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

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

Matthieu Chavaz[(1)] and David Elliott[(2)]

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, regulators and politicians have renewed calls for stronger "structural separation" in a range of jurisdictions.[1]

Recent structural separation proposals would require universal banks to conduct retail deposit-taking and certain investment banking activities in separate entities, in order to protect retail customers and taxpayers from investment banking risks *in crisis times*.[2] 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.

We study the recent introduction of "ring-fencing" requirements in the UK, which require large universal banks to separate into retail and non-retail subsidiaries. For identification, we exploit variation in the degree to which different banks are required to restructure their balance sheets, driven by pre-existing differences in business models. After separation, any activity in the non-retail entity loses access to retail deposit funding. These deposits instead become available for activities in the retail entity—such as retail lending. We show that, in response, the affected banks rebalance towards retail mortgage lending. This increases household credit supply, but also increases the concentration of the mortgage market 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 respond to the loss of deposit funding in the non-retail entity by reducing syndicated lending to large corporates.

[1]Including the US, Switzerland, and Italy (Hoenig, 2017; Financial Times, 2017, 2023; Reuters, 2023).
[2]See ICB (2011), Liikanen (2012), and Hoenig (2017) for UK, EU, and US proposals.

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.

Our results also resonate with an ongoing policy debate in the UK.[3] Affected banks have argued that ring-fencing has harmed their competitiveness in global syndicated lending markets (Reuters, 2017), while other commentators, including regulators and smaller banks, argue 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 by regulators 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

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

[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.

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 this restructuring generates a substantial shock to the funding structures of activities on either side of the fence. Relative to the pre-ring-fencing 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. We also estimate that, for the banks affected by ring-fencing, retail deposit funding is around 70 basis points cheaper than wholesale funding over our main sample period (2010–2019), consistent with safety and liquidity premia due to deposit insurance and household preferences for liquidity (Stein, 1998, 2012). The shift in deposit funding from the NRFB to the RFB should therefore reduce the cost of funding RFB activities and increase the cost of funding NRFB activities.

We evaluate the impact of this "deposit funding channel" on credit markets that are not directly targeted by the reform but which are crucial sources of financing for households and 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 ring-fenced banks in the mortgage market, we estimate spillover effects on market structure and risk-taking by unaffected banks in this market. Finally, we analyse syndicated corporate lending. All banks have placed syndicated lending in the NRFB, in order to continue to serve large corporate clients from one side of the fence and hence maintain established 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.

To test the impact of the deposit funding channel, we use a difference-in-difference-in-differences design that exploits variation in exposure to the effects of ring-fencing across both banks and individual loans. On the bank-level dimension, 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. We can therefore construct a continuous measure of exposure to ring-fencing that varies substantially *within* the group of affected banks.

On the loan-level dimension, a second advantage is that for the two markets we study, we have access to loan-level data from 2010 to 2019. 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 becomes binding in January 2019. The impact of ring-fencing 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 share of the loan's maturity that falls after January 2019.[5]

[5]This argument assumes that the bank retains the loan on its balance sheet. This assumption is natural for UK mortgages, which are almost entirely retained by the originating bank. For our analysis of syndicated lending, we relax this assumption by distinguishing between loans that are more or less likely to be retained.

Combining this bank-level and loan-level variation 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 other changes in regulation and the Brexit referendum. We also control for confounding demand-side factors through location-time or borrower-time fixed effects (Khwaja and Mian, 2008), as well as a range of loan-level controls.

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 16 basis point reduction in the interest rates on mortgages originated after ring-fencing, and with a 9 percentage point increase in the bank's market share for a given mortgage product.[6] 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). But we find no evidence that the increase in credit supply is larger for riskier mortgages, 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. Increased mortgage lending by these banks is therefore likely to lead to increased market concentration. We verify this by constructing a regional measure of exposure to credit supply from ring-fenced banks, based on their historical lending footprints. Consistent with our prior, we find that mortgage market concentration increases in more exposed regions. Increased competition from ring-fenced banks is also 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. Consistent with the franchise value model of Keeley (1990), we find that smaller banks operating in regions more exposed to higher competitive pressure increase their risk-taking, by cutting the rates on high-LTV mortgages more, and increasing the share of high-LTV mortgages in their lending portfolios.

[6] We also confirm that the increase in credit supply holds when we restrict the sample to affected banks only, which ensures that it is not driven by more general differences between large and small banks.

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 syndicated corporate credit supply: a one-standard-deviation (11 percentage point) decrease in deposit funding is associated with a 7% reduction in syndicated loan size. Consistent with the results being driven by the stability of deposit funding, the reduction is larger for credit lines and non-leveraged loans, which are more likely to be retained by the originator.

Contributions to existing literature Our main contribution is to a large literature debating the implications of structural separation of universal banks. Existing empirical studies have mostly focused on the impact of separating corporate lending from securities underwriting.[7] We expand the literature in three main directions. First, we emphasise the importance of bank funding structures, in particular the implications of constraining the use of deposit funding. Second, we document implications not only for large corporates but also for retail lending.[8] 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

[7]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. 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 (e.g., 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 (e.g., Fang et al., 2013; Franzoni and Giannetti, 2019).

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

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 market structure. Gande et al. (1999) show that preventing banks from entering the corporate debt underwriting market reduces competition in that market. Our finding 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. On the other hand, Favara and Giannetti (2017) show that greater mortgage market concentration can better incentivize lenders to internalize negative externalities associated with the liquidation of defaulting mortgages.

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; Carletti et al., 2021; Choudhary and Limodio, 2021; Drechsler et al., 2021; Li et al., 2023) 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 their 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 incentives for excessive risk-taking.

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.[9]

[9] 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. Appendix A compares the UK ring-fence with regulations and proposals in the US and Europe in more detail.

2 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 constraint has side effects on two 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.

Previous literature has argued that deposit insurance (Stein, 1998), household preferences for liquidity (Stein, 2012), and market power in deposit markets (Drechsler et al., 2017) reduce the cost of retail deposits relative to wholesale funding. To verify this in our sample, Figure 1 shows the spread between wholesale and retail funding costs for the five banks affected by ring-fencing over our main sample period (2010–2019). To proxy for retail funding costs, we use the spread on retail sight deposits over the monetary policy rate, since for the five affected banks, sight deposits account for over 70% of retail funding during our sample period. To proxy for wholesale funding costs, we use the 5-year senior CDS spread, which is a common benchmark measure of wholesale funding costs for large UK banks (e.g., Beau et al., 2014; Dent et al., 2021). The figure shows that wholesale funding is more expensive than retail funding over almost all of the sample, with an average spread of around 70 basis points.[10]

Given the difference in funding costs, redirecting retail deposit funding entirely to RFB activities is therefore likely to 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

[10]Some sources of short-term wholesale funding (such as short-term wholesale deposits and repo) might pay 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 is not the case in our study, as the Bank of England's policy rate remains above zero throughout our sample.

funding channel" of ring-fencing.

The strength of this channel across markets is an empirical question. Whether a bank would pass on changes in funding costs 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. In addition, 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 (Boyd et al., 1998; Freixas et al., 2007).

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 bank lending—we estimate difference-in-difference-in-differences regressions with the following general form over the sample period 2010–2019:

$$\text{Loan}_{i,l,t} = \beta \left(\text{Bank Exposure}_i \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) + \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \quad (1)$$

where $\text{Loan}_{i,l,t}$ is the price or volume of loan l originated by bank i at time t, and Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted.[11]

As we explain below, the extent to which ring-fencing should affect bank lending through the deposit funding channel should vary not only across banks but also across loans. We therefore fully exploit the granularity of our loan-level datasets and include

[11] The results are very similar if we instead set this variable to one from September 2011 (when ring-fencing was proposed by the ICB) or October 2012 (when draft legislation was published).

in our model two key treatment variables to capture the strength of the deposit funding channel across banks (Bank Exposure$_i$) and loans (Loan Exposure$_{l,t}$), respectively. We now explain these two variables in turn, as well as our instrumental variable strategy, and how our setup addresses key identification challenges.

Bank Exposure$_i$ Our first treatment variable captures variation in the strength of the deposit funding channel across banks. Since only banks with more than £25 billion of retail deposits are subject to ring-fencing, a binary comparison of affected and unaffected banks would be likely to pick up size-related heterogeneities beyond exposure to ring-fencing. Therefore, our bank-level treatment variable also exploits variation in exposure to the deposit funding channel *within* the group of affected banks.[12]

Specifically, for banks that have less than £25 billion of retail deposits, Bank Exposure$_i$ 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, Bank Exposure$_i$ 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:

$$\text{Bank Exposure}^{\text{RFB}} = \frac{\text{RFB retail deposits}}{\text{RFB total assets}} - \frac{\text{Group retail deposits}}{\text{Group total assets}} \quad (2)$$

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:

$$\text{Bank Exposure}^{\text{NRFB}} = \frac{\text{Group retail deposits}}{\text{Group total assets}} \quad (3)$$

These variables are illustrated in Figure 2 for a stylised universal bank. 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%

[12] In addition, we show that our main results are robust to dropping all non-affected banks and thus *only* exploiting variation between affected banks.

by retail deposits and 50% by wholesale funding (we ignore equity for simplicity). Before ring-fencing, the group is structured as a single legal entity, with a retail funding ratio of 50%. After ring-fencing, 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 Bank $\text{Exposure}_i^{\text{RFB}}$ is 25 percentage points. For the NRFB, the retail funding ratio falls from 50% to 0%, so Bank $\text{Exposure}_i^{\text{NRFB}}$ is 50 percentage points.

In practice, the average values of Bank Exposure_i across the affected banks once ring-fencing is implemented are 18pp for RFBs and 45pp for NRFBs, implying that ring-fencing resulted in large shifts in deposit funding from NRFB activities to RFB activities (Tables 1 and 2). 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.

One challenge is that the realised values of Bank Exposure_i are only observable at end-2018, after the affected banks completed their restructuring (the RFBs and NRFBs did not exist as distinct legal entities before this point, so did not have separate balance sheets). In order to address endogeneity issues associated with using a variable measured in 2018, we estimate our empirical model (1) via an instrumental variables approach, whereby we instrument Bank Exposure_i using predictors measured in 2011, before the legislation was proposed.

Specifically, we instrument Bank $\text{Exposure}_i^{\text{NRFB}}$ using the retail funding ratio of the group measured in 2011. The group retail funding ratio is strongly correlated over time, meaning that its value in 2011 (before the legislation is proposed) is a good predictor of its value at end-2018 (when the requirements come into force).

