159	0.234
Averaged borrower controls	Yes	Yes
Bank controls	Yes	Yes
Bank×Region FE	Yes	Yes
Time×Region FE	Yes	Yes
No. of groups: Bank-Region	267	267
No. of groups: Region-Time	96	96

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: The table reports results of the regressions in equation 7 with the *log* number of loans as the dependent variable in column (1) and 8 with *log* volume of loans as the dependent variable in column (2). For this analysis, we aggregate loan-level data up to the bank-region-quarter level, which allows for the inclusion of region × quarter and bank × region fixed effects. Borrower and loan level controls that are included in the baseline regressions are also averaged and interacted with the Post-Covid$_t$ dummy here, for consistency with the baseline. Bank controls are also included, specifically cash to assets, deposits to assets, retained earnings to assets, size, density ratio, share of household lending to assets, plus two interactions of the Post-Covid$_t$ dummy with: the average total capital in 2019 and a dummy for whether the bank was subject to distribution restrictions in March 2020.

Table 9: **Extensive margin: Probability of issuing a risky loan**

	Probability of issuing risky loan		
	(1)	(2)	(3)
Post-Covid$_t$ x More capital-constrained$_b$	0.001	0.001	0.002
	(0.001)	(0.001)	(0.002)
Post-Covid$_t$ x High capital-relief$_b$	0.009***	0.010***	0.008**
	(0.002)	(0.002)	(0.004)
Risky loan comparison group			
LTI $\geq$ 4.5 & LTV $\geq$ 90 vs LTI $<$ 4.5 & LTV $<$	90	75	60
No. of observations	1250693	777043	478904
R^2 (within)	0.025	0.074	0.157
Bank controls	Yes	Yes	Yes
Borrower risk controls	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes
Bank×Time FE	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes
No. of groups: Bank-Region	36626	33334	30112
No. of groups: Region-Time	21467	21188	20787

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: The table reports results of the regressions in equation 11 using loan-level data. Column (1) relies on the baseline definition of risky loan, and in columns (2)-(3), we present robustness checks with alternate "comparison" groups, indicated by the "risky loan comparison group" row: LTI $<$ 4.5 & LTV $<$ 75% (column 2) or LTI $<$4.5 & LTV $<$ 60% (column 3). Borrower and loan level controls that are included in the baseline regressions are included here as interactions with the $Post - Covid_t$ dummy. Bank controls are also included, specifically cash to assets, deposits to assets, retained earnings to assets, size, density ratio, share of household lending to assets, plus two interactions of the Post-Covid$_t$ dummy with the average total capital in 2019 and a dummy for whether the bank was subject to distribution restrictions in March 2020.

Appendices

A Robustness tables

Table 10: **Capital results with alternate definitions of more capital-constrained and high capital-relief banks**

	CET1 surplus (%)		CET1 surplus (logs)	
	(1)	(2)	(3)	(4)
Post-Covid$_t$ x More capital-constrained (alt. dummy)	0.20		0.34***	
	(0.41)		(0.09)	
Post-Covid$_t$ x Capital-relief (cont., 2019)		-1.40^{**}		-0.16
		(0.67)		(0.10)
No. of observations	890	891	886	887
R^2 (within)	0.09	0.11	0.08	0.04
Bank FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank level are reported in brackets.

Notes: The table is a robustness check on Table 4. It replaces our baseline definition of *More capital-constrained* banks (based on the 2% threshold) with an alternate based on the bottom quartile of the cross-sectional 2019 capital surplus distribution in columns (1) and (3). The baseline dummy definition of the *High capital-relief* bank (based on the 50% threshold) is replaced with its continuous version in columns (2) and (4). The dependent variable in columns (1) & (3) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and its log respectively. The dependent variable in columns (2) & (4) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and the CCyB and its log respectively. Other bank controls are also included, specifically cash to assets, deposits to assets, retained earnings to assets, loans to assets; contemporaneous capital requirement (appropriately calculated), plus two interactions of the Post-Covid$_t$ dummy with the average total capital in 2019 and a dummy for whether the bank was subject to distribution restrictions in March 2020.

