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 only 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 compresses the price 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.

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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 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, private funding could get crowded out, and the transfer of private risk to the public sector could create moral hazard (Bolton et al. 2009). Another potential concern is that public funds could support bank activities other rather than lending to the real economy. And if reducing such leaks requires adding "strings attached", this could make public funding less attractive 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 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 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 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 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 reduces loan rates by more after the announcement – irrespective of how much they use the scheme. While participation in the scheme also translates into 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 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 (TLTRO). However, unlike the TLTROs and most other recent central bank funding schemes, the announcement of the FLS did not coincide with policy rate cuts, new asset purchase announcements, or government credit support schemes, which facilitates identification. In addition, the FLS has been 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 would benefit more from that effect, and would need to borrow less FLS funds as a result. The data is in line with the second hypothesis: when a bank's wholesale exposure goes up by 10 percentage points, FLS take-up as a proportion of initial

borrowing allowance goes down by 0.6 percentage points.

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 on estimating how this fall in private market wholesale funding costs 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 exploit pre-determined heterogeneities in wholesale funding reliance in loan-level differences-in-differences regressions, controlling for confounding trends with fixed effects and a host of controls.[1] To control for the participation effect, we 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 30% (our sample average) would reduce mortgage spreads by 24 to 83 basis points after the FLS announcement. This suggests that the equilibrium effect is economically large. To put it into context, over the year before the FLS was announced, wholesale funding costs had increased by around 100 basis points, and mortgage spreads had risen by around 55 basis points. Importantly, this equilibrium effect remains statistically significant – and becomes economically larger – 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.

[1] Carpinelli & Crosignani (2021) also use measures of funding structure to estimate the impact of central bank funding schemes, without distinguishing between the equilibrium and participation channels.

First, the availability of public funding could reduce banks' riskiness in the eyes of wholesale market lenders such as corporate depositors or money market mutual funds ("risk channel"). Second, the existence of an outside funding option could reduce banks' (expected) demand for wholesale funding and therefore reduce the price of wholesale funding ("demand channel").

Exploiting granular confidential data on banks' liabilities structures, we find evidence in line with the risk channel and 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, and not exposure to stickier long-term funding. In addition, the equilibrium effect is significant when the FLS is first announced in 2012 in a context of stressed markets, but not when a new FLS program ("FLS2") is announced in 2013, at a time when indicators of wholesale funding stressed have 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 benefits of central bank funding, but also to avoid any non-pecuniary costs associated with this funding. 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 main shocks to the reach of this conditionality.

First, in April 2013, the BoE announced a second wave of FLS funding ("FLS2"), to start in February 2014. During the transition period between these two waves, 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, these FLS2 drawings 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 period, banks more reliant on FLS funding reduce spreads on new mortgages.

Second, in October 2013, the BoE unexpectedly amended the terms of FLS2. In order to incentivise corporate lending, mortgage lending 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 bank lending. In addition, during the short time window before the amendment becomes binding, we find that banks more reliant on FLS reduce mortgage spreads, consistent with an attempt to secure future FLS funding 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 enjoy the benefits of central bank funding while avoiding its costs. So far, empirical studies have mostly focused on comparing participants and non-participants. Similarly to us, these studies find that this "participation effect" boosts credit supply (for example, Benetton & Fantino (2021) and Benetton et al. (2021)). But we show that in terms of the scheme's aggregate effect, this participation effect makes a smaller contribution than the equilibrium effect. This suggests that funding schemes can be significantly more powerful than previously thought.[2] However we also show that the equilibrium channel only

[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

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 separating roles of participation and equilibrium effects in that context.

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 does not hinge on there being any participation in the FLS, 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, a scheme that allowed banks to sell eligible business loans to the Federal Reserve. Even if loan sales were low, participant banks still increased supply or business loans outside the program. The authors attribute this effect to the idea that participating and therefore having an option to sell loans and transfer risk exposures reduces banks' risk aversion and expected balance-sheet constraints. In contrast, we study a program where targeted loans must be retained by the originator; therefore implications for expected credit risk and balance sheet constraints cannot explain our key result. Our mechanism can also extend to all banks (participants or not) through demand effects in funding markets rather than only to participant banks, in line with Farhi & Tirole (2021). In addition, we can disentangle and compare the equilibrium and participation effects, which operate through different mechanisms and channels, and study how these effects vary with market conditions. These points are important for understanding when and how policymakers can expect funding for lending schemes to be 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

methods, whereas we exploit loan-level data for identification. Using a structural model, Albertazzi et al. (2022) estimate that TLTRO had an (unobservable) benefit of reducing the probability of runs on European banks.