On the other hand, Bank $\text{Exposure}_i^{\text{RFB}}$ depends not only on the group's funding structure, but also on the composition of its assets (as shown by equation (2)). In particular, for banking groups with a larger exposure to activities that are prohibited from the RFB, the RFB balance sheet is substantially smaller than the original group, implying a larger increase in the RFB retail funding share. To capture this heterogeneity, we instrument Bank $\text{Exposure}_i^{\text{NRFB}}$ using the ratio of the bank's non-interest income to total operating income (NII ratio), measured in 2011. The NII ratio is a common measure of a bank's business model (e.g., Laeven and Levine, 2007; Brunnermeier et al., 2020): banks with high NII ratios derive much of their income from fees, commission, and trading, and hence tend to have a higher exposure to activities that are prohibited from the RFB such as investment banking and proprietary trading.[13]

As well as instrumental variable regressions—where we instrument the realised values of Bank Exposure_i (measured in 2018) with the respective instrumental variables (measured in 2011)—we also run "reduced form" regressions, where we include the instruments directly in model (1), in place of the realised values of Bank Exposure_i. In our tables, we refer to the realised values as Bank $\text{Exposure}_{2018,i}$ and the instruments as Bank $\text{Exposure}_{2011,i}$.

Loan $\text{Exposure}_{l,t}$ Ring-fencing legislation was finalised in December 2013 but only came into force from January 2019, giving banks several years to restructure. And during this period, several other factors (such as other changes in regulation) could have also affected bank behaviour. A simple difference-in-differences comparison of banks before and after December 2013 or before and after January 2019 is therefore unlikely to capture the true effect of ring-fencing.

Our second treatment variable therefore exploits a key source of variation in the extent to which ring-fencing should affect the cost of funding individual loans *within* a given bank and time period in the run-up to implementation. Ring-fencing leaves retail deposits available to fund activities in the RFB (such as mortgages) but not activities in the NRFB (such as syndicated lending). We exploit the idea that the impact of this change on the expected cost of funding a loan should depend on the maturity of the loan.

To see this, consider first a loan that matures before January 2019. Since this loan will have dropped from the bank's balance sheet before ring-fencing is implemented, ring-

[13] We set both instruments to zero for banks below the £25 billion threshold.

fencing should not affect its expected funding mix. In contrast, consider a syndicated loan that is originated after December 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 wholesale funding is more expensive than deposit funding (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, implying a lower expected funding cost.

In line with this idea, we define Loan $\text{Exposure}_{l,t}$ to be the proportion of the loan's term that falls after January 2019. Variation in the value of this variable across loans is illustrated in Figure 3. For example, for a five-year loan originated in January 2017, Loan $\text{Exposure}_{l,t}$ 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%.

This variable is based on standard Net Present Value (NPV) logic: when valuing an asset, one should use discount rates that match the maturities of the cashflows. In the case of a loan, this logic implies that the NPV of the loan depends on the expected cost of funding the loan throughout its full term. In turn, this depends on the availability of funding sources not only when the loan is originated, but throughout its entire term.

One condition for this logic to hold is that the bank expects to retain the loan on its balance sheet. This assumption is natural for the UK mortgage market, where banks retain the vast majority of mortgages. For our analysis of syndicated lending (Section 6), we relax this assumption by distinguishing between loans that are more or less likely to be retained. In line with theory, we find that that the effect of ring-fencing is weaker for loans less likely to be retained.

Controls The fact that we can exploit variation both across banks (Bank Exposure$_i$) and across loans (Loan Exposure$_{l,t}$) allows us to employ a rich set of fixed effects to control for a wide range of potential 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 control for any bank-level characteristics that might be correlated with Bank Exposure$_i$ and affect lending decisions irrespective of ring-fencing.

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, as well as loan-level control variables (e.g. borrower income and age). Meanwhile, our syndicated loan regressions include borrower-time fixed effects. We discuss these controls in more detail in Sections 4 and 6.

Remaining threats to identification and the exclusion restriction The combination of our extensive set of fixed effects and controls, and instrumental variables 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 Bank Exposure$_i$; (ii) vary across loan maturities and time in a way that is correlated with Loan Exposure$_{l,t}$; 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 Bank Exposure$_i$.

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 the share of a loan's maturity that falls after January 2019 (criterion ii).

Similarly, there are several factors that could plausibly correlate with our instruments and influence lending decisions in ways unrelated to ring-fencing; these include for instance 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 Loan Exposure$_{l,t}$ 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. These include the impact of ring-fencing on a bank's perceived riskiness (and hence wholesale funding costs) and regulatory capital or liquidity ratios; as well as changes in LTV-specific capital requirements on mortgages (Benetton, 2021). However we show 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, 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 UK mortgages are retained by the originator.

We focus on vanilla fixed-rate mortgages originated between January 2010 and December 2019.[14] Summary statistics for this sample are provided in Table 1. The sample consists of around five million loans. The average loan is around £140,000 and has a 67% LTV ratio.

The large majority of UK mortgages have a "fixation period" of between two and five years.[15] 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). 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 January 2010 to December 2019. 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.

[14]We start the sample in 2010 to avoid the effects of the financial crisis, and because there were several bank mergers in 2009. We end the sample in 2019 to avoid the effects of COVID-19.

[15]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.

We consolidate borrowers and lenders at the ultimate parent level. Following Roberts (2015), we drop observations that are likely to be amendments to existing loans, because these do not necessarily involve new credit. We also drop loans to public sector and financial sector borrowers. 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.

To set the scene, Figure 4 plots average quoted spreads for common UK mortgage products. Spreads fell substantially in the years leading up to ring-fencing implementation across all major market segments (Panel A), largely driven by banks subject to ring-fencing (Panel B). Industry commentary links these trends to ring-fencing, with several banks arguing that, by requiring deposits to fund domestic retail lending, ring-fencing 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 diff-in-diff-in-diff model (1):

$$
\begin{aligned}
\text{Spread}_{i,l,j,t} = \beta\left(\text{Bank Exposure}_i^{\text{RFB}} \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) \\
+ \alpha_{i,t} + \delta_{j,t} + \eta_{i,j} + \phi_1 \text{Loan-level controls}_{l,t} \\
+ \phi_2\left(\text{Bank-level controls}_{i,t-1} \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) + \varepsilon_{i,l,j,t},
\end{aligned} \tag{4}
$$

where $\text{Spread}_{i,l,j,t}$ 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. Subscript j refers to the "product" category into which the mortgage falls, defined by the combination of maturity and LTV ratio.[16] $\text{Bank Exposure}_i^{\text{RFB}}$ measures the increase in bank i's retail funding share (for RFB activities) as a result of ring-fencing (see Section 2.2). $\text{Loan Exposure}_{l,t}$ is the proportion of mortgage l's maturity that falls after January 2019, when ring-fencing requirements become binding. And Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. 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.[17]

We include a rich set of fixed effects to control for a range of potential confounding

[16] We measure maturity (fixation period) 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). So one example of a product would be a 24-month maturity with an LTV ratio between 70% and 75%.

[17] We do not include the double interaction ($\text{Bank Exposure}_i^{\text{RFB}} \times \text{Loan Exposure}_{l,t}$) because it is almost perfectly collinear with the triple interaction ($\text{Bank Exposure}_i^{\text{RFB}} \times \text{Loan Exposure}_{l,t} \times \text{Post}_t$). To see this, note that after December 2013 (when $\text{Post}_t = 1$), the double interaction is always equal to the triple interaction. Meanwhile, before December 2013 (when $\text{Post}_t = 0$), the triple interaction is always equal to zero, and so the double interaction only differs from the triple interaction when $\text{Loan Exposure}_{l,t} > 0$. However, in practice, $\text{Loan Exposure}_{l,t}$ is equal to zero (or close to zero) for the vast majority of mortgages originated before December 2013, because the vast majority of UK mortgages have maturities (fixation periods) of five years or less, meaning that very few mortgages originated before December 2013 mature after January 2019. We also show that our results are robust to restricting the sample to loans with maturities of five years or less, which ensures that the collinearity between the double interaction and triple interaction is exact.

factors. These include bank-month fixed effects $\alpha_{i,t}$ 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 $\delta_{j,t}$. 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. And we include bank-product fixed effects $\eta_{i,j}$ 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 risk.

We control for several lagged quarterly bank-level control variables interacted with $\text{Loan Exposure}_{l,t} \times \text{Post}_t$: log(total assets); return on assets; cash / total assets; capital / risk-weighted assets; and wholesale funding / total assets. We also include 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, our most conservative specifications also add property location-month fixed effects, to control for changes in local economic conditions.[20]

Estimated regressions results for several versions of equation (4) are reported in Table 3. The sample consists of fixed-rate mortgages originated between January 2010 and December 2019. Standard errors are clustered by bank.

In columns 1–3, we estimate the "reduced form" version of the model—that is, we measure bank exposure to ring-fencing using the bank's non-interest income (NII) ratio measured in 2011 ($\text{Bank Exposure}_{2011,i}$). Our estimate of β is negative and highly significant, including when we add loan-level controls (column 2) and location-month fixed

[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 (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.

effects (column 3). 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 columns 4–6, we move to our preferred IV approach, whereby we instrument the increase in the RFB retail funding ratio as realised in 2018 (Bank $\text{Exposure}_{2018,i}$) with the NII ratio measured in 2011 (Bank $\text{Exposure}_{2011,i}$).[21] We report the corresponding first-stage regressions in Table C1: the instrument is a very strong predictor of Bank $\text{Exposure}_{2018,i}$, yielding first-stage Kleibergen-Paap F-statistics of over 30.

Our estimate of β in the second-stage regressions is again negative and highly statistically significant across a range of specifications (Table 3, columns 4–6). The result is also robust to restricting the sample to loans with maturities of five years or less, which is the longest standard maturity for UK mortgages (column 7).

The estimated effect is highly economically significant. Across our key IV specifications (columns 4–6), the estimate of β averages around -0.75. Comparing mortgages originated after ring-fencing implementation (Loan $\text{Exposure}_{l,t} = 1$) to mortgages that mature before ring-fencing implementation (Loan $\text{Exposure}_{l,t} = 0$), a one-standard-deviation (22 percentage point) increase in Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ is therefore consistent with a reduction in mortgage spreads of around 16 basis points, which is around 17% of the standard deviation of spreads over the sample period (Table 1). This estimate of β (around -0.75) is also close to the average wholesale-retail funding cost spread over our sample (around 70 basis points; see Figure 1). This suggests that the reduction in the cost of funding mortgages as a result of ring-fencing is approximately fully passed through to mortgage spreads.