Table 11: **Capital results with additional balance sheet variables and alternate channels controlled for**

	CET1 surplus (%)				CET1 surplus (Logs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Covid$_t$ x More capital-constrained$_b$	0.04	-0.21			0.43***	0.38***		
	(0.36)	(0.56)			(0.10)	(0.14)		
Post-Covid$_t$ x High capital-relief$_b$			-1.11^{*}	-1.57^{**}			-0.11	-0.18^{*}
			(0.60)	(0.71)			(0.08)	(0.10)
No. of observations	803	698	804	699	799	694	800	695
R^2 (within)	0.17	0.14	0.19	0.17	0.12	0.08	0.07	0.06
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alt. channel controlled for	Assets	Density ratio	Assets	Density ratio	Assets	Density ratio	Assets	Density ratio

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: The table is a robustness check on Table 4. It controls for two alternate channels that can affect banks' capital ratios: either size (*log total assets*) in columns (1), (3), (5) and (7); or density ratio (*risk-weighted assets by total assets*) in columns (2), (4), (6), and (8). In addition to the controls included in Table 10, these columns also include the provisions to assets ratio. The dependent variable in columns (1)-(2) and (5)-(6) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and its log respectively. The dependent variable in columns (3)-(4) and (7)-(8) is the effective CET1 surplus (in pp) calculated after excluding the PRA buffer and the CCyB and its log respectively.

Table 12: **Credit supply results with alternate definitions of more capital-constrained and high capital-relief banks**

	Interest rate (logs)			Loan value (logs)		
Alt. definitions:	Baseline	Bottom quartile	Continuous	Baseline	Bottom quartile	Continuous
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Covid$_t$ x Capital-constrained	0.037***	0.036**	-0.042^{***}	-0.022^{***}	-0.010	0.014***
	(0.009)	(0.013)	(0.008)	(0.004)	(0.007)	(0.003)
Post-Covid$_t$ x Capital-relief	-0.037^{***}	-0.014	-0.000	0.023***	0.014*	0.000***
	(0.013)	(0.014)	(0.000)	(0.005)	(0.008)	(0.000)
No. of observations	1602650	1602650	1602650	1602650	1602650	1602650
R^2 (within)	0.207	0.207	0.207	0.469	0.469	0.469
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Postcode × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Bank-Postcode	38916	38916	38916	38916	38916	38916
No. of groups: Postcode-Time	21570	21570	21570	21570	21570	21570

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on Table 5. The baseline results are replicated in columns (1) and (4). In columns (2) and (5), we replace the baseline definition of *More capital-constrained* banks (based on the 2% threshold) with an alternate based on the bottom quartile of the cross-sectional 2019 capital surplus distribution. In columns (3) and (6), we replace the baseline definition of the *High capital-relief* bank (based on the 50% threshold) with its continuous version. The rest of the specification is the same as in Table 5 with the inclusion of various borrower, loan, and bank controls. The results are qualitatively in line with those reported before, although the statistical significance changes.

Table 13: **Results for Covid-19 specifc measure of borrower risk with alternate definition of *High-risk area***

	Interest rate (logs)	Loan value (logs)
	(1)	(2)
Post-Covid$_t$ x More capital-constrained$_b$ x High-risk area (median)$_l$	-0.001	-0.005^{*}
	(0.002)	(0.003)
Post-Covid$_t$ x High capital-relief$_b$ x High-risk area (median)$_l$	-0.016^{***}	0.006**
	(0.002)	(0.003)
No. of observations	1368512	1368512
R^2 (within)	0.190	0.480
Borrower risk controls	Yes	Yes
Bank × Postcode FE	Yes	Yes
Bank × Time FE	Yes	Yes
Postcode × Time FE	Yes	Yes
No. of groups: Bank-Postcode	32075	32075
No. of groups: Bank-Time	186	186
No. of groups: Postcode-Time	16498	16498

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on Table 6 using an alternate definition of high case rate. The dependent variable in columns (1) is the log interest rate (in pp), and in columns (2) is the log loan value (in GBP). *High-risk area* is now a dummy that takes value 1 when the local area l has an average Covid-19 case rate between March and December 2020 above the median. Borrower risk controls interacted with Post-Covid$_t$ are included as in the baseline. We drop bank balance sheet controls due to the inclusion of bank × time fixed effects.