[3]Our focus on the impact of the announcement of funding-for-lending scheme rather than the effect or actual lending operations echoes a large literature studying the impact of announcement of central bank purchases of government or corporate bonds (see e.g. Gagnon et al. (2011) and Boyarchenko et al. (2022)).

results suggest that focusing on take-up risks 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 conducive to 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 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 that targets a broad range of sectors would be seen as a closer substitute for private funding; this would reinforce the "equilibrium" effect and make the scheme more powerful overall. But the more that's the case, the less conditionality actually binds, and the bigger the risk that the benefit of the scheme spill beyond the targeted sectors.

[4]For instance, TLTRO-I allowances were partly linked to new net 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.

2 Data and sample

Our main source of data on bank lending is the Product Sales Database (PSD), a confidential regulatory 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 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 "standard variable rate" that significantly exceeds the original interest rate; the vast majority of borrowers therefore refinances 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). Rates available for different combinations of fixation period and LTV 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 data in time (see Section 7). We focus on vanilla fixed rate and adjustable rate mortgages.

We match PSD to quarterly regulatory balance sheet and income statement data from the Bank of England, as well as 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 4.1) as well as bank-level control variables. After these matchings, our baseline matched sample consists of 415,671 loans.

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 importance of fees. Second, we use a confidential Bank of England regulatory dataset (FSA 47/48) 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 Eurozone 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

In a speech given on 14 June 2012, Governor Mervyn King announced that the Bank of England would launch a funding-for-lending scheme 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" and the associated "private sector risk aversion". 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 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]

[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.

Loans would be secured by collateral eligible for discount window borrowing, i.e. portfolios of loans, asset-backed securities, covered bonds, and sovereign and central bank debt pre-positioned with the Bank of England.[8]

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 course of the drawdown window. 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. If a bank obtained FLS funding via initial allowances, this funding could be used to fund any asset; this includes e.g. loans to sectors not targeted by the FLS such as financial firms. In that case, FLS funding can therefore be considered unconditional funding. In contrast, if a bank obtained FLS funding via additional allowances, it can be considered as conditional funding: since these allowances could only be generated by new lending to the targeted sectors, in that case FLS funding could effectively only fund loans to these sectors. We exploit this heterogeneity in conditionality between initial and additional allowances in Section 7.

Turning to pricing-based mechanisms, the rate on 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 0.25% only (the level of the Bank of England's policy rate at the time); instead if lending declined, the fee would increase linearly to a maximum of 1.5%. That pricing effectively ensured that, as long

[8] https://www.bankofengland.co.uk/-/media/boe/files/quarterly-bulletin/2012/the-funding-for-lending-scheme.pdf

as bank lending grew, the cost of FLS could not be higher than the cost of private funding – abstracting from non-pecuniary costs such as stigma or costs associated with conditionality. 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; this includes the ECB's Longer-Term Refinancing Operations (LTROs), which started in 2011. The key innovation of the FLS was to explicitly design the scheme to incentive 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 Long-Term Refinancing Operations (TLTROs), which started in 2014. As for the FLS, under TLTROs banks could borrow funds for up to four years, and borrowing allowances increased with outstanding and net new eligible loans to households and financial firms. Importantly however, mortgages did not count towards TLTRO borrowing allowances, unlike for the FLS. As we explain in the next section, however, mortgages were subsequently excluded from eligible loans for the second wave of FLS funding ("FLS2").

After the FLS, the Bank of England deployed two subsequent funding-for-lending schemes. Unlike the FLS however, these schemes were launched simultaneously with other major monetary or fiscal policy measures, which complicates identification. Specifically, the 2016 Term Funding Scheme (TFS) was launched in response to the Brexit vote alongside a round of government and corporate bond purchases and a Bank Rate cut. Meanwhile, the 2020 Term Funding Scheme with additional incentives for SMEs (TFSME) was launched in response to Covid alongside unprecedented QE and government interventions in the household and corporate sector.

3.3 Extension and amendment

Our baseline tests exploit the introduction of the original FLS ("FLS1"). However in further tests, we exploit the subsequent extension of the scheme. The original FLS drawdown window was set to close on 31 January 2014. On 24 April 2013 however, 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 firms in the last three quarters of 2013 (the FLS2 "reference period"). Similarly to the original FLS, additional allowances would then increase one-for-one with any net lending to households and firms during the FLS2 drawdown window.[9] In November 2013, however, the Bank announced that (unlike what had been announced previously) mortgages would not count towards lending eligible for additional borrowing allowances. This was motivated by a desire to "re-focus" the benefits of the FLS towards corporate lending, against a backdrop of rising house prices.

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. Before doing this, we motivate our hypothesis with a simple exploration of the determinants of 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 complement to private funding. In theory, such complementarity could arise if the mere availability of central bank funding liquidity 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 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.

To estimate the link between FLS participation and wholesale funding exposure we run a simple cross-sectional analysis using three alternative dependent variables. To capture the extensive margin of participation, we construct an indicator variable equal to 1 if the bank

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

participates, and 0 otherwise. For the intensive margin, we measure how much a bank borrows from the program, normalised by its initial allowance. To measure borrowing, we alternatively use the bank's average or maximum borrowing amount over the FLS drawdown window.

