A Public-Private Partnership? Central Bank Funding and Credit Supply*

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Abstract

We exploit the surprise announcement of a central bank funding scheme to test how public liquidity provision affects credit market outcomes. Contrary to the notion that public liquidity is primarily a substitute for private liquidity, banks that are more exposed to stress in private wholesale funding markets use *less* central bank funding. We rationalise this pattern by establishing an "equilibrium channel" of public liquidity. The mere *availability* of central bank funding reduces the cost of private wholesale funding. This stimulates lending by banks exposed to wholesale funding, regardless of whether they actually use the central bank funding. Using a shock to the design of the scheme, we show that the "strings attached" to central bank funding help to explain why it is an imperfect substitute for private funding.

Keywords: Central bank funding, Mortgage lending, Bank funding risk.

JEL classification: E52, E58, G21.

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1 Introduction

Public authorities can improve credit market outcomes by supplying liquidity to agents when private liquidity supply is subject to frictions (Holmström & Tirole 1998). One real-world test for this idea is the large-scale provision of funding by central banks in response to stress in private wholesale funding markets.

In that case, most obviously, banks can use public funding as a *substitute* for stressed private funding, and this can boost their lending to the real economy. However, such substitution could have a range of side-effects. For instance, the transfer of private risk to the public sector could create moral hazard (Bolton et al. 2009), and public funds could support bank activities other than lending to the real economy. And if reducing such leaks requires adding "strings attached", this could make public funding less attractive to banks and thus less effective at stimulating lending (Farhi & Tirole 2021, Bernanke 2022).

However, a less obvious possibility is that public liquidity acts as a *complement* to private liquidity, e.g. because the mere availability of a public outside option helps to resolve frictions in private liquidity supply (Farhi & Tirole 2012, Philippon & Skreta 2012). Such an "equilibrium effect" could help to improve credit market outcomes without public liquidity actually being used, thus mitigating the potential side-effects from substitution into public funding.

The main contribution of this paper is to establish the existence, drivers, and consequences of this equilibrium effect. To do so, we exploit the surprise announcement of a Bank of England funding scheme, which was launched in response to stress in wholesale funding markets and offered banks access to long-term funding, conditional on banks' lending to households and firms. Exploiting confidential loan-level data, we quantify the impact of this announcement on credit supply via an equilibrium effect, while controlling for the direct impact on users of the public liquidity ("participation effect") that most

research has focused on to date.

Overall, our results suggest that the equilibrium effect is the dominant channel through which central bank funding stimulates lending, and that this effect allows banks to enjoy the benefits of central bank funding while avoiding its costs. Contrary to the notion that public liquidity is primarily a substitute for private liquidity, we show that banks more exposed to stressed wholesale funding markets are *less* likely to use the scheme. In line with models where public funding availability alleviates private market frictions (Farhi & Tirole 2012, Philippon & Skreta 2012), wholesale funding rates fall sharply in response to the mere announcement of the scheme. We show that banks more exposed to stressed funding markets reduce loan rates by more after the announcement—irrespective of how much they use the scheme. While participation in the scheme also leads to lower lending rates, this effect is substantially smaller than the equilibrium effect in aggregate. The equilibrium effect appears to operate through a reduction in perceptions of banks' funding risk, rather than through an increase in their bargaining power in funding markets. Finally, using a surprise change to the terms of the scheme, we show that the conditionality ("strings attached") to central bank funding can be a significant (non-pecuniary) cost of using public liquidity relative to private funding.

The Funding for Lending Scheme (FLS) was announced in June 2012, when the Eurozone crisis was escalating and UK banks' wholesale funding costs were reaching levels last seen during the Global Financial Crisis. Under the FLS, UK banks could get four-year loans from the BoE. To incentivise banks to use the funding for lending to the real economy, the quantity and price of funding was conditional on banks' lending to households and firms—a design that was subsequently adopted by the ECB's Targeted Long-Term Refinancing Operations (TLTROs). However, unlike the TLTROs and most other recent central bank funding schemes, the announcement of the FLS did not coincide with policy rate cuts, asset purchase announcements, or government credit support schemes, which

facilitates identification. In addition, the FLS was subsequently extended and amended, which helps us to identify the importance of conditionality and the role of different transmission channels in stressed vs. normal periods.

Our analysis starts by examining how participation in the FLS varies with banks' pre-announcement exposure to wholesale funding. If FLS funding is mainly a substitute for wholesale funding, banks more exposed to wholesale funding should borrow *more* from the scheme. By contrast, if the FLS mainly works through an "equilibrium channel" whereby the mere availability of public funding improves conditions in private wholesale funding markets, banks more exposed to wholesale funding might have *less* need to borrow directly from the scheme, since they would benefit from the improvement in private wholesale funding conditions. Our results are in line with the second hypothesis: a 10 percentage point increase in a bank's wholesale funding exposure is associated with a 0.6 percentage point reduction in FLS borrowing (as a proportion of initial borrowing allowances).

Motivated by this pattern, our main empirical analysis examines the evidence for an equilibrium effect of public liquidity and its impact on bank lending. Indicators of UK banks' wholesale funding costs fall sharply from their stressed levels when the FLS is announced, in line with the idea that public liquidity availability alleviates frictions in private liquidity supply. The main focus of our empirical analysis is to estimate how this improvement in wholesale funding conditions affects bank lending. Importantly, we separate this "equilibrium effect" from a "participation effect", i.e. the potential effect on bank lending from a bank's direct participation in the funding scheme, which previous literature has found to be significant (Benetton et al. 2021). To identify the "equilibrium effect", we run loan-level difference-in-differences regressions, where we exploit predetermined heterogeneities in wholesale funding reliance while controlling for confounding trends with granular fixed effects and a host of controls.[1] To control for the participation effect, we

[1] Carpinelli & Crosignani (2021) also use measures of funding structure to estimate the impact of central bank funding schemes. However, they do not distinguish between the equilibrium and participation effects.

use banks' initial FLS borrowing capacity (which is measured before the announcement) as an instrument for realised FLS take-up in the spirit of Benetton & Fantino (2021).

We find that, relative to a bank without any wholesale funding, a bank with a wholesale funding reliance of 32% (our sample average) would reduce mortgage spreads by around 68 basis points after the FLS announcement. This suggests that the equilibrium effect is economically large. To put this effect into context, over the months leading up to the announcement of the FLS, mortgage spreads had risen by around 60 basis points. Importantly, this equilibrium effect remains large and statistically significant when we control for the participation effect.

Our results suggest that for a medium-sized bank, the equilibrium and participation effects have similar impacts on lending. In the UK, however, a small number of large banks account for the majority of aggregate lending. And for large banks, the equilibrium effect is significantly larger than the participation effect, implying that the equilibrium effect is the dominant channel of transmission. This suggests that only accounting for the participation effect is likely to significantly underestimate the overall impact of central bank funding.

The equilibrium effect could be explained by two (non-mutually exclusive) mechanisms. First, the availability of a risk-insensitive public funding option could reduce banks' funding liquidity risk, and hence the reduce risk premia required by private wholesale lenders when providing funding to banks ("risk channel"). Second, the existence of a public outside funding option could reduce banks' (expected) demand for private wholesale funding and therefore reduce the mark-up that wholesale lenders can charge on this funding ("demand channel").

Exploiting granular confidential data on banks' liabilities structures, we find evidence in line with the risk channel but not the demand channel. In particular, we find that the negative relationship between wholesale funding reliance and mortgage spreads after

the FLS is driven by exposure to short-term wholesale funding (which exposes banks to greater funding risk), and not exposure to stickier long-term funding. In addition, the equilibrium effect is significant when the FLS is first announced in 2012, when wholesale funding markets were stressed, but not when a new FLS program ("FLS2") is announced in 2013, when wholesale funding costs had returned to normal levels.

Taken together, our results are consistent with the idea that public liquidity "creates its own competition" (Farhi & Tirole 2021). By indirectly lowering the price of private funding, the equilibrium effect allows banks to benefit from central bank funding without having to actually use this funding. This helps to explain why banks that stand to benefit more from the equilibrium effect are less likely to use central bank funding.

The existence of a substantial equilibrium effect allows banks to not only indirectly reap the benefits of central bank funding, but also to avoid any non-pecuniary costs associated with using this funding directly. In the last part of the paper, we look for evidence for such costs. Well-known costs from using public funding include stigma (Philippon & Skreta 2012) and political pressure (Chavaz & Rose 2019). Instead, we explore a cost that has attracted less attention to date: if authorities attach conditions to public liquidity, this might constrain banks' ability to deploy it towards the most profitable uses.

Our setting provides an ideal laboratory to test the importance of these "strings attached" because conditionality was a central innovation behind the FLS, and because subsequent changes to the program create two important shocks to the reach of this conditionality.

First, in April 2013, the BoE announced a second wave of FLS funding ("FLS2"), which would start in February 2014. The design of FLS2 implied variation over time in the conditionality of the funding. During the transition period between FLS1 and FLS2, new mortgages could still be funded with FLS1 drawings, but would also generate "initial allowances" for future FLS2 drawings. Importantly, these future drawings could be used

to finance any asset; therefore, FLS2 drawings based on initial allowances constitute *unconditional* funding. In contrast, after February 2014, FLS2 drawings could only be unlocked by originating new loans to households or firms, thereby constituting *conditional* funding. Therefore, if banks find conditionality costly, they should have an incentive to unlock future unconditional funding by originating more mortgages during the transition period. In line with this idea, we find that during this transition period, banks more reliant on FLS funding reduce spreads more on new mortgages.

Second, in November 2013, the BoE unexpectedly amended the terms of FLS2. In order to incentivise corporate lending, mortgage lending during 2014 would no longer increase FLS2 borrowing allowances. We find that this amendment reduces the impact of FLS participation on mortgage spreads, consistent with the conditionality of FLS2 funding significantly reducing its impact on lending. In addition, during the short time window before the amendment becomes binding, we find that banks more reliant on FLS funding reduce mortgage spreads further, consistent with an attempt to secure future FLS borrowing allowances before conditionality becomes tighter.

Together, these results suggest that conditionality matters, and that banks prefer public liquidity with fewer strings attached. This suggests a trade-off in the design of central bank funding schemes. Looser conditionality makes central bank funding a closer substitute to private funding, which is likely to strengthen the equilibrium effect and hence allow the central bank to support credit provision without taking risk onto its own balance sheet. However this also weakens the central bank's ability to use the scheme to target specific sectors.

