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Side effects of separating retail and investment banking: evidence from the UK

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Abstract

The idea of separating retail and investment banking remains controversial. Exploiting the introduction of UK ring-fencing requirements, we show that this separation has a range of previously undocumented side effects for credit supply, competition, and risk-taking in credit markets not directly targeted by the reform. By redirecting the benefits of deposit funding towards retail activities, ring-fencing incentivises universal banks to expand mortgage lending. This rebalancing reduces the cost of household credit, without eroding lending standards. But it also increases mortgage market concentration, pushes smaller banks towards riskier lending, and is mirrored by a reduction in syndicated loans and credit lines.

Key words: Bank regulation, universal banking, mortgages, syndicated lending, competition.

JEL classification: G21, G24, G28.

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Whether "core" banking services to households and SMEs should be separated from "non-core" investment banking activities has been debated since at least the Glass-Steagall Act of 1933. But global consensus remains elusive. European and Canadian banking groups can operate as integrated "universal" banks, whereas US and Japanese banks must split their retail and investment banking activities into separate subsidiaries. In recent years, prominent US regulators and politicians—including Joe Biden—have renewed calls for stronger "structural separation", while others such as Janet Yellen (2017) have expressed scepticism. Following recent turmoil in the banking sector, there have been similar calls by parliamentarians in Switzerland and Italy.[1]

The main focus of recent structural separation proposals is to require universal banks to conduct retail deposit-taking and certain investment banking activities in separate entities.[2] To their supporters, such reforms would protect retail customers and taxpayers from investment banking risks *in crisis times*. However, this separation also has the potential to fundamentally change the funding structure of a wide range of universal banking activities *in normal times*. In this paper, we show that, as a result, separation involves a range of previously undocumented side effects for credit supply, competition, and risk-taking in important credit markets—even where those markets are not directly targeted by the reform.

These side effects have one common cause. After separation, any activity in the non-retail entity loses access to retail deposit funding. These deposits instead become available for activities placed in the retail entity—such as retail lending. To the extent that retail deposits benefit from a safety or liquidity premium (for example due to deposit insurance or household preferences for liquidity), this increase in deposit funding increases the competitiveness of affected banks in retail lending markets. This causes affected banks

[1]The 2016 Republican and Democratic presidential platforms both included proposals to impose stricter separation between commercial and investment banking, as did the 2020 Democratic platform. In 2017, Senators Elizabeth Warren and John McCain submitted a "21st Century Glass-Steagall" bill, and FDIC Vice Chairman Thomas Hoenig (2017) proposed that core and non-core activities should be "partitioned" into separate subsidiaries. For Swiss and Italian developments, see Financial Times (2023) and Reuters (2023).

[2]See ICB (2011), Liikanen (2012), and Hoenig (2017) for UK, EU, and US proposals.

to rebalance towards retail lending, with potential knock-on effects for market structure and risk-taking by other banks in this market. Meanwhile, if banks wish to maintain established synergies between corporate lending and investment banking activities such as underwriting (Drucker and Puri, 2005; Neuhann and Saidi, 2018), then they must place corporate lending in the non-retail entity, which implies a loss of deposit funding. This reduces affected banks' competitiveness in corporate lending markets, leading them to retrench from these markets.

To explore these side effects, we exploit the recent introduction of "ring-fencing" requirements in the UK. We show that when universal banks are required to separate, they respond by rebalancing towards retail mortgage lending. This increases household credit supply—without eroding lending standards—but also increases the concentration of mortgage lending in the hands of large universal banks. This erodes the market share of smaller banks, and pushes them to rebalance towards higher-yielding but riskier mortgage lending. Meanwhile, the affected universal banks cut back on providing syndicated loans and credit lines to large corporates, particularly to foreign borrowers.

Taken together, our results suggest that assessing the merits of structural separation requires weighing its intended financial stability benefits in crisis times with its side effects for credit markets in normal times. The shift by affected banks towards mortgage lending reduces the cost of credit for households. But the increased mortgage market concentration suggests that separation could reduce competition in this market over the longer term. It also casts doubt on the idea that separation reduces the systemic importance of the largest banks (Warren, 2017), or their political clout (Zingales, 2012). Meanwhile, the increased risk-taking by small banks suggests that separation does not unambiguously improve financial stability across the banking system. These mixed conclusions resonate with an ongoing policy controversy in the UK,[3] with affected banks arguing that ring-fencing has harmed their competitiveness in global syndicated credit markets (Reuters, 2017), while other commentators, including regulators and smaller banks, argue

[3]See Ring-fencing and Proprietary Trading Independent Review (2022).

that ring-fencing has contributed to a "price war" in the UK mortgage market (Bank of England, 2019a,b; Financial Times, 2019a; Building Societies Association, 2021), resulting in increased risk-taking by small banks (Bloomberg, 2019; Financial Times, 2019b). Internationally, our results suggest that persistent differences between regulatory regimes close to ring-fencing (as in the US and Japan) and regimes that allow integrated universal banks (as in the EU and Canada) can matter for the pattern of credit supply and competition across retail and capital markets.

A key obstacle to identifying the impact of structural separation is that plausibly exogenous shocks to universal bank structures are rare.[4] By affecting banks representing around 60% of total banking assets, the ring-fencing reform—described as "one of the largest ever reforms to the structure of the UK banking industry" (Proudman, 2018)—provides us with a large-scale shock to bank structures.

Ring-fencing requires banks with more than £25 billion of retail deposits to split certain key activities into legally separate subsidiaries: retail deposits must be held in the Ring-Fenced Bank (RFB), while investment banking activities such as underwriting and proprietary trading must be held in the Non-Ring-Fenced Bank (NRFB). The legislation also restricts banks' ability to undo this separation via intragroup loans or other contracts. Combined, these requirements effectively transform large UK banks from European-style integrated universal banks into structures closer to US Bank Holding Companies, where deposit-taking and certain investment banking activities are split across subsidiaries.

We show that the restructuring caused by ring-fencing generates a substantial shock to the funding structure of activities on either side of the fence. Relative to the pre-ring-fencing funding structure, the share of retail deposits in the funding mix of the RFB increases by 18 percentage points on average, whereas it falls by 45 percentage points in the NRFB. To the extent that retail deposits and wholesale funding are imperfect substitutes, this shock should reduce the cost of funding RFB activities and increase the

[4] Most of the existing literature compares different types of bank before and after the introduction of Glass-Steagall or its weakening in the 1990s. One recent exception is Akiyoshi (2019), who investigates the impact of the break-up of a Japanese bank on the valuation of its corporate clients.

cost of funding NRFB activities.

To evaluate the impact of this "deposit funding channel" on credit markets, we study lending activities that are not directly targeted by the reform but which are crucial sources of financing for households and large corporates. First, we analyse retail mortgage lending, which is placed in the RFB, where it can benefit from increased access to retail deposit funding. Second, given the dominant role played by affected banks in the mortgage market, we estimate not only the direct impact of ring-fencing on mortgage lending by the affected banks, but also spillover effects on market structure and risk-taking. Finally, we analyse syndicated corporate lending. All banks have placed syndicated lending in the NRFB, in order to serve large corporate clients from one side of the fence and hence maintain synergies with investment banking activities such as underwriting (Drucker and Puri, 2005; Yasuda, 2005; Neuhann and Saidi, 2018). This implies that it loses access to retail deposit funding.

In testing the impact of the deposit funding channel, a first advantage of our setting is that we do not need to rely on a binary comparison of banks above and below the £25 billion threshold. This is because there is substantial cross-bank variation in the impact of ring-fencing on funding structure, reflecting large cross-bank differences in exposure to investment banking prior to the reform. This variation means that we can construct a continuous measure of exposure to ring-fencing that varies substantially within the group of affected banks.

A second advantage is that for the two markets we study, we have access to loan-level data throughout the 2010–2019 period. This granularity allows us to also exploit variation in the impact of the funding shock on the expected cost of funding different loans originated by the same bank within the same time period. Ring-fencing legislation is passed in 2013, but only starts to affect the availability of deposit funding when it becomes binding in January 2019. The impact of this change on the expected funding mix of a given loan will therefore depend on the maturity of the loan. For example, a *one-year* loan originated in January 2017 will have dropped from the bank's balance sheet before ring-fencing takes effect in January 2019; ring-fencing therefore should not affect its expected funding mix. In contrast, for a *five-year* syndicated loan originated at the same time, the bank would anticipate that the loan would be transferred to the NRFB in 2019, at which point any deposit funding would need to be replaced by wholesale funding. And for a five-year mortgage, the bank would anticipate that more deposits would become available at that point. To the extent that deposits and wholesale funding are imperfect substitutes, these changes in expected funding mix should affect the expected cost of funding the loan over its full maturity, and are therefore likely to affect the terms on which the bank originates the loan. And the strength of this anticipatory effect should be increasing in the fraction of the loan's maturity that falls after January 2019.

Combining this variation across and within banks allows us to estimate the effect of ring-fencing on credit supply while including bank-time fixed effects to control for a wide range of potentially confounding factors, such as changes in capital and liquidity requirements and the Brexit referendum. We can also control for confounding demand-side factors through location-time or borrower-time fixed effects (Khwaja and Mian, 2008).

Our first set of results establishes that ring-fencing causes affected banks to substantially increase retail credit supply. A one-standard-deviation (22 percentage point) increase in deposit funding as a result of ring-fencing is associated with a 20 basis point reduction in the interest rates on mortgages originated after ring-fencing, and with a 5.5 percentage point increase in the bank's market share for a given mortgage product.[5] We find larger effects for longer-maturity mortgages, in line with theories suggesting synergies between stable deposit funding and maturity transformation (Hanson et al., 2015; Drechsler et al., 2021). In contrast, we do not find evidence that the reduction in interest rates is larger for mortgages with high loan-to-value (LTV) ratios, unlike what theories about the moral hazard implications of universal banks' access to retail deposits would suggest (Boyd et al., 1998; Freixas et al., 2007). That is, the increase in mortgage credit supply is

[5]We also confirm that the increase in credit supply holds when we restrict the sample to affected banks only, which ensures that our results are driven by differences in exposure to ring-fencing across affected banks, rather than more general differences between large and small banks.

not accompanied by a reduction in lending standards, limiting potential financial stability concerns (Mian et al., 2013).

We next consider spillover effects on the wider mortgage market. The banks subject to ring-fencing hold dominant positions in the UK mortgage market. This suggests two potential implications of their increased mortgage lending. First, it is likely to lead to increased market concentration. We verify this by constructing a regional measure of exposure to increased credit supply from ring-fenced banks, based on their historical lending footprints. Consistent with our prior, we find that mortgage market concentration increases in regions more exposed to the increased credit supply. Second, increased competition from ring-fenced banks is likely to put pressure on the profitability of smaller banks that are out of scope of ring-fencing but which draw much of their income from the domestic mortgage market. We therefore examine whether smaller banks behave differently if they are operating in regions more exposed to increased competitive pressure from ring-fenced banks. Consistent with the franchise value model of Keeley (1990), we find that smaller banks that are exposed in this way increase their risk-taking, by cutting the rates on high-LTV (riskier) mortgages more, and increasing the share of high-LTV mortgages in their lending portfolios.

In the final section of the paper, we turn to the other side of the fence. We find that ring-fencing leads to a large reduction in corporate credit supply: a one-standard-deviation (11 percentage point) decrease in deposit funding is associated with a 6% reduction in syndicated loan size (intensive margin) and a 15% reduction in the number of syndicated loans in which the bank participates (extensive margin). Consistent with the results being driven by the stability of deposit funding, the reduction in syndicated lending is larger for credit lines and non-leveraged loans, which are more likely to be retained (and hence funded to maturity) by the originator. We also find that the reduction in syndicated lending is larger for loans to non-UK borrowers, consistent with the idea that banks have informational advantages with respect to domestic borrowers (Giannetti and Laeven, 2012).

Contributions to existing literature Our main contribution is to a large literature debating the implications of structural separation of universal banks. In line with a key motivation for Glass-Steagall, existing empirical studies have mostly focused on the impact of separating corporate lending from securities underwriting.[6] We expand the literature in three main directions. First, we emphasise the importance of bank funding structures, in particular the implications of constraining universal banks' use of deposit funding. Second, we document implications not only for large corporates but also for retail lending.[7] Third, we establish not only direct impacts of structural separation on universal banks themselves, but also spillover effects on their competitors.

These new perspectives provide novel insights on several questions central to this literature. First, existing research finds that structural separation increases the cost of credit for large corporate borrowers, for example by preventing synergies between lending and underwriting (Calomiris, 2000; Drucker and Puri, 2005; Yasuda, 2005; Neuhann and Saidi, 2018; Akiyoshi, 2019). Our findings highlight an additional mechanism through which structural separation can affect the cost of credit: by redirecting the benefits of deposit funding towards retail lending. We find that this reduces the cost of household credit at the expense of corporate credit.

Second, we provide new perspectives on the impact of structural separation on competition and market structure. Gande et al. (1999) show that preventing banks from entering the corporate debt underwriting market reduces competition in that market. Our finding

[6] One key concern behind the Glass-Steagall Act was that combining lending and underwriting created conflicts of interest and allowed banks to dupe securities investors. Among others, Kroszner and Rajan (1994), Puri (1994, 1996), and Gande et al. (1997) reject this concern empirically. Instead, later research emphasises that combining lending and underwriting creates informational economies of scope, which lowers firms' borrowing costs (Drucker and Puri, 2005; Neuhann and Saidi, 2018). White (1986) documents evidence against the idea that banks with securities affiliates were more fragile during the Great Depression, another key motive for Glass-Steagall. A broader literature on universal banks studies issues around diversification, economies of scope, internal capital markets, and cross-selling (among others, Campello (2002); Laeven and Levine (2007); Laux and Walz (2009); Lóránth and Morrison (2012)). Several recent papers examine the impact of the Volcker Rule on bond market liquidity (Bessembinder et al., 2018; Bao et al., 2018; Dick-Nielsen and Rossi, 2019). And a related empirical literature studies the benefits to nonbanks from being affiliated with commercial banks; see for instance Fang et al. (2013) and Franzoni and Giannetti (2019).

[7] Hakenes and Schnabel (2014) and Shy and Stenbacka (2017) investigate the theoretical effects of separating retail and investment banking for retail customers, but we are not aware of any empirical evidence.

that ring-fencing increases mortgage market concentration suggests that separation could also have anti-competitive effects in retail markets, as the redeployment of retail deposits leads large universal banks to outcompete smaller banks. The increase in concentration also challenges the idea that structural separation can significantly mitigate the risk of universal banks being too-big-to-fail (King, 2009).