As we discuss above, our rich set of fixed effects and control variables rules out a wide range of potential confounding factors, including developments that might affect different banks differently over time. In Appendix B, we provide a more detailed description of several potential alternative mechanisms and how they are controlled for by our setup.

[21]Specifically, we instrument (Bank $\text{Exposure}^{\text{RFB}}_{2018,i} \times$ Loan $\text{Exposure}_{l,t} \times \text{Post}_t$) with (Bank $\text{Exposure}^{\text{RFB}}_{2011,i} \times$ Loan $\text{Exposure}_{l,t} \times \text{Post}_t$).

In particular, we show that our results are not explained by the impact of ring-fencing on banks' perceived riskiness (and hence on wholesale funding costs); changes to regulatory capital or liquidity ratios; or changes in LTV-specific capital requirements on mortgages (Benetton, 2021).

To further assuage concerns about differences between the (large) banks affected by ring-fencing and other (smaller) banks, we also re-run our regressions including only affected banks (Appendix C, Table C2): this ensures that our results are driven by variation in exposure to ring-fencing across affected banks, rather than by more general differences between large and small banks. The results are similar to our baseline results.

Finally, in Figure 5, we show estimates of β estimated separately for each year from 2014 to 2018.[22] The estimated coefficient is broadly stable over this period. This suggests that our results are not driven by differential trends in credit supply across the maturity spectrum for reasons other than ring-fencing, nor by specific shocks during the period after 2014.[23]

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:

$$\begin{aligned} \text{Market share}_{i,j,t} = {} & \beta \left(\text{Bank Exposure}_i^{\text{RFB}} \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right) \qquad (5) \\ & + \alpha_{i,t} + \eta_{i,j} + \phi \left(\text{Bank-level controls}_{i,t-1} \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right) + \varepsilon_{i,j,t}, \end{aligned}$$

[22] As noted above, very few mortgages originated before 2014 have non-zero values for Loan $\text{Exposure}_{l,t}$, since the vast majority of mortgages have maturities of five years or less and hence mature before 2019. Note also that we cannot estimate a coefficient for 2019, because Loan $\text{Exposure}_{l,t} = 1$ for all mortgages originated in 2019.

[23] The coefficient for 2014 is estimated with lower precision than the coefficients for 2015–2018. This is likely to reflect the fact that mortgage maturities are not always reported in PSD prior to 2015, and hence the sample size in 2015 is much smaller than in the subsequent years.

where Market $\text{share}_{i,j,t}$ is the market share of bank i in product j in quarter t. Our main explanatory variable $\left(\text{Bank Exposure}_i^{\text{RFB}} \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right)$ is as defined in equation (4). We include bank-quarter fixed effects $\alpha_{i,t}$; bank-product fixed effects $\eta_{i,j}$; and the interaction of the five bank-level controls used in equation (4) with $\text{Loan Exposure}_{j,t} \times \text{Post}_t$. Standard errors are clustered by bank.

The results 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. As before, we estimate reduced form regressions where we measure bank exposure to ring-fencing using the bank's NII ratio measured in 2011 (columns 1 and 2), and IV regressions where we instrument the increase in the RFB retail funding ratio as realised in 2018 with the NII ratio measured in 2011 (columns 3 and 4). The full sample period is 2010:Q1–2019:Q4 (columns 1 and 3). We also estimate regressions for the subsample 2015:Q1–2019Q4 (columns 2 and 4), because loan maturities are not always reported in our dataset prior to 2015, which is likely to add noise to the measurement of market shares in the pre-2015 period.[24]

Our baseline IV estimate for the full sample period (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 9 percentage points.

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 provide different predictions about where

[24] In all columns, we restrict the sample to mortgage products with maturities of five years or less, since this is the longest standard maturity for UK mortgages. This captures around 98% of the market.

the effect is likely to be strongest.

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).

To explore these ideas, we expand equation (4) by interacting our main variable 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 are reported in Table 5.[25] We find that the negative impact of ring-fencing on mortgage spreads is larger for longer-term loans, both when we estimate in reduced form (column 1) and using instrumental variables (columns 3 and 4). We also find some evidence that the impact of ring-fencing is smaller for high-LTV loans (column 2), although this effect is statistically insignificant when we estimate using IV (columns 5 and 6).

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 3) but not for higher-risk mortgages (columns 2 and 4).

In summary, we observe larger impacts of ring-fencing on longer-term mortgages, consistent with synergies between deposit-taking and long-term lending. On the other hand, we find no evidence that ring-fencing causes treated banks to increase the riskiness of their mortgage lending.

[25] In the IV regressions, all interactions involving Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ are instrumented with the corresponding interactions involving Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$.

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,[26] 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.

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 (specifically, Table 3 shows that they reduce the spreads on these mortgages, and Table 4 shows that they increase market share for these mortgages). To capture this effect, we use Loan Exposure measured at the mortgage 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. There

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

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 share using all loans, the estimated coefficient is 0.424 (column 1), and highly statistically significant. Comparing columns 2 and 3 shows that this persistence is primarily driven by non-brokered loans, suggesting that branch presence is an important factor (Robles-Garcia, 2019).

Given this 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 (using non-brokered loans), and use these to construct the weighted average of Bank Exposure$_i^{\text{RFB}}$. That is, for district g, we compute:

$$\text{District Exposure}_g = \sum_{i=1}^{N} \text{Market share}_{i,g}^{2011} \times \text{Bank Exposure}_i^{\text{RFB}}. \tag{6}$$

We construct two versions of this measure: one using Bank Exposure$_{2018,i}^{\text{RFB}}$ (the increase in the RFB retail funding ratio as realised in 2018), and one using Bank Exposure$_{2011,i}^{\text{RFB}}$ (the NII ratio measured in 2011).

We then run regressions of the form:

$$\begin{aligned} \text{HHI}_{g,j,t} = \beta \left(\text{District Exposure}_g \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right) \\ + \alpha_{j,t} + \delta_{g,j} + \eta_{g,t} + \varepsilon_{g,j,t}, \end{aligned} \tag{7}$$

where g indexes districts, j indexes products (defined by maturity quarter and LTV bucket), and t indexes origination quarters. We control for product-quarter fixed effects $\alpha_{j,t}$; district-product fixed effects $\delta_{g,j}$; and district-quarter fixed effects $\eta_{g,t}$ to control for local economic conditions. The sample period is 2010:Q1–2019:Q4.

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 mortgage 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 which 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 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:

$$\text{Competitor Exposure}_i = \sum_{g=1}^{G} \text{Portfolio share}_{i,g}^{2011} \times \text{District Exposure}_g, \tag{8}$$

where $\text{Portfolio share}_{i,g}^{2011}$ is the proportion of bank i's 2011 mortgage lending portfolio originated in district g; and $\text{District Exposure}_g$ is district g's exposure to ring-fencing, as estimated in equation (6). Again, we estimate one version of $\text{Competitor Exposure}_i$ where $\text{District Exposure}_g$ is based on $\text{Bank Exposure}_{2018}^{\text{RFB}}$, and one version where $\text{District Exposure}_g$ is based on $\text{Bank Exposure}_{2011}^{\text{RFB}}$.

We then drop the banks directly affected by ring-fencing from our sample, and for the remaining banks we estimate the model:

$$\begin{aligned}\text{Spread}_{i,l,j,t} = {} & \beta \left(\text{Competitor Exposure}_i \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) \\ & + \alpha_{i,t} + \delta_{j,t} + \eta_{i,j} + \phi_1 \text{Loan-level controls}_{l,t} \\ & + \phi_2 \left(\text{Bank-level controls}_{i,t-1} \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) + \varepsilon_{i,l,j,t},\end{aligned} \tag{9}$$

where the set of control variables is the same as used in equation (4).

The results for model (9) are reported in Table 9, including additional interaction terms for long-maturity mortgages (maturity greater than two years) and high-LTV mortgages (LTV greater than 90%). Columns 1 and 2 suggest that more exposed competitors do not significantly change their pricing of longer-maturity mortgages relative to other mortgages. However, columns 3 and 4 show that more exposed competitors significantly reduce spreads on high-LTV mortgages relative to other 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 -6, suggesting that a one-standard-deviation (3.3pp) increase in Competitor Exposure$_i$ is associated with a reduction in the spread on high-LTV mortgages (relative to other mortgages) of around 20 basis points.

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

$$\begin{aligned}\text{Portfolio share}_{i,j,t} = {} & \beta \left(\text{Competitor Exposure}_i \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right) \\ & + \gamma \left(\text{Competitor Exposure}_i \times \text{Loan Exposure}_{j,t} \times \text{Post}_t \times \text{High LTV}_j\right) \\ & + \delta_{j,t} + \eta_{i,j} + \phi \left(\text{Bank-level controls}_{i,t-1} \times \text{Loan Exposure}_{j,t} \times \text{Post}_t\right) + \varepsilon_{i,j,t},\end{aligned} \tag{10}$$

where Portfolio share$_{i,j,t}$ is bank i's mortgage lending volume in product j in quarter t, divided by bank i's total mortgage lending volume in quarter t. We include product-quarter fixed effects $\delta_{j,t}$; bank-product fixed effects $\eta_{i,j}$; and lagged bank-level control variables interacted with Loan Exposure$_{j,t}$ × Post$_t$. The sample again consists only of banks not subject to ring-fencing requirements.

The results are reported in Table 10. When we estimate the model using OLS or unweighted IV, the estimate of γ is positive but insignificant (columns 1 and 3). However, since equation (10) is estimated at the bank-product level, and the dependent variable is normalised by the bank's total lending volume, these unweighted estimators do not fully reflect differences in the relative economic importance of different banks. We therefore also estimate WLS regressions where we weight observations by the bank's total lending volume in the quarter. In this case, the estimate of γ is positive and statistically significant, indicating that more exposed competitors increase the share of riskier mortgages in their portfolios (columns 2 and 4).

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 one side of the fence and preserve informational synergies between corporate lending and securities underwriting (Drucker and Puri, 2005; Yasuda, 2005; Neuhann and Saidi, 2018).