Relation to existing literature Our main contribution is to show evidence for an "equilibrium effect" that allows banks to benefit from central bank funding while avoiding its costs. So far, empirical studies have mostly focused on comparing participants and non-participants. These studies find that this "participation effect" boosts credit supply,

in line with our findings (for example, Benetton & Fantino 2021, Benetton et al. 2021). However, we show that the equilibrium effect makes a larger aggregate contribution than the participation effect. This suggests that funding schemes can be significantly more powerful than previously thought.[2] However, we also show that the equilibrium channel only operates in stressed market conditions. Our results complement those of Carpinelli & Crosignani (2021), who also find that banks more exposed to wholesale funding increase lending in response to ECB long-term refinancing operations, without distinguishing between the participation and equilibrium effects.

The equilibrium effect we establish complements two related but distinct effects of central bank liquidity provision. First, Andreeva & García-Posada (2021) find that banks whose competitors make greater use of TLTROs are more likely to report an easing in credit standards. The authors attribute this finding to the idea that participants substitute deposit funding for TLTROs, which lowers non-participants' deposit funding costs. In contrast, our equilibrium effect can operate even if there is no actual take-up of FLS funding, and works through a reduction in bank funding risk, rather than a change in deposit market competition.

Second, Minoiu et al. (2021) study the 2020 Main Street Lending Program, which allowed banks to sell eligible business loans to the Federal Reserve. Even if loan sales were low, participant banks still increased the supply of business loans. The authors attribute this effect to the idea that the option to sell loans reduces banks' risk aversion and expected balance sheet constraints. In contrast, we study a program where targeted loans must be retained by the originator, which implies that our results cannot be explained by the option to sell loans. In line with Farhi & Tirole (2021), our mechanism can also extend to all banks (participants and non-participants) via conditions in wholesale funding markets,

[2] Churm et al. (2021) also document that the FLS had a strong announcement effect on indicators of major UK banks' wholesale funding costs. They estimate the aggregate implications of this effect using time-series methods, whereas we exploit loan-level data for identification. Using a structural model, Albertazzi et al. (2022) estimate that the TLTROs reduced the probability of runs on European banks.

rather than only to participant banks. In addition, we can disentangle the equilibrium and participation effects, and study how these effects vary with market conditions, which have important implications for when and how funding schemes are likely to be most effective.[3]

Implications for policy debates One important question is whether the success of central bank lending schemes depends on ample take-up (Bernanke 2022, BIS 2023). Our results suggest that focusing on take-up risks significantly underestimating the impact of these scheme; this is because, the more successful the scheme is at "rejuvenating" funding markets (Farhi & Tirole 2021), the smaller participation might be. Therefore, funding-for-lending can be powerful in stimulating credit supply even if take-up is low.

Our findings also nuance concerns that central bank lending "crowds out" private funding (Bolton et al. 2009), resulting in a transfer of private risk to the (consolidated) public sector balance sheet and hence moral hazard (Flanagan 2019). In line with this concern, LTRO participants partly used long-term central bank funding to replace private wholesale funding (Carpinelli & Crosignani 2021). Our results are more consistent with a "crowding in" effect whereby the availability of central bank funding improves access to private funding. While banks more exposed to riskier private funding benefit more from this effect, the associated risk is not transferred to the central bank; instead the central bank indirectly helps to "rejuvenate" private funding markets.

Another debate is whether funding schemes meet their stated objective of steering the benefits of central bank funding to targeted sectors, therefore mitigating "leakages" associated with earlier unconditional long-term refinancing operations (Acharya & Steffen 2015, Crosignani et al. 2020). This question is important because there is substantial

[3]Our focus on the impact of the *announcement* of the Funding for Lending Scheme, rather than the effect of actual lending operations, echoes a large literature studying the effects of central bank asset purchase announcements (e.g., Gagnon et al. 2011, Boyarchenko et al. 2022).

variation across programs in terms of conditionality, both across and within central banks.[4] Our results point to significant limits to central banks' ability to use conditionality to steer funding towards selected sectors. By design, conditionality only binds for banks borrowing from the scheme, and not for those that benefit indirectly without participating. This points to a trade-off in designing funding schemes: a scheme with weaker conditionality is likely to be a closer substitute for private funding, which should reinforce the "equilibrium effect" and hence make the scheme more powerful overall, but at the cost of greater leakages beyond targeted sectors.

2 Datasets and sample

Product Sales Database Our main source of data on bank lending is the Product Sales Database (PSD), a confidential regulatory loan-level dataset collected by the UK Financial Conduct Authority (FCA) that covers all residential mortgages originated in the UK. For each loan, we observe the name of the lender, as well as a range of loan characteristics including: the borrower income, age, credit history, and type (first-time buyer, home mover, refinancer); the property location and type; and the mortgage origination date, size, initial interest rate, fixation period, loan-to-value (LTV) ratio, loan-to-income (LTI) ratio, and term.

Unlike the US Home Mortgage Disclosure Act (HMDA) dataset, PSD does not report loan sales. However, the overwhelming majority of UK mortgages are retained during our sample period (Chavaz & Elliott 2023). UK mortgages also typically have a short "fixation period" (typically 2 to 5 years). After this period, the interest rate switches to a variable rate that significantly exceeds the original interest rate; the vast majority

[4]For instance, TLTRO-I allowances were partly linked to net new lending to corporates, but not TLTRO-II. Meanwhile TLTRO and FLS2 targeted only corporate loans, whereas FLS1 targeted both mortgages and corporate loans. These variations suggest that authorities view conditionality as important, but existing evidence on the impact of conditionality is limited.

of borrowers therefore refinance at this point (Cloyne et al. 2019). Unlike in the US, borrower characteristics play little role in the pricing of mortgages in the UK. Instead, pricing is based almost entirely on the fixation period and LTV ratio (Robles-Garcia 2019, Benetton et al. 2021); in the remainder of the paper, we therefore refer to the combination of fixation period and LTV ratio as the mortgage "product". Rates available for different mortgage products are published transparently by all banks, and contracted mortgage rates are similar to advertised rates.

Our main analysis of the effect of the FLS on mortgage lending uses mortgages originated between January 2012 and June 2013, which covers six months before the announcement of the FLS in June 2012 and one year after. We end the sample in June 2013 because the second FLS program ("FLS2") is announced at this time. When we analyse the impact of further announcements, we extend the sample further (see Section 7). We focus on vanilla fixed rate and adjustable rate mortgages.

Bank-level data We match PSD to quarterly regulatory data on bank balance sheets and income statements from the Bank of England, as well as bank-level data on FLS drawdowns and borrowing allowances. We use these datasets to construct our measures of the equilibrium effect and participation effect (discussed in Section 4.3) as well as bank-level control variables. After matching PSD to the bank-level variables, our baseline sample consists of 415,671 mortgages.

Other datasets Our additional tests use three supplementary data sources. First, we use a dataset of mortgage products advertised by all UK banks collected by Moneyfacts. For every mortgage product, the dataset reports the mortgage rate and fee, among other information. Since PSD only partially reports information on mortgage fees, we use Moneyfacts data to control for the role of fees. Second, we use a confidential Bank of England regulatory dataset (FSA047/048) which provides granular data on the maturity

structure of bank balance sheets. For each bank, we observe the outstanding balance for different asset and liability categories, broken down by remaining maturity. We use this information to shed more light on the mechanism behind our key result. Finally, to control for confounding euro area developments, we use data on CDS prices of euro area sovereigns and banks from Bloomberg.

3 The Funding for Lending Scheme

3.1 Original Funding for Lending Scheme ("FLS1")

In a speech given on 14 June 2012, Governor Mervyn King announced that the Bank of England would launch a Funding for Lending Scheme (FLS) jointly with the UK government.[5] The stated ambition was to "prevent an aggregate deleveraging of the banking system that might hold back recovery" by reducing "risk premia and bank funding costs". This was against the backdrop of a "deterioration in the outlook" for the UK economy, driven in large part by the euro area debt crisis. The speech set out the key features of the scheme, i.e. the provision of "funding to banks for an extended period of several years, at rates below current market rates and linked to the performance of banks in sustaining or expanding their lending to the UK non-financial sector".

The details of the scheme were published on 13 July 2012 in a joint statement by the Bank of England and Her Majesty's Treasury (HMT).[6] At any time during an 18-month drawdown window starting on 1 August 2012, all banks and building societies with access to the Bank of England's Discount Window Facility (DWF) would be eligible to borrow funds for four years.[7] Loans would be secured by collateral eligible for discount window

[5] https://www.bankofengland.co.uk/-/media/boe/files/speech/2012/mansion-house.pdf

[6] https://www.gov.uk/government/news/bank-of-england-and-hm-treasury-announce-launch-of-funding-for-lending-scheme

[7] Building societies typically have a regional footprint and focus mainly on mortgage lending and deposit-taking.

borrowing, i.e. portfolios of loans, asset-backed securities, covered bonds, and sovereign and central bank debt (Churm et al. 2012).

In line with its stated ambition, the FLS was designed to incentivise lending to the real economy. Specifically, the terms of borrowing were conditioned on a bank's lending performance via both a quantity-based and price-based mechanism.

Under the quantity-based mechanism, the maximum amount that a bank could borrow was the sum of an "initial allowance" and "additional allowance." The initial allowance was set to 5% of the bank's stock of lending to households and non-financial businesses as of June 2012. Banks could draw down on their initial allowance as soon as the scheme opened or any time thereafter. The additional allowance was set equal to the bank's net lending to households and non-financial businesses over the period July 2012 to December 2013. Therefore, additional allowances could be built up over the course of the scheme. In principle, there was no limit to the additional allowance that a bank could generate via new lending.

Importantly, the existence of both initial and additional allowances generates heterogeneity in the conditionality of FLS funding. Funding obtained via *initial* allowances could be used to fund any asset, including loans to sectors not targeted by the FLS (such as financial firms). This could therefore be considered *unconditional* funding. In contrast, funding obtained via *additional* allowances can be considered *conditional* funding, since additional allowances could only be generated by new lending to the targeted sectors, and hence could effectively only fund loans to these sectors. We exploit this heterogeneity in conditionality between initial and additional allowances in Section 7.

Turning to the pricing-based mechanism, the cost of borrowed FLS funds would decrease with the bank's net lending to households and firms during the drawdown window. If a bank maintained or expanded its stock of eligible lending, it would pay an annual fee of only 25 basis points; instead if lending declined, the fee would increase linearly

to a maximum of 150 basis points. That pricing effectively ensured that, as long as bank lending grew, the cost of FLS funding would be lower than the cost of private funding—abstracting from non-pecuniary costs such as stigma or costs associated with conditionality.[8] The pricing also meant that the cost of FLS funding would not vary with a bank's riskiness, unlike the cost of funding from private markets.