Third, several theoretical papers suggest that, by preventing universal banks from extending the benefits of deposit insurance to riskier investment bank activities, structural separation could reduce moral hazard and risk-taking (Chen and Mazumdar, 1997; Boyd et al., 1998; Kwast and Passmore, 2000; Pennacchi, 2006; Freixas et al., 2007; Farhi and Tirole, 2021). Consistent with this idea, our results suggest that preventing retail deposits from supporting capital market activities incentivises banks to rebalance towards retail lending, which is often considered less risky by proponents of structural separation (King, 2009; Liikanen, 2012). However we also find that this effect might be offset in part by the indirect consequences of this rebalancing for the risk-taking incentives of smaller banks.

We also add to the literature on the benefits of deposit funding. Consistent with existing papers, we find evidence suggesting synergies between deposit-taking and the supply of illiquid loans (Hanson et al., 2015; Li et al., 2019; Carletti et al., 2021; Choudhary and Limodio, 2021; Drechsler et al., 2021) and credit lines (Kashyap et al., 2002; Pennacchi, 2006; Gatev and Strahan, 2006, 2009; Gatev et al., 2009; Acharya and Mora, 2015). Finally, we contribute to the literature on internal capital markets in financial conglomerates (Campello, 2002; Franzoni and Giannetti, 2019; Fecht et al., 2020; Gil-Bazo et al., 2020). We show that preventing universal banks from allocating deposit funding freely across both their retail and investment banking activities causes them to rebalance towards retail lending.

1 The UK ring-fencing regulation

In June 2010, the UK government established the Independent Commission on Banking (ICB) "to consider structural and related non-structural reforms to the UK banking sector to promote financial stability and competition" (ICB, 2011). Chaired by Sir John Vickers, the ICB published its final report in September 2011. One of the report's key recommendations was that core UK retail activities should be ring-fenced—that is, that taking deposits from, and providing overdrafts to, individuals and SMEs should be carried out in separate subsidiaries to wholesale and investment banking activities. The ICB judged that this would: make it easier to resolve troubled banks without requiring taxpayer support; insulate vital retail banking services from external financial shocks; and curtail implicit government guarantees, thus reducing risks to the sovereign, and reducing incentives for excessive risk-taking by reducing the expectation of bail-outs.

Timing and scope The government accepted the majority of the ICB's proposals on ring-fencing. Draft legislation was published in October 2012, and became law in December 2013 as part of the Financial Services (Banking Reform) Act 2013. The law specified that the requirements would come into effect on 1 January 2019, and apply to banking groups with more than £25 billion of retail deposits. Building societies are exempt. In practice, the five largest banking groups were required to restructure: Barclays, HSBC, Lloyds Banking Group, Royal Bank of Scotland, and Santander UK. Together, these groups made up around 60% of both total UK banking assets and total UK mortgage lending as of 2018.

The legislation The legislation requires banking groups to house a number of key domestic retail businesses in a ring-fenced bank (RFB) subsidiary that is legally separate from a non-ring-fenced bank (NRFB) subsidiary where certain investment banking activities must be housed. To do so, the legislation first specifies retail "core activities" that can only be performed by RFBs. These include taking deposits from individuals and SMEs, as well as providing payment services and overdrafts to individuals and SMEs. Second, it specifies "excluded activities" that can only be performed by NRFBs. These include proprietary trading in securities, commodities, and derivatives; underwriting securities; having exposures to financial institutions other than building societies and other RFBs; having operations outside the European Economic Area (EEA); and buying securitisations of other financial institutions.

The two activities that we study in this paper (mortgages and syndicated lending) are considered neither 'core' nor 'excluded', and are therefore not *directly* constrained by the legislation. In practice, however, the legislation provides strong incentives for banks to place these activities on opposite sides of the fence. If banks wish to serve retail customers from one side of the fence, then they must place mortgage lending in the RFB, alongside retail deposits. Similarly, if banks wish to serve large corporate clients from one side of the fence—and hence preserve established synergies between lending to these corporates and underwriting their securities (Drucker and Puri, 2005; Yasuda, 2005; Neuhann and Saidi, 2018)—then they must place syndicated lending in the NRFB. Indeed, all affected banks have restructured their loan books in this manner, with mortgages in the RFB and syndicated loans in the NRFB. These restructuring incentives imply that ring-fencing has the potential to alter banks' behaviour in credit markets, despite those markets not being directly targeted by the reform.

The legislation, and associated rules set by the Prudential Regulation Authority (PRA), also impose several requirements to ensure that RFBs are sufficiently independent and insulated from other entities in their banking groups. For example, the RFB must meet regulatory capital and liquidity requirements on its own; have independent governance; and manage any exposures to NRFBs within its group on third-party and arm's length terms. These requirements ensure that ring-fencing cannot be "undone" via intragroup contracts.[8]

[8] See Britton et al. (2016) for more detail. The UK ring-fence is similar to the US Bank Holding Company approach in that it allows universal banks to engage in both retail and investment banking, but in separate, self-sufficient subsidiaries. Most of the more recent structural separation proposals share

2 Ring-fencing and deposit funding: Theory and identification

2.1 Theory

Ring-fencing implies that retail deposits can only be used to fund activities in the ring-fenced bank (RFB) and cannot fund activities in the non-ring-fenced bank (NRFB), which must instead rely on wholesale funding. Our aim is to test whether this restriction has side effects on two macroeconomically important credit markets: retail mortgage lending, which is placed in the RFB and therefore experiences an increase in deposit funding, and syndicated corporate lending, which is placed in the NRFB and therefore loses access to deposit funding.

This constraint has the potential to alter the lending behaviour of universal banks to the extent that retail and wholesale funding are imperfect substitutes. Several theories suggest that this might be the case. In particular, household preferences for liquidity (Stein, 2012), deposit insurance (Stein, 1998), and market power in deposit markets (Drechsler et al., 2017) might reduce the cost of retail deposits relative to wholesale funding.[9] Assuming that these reduced funding costs would be shared across the bank's activities absent ring-fencing, redirecting deposit funding entirely to RFB activities would reduce the cost of funding RFB activities and increase the cost of funding NRFB activities. All else equal, this would incentivise banks to rebalance towards RFB activities and away from NRFB activities. We refer to this as the "deposit funding channel" of ring-fencing.

The strength of the deposit funding channel across markets is an empirical question, for a number of reasons. First, whether a bank would pass on changes in funding costs

a similar "middle-ground" approach. Appendix A compares the UK ring-fence with regulations and reform proposals in the US and Europe in more detail.

[9]Some sources of short-term wholesale funding (such as short-term wholesale deposits and repo) might carry lower interest rates than retail deposits. However these funding sources are likely to be significantly flightier than retail deposits. Deposit funding might also be more expensive than wholesale funding when the central bank policy rate is negative (Heider et al., 2019). This not the case in our study, as the Bank of England's policy rate remains above zero throughout our sample period.

to borrowers might also depend on any overhead costs created by the need to comply with ring-fencing, as well as on broader competitive dynamics. Second, theory offers different predictions about the extent to which different activities are affected by deposit funding. Theories stressing the benefits of deposit funding for maturity transformation and liquidity risk management suggest that any rebalancing would mainly affect activities known to benefit from such synergies, such as providing long-term loans (Hanson et al., 2015; Drechsler et al., 2021) and credit lines (Kashyap et al., 2002). Meanwhile, theories stressing the risk-insensitive nature of deposits, and its impact on moral hazard, suggest that the rebalancing would particularly affect risky activities (Freixas et al., 2007; Boyd et al., 1998).[10]

2.2 Empirical model

In order to estimate the impact of the deposit funding channel—that is, the impact of the change in funding structure as a result of ring-fencing on a bank's lending behaviour across different markets—we estimate regressions with the following general form:

$$\text{Loan}_{i,l,t} = \beta \left(\Delta \text{Retail funding}_i \times \%(\text{Post})_{l,t} \right) + \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \tag{1}$$

where $\text{Loan}_{i,l,t}$ is the price or volume of loan l originated by bank i at time t, and $\Delta\text{Retail funding}_i \times \%(\text{Post})_{l,t}$ is a loan-level measure of the impact of ring-fencing on funding structure. Our main sample period is 2010–2019, covering the adoption and implementation of ring-fencing requirements. The two main explanatory variables ($\Delta\text{Retail funding}_i$ and $\%(\text{Post})_{l,t}$) capture two key sources of variation in the extent to which the deposit funding channel should affect banks' lending behaviour. We now explain these two variables in turn, before discussing the set of controls.

[10] Chan et al. (1992) and Freixas and Rochet (1998) show that regulators are unlikely to be able to price deposit insurance fairly with respect to risk.

ΔRetail funding$_i$ Our first explanatory variable captures variation in the strength of the deposit funding channel across banks. To do so, it measures the bank-level change in funding structure caused by ring-fencing. For banks that have less than £25 billion of retail deposits, ΔRetail funding$_i$ is defined to be zero, because these banks are not subject to ring-fencing and so do not need to restructure. For banks above the threshold, ΔRetail funding$_i$ measures the extent to which the retail funding ratio of the RFB (NRFB) increases (decreases) as a result of ring-fencing. For RFBs, we compute the increase in retail funding as the difference between the retail funding ratio of the RFB and the retail funding ratio of the group:

$$\Delta\text{Retail funding}^{\text{RFB}} = \frac{\text{RFB retail deposits}}{\text{RFB total assets}} - \frac{\text{Group retail deposits}}{\text{Group total assets}}$$

For NRFBs, we simply compute the decrease in retail funding as equal to the retail funding ratio of the group, because the retail funding ratio of the NRFB is zero by definition; that is:

$$\Delta\text{Retail funding}^{\text{NRFB}} = \frac{\text{Group retail deposits}}{\text{Group total assets}}$$

These variables are illustrated in Figure 1 for a stylised universal bank subject to ring-fencing. At the group level, two-thirds of the bank's assets are mortgages, while one-third are syndicated loans and assets associated with investment banking. On the liability side, the group is funded 50% by retail deposits and 50% by wholesale funding (we ignore equity for simplicity). Before ring-fencing (left panel), the group is structured as a single legal entity, with a retail funding ratio of 50%. After ring-fencing (right panel), the group must restructure into two separate subsidiaries. Retail deposits and mortgages are housed in the RFB, while investment banking and syndicated loans are housed in the NRFB and entirely funded by wholesale funding. For the RFB, the retail funding ratio increases from 50% to 75%, so ΔRetail funding$_i^{\text{RFB}}$ is 25 percentage points. For the NRFB, the retail funding ratio falls from 50% to 0%, so ΔRetail funding$_i^{\text{NRFB}}$ is 50 percentage points.

In practice, across the banking groups affected by ring-fencing, the average values of

ΔRetail funding$_i^{\text{RFB}}$ and ΔRetail funding$_i^{\text{NRFB}}$ are 18pp and 45pp, respectively, implying that ring-fencing resulted in very substantial shocks to the funding structures of affected banks. However there is also substantial variation in these measures (the standard deviations are 22pp and 11pp), reflecting large differences in the pre-ring-fencing business models of the affected banks.[11]

$\%(\mathbf{Post})_{l,t}$ Ring-fencing legislation was passed in December 2013 but only came into force from January 2019. Our second explanatory variable ($\%(\text{Post})_{l,t}$) takes advantage of this fact by exploiting variation in the extent to which ring-fencing should affect individual loans within a given bank and time period in the run-up to ring-fencing implementation.

When a bank originates a loan and sets its interest rate, a key factor affecting the valuation of the loan is the expected cost of funding the loan throughout its term. In turn, the expected funding cost is a function of the *mix* of funding sources that the bank expects to use to fund the loan, and the expected *cost* of those funding sources. Importantly, the expected funding mix depends on the availability of funding sources not only when the loan is originated, but throughout its entire term.

Ring-fencing acts as a shock to the availability of retail deposits as a funding source for different loans, because it implies that deposits can only fund activities in the RFB (such as mortgages) and not activities in the NRFB (such as syndicated loans). And importantly, the impact of this change in availability depends on the maturity of the loan.

In particular, any loan that matures before January 2019 will have dropped from the bank's balance sheet before ring-fencing affects the availability of deposit funding; its expected funding mix (and hence expected funding cost) should therefore not be affected by ring-fencing. In contrast, consider a syndicated loan that is originated after December

[11] ΔRetail funding$^{\text{RFB}}$ is relatively small for retail-focused banking groups, because these groups have relatively few prohibited activities; therefore, the RFB balance sheet will be relatively similar to that of the group, and its funding structure will change little. Meanwhile, ΔRetail funding$^{\text{RFB}}$ is larger for a group with substantial exposure to prohibited activities such as investment banking, because this group's RFB will be substantially smaller than the original group, meaning that there will be a large increase in the deposit funding share of RFB activities.

2013 and matures after January 2019. When originating this loan, the bank would be able to anticipate that the loan would be transferred to the NRFB in 2019, at which point any retail deposits that are funding the loan would need to be replaced by wholesale funding. The bank would therefore expect wholesale funding to constitute a larger share of the loan's overall funding mix, relative to the counterfactual without ring-fencing. If deposit funding is cheaper than wholesale funding (conditional on maturity) (see Section 2.1), then this implies a higher expected funding cost. Conversely, any deposits that had been funding NRFB activities before ring-fencing would, from January 2019, become available to fund RFB activities such as mortgages. Therefore, when originating a mortgage that matures after January 2019, the bank could expect the mortgage to be funded with a higher share of deposit funding (relative to the counterfactual without ring-fencing), implying a lower expected funding cost.

In line with this idea, we define $\%(\text{Post})_{l,t}$ to be the proportion of the loan's term that falls after January 2019. This measures the proportion of the loan's term that will be affected by the change in deposit availability caused by ring-fencing. Variation in the value of this variable across loans is illustrated in Figure 2. For example, for a five-year loan originated in January 2017, $\%(\text{Post})_{l,t}$ is equal to 60%; for loans that mature before January 2019, it is equal to zero; and for loans originated after January 2019, it is equal to 100%.[12]

In addition to expected funding cost, the value of a loan also depends on any *risks* associated with originating the loan. This includes the risk of having to inefficiently liquidate the loan in case funding is insufficient. Ring-fencing can affect this 'rollover risk' because retail deposits are likely to be a more stable funding source than wholesale funding (Paligorova and Santos, 2017). As with expected funding cost, the effect of ring-fencing on expected rollover risk should vary with $\%(\text{Post})_{l,t}$, i.e. the proportion of the loan's

[12] We define $\%(\text{Post})_{l,t}$ to be equal to zero for mortgages originated before December 2013, when ring-fencing legislation was finalised. The results are very similar if we instead set $\%(\text{Post})_{l,t}$ to zero for mortgages originated before September 2011 (when ring-fencing was proposed by the ICB) or before October 2012 (when draft legislation was published). This is natural because very few UK mortgages have maturities beyond five years.

term that falls after January 2019. Concretely, the longer a syndicated loan is expected to be held on the NRFB balance sheet, which cannot be funded by stable deposit funding, the greater its expected rollover risk. Conversely, the longer a mortgage is expected to be held on the RFB balance sheet, the lower its expected rollover risk.