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, which allows us to control for credit demand in the spirit of Khwaja and Mian (2008). We estimate the following regression:

$$\text{Log(Loan size)}_{i,l,t} = \beta \left(\text{Bank Exposure}_i^{\text{NRFB}} \times \text{Loan Exposure}_{l,t} \times \text{Post}_t\right) + \alpha_{i,t} + \delta_l + \varepsilon_{i,l,t}, \tag{11}$$

where $\text{Log(Loan size)}_{i,l,t}$ is the log of the amount of credit extended by bank i in loan facility l in month t.[27] $\text{Bank Exposure}_i^{\text{NRFB}}$ measures the decrease in bank i's retail funding share (for NRFB activities) as a result of ring-fencing. $\text{Loan Exposure}_{l,t}$ is the proportion of the loan maturity period that falls after January 2019. Post_t is an indicator variable equal to one from December 2013. The sample consists of global syndicated loan originations (excluding loans to public sector and financial sector borrowers) from January 2010 to December 2019.

As in our mortgage regressions, the set of control variables includes bank-month fixed effects $\alpha_{i,t}$ to control for confounding supply-side factors. We also include loan facility fixed effects δ_l. These fixed effects nest borrower-time fixed effects, and control for all observed and unobserved borrower and loan characteristics, including the borrower's credit demand.

Results The results are reported in Table 11. In columns 1–3 we only include loans where lender-level quantities are observed in DealScan. In column 1, we estimate the reduced form version of the model, where we measure $\text{Bank Exposure}_i^{\text{NRFB}}$ using the group retail funding ratio as of 2011. In columns 2 and 3, we estimate IV regressions where we measure $\text{Bank Exposure}_i^{\text{NRFB}}$ using the group retail funding ratio as of 2018, instrumented by its value in 2011 (see Section 2.2). We show the first-stage regressions corresponding to these IV regressions in Table C4; as for the mortgage market, the instrument is a very strong predictor of $\text{Bank Exposure}_{2018,i}^{\text{NRFB}}$, with first-stage Kleibergen-Paap F-statistics of over 80.

We find that greater exposure to ring-fencing is associated with a significant decrease

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

in syndicated lending. Our baseline IV specification (column 2) suggests that a one-standard-deviation (11 percentage point) decrease in deposit funding as a result of ring-fencing is associated with a 7% reduction in loan size. As for mortgages, the result is also robust to restricting the sample to loans with maturities of five years or less (column 3).

In Figure 5, we show estimates of β separately for each year from 2014 to 2018. As for mortgages, the estimated coefficient is broadly stable over this period, with the exception of 2017, where we estimate no significant effect; this may reflect heightened political and regulatory uncertainty during the Brexit negotiations.

Lender-level quantities are often missing in DealScan, so for loans with missing values we also construct imputed lender shares by dividing the total loan size equally among lead arrangers, in line with Ivashina and Scharfstein (2010), Giannetti and Laeven (2012), and Bräuning and Ivashina (2020). Columns 4–6 show regressions using these imputed lender shares. We again observe a highly significant negative impact of ring-rencing on syndicated loan supply.

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, including those whose impact could vary across banks and time.

Unlike UK mortgages, a substantial share of syndicated loans are sold by the originator, and so should be less affected by the change in funding structure caused by ring-fencing. In Table 12, we therefore test whether the impact of ring-fencing on syndicated lending varies across loans depending on how likely they are to be sold. 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). We estimate our main result separately for term vs non-term loans (columns 1 and 3), and leveraged vs non-leveraged loans (column 2 and 4).[28] Consistent with our prior, we find that the effect is significantly larger for non-term loans and non-leveraged loans (with the exception of non-leveraged loans when using observed loan shares only).

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 forces universal banks to split retail deposit-taking and investment banking into separate subsidiaries. We show that this "ring-fencing" leads to a large 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 increases the supply 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

[28] 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.

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 (Mian et al., 2013). The expansion of consumer credit is mirrored by a reduction in credit supply to large corporates, implying uncertain net effects on welfare.

Second, our results suggest ambiguous longer-term implications for competition in the retail credit market. By reducing the ability of smaller banks to compete, structural separation leads to more concentrated markets. Indeed, ring-fencing is reported to have 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 over the longer term. Alternatively, increased concentration might simply reflect less efficient banks leaving the market. Greater concentration in mortgage lending could also incentivize lenders to internalize the negative spillovers associated with the liquidation of defaulting mortgages, and hence reduce foreclosures and house price declines (Favara and Giannetti, 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. 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 the vulnerability of these smaller lenders.

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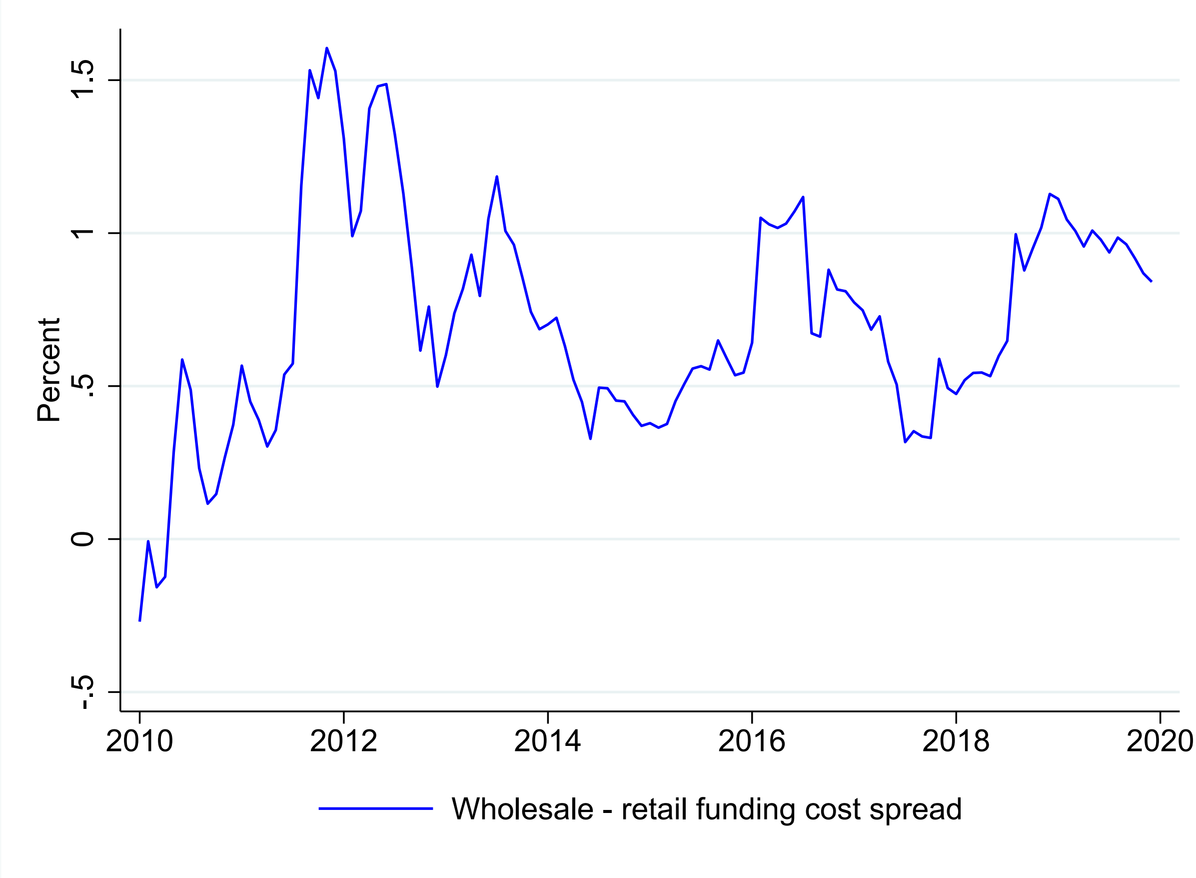
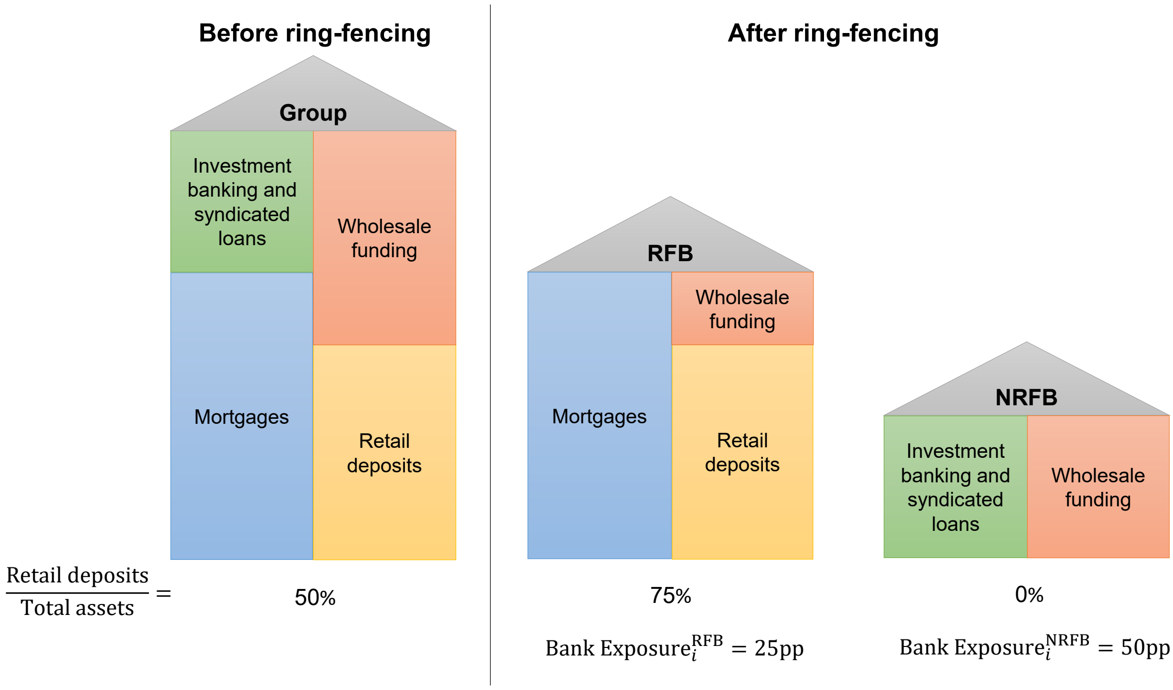
Figure 1: Spread between wholesale and retail funding costs