Table 14: **Results for conventional measures of borrower risk with alternate definitions of *Risky loan* for all borrower types**

	Interest rate (logs)			Loan value (logs)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$	0.071**	0.093**	0.077**	−0.020	−0.005	−0.053
	(0.030)	(0.038)	(0.033)	(0.021)	(0.023)	(0.033)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$	−0.087***	−0.103***	−0.102***	0.086***	0.084***	0.054***
	(0.029)	(0.030)	(0.032)	(0.018)	(0.015)	(0.011)
Risky loan comparison group						
LTI $\geq$ 4.5 & LTV $\geq$ 90 vs LTI $<$ 4.5 & LTV $<$	90	75	60	90	75	60
No. of observations	1272317	790900	488336	1272317	790900	488336
R^2 (within)	0.121	0.115	0.131	0.552	0.530	0.519
Borrower type	All	All	All	All	All	All
Borrower risk controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Postcode-Bank	46367	40476	35479	46367	40476	35479
No. of groups: Bank-Time	414	411	405	414	411	405
No. of groups: Postcode-Time	21479	21202	20805	21479	21202	20805

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on Table 7 for all borrower types. The baseline results – with "risky loan" being a dummy that takes value 1 for loans with LTV $\geq$ 90% and LTI $\geq$ 4.5, and value 0 for loans with LTV $\leq$ 90% and LTI $\leq$ 4.5 – are replicated in columns (1) and (4) for interest rates and loan values respectively. In column (2)-(3) for rates and (5)-(6) for loan values, we present robustness checks with alternate "comparison" groups, indicated by the "risky loan comparison group" row: LTI $<$ 4.5 & LTV $<$ 75% (columns 2 and 5) or LTI $<$4.5 & LTV $<$ 60% (columns 3 and 6). The rest of the specification is the same as in Table 7 with the inclusion of various borrower and loan controls. We drop bank balance sheet controls due to the inclusion of bank × time fixed effects.

Table 15: **Results for conventional measures of borrower risk with alternate definitions of *Risky loan* for sub-sample of first time buyers**

	Interest rate (logs)			Loan value (logs)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$	0.117^{***}	0.108^{***}	0.087^{***}	-0.062^{***}	-0.089^{***}	-0.046^{**}
	(0.021)	(0.012)	(0.012)	(0.013)	(0.030)	(0.022)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$	-0.092^{***}	-0.136^{***}	-0.138^{***}	0.051^{***}	0.096^{***}	0.083^{***}
	(0.014)	(0.013)	(0.012)	(0.012)	(0.020)	(0.016)
Risky loan comparison group:						
LTI > 4.5 & LTV > 90 vs LTI < 4.5 & LTV <	90	75	60	90	75	60
No. of observations	319075	137035	73613	319075	137035	73613
R^2 (within)	0.083	0.085	0.085	0.672	0.623	0.620
Borrower type	FTB	FTB	FTB	FTB	FTB	FTB
Borrower risk controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Postcode-Bank	27281	20025	15687	27281	20025	15687
No. of groups: Bank-Time	348	326	298	348	326	298
No. of groups: Postcode-Time	19981	18175	15825	19981	18175	15825

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on Table 7 for the sub-sample of *first-time borrowers*, indicated by the "borrower type" row. The main sub-sample results – with "risky loan" being a dummy that takes value 1 for loans with LTV $\geq$ 90% and LTI $\geq$ 4.5, and value 0 for loans with LTV $\leq$ 90% and LTI $\leq$ 4.5 – are replicated in columns (1) and (4) for interest rates and loan values respectively. In column (2)-(3) for rates and (5)-(6) for loan values, we present robustness checks with alternate "comparison" groups, indicated by the "risky loan comparison group" row: LTI < 4.5 & LTV < 75% (columns 2 and 5) or LTI <4.5 & LTV < 60% (columns 3 and 6). The rest of the specification is the same as in Table 7 with the inclusion of various borrower and loan controls. We drop bank balance sheet controls due to the inclusion of bank × time fixed effects.