Controls The fact that we can exploit variation both across banks (ΔRetail funding$_i$) and across loans within the same bank and time period ($\%(\text{Post})_{l,t}$) allows us to employ a rich set of fixed effects to control for a wide range of potential supply-side and demand-side confounding factors.

In particular, our general model (1) includes bank-time fixed effects to control for other supply-side developments coinciding with the introduction of ring-fencing—even those that might affect individual banks differently. For example, these fixed effects control for impacts from the 2016 Brexit referendum and other changes in bank regulation. More broadly, these fixed effects also control for any balance-sheet and business-model characteristics that might be correlated with ΔRetail funding$_i$ and affect lending decisions irrespective of ring-fencing. For some confounding factor to nonetheless bias our estimates, it would need to vary not only *across* banks in a way that correlates with ΔRetail funding$_i$, but also *within* banks in a way that correlates with $\%(\text{Post})_{l,t}$. In Section 2.4 below, we discuss remaining threats to identification in more detail.

The granularity of our loan-level datasets also allows us to use additional fixed effects and control variables to rule out a wide range of potential confounding demand-side factors, e.g. related to changes in the pool of borrowers and changes in credit demand. In particular, depending on the specification, our mortgage regressions include product-time and location-time fixed effects, and our syndicated loan regressions include borrower-time fixed effects. We discuss these controls in more detail in Sections 4 and 6.

2.3 Instrumental variable approach

Throughout the paper, our preferred approach to estimating our empirical model (1) is to use an instrumental variable strategy, whereby we instrument ΔRetail funding$_i$ using credibly exogenous predictors.

This approach allows us to mitigate two endogeneity issues about ΔRetail funding$_i$ that our extensive set of fixed effects and controls do not address. First, in principle, banks could exert some influence over the extent to which their funding mix would change, because they had flexibility over whether to place assets that are neither 'core' nor 'excluded' in the RFB or NRFB (see Section 1). Second, ΔRetail funding$_i$ is only observable at end-2018, after the affected banks completed their restructuring. We cannot compute the ratio earlier than this because the RFBs and NRFBs did not exist as distinct legal entities before this point, so did not have separate balance sheets.

To address these issues, we instrument ΔRetail funding$_i$ using predictors measured in 2011, before the legislation was proposed. This mechanically mitigates the two endogeneity concerns because it ensures that our preferred estimator only exploits variation in ΔRetail funding$_i$ that cannot be influenced by banks' endogenous response to the reform.

The variable we seek to instrument (ΔRetail funding$_i$) is zero for banks not subject to ring-fencing, and varies between zero and one for banks subject to the reform. To capture these two dimensions, we use two instruments. Our first instrument is an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and zero otherwise ($I(>25)_{2011}$). A bank that exceeds this threshold in 2011 is very likely to do so in subsequent years; this indicator is therefore a strong predictor of whether or not a bank will be subject to ring-fencing.

Our second instrument is the ratio of the bank's non-interest income to total operating income (NII ratio), measured in 2011 ($\%(\text{NII})_{2011}$). A bank's NII ratio is strongly correlated with its prior exposure to investment banking activities prohibited by the legislation, and thus with the extent to which it would be required to restructure. Banks with low NII ratios are unlikely to derive much of their income from excluded activities such as investment banking, and therefore do not need to substantially restructure their businesses to meet ring-fencing requirements. On the other hand, banks with high NII ratios derive more of their income from these activities, and therefore need to restructure more substantially. As a result, their RFBs experience a larger increase in the retail funding share.[13]

2.4 Remaining threats to identification and the exclusion restriction

The fact that we exploit variation both across and within banks, combined with our extensive set of controls and instrumental variable strategy, guards against a wide range of challenges to identification.

Given our setup, for an alternative channel to explain our results, it would need to: (i) vary across banks in a way that is correlated with $\Delta\text{Retail funding}_i$; (ii) vary across loan maturities and time in a way that is correlated with $\%(\text{Post})_{l,t}$; and (iii) not be controlled for by other regressors. For this alternative channel to also lead to a violation of the exclusion restriction in our IV regressions, it would additionally need to be correlated with our instruments for $\Delta\text{Retail funding}_i$.

In Appendix B, we provide a detailed discussion of potential alternative mechanisms and the extent to which they could meet the criteria. While a number of developments parallel to the introduction of ring-fencing could meet some of the criteria, it is harder to think of mechanisms that could meet all of them. For example, while the impact of the Brexit referendum might vary across banks (criterion i), it is not clear why its impact would vary with a loan's maturity and distance between origination and January 2019 (criterion ii).

Similarly, there are several factors that could plausibly correlate with our instruments

[13]Specifically, we instrument $\Delta\text{Retail funding}_i \times \%(\text{Post})_{l,t}$ with $I(> 25)_i^{2011} \times \%(\text{Post})_{l,t}$ and $\%(\text{NII})_i^{2011} \times \%(\text{Post})_{l,t}$.

and influence lending decisions in ways unrelated to ring-fencing; these include for instance heterogeneity in banks' size, diversification, and business model. However, these factors are unlikely to threaten the exclusion restriction because our regressions include bank-time fixed effects, as well as interactions between $\%(\text{Post})_{l,t}$ and a range of balance-sheet characteristics (criterion iii).

In Appendix B, we identify a small number of mechanisms that could plausibly meet all the criteria: changes to the bank's perceived riskiness due to ring-fencing; changes to regulatory capital or liquidity ratios caused by ring-fencing; and changes in LTV-specific capital requirements on mortgages (Benetton, 2021). However we provide additional empirical tests to demonstrate that these mechanisms do not explain our main findings.

3 Data and sample construction

To implement our identification strategy, we combine three data sources.

Mortgage lending Our analysis of the mortgage market uses the Product Sales Database (PSD), a confidential regulatory loan-level dataset covering the universe of residential mortgage originations in the UK. The PSD is collected by the UK Financial Conduct Authority (FCA) and extends back to 2005. For each loan, the dataset provides the identity of the lender, and information on the borrower (including age, income, and credit history), the property (including its location), and mortgage characteristics (including origination date, loan size, initial interest rate, fixation period, loan-to-value (LTV) ratio, loan-to-income (LTI) ratio, and term). The dataset does not record whether the mortgage is retained or sold by its originator. Unlike in the US, however, the vast majority of mortgages are retained during our sample period.

We focus on vanilla fixed-rate mortgages originated between January 2010 and June 2019 (we exclude observations before 2010 in order to avoid the effects of the financial crisis, and because there were several bank mergers in 2009). Summary statistics for this sample are provided in Table 1. The sample consists of over four million loans. Around 34% of mortgagors are home movers and around 28% are first-time buyers. The average loan is around £140,000 and has a 66% LTV ratio.

The large majority of UK mortgages have a "fixation period" of between two and five years.[14] During this period, there is typically a substantial early repayment charge; and after the fixation period ends, the mortgage typically reverts to a floating reset rate that is significantly higher than the rates available on new mortgages. The vast majority of borrowers thus tend to remortgage around the time that the fixation period ends (Cloyne et al., 2019). For simplicity, we thus refer to the fixation period as "maturity" in the rest of the paper.

UK mortgages are typically highly standardised, and priced based on maturity and LTV ratio only. That is, unlike in the US, other factors such as loan-to-income ratio and borrower credit history have only a limited impact on pricing, so long as the borrower qualifies for the product (Robles-Garcia, 2019; Benetton et al., 2022).[15] We therefore refer to the combination of maturity and LTV ratio as the mortgage "product".

Syndicated lending Syndicated loans are loans extended to one borrower (primarily large non-financial corporates) by multiple lenders. The group of lenders is known as the syndicate, and includes at least one lead arranger, who negotiates the terms of the loan and recruits other lenders (known as participants) via a book-building process.

We obtain loan-level data on global syndicated loan originations from Refinitiv LPC's DealScan dataset for the period 2010–2018. DealScan provides detailed information on individual loan facilities, including the identity of the borrower, the identities of the lenders in the syndicate (including lead arrangers and participants), the loan type (typically term loan or credit line), loan amount, maturity, and interest rate. We consolidate borrowers and lenders at the ultimate parent level. Following Roberts (2015), we drop observations

[14]Some mortgages with fixation periods of up to ten years are available, but thirty-year fixed-rate mortgages as seen in the US are very rare.

[15]Relatedly, UK borrowers typically pay the rates that banks advertise, unlike in the US where contracted mortgage rates often differ substantially from advertised rates (Bhutta et al., 2019).

that are likely to be amendments to existing loans, because these do not necessarily involve new credit. Summary statistics for this dataset are reported in Table 2.

Bank balance sheets We use quarterly regulatory balance sheet and income statement data from the Bank of England to estimate banks' funding structures before and after ring-fencing implementation, and to construct bank-level controls.

4 Ring-fencing and mortgage lending

In this section, we estimate how the change in funding mix caused by ring-fencing affects banks' behaviour in the mortgage market.

Before turning to our loan-level analysis, we consider aggregate trends in mortgage prices. Figure 3 plots average quoted spreads for common UK mortgage products. It shows that spreads fell substantially in the years leading up to ring-fencing implementation across all major market segments (Panel A), and that this trend was largely driven by banks subject to ring-fencing (Panel B). Industry commentary links these trends to ring-fencing: several banks have argued that, by requiring deposits to fund domestic retail lending, ring-fencing has contributed to a "price war" in the UK mortgage market (Financial Times, 2019a; Building Societies Association, 2021).

We now seek to isolate the role of ring-fencing in driving these developments from potential confounding factors using the identification strategy described in Section 2.

4.1 Mortgage spreads

To test how ring-fencing affects banks' mortgage lending behaviour, we estimate the following variant of our general model (1):

$$\text{Spread}_{i,l,t} = \beta \left(\Delta\text{Retail funding}_i^{\text{RFB}} \times \%(\text{Post})_{l,t}\right) + \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \qquad (2)$$

where $\text{Spread}_{i,l,t}$ is the interest rate spread on mortgage l originated by bank i in month t, measured as the interest rate minus the maturity-matched OIS rate. Our main explanatory variable $\Delta\text{Retail funding}_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing; and $\%(\text{Post})_{l,t}$ is the proportion of mortgage l's fixation period that falls after January 2019 (see Section 2.2). If the increased retail funding share of RFB activities (such as mortgages) makes these activities more attractive to the bank, then β should be negative—that is, mortgage spreads should decrease with exposure to ring-fencing.

Estimated regressions results for several versions of equation (2) are reported in Table 3. The sample consists of fixed-rate mortgages originated between 2010 and 2019. Standard errors are clustered by bank.

As discussed in Section 2.3, our preferred specification instruments $\Delta\text{Retail funding}_i^{\text{RFB}}$ using pre-determined predictors. For comparison, in column 1 we start by estimating the model by OLS—that is, we simply use the realised value of $\Delta\text{Retail funding}_i^{\text{RFB}}$. We include a rich set of fixed effects to control for a range of potential confounding factors. These include bank-month fixed effects to control for supply-side shocks that might affect individual banks differently, such as other changes in bank regulation and the Brexit referendum. We also include product-month fixed effects, where "product" refers to the combination of maturity (fixation period) and LTV ratio.[16] As discussed in Section 3, UK mortgages are typically highly standardised and priced based on maturity and LTV only (Robles-Garcia, 2019; Benetton et al., 2022), so these product-month fixed effects control for changes in borrower preferences across maturities, and changes in industry-level lending standards. Finally, we include bank-product fixed effects to control for time-invariant determinants of a bank's lending behaviour in a given product category, e.g. related to bank specialisation across maturities or borrower risk.

The parameter estimate for our main coefficient of interest β is negative and significant

[16] To construct these fixed effects, we measure maturity in months, and assign each mortgage to one of ten LTV buckets (0-50, 50-60, 60-65, 65-70, 70-75, 75-80, 80-85, 85-90, 90-95, 95-100).

at the 1% confidence level. That is, consistent with the deposit funding channel, banks more affected by ring-fencing reduce rates for mortgages with maturities extending further past the implementation date.

In column 2 we move to estimating the model with an IV approach. Specifically, we instrument ΔRetail funding$_i^{\text{RFB}}$ using two variables measured in 2011, before ring-fencing legislation was proposed: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011; and the ratio of the bank's non-interest income to total operating income in 2011 (see Section 2.3 for further discussion).[17] The instruments are strong, yielding a first-stage Kleibergen-Paap F-statistic of around 30. The estimated coefficient is again negative and highly statistically significant, with a similar magnitude to the OLS estimate.

In columns 3–5 we add a progressively larger set of additional control variables and fixed effects to the IV regression. In column 3, we add a vector of (lagged) quarterly bank-level control variables interacted with $\%(\text{Post})_{l,t}$: log(total assets); return on assets; cash / total assets; capital / risk-weighted assets; and wholesale funding / total assets. In column 4, we add several loan-level control variables: LTV ratio;[18] LTI ratio; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, borrowers with an impaired credit history, and brokered loans.[19] While the standardised nature of UK mortgages mitigates concerns about unobserved borrower quality, in column 5 we add property location-month fixed effects, to control for changes in local economic conditions.[20] The negative relationship between mortgage spreads and ring-fencing is robust to all of these additional controls.

The estimated effect is highly economically significant. For our baseline IV specification including the full set of controls (column 5), the estimate of β is around -0.9. Com-

[17]Specifically, we instrument ΔRetail funding$_i^{\text{RFB}} \times \%(\text{Post})_{l,t}$ with $I(>25)_i^{2011} \times \%(\text{Post})_{l,t}$ and $\%(\text{NII})_i^{2011} \times \%(\text{Post})_{l,t}$.

[18]This controls for any residual variation in pricing *within* the ten LTV buckets.

[19]Around 70% of mortgages in the UK are originated via brokers. This allows banks to lend in areas where they have few branches (Robles-Garcia, 2019).

[20]We measure property location at the electoral ward level (LAU2). There are around 10,000 electoral wards in the UK. The average population of a ward is therefore around 6,000, which is slightly smaller than the average population of US ZIP codes.

paring mortgages originated after ring-fencing implementation (for which $\%(\text{Post})_{l,t} = 1$) to mortgages that mature before ring-fencing implementation (for which $\%(\text{Post})_{l,t} = 0$), a one-standard-deviation (22 percentage point) increase in $\Delta\text{Retail funding}_i^{\text{RFB}}$ is therefore consistent with a reduction in the interest rate spread of around 20 basis points. For comparison, the standard deviation of spreads over the sample period is 97 basis points (Table 1).