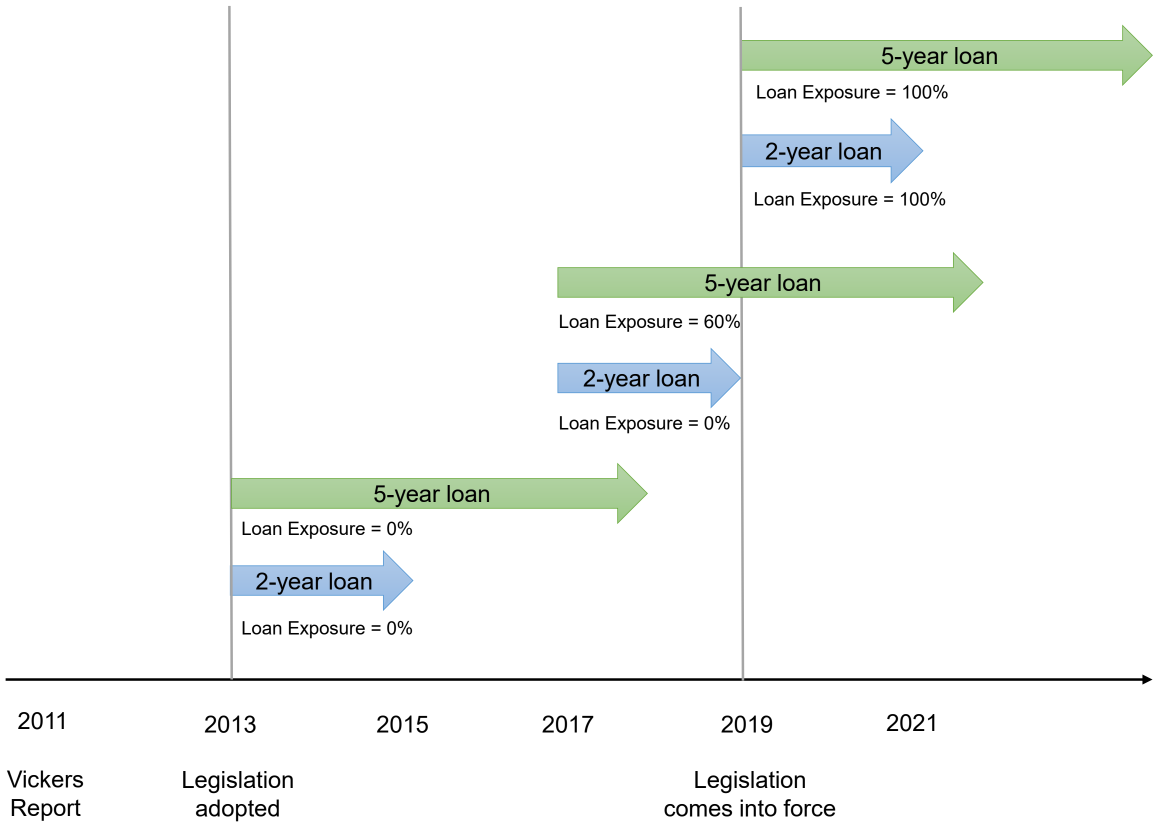
Notes: The chart shows the spread between wholesale and retail funding costs, averaged across banks subject to ring-fencing. Wholesale funding costs are proxied by 5-year senior CDS spreads. Retail funding costs are proxied by the spread between retail sight deposit rates and the Bank of England policy rate. Monthly averages.
Sources: Moneyfacts, Bank of England, Barclays.

Figure 2: Impact of ring-fencing on funding structure (Bank Exposure$_i$)



Notes: The figure illustrates the impact of ring-fencing on the funding structure of a stylised banking group. The left panel shows the group before ring-fencing. All assets and liabilities are held in the same legal entity. The right panel shows the group after ring-fencing, once it 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. Bank Exposure$_i$ measures the extent to which the retail funding ratio of the RFB (NRFB) increases (decreases) as a result of ring-fencing.

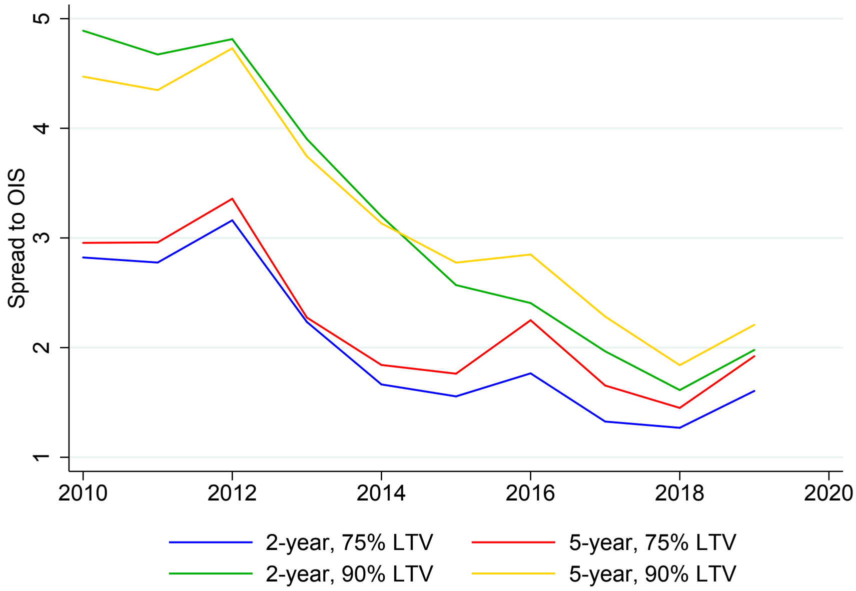
Figure 3: Impact of ring-fencing across time and loans (Loan Exposure$_{l,t}$)



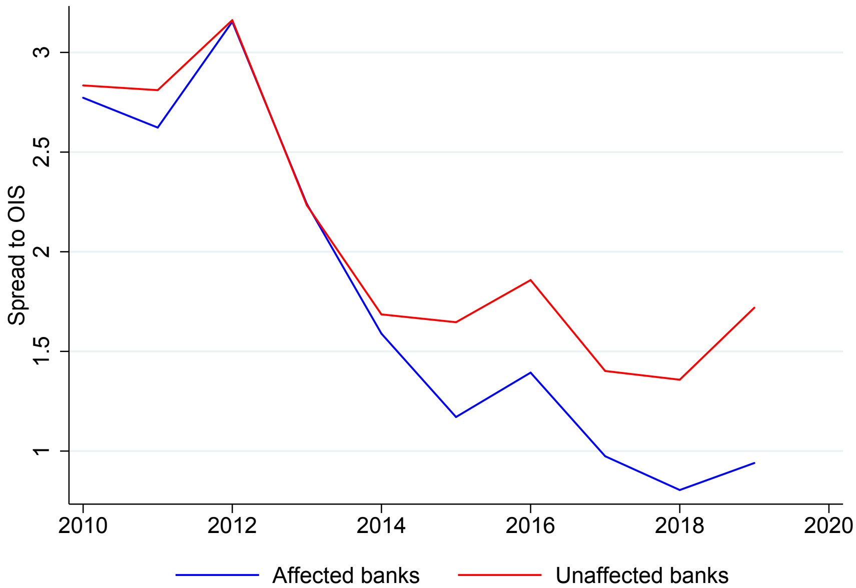
Notes: The figure illustrates how the impact of ring-fencing on funding structure varies across loans with different maturities and origination dates. Each arrow represents a loan, extending from its origination date to its maturity date. The label inside the arrow corresponds to the loan's maturity (fixation period for mortgages). The label below the arrow shows the corresponding value of Loan Exposure$_{l,t}$, defined as the proportion of the loan's maturity that falls after January 2019, when ring-fencing requirements come into effect.

Figure 4: Average quoted mortgage spreads

(A) Average spreads by product



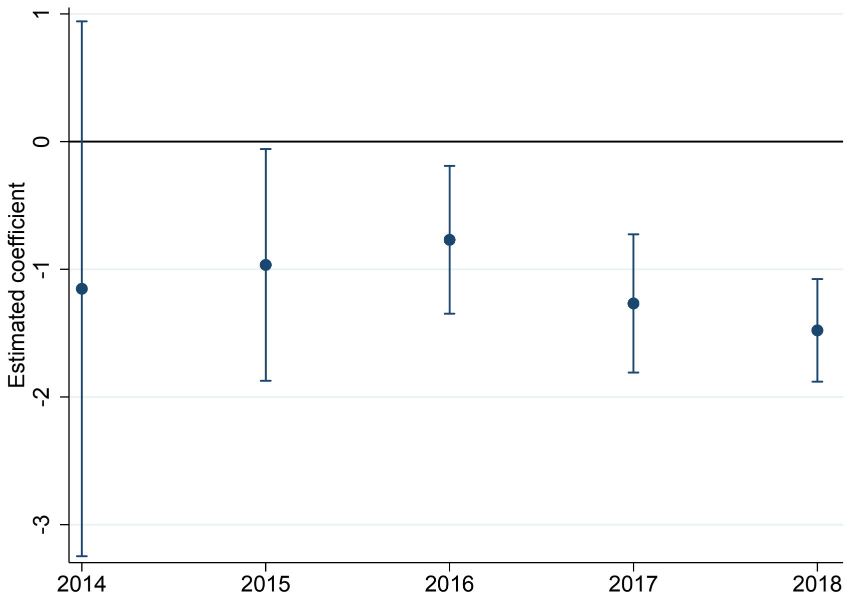
(B) Average spreads for 2-year, 75% LTV mortgages



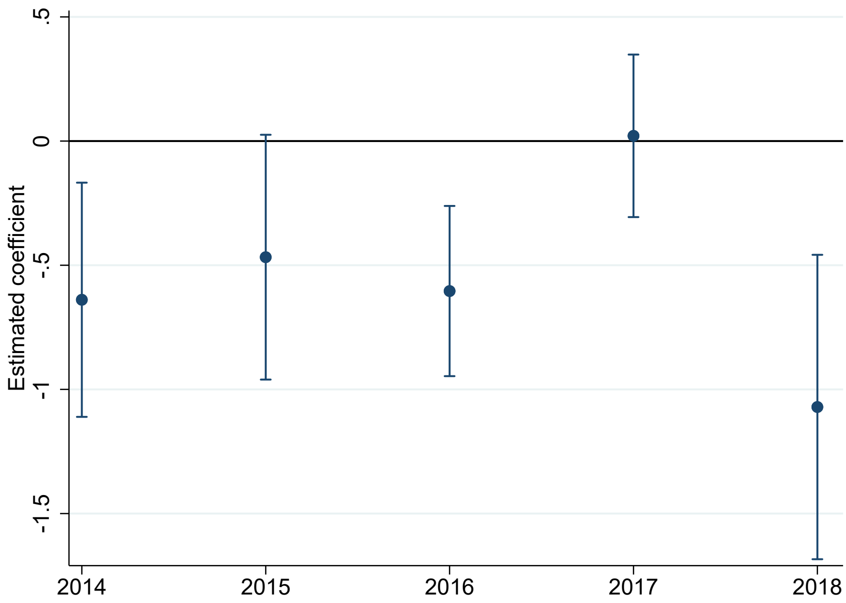
Notes: The charts show quoted mortgage spreads. Panel A shows spreads for several major mortgage products, averaged across all lenders. Panel B shows spreads for 2-year, 75% LTV mortgages, averaged across banks subject to ring-fencing (blue line) and other lenders (red line). Annual averages.
Sources: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

Figure 5: Estimated impact of ring-fencing by year

(A) Mortgages



(B) Syndicated loans



Notes: The figures show estimated coefficients on the interaction variable Bank Exposure$_i$ $\times$ Loan Exposure$_{l,t}$, estimated separately for each year 2014–2018, and 95% confidence intervals. The full regression models correspond to Table 3, column 1 for mortgages; and Table 11, column 1 for syndicated loans.