To measure a bank's pre-FLS exposure to wholesale funding, we use the ratio of wholesale funding to total assets as of 2012:Q1 ($\%(Wholesale)_{i,2012}$). We control for the bank's log total assets, cash ratio (% total assets), capital ratio (% total assets), and return on assets (income over total assets). When the dependent variable is an indicator variable (extensive margin), we use a probit model; for the other two continuous dependent variables (intensive margin), we use ordinary least squares.[10]

Table 1 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 total allowance in the average quarter during the drawdown window, and peaks at 4.3% of total allowances (these statistics include both participants and non-participants). For the average bank, wholesale funding constitutes 16.3% of total assets, with substantial variation across banks (the standard deviation is 23.9%).

The results are reported in Table 2. 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. Looking at intensive margin variables (columns 3-6), the estimates suggest that increasing wholesale funding exposure from 0% to 16.3% (the cross-sectional average) is associated with borrowing decreasing by 0.8 percentage points to 2 percentage points - i.e. around half of the average drawing. These results are in line with Fudulache & Goetz (2023), who find that Eurozone 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 when private funding is stressed. 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

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

availability of FLS funding without using it. We now discuss this idea in more details.

4.2 Theory

Motivated by these patterns, the main hypothesis we want to test is that the availability of central bank funding simulates 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, the FLS was launched in response to stress in UK wholesale funding markets, which partly reflected concerns around UK banks' (actual or perceived) exposures to the Eurozone. In addition, the cost of FLS funding was designed to fall below the cost of private funding at the time of the announcement and to not vary with a bank's riskiness. 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, indicators of wholesale funding costs increase sharply. When the FLS is announced, there is a sharp drop in these indicators.[11]

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.

[11] Weale & Wieladek (2016) and Churm et al. (2021) 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 a window around the announcement of the FLS in June 2012; the baseline sample starts in January 2012 and ends in June 2013, when a new FLS scheme was announced. We use 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); in line with this, indicators of wholesale funding 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 pre-determined and 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.

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 benefits to banks from directly participating in the FLS and therefore enjoying lower funding costs (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 – 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). 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 this 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, 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 use it 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. Second, we include $Initial\ Allowance_i$ directly in our regressions in a reduced-form approach.

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 products are defined by the combination of mortgage fixation periods and LTV bucket. This controls for any unobservable heterogeneity across banks that might correlate with our key coefficient $\%(Wholesale)_{i,2012}$; this includes 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 different across mortgage categories such as a change in the demand for mortgages by riskier borrowers. Mortgage controls include log(loan size), maturity, mortgage type (fixed or floating), rate reset period, LTV, 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. To guard against this issue, we include interactions between $PostFLS_t$ and a range of controls for bank characteristics: (log) total assets, cash (% total assets); income (% total assets), and equity (% total assets), all measured in 2012:Q1.

Another remaining challenge is that developments in the Eurozone crisis could affect UK banks' wholesale funding costs and therefore lending behaviour 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.[12] 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 Eurozone CDS spreads, which summarises changes in Eurozone risk perceptions over time.[13]

[12] 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 Eurozone might have affected UK bank funding costs, or the impact could have built more gradually over time.

[13] We collect daily 5-year CDS spreads for eight eurozone sovereigns (Belgium, France, Germany, Ireland, Italy, Netherlands, Portugal, and Spain) and twelve major eurozone 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 Eurozone crisis, with higher values indicating more stress. We then aggregate the principal component to the monthly frequency by taking the mean.

5 Main Results

Table 4 reports estimates from a range of increasingly conservative variants of our benchmark model. 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 prices in private wholesale funding markets, and that banks more exposed to these markets pass these lower funding costs through to mortgage spreads.

In general, the size of the estimate for β increases as the model becomes more conservative. For instance, relative to the specification without any controls (column 1), this estimate approximately doubles when we add fixed effects (column 2) and loan-level controls (column 3). The estimate is robust to controlling for developments in the Eurozone (column 4). In itself, the parameter estimate for the time-series Eurozone control is mostly 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 Eurozone 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 we show in Section 4.1, participation 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 add our control for the participation effect, the parameter estimate for our key coefficient for the equilibrium effect increases further in size. This is true both when we control for this channel in a reduced-form way by adding a measure of banks' initial allowances (column 5), and when we use this measure 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 statistic well in excess of 20.

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 banks reducing mortgage spreads after the FLS announcement. This result 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 of interest for the equilibrium effect separately for banks that do and do not participate. The two coefficient estimates are both 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

Across the different columns of Table 4, the parameter estimate for the equilibrium effect $\%(Wholesale)_{i,2012} \times PostFLS_t$ ranges between -0.75 and -2.6, with more conservative specifications yielding larger estimates. To assess the economic magnitude of this effect, we use the weighted average value of $\%(Wholesale)_{i,2012}$, where we weight by the number of mortgages originated during our sample period. Since a relatively small number of large banks account for a large share of UK mortgage lending, focusing on the weighted average gives a more accurate read of the aggregate impact of the FLS.