As we discuss above, our rich set of fixed effects and control variables rules out a wide range of potential confounding factors and exclusion restriction violations, including developments that might affect different banks differently over time. In Appendix B, we provide a more detailed description of potential alternative mechanisms and how they are controlled for by our set-up. We identify a small number of potential mechanisms that our baseline regressions do not fully control for: changes to the bank's perceived riskiness due to ring-fencing; changes to regulatory capital or liquidity ratios caused by ring-fencing; and changes in LTV-specific capital requirements on mortgages (Benetton, 2021). However we provide additional empirical tests to demonstrate that these mechanisms do not explain our findings.

Our control variables and fixed effects control for differences in bank size and business model along several dimensions. However, to further assuage concerns about unobserved differences between the banks affected by ring-fencing and other banks active in the mortgage market, we also re-run our regressions including only the sample of affected banks: this ensures that our results are driven by variation in our continuous measure of exposure to ring-fencing across affected banks, rather than more general differences between large and small banks. The results are similar to our baseline results (Appendix C, Table C.1).

4.2 Mortgage market shares

We next test whether the decrease in mortgage spreads associated with ring-fencing translates into relatively higher mortgage volumes. To do so, we aggregate mortgage lending volumes by bank, origination quarter, and product (where product is defined as the combination of maturity quarter and LTV bucket). We then estimate regressions of the form:

$$\text{Market share}_{i,j,t} = \beta \left(\Delta\text{Retail funding}_i^{\text{RFB}} \times \%(\text{Post})_{j,t} \right) + \text{Controls}_{i,j,t} + \varepsilon_{i,j,t}, \tag{3}$$

where Market $\text{share}_{i,j,t}$ is the market share of bank i in product j in quarter t. Our main explanatory variable $\Delta\text{Retail funding}_i^{\text{RFB}} \times \%(\text{Post})_{j,t}$ is as defined in equation (2). $\text{Controls}_{i,j,t}$ includes bank-quarter fixed effects; bank-product fixed effects; and the interaction of the five bank-level controls used in equation (2) with $\%(\text{Post})_{j,t}$. Standard errors are clustered by bank.[21]

The results reported in Table 4 provide evidence that banks more affected by ring-fencing originate relatively larger mortgage volumes, and so gain market share at the expense of competitors. This result is robust to estimating by OLS (column 1), instrumenting $\Delta\text{Retail funding}_i^{\text{RFB}}$ using variables determined in 2011 (column 2), and adding bank-level control variables (column 3).

Our baseline IV estimate including the full set of controls (column 3) suggests that a one-standard-deviation (22 percentage point) increase in the retail funding ratio as a result of ring-fencing is associated with an increase in product market share of around 5.5 percentage points.

The dependent variable in equation (3) is constructed by dividing bank i's lending volume in product j by total (market-wide) lending in product j. This normalisation means that the estimates in columns 1–3 assign equal weight to all products. However some products are substantially more common than others (for example, most mortgages have two-year or five-year maturity), meaning that increased market shares in these products would be stronger evidence of increased credit supply overall. We therefore re-estimate the equation by WLS, weighting by total (market-wide) lending in the product. The positive relationship between exposure to ring-fencing and market share is robust to this

[21] The sample period for these regressions begins in January 2015, because mortgage maturities (necessary for computing market shares at the maturity level) are not always reported prior to 2015.

weighting (column 4), including when estimated using instrumental variables (column 5).

4.3 Heterogeneous impacts

The results above establish that universal banks affected by ring-fencing increase their retail mortgage lending, suggesting that ring-fencing incentivises a rebalancing towards activities that can still be funded with retail deposits. These results are consistent with theories where deposit funding has advantages relative to wholesale funding, for instance due to deposit insurance, household preferences for liquidity, or market power. However, as explained in Section 2.1, different theories about the implications of deposit funding for credit supply provide different predictions about where the effect is likely to be strongest. We therefore consider how the impact of ring-fencing varies across mortgages.

Theories emphasising synergies between deposit-taking and maturity transformation (Hanson et al., 2015; Drechsler et al., 2021) suggest that the increase in credit supply would be larger for *longer-term* mortgages. Theories emphasising the moral hazard implications of deposit insurance would instead predict that, by redirecting risk-insensitive funding to the RFB, ring-fencing should lead to a larger increase in credit supply for *higher-risk* mortgages (Freixas et al., 2007). Increased availability of deposit funding might also incentivise riskier lending due to agency problems within the bank (Acharya and Naqvi, 2012).

To explore these ideas, we expand equation (2) by interacting our main coefficient $\Delta\text{Retail funding}_i^{\text{RFB}} \times \%(\text{Post})_{l,t}$ first with an indicator variable for long-maturity mortgages (defined as maturity greater than two years), and then with an indicator variable for high-LTV mortgages (defined as LTV greater than 90%). The results using these triple interactions are reported in Table 5.[22] Columns 1–3 show that the negative impact of ring-fencing on mortgage spreads is larger for longer-term loans, consistent with synergies between deposit-taking and long-term lending. On the other hand, across all

[22] In the IV regressions, all interactions involving $\Delta\text{Retail funding}_i^{\text{RFB}}$ are instrumented with the corresponding interactions involving the instrumental variables.

specifications, the coefficient estimate on the high-LTV triple interaction is statistically insignificant (columns 4–6). In other words, we find no evidence that ring-fencing causes treated banks to reduce the risk-sensitivity of their mortgage pricing. Table 6 reports consistent results using mortgage market shares as the dependent variable: the increase in market share is larger for longer-term mortgages (columns 1 and 2) but no larger for higher-risk mortgages (columns 3 and 4).

5 Effects on competition and risk-taking

In the previous section, we establish that banks more affected by ring-fencing rebalance towards the domestic retail market by reducing mortgage spreads, and hence gain mortgage market shares. In this section, we first examine how this rebalancing impacts mortgage market structure, and then consider spillover effects on the behaviour of banks not directly affected by ring-fencing.

5.1 Market structure

The UK mortgage market is very concentrated, with the five banks subject to ring-fencing accounting for around 60% of total volume as of 2018. An increase in the market shares of these banks is therefore likely to be associated with an increase in market concentration. To verify this, we compute the Herfindahl-Hirschman Index (HHI) at the level of local markets, and test whether concentration increases more in local markets more exposed to the effects of ring-fencing.

We define a local market as the combination of property location and product, where property location is measured at the district level,[23] and product is defined as the combination of maturity (measured in quarters) and LTV bucket. We then exploit two sources of variation in the exposure of a given market to the increased credit supply from treated banks: one at the product level, and one at the district level.

[23] There are 390 districts (LAU1) in our sample.

First, our results in Section 4 show that treated banks increase credit supply more for mortgages with maturities extending further past the ring-fencing implementation date. To capture this effect, we measure a product's exposure to ring-fencing using %(Post) measured at the product level, in line with our baseline regressions.

Second, we exploit the idea that the increase in credit supply due to ring-fencing is likely to be larger in districts where treated banks have a larger historical presence. This follows from the fact that there is a substantial degree of persistence in banks' geographical lending footprints over time. To illustrate this persistence, we regress bank market shares in 2018 (measured at the district level) on market shares in 2011, plus bank fixed effects. The results are shown in Table 7. When we measure market shares using all loans, the estimated coefficient is 0.424 (column 1), and highly statistically significant. When we instead use either non-brokered loans (column 2) or brokered loans (column 3), we see that the persistence is primarily driven by non-brokered loans, suggesting that branch presence is an important factor (Robles-Garcia, 2019).

Given this strong persistence, we can use banks' 2011 market shares in a district to construct a proxy for the district's exposure to ring-fencing. Specifically, for each district, we compute the 2011 market shares for each bank, and use these to construct the weighted average of $\Delta\text{Retail funding}_i^{\text{RFB}}$. That is, for district g, we compute:

$$\text{Exposure}_g = \sum_{i=1}^{N} \text{Market share}_{i,g}^{2011} \times \Delta\text{Retail funding}_i^{\text{RFB}}. \tag{4}$$

We use non-brokered loans to compute the market shares, because these exhibit more geographical persistence. Results are similar if we instead use all loans. We then run regressions of the form:

$$\text{HHI}_{g,j,t} = \beta \left(\text{Exposure}_g \times \%(\text{Post})_{j,t}\right) + \text{Controls}_{g,j,t} + \varepsilon_{g,j,t}, \tag{5}$$

where g indexes districts, j indexes products (defined by maturity quarter and LTV bucket), and t indexes origination quarters. To mitigate endogeneity concerns associated with ΔRetail funding$_i^{\text{RFB}}$, we follow a similar approach to our loan-level regressions by instrumenting Exposure$_g$ using equivalent weighted averages of the two instruments for ΔRetail funding$_i^{\text{RFB}}$ discussed in Section 2.3. The set of controls consists of product-quarter fixed effects to control for product-level changes in credit demand and lending standards; district-product fixed effects to control for differences in the pool of borrowers across districts; and district-quarter fixed effects to control for changes in local economic conditions.

Consistent with our prior, the results in Table 8 suggest that markets more exposed to ring-fencing experience a larger increase in concentration. Our baseline IV specification including the full set of controls (column 4) suggests that a one-standard-deviation (3.5pp) increase in geographical exposure to ring-fencing is associated with an increase in the HHI of around 3.2% of one standard deviation.

5.2 Competitors' response

Increased credit supply by large banking groups is likely to put pressure on the profitability of smaller banks that are out of scope of ring-fencing but draw much of their income from the domestic mortgage market. To understand how this affects the behaviour of smaller banks, we follow a similar approach to Section 5.1. Specifically, we exploit the idea that the competitive pressure created by ring-fencing should differ (i) across mortgage maturities and time periods, because treated banks increase credit supply more for mortgages with maturities extending further past the ring-fencing implementation date; and (ii) across geographies, due to persistent heterogeneities in the geographical lending footprints of treated banks.

To capture the geographical dimension, we compute the following variable for each competitor bank i:

$$\text{Exposure}_i = \sum_{g=1}^{G} \text{Portfolio share}_{i,g}^{2011} \times \text{Exposure}_g, \tag{6}$$

where Portfolio $\text{share}_{i,g}^{2011}$ is the proportion of bank i's 2011 mortgage lending portfolio originated in district g; and Exposure_g is district g's exposure to ring-fencing, as estimated in equation (4). We then drop the banks directly affected by ring-fencing from our sample, and for the remaining banks we estimate the model:

$$\text{Spread}_{i,l,t} = \beta\left(\text{Exposure}_i \times \%(\text{Post})_{l,t}\right) + \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \tag{7}$$

where the set of control variables is the same as used in equation (2). Again, we instrument Exposure_i using equivalent weighted averages based on the two instruments for $\Delta\text{Retail funding}_i^{\text{RFB}}$ discussed in Section 2.3.

The regression results for model (7) are reported in Table 9, columns 1 and 2. The estimate of β is statistically insignificant, suggesting that competitors more exposed to increased competition from ring-fenced banks do not systematically reduce mortgage spreads across all products.

In columns 3–6, we test whether more exposed competitors respond to ring-fencing differently across different product types. In columns 3 and 4, we find that more exposed competitors relatively reduce mortgage spreads on longer-maturity mortgages (maturity greater than two years), consistent with the increased competitive pressure from treated banks in this segment (Section 4.3). Meanwhile, in columns 5 and 6 we find that more exposed competitors significantly reduce their spreads on high-LTV mortgages (LTV greater than 90%) relative to lower LTV mortgages. That is, more exposed competitors respond to increased competition from ring-fenced banks by reducing the risk-sensitivity of their mortgage pricing. The parameter estimate is around -5, suggesting that a one-standard-deviation (3.3pp) increase in Exposure_i is associated with a reduction in the spread on high-LTV mortgages (relative to other mortgages) of around 16 basis points.

In order to test whether this reduced risk-sensitivity in mortgage pricing translates into riskier mortgage portfolios, we estimate:

$$\begin{aligned} \text{Portfolio share}_{i,j,t} \quad = \quad & \beta \left(\text{Exposure}_i \times \% \left(\text{Post} \right)_{j,t} \right) \\ & + \gamma \left(\text{Exposure}_i \times \% \left(\text{Post} \right)_{j,t} \times \text{High LTV}_j \right) + \text{Controls}_{i,j,t} + \varepsilon_{i,j,t}, \end{aligned} \tag{8}$$

where Portfolio $\text{share}_{i,j,t}$ is bank i's mortgage lending volume in product j (defined by the combination of maturity quarter and LTV bucket) in quarter t, divided by bank i's total mortgage lending volume in quarter t. $\text{Controls}_{i,j,t}$ includes product-quarter fixed effects; bank-product fixed effects; and the interaction of the five bank-level controls used in equation (2) with $\%(\text{Post})_{j,t}$. The sample again consists only of banks not subject to ring-fencing requirements.

The results for this regression are reported in Table 10. As for the market share regressions in Table 4, we consider both unweighted and weighted estimators. Since equation (8) is estimated at the bank-product level, and the dependent variable is normalised by the bank's total lending volume, the unweighted estimators do not fully reflect differences in the relative economic importance of different banks. In contrast, weighting observations by the bank's total lending volume in the quarter allows us to put more weight on banks that account for a larger share of loans.

When we estimate the model using OLS or unweighted IV (columns 1–3), the estimate of γ is positive but insignificant. When we instead use weighted estimators (columns 4 and 5), the estimate of γ is positive and statistically significant, indicating that more exposed competitors increase the share of riskier mortgages in their portfolios.

The finding that smaller banks shift towards higher-risk mortgages in response to ring-fencing is consistent with industry reports (Bloomberg, 2019; Financial Times, 2019b). It is also consistent with the model of Keeley (1990): competitive pressure from ring-fencing reduces the franchise value of smaller banks, which incentivises increased risk-taking.

6 Ring-fencing and syndicated lending

The results in Section 4 establish that an increase in the deposit funding of the RFB as a result of ring-fencing is associated with an expansion of domestic mortgage lending. We now investigate how the loss of deposit funding in the NRFB affects syndicated lending, which is a key source of credit for large corporates. As discussed in Section 1, ring-fencing effectively forces banks to place syndicated lending in the NRFB if they wish to serve large corporate customers from the same side of the fence and preserve informational synergies between corporate lending and securities underwriting (Drucker and Puri, 2005; Yasuda, 2005; Neuhann and Saidi, 2018).