Table 1: Summary statistics for mortgage regressions

	Observations	Mean	Std Dev	p25	p50	p75
Dependent variables						
Interest rate spread (percent)	5,003,625	1.83	0.92	1.17	1.58	2.27
Market share	379,724	0.021	0.084	0	0	0
Herfindahl-Hirschman Index (HHI)	577,411	0.496	0.252	0.300	0.498	0.588
Portfolio share (competitor banks)	57,765	0.030	0.078	0.001	0.006	0.027
Measures of exposure to ring-fencing						
Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ (treated banks)	5	0.286	0.160	-	-	-
Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ (treated banks)	5	0.182	0.216	-	-	-
District $\text{Exposure}_{2011,g}$	390	0.190	0.025	0.176	0.190	0.205
District $\text{Exposure}_{2018,g}$	390	0.114	0.035	0.092	0.118	0.137
Competitor $\text{Exposure}_{2011,i}$	58	0.173	0.025	0.166	0.176	0.183
Competitor $\text{Exposure}_{2018,i}$	58	0.103	0.033	0.092	0.110	0.121
Loan $\text{Exposure}_{l,t}$	5,003,625	0.430	0.412	0.000	0.354	0.881
Loan-level controls						
Maturity (months)	5,003,625	39.1	19.9	24	26	60
Loan-to-value ratio (LTV)	5,003,625	66.6	21.6	52.6	72.5	85.0
Loan-to-income ratio (LTI)	5,003,625	3.10	1.07	2.32	3.17	3.97
Mortgage term (months)	5,003,625	275	105	204	300	360
Log(Loan value)	5,003,625	11.9	0.7	11.5	11.9	12.3
Borrower age (years)	5,003,625	38.4	9.9	31	37	45
First-time buyer indicator	5,003,625	0.276	0.447	0	0	1
Home mover indicator	5,003,625	0.340	0.474	0	0	1
Council buyer indicator	5,003,625	0.010	0.101	0	0	0
Impaired credit history indicator	5,003,625	0.003	0.055	0	0	0
Brokered indicator	5,003,625	0.710	0.454	0	1	1
Long maturity indicator	5,003,625	0.448	0.497	0	0	1
High LTV indicator	5,003,625	0.080	0.272	0	0	0
Bank-level controls						
Log(Total assets)	5,003,625	12.6	1.5	12.3	13.3	13.5
Return on assets	5,003,625	0.004	0.005	0.002	0.004	0.007
Cash / Total assets	5,003,625	0.078	0.030	0.059	0.079	0.097
Capital / Risk-weighted assets	5,003,625	0.189	0.067	0.148	0.167	0.205
Wholesale funding / Total assets	5,003,625	0.272	0.119	0.190	0.269	0.345

Notes: The table shows summary statistics for variables used in the mortgage regressions. The sample period is January 2010 to December 2019.

Table 2: Summary statistics for syndicated lending regressions

	Observations	Mean	Std Dev	p25	p50	p75
Dependent variables						
Log(Loan size)	144,781	1.99	2.68	0.49	2.80	3.91
Log(Imputed loan size)	370,293	2.81	2.44	1.90	3.44	4.45
Measures of exposure to ring-fencing						
Bank $\text{Exposure}^{\text{NRFB}}_{2011,i}$ (treated banks)	5	0.535	0.090	-	-	-
Bank $\text{Exposure}^{\text{NRFB}}_{2018,i}$ (treated banks)	5	0.454	0.107	-	-	-
Loan $\text{Exposure}_{l,t}$	370,293	0.355	0.384	0.000	0.202	0.722
Facility-level variables						
Term loan indicator	370,293	0.451	0.498	0	0	1
Leveraged loan indicator	370,293	0.333	0.471	0	0	1

Notes: The table shows summary statistics for variables used in the syndicated lending regressions. The sample period is January 2010 to December 2019.

Table 3: Effect of ring-fencing on mortgage spreads

Dependent variable:	Interest rate spread$_{i,l,j,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	IV	IV	IV	IV
Bank $\text{Exp}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t	-1.017***	-0.812***	-0.756***				
	(0.290)	(0.258)	(0.245)				
Bank $\text{Exp}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t				-0.880***	-0.703***	-0.656***	-0.711***
				(0.250)	(0.207)	(0.201)	(0.209)
Loan-level controls	No	Yes	Yes	No	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	No	Yes	No	No	Yes	Yes
Sample	Full	Full	Full	Full	Full	Full	≤5-year
Observations	4,996,279	4,985,651	4,781,808	4,996,279	4,985,651	4,781,808	4,690,228
R^2	0.815	0.842	0.863	-	-	-	-
Kleibergen-Paap F-statistic	-	-	-	37.8	37.8	40.9	41.4

Notes: The table shows loan-level regression results for equation (4). The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ is the bank's non-interest income ratio in 2011 (defined to be zero for banks not subject to ring-fencing). Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan $\text{Exposure}_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Bank-level controls are interactions between Loan $\text{Exposure}_{l,t} \times \text{Post}_t$ 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 4–7, (Bank $\text{Exposure}^{\text{RFB}}_{2018,i} \times$ Loan $\text{Exposure}_{l,t} \times \text{Post}_t$) is instrumented by (Bank $\text{Exposure}^{\text{RFB}}_{2011,i} \times$ Loan $\text{Exposure}_{l,t} \times \text{Post}_t$). The corresponding first-stage regressions are reported in Table C1. The sample period is January 2010 to December 2019. In column 7, the sample consists of mortgages with maturities of five years or less. 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)
	OLS	OLS	IV	IV
Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{j,t}$ × Post$_t$	0.333***	0.124**		
	(0.069)	(0.054)		
Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{j,t}$ × Post$_t$			0.399***	0.148**
			(0.071)	(0.058)
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Quarter fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Sample start	2010	2015	2010	2015
Observations	325,956	229,816	325,956	229,816
R^2	0.570	0.712	-	-
Kleibergen-Paap F-statistic	-	-	27.0	27.0

Notes: The table shows bank-product-quarter-level regression results for equation (5). The dependent variable is bank i's market share for mortgage product j in quarter t, where product is defined by the combination of maturity quarter and LTV bucket. Bank Exposure$^{\text{RFB}}_{2011,i}$ is the bank's non-interest income ratio in 2011 (defined to be zero for banks not subject to ring-fencing). Bank Exposure$^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan Exposure$_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Bank-level controls are interactions between Loan Exposure$_{j,t}$ × Post$_t$ 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 3 and 4, (Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{j,t}$ × Post$_t$) is instrumented by (Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{j,t}$ × Post$_t$). The sample period is 2010:Q1 to 2019:Q4 (columns 1 and 3) or 2015:Q1 to 2019:Q4 (columns 2 and 4). The sample consists of mortgage products with maturities of five years or less. 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$_{i,l,j,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV	IV	IV	IV
Bank $\text{Exp}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t	-0.922***	-0.861***				
	(0.249)	(0.258)				
Bank $\text{Exp}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t × Long maturity_l	-0.282*					
	(0.151)					
Bank $\text{Exp}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t × High LTV_l		0.525*				
		(0.273)				
Bank $\text{Exp}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t			-0.772***	-0.740***	-0.760***	-0.713***
			(0.196)	(0.185)	(0.209)	(0.203)
Bank $\text{Exp}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t × Long maturity_l			-0.281**	-0.277**		
			(0.139)	(0.134)		
Bank $\text{Exp}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t × High LTV_l					0.566	0.549
					(0.370)	(0.360)
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	No	No	Yes	No	Yes
Observations	4,985,651	4,985,651	4,985,651	4,781,808	4,985,651	4,781,808
R^2	0.842	0.842	-	-	-	-
Kleibergen-Paap F-statistic	-	-	17.0	18.5	13.5	14.1

Notes: The table shows loan-level regression results for equation (4), with additional interaction terms. The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ is the bank's non-interest income ratio in 2011 (defined to be zero for banks not subject to ring-fencing). Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan $\text{Exposure}_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Long maturity_l is an indicator variable for mortgages with maturity greater than two years. High LTV_l is an indicator variable for mortgages with loan-to-value ratio greater than 90%. Bank-level controls are interactions between Loan $\text{Exposure}_{l,t}$ × Post_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 3–6, all interactions involving Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ are instrumented by corresponding interactions involving Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$. The sample period is January 2010 to December 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	OLS	IV	IV
Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{j,t}$ × Post$_t$	0.256***	0.345***		
	(0.078)	(0.069)		
Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{j,t}$ × Post$_t$ × Long maturity$_j$	0.120***			
	(0.027)			
Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{j,t}$ × Post$_t$ × High LTV$_j$		-0.090		
		(0.096)		
Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{j,t}$ × Post$_t$			0.297***	0.415***
			(0.065)	(0.079)
Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{j,t}$ × Post$_t$ × Long maturity$_j$			0.169***	
			(0.057)	
Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{j,t}$ × Post$_t$ × High LTV$_j$				-0.121
				(0.139)
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Quarter fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Observations	325,956	325,956	325,956	325,956
R^2	0.571	0.570	-	-
Kleibergen-Paap F-statistic	-	-	13.7	13.5