Table 16: **Results for conventional measures of borrower risk with alternate definition of *Risky loan***

	Interest rate (logs)		Loan value (logs)	
	(1)	(2)	(3)	(4)
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan (alt.)$_i$	0.029	0.037***	−0.004	−0.009
	(0.018)	(0.011)	(0.020)	(0.014)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan (alt.)$_i$	−0.059***	−0.094***	0.059**	0.047***
	(0.017)	(0.006)	(0.022)	(0.015)
Risky loan comparison group	LTI $\geq$ 4.5 & LTV $\geq$ 85 vs LTI < 4.5 & LTV < 85%			
Borrower type	All	FTB	All	FTB
Borrower risk controls	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes
Bank×Time FE	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes
No. of observations	1114156	248059	1114156	248059
R^2 (within)	0.118	0.091	0.552	0.670
No. of groups: Postcode-Bank	44725	24599	44725	24599
No. of groups: Bank-Time	414	341	414	341
No. of groups: Postcode-Time	21418	19546	21418	19546

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on Table 7 for all borrower types. "Risky loan (alt)" is now defined as a dummy variable that takes value 1 for loans with LTV $\geq$ 85% and LTI $\geq$ 4.5, and value 0 for loans with LTV $\leq$ 85% and LTI $\leq$ 4.5 for interest rates in columns (1) and (2) and loan values in columns (3) and (4). Columns (1) and (3) are results based on the entire sample, and columns (2) and (4) are based on first-time-buyers only. The rest of the specification is the same as in Table 7 with the inclusion of various borrower and loan controls. We drop bank balance sheet controls due to the inclusion of bank × time fixed effects.

Table 17: **Main loan-level results with top quartile of surplus distribution included**

	Interest rate (logs) (1)	Loan value (logs) (2)	Interest rate (logs) (3)	Loan value (logs) (4)	Interest rate (logs) (5)	Loan value (logs) (6)
Post-Covid$_t$ x More capital-constrained$_b$	0.038^{***}	-0.022^{***}				
	(0.011)	(0.005)				
Post-Covid$_t$ x High capital-relief$_b$	-0.032^{**}	0.024^{***}				
	(0.014)	(0.005)				
Post-Covid$_t$ x More capital-constrained$_b$ x High-risk area$_l$			0.000	-0.013^{***}		
			(0.002)	(0.003)		
Post-Covid$_t$ x High capital-relief$_b$ x High-risk area$_l$			-0.015^{***}	0.006^{**}		
			(0.002)	(0.003)		
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$					0.068^{**}	-0.020
					(0.027)	(0.019)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$					-0.087^{***}	0.086^{***}
					(0.027)	(0.017)
No. of observations	1704265	1704265	1477188	1477188	1354490	1354490
R^2 (within)	0.203	0.472	0.188	0.484	0.121	0.549
Borrower risk controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Time FE	No	No	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Postcode-Bank	46276	46276	49267	49267	53870	53870
No. of groups: Bank-Time	0	0	443	443	467	467
No. of groups: Postcode-Time	21599	21599	16504	16504	21511	21511

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on the three main loan-level results with the top quartile of the surplus distribution included in the sample - this increases the number of observations. The remaining setup of the specifications is the same as before. Columns (1)-(2) are counterparts to Table 5, columns (3)-(4) are counterparts to Table 6, and columns (5)-(6) are counterparts to Table 7 for the entire sample. Wherever bank × time fixed effects are included, we drop bank balance sheet controls.