One caveat is that unlike UK mortgages, a substantial share of syndicated loans are sold by the originator, and so might be less affected by the change in funding structure caused by ring-fencing. We mitigate this issue by exploring characteristics that are known to correlate with the propensity for the loan to be retained.

Specification In a typical syndicated loan, the borrower takes out a "package" that includes several individual loan "facilities" (principally term loans and credit lines). Importantly, facilities are extended by multiple lenders to the same borrower. This allows us to use borrower-time fixed effects to control for unobserved borrower characteristics, including credit demand, in line with Khwaja and Mian (2008). We estimate the following regression:

$$\text{Log(Loan size)}_{i,l,t} = \beta \left(\Delta\text{Retail funding}_i^{\text{NRFB}} \times \%(\text{Post})_{l,t}\right) + \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \tag{9}$$

where $\text{Log(Loan size)}_{i,l,t}$ is the log of the amount of credit extended by bank i in loan facility l during month t.[24] $\Delta\text{Retail funding}_i^{\text{NRFB}}$ is the amount by which bank i's retail funding share (for NRFB assets) decreases upon implementation of ring-fencing. As in our mortgage regressions, we instrument $\Delta\text{Retail funding}_i^{\text{NRFB}}$ using variables determined in

[24] We sum over loan facilities of the same type and with the same maturity in the same package.

2011 (see Section 2.3 for details). $\%(\text{Post})_{l,t}$ is the proportion of the loan maturity period that falls after January 2019. The sample consists of global syndicated loan originations over 2010–2018.

As in our mortgage regressions, the set of control variables includes bank-month fixed effects to control for confounding supply-side factors. In addition, we include loan facility fixed effects to control for all observed and unobserved borrower and loan characteristics, including the borrower's credit demand.

The specification above only exploits variation in lending quantities conditional on participation in the loan (intensive margin). In addition, bank-level lending quantities are often unobserved in DealScan. We therefore complement the intensive margin analysis with an extensive margin analysis by estimating the regression:

$$\text{Log(Number loans)}_{i,j,t} = \beta \left(\Delta\text{Retail funding}_i^{\text{NRFB}} \times \%(\text{Post})_{j,t}\right) + \text{Controls}_{i,j,t} + \varepsilon_{i,j,t}, \tag{10}$$

where $\text{Log(Number loans)}_{i,j,t}$ is the log of the number of loans with maturity j in which bank i participates in quarter t. The vector of control variables includes bank-quarter fixed effects to control for confounding supply-side factors, and maturity-quarter fixed effects to control for trends in credit demand.

Results The results are reported in Table 11 (intensive margin) and Table 12 (extensive margin).[25] We find that a one-standard-deviation (11 percentage point) decrease in deposit funding is associated with a 6% decrease in loan size conditional on participation (Table 11, column 1) and a 15% reduction in the number of loans in which the bank participates (Table 12, column 1). In other words, universal banks respond to a loss of deposit funding by reducing their supply of syndicated loans.

We next decompose this effect across the main categories of syndicated loan. The coefficient of interest is negative and significant for both term and non-term loans (prin-

[25] These tables show instrumental variable results only. OLS results are very similar and reported in Appendix C.

cipally credit lines) (column 2); leveraged and non-leveraged loans (column 3);[26] and lead arranger and participant tranches (column 4). That is, the reduction in syndicated lending is robust across loan categories.

These findings are consistent with industry reports that ring-fencing has caused large UK banks to retrench from syndicated lending (Reuters, 2017). They are also consistent with existing evidence about the benefits of stable deposit funding for the provision of syndicated credit (Irani and Meisenzahl, 2017; Paligorova and Santos, 2017) and liquidity insurance via credit lines (Kashyap et al., 2002; Gatev and Strahan, 2009; Gatev et al., 2009). As with our results for mortgages, the inclusion of bank-time fixed effects rules out a wide range of potential confounding factors and exclusion restriction violations, including those whose impact could vary across banks and time, such as the 2016 Brexit referendum.

If the negative relationship between ring-fencing and credit supply is driven by the change in funding structure, then we would expect it to be stronger for loans that are more likely to be retained, and hence funded to maturity by the originator. While DealScan does not record whether a loan is retained or sold, we exploit the fact that term loans and leveraged loans are more likely to be sold (Ivashina and Sun, 2011; Blickle et al., 2020). Consistent with our prior, we find that the effect is substantially larger for non-term loans (column 2) and non-leveraged loans (column 3), for both the intensive and extensive margins. The statistics reported on the last row confirm that these differences are statistically significant (with the exception of non-leveraged loans for the intensive margin).

Incidentally, the larger reduction in lending for non-leveraged loans (i.e. lower-risk loans) provides little support for the idea that removing risk-insensitive deposit funding from the NRFB leads to reduced risk-taking in syndicated lending. This is consistent with our finding that the increased deposit funding of the RFB does not disproportionately

[26] Following Bruche et al. (2020) and Standard & Poor's, we define a loan as leveraged if it is secured and has a spread of 125 basis points or higher.

benefit riskier mortgages (Section 4.3).

An important question for assessing the overall impact of ring-fencing on UK credit supply is whether the reduction in syndicated lending is focused on loans to UK or foreign borrowers. The results in column 5 of Tables 11 and 12 indicate that the reduction in lending is larger for foreign borrowers than for UK borrowers (although we do observe a statistically significant negative effect for both groups on the extensive margin). This is consistent with the idea that UK banks are better able to extract surplus from UK borrowers due to informational advantages, and with existing evidence on home bias in global syndicated lending markets (Carey and Nini, 2007).

7 Discussion and conclusions

In recent years, a range of Glass-Steagall-type proposals have been debated in the US and other advanced economies. While these proposals primarily aim at insulating retail customers and taxpayers from potential investment banking losses *in crisis times*, they also imply a fundamental change to the funding structure of a wide range of universal banking activities *in normal times*. In this paper, we show that, as a result, structural separation involves a range of previously undocumented side effects for credit supply, competition, and risk-taking in important credit markets that are not directly targeted by the reform.

We study a recent UK reform that prevents large banking groups from operating as fully integrated universal banks, as is allowed in Europe and Canada. Instead the reform forces these banks to split their retail deposit-taking and investment banking activities into separate subsidiaries, similarly to US Bank Holding Companies. We show that this "ring-fencing" leads to a large, opposite shock to the funding available for activities on either side of the fence, as retail deposits are restricted to funding activities in the retail subsidiary. In response, universal banks rebalance their activities towards domestic retail lending. This rebalancing lowers the price and increases the quantity of domestic mortgages, but also increases concentration in the mortgage market. Smaller banks out of scope of the reform respond to the increased competitive pressure by increasing the riskiness of their mortgage lending. And on the other side of the fence, the decrease in deposit funding is associated with a reduction in the provision of credit and liquidity insurance to large corporates.

By documenting the role of deposit funding for universal banking, and by studying both retail and corporate lending, our findings highlight several novel policy implications of structural separation.

First, by redirecting the benefits of deposit funding to retail credit markets, separating deposit-taking from investment banking can reduce the cost of credit for consumers. The cheaper credit is not concentrated in the higher-risk segment of the mortgage market, limiting financial stability concerns related to rising household indebtedness (Mian et al., 2013). The expansion of consumer credit is mirrored by a reduction in credit supply to large corporates. The net welfare effects of this rebalancing are uncertain, and likely to depend on broader macroeconomic conditions and on corporates' ability to switch lenders. However, we note that the reduction in corporate credit supply is mainly focused on lending to foreign borrowers, who are less likely to be reliant on relationships with domestic banks.

Second, however, our results suggest more ambiguous implications for competition in the retail credit market over the longer term. By reducing the ability of smaller banks to compete, structural separation leads to more concentrated markets. Indeed, ring-fencing is reported to have already contributed to the exit of smaller lenders from the UK mortgage market (Financial Times, 2019a). The increased market power of large banks could lead to more expensive credit and reduced quality of service over the longer term; alternatively, increased concentration might simply reflect less efficient banks leaving the market. The increased market share of large banks in the retail credit market also casts doubt on the idea that structural separation significantly reduces the risk that universal banks become too-big-to-fail (King, 2009; Hoenig, 2017; Warren, 2017).

Our results also question the idea that structural separation unambiguously improves financial stability. On the one hand, the increased retail focus by universal banks should reduce their exposure to international and capital market shocks—a key motive of most structural separation proposals. On the other hand, this rebalancing leaves the supply of retail credit more exposed to the health of a few large lenders with increased exposures to domestic shocks. And by indirectly encouraging smaller banks to take more risk, structural separation might increase these smaller banks' vulnerability to shocks, and hence reduce their ability to continue lending during an economic downturn.

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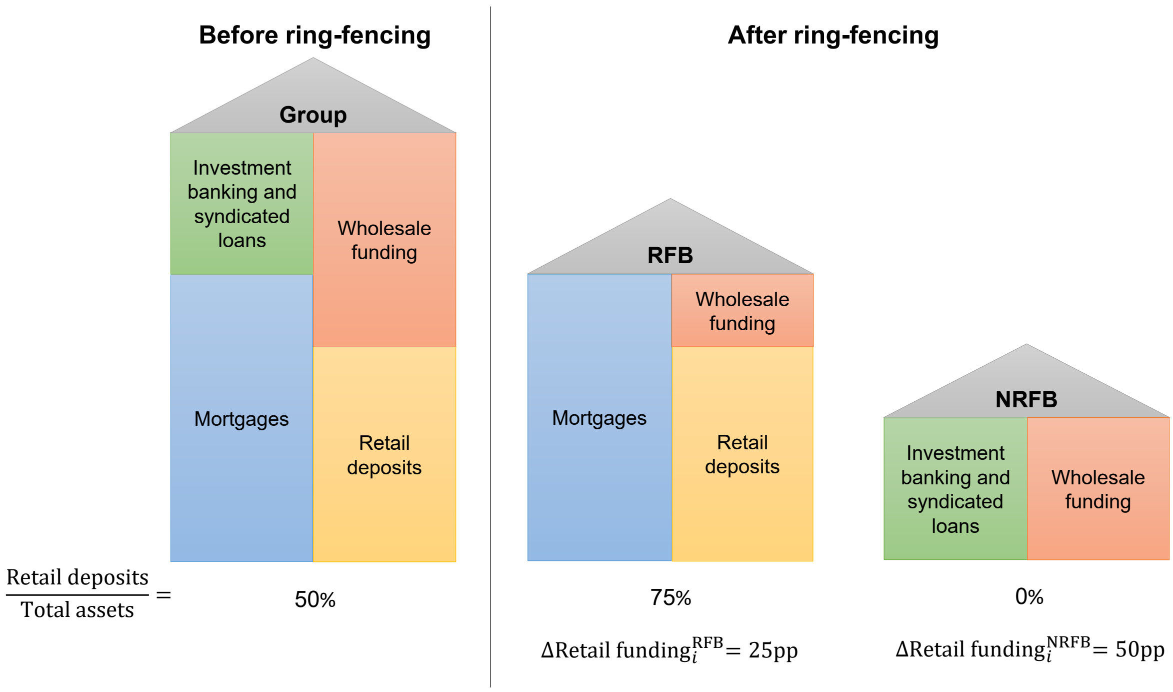
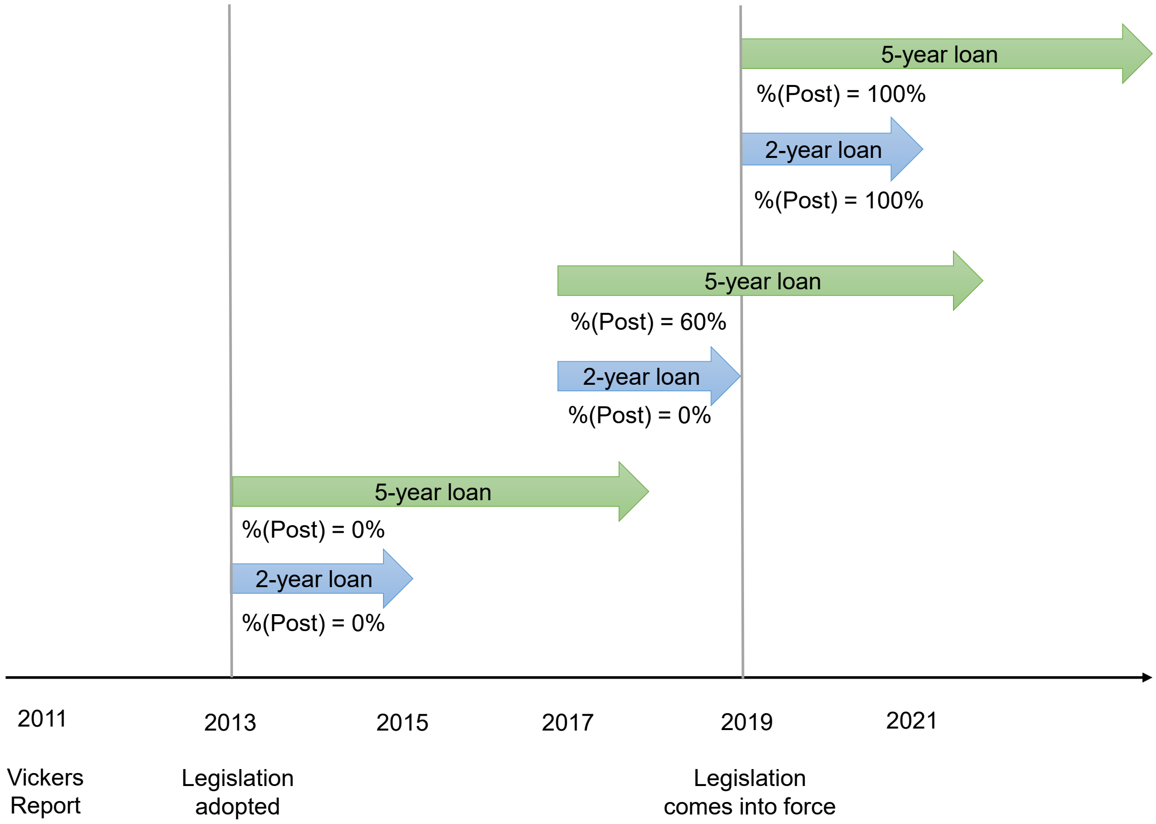
Figure 1: Impact of ring-fencing on funding structure (ΔRetail funding$_i$)



Notes: The figure illustrates the impact of ring-fencing on the funding structure of a stylised banking group. The left panel shows the banking group before ring-fencing. All assets and liabilities are held in the same legal entity. The right panel shows the banking group after ring-fencing, when the group has restructured into two separate subsidiaries. Retail deposits and mortgage lending are housed in the ring-fenced bank (RFB), while investment banking and syndicated lending are housed in the non-ring-fenced bank (NRFB), which must be entirely funded by wholesale funding. We exclude equity for simplicity. ΔRetail funding$_i$ measures the extent to which the retail funding ratio of the RFB (NRFB) increases (decreases) as a result of ring-fencing.