Notes: The table shows bank-product-quarter-level regression results for equation (5), with additional interaction terms. The dependent variable is bank i's market share for mortgage product j in quarter t, where product is defined by the combination of maturity quarter and LTV bucket. Bank Exposure$^{\text{RFB}}_{2011,i}$ is the bank's non-interest income ratio in 2011 (defined to be zero for banks not subject to ring-fencing). Bank Exposure$^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan Exposure$_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Long maturity$_j$ is an indicator variable for products with maturity greater than two years. High LTV$_j$ is an indicator variable for products with loan-to-value ratio greater than 90%. Bank-level controls are interactions between Loan Exposure$_{j,t}$ × Post$_t$ 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 3 and 4, all interactions involving Bank Exposure$^{\text{RFB}}_{2018,i}$ are instrumented by corresponding interactions involving Bank Exposure$^{\text{RFB}}_{2011,i}$. The sample period is 2010:Q1 to 2019:Q4. The sample consists of mortgage products with maturities of five years or less. 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. 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:	$HHI_{g,j,t}$			
	(1)	(2)	(3)	(4)
	OLS	OLS	IV	IV
District $\text{Exposure}_{2011,g}$ × Loan $\text{Exposure}_{j,t}$ × Post_t	0.441***	0.263**		
	(0.096)	(0.126)		
District $\text{Exposure}_{2018,g}$ × Loan $\text{Exposure}_{j,t}$ × Post_t			0.380***	0.227**
			(0.085)	(0.107)
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	563,488	563,403	563,488	563,403
R^2	0.543	0.563	-	-
Kleibergen-Paap F-statistic	-	-	1,060.7	1,074.2

Notes: The table shows district-product-quarter-level regression results for equation (7). The dependent variable is the Herfindahl-Hirschman Index (HHI) for mortgage product j in district g in quarter t, where product is defined by the combination of maturity quarter and LTV bucket. District $\text{Exposure}_{2011,g}$ is the volume-weighted average of Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ across banks active in district g in 2011. District $\text{Exposure}_{2018,g}$ is the volume-weighted average of Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ across banks active in district g in 2011; see equation (6). Loan $\text{Exposure}_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing requirements become binding. Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. In columns 3 and 4, (District $\text{Exposure}_{2018,g}$ × Loan $\text{Exposure}_{j,t}$ × Post_t) is instrumented by (District $\text{Exposure}_{2011,g}$ × Loan $\text{Exposure}_{j,t}$ × Post_t). The sample period is 2010:Q1 to 2019:Q4. The sample consists of mortgage products with maturities of five years or less, and district-product-quarter observations where there are at least two loan originations. 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$_{i,l,j,t}$			
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Competitor Exposure$_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$	2.989	3.821	4.684**	5.234**
	(2.291)	(2.307)	(2.251)	(2.134)
Competitor Exposure$_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Long maturity$_l$	-1.238	-0.837		
	(0.894)	(1.369)		
Competitor Exposure$_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × High LTV$_l$			-5.817***	-7.003***
			(1.071)	(1.719)
Loan-level controls	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	Yes	No	Yes
Observations	1,495,489	1,232,772	1,495,489	1,232,772
Kleibergen-Paap F-statistic	525.9	685.0	476.5	571.1

Notes: The table shows loan-level instrumental variable regression results for equation (9), with additional interaction terms. The sample consists only of banks not subject to ring-fencing requirements. The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Competitor Exposure$_{2018,i}$ is a weighted average of District Exposure$_{2018,g}$ based on bank i's mortgage lending portfolio in 2011; see equation (8). This is instrumented by an equivalent weighted average of District Exposure$_{2011,g}$. Loan Exposure$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Long maturity$_l$ is an indicator variable for mortgages with maturity greater than two years. High LTV$_l$ is an indicator variable for mortgages with loan-to-value ratio greater than 90%. Bank-level controls are interactions between Loan Exposure$_{l,t}$ × Post$_t$ 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 December 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Corresponding regressions that use Competitor Exposure$_{2011,i}$ rather than Competitor Exposure$_{2018,i}$ 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)
	OLS	WLS	IV	W2SLS
Competitor $\text{Exposure}_{2011,i}$ × Loan $\text{Exposure}_{j,t}$ × Post_t	-0.148	-0.044		
	(0.114)	(0.029)		
Competitor $\text{Exposure}_{2011,i}$ × Loan $\text{Exposure}_{j,t}$ × Post_t × High LTV_j	0.193	0.204***		
	(0.151)	(0.051)		
Competitor $\text{Exposure}_{2018,i}$ × Loan $\text{Exposure}_{j,t}$ × Post_t			-0.150	-0.046
			(0.115)	(0.030)
Competitor $\text{Exposure}_{2018,i}$ × Loan $\text{Exposure}_{j,t}$ × Post_t × High LTV_j			0.207	0.216***
			(0.158)	(0.058)
Bank-level controls	Yes	Yes	Yes	Yes
Product × Quarter fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Observations	47,547	47,547	47,547	47,547
R^2	0.629	0.817	-	-
Kleibergen-Paap F-statistic	-	-	151.5	192.8

Notes: The table shows bank-product-quarter-level regression results for equation (10). The sample consists only of banks not subject to ring-fencing requirements. The dependent variable is the share of product j in bank i's mortgage lending portfolio in quarter t, where product is defined by the combination of maturity quarter and LTV bucket. Competitor $\text{Exposure}_{2011,i}$ is a weighted average of District $\text{Exposure}_{2011,g}$ based on bank i's mortgage lending portfolio in 2011. Competitor $\text{Exposure}_{2018,i}$ is a weighted average of District $\text{Exposure}_{2018,g}$ based on bank i's mortgage lending portfolio in 2011; see equation (8). Loan $\text{Exposure}_{j,t}$ is the proportion of product j's maturity that falls after January 2019, when ring-fencing requirements become binding. Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. High LTV_j is an indicator variable for products with loan-to-value ratio greater than 90%. Bank-level controls are interactions between Loan $\text{Exposure}_{j,t}$ × Post_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 3 and 4, all interactions involving Competitor $\text{Exposure}_{2018,i}$ are instrumented by corresponding interactions involving Competitor $\text{Exposure}_{2011,i}$. In columns 2 and 4, observations are weighted by bank i's total mortgage lending in quarter t (summed across all products). The sample period is 2010:Q1 to 2019:Q4. The sample consists of mortgage products with maturities of five years or less. 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

Dependent variable:	Log(Loan size)$_{i,l,t}$					
	Observed loan size			Imputed loan size		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$	-0.437***			-0.146***		
	(0.156)			(0.047)		
Bank Exposure$^{\text{NRFB}}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$		-0.639***	-0.524***		-0.215***	-0.189***
		(0.219)	(0.192)		(0.070)	(0.068)
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	≤5-year	Full	Full	≤5-year
Observations	128,438	128,438	102,223	317,844	317,844	232,285
R^2	0.971	-	-	0.981	-	-
Kleibergen-Paap F-statistic	-	83.6	98.8	-	94.3	92.1

Notes: The table shows loan-level regression results for equation (11). The dependent variable is the log of the amount of credit extended by bank i in loan facility l in month t. In columns 1–3, this is measured based on observed lender shares in DealScan. In columns 4–6, missing lender shares are imputed by dividing the total loan size equally among lead arrangers. Bank Exposure$^{\text{NRFB}}_{2011,i}$ is the bank's retail deposit share in 2011 (defined to be zero for banks not subject to ring-fencing). Bank Exposure$^{\text{NRFB}}_{2018,i}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing (measured at end-2018). Loan Exposure$_{l,t}$ is the proportion of loan facility l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. In columns 2, 3, 5, and 6, (Bank Exposure$^{\text{NRFB}}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$) is instrumented by (Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$). The corresponding first-stage regressions are reported in Table C4. The sample period is January 2010 to December 2019. In columns 3 and 6, the sample consists of loan facilities with maturities of five years or less. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table 12: Effect of ring-fencing on syndicated lending – by loan type

Dependent variable:	Log(Loan size)$_{i,l,t}$			
	Observed loan size		Imputed loan size	
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Bank Exposure$^{NRFB}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Term loan$_l$	-0.492**		-0.177***	
	(0.196)		(0.066)	
Bank Exposure$^{NRFB}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Non-term loan$_l$	-0.715***		-0.243***	
	(0.202)		(0.070)	
Bank Exposure$^{NRFB}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Leveraged loan$_l$		-0.626***		-0.152***
		(0.202)		(0.054)
Bank Exposure$^{NRFB}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Non-leveraged loan$_l$		-0.651***		-0.230***
		(0.219)		(0.069)
Difference between coefficients	-0.223***	-0.025	-0.066*	-0.078*
	(0.082)	(0.174)	(0.035)	(0.040)
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes
Bank × Loan-category fixed effects	Yes	Yes	Yes	Yes
Observations	127,851	128,283	316,879	317,634
Kleibergen-Paap F-statistic	42.0	37.3	39.2	38.2

Notes: The table shows loan-level regression results for equation (11), with additional interaction terms. The dependent variable is the log of the amount of credit extended by bank i in loan facility l in month t. In columns 1 and 2, this is measured based on observed lender shares in DealScan. In columns 3 and 4, missing lender shares are imputed by dividing the total loan size equally among lead arrangers. Bank Exposure$^{NRFB}_{2018,i}$ is the amount by which bank i's retail funding share decreases upon implementation of ring-fencing (measured at end-2018). Loan Exposure$_{l,t}$ is the proportion of loan facility l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Bank × loan-category fixed effects are the interaction between bank indicator variables and indicator variables for term loans (columns 1 and 3) and leveraged loans (columns 2 and 4). 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. All interactions involving Bank Exposure$^{NRFB}_{2018,i}$ are instrumented by corresponding interactions involving Bank Exposure$^{NRFB}_{2011,i}$. The sample period is January 2010 to December 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively. Corresponding regressions that use Bank Exposure$^{NRFB}_{2011,i}$ rather than Bank Exposure$^{NRFB}_{2018,i}$ are shown in Appendix C.

APPENDICES

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 GFC.

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 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. Inspired in part by the UK ring-fence, FDIC Vice Chairman Thomas Hoenig (2017) proposed a "partition" of depository and investment banks into separately capitalised and managed intermediate holding companies.