3.2 Comparison with other schemes

Prior to the launch of the FLS in 2012, other central banks had deployed schemes providing long-term funding to banks, for example the ECB's Longer-Term Refinancing Operations (LTROs) launched in 2011. The key innovation of the FLS was to explicitly design the scheme to incentivise banks to use central bank funding to lend to households and firms. Several subsequent schemes have adopted a similar approach; this includes the ECB's Targeted Longer-Term Refinancing Operations (TLTROs), which started in 2014. As for the FLS, under TLTROs banks could borrow funds for several years, and borrowing allowances increased with outstanding and net new eligible loans to households and non-financial firms. However, unlike the original FLS, mortgages did not count towards TLTRO borrowing allowances.[9]

After the FLS, the Bank of England deployed two subsequent funding-for-lending schemes: the 2016 Term Funding Scheme (TFS), launched in response to the Brexit referendum, and the 2020 Term Funding Scheme with additional incentives for SMEs (TFSME), launched in response to Covid-19. Unlike the FLS however, these schemes were launched alongside other major monetary or fiscal policy measures, which compli-

[8] In practice, the FLS lent UK Treasury Bills rather than cash, and so the full cost of FLS funding would incorporate both the FLS fee and the cost of converting the Treasury Bills into cash, for example via repo markets. Churm et al. (2012) estimate that at the time the FLS was announced in June 2012, the all-in cost of FLS funding was around 200 basis points cheaper than comparable sources of wholesale funding such as covered bonds.

[9] As we explain below, mortgages were subsequently excluded from eligible loans for the second wave of FLS funding ("FLS2").

cates identification. Specifically, both the TFS and TFSME were launched alongside new Quantitative Easing purchases and cuts in the policy rate, while the TFSME was also launched alongside other government credit market interventions.

3.3 Extension and amendment ("FLS2")

Our baseline tests exploit the introduction of the original FLS ("FLS1"). However, in further tests, we exploit the subsequent extension of the scheme ("FLS2").

The original FLS1 drawdown window was set to close on 31 January 2014. But on 24 April 2013, the Bank of England and HM Treasury announced that a new one-year drawdown window would open from 1 February 2014. During this FLS2 window, banks' *initial* borrowing allowance would be a function of their net lending to households and non-financial businesses in the last three quarters of 2013 (the FLS2 "reference period"). Similarly to the original FLS1, *additional* allowances would then increase with net new lending to households and businesses during the FLS2 drawdown window.[10]

However, on 28 November 2013, the Bank and HMT announced that (in contrast to the previous announcement) mortgages would not count towards additional FLS2 borrowing allowances. This was motivated by a desire to "re-focus" the benefits of FLS2 towards business lending (especially to SMEs), against a backdrop of rising house prices.[11] These announcements are summarised in Table 1.

4 Hypothesis and identification

In this section, we discuss our key hypothesis for the "equilibrium effect" of central bank funding, and how we identify this effect empirically. However, we first motivate our

[10] https://www.gov.uk/government/news/bank-of-england-and-hm-treasury-announce-extension-to-the-funding-for-lending-scheme. In FLS2, lending to SMEs increased both initial and additional allowances by more than lending to other sectors.

[11] https://www.gov.uk/government/news/bank-of-england-and-hm-treasury-re-focus-the-funding-for-lending-scheme-to-support-business-lending-in-2014

hypothesis by exploring the determinants of bank participation in the FLS.

4.1 FLS participation and wholesale funding exposure

In this section, we investigate the relationship between participation in the FLS and banks' exposure to wholesale funding. The FLS was launched in response to stress in UK wholesale funding markets. If FLS funding is mainly a *substitute* for private market wholesale funding, then banks more exposed to wholesale funding should make *more* use of FLS funding than other banks. However, the opposite could hold if the FLS mainly acts as a *complement* to private funding. In theory, such complementarity could arise if the mere availability of central bank funding helps to alleviate frictions in private liquidity supply (Farhi & Tirole 2021, Philippon & Skreta 2012). For example, the option for banks to obtain central bank funding at a low, risk-insensitive price might reduce risk premia in private wholesale funding, or might increase banks' bargaining power vis-à-vis lenders in private funding markets. In that case, banks more exposed to wholesale funding might have *less* need for FLS funding.

To investigate this relationship, we run simple cross-sectional regressions of FLS participation on exposure to wholesale funding. To measure participation, we consider three different dependent variables. To capture the extensive margin of participation, we construct an indicator variable equal to 1 if the bank participates, and 0 otherwise. For the intensive margin, we measure how much a bank borrows from the scheme, measured either as the bank's average or maximum borrowing amount over the FLS drawdown window (in both cases, we normalise borrowing by the bank's initial borrowing allowance).

Our key explanatory variable is the bank's pre-FLS exposure to wholesale funding, measured as the ratio of wholesale funding to total assets as of 2012:Q1 ($\%(Wholesale)_{i,2012}$). We also control for the bank's log total assets, cash ratio (cash / total assets), capital ratio (capital / total assets), and return on assets (net income / total assets). When the depen-

dent variable is an indicator variable (extensive margin), we use a probit model; for the two continuous dependent variables (intensive margin), we use ordinary least squares.[12]

Table 2 reports summary statistics for the sample. Around half of banks (46%) participate in the FLS. For the average bank, outstanding borrowing is equal to 1.5% of initial allowance in the average quarter during the drawdown window, and peaks at 4.3% of initial allowances (these statistics include both participants and non-participants). For the average bank, the ratio of wholesale funding to total assets is 16.3%, with substantial variation across banks (the standard deviation is 23.9%).

The results are reported in Table 3. For all three dependent variables, a higher exposure to wholesale funding is associated with *lower* participation in the program—both on the intensive and extensive margin. Focusing on the intensive margin (columns 3–6), the estimates suggest that an increase in wholesale funding exposure from 0% to 16.3% (the cross-sectional average) is associated with a reduction in *average* FLS borrowing of nearly 1 percentage point (columns 3 and 4), and a reduction in *peak* FLS borrowing of over 2 percentage points (columns 5 and 6). These results are in line with Fudulache & Goetz (2023), who find that euro area banks more reliant on wholesale funding participate *less* in the ECB's second TLTRO programme.

While our analysis does not allow for a causal interpretation, at face value the results are at odds with the notion that central bank funding is primarily a substitute for private funding. Instead, our results raise the possibility of a complementarity between central bank funding and private funding markets. As discussed above, such complementary could reflect an equilibrium effect whereby banks can benefit indirectly from the availability of FLS funding without actually using it. We now discuss this idea in more detail.

[12]For a causal analysis of the relationship between reliance on long-term deposit funding and participation in the ECB's TLTRO-II program, see Fudulache & Goetz (2023).

4.2 Theory

Motivated by these patterns, the main hypothesis we want to test is that the availability of central bank funding stimulates lending through an "equilibrium effect". That hypothesis has two main parts.

The first part of our hypothesis is that the mere availability of central bank funding reduces private wholesale funding costs. This idea relates to models where the mere availability of public liquidity helps to alleviate frictions in private liquidity supply and therefore reduces the price of private liquidity (Farhi & Tirole 2021, Philippon & Skreta 2012). As discussed in Section 3.1, the FLS was launched in response to stress in private wholesale funding markets, and the cost of FLS funding was designed to fall below the cost of private funding and to be insensitive to the riskiness of the bank. Therefore, if banks view FLS funding as a (perfect or imperfect) substitute for wholesale funding, the mere availability of an outside option (FLS funding) could put downward pressure on the price of wholesale funding. For example, the outside option could increase banks' bargaining power in wholesale funding markets, which could reduce the mark-up charged by wholesale lenders. In addition, the availability of a public funding backstop could reduce banks' rollover risk, and hence reduce the risk premia charged by wholesale lenders.

Figure 1 provides support for the first part of our hypothesis. As the Euro crisis escalates over 2011 and 2012, indicators of wholesale funding costs for UK banks increase sharply. When the FLS is announced, there is a sharp drop in these indicators.[13]

The second part of our hypothesis is that this reduction in wholesale funding costs should lead to lower lending rates, particularly for banks with a greater reliance on wholesale funding. This is the key relationship that we want to test.

[13] Churm et al. (2021) estimate that, after controlling for developments in the euro area, the announcement of the FLS reduced the cost of long-term wholesale funding for major UK banks by around 75 basis points. Weale & Wieladek (2016) also document falls in UK bank funding costs following the FLS announcement.

4.3 Identification

To assess whether the availability of central bank funding affects bank lending rates via an equilibrium effect, we test whether banks more exposed to the fall in wholesale funding costs caused by the announcement of the FLS reduce their lending rates by more than other banks. In doing so, we want to control for any impact of the FLS on bank lending rates via banks' direct participation in the scheme.

We focus on an 18-month sample period (January 2012 to June 2013) around the announcement of the FLS in June 2012. We estimate various forms of the following empirical model:

$$Spread_{i,l,t} = \beta \ \%(Wholesale)_{i,2012} \times PostFLS_t + \gamma \ Controls_i \times PostFLS_t + \theta_{i,p} + \vartheta_{p,t} + \epsilon_{i,l,t}, \tag{1}$$

where $Spread_{i,l,t}$ is the interest rate on mortgage l originated by bank i during month t, net of the maturity-matched risk-free rate, and p refers to mortgage l's product category (discussed further below). $PostFLS_t$ is an indicator variable equal to 1 after the announcement of the FLS in June 2012, and 0 otherwise. We use the date that the FLS was originally announced (June 2012) rather than the date that full details were published (July 2012) because the original announcement introduced all the key features of the scheme (see Section 3.1); in line with this, indicators of wholesale funding costs fall sharply in reaction to the original announcement and not the publication of further details (see Figure 1).

To measure the strength of the equilibrium effect, our key explanatory variable of interest is $\%(Wholesale)_{i,2012}$, defined as the ratio of a bank's wholesale funding to total assets as of 2012:Q1, before the FLS was announced. Our prior is that β should be negative and significant: the more a bank relies on wholesale funding, the more it should be affected by the fall in wholesale funding costs after the announcement of the FLS,

and hence the more it should reduce its mortgage lending spreads. $\%(Wholesale)_{i,2012}$ is measured before the announcement of the FLS and is therefore not subject to concerns around reverse causality. However, this variable is not randomly distributed, which raises challenges around omitted variable bias. We therefore include a range of controls and fixed effects, which we now explain in detail.

4.3.1 Controlling for the participation effect

A key part of our identification strategy is to control for the "participation effect", i.e. the potential reduction in funding costs that banks could achieve by *directly* participating in the FLS (Benetton et al. 2021). This is important because failing to control for this effect could bias the estimate of our key parameter for the equilibrium effect β. Indeed, in Section 4.1 we have shown that a bank's propensity to participate in the FLS is correlated with its wholesale funding exposure.