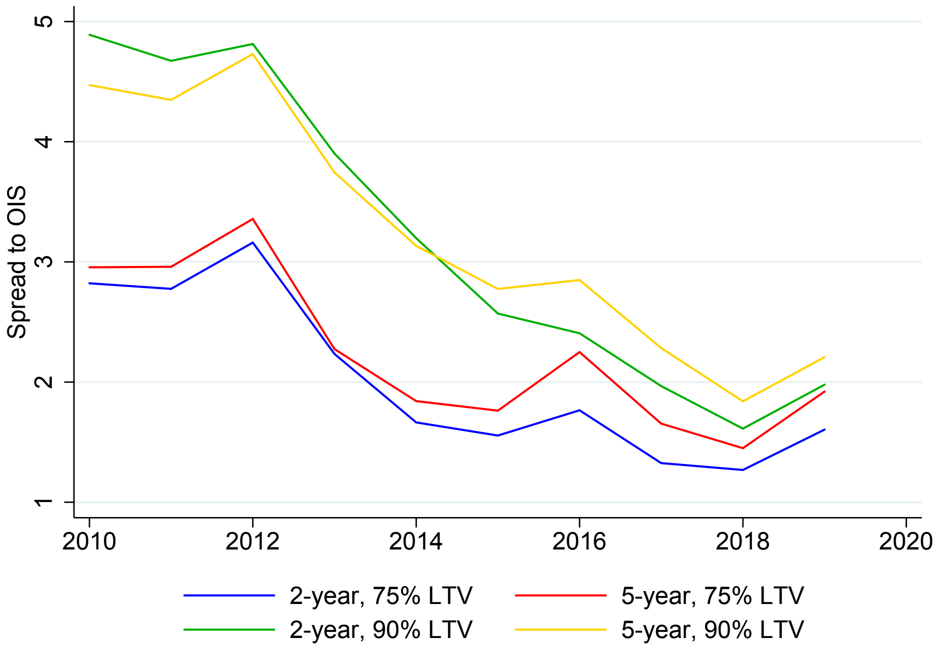
Figure 2: Impact of ring-fencing across time and loans (%(Post))



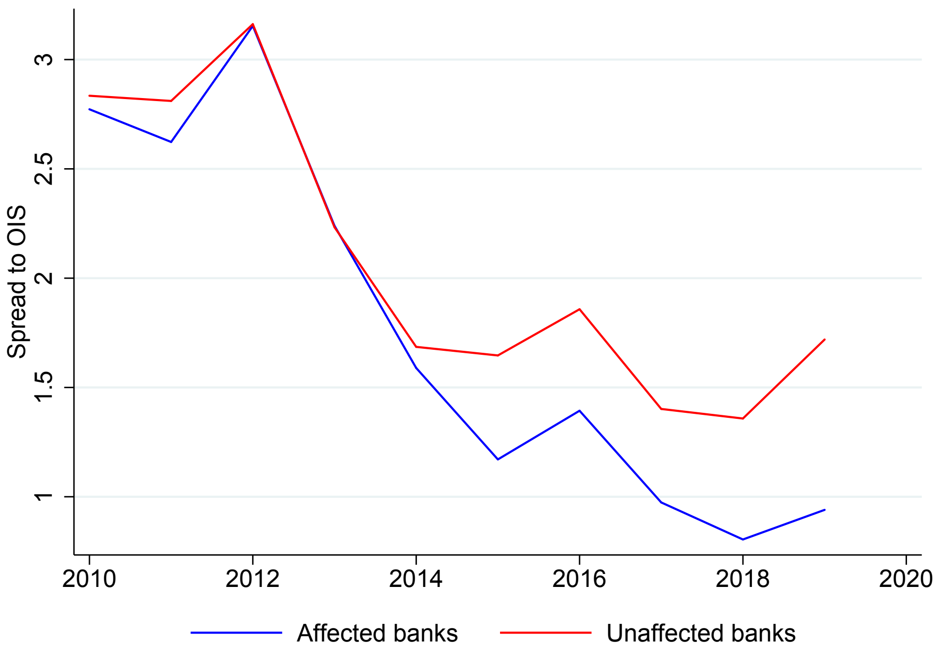
Notes: The figure illustrates how the impact of ring-fencing on funding structure varies across loans with different maturities and origination dates. Each arrow represents a loan, extending from its origination date to its maturity date. The label inside the arrow corresponds to the loan's maturity (fixation period for mortgages). The label below the arrow shows the corresponding value of %(Post), defined as the proportion of the loan's maturity that falls after January 2019, when ring-fencing requirements come into effect.

Figure 3: Average quoted mortgage spreads

(A) Average spreads by product



(B) Average spreads for 2-year, 75% LTV mortgages



Notes: The charts show quoted mortgage spreads (averages across lenders). Panel A shows spreads for several major mortgage products, averaged across all lenders. Panel B shows spreads for 2-year, 75% LTV mortgages, averaged across banks subject to ring-fencing (blue line) and other lenders (red line). Annual frequency.
Sources: Moneyfacts, Bank of England.

Table 1: Summary statistics for mortgage regressions

	Observations	Mean	Std Dev	p25	p50	p75
Dependent variables						
Interest rate spread (percent)	4,588,093	1.87	0.97	1.17	1.62	2.36
Market share	242,347	0.015	0.060	0	0	0
Herfindahl-Hirschman Index (HHI)	590,587	0.659	0.327	0.347	0.587	1.000
Portfolio share (competitor banks)	37,819	0.029	0.077	0.001	0.006	0.027
Measures of exposure to ring-fencing						
ΔRetail funding$_i^{\text{RFB}}$ (treated banks)	5	0.182	0.216	-	-	-
District exposure$_g$	390	0.114	0.035	0.092	0.118	0.137
Competitor exposure$_i$	58	0.103	0.033	0.092	0.110	0.121
%(Post)	4,588,093	0.370	0.388	0.000	0.250	0.760
Loan-level controls						
Maturity (months)	4,588,093	38.6	19.8	24	26	60
Loan-to-value ratio (LTV)	4,588,093	66.4	21.5	52.5	72.3	85.0
Loan-to-income ratio (LTI)	4,577,450	3.09	1.07	2.31	3.15	3.95
Mortgage term (months)	4,588,093	273	106	204	300	360
Log(Loan value)	4,588,093	11.8	0.7	11.4	11.9	12.3
Borrower age (years)	4,588,093	38.4	9.9	31	37	45
First-time buyer indicator	4,588,093	0.275	0.446	0	0	1
Home mover indicator	4,588,093	0.342	0.475	0	0	1
Council buyer indicator	4,588,093	0.010	0.101	0	0	0
Impaired credit history indicator	4,588,093	0.004	0.066	0	0	0
Brokered loan indicator	4,588,093	0.701	0.458	0	1	1
Long maturity indicator	4,588,093	0.435	0.496	0	0	1
High LTV indicator	4,588,093	0.076	0.265	0	0	0
Bank-level controls						
Log(Total assets)	4,545,940	12.6	1.5	12.3	13.3	13.5
Return on assets	4,545,782	0.004	0.005	0.002	0.004	0.007
Cash / Total assets	4,545,940	0.078	0.031	0.056	0.078	0.096
Capital / Risk-weighted assets	4,546,590	0.187	0.066	0.146	0.166	0.205
Wholesale funding / Total assets	4,545,940	0.272	0.120	0.187	0.268	0.341

Notes: The table shows summary statistics for the variables used in the mortgage regressions.

Table 2: Summary statistics for syndicated lending regressions

	Observations	Mean	Std Dev	p25	p50	p75
Dependent variables						
Log(Loan size)	155,915	2.11	2.57	0.92	2.87	3.91
Log(Number loans)	1,168,600	0.119	0.428	0	0	0
Measures of exposure to ring-fencing						
ΔRetail funding$_i^{NRFB}$ (treated banks)	5	0.454	0.107	-	-	-
%(Post)	155,915	0.168	0.267	0.000	0.000	0.310
Facility-level variables						
Term loan indicator	155,915	0.513	0.500	0	1	1
Leveraged loan indicator	155,915	0.265	0.441	0	0	1
Lead arranger indicator	155,915	0.535	0.499	0	1	1
UK borrower indicator	155,915	0.030	0.171	0	0	0

Notes: The table shows summary statistics for the variables used in the syndicated lending regressions.

Table 3: Effect of ring-fencing on mortgage spreads

Dependent variable:	Interest rate spread$_{i,l,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	IV	IV	IV	IV
ΔRetail funding$_i^{\text{RFB}}$ × %(Post)$_{l,t}$	-0.461***	-0.476***	-1.128***	-0.955***	-0.938***
	(0.157)	(0.162)	(0.236)	(0.184)	(0.184)
Loan-level controls	No	No	No	Yes	Yes
Bank-level controls	No	No	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	No	No	No	Yes
Observations	4,570,771	4,570,771	4,528,616	4,518,056	4,324,803
R^2	0.824	-	-	-	-
Kleibergen-Paap F-statistic	-	29.7	43.3	43.3	46.0

Notes: The table shows loan-level regression results for equation (2). i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. ΔRetail funding$_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. In columns 2–5, ΔRetail funding$_i^{\text{RFB}}$ is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). The sample period is January 2010 to June 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 4: Effect of ring-fencing on mortgage product market shares

Dependent variable:	Market share$_{i,j,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	IV	IV	WLS	W2SLS
ΔRetail funding$_i^{\text{RFB}}$ × %(Post)$_{j,t}$	0.149***	0.167***	0.249***	0.133**	0.168*
	(0.043)	(0.041)	(0.053)	(0.061)	(0.088)
Bank-level controls	No	No	Yes	Yes	Yes
Bank × Quarter fixed effects	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	241,009	241,009	204,086	204,086	204,086
R^2	0.721	-	-	0.901	-
Kleibergen-Paap F-statistic	-	18.1	20.4	-	19.1

Notes: The table shows bank-product-quarter-level regression results for equation (3). i indexes banks, j indexes products (defined by the combination of maturity quarter and LTV bucket), and t indexes origination quarters. The dependent variable is bank i's market share for product j in quarter t. ΔRetail funding$_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing becomes binding. Bank-level controls are interactions between %(Post)$_{j,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. In columns 2, 3, and 5, ΔRetail funding$_i^{\text{RFB}}$ is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). In columns 4 and 5, observations are weighted by total lending in product j and quarter t (summed across all banks). The sample period is 2015:Q1 to 2019:Q2. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 5: Effect of ring-fencing on mortgage spreads – by maturity and risk

Dependent variable:	Interest rate spread$_{i,l,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{l,t}$	-0.848***	-0.962***	-0.966***	-0.861***	-0.971***	-0.952***
	(0.138)	(0.199)	(0.187)	(0.137)	(0.187)	(0.189)
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Long maturity$_l$	-0.297**	-0.287**	-0.297**			
	(0.122)	(0.132)	(0.129)			
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ High LTV$_l$				0.026	0.189	0.155
				(0.117)	(0.235)	(0.235)
Loan-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank $\times$ Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Product $\times$ Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank $\times$ Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location $\times$ Month fixed effects	No	No	Yes	No	No	Yes
Observations	4,518,056	4,518,056	4,324,803	4,518,056	4,518,056	4,324,803
R^2	0.846	-	-	0.846	-	-
Kleibergen-Paap F-statistic	-	19.3	20.5	-	21.7	23.2

Notes: The table shows loan-level regression results for equation (2), with additional interaction terms. i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. ΔRetail funding$_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Long maturity$_l$ is equal to one for mortgages with maturity greater than two years, and zero otherwise. High LTV$_l$ is equal to one for mortgages with loan-to-value ratio greater than 90%, and zero otherwise. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. In columns 2, 3, 5, and 6, ΔRetail funding$_i^{\text{RFB}}$ is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). The sample period is January 2010 to June 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 6: Effect of ring-fencing on mortgage product market shares – by maturity and risk

Dependent variable:	Market share$_{i,j,t}$			
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{j,t}$	0.178***	0.200***	0.223***	0.265***
	(0.026)	(0.044)	(0.044)	(0.067)
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Long maturity$_j$	0.137**	0.175**		
	(0.057)	(0.071)		
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ High LTV$_j$			-0.055	-0.117
			(0.103)	(0.131)
Bank-level controls	Yes	Yes	Yes	Yes
Bank $\times$ Quarter fixed effects	Yes	Yes	Yes	Yes
Bank $\times$ Product fixed effects	Yes	Yes	Yes	Yes
Observations	204,086	204,086	204,086	204,086
R^2	0.721	-	0.721	-
Kleibergen-Paap F-statistic	-	10.9	-	10.5

Notes: The table shows bank-product-quarter-level regression results for equation (3), with additional interaction terms. i indexes banks, j indexes products (defined by the combination of maturity quarter and LTV bucket), and t indexes origination quarters. The dependent variable is bank i's market share for product j in quarter t. ΔRetail funding$_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing becomes binding. Long maturity$_j$ is equal to one for products with maturity greater than two years, and zero otherwise. High LTV$_j$ is equal to one for products with loan-to-value ratio greater than 90%, and zero otherwise. Bank-level controls are interactions between %(Post)$_{j,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. In columns 2 and 4, ΔRetail funding$_i^{\text{RFB}}$ is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). The sample period is 2015:Q1 to 2019:Q2. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 7: Persistence of local mortgage market shares over time

Dependent variable:	2018 market share$_{i,g}$		
	(1)	(2)	(3)
	All loans	Non-brokered loans	Brokered loans
2011 market share$_{i,g}$	0.424***	0.470***	0.199**
	(0.067)	(0.058)	(0.089)
Bank fixed effects	Yes	Yes	Yes
Observations	38,318	38,318	38,318
R^2	0.938	0.912	0.918

Notes: The table shows results from bank-district-level regressions of 2018 market shares on 2011 market shares. i indexes banks and g indexes districts. Market share is defined as bank i's mortgage origination volume in district g divided by total mortgage origination volume in district g. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 8: Effect of ring-fencing on mortgage product market concentration

Dependent variable:	$\text{HHI}_{g,j,t}$			
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
$\text{Exposure}_g \times \% \, (\text{Post})_{j,t}$	0.379***	0.303***	0.401***	0.300***
	(0.037)	(0.080)	(0.038)	(0.083)
Product × Quarter fixed effects	Yes	Yes	Yes	Yes
District × Product fixed effects	Yes	Yes	Yes	Yes
District × Quarter fixed effects	No	Yes	No	Yes
Observations	573,933	573,933	573,933	573,933
R^2	0.700	0.701	-	-
Kleibergen-Paap F-statistic	-	-	4,063.5	4,077.3