The weighted average wholesale funding exposure is 32% (see Table 4). Therefore, relative to a bank without any wholesale funding exposure, after the announcement of the FLS, a bank with an average wholesale funding exposure would see a 24–83 basis points reduction in mortgage spreads through the equilibrium effect. To put these numbers into context, over the year before the FLS, indicators of major UK banks' marginal funding costs had increased by around 100bps. Churm et al. (2021) estimate that, when controlling for confounding Eurozone developments, the FLS announcement reduced these costs by around 35–85 basis points.[14] Therefore, similarly to Churm et al. (2021), our results are consistent with the FLS offsetting a substantial share of the pre-FLS increase in wholesale funding costs. In addition, our results suggest that banks passed a substantial share of the fall in wholesale funding costs through to borrower via lower mortgage spreads.

[14]The smaller estimate is obtained by proxying wholesale funding costs with CDS spreads, whereas the largest estimate use senior unsecured bond spreads.

To add further context, over the year before the FLS, average two-year fixed rate mortgage rates had increased by 55 basis points; in our sample, average spreads for a 2-year 75% LTV mortgage peak at around 3.8% when the FLS is announced, and fall by around 130 and 210 basis points by April and October 2012, respectively. Therefore, our estimates are consistent with the equilibrium effect explaining a substantial share of the fall in mortgage spreads following the FLS announcement.

Meanwhile, the instrumental variable coefficient for the participation effect ranges between -6.3 and -10.4. For the weighted average bank, total FLS drawing amounts to 2.4% of total assets. This suggests that relative to a bank that does not draw from the FLS at all, after the announcement of the FLS, this representative bank would see a 15–24 basis points fall in spreads through the participation channel. This estimate is lower than the estimate obtained by Benetton et al. (2021), who focus only on the participation effect and measure it in a binary fashion by comparing participants to non-participants. Specifically, they estimate that the participation effect is 57–70 basis points, and that around two thirds of this is transmitted into mortgage rates.

Overall, our results suggest that the aggregate impact of the equilibrium effect on lending is substantially larger than the participation effect. This reflects the fact that the large banks that account for the bulk of UK mortgage lending have large wholesale funding exposures (and therefore benefit more from the equilibrium effect) and draw less on the FLS (as shown in Section 4.1). By contrast, the average bank in the cross-section of banks in our sample uses less wholesale funding and borrows more from the FLS. For such a bank, our estimates suggest that the participation effect is comparable or larger than the equilibrium effect.[15]

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

[15] For the cross-sectional average bank, wholesale funding exposure is 16.3% and FLS drawings is 5.1%. Relative to a bank without any wholesale funding exposure, for this bank the equilibrium effect is associated with a 12 to 42 basis points drop in mortgage spreads. Meanwhile, relative to a bank without any FLS borrowing, the participation effect is associated with a 32 to 53 basis points drop in mortgage spreads.

an outside funding option reduces prices in private wholesale funding markets, allowing banks more exposed to these funding markets to offer cheaper loans. In the next section, we shed more light on the mechanism underlying the equilibrium effect.

Before doing this, we explore a range of mechanisms that have been put forward by existing literature on central bank funding-for-lending schemes, and test whether controlling for these mechanisms changes the parameter estimate for the equilibrium effect $\%(Wholesale)_{i,2012} \times PostFLS_t$. To do so we consider a range of variations to the instrumental variable estimation of our baseline model.

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 below, we restrict our baseline panel dataset 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.[16] In line with Benetton et al. (2021), we find that higher fees are associated with lower loan spreads, and that controlling for fees reduces the estimated significance of the participation effect. Our key coefficient for the equilibrium effect is unchanged, however.

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. Second, we therefore control for UK banks' exposures to inflows of reserves from the QE program announced in July 2012 (a month after the FLS announcement). The results reported in column 2 show that our key coefficient is unaffected.

[16] 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. Similar to them 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 taking the actual rather than log amount, or using the ratio of fee to loan rate.

Competition

Next, we control for potential indirect effects of the FLS through competitive dynamics. Andreeva & García-Posada (2021) argue that if participating in TLTROs allows banks to offer lower loan rates, non-participants competing with participants might be forced to reduce loan rates as well or lower credit standards in 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 proxies, we first compute the average initial allowance, realised FLS borrowing, and wholesale funding exposure of banks active in each segment of the UK mortgage market, weighted by each bank's share of total lending in that market before the FLS. We define a market by the combination of a product (LTV bucket and maturity) 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 a given bank is active in, 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 are unchanged. This confirms that our equilibrium effect is different from the competitive channels in Andreeva & García-Posada (2021).