The drawdown window opened on 1 August 2012—around 6 weeks after the original announcement of the FLS. Drawdowns picked up gradually from this point, with the majority of drawdowns falling after the end of our baseline sample (June 2013). However, if banks are forward-looking, they should anticipate the benefits of future borrowing immediately after the announcement rather than only when they receive the funding.

To control for the participation effect effect, we use a pre-determined source of variation in the amount that a bank can expect to borrow from the scheme, in the spirit of Benetton & Fantino (2021). Specifically, we use the ratio of the bank's initial borrowing allowance to total assets ($Initial\ Allowance_i$). As discussed in Section 3.1, initial allowance is based on the bank's pre-FLS stock of lending, and is therefore unaffected by its response to the FLS. While a bank's total borrowing allowance is also a function of its lending during the drawdown window ("additional allowance"), exploiting only initial allowance allows us to focus on variation that is outside of the bank's control once the FLS is announced.

In our estimation, we use $Initial\ Allowance_i$ in two ways. First, we include it directly in our regressions as an additional bank-level control variable (interacted with $PostFLS_t$). Second, we use $Initial\ Allowance_i$ as an instrument for a measure of the bank's *actual* borrowing from the scheme ($FLS\ Drawdown_i$), defined as the ratio of total drawing to total assets (again interacted with $PostFLS_t$). As demonstrated in our regression tables, $Initial\ Allowance_i$ is a good predictor of $FLS\ Drawdown_i$, i.e. the instrument is strong.

4.3.2 Further controls

In addition to our proxies for the equilibrium and participation effects, our model also includes a range of controls for potential confounding factors. First, we add bank-product fixed effects $\theta_{i,p}$, where a product is defined by the combination of mortgage fixation period and LTV bucket (for example, one product would be a two-year fixation period with an LTV of between 75% and 80%). This controls for any unobservable heterogeneity across banks, including if the effect of this heterogeneity varies across products (e.g. bank specialisation across products). Second, we control for product-time fixed effects $\vartheta_{p,t}$. This controls for confounding aggregate developments that might coincide with the announcement of the FLS (such as changes in credit demand), including developments whose impact could differ across mortgage categories (such as a change in the demand for mortgages by riskier borrowers). Mortgage controls include log(loan size), mortgage term, mortgage type (fixed or floating), loan-to-value ratio (LTV), loan-to-income ratio (LTI), borrower age, and indicator variables for first-time buyers, home movers, borrowers with an impaired credit history, and brokered loans.

One remaining challenge is that $\%(Wholesale)_{i,2012}$ might correlate with other bank characteristics that might also shape the effect of the announcement of the FLS on banks. We therefore include interactions between $PostFLS_t$ and a range of controls for bank characteristics: log total assets, cash ratio (cash / total assets), capital ratio (capital /

total assets), and return on assets (net income / total assets), all measured in 2012:Q1.

Another remaining challenge is that developments in the euro area crisis could affect UK banks' wholesale funding costs (and therefore lending) for reasons unrelated to the FLS. For example, Mario Draghi's "whatever it takes" speech was given in July 2012, around one month after the announcement of the FLS.[14] To address this challenge, we follow Churm et al. (2021) and interact our main cross-sectional variable $\%(Wholesale)_{i,2012}$ with the first principle component of CDS spreads for several euro area sovereigns and banks, which summarises changes in euro area risk perceptions over time.[15]

5 Main Results

Table 4 reports estimates from a range of increasingly conservative variants of our benchmark model (1). Across all specifications, the parameter estimate β for our key coefficient of interest $\%(Wholesale)_{i,2012} \times PostFLS_t$ is negative and significant. In other words, after the FLS is announced, banks more exposed to wholesale funding reduce spreads on new mortgages. This is consistent with the idea that the FLS announcement reduces the cost of wholesale funding, and that banks more exposed to wholesale funding markets pass these lower funding costs through to mortgage spreads; that is, the FLS operates via an "equilibrium effect".

Relative to the specification without any controls (column 1), our estimate of β approximately doubles when we add fixed effects (column 2) and loan-level controls (column

[14] Figure 1 shows that measures of UK bank wholesale funding costs fell sharply when the FLS was announced, but did not react to Draghi's speech. However, other developments in the euro area might have affected UK bank funding costs, or the impact could have built more gradually over time.

[15] We collect daily 5-year CDS spreads for eight euro area sovereigns (Belgium, France, Germany, Ireland, Italy, Netherlands, Portugal, and Spain) and twelve major euro area banks (BNP Paribas, Societe General, Credit Agricole, BBVA, Santander, Intesa Sanpaolo, Miediobanca, Commerzbank, Deutsche, Unicredit, Banca Monte Dei Paschi, Banco Comercial Portuguese) and extract their first principal component, which explains 86% of their common variation. The time series variation of this principal component tracks stress episodes during the euro area crisis, with higher values indicating more stress. We then aggregate the principal component to the monthly frequency by taking the mean.

3). The estimate is also robust to controlling for developments in the euro area, as proxied by the first principal component of euro area CDS spreads (column 4). In itself, the parameter estimate for this time-varying euro area control is statistically insignificant. This is consistent with Figure 1, which shows that while the announcement of the FLS had a large impact on wholesale funding costs for UK banks, Draghi's "whatever it takes" speech had no immediate impact. More broadly, this estimate suggests that euro area developments were not a key driver of UK mortgage rates once the FLS is accounted for.

In the remaining columns of the table, we introduce our control for the "participation effect" of the FLS. As shown in Section 4.1, participation in the FLS is negatively correlated with wholesale funding exposure. Therefore, failing to control for the potential downward pressure on mortgage spreads associated with participating in the FLS could bias our estimate for the equilibrium effect downwards in magnitude. In line with this, when we control for the participation effect, our estimated coefficient for the equilibrium effect increases further in size. This is true both when we control for the participation effect in a reduced-form way by adding a measure of banks' initial borrowing allowances (column 5), and when we use this measure of initial borrowing allowances as an instrument for realised take-up (column 7). The IV first-stage regression confirms that the instrument is a strong predictor of realised take-up (column 6), with the Kleibergen-Paap first-stage F-statistic in excess of 40.

For both the reduced-form and IV approaches, the parameter estimate for the participation effect is negative and significant (columns 5 and 7). In other words, much like the equilibrium effect, the participation effect is associated with reductions in mortgage spreads after the FLS announcement. This result for the participation effect is in line with existing evidence from the ECB's TLTRO (e.g., Benetton & Fantino 2021) and the FLS (Benetton et al. 2021).

Finally, in column 8, we estimate our coefficient for the equilibrium effect separately

for banks that do and do not participate. The two coefficient estimates are both negative, strongly significant, and statistically indistinguishable. This provides the most direct evidence that there is a strong equilibrium effect in addition to any participation effect, since the fall in lending rates holds even for banks that do not participate in the FLS at all.

5.1 Economic magnitude

To assess the economic magnitude of the equilibrium and participation effects, we first consider the impact on the average bank in our sample. For the average bank, $\%(Wholesale)_{i,2012}$ is equal to 16.3%, and total FLS borrowing is 5.1% of total assets. Meanwhile, in our benchmark IV regression with the full set of controls (Table 4, column 7), our key coefficient estimates are -2.115 for the equilibrium effect ($\%(Wholesale)_{i,2012} \times PostFLS_t$) and -10.4 for the participation effect ($FLS\ Drawdown_i \times PostFLS_t$). This implies that, for the average bank, the equilibrium and participation effects are associated with reductions in mortgage spreads of around 34 basis points and 53 basis points, respectively.

However, these estimates for the average bank are unlikely to reflect the true relative importance of the equilibrium and participation effects. This is because mortgage lending in the UK is dominated by a small number of large banks, and large banks tend to have a greater exposure to wholesale funding (and can therefore benefit more from the equilibrium effect) and borrow less from the FLS (participation effect). Specifically, if we weight by the number of mortgages originated during our sample period, $\%(Wholesale)_{i,2012}$ is equal to 32% on average, and total FLS borrowing is 2.4% of total assets on average (Table 2). Therefore, once we weight by mortgage lending, the equilibrium and participation effects are associated with reductions in mortgage spreads of around 68 basis points and

25 basis points, respectively.[16]

Our results therefore suggest that the aggregate impact of the equilibrium effect is substantially larger than the participation effect, which reflects the high reliance on wholesale funding by large UK banks. To put these estimates into context, the average quoted spread for 2-year 75% LTV mortgages rose by around 60 basis points in the months leading up to the announcement of the FLS, before peaking at around 3.8% in June 2012 (when the FLS was announced), and then falling by around 110 basis points by June 2013 (the end of our baseline sample). Our estimates are therefore consistent with the equilibrium effect explaining a large share of the fall in mortgage spreads following the FLS announcement.

5.2 Alternative explanations

Our key result is that when the FLS is announced, banks more exposed to wholesale funding offer cheaper mortgages—irrespective of how much they can expect to borrow from the FLS. Our preferred interpretation is that this reflects an "equilibrium effect", whereby the availability of an outside funding option reduces the cost of wholesale funding, which allows banks more exposed to wholesale funding markets to offer cheaper loans. In the next section, we shed more light on the mechanisms potentially underlying the equilibrium effect.

However, before doing so, we explore a range of mechanisms that have been put forward by existing literature on central bank funding-for-lending schemes, and test whether our estimate of the equilibrium effect is robust to controlling for these other mechanisms. In summary, after adding a range of additional control variables to our benchmark IV regression, our estimated coefficient for the equilibrium effect is unchanged.

[16] This estimate for the participation effect is somewhat lower than the estimate obtained by Benetton et al. (2021), who focus only on the participation effect, which they measure in a binary fashion by comparing participants to non-participants.

Fees First, we control for any fees that mortgagors must pay in addition to the loan rate. Using a structural model, Benetton et al. (2021) estimate that after the FLS, UK banks participating in the FLS reduce their mortgage rates but increase mortgage origination fees. In our baseline regression, we do not control for fees because in our sample period, fees are reported for only a minority of mortgages. In column 1 of Table 5, we therefore restrict the sample to a subset of mortgages for which we can match fees using the dataset of advertised mortgage rates collected by Moneyfacts. We then re-run our baseline regression, controlling for the (log) fee amount.[17] In line with Benetton et al. (2021), we find that higher fees are associated with lower loan spreads, and that controlling for fees significantly reduces our estimate of the participation effect ($FLS\ Drawdown_i \times PostFLS_t$). However, our key coefficient for the equilibrium effect ($\%(Wholesale)_{i,2012} \times PostFLS_t$) is unchanged.