Notes: The table shows market-quarter-level regression results for equation (5). g indexes districts, j indexes mortgage products (defined by the combination of maturity quarter and LTV bucket), and t indexes origination quarters. The dependent variable is the Herfindahl-Hirschman Index (HHI) for product j in district g in quarter t. Exposure_g is district g's exposure to ring-fencing, defined as the volume-weighted average of $\Delta\text{Retail funding}_i^{\text{RFB}}$ across banks active in the district in 2011 (see equation (4)). $\%(\text{Post})_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing becomes binding. In columns 3 and 4, Exposure_g is instrumented by volume-weighted averages of the two instruments for $\Delta\text{Retail funding}_i^{\text{RFB}}$ discussed in Section 2.3. The sample period is 2015:Q1 to 2019:Q2. Standard errors are reported in parentheses and clustered by district. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 9: Effect of ring-fencing on mortgage spreads for banks not subject to ring-fencing

Dependent variable:	Interest rate spread$_{i,l,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	IV	IV	IV
Exposure$_i$ × %(Post)$_{l,t}$	2.209	2.665	1.074	1.603	3.001	3.384*
	(1.844)	(1.837)	(1.871)	(1.908)	(1.906)	(1.895)
Exposure$_i$ × %(Post)$_{l,t}$ × Long maturity$_l$			-1.680***	-1.520**		
			(0.593)	(0.739)		
Exposure$_i$ × %(Post)$_{l,t}$ × High LTV$_l$					-4.714***	-4.886***
					(1.089)	(1.572)
Loan-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	Yes	No	Yes	No	Yes
Observations	1,376,607	1,129,671	1,376,607	1,129,671	1,376,607	1,129,671
Kleibergen-Paap F-statistic	2,223.1	2,348.9	624.7	517.4	1,114.1	1,318.1

Notes: The table shows loan-level instrumental variable regression results for equation (7), with additional interaction terms. The sample consists only of banks not subject to ring-fencing requirements. i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. Exposure$_i$ is bank i's exposure to the effects of ring-fencing, based on its mortgage lending portfolio in 2011 (see equation (6)). This is instrumented by equivalent weighted averages based on the two instruments for ΔRetail funding$_i^{\text{RFB}}$ discussed in Section 2.3. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Long maturity$_l$ is equal to one for mortgages with maturity greater than two years, and zero otherwise. High LTV$_l$ is equal to one for mortgages with loan-to-value ratio greater than 90%, and zero otherwise. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. The sample period is January 2010 to June 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Corresponding OLS regressions are shown in Appendix C.

Table 10: Effect of ring-fencing on mortgage portfolio shares for banks not subject to ring-fencing

Dependent variable:	Portfolio $\text{share}_{i,j,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	IV	IV	WLS	W2SLS
$\text{Exposure}_i \times \%(\text{Post})_{j,t}$	-0.037	-0.032	-0.092	-0.046	-0.039
	(0.117)	(0.101)	(0.116)	(0.030)	(0.026)
$\text{Exposure}_i \times \%(\text{Post})_{j,t} \times \text{High LTV}_j$	0.227	0.119	0.179	0.173***	0.166***
	(0.162)	(0.131)	(0.128)	(0.062)	(0.053)
Bank-level controls	No	No	Yes	Yes	Yes
Product × Quarter fixed effects	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	30,756	30,756	30,592	30,592	30,592
R^2	0.691	-	-	0.858	-
Kleibergen-Paap F-statistic	-	745.3	501.9	-	3,389.5

Notes: The table shows bank-product-quarter-level regression results for equation (8). The sample consists only of banks not subject to ring-fencing requirements. i indexes banks, j indexes products (defined by the combination of maturity quarter and LTV bucket), and t indexes origination quarters. The dependent variable is the share of product j in bank i's mortgage lending portfolio in quarter t. Exposure_i is bank i's exposure to the effects of ring-fencing, based on its mortgage lending portfolio in 2011 (see equation (6)). $\%(\text{Post})_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing becomes binding. High LTV_j is equal to one for products with loan-to-value ratio greater than 90%, and zero otherwise. Bank-level controls are interactions between $\%(\text{Post})_{j,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. In columns 2, 3, and 5, Exposure_i is instrumented by equivalent weighted averages of the two instruments for $\Delta\text{Retail funding}_i^{\text{RFB}}$ discussed in Section 2.3. In columns 4 and 5, observations are weighted by bank i's total mortgage lending in quarter t (summed across all products). The sample period is 2015:Q1 to 2019:Q2. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 11: Effect of ring-fencing on syndicated lending – Intensive margin

Dependent variable:	Log(Loan size)$_{i,l,t}$				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$	-0.542***				
	(0.202)				
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Term loan$_l$		-0.378**			
		(0.179)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Non-term loan$_l$		-0.604***			
		(0.187)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Leveraged loan$_l$			-0.417**		
			(0.189)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Non-leveraged loan$_l$			-0.580**		
			(0.240)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Lead arranger$_{i,l}$				-0.440**	
				(0.174)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Participant$_{i,l}$				-0.734***	
				(0.207)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ UK borrower$_l$					-0.187
					(0.208)
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Foreign borrower$_l$					-0.634***
					(0.228)
Difference between coefficients		-0.225*	-0.163	-0.294**	-0.447**
		(0.115)	(0.235)	(0.132)	(0.200)
Bank $\times$ Month fixed effects	Yes	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes	Yes
Bank $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Observations	139,779	139,157	139,602	139,653	139,710
Kleibergen-Paap F-statistic	2,960.5	10,279.1	4,244.5	1,331.1	3,820.7

Notes: The table shows instrumental variable regression results for equation (9). i indexes banks, l indexes loan facilities, and t indexes origination months. The dependent variable is the log of the amount of credit extended by bank i in loan facility l in month t. ΔRetail funding$_i^{\text{NRFB}}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing. This is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Bank $\times$ loan-category fixed effects are the interaction between bank indicator variables and indicator variables for: term loans (column 2), leveraged loans (column 3), lead arranger tranches (column 4), and UK borrowers (column 5). A loan is defined as leveraged if it is secured and has a spread of 125bp or higher. "Difference between coefficients" shows the difference between the parameter estimates in the column, with standard errors in parentheses. The sample period is January 2010 to March 2018. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Corresponding OLS regressions are shown in Appendix C.

Table 12: Effect of ring-fencing on syndicated lending – Extensive margin

Dependent variable:	Log(Number loans)$_{i,j,c,t}$				
	(1)	(2)	(3)	(4)	(5)
	IV	IV	IV	IV	IV
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$	-1.352***				
	(0.470)				
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Term loan$_c$		-0.713***			
		(0.265)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Non-term loan$_c$		-1.297***			
		(0.445)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Leveraged loan$_c$			-0.481**		
			(0.201)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Non-leveraged loan$_c$			-1.423***		
			(0.456)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Lead arranger$_c$				-1.030***	
				(0.337)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Participant$_c$				-1.126**	
				(0.466)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ UK borrower$_c$					-0.782***
					(0.274)
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Foreign borrower$_c$					-1.134***
					(0.355)
Difference between coefficients		-0.584***	-0.942***	-0.096	-0.352**
		(0.202)	(0.288)	(0.210)	(0.153)
Bank $\times$ Quarter fixed effects	Yes	Yes	Yes	Yes	Yes
Maturity $\times$ Quarter fixed effects	Yes	-	-	-	-
Maturity $\times$ Quarter $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Bank $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Observations	1,168,600	2,337,200	2,337,200	2,337,200	2,337,200
Kleibergen-Paap F-statistic	3,403.9	39.0	39.0	39.0	39.0

Notes: The table shows instrumental variable regression results for equation (10). i indexes banks, j indexes loan maturities (measured in quarters), c indexes loan categories, and t indexes origination quarters. The dependent variable is the log of the number of loans with maturity j in category c in which bank i participates in quarter t. ΔRetail funding$_i^{\text{NRFB}}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing. This is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). %(Post)$_{j,t}$ is the proportion of the loan maturity that falls after January 2019, when ring-fencing becomes binding. Loan categories are: term / non-term (column 2), leveraged / non-leveraged (column 3), lead arranger / participant (column 4), UK borrower / foreign borrower (column 5). A loan is defined as leveraged if it is secured and has a spread of 125bp or higher. "Difference between coefficients" shows the difference between the parameter estimates in the column, with standard errors in parentheses. The sample period is 2010:Q1 to 2018:Q1. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Corresponding OLS regressions are shown in Appendix C.

INTERNET APPENDIX

A Comparison with other policies

The UK ring-fencing legislation lies between two opposite approaches to regulating the relationship between deposit-taking and investment banking: full separation (for instance under narrow banking or the original 1933 Glass-Stegall Act), and full integration (for instance under the universal banking model prevalent in Europe and Canada).

Glass-Steagall prohibited commercial banks—that is, entities issuing insured deposits—from engaging in a range of investment banking activities such as securities underwriting and trading, as well as from affiliating with entities engaged in those activities.

In line with this "separation" approach, the UK ring-fence seeks to separate retail and investment banking into financially and operationally self-sufficient entities. But similarly to the "integration" approach, these entities are still allowed to belong to the same group. However, the ring-fenced subsidiary's exposures to the non-ring-fenced subsidiary are limited quantitatively via large exposures regulation, as well as through the requirement that these exposures must be treated as arm's length transactions.

This "middle ground" approach makes the UK ring-fencing close to the "structured universal banking" approach that now characterises US regulation (Vickers, 2014). Glass-Steagall provisions were gradually relaxed from the 1960s. From 1986, commercial banks were allowed to affiliate with a securities firm under the umbrella of a bank holding company. However, these companies' bank and nonbank activities must remain in separate and financially self-sufficient subsidiaries. In addition, cross-exposures between bank and nonbank entities remained limited via Sections 23A and 23B of the Federal Reserve Act and Regulation W. This remained the case when the 1999 Gramm-Leach-Bliley Act further relaxed Glass-Steagall provisions by allowing deposit-takers, securities firms, and insurance firms to affiliate under a Financial Holding Company.

The Dodd-Frank Act amends the pre-crisis structured universal banking model by forbidding banks and their affiliates from engaging in proprietary trading and from sponsoring hedge funds and private-equity funds (Volcker Rule), as well as by extending the reach of Section 23A regulation. This limits deposit-takers' ability to support nonbank affiliates' tri-party repo market activities, which was allowed during the crisis.

Despite sharing a common middle ground approach, UK ring-fencing differs from the US post-crisis structured universal banking in several dimensions. First, the scope of activities prohibited under the two regulatory regimes differs. The Volcker Rule fully bans deposit-taking entities from engaging in (or affiliating with entities engaged in) proprietary trading, whereas ring-fencing allows deposit-taking and proprietary trading to be performed by different subsidiaries in the same group. On the other hand, the UK ring-fence prohibits the deposit-taking subsidiary from performing a wider range of investment-banking activities (including underwriting and market-making). Second, under UK ring-fencing, the bank and nonbank subsidiaries must have separate management boards, providing further operational independence.

These differences might partly explain recent calls to implement a UK-style ring-fence in the US. For instance, the bipartisan "21st Century Glass-Steagall" bill sponsored in 2017 by Elizabeth Warren and John McCain (among others) aimed to limit depository institutions' ability to engage in a range of investment banking activities extending well beyond proprietary trading. Proposals suggesting a stricter separation between commercial and investment banking were also submitted as part of debates around the Dodd-Frank Act, without success (for example the 2009 "Banking Integrity Act"). Inspired in part by the UK ring-fence, FDIC Vice Chairman Thomas Hoenig (2017) proposes a "partition" of depository and investment banks into separately capitalised intermediate holding companies with separate management and boards. Treasury Secretary Mnuchin said that such plans were "one of the things we could consider" (Financial Times, 2017), which some observers took to mean that he was looking for a "British makeover for US banks" (Wall Street Journal, 2017).

Since the global financial crisis, legislators in other jurisdictions have also proposed structural reforms aimed at better insulating retail banking from investment banking (see Financial Stability Board (2014) for a fuller discussion). Most of these plans, including the recommendations from the EU's Liikanen (2012) report, followed a similar "middle ground" approach and did not require a full separation.

B Threats to identification: Further discussion

Our key finding is that the impact of ring-fencing on funding structure leads affected banks to increase mortgage lending. Our preferred interpretation is that this result is driven by a *deposit funding channel*: redirecting the benefits of deposit funding towards RFB activities makes these activities more attractive.

We can rule out a range of other potential explanations for our results because our granular data and identification strategy allow us to exploit variation in the strength of this channel both across banks (ΔRetail funding$_i$) and within banks ($\%(\text{Post})_{l,t}$). This allows us to use bank-time fixed effects to control for a wide range of confounding developments that could coincide with the run-up to ring-fencing, even those whose impact might differ across banks and time.

For an alternative channel to explain our results, it would therefore need to meet three criteria: (i) it would need to vary across banks in a way that is correlated with ΔRetail funding$_i$; (ii) it would need to vary across loan maturities and time in a way that is correlated with $\%(\text{Post})_{l,t}$; and (iii) it should not be controlled for by other regressors. For this alternative channel to also lead to a violation of the exclusion restriction in our IV regressions, it would additionally need to be correlated with our two instruments for ΔRetail funding$_i$.

As discussed in Section 2.4, several developments might have affected bank lending behaviour in the run-up to ring-fencing, including the 2016 Brexit referendum and other regulatory developments such as changes in capital requirements. However it is unclear that these would meet all three of the criteria above. For example, is not clear why any impact from the Brexit referendum would vary with a loan's maturity and distance between origination and January 2019 (criterion ii).

In the remainder of this section we discuss a range of potential alternative explanations for our results in more detail.

Impact of ring-fencing on wholesale funding costs

The deposit funding channel emphasises the impact of ring-fencing on the availability of *deposit* funding. One alternative mechanism is that ring-fencing could incentivise an increase in retail lending by reducing the RFB's *wholesale* funding costs.

Ring-fencing might lead to a reduction in RFB wholesale funding costs for two reasons. First, authorities might consider the services provided by RFBs (such as payment services, deposit-taking, and lending to households and SMEs) to be more important for welfare than those provided by NRFBs. By making it easier to resolve or bail-out the RFB and NRFB separately, ring-fencing could therefore increase the perceived probability that RFB debtholders would be bailed out in the event of stress (because this would not imply a parallel bail-out of investment banking or foreign activities). Second, ring-fencing might reduce investors' perceptions of the riskiness of the RFB's assets and funding. These factors suggest that ring-fencing could cause an increase in retail lending via the impact on wholesale funding costs, rather than the deposit funding channel.