We then control for local competition. Benetton & Fantino (2021) find that banks that borrow more from the TLTRO increase lending more in Italian provinces with higher banking competition. To control for this, we construct a Herfindahl-index at the level of a market, and interact it with $\%(FLS\ Drawing)_i \times PostFLS_t$. The results in column 5 show that our key coefficient is 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 also reflect a compression in borrower risk premia. This could also be the case if the announcement of the FLS reduces banks' risk aversion. In line with this, 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.

Our regressions already control for standard measures of borrower risk (LTV, LTI, credit history, etc.). To provide further reassurance, we test how our key result varies across borrower risk categories. A reduction in credit risk or risk aversion should have a disproportionate 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 suggest that the equilibrium effect is *weaker* for high-LTV mortgages. Whilst we cannot rule out that the FLS affects banks' risk-taking through other dimensions, these results are suggest that the FLS lowers the cost of funding mortgages across the risk spectrum rather than by compressing premia on riskier mortgages.

6 Mechanism

In the previous section, we have reported evidence consistent with an *equilibrium* effect: when the FLS is announced, banks more exposed to wholesale funding offer cheaper loans – irrespective of how much they can expect to borrow from the FLS. This result is consistent with the idea that when banks have an outside option to obtain funding, this reduces prices in private wholesale funding markets, allowing banks more exposed to these funding markets to offer cheaper loans.

In this section, we confront two non-mutually exclusive mechanisms that could explain the equilibrium effect. First, the existence of a risk-insensitive outside option on public funding could reduce banks' funding liquidity risk – i.e. the risk of being unable to roll over funding at economical conditions.[17] All else equal, this *risk channel* should make private wholesale lenders require smaller risk premia on new loans to banks. Second, the existence of a funding outside

[17] In line with this idea, the aggregate evidence in Section 3 shows that CDS prices for major UK banks decrease strongly after the FLS announcement.

option could lower the price banks would be willing to pay for private funding. All else equal this demand channel should lower the mark-up that wholesale lenders can otherwise charge from borrowers.[18]

6.1 Evidence from banks' liability structure

To weigh up these channels, we start by exploiting granular confidential data on banks' funding structure broken down by maturity and currency.

To start with, we exploit heterogeneity in funding maturity by replacing our benchmark wholesale funding exposure variable $\%(Wholesale)_{i,2012}$ with separate measures for a bank's i) short-term (e.g. repurchase agreements (repos) or demandable uninsured deposits) and ii) longer-term wholesale funding (e.g. bonds or covered bonds). We do so because the risk and demand channels above have opposite predictions regarding which type of funding could drive our main result whereby banks more exposed to wholesale funding increase lending after the FLS announcement.

On the one hand, if the risk channel dominates, our main result is more likely to be driven by banks' exposure to shorter-term wholesale funding. This is because short-term funding entails relatively more funding liquidity risk relative to longer-term funding. Concretely, shorter-term wholesale funding is more likely to re-price after the FLS announcement. Therefore, all else equal, the cost of shorter-term funding is more likely to capture any impact from the announcement on wholesale funding costs and therefore mortgage rates.

Instead, if the demand channel dominates, our main result is more likely to be driven by banks' exposure to longer-term wholesale funding. Since FLS funding consists of four-year loans, banks are more likely to see FLS funding as a substitute for longer-term wholesale funding than short-term funding. (For instance, banks might have a preference for short-term wholesale funding like repurchase agreements to back reverse repos; it is therefore unlikely that they would consider FLS funding as a perfect substitute). Therefore, after the FLS is announced, wholesale lenders are likely to expect to see a larger fall in banks' demand for long-term wholesale debt

[18] 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.

than in the demand for short-term debt.[19] In turn, this means that the mark-up that lenders can charge on long-term wholesale funding is likely to fall by a relatively larger amount after the FLS announcement.

To measure exposure to short-term and long-term wholesale funding, we use our granular confidential regulatory dataset (FSA 47/48). The data reports the outstanding balance for different funding instrument categories, broken down by maturity. First, we exploit this information to measure each bank's exposure to wholesale funding expiring within six months ($\%(Wholesale < 6m)_{i,2012}$) from other funding ($\%(Wholesale >= 6m)_{i,2012}$). Second, we measure each bank's exposure to short-term funding ($\%(Wholesale\ Short)_{i,2012}$) and long-term funding ($\%(Wholesale\ Long)_{i,2012}$) defined as more or less than one year, *remaining* maturity. The main benefit of the first approach is that it can capture heterogeneity in remaining maturity between funding with the same original maturity.