Quantitative easing Wanengkirtyo & Miller (2020) find that the inflow of reserves into UK banks generated by the Bank of England's Quantitative Easing (QE) programs affects banks' mortgage spreads. In column 2 of Table 5, we therefore control for bank-level inflows of reserves (as a ratio of total assets) generated by the QE program announced in July 2012 (a month after the FLS announcement). Again, our key coefficient for the equilibrium effect is unaffected.

Competition Next, we control for potential indirect effects of the FLS through competitive dynamics. Andreeva & García-Posada (2021) argue that if participating in the ECB's TLTROs allows banks to offer lower loan rates, then non-participants competing with participants might also be forced to reduce loan rates or lower credit standards in

[17] Matching PSD and Moneyfacts data is not trivial because there is a significant number of cases in which an originated mortgage can be matched to more than one quoted mortgage product. For comparability, we follow the approach described in the appendix to Benetton et al. (2021): specifically, we only consider mortgages to first-time buyers, and when there are multiple matches we take the highest observed fee. Our results are similar when using the average matched fee instead. We also find similar results when using the actual fee rather than log(fee), or using the ratio of fee to loan rate.

order to maintain market shares. To control for this, we construct a proxy for how much a given bank could expect its competitors to benefit from FLS participation. Given our main findings, we also measure how a much a bank could expect its competitors to benefit from the equilibrium effect.

To construct these two proxies, we first compute the weighted average values of initial borrowing allowance, realised FLS borrowing, and wholesale funding exposure of banks active in each segment of the UK mortgage market, where we weight by each bank's share of total lending in that market before the FLS. We define a market by the combination of mortgage product (LTV bucket and fixation period) and geographical location (three-digit postcode). To convert these market-level measures into bank-level measures, we then aggregate each market-level measure across all the markets in which a given bank is active, weighted by the share of the market in the bank's lending portfolio. Finally, we interact the weighted average realised FLS borrowing (instrumented by weighted average initial allowance) and weighted average wholesale funding exposure with $PostFLS_t$.

The results are reported in columns 3 and 4 in Table 5. Our key results (for both the equilibrium effect and participation effect) are unchanged. This confirms that our equilibrium effect is distinct from the competitive channels in Andreeva & García-Posada (2021).

We then control for local competition. Focusing on Italy, Benetton & Fantino (2021) find that banks that borrow more from the TLTRO increase lending more in areas with higher banking competition. To control for local competition, we therefore construct a Herfindahl index at the level of a local market, and interact it with $FLS\ Drawdown_i \times PostFLS_t$. The results in column 5 show that our key coefficient is again unchanged.

Borrower risk Finally, our preferred mechanism implies that the reduction in mortgage spreads associated with the equilibrium effect reflects a reduction in banks' funding costs following the announcement of the FLS. However, if banks expect the FLS to improve the

economic outlook and therefore reduce borrower credit risk, the fall in mortgage spreads could instead reflect a compression in borrower risk premia. This could also be the case if the announcement of the FLS reduces banks' risk aversion.[18]

Our regressions already control for several measures of borrower risk (LTV, LTI, credit history). To provide further reassurance that our results are not driven by changes in borrower risk premia, we test how our key result varies across borrower risk categories. A reduction in credit risk or risk aversion should have a larger impact on higher-LTV loans. To test this idea, we construct an indicator variable equal to 1 for mortgages with LTV ratio greater than 75%, and we interact it with our key coefficient $\%(Wholesale)_{i,2012} \times PostFLS_t$. The results reported in column 6 of Table 5 suggest that the equilibrium effect is *weaker* for high-LTV mortgages. This suggests that the FLS lowers the cost of mortgages across the risk spectrum, rather than compressing premia on riskier mortgages.

6 Mechanism

In the previous section, we report evidence consistent with an "equilibrium effect": when the FLS is announced, banks more exposed to wholesale funding offer cheaper mortgages—irrespective of how much they can expect to borrow from the FLS. This result is consistent with the idea that the presence of an outside funding option reduces prices in private wholesale funding markets, which allows banks more exposed to these funding markets to offer cheaper loans.

In this section, we consider two (non-mutually exclusive) mechanisms that could explain the equilibrium effect. First, under a *risk channel*, the existence of a risk-insensitive outside funding option could reduce banks' funding liquidity risk (i.e. the risk of being unable to obtain sufficient funding to meet payment and debt obligations as they fall

[18] Minoiu et al. (2021) find that banks registered to the Federal Reserve's 2020 Main Street Lending Program were less likely to tighten their lending standards.

due).[19] All else equal, this risk channel should reduce the risk premia required by private wholesale lenders when providing funding to banks. Second, under a *demand channel*, the existence of a public outside funding option could increase banks' bargaining power in private funding markets and hence lower the price banks would be willing to pay for private funding. All else equal, this demand channel should reduce the mark-up that wholesale lenders can charge when providing funding to banks.[20]

6.1 Evidence from banks' liability structures

To weigh up these two channels, we start by exploiting the fact that they make different predictions for where the equilibrium effect should be strongest depending on the structure of banks' liabilities, in particular along the dimensions of maturity, collateralisation, and currency.

Maturity If the risk channel dominates, then our main result for the equilibrium effect is more likely to be driven by banks' exposure to *short-term* wholesale funding. This is because short-term funding must be rolled over more frequently than long-term funding, and therefore entails more funding liquidity risk. Under the risk channel, the FLS is therefore more likely to impact the cost of short-term funding.

On the other hand, if the demand channel dominates, then our main result is more likely to be driven by banks' exposure to *long-term* wholesale funding. This is because the FLS provides long-term funding (four years), and so banks are more likely to see FLS funding as a substitute for long-term wholesale funding, rather than short-term funding. Therefore, under the demand channel, the FLS is more likely to reduce banks' demand

[19] In line with this idea, Figure 1 shows that CDS spreads for major UK banks decrease strongly after the FLS announcement.

[20] In line with this idea, Aldasoro et al. (2022) find that when money market funds are constrained from providing wholesale (short-term unsecured) funding to banks, this increases rates on short-term unsecured funding, consistent with an increase in the bargaining power of funds over banks. The shock from the FLS announcement can be understood as a similar shock in reverse, with the arrival of a new outside option for banks increasing banks' bargaining power vis-à-vis lenders.

for long-term wholesale funding, and hence reduce the mark-up that wholesale lenders can charge on this funding.[21]

To test which channel dominates, we therefore exploit heterogeneity across banks in terms of funding maturity, using the granular regulatory dataset FSA047/048. This dataset reports outstanding balances for a range of funding instruments, broken down by residual maturity. Specifically, we replace our baseline measure of total wholesale funding exposure $\%(Wholesale)_{i,2012}$ with separate measures for short-term wholesale funding, defined as wholesale funding with residual maturity of less than one year, and long-term wholesale funding, defined as long-term instruments such as bonds and covered bonds.[22]

Results are reported in Table 6. To ease comparison, in column 1 we replicate our benchmark regression using the FSA047/048 data to construct our measure of *total* wholesale funding exposure ($\%(Wholesale)_{i,2012}$), rather than the data used in our main regressions. The results are analogous to our baseline regressions: the estimated coefficient for $\%(Wholesale)_{i,2012} \times PostFLS_t$ is negative and significant, i.e. banks more exposed to wholesale funding reduce mortgage spreads after the FLS announcement.

To shed light on the underlying mechanism, in column 2 we replace $\%(Wholesale)_{i,2012}$ with our separate measures of short-term and long-term wholesale funding. The estimated coefficient for short-term funding is negative and significant, and similar in magnitude to the effect for total wholesale funding in column 1; in contrast, the estimate for long-term funding is insignificant. In other words, the equilibrium effect appears to be driven by exposure to short-term wholesale funding rather than long-term wholesale funding, consistent with the risk channel.

[21] In line with this idea, Fudulache & Goetz (2023) find that banks that participate more in the ECB's TLTRO tend to increase their money market funding and decrease their bond funding, consistent with long-term central bank funding being a closer substitute for longer-term private funding.

[22] Results are robust to alternative ways of measuring short-term vs long-term.

Collateralisation While exposure to long-term wholesale funding does not seem to explain the fall in mortgage spreads after the FLS announcement, this could mask heterogeneities between different forms of long-term funding. To strengthen our interpretation, we next decompose our measure of long-term wholesale funding into measures of long-term secured wholesale funding (such as covered bonds) and long-term unsecured funding (such as unsecured bonds). We then interact both measures with $PostFLS_t$.

If the risk channel dominates, we would expect the coefficient for both these measures to be insignificant, since long-term secured and unsecured funding both involve limited funding liquidity risk. Instead, if the demand channel dominates, we would expect the coefficient for exposure to long-term *secured* funding to be significant. This is because the FLS provides long-term secured funding, and is therefore likely to be a closer substitute for long-term secured funding than for other funding sources. Therefore, the announcement of the FLS should have a relatively larger impact on the bargaining power of banks more exposed to long-term secured funding.

In line with our previous results in column 2, the results reported in column 3 are consistent with the risk channel and less with the demand channel. Specifically, exposure to both secured and unsecured long-term wholesale funding has no significant impact on mortgage spreads after the FLS announcement.

Currency Finally, in column 4, we decompose our measure of total wholesale funding exposure into measures of exposure to wholesale funding denominated in sterling and euros.[23] Under the risk channel, we would expect the effect to be similar regardless of funding currency, since the presence of a public funding backstop should reduce the funding liquidity risk of all banks exposed to wholesale funding, regardless of the currency of that funding. On the other hand, under the demand channel, we would expect the effect

[23]For this test, we use the Bank of England's Form BT dataset, which provides bank balance sheet data decomposed into sterling, euro, and all other currencies.

to be larger for banks more exposed to sterling wholesale funding, since the FLS provides sterling funding and is therefore likely to be a closer substitute to other sterling funding sources. For instance, banks with mostly sterling assets might prefer sterling funding if cross-currency swaps are costly, and wholesale funding providers with mostly sterling liabilities (e.g. UK pension funds) might have a preference for lending to banks in sterling to avoid any currency mismatch. In this case, the FLS announcement is likely to exert greater downward pressure on the cost of sterling wholesale funding.

Consistent with the previous tests, the results are more in line with the risk channel: the estimated effects for both sterling and euro funding exposure are negative and significant, and are statistically indistinguishable from each other.

6.2 Evidence from the FLS extension announcement (FLS2)

To shed further light on the channels underlying the equilibrium effect, we now exploit the April 2013 announcement of the extension of the FLS ("FLS2"; see Section 3.3). This announcement provides a useful shock because its impact on bank lending should depend on which channel dominates.