Since the global financial crisis, legislators in other jurisdictions have also proposed structural reforms aimed at better insulating retail banking from investment banking (Financial Stability Board, 2014). Most of these proposals, including the EU's Liikanen (2012) Report, followed a similar "middle ground" approach and did not propose 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. We argue that this 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 (Bank Exposure_i) and within banks (Loan $\text{Exposure}_{l,t}$). 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 Bank Exposure_i; (ii) it would need to vary across loan maturities and time in a way that is correlated with Loan $\text{Exposure}_{l,t}$; 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 instruments for Bank Exposure_i.

As discussed in Section 2.2, 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 the share of a loan's maturity that falls after January 2019 (criterion ii).

In the remainder of this appendix 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 to be more important 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 ambiguous. 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.[29] As for Bank Exposure_i, we measure this difference at the end of 2018, and interact it with Loan $\text{Exposure}_{l,t} \times \text{Post}_t$.[30] The results

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

[30] We cannot measure the difference earlier than end-2018 because the RFBs and NRFBs did not exist

for equation (4) including this additional control variable are reported in Table B1, column 1. Our main result is robust.

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 $\text{Loan Exposure}_{l,t} \times \text{Post}_t$. Our main result is robust to including this additional control variable (Table B1, column 2).

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 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

as distinct entities before this point.

channel, we interact Loan $\text{Exposure}_{l,t} \times \text{Post}_t$ with the difference between the RFB LCR and group LCR. Again, our main result is robust to including this additional control (Table B1, column 3).

Changes in LTV-specific capital requirements on mortgages

Our baseline regressions include 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 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 B1, column 4). 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 B1: Effect of ring-fencing on mortgage spreads – Alternative channels

Dependent variable:	Interest rate spread$_{i,l,j,t}$			
	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$	-0.474***	-0.649***	-0.468***	-0.648**
	(0.177)	(0.172)	(0.168)	(0.217)
ΔCredit rating$^{\text{RFB}}_i$ × Loan Exposure$_{l,t}$ × Post$_t$	0.435***			
	(0.065)			
ΔLeverage ratio$^{\text{RFB}}_i$ × Loan Exposure$_{l,t}$ × Post$_t$		0.360**		
		(0.147)		
ΔLCR$^{\text{RFB}}_i$ × Loan Exposure$_{l,t}$ × Post$_t$			-0.015***	
			(0.002)	
Loan-level controls	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Location × Month fixed effects	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	IRB
Observations	4,781,808	4,781,808	4,781,808	4,217,748
Kleibergen-Paap F-statistic	44.3	100.9	35.1	43.2

Notes: The table shows loan-level regression results for equation (4), with additional control variables. The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Bank Exposure$^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan Exposure$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. ΔCredit rating$^{\text{RFB}}_i$ is the difference between the RFB credit rating and the group credit rating as of end-2018. ΔLeverage ratio$^{\text{RFB}}_i$ is the difference between the RFB regulatory leverage ratio and the group regulatory leverage ratio as of 2019:Q1. ΔLCR$^{\text{RFB}}_i$ is the difference between the RFB liquidity coverage ratio (LCR) and the group LCR as of 2019:Q1. Bank-level controls are interactions between Loan Exposure$_{l,t}$ × Post$_t$ 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. (Bank Exposure$^{\text{RFB}}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$) is instrumented by (Bank Exposure$^{\text{RFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$). The sample period is January 2010 to December 2019. In column 4, 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 First-stage regressions and further robustness tests

Table C1: First-stage for mortgage spread regressions

Dependent variable:	Bank Exposure$^{RFB}_{2018,i}$ × Loan Exposure$_{l,t}$ × Post$_t$			
	(1)	(2)	(3)	(4)
Bank Exposure$^{RFB}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$	1.155***	1.155***	1.151***	1.168***
	(0.188)	(0.188)	(0.180)	(0.181)
Loan-level controls	No	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	No	Yes	Yes
Sample	Full	Full	Full	≤5-year
Observations	4,996,279	4,985,651	4,781,808	4,690,228
Kleibergen-Paap F-statistic	37.8	37.8	40.9	41.4

Notes: The table shows the first-stage regressions corresponding to columns 4–7 of Table 3.

Table C2: Effect of ring-fencing on mortgage spreads – Affected banks only

Dependent variable:	Interest rate $\text{spread}_{i,l,j,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	IV	IV	IV	IV
Bank $\text{Exp}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t	-2.038***	-1.804***	-1.678***				
	(0.401)	(0.394)	(0.355)				
Bank $\text{Exp}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exp}_{l,t}$ × Post_t				-1.650***	-1.460***	-1.366***	-1.393***
				(0.259)	(0.246)	(0.238)	(0.246)
Loan-level controls	No	Yes	Yes	No	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	No	Yes	No	No	Yes	Yes
Sample	Full	Full	Full	Full	Full	Full	≤5-year
Observations	3,418,755	3,412,282	3,185,141	3,418,755	3,412,282	3,185,141	3,137,357
R^2	0.805	0.835	0.859	-	-	-	-
Kleibergen-Paap F-statistic	-	-	-	73.7	73.9	83.9	82.0

Notes: The table shows loan-level regression results for equation (4). The sample consists only of banks subject to ring-fencing requirements. The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ is the bank's non-interest income ratio in 2011 (defined to be zero for banks not subject to ring-fencing). Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ is the increase in bank i's retail funding share upon implementation of ring-fencing (measured at end-2018). Loan $\text{Exposure}_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post_t is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Bank-level controls are interactions between Loan $\text{Exposure}_{l,t}$ × Post_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 4–7, (Bank $\text{Exposure}^{\text{RFB}}_{2018,i}$ × Loan $\text{Exposure}_{l,t}$ × Post_t) is instrumented by (Bank $\text{Exposure}^{\text{RFB}}_{2011,i}$ × Loan $\text{Exposure}_{l,t}$ × Post_t). The sample period is January 2010 to December 2019. In column 7, the sample consists of mortgages with maturities of five years or less. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table C3: Effect of ring-fencing on mortgage spreads for banks not subject to ring-fencing (2011 measure of Competitor Exposure)

Dependent variable:	Interest rate spread$_{i,l,j,t}$			
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Competitor Exposure$_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$	2.866	3.648	4.464**	4.950**
	(2.201)	(2.191)	(2.181)	(2.056)
Competitor Exposure$_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Long maturity$_l$	-1.176	-0.785		
	(0.865)	(1.305)		
Competitor Exposure$_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × High LTV$_l$			-5.601***	-6.758***
			(1.042)	(1.680)
Loan-level controls	Yes	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes	Yes
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Product × Month fixed effects	Yes	Yes	Yes	Yes
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Location × Month fixed effects	No	Yes	No	Yes
Observations	1,495,489	1,232,772	1,495,489	1,232,772
R^2	0.875	0.904	0.875	0.904

Notes: The table shows loan-level regression results for equation (9), with additional interaction terms. The sample consists only of banks not subject to ring-fencing requirements. The dependent variable is the interest rate spread (over OIS) on mortgage l originated by bank i in month t. Subscript j refers to the product category into which the mortgage falls, defined by the combination of maturity month and LTV bucket. Competitor Exposure$_{2011,i}$ is a weighted average of District Exposure$_{2011,g}$ based on bank i's mortgage lending portfolio in 2011; see equation (8). Loan Exposure$_{l,t}$ is the proportion of loan l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Long maturity$_l$ is an indicator variable for mortgages with maturity greater than two years. High LTV$_l$ is an indicator variable for mortgages with loan-to-value ratio greater than 90%. Bank-level controls are interactions between Loan Exposure$_{l,t}$ × Post$_t$ 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 December 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.

Table C4: First-stage for syndicated lending regressions

Dependent variable:	Bank $\text{Exposure}^{\text{NRFB}}_{2018,i}$ × Loan $\text{Exposure}_{l,t}$ × Post_t			
	Observed loan size		Imputed loan size	
	(1)	(2)	(3)	(4)
Bank $\text{Exposure}^{\text{NRFB}}_{2011,i}$ × Loan $\text{Exposure}_{l,t}$ × Post_t	0.684***	0.672***	0.681***	0.679***
	(0.075)	(0.068)	(0.070)	(0.071)
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes
Sample	Full	≤5-year	Full	≤5-year
Observations	128,438	102,223	317,844	232,285
Kleibergen-Paap F-statistic	83.6	98.8	94.3	92.1

Notes: The table shows the first-stage regressions corresponding to columns 2, 3, 5, and 6 of Table 11.

Table C5: Effect of ring-fencing on syndicated lending – by loan type (2011 measure of Bank Exposure)

Dependent variable:	Log(Loan size)$_{i,l,t}$			
	Observed loan size		Imputed loan size	
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Term loan$_l$	-0.339**		-0.120***	
	(0.136)		(0.038)	
Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Non-term loan$_l$	-0.487***		-0.166***	
	(0.136)		(0.043)	
Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Leveraged loan$_l$		-0.434***		-0.102***
		(0.133)		(0.037)
Bank Exposure$^{\text{NRFB}}_{2011,i}$ × Loan Exposure$_{l,t}$ × Post$_t$ × Non-leveraged loan$_l$		-0.441***		-0.155***
		(0.160)		(0.049)
Difference between coefficients	-0.149***	-0.006	-0.045*	-0.053*
	(0.051)	(0.117)	(0.027)	(0.029)
Bank × Month fixed effects	Yes	Yes	Yes	Yes
Loan facility fixed effects	Yes	Yes	Yes	Yes
Bank × Loan-category fixed effects	Yes	Yes	Yes	Yes
Observations	127,851	128,283	316,879	317,634
R^2	0.971	0.971	0.981	0.981

Notes: The table shows loan-level regression results for equation (11), with additional interaction terms. The dependent variable is the log of the amount of credit extended by bank i in loan facility l in month t. In columns 1 and 2, this is measured based on observed lender shares in DealScan. In columns 3 and 4, missing lender shares are imputed by dividing the total loan size equally among lead arrangers. Bank Exposure$^{\text{NRFB}}_{2011,i}$ is the bank's retail deposit share in 2011 (defined to be zero for banks not subject to ring-fencing). Loan Exposure$_{l,t}$ is the proportion of loan facility l's maturity that falls after January 2019, when ring-fencing requirements become binding. Post$_t$ is an indicator variable equal to one from December 2013, when ring-fencing legislation is adopted. Bank × loan-category fixed effects are the interaction between bank indicator variables and indicator variables for term loans (columns 1 and 3) and leveraged loans (columns 2 and 4). 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 December 2019. Standard errors are reported in parentheses and clustered by bank. *, ** and *** indicate significance at 10%, 5% and 1%, respectively.