Table 18: **Main loan-level results with 2020 Q1 dropped**

	Interest rate (logs) (1)	Loan value (logs) (2)	Interest rate (logs) (3)	Loan value (logs) (4)	Interest rate (logs) (5)	Loan value (logs) (6)
Post-Covid$_t$ x More capital-constrained$_b$	0.055^{***}	-0.027^{***}				
	(0.007)	(0.008)				
Post-Covid$_t$ x High capital-relief$_b$	-0.042^{***}	0.030^{***}				
	(0.012)	(0.006)				
Post-Covid$_t$ x More capital-constrained$_b$ x High-risk area$_l$			0.002	-0.020^{***}		
			(0.002)	(0.004)		
Post-Covid$_t$ x High capital-relief$_b$ x High-risk area$_l$			-0.017^{***}	0.008^{**}		
			(0.002)	(0.004)		
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$					0.075^{*}	-0.018
					(0.040)	(0.029)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$					-0.105^{**}	0.081^{***}
					(0.040)	(0.025)
No. of observations	1412651	1412651	1222121	1222121	1124261	1124261
R^2 (within)	0.207	0.467	0.190	0.478	0.122	0.555
Borrower risk controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Time FE	No	No	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Postcode-Bank	37943	37943	41368	41368	44876	44876
No. of groups: Bank-Time	0	0	339	339	360	360
No. of groups: Postcode-Time	18877	18877	14434	14434	18796	18796

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on the three main loan-level results with 2020 Q1 data dropped - this reduces the number of observations. The remaining setup of the specifications is the same as before. Columns (1)-(2) are counterparts to Table 5, columns (3)-(4) are counterparts to Table 6, and columns (5)-(6) are counterparts to Table 7 for the entire sample. *Post-Covid$_t$* is a dummy variable that takes value 1 after 2020 Q2, and 0 otherwise; data for 2020 Q1 is dropped. Wherever bank × time fixed effects are included, we drop bank balance sheet controls.

Table 19: **Main loan-level results with expanded set of borrower, loan, and bank controls**

	Interest rate (logs) (1)	Loan value (logs) (2)	Interest rate (logs) (3)	Loan value (logs) (4)	Interest rate (logs) (5)	Loan value (logs) (6)
Post-Covid$_t$ x More capital-constrained$_b$	0.008^{**}	-0.025^{***}				
	(0.004)	(0.003)				
Post-Covid$_t$ x High capital-relief$_b$	-0.031^{***}	0.016^{***}				
	(0.006)	(0.004)				
Post-Covid$_t$ x More capital-constrained$_b$ x Risky loan$_i$					0.082^{***}	-0.043
					(0.021)	(0.027)
Post-Covid$_t$ x High capital-relief$_b$ x Risky loan$_i$					-0.075^{**}	0.081^{***}
					(0.029)	(0.015)
Post-Covid$_t$ x More capital-constrained$_b$ x High-risk area$_l$			-0.001	-0.014^{***}		
			(0.003)	(0.003)		
Post-Covid$_t$ x High capital-relief$_b$ x High-risk area$_l$			-0.017^{***}	0.004		
			(0.002)	(0.003)		
No. of observations	1602650	1602650	1387719	1387719	1272317	1272317
R^2 (within)	0.286	0.482	0.274	0.493	0.233	0.613
Borrower risk controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Postcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank×Time FE	No	No	Yes	Yes	Yes	Yes
Postcode×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups: Postcode-Bank	38916	38916	42872	42872	46367	46367
No. of groups: Bank-Time	0	0	389	389	414	414
No. of groups: Postcode-Time	21570	21570	16498	16498	21479	21479

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$ Robust standard errors clustered at bank and postcode are reported in brackets.

Notes: The table is a robustness check on the three main loan-level results with an expanded set of borrower, loan, and bank controls. Specifically, we include interactions of Post-Covid$_t$ with a dummy for the type of fixed term of the mortgage, and with age buckets, as well as the ratio of provisions to assets. Columns (1)-(2) are counterparts to Table 5, columns (3)-(4) are counterparts to Table 6, and columns (5)-(6) are counterparts to Table 7 for the entire sample. *Post-Covid* is a dummy variable that takes value 1 between 2020 Q1 and 2020 Q4, and 0 between 2019 Q1 and 2019 Q4. The remaining setup of the specifications is the same as before. Wherever bank × time fixed effects are included, we drop bank balance sheet controls.