If the risk channel dominates, then we would not expect the FLS extension announcement to trigger a significant equilibrium effect. This is because, by the time the extension was announced, indicators of UK bank wholesale funding costs had largely normalised (Figure 1; see also Bank of England (2013)), suggesting that risk premia had fallen from the elevated levels observed before the original FLS announcement. Wholesale funding providers were therefore unlikely to associate the extension of the FLS with a significant further reduction in UK banks' riskiness. On the other hand, if the demand channel dominates, then the FLS extension might strengthen the equilibrium effect, because it significantly prolongs the period during which banks have access to a public outside funding option and are therefore likely to have lower demand for private wholesale funding.

To confront these ideas, we use a variant of our baseline empirical model (1) to test whether banks more exposed to wholesale funding reduce their mortgage spreads by more after the FLS extension announcement. Our key variable of interest is $\%(Wholesale)_{i,2012} \times PostExtension_t$, where $\%(Wholesale)_{i,2012}$ is defined as in the baseline regression, and $PostExtension_t$ is an indicator variable equal to 1 after the announcement of the FLS extension in April 2013, and 0 otherwise. If the risk channel dominates, we expect the parameter estimate to be insignificant (since the extension announcement would not be associated with a significant equilibrium effect), whereas it would be negative and significant if the demand channel dominates (consistent with the extension announcement strengthening the equilibrium effect). To capture the extension announcement (April 2013), we shift the sample period to start in July 2012 and end in October 2013.[24] Our controls and fixed effects are otherwise similar to the baseline model.

The results are reported in column 1 of Table 7. The parameter estimate for $\%(Wholesale)_{i,2012} \times PostExtension_t$ is insignificant. This suggests that the risk channel, rather than the demand channel, is the key driver of the equilibrium effect (consistent with our results in Section 6.1).

7 The role of conditionality

In the previous sections, we establish an equilibrium effect that allows banks to reap the benefits of central bank funding without actually using it. This helps to explain why banks *more* exposed to wholesale funding use *less* central bank funding, in line with the idea that public funding "creates its own competition" (Farhi & Tirole 2021). In this section, we test how the impact of central bank funding on lending is affected by funding conditionality ("strings attached"), i.e. constraints on banks' ability to deploy funding to

[24] We start the sample in July 2012 to avoid the original FLS1 announcement in June 2012. We end the sample in October 2013 because an amendment to the extension was announced in November 2013; see Section 3.3.

their preferred purpose. To do so, we exploit three shocks to the degree of conditionality embedded in FLS funding.

7.1 Effect of the FLS extension announcement (FLS2)

To test the importance of conditionality, we first return to the April 2013 announcement of the FLS extension ("FLS2"). We exploit the fact that the design of FLS2 essentially gave banks access to two new forms of FLS funding. First, it extended the availability of *conditional* funding (which had been due to expire in January 2014 when the original FLS1 drawdown window closed) by one year. Second, during a shorter transition period, it also gave banks access to *unconditional* funding.

To see this, recall from Section 3.3 that FLS2 borrowing allowances are equal to the sum of "initial allowances" and "additional allowances." FLS2 *initial* allowances would be based on net lending to households and businesses during the last three quarters of 2013 (which approximately corresponds to the transition period between the announcement of FLS2 and the start of the FLS2 drawdown window). During this transition period, new mortgages could still be funded with FLS1 drawings, but would *also* generate initial allowances for future FLS2 drawings. Importantly, once unlocked, these FLS2 initial allowances could be used to fund any asset; therefore, FLS2 drawings based on initial allowances constitute *unconditional* funding.

Meanwhile, *additional* allowances would be based on net new lending to households and businesses during the FLS2 drawdown window (which starts in February 2014). Since additional allowances can only be unlocked by originating new loans to specific types of borrower, this funding constitutes *conditional* funding.

Therefore, if banks value unconditional FLS funding more than conditional FLS funding, then we should observe an increase in mortgage lending during the transition period, as banks take the opportunity to unlock future unconditional funding. And this effect

should be larger for banks more reliant on FLS funding.

To test this idea, we return to our estimates in column 1 of Table 7, which compare bank behaviour before and after the announcement of FLS2 in April 2013. To isolate the impact of the announcement, we focus on the period between FLS1 and FLS2: specifically, our sample period is from July 2012 (after FLS1 is announced) to October 2013 (before FLS2 is amended; we return to this amendment in Section 7.2 below). Figure 2 illustrates our research design graphically.

This time, however, we focus on how the FLS2 announcement affects bank lending depending on a bank's reliance on FLS funding. To measure reliance on FLS funding, we use $FLS\ Drawdown_i$, defined as the ratio of total FLS1 drawing to total assets. As for our baseline regressions, we instrument $FLS\ Drawdown_i$ with $Initial\ Allowance_i$, defined as the ratio of FLS1 initial allowance (measured in June 2012) to total assets. We interact $FLS\ Drawdown_i$ with $PostExtension_t$, an indicator variable equal to 1 after the FLS extension announcement in April 2013. If banks more reliant on FLS funding have a stronger incentive to unlock future unconditional borrowing, then the coefficient on the interaction term $FLS\ Drawdown_i \times PostExtension_t$ should be negative and significant.

The results are in line with our prior: the more a bank relies on FLS funding, the more mortgage spreads fall after the extension announcement (Table 7, column 1). This effect cannot reflect a change in the expected cost of funding mortgages *during* the transition period, because all mortgages originated during this period could already be funded by FLS1 funding. In contrast, the finding is consistent with the idea that during the transition period, increasing lending is more attractive because it unlocks *future* unconditional borrowing allowances under FLS2, which is especially valuable for larger FLS users. In other words, banks prefer unconditional to conditional funding, and so the conditionality of the funding affects bank lending.

7.2 Effect of the FLS2 amendment

To further examine the importance of conditionality, we turn to the subsequent amendment of the FLS2 program. In November 2013, the Bank of England and HMT announced that, unlike FLS1, and in contrast to the initial announcement of FLS2 in April 2013, any new household lending during the FLS2 drawdown window would *not* generate additional borrowing allowances; instead, additional allowances would be based on business lending only.[25] The stated objective was to strengthen banks' incentives to expand business lending rather than mortgage lending, which was seen as no longer in need of support, given rising house prices.

For our purpose, this amendment is a useful shock because it tightens the conditionality of FLS funding. After the amendment, only loans to businesses unlock additional allowances, and not loans to households. Therefore, FLS funds obtained in this way can no longer fund mortgages. All else equal, this shock should increase the cost of originating mortgages during the FLS2 drawdown window, particularly for banks more reliant on FLS funding.

To test this idea, we interact our measure of FLS usage ($FLS\ Drawdown_i$) with an indicator variable equal to one after the FLS2 window opens in February 2014 ($PostFLS2_t$). As illustrated by Figure 2, the sample starts in May 2013 (after the initial announcement of FLS2) and ends in November 2014 (before a one-year extension of the FLS2 drawdown window is announced).[26] We omit the period between the amendment being announced and coming into effect (November to December 2013), since the effect of the amendment on the cost of mortgage lending will not yet have taken effect during this period (we examine this period in Section 7.3 below).

[25] https://www.gov.uk/government/news/bank-of-england-and-hm-treasury-re-focus-the-funding-for-lending-scheme-to-support-business-lending-in-2014. It is credible that the announcement was unexpected; for instance the Council of Mortgage Lenders described the amendment as "a surprise."

[26] https://www.gov.uk/government/news/funding-for-lending-scheme-bank-of-england-and-hm-treasury-announce-extension

Results in column 2 of Table 7 show that after the start of the FLS2 window, banks more reliant on FLS funding tend to charge *higher* mortgage spreads. This is consistent with the idea that tightening conditionality mitigates the positive impact of the FLS on mortgage rates. This is the opposite impact to what we found for the announcement of the original FLS (Section 5) and its extension (Section 6.2). In other words, the amendment appears to reverse some of the beneficial impact of the FLS on the cost of mortgage lending.

7.3 The "reference window effect"

Finally, we examine how the amendment affects banks' behaviour during the transition period after the amendment is announced but before it takes effect (November to December 2013), which we omitted from our previous test. During this period, banks already know that, from 2014, mortgage lending will no longer generate "additional" FLS2 borrowing allowances. However, during this transition period, mortgage lending still generates "initial" FLS2 borrowing allowances, which are based on net lending (including mortgages) during the last three quarters of 2013. Therefore, banks that are more reliant on FLS funding *and* expect the amendment to limit their ability to obtain FLS funds in the future might have an incentive to increase lending during these two months. And that "reference window effect" should increase with the share of mortgage lending in their business model, since banks that are highly reliant on mortgage lending will see their ability to generate FLS2 borrowing allowances reduce once the amendment takes effect in 2014.

To test this idea, we interact our measure of FLS reliance ($FLS\ Drawdown_i$) with an indicator variable equal to 1 during the transition period (November to December 2013) and 0 before ($PostAmendment_t$). The "pre" period runs from May 2013 (after the extension is announced) to October 2013 (before the amendment is announced). We

then interact this measure with a proxy for a bank's reliance on mortgage lending, namely the ratio of mortgages to total loans as measured in 2012:Q1, before the original FLS1 announcement ($\%(Mortgages)_{i,2012}$).

We find that the estimated coefficient on the triple interaction term ($FLS\ Drawdown_i \times PostAmendment_t \times \%(Mortgages)_{i,2012}$) is negative and significant (Table 7, column 3). That is, after the amendment is announced, banks that are more reliant on FLS funding on the liability side *and* mortgage lending on the asset side reduce mortgage spreads relative to other banks. This is consistent with the idea that these banks are incentivised to secure (initial) FLS2 borrowing allowances before conditionality becomes tighter.[27]

Together, the results in this section suggest that conditionality matters: tightening the conditionality of central bank funding makes it less attractive to banks. This illustrates a key trade-off in the design of funding-for-lending schemes: the tighter the conditionality (in this case, captured by a larger role for additional vs. initial allowances), the more central banks can ensure that funding supports lending to targeted sectors, but the smaller the equilibrium effect.

8 Conclusion

In response to recent economic shocks, many central banks across advanced and emerging economies have deployed "funding-for-lending" schemes. Under these programs, banks can obtain long-term funding at below-market rates, subject to expanding lending to targeted sectors in the real economy. While these schemes are generally thought to have succeeded in stimulating credit supply, there have been concerns about private funding markets being crowded out and private sector risk being transferred to the central bank,

[27]Meanwhile, the estimated coefficient on the double interaction ($FLS\ Drawdown_i \times PostAmendment_t$) is statistically insignificant. This coefficient captures the effect of the reference window for a hypothetical bank with zero mortgage exposure. Given that such a bank is completely inactive in mortgage lending, it is unsurprising that it would be unaffected by the removal of mortgage lending from FLS2 additional allowances.

and about the relatively low take-up of some of these schemes (BIS 2023).[28]

In this paper, we show that central bank funding is more powerful than previously established, via a mechanism that does not require direct participation and therefore does not directly transfer risk to the central bank balance sheet. We refer to this as the "equilibrium effect" of central bank funding. If central bank funding is an attractive substitute for private funding, this should not only lower the funding costs of those banks that borrow directly from the central bank, but should also reduce the cost of private funding—even for banks that do not directly participate in the scheme. And the stronger this equilibrium effect, the less banks need to participate directly, implying that judging funding schemes on the basis of participation potentially dramatically underestimates their true effect.

[28]During Covid, fourteen central banks deployed such schemes to stimulate lending and economic activity (BIS 2023). Some central banks have also deployed or discussed similar schemes aimed at less traditional objectives such as stimulating "green" lending (Bank of Japan 2021, Schnabel 2022).

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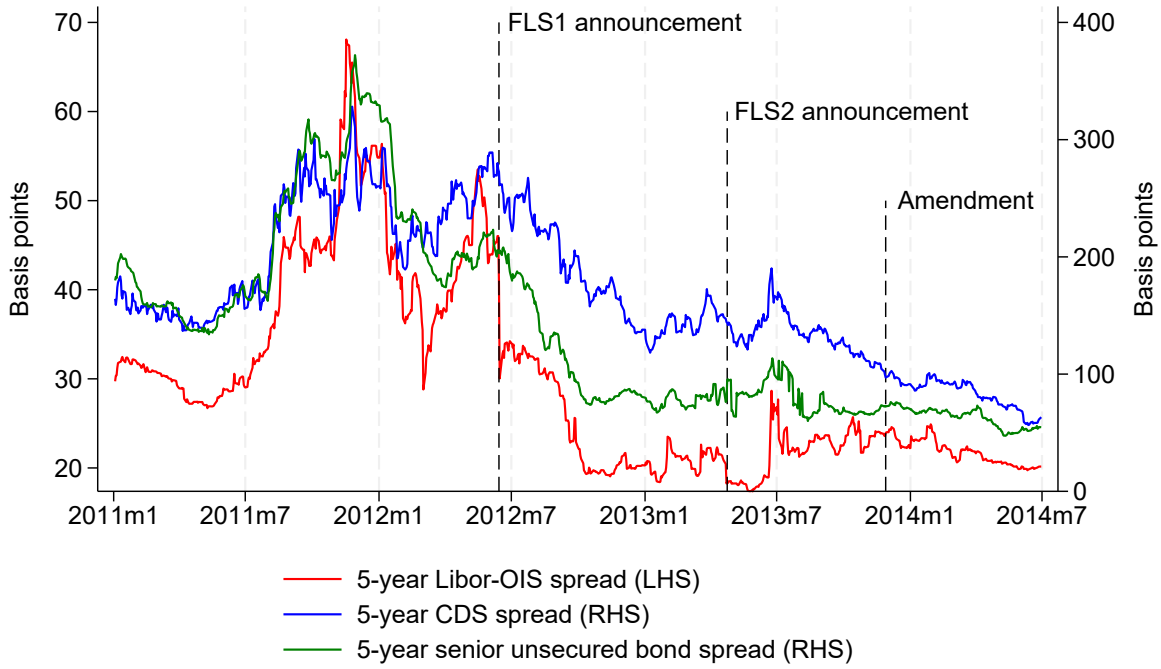
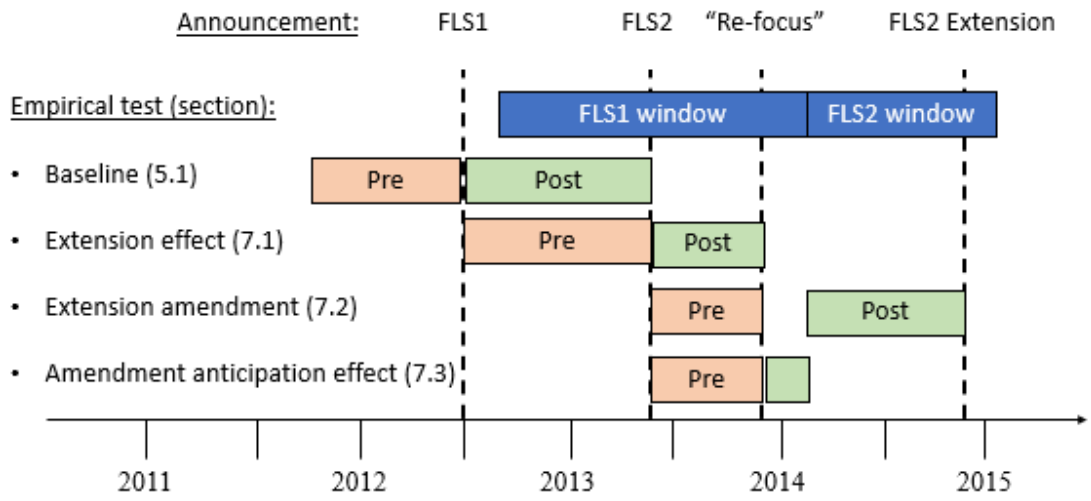
Figure 1: Wholesale funding costs for UK banks



Notes: The chart shows measures of long-term wholesale funding costs for UK banks. The red line shows the difference between the 5-year sterling LIBOR swap rate and the 5-year sterling OIS rate. This provides a measure of expected bank credit risk premia over the next five years. The blue line shows the average 5-year senior CDS spread across major UK banks. The green line shows the average spread for senior unsecured bonds of approximately 5-year maturity across major UK banks.

Figure 2: FLS timeline and sample periods for empirical analysis



Notes: The figure shows the timing of key FLS announcements and how they relate to the sample periods used in the empirical analysis.

Table 1: Timeline of key FLS announcements

Announcement	Date	Summary
FLS1 announcement (Mansion House speech)	14 June 2012	BoE and HMT to introduce "funding for lending scheme" providing long-term funding to banks at below-market rates, linked to banks' real-economy lending.
FLS1 details	13 July 2012	Drawdown window to last from 1 August 2012 to 31 January 2014. Term of borrowing to be 4 years. Borrowing allowances equal to "initial allowances" plus "additional allowances", where initial allowances are equal to 5% of stock of existing real-economy lending as of end-June 2012, and additional allowances are equal to net new real-economy lending from end-June 2012 to end-December 2013. Price of borrowing depends on net new real-economy lending over the same period.
FLS2 announcement	24 April 2013	New one-year drawdown window to open on 1 February 2014. Initial allowances based on net real-economy lending over last three quarters of 2013 (the FLS2 "reference period"). Additional allowances based on net new real-economy lending over 2014.
FLS2 amendment	28 November 2013	Contrary to previous announcement, household lending during 2014 will not contribute to additional allowances.

Table 2: Summary statistics

	Mean	Std Dev	Min	Max
Summary statistics for cross-sectional analysis				
Indicator variable for participation in FLS	0.461	0.502	0	1
Average drawing / Initial allowance	0.015	0.062	0	0.512
Maximum drawing / Initial allowance	0.043	0.172	0	1.492
Wholesale funding / Total assets	0.163	0.239	0	0.991
Log(Total assets)	7.526	2.691	3.479	14.25
Capital / Total assets	0.175	0.059	0.074	0.452
Return-on-assets	0.052	1.838	-13.386	2.716
Cash / Total assets	0.046	0.054	0	0.276
Summary statistics for loan-level analysis				
Wholesale funding / Total assets	0.32	0.16	0	0.65
Initial allowance / Total assets	0.02	0.01	0.01	0.04
FLS drawdown / Total assets	0.02	0.03	0	1.49
First principal component of euro area CDS spreads	4.01	3.25	-1.01	8

Notes: The table shows summary statistics for variables used in the regressions. Balance sheet variables are measured as of 2012:Q1. The sample period for the loan-level regressions is January 2012 to June 2013.

Table 3: Wholesale funding exposure and propensity to participate in FLS

Dependent variable:	Indicator variable for participation		Average drawing / Initial allowance		Maximum drawing / Initial allowance	
Model:	Probit	Probit	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
$\%(\text{Wholesale})_{i,2012}$	-6.903***	-8.592***	-0.058*	-0.057**	-0.141*	-0.133***
Log(Total assets)	Yes	Yes	Yes	Yes	Yes	Yes
Other bank-level controls	No	Yes	No	Yes	No	Yes
Observations	71	67	71	67	71	67
R^2	0.198	0.230	0.018	0.743	0.018	0.812

Notes: The table shows cross-sectional bank-level regression results of FLS1 participation on exposure to wholesale funding (see Section 4.1). The dependent variables are an indicator variable equal to one if the bank registers to participate in FLS1 (columns 1 and 2); the bank's average drawing from FLS1 over the full drawdown window (August 2012 to January 2014), divided by initial allowance (columns 3 and 4); and the bank's maximum drawing from FLS1 over the full drawdown window, divided by initial allowance (columns 5 and 6). $\%(\text{Wholesale})_{i,2012}$ is the bank's ratio of wholesale funding to total assets as of 2012:Q1. All columns control for log(total assets). Columns 2, 4, and 6 also control for the bank's cash ratio, capital ratio, and return-on-assets. All control variables are measured as of 2012:Q1. Columns 1 and 2 are estimated by probit. Columns 3–6 are estimated by OLS. Robust standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Main results

Dependent variable:	$\text{Spread}_{i,l,t}$	$\text{Spread}_{i,l,t}$	$\text{Spread}_{i,l,t}$	$\text{Spread}_{i,l,t}$	$\text{Spread}_{i,l,t}$	FLS drawdown$_i$ ×Post FLS$_t$	$\text{Spread}_{i,l,t}$	$\text{Spread}_{i,l,t}$
Model:	OLS	OLS	OLS	OLS	OLS	IV stage 1	IV stage 2	IV stage 2
Instrument:							Initial allow$_i$ × Post FLS$_t$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
%(Wholesale)$_{i,2012}$ × Post FLS$_t$	-0.746***	-1.596**	-1.565**	-1.380***	-2.581***	0.047	-2.115***	
	(0.147)	(0.616)	(0.593)	(0.481)	(0.670)	(0.039)	(0.316)	
%(Wholesale)$_{i,2012}$ × Euro PCA$_t$				0.059	0.062	0.001	0.071	
				(0.047)	(0.047)	(0.000)	(0.047)	
Initial allowance$_i$ × Post FLS$_t$					-22.673***	2.176***		
					(7.152)	(0.340)		
FLS drawdown$_i$ × Post FLS$_t$							-10.420***	-6.345***
							(2.395)	(1.366)
%(Wholesale)$_{i,2012}$ × Post FLS$_t$ × Part$_i$								-1.783***
								(0.208)
%(Wholesale)$_{i,2012}$ × Post FLS$_t$ × Non-part$_i$								-1.206***
								(0.298)
Bank × Product fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product × Time fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls × Post FLS$_t$	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mortgage-level controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	415,671	415,671	415,671	415,671	415,671	415,671	415,671	415,671
R^2		0.685	0.692	0.692		0.828		
Kleibergen-Paap F-statistic							41.0	29.4

Notes: The table shows loan-level regression results for equation (1). The sample period is January 2012 to June 2013. $\text{Spread}_{i,l,t}$ is the interest rate on mortgage l originated by bank i in month t, net of the maturity-matched OIS (overnight indexed swap) rate. $\%(\text{Wholesale})_{i,2012}$ is bank i's ratio of wholesale funding to total assets as of 2012:Q1. Post FLS_t is an indicator variable equal to one after June 2012. Euro PCA_t is the first principal component of CDS spreads for several euro area sovereigns and banks. Initial allowance_i is bank i's ratio of initial FLS1 borrowing allowance to total assets (measured in June 2012). FLS drawdown_i is bank i's ratio of total FLS1 borrowing to total assets. Part_i is an indicator variable equal to one for banks that register to participate in FLS1. Bank-level controls are: log(total assets), cash ratio, capital ratio, and return-on-assets, measured as of 2012:Q1. Mortgage-level controls are: log(loan size), mortgage term, mortgage type (fixed or floating), LTV ratio, LTI ratio, borrower age, and indicator variables for first-time buyers, home movers, borrowers with an impaired credit history, and brokered loans. Columns 1–5 are estimated by OLS. In columns 7 and 8, (FLS $\text{drawdown}_i \times$ Post FLS_t) is instrumented by (Initial $\text{allowance}_i \times$ Post FLS_t). Column 6 shows the first-stage regression for the IV regression in column 7. Standard errors are clustered by bank and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Controlling for alternative explanations

Dependent variable:	$\text{Spread}_{i,l,t}$					
Model:	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
$\%(\text{Wholesale})_{i,2012} \times \text{Post FLS}_t$	-1.566***	-2.266***	-2.384***	-2.346***	-2.338***	-2.794***
	(0.253)	(0.355)	(0.374)	(0.351)	(0.359)	(0.384)
$\text{FLS drawdown}_i \times \text{Post FLS}_t$	-2.054	-9.682***	-10.020***	-10.689***	-10.187***	-12.701***
	(1.316)	(2.088)	(2.099)	(2.835)	(2.352)	(2.916)
Log(Fee)_l	-0.0005***					
	(0.0001)					
QE inflow_i		-14.519***				
		(6.094)				
$\text{Competitor } \%(\text{Wholesale})_{i,2012} \times \text{Post FLS}_t$			-0.144			
			(0.250)			
$\text{Competitor FLS drawdown}_i \times \text{Post FLS}_t$				-1.458		
				(2.947)		
$\text{FLS drawdown}_i \times \text{Post FLS}_t \times \text{Herfindahl}$					-0.144	
					(0.214)	
$\%(\text{Wholesale})_{i,2012} \times \text{Post FLS}_t \times \text{High-LTV}_l$						1.367***
						(0.374)
$\text{FLS drawdown}_i \times \text{Post FLS}_t \times \text{High-LTV}_l$						6.063**
						(2.966)
Bank × Product fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Product × Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Bank-level controls × Post FLS_t	Yes	Yes	Yes	Yes	Yes	Yes
Mortgage-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67,895	415,671	384,034	384,034	413,895	415,671

Notes: The table shows loan-level regression results for equation (1), with additional control variables (see Section 5.2). The sample period is January 2012 to June 2013. $\text{Spread}_{i,l,t}$ is the interest rate on mortgage l originated by bank i in month t, net of the maturity-matched OIS (overnight indexed swap) rate. $\%(\text{Wholesale})_{i,2012}$ is bank i's ratio of wholesale funding to total assets as of 2012:Q1. Post FLS_t is an indicator variable equal to one after June 2012. FLS drawdown_i is bank i's ratio of total FLS1 borrowing to total assets. All variables involving FLS drawdown_i are instrumented by corresponding variables involving $\text{Initial allowance}_i$, i.e. bank i's ratio of initial FLS1 borrowing allowance to total assets (measured in June 2012). Log(Fee)_l is the log of the mortgage fee (from Moneyfacts); column 1 only includes mortgages to first-time buyers. QE inflow_i is the quantity of reserves received by bank i as a result of the QE programme announced by the BoE in July 2012, divided by total assets. $\text{Competitor } \%(\text{Wholesale})_{i,2012}$ and $\text{Competitor FLS drawdown}_i$ are, respectively, the weighted average values of $\%(\text{Wholesale})_{i,2012}$ and FLS drawdown_i in local markets to which bank i is exposed (see Section 5.2 for details), where a local market is defined by the combination of mortgage product (LTV bucket and fixation period) and location (three-digit postcode). Herfindahl is the Herfindahl index for the local market. High-LTV_l is an indicator variable for mortgages with LTV ratio greater than 75%. Bank-level controls and mortgage-level controls are as detailed in Table 4. Standard errors are clustered by bank and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Mechanism for the equilibrium effect

Dependent variable:	$\text{Spread}_{i,l,t}$			
Model:	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
$\%(\text{Wholesale})_{i,2012} \times \text{Post FLS}_t$	-2.134**			
	(0.839)			
$\%(\text{Wholesale Short})_{i,2012} \times \text{Post FLS}_t$		-2.320***	-2.186***	
		(0.653)	(0.526)	
$\%(\text{Wholesale Long})_{i,2012} \times \text{Post FLS}_t$		2.879		
		(2.517)		
$\%(\text{Wholesale Long Secured})_{i,2012} \times \text{Post FLS}_t$			-1.288	
			(1.119)	
$\%(\text{Wholesale Long Unsecured})_{i,2012} \times \text{Post FLS}_t$			1.215	
			(1.366)	
$\%(\text{Wholesale Sterling})_{i,2012} \times \text{Post FLS}_t$				-3.089**
				(1.225)
$\%(\text{Wholesale Euro})_{i,2012} \times \text{Post FLS}_t$				-4.601***
				(0.522)
$\text{FLS drawdown}_i \times \text{Post FLS}_t$	-3.554*	-10.010*	-10.031***	-10.309***
	(2.101)	(5.134)	(2.064)	(2.356)
Bank × Product fixed effects	Yes	Yes	Yes	Yes
Product × Time fixed effects	Yes	Yes	Yes	Yes
Bank-level controls × Post FLS_t	Yes	Yes	Yes	Yes
Mortgage-level controls	Yes	Yes	Yes	Yes
Observations	415,671	415,671	415,671	415,671

Notes: The table shows loan-level regression results for equation (1), with additional decompositions of wholesale funding exposure (see Section 6). The sample period is January 2012 to June 2013. $\text{Spread}_{i,l,t}$ is the interest rate on mortgage l originated by bank i in month t, net of the maturity-matched OIS (overnight indexed swap) rate. $\%(\text{Wholesale})_{i,2012}$ is bank i's ratio of total wholesale funding to total assets as of 2012:Q1. Short-term wholesale funding is defined as wholesale funding with residual maturity of less than one year. Long-term wholesale funding is defined as covered bonds, securitised bonds, and unsecured debt securities. Long-term secured wholesale funding is defined as covered bonds and securitised bonds. Long-term unsecured wholesale funding is defined as unsecured debt securities. Sterling and euro wholesale funding refer to the currency of denomination. Wholesale funding measures are based on FSA047/048 (columns 1–3) and Form BT (column 4) as of 2012:Q1. Post FLS_t is an indicator variable equal to one after June 2012. FLS drawdown_i is bank i's ratio of total FLS1 borrowing to total assets. ($\text{FLS drawdown}_i \times \text{Post FLS}_t$) is instrumented by ($\text{Initial allowance}_i \times \text{Post FLS}_t$), where $\text{Initial allowance}_i$ is bank i's ratio of initial FLS1 borrowing allowance to total assets (measured in June 2012). Bank-level controls and mortgage-level controls are as detailed in Table 4. Standard errors are clustered by bank and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Impact of the FLS extension

Dependent variable:	$\text{Spread}_{i,l,t}$		
Sample period:	Jul 2012–Oct 2013	May 2013–Nov 2014	May 2013–Dec 2013
Model:	IV	IV	IV
	(1)	(2)	(3)
$\%(\text{Wholesale})_{i,2012} \times \text{Post Extension}_t$	-0.240		
	(0.254)		
$\text{FLS drawdown}_i \times \text{Post Extension}_t$	-3.530**		
	(1.622)		
$\%(\text{Wholesale})_{i,2012} \times \text{Post FLS2}_t$		0.091	
		(0.143)	
$\text{FLS drawdown}_i \times \text{Post FLS2}_t$		2.210**	
		(0.947)	
$\text{FLS drawdown}_i \times \text{Post Amendment}_t$			24.246
			(20.353)
$\text{FLS drawdown}_i \times \text{Post Amendment}_t \times \%(\text{Mortgages})_{i,2012}$			-34.996**
			(16.253)
Bank × Product fixed effects	Yes	Yes	Yes
Product × Time fixed effects	Yes	Yes	Yes
Bank-level controls × Post FLS_t	Yes	Yes	Yes
Mortgage-level controls	Yes	Yes	Yes
Observations	417,819	467,589	242,379
Kleibergen-Paap F-statistic	25.3	21.4	

Notes: The table shows loan-level regression results for several variants of equation (1) capturing the announcement, drawdown window, and amendment of FLS2 (see Section 7). The sample period varies across columns; the sample period in column 2 excludes November and December 2013. $\text{Spread}_{i,l,t}$ is the interest rate on mortgage l originated by bank i in month t, net of the maturity-matched OIS (overnight indexed swap) rate. $\%(\text{Wholesale})_{i,2012}$ is bank i's ratio of total wholesale funding to total assets as of 2012:Q1. FLS drawdown_i is bank i's ratio of total FLS1 borrowing to total assets. All variables involving FLS drawdown_i are instrumented by corresponding variables involving $\text{Initial allowance}_i$, i.e. bank i's ratio of initial FLS1 borrowing allowance to total assets (measured in June 2012). Post Extension_t is an indicator variable equal to one after April 2013. Post FLS2_t is an indicator variable equal to one after February 2014. Post Amendment_t is an indicator variable equal to one in November and December 2013. $\%(\text{Mortgages})_{i,2012}$ is bank i's ratio of mortgages to total loans as of 2012:Q1. Bank-level controls and mortgage-level controls are as detailed in Table 4. Standard errors are clustered by bank and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.