Results are reported in Table 6. To ease comparison, in column 1 we replicate our benchmark regression using the FSA 47/48 data to construct our measure of total wholesale funding exposure ($\%(Wholesale)_{i,2012}$) rather than the data used in our main regression. The results are analogue to our baseline regression: the 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 understand the underlying mechanism, in columns 2 and 3 we replace $\%(Wholesale)_{i,2012}$ with our separate measures of short-term and long-term wholesale exposures, alternating between the two proxies above. For both proxies, the estimate for the interaction between short-term wholesale funding exposure and $PostFLS_t$ is negative and significant and similar in magnitude to the estimate for $\%(Wholesale)_{i,2012} \times PostFLS_t$ in column 1. In contrast, the estimate for the interaction between long-term wholesale funding exposure and $PostFLS_t$ is insignificant. In other words, our key baseline result in column 1 appears to reflect variation in banks' exposure to short-term wholesale funding. Since short-term wholesale funding is more conducive to funding liquidity risk and a less perfect substitute for FLS funding, this result is consistent with the risk channel and not with the demand channel.

[19] In line with this, 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 (presumably) longer-term funding.

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 then decompose our measure of long-term wholesale funding into a measure of long-term secured ($\%(Wholesale\ Long\ Secured)_{i,2012}$) and unsecured funding ($\%(Wholesale\ Long\ Unsecured)_{i,2012}$) and interact them with $PostFLS_t$.

If the risk channel dominates, we expect the coefficient for both these measures to be insignificant. 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 consists of long-term secured funding; under this channel, it therefore more likely that banks would see FLS funding as a 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 columns 2-3, the results reported in column 4 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 relationship with mortgage spreads after the FLS announcement.

Finally, in column 5, we decompose our measure of total wholesale exposure into a measure of exposure to wholesale debt denominated in i) Sterling and ii) Euros – the two most important currencies for UK banks' wholesale funding. Similarly, to maturity and collateralisation, the funding currency should not be relevant if our key result mainly reflects a *risk channel*. Specifically, if the FLS announcement mainly acts to reduce risk premia on UK banks' wholesale funding debt – e.g. reflecting these banks' exposure to the Eurozone crisis - that should be the case irrespective of currency; equivalently, if the FLS pushes down these risk premia, that should be reflected in the price of debt irrespective of currency. By contrast, issuance currency could be relevant to the "demand channel" to the extent that banks and lenders don't see Sterling and Euro debt as perfect substitutes. For instance, banks with mostly Sterling assets might prefer Sterling debt if cross-currency swaps are costly. And lenders with 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 Sterling wholesale prices. Results do not support this idea. Exposure to Sterling and Euro wholesale debt are both negative and significant and are statistically indistinguishable from

each other.

6.2 Evidence from the FLS extension announcement

To shed further light on the importance of the channel underlying the equilibrium effect, we now exploit the April 2013 announcement of the extension of the FLS (Section 3.3). This announcement is a useful laboratory because its impact should depend on the weight of these channels. When the extension was announced, indicators of UK banks wholesale funding costs had largely normalised (Figure 2).[20] At this point, wholesale funding costs were therefore unlikely to still incorporate significant risk premia associated with the Eurozone crisis. In this context, it seems unlikely that wholesale lenders would associate the extension of the FLS with a significant further reduction in UK banks' riskiness. As a result, if the risk channel dominates, the extension announcement should not trigger an equilibrium effect analogue to that we observe for the FLS announcement. However, that might not be true if the demand channel dominates. This is because the extension significantly prolongs the period during which banks have an outside option for private wholesale funding, and during which wholesale lenders might thus expect reduced demand from banks for private wholesale funding.

To confront these ideas, we use a variant of our baseline empirical model Equation (1) and test whether after the FLS extension announcement, banks more exposed to wholesale funding reduce their mortgage spreads by more. Our key coefficient of interest is $\%(Wholesale)_{i,2012} \times PostExtension_t$, where $\%(Wholesale)_{i,2012}$ is defined as in the baseline regression, and $PostExtension_t$ is 1 after the announcement of the FLS extension in April 2013 and 0 otherwise. If the risk channel dominates, we expect the corresponding parameter estimate to be insignificant, whereas it would be negative and significant if the demand channel dominates. To capture the announcement we shift our baseline sample period to start in July 2012 and end in October 2013, just before the amendment to the extension was announced. Our controls and fixed effects are otherwise similar to the baseline model.

The results are reported in column 1 in Table 7. In line with the first hypothesis, the parameter estimate for $\%(Wholesale)_{i,2012} \times PostExtension_t$ is insignificant. This is consistent with our results in Section 6.1, which were in line with a risk channel for the equilibrium effect

[20] See e.g. Bank of England's November 2014 Inflation Report.

and less with a demand channel.

7 The role of conditionality

We have established an equilibrium effect that allows banks can reap the benefits of central bank funding without 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 ask whether the equilibrium effect also allows banks to avoid non-pecuniary costs of central bank funding. In particular, we focus on costs associated with funding conditionality ("strings attached"), i.e. constraints on banks' ability to deploy funding to 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

To test the importance of conditionality, we first return to examining the impact of the announcement of the FLS extension. Figure 3 illustrates our research design. We compare bank' behaviour before and after the announcement of the FLS extension in April 2013. To isolate the impact of the announcement, we focus the sample on the transition period after the FLS1 window opens in August 2012, and before the FLS2 gets amended in October 2023 (we return to this amendment in the next section).

To assess the importance of conditionality, we test whether banks more reliant on FLS funding increase mortgage lending during the transition period between the two programs. This test exploits the fact that during this transition, increasing mortgage supply allows banks to obtain allowances for future *unconditional* funding (i.e. funding not contingent on lending to specific sectors), whereas banks' ability to obtain future *conditional* funding is unchanged. Therefore, any increase in mortgage lending during the transition period would suggest that banks prefer unconditional to conditional funding.

To see this, remember that allowances to borrow from the FLS are determined by two parameters (Section 3). First, "additional allowances" increase one-for-one with any loan to households and businesses extended *during the drawdown window*. Such funding thus constitutes

conditional funding. Second, banks receive "initial allowances" based on loans to households and businesses *before the drawdown window*. Once unlocked, such allowances can fund loans to any sector (or other asset), thus constituting "unconditional funding".

Under the FLS2 program, initial allowances were determined by a bank's total lending to households and businesses until the start of the FLS2 window. During this transition period, the FLS1 window remain open. Therefore, if a bank extended a new loan, it would both increase i) additional allowances from the FLS1, and ii) initial allowances from the FLS2. This added benefit can be attractive insofar as banks prefer to fund future assets via unconditional funding (initial allowances) rather than additional (conditional) allowances. And this attractiveness should increase with a bank's reliance on FLS funding.

To test this idea, we return to our estimates in column 1 of Table 7. This time however, we focus on how this announcement affects bank lending depending on a bank's reliance on FLS funding, as captured by *FLS Draw x Post Extension*. If banks more reliant on FLS funding have an incentive to unlock future unconditional borrowing, we expect the parameter estimate for *FLS Draw x Post Extension* to be negative and significant.

The results are in line with our prior: the more a bank borrows from the FLS1, the more mortgage spreads fall after the extension announcement. As we discuss above, this effect cannot reflect a change in the expected cost of funding mortgages during the transition period. In contrast, the finding is consistent with the idea that the during the transition period, increasing lending is more attractive because it increases (unconditional) initial borrowing allowances under the FLS2, which is especially valuable for larger FLS users. This supports the view that additional allowances are an imperfect substitute for initial allowances, and therefore banks have a preference for FLS funds without conditions. This result suggests conditionality plays a core role in reducing the value of the FLS to banks.

7.2 Effect of the FLS2 amendment

To further examine the importance of conditionality, we turn to the subsequent amendment of the FLS2 program. On 28 November 2013, the Bank of England and HMT announced that, unlike for the FLS1, and in contrast to the initial announcement of the FLS2 in April 2013, any new household lending during the FLS2 drawdown window would not generate any addi-

tional borrowing allowance.[21] The stated objective was to strengthen participants' incentives to expand business lending rather than mortgage lending, which was seen as unconstrained in a context of rising house prices.

For our purpose, this amendment is a useful laboratory because it tightens the conditionality of FLS funding. After the amendment, only loans to firms 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 costs of originating mortgages during the FLS2 window, and this effect should increase with a bank's reliance on FLS funding.

To test this idea, we interact our measure of FLS usage (*FLS Draw*) with an indicator equal to 1 after the FLS2 window opens in January 2014, and 0 otherwise (*Post FLS2*). As Figure 3 illustrates, our sample ends in December 2014, when a one-year extension of the FLS2 window is announced.[22] Meanwhile, the sample starts in [April] 2013, after the announcement of the FLS2 extension. We omit the transition period between the announcement of the amendment and the start of the FLS2 window (November to December 2013) during which the amendment does not bind yet (we exploit this in Section 7.3 below).[23]

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. To the extent that this effect would be matched by a higher benefit for corporate lending (which we cannot examine for lack of data), this would be in line with the stated intention of the amendment to "re-focus" the benefits of FLS funding away from mortgages.

[21] 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. The initial allowance was based on net lending to households and SMEs during the last three quarters of 2013; the additional allowance was based on lending to SMEs during 2014. It is credible that the announcement was unexpected; for instance the Council for Mortgage Lenders noted that the extension was "a surprise."

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

[23] During this transition period, the effect of the FLS2 on the cost of mortgage funding will not have taken effect yet. And as we discuss in Section 7.1 banks' incentives to originate mortgage are likely to differ in this period.

7.3 The "reference window" effect

Finally, we examine how the amendment affects banks' behaviour during the transition period after the amendment is announced and before it takes effect (November to December 2013), which we've omitted from our previous test. During this period, banks already know that mortgages will be excluded from lending eligible for additional borrowing allowances, but this exclusion does not apply yet. 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 their reliance on mortgage funding.

To test this idea, we interact our measure of FLS reliance (*FLS Draw*) with an indicator that is one during the transition period (November and December 2013) and 0 before (*Post-Amendment*). 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 funding, namely the ratio of mortgages to total loans as measured before the FLS in 2012q1 (*%(Mortgages)*).

Including this additional interaction term allows us to test how the reference window effect varies with the extent to which the exclusion of mortgages could affect a bank's ability to get FLS funds. Specifically, the coefficient for *FLS Draw x Post Amendment* captures the effect of the reference window for a bank with zero mortgage exposure. Presumably, such a specialised bank would be unlikely to plan any mortgage lending during the FLS2 window. Therefore, that bank would not have an incentive to increase mortgage lending during the FLS2 reference window to boost its FLS2 initial allowance. In line with this, the parameter estimate for *FLS Draw x Post Amendment* is insignificant.

In contrast, the sum of the coefficients for *FLS Draw x Post Amendment* and *FLS Draw x Post Amendment x %(Mortgages)* captures the reference window effect for a hypothetical bank that fully specialises in mortgages. For this bank, the re-focus strongly increases the value of originating a new mortgage during the reference period and therefore unlock additional allowances; instead originating a new mortgage just after the reference window would not unlock any FLS fund. In line with this idea, the sum of the two coefficients is negative and significant. In

other words, after the re-focus is announced, banks more reliant on FLS funding and that might be constrained in obtaining FLS2 funds tend to lower mortgage spreads during the reference window.

This result is line with our previous findings in that it reinforces the notion that conditionality matters: restricting the conditionality of FLS funding to a narrower range of sectors makes central bank funding less attractive to banks. This illustrates a key trade-off in the design of funding-for-lending schemes: the narrower the conditionality, and the larger the role of additional vs. initial allowances, the more central banks can ensure that funding supports lending to targeted sectors, but the smaller the equilibrium effect. This points to a trade-off between minimising leakages and stimulating the targeted sectors. In the present case, our results also suggests that, while the amendment appears to have the potential to succeed in reducing leakages to mortgage lending during the FLS2, this is partly offset by a reference window effect – albeit this is concentrated on some banks only.

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, and about the relatively low take-up of some of these schemes (BIS 2023).[24]

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 channel" 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

[24]During Covid, fourteen central banks deployed such schemes to stimulate and lending and 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).

bank, but also put downward pressure on the price of private funding – regardless of participation. 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.

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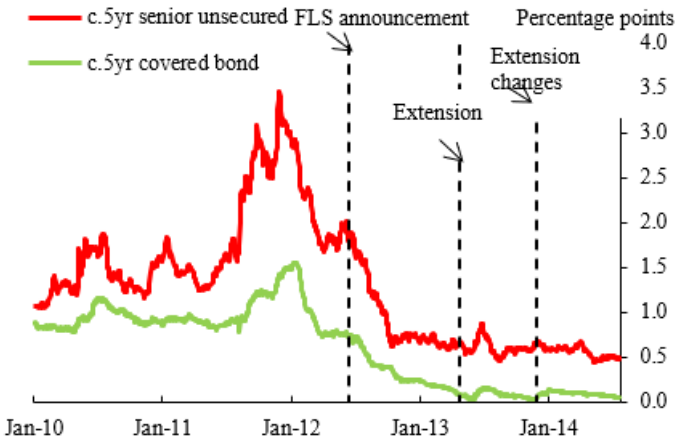
9 Figures and Tables

Figure 1: Bank funding costs

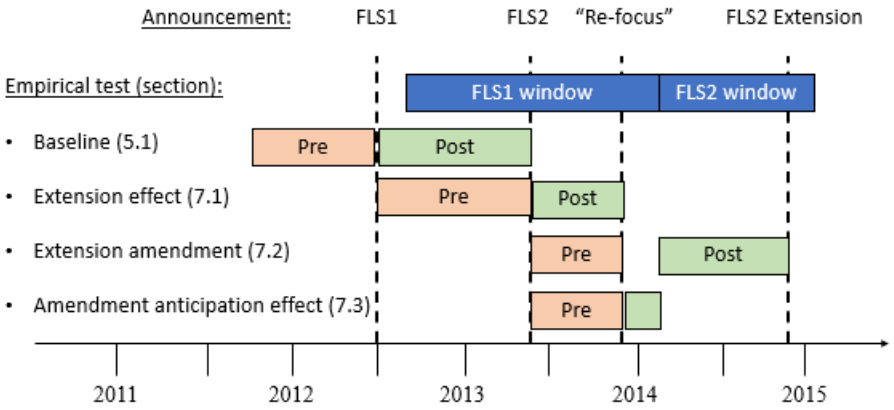
The chart shows measures of long-term wholeseale funding costs for UK banks. The blue line shows the difference between 5-year sterling LIBOR swap rates and 5-year sterling OIS rates