However, this impact could be offset by several countervailing factors. The RFB's wholesale funding costs might *increase* due to the reduction in diversification. Restructured groups might also decide to reallocate capital and liquidity across subsidiaries to offset the underlying change in risk profile. The increase in the RFB's share of insured deposits would also tend to reduce the position of the RFB's wholesale creditors in the creditor hierarchy.

These offsetting factors suggest that the impact of ring-fencing on relative riskiness is ultimately unclear. In line with this, while the major credit rating agencies generally rate RFBs higher than NRFBs, the difference is typically small (less than one notch, on average).

Nevertheless, to control for this channel, we compute the difference between the RFB credit rating and the group credit rating.[27] As for $\Delta\text{Retail funding}_i$, we measure this

[27] Rather than the observed group credit rating, we use the asset-weighted average of the RFB and NRFB credit ratings. This is because the observed group credit rating is also affected by the proportion of debt that is issued from the group rather than the subsidiaries, because for UK banks, debt issued

difference at the end of 2018, and interact it with $\%(\text{Post})_{l,t}$.[28] The results for equation (2) including this additional control variable are reported in Table B.1, columns 1 and 2. The coefficient on our main variable of interest ($\Delta\text{Retail funding}_i^{\text{RFB}} \times \%(\text{Post})_{l,t}$) remains statistically significant and of a similar magnitude to our baseline results.

Impact of ring-fencing on capital requirements

Another potential confounding factor is the impact of ring-fencing on capital requirements. UK banks are subject to two capital requirements: a risk-weighted capital requirement, and a leverage ratio requirement, which is designed to be risk-insensitive. On average, RFB assets (such as mortgages and SME loans) carry higher risk-weights than NRFB assets (such as reverse repo). This means that ring-fencing tends to make the *risk-weighted* capital requirement *more* binding for RFBs. Assuming that equity is more expensive than other forms of funding, this would predict a *reduction* in retail lending—the opposite of what we find.

However, for the same reason, ring-fencing tends to make the *leverage ratio* requirement less binding for RFBs. This channel could incentivise an increase in retail lending, and so could potentially explain our results. To control for this channel, we compute the difference between the RFB regulatory leverage ratio and the group regulatory leverage ratio, and interact this difference with $\%(\text{Post})_{l,t}$. Our main result is robust to including this additional control variable (Table B.1, columns 3 and 4).

Impact of ring-fencing on liquidity requirements

UK banks are also subject to liquidity regulation through the liquidity coverage ratio (LCR). The LCR requires banks to hold a sufficient stock of high-quality liquid assets (HQLA) to meet potential funding outflows in stress. Assets that qualify as HQLA typically have low returns and so are costly to hold. Since the LCR typically treats

from the group is junior to debt issued from the subsidiaries ("structural subordination").

[28] We cannot measure the difference earlier than end-2018 because the RFBs and NRFBs did not exist as distinct entities before this point.

retail deposits as more stable than wholesale funding, ring-fencing tends to decrease the HQLA requirement for RFBs, leading to reduced costs for RFBs. This would potentially incentivise an increase in RFB activities, in line with our results. To control for this channel, we interact $\%(\text{Post})_{l,t}$ with the difference between the RFB LCR and group LCR. Again, our main result is robust to including this additional control variable (Table B.1, columns 5 and 6).

Changes in LTV-specific capital requirements on mortgages

Our baseline set-up includes product-time fixed effects, where "product" corresponds to a given combination of loan-to-value (LTV) and maturity. Among other factors, this controls for any *sector-wide* changes over time in LTV-specific regulatory requirements. However, these fixed effects would not control for any changes in LTV-specific requirements that also vary across banks.

One potential concern is that during our sample period, there is significant heterogeneity in LTV-specific requirements between two groups of UK banks: those calculating risk weights using internal models ("IRB banks"), and those using the standardised approach set by regulators ("SA banks"). Specifically, IRB banks have lower average risk weights than SA banks, and this wedge is larger for low-LTV mortgages. As a result, IRB banks offer lower mortgage rates on low-LTV loans (Benetton, 2021).

To rule out this potential alternative channel, we repeat our main regression using only the sample of IRB banks (Table B.1, columns 7 and 8). Our key result is unchanged.

Size, diversification, and internal capital markets

By construction, ring-fencing makes banks smaller and less diversified, and places significant restrictions on their internal capital markets.

Existing literature proposes several mechanisms through which large, diversified conglomerates might differ from smaller, more specialised institutions. Among others, see Stein (1997); Campello (2002); Laeven and Levine (2007); Goetz et al. (2013, 2016). On the one hand, conglomerates might benefit from economies of scale and scope; diversification across products and geographies might mitigate idiosyncratic risks; and internal capital markets might better enable conglomerates to allocate funding to profitable investment opportunities. On the other hand, conglomerates might face more severe agency frictions; and there might be a "dark side of diversification", with internal capital markets leading to inefficient cross-subsidisation across business areas.

By forcing universal banks to split into smaller and less diversified subsidiaries, ring-fencing could affect lending through these mechanisms. However the reduction in size and diversification, and the restrictions on internal capital markets, would affect both the RFB and NRFB. So such mechanisms would be unable to explain our finding that RFB lending increases while NRFB lending decreases.

Table B.1: Effect of ring-fencing on mortgage spreads – Alternative channels

Dependent variable:	Interest rate spread$_{i,l,t}$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
ΔRetail funding$_i^{RFB}$ × %(Post)$_{l,t}$	-0.746***	-0.719***	-0.981***	-0.938***	-0.678***	-0.787***	-0.871***	-1.009***
	(0.144)	(0.128)	(0.122)	(0.112)	(0.134)	(0.144)	(0.172)	(0.195)
ΔCredit rating$_i^{RFB}$ × %(Post)$_{l,t}$	0.509***	0.512***						
	(0.052)	(0.053)						
ΔLeverage ratio$_i^{RFB}$ × %(Post)$_{l,t}$			0.514***	0.502***				
			(0.086)	(0.099)				
ΔLCR$_i^{RFB}$ × %(Post)$_{l,t}$					-0.013***	-0.012***		
					(0.003)	(0.003)		
Loan-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	Full	Full	IRB	IRB
Observations	4,324,803	4,324,803	4,324,803	4,324,803	4,324,803	4,324,803	3,789,416	3,789,416
R^2	0.867	-	0.867	-	0.867	-	0.854	-
Kleibergen-Paap F-statistic	-	166.0	-	807.7	-	50.8	-	46.2

Notes: The table shows loan-level regression results for equation (2), with additional control variables. i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. ΔRetail funding$_i^{RFB}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. ΔCredit rating$_i^{RFB}$ is the difference between the RFB credit rating and the group credit rating as of end-2018. ΔLeverage ratio$_i^{RFB}$ is the difference between the RFB regulatory leverage ratio and the group regulatory leverage ratio as of 2019:Q1. ΔLCR$_i^{RFB}$ is the difference between the RFB liquidity coverage ratio (LCR) and the group LCR as of 2019:Q1. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. In columns 2, 4, 6, and 8, ΔRetail funding$_i^{RFB}$ is instrumented by two variables: an indicator variable equal to one for banks that have more than £25 billion of retail deposits in 2011, and the ratio of non-interest income to total operating income in 2011 (see Section 2.3). The sample period is January 2010 to June 2019. In columns 7 and 8, the sample consists of IRB banks only. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

C Additional robustness tests

Table C.1: Effect of ring-fencing on mortgage spreads – Affected banks only

Dependent variable:	Interest rate spread$_{i,l,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	IV	IV	IV	IV
ΔRetail funding$_i^{\text{RFB}}$ $\times$ %(Post)$_{l,t}$	-0.907***	-0.988**	-1.882***	-1.660***	-1.558***
	(0.136)	(0.251)	(0.312)	(0.292)	(0.290)
Loan-level controls	No	No	No	Yes	Yes
Bank-level controls	No	No	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	No	No	No	Yes
Observations	3,076,175	3,076,175	3,076,175	3,069,712	2,853,176
R^2	0.810	-	-	-	-
Kleibergen-Paap F-statistic	-	47.2	68.2	68.3	77.9

Notes: The table shows loan-level regression results for equation (2). The sample consists only of banks subject to ring-fencing requirements. i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. ΔRetail funding$_i^{\text{RFB}}$ is the increase in bank i's retail funding share upon implementation of ring-fencing. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. In columns 2–5, ΔRetail funding$_i^{\text{RFB}}$ is instrumented by the ratio of non-interest income to total operating income in 2011 (see Section 2.3). The sample period is January 2010 to June 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table C.2: Effect of ring-fencing on mortgage spreads for banks not subject to ring-fencing (OLS)

Dependent variable:	Interest rate spread$_{i,l,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Exposure$_i$ × %(Post)$_{l,t}$	2.252	2.775	1.526	2.216	3.194	3.678*
	(1.898)	(1.907)	(1.882)	(1.946)	(1.937)	(1.965)
Exposure$_i$ × %(Post)$_{l,t}$ × Long maturity$_l$			-1.279*	-0.909		
			(0.708)	(1.016)		
Exposure$_i$ × %(Post)$_{l,t}$ × High LTV$_l$					-5.834***	-6.622***
					(1.177)	(1.677)
Loan-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	Yes	No	Yes	No	Yes
Observations	1,376,607	1,129,671	1,376,607	1,129,671	1,376,607	1,129,671
R^2	0.878	0.907	0.878	0.907	0.878	0.907

Notes: The table shows loan-level OLS regression results for equation (7), with additional interaction terms. The sample consists only of banks not subject to ring-fencing requirements. i indexes banks, l indexes loans, and t indexes origination months. The dependent variable is the interest rate spread (over OIS) on loan l originated by bank i in month t. Exposure$_i$ is bank i's exposure to the effects of ring-fencing, based on its mortgage lending portfolio in 2011 (see equation (6)). %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Long maturity$_l$ is equal to one for mortgages with maturity greater than two years, and zero otherwise. High LTV$_l$ is equal to one for mortgages with loan-to-value ratio greater than 90%, and zero otherwise. Bank-level controls are interactions between %(Post)$_{l,t}$ and one-quarter lags of: log(total assets), return on assets, cash / total assets, capital / risk-weighted assets, and wholesale funding / total assets. Loan-level controls are: LTV; LTI; mortgage term; log(loan value); borrower age; and indicator variables for first-time buyers, home movers, council buyers, borrowers with an impaired credit history, and brokered loans. The sample period is January 2010 to June 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table C.3: Effect of ring-fencing on syndicated lending – Intensive margin (OLS)

Dependent variable:	Log(Loan size)$_{i,l,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$	-0.520***				
	(0.186)				
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Term loan$_l$		-0.367**			
		(0.171)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Non-term loan$_l$		-0.584***			
		(0.172)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Leveraged loan$_l$			-0.416**		
			(0.185)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Non-leveraged loan$_l$			-0.550**		
			(0.220)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Lead arranger$_{i,l}$				-0.420***	
				(0.158)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Participant$_{i,l}$				-0.717***	
				(0.197)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ UK borrower$_l$					-0.185
					(0.204)
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{l,t}$ $\times$ Foreign borrower$_l$					-0.606***
					(0.208)
Difference between coefficients		-0.217*	-0.135	-0.297**	-0.421**
		(0.114)	(0.229)	(0.132)	(0.192)
Bank $\times$ Month fixed effects	Yes	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes	Yes
Bank $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Observations	139,779	139,157	139,602	139,653	139,710
R^2	0.968	0.968	0.968	0.974	0.968

Notes: The table shows OLS regression results for equation (9). i indexes banks, l indexes loan facilities, and t indexes origination months. The dependent variable is the log of the amount of credit extended by bank i in loan facility l in month t. ΔRetail funding$_i^{\text{NRFB}}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing. %(Post)$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing becomes binding. Bank $\times$ loan-category fixed effects are the interaction between bank indicator variables and indicator variables for: term loans (column 2), leveraged loans (column 3), lead arranger tranches (column 4), and UK borrowers (column 5). A loan is defined as leveraged if it is secured and has a spread of 125bp or higher. "Difference between coefficients" shows the difference between the parameter estimates in the column, with standard errors in parentheses. The sample period is January 2010 to March 2018. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table C.4: Effect of ring-fencing on syndicated lending – Extensive margin (OLS)

Dependent variable:	Log(Number loans)$_{i,j,c,t}$				
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	OLS
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$	-1.359***				
	(0.486)				
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Term loan$_c$		-0.651**			
		(0.263)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Non-term loan$_c$		-1.114***			
		(0.415)			
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Leveraged loan$_c$			-0.419**		
			(0.195)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Non-leveraged loan$_c$			-1.239***		
			(0.435)		
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Lead arranger$_c$				-0.958***	
				(0.331)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Participant$_c$				-0.935**	
				(0.415)	
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ UK borrower$_c$					-0.735***
					(0.272)
ΔRetail funding$_i^{\text{NRFB}}$ $\times$ %(Post)$_{j,t}$ $\times$ Foreign borrower$_c$					-1.005***
					(0.354)
Difference between coefficients		-0.463***	-0.820***	0.023	-0.270*
		(0.175)	(0.275)	(0.162)	(0.160)
Bank $\times$ Quarter fixed effects	Yes	Yes	Yes	Yes	Yes
Maturity $\times$ Quarter fixed effects	Yes	-	-	-	-
Maturity $\times$ Quarter $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Bank $\times$ Loan-category fixed effects	-	Yes	Yes	Yes	Yes
Observations	1,168,600	2,337,200	2,337,200	2,337,200	2,337,200
R^2	0.411	0.335	0.340	0.332	0.404

Notes: The table shows OLS regression results for equation (10). i indexes banks, j indexes loan maturities (measured in quarters), c indexes loan categories, and t indexes origination quarters. The dependent variable is the log of the number of loans with maturity j in category c in which bank i participates in quarter t. ΔRetail funding$_i^{\text{NRFB}}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing. %(Post)$_{j,t}$ is the proportion of the loan maturity that falls after January 2019, when ring-fencing becomes binding. Loan categories are: term / non-term (column 2), leveraged / non-leveraged (column 3), lead arranger / participant (column 4), UK borrower / foreign borrower (column 5). A loan is defined as leveraged if it is secured and has a spread of 125bp or higher. "Difference between coefficients" shows the difference between the parameter estimates in the column, with standard errors in parentheses. The sample period is 2010:Q1 to 2018:Q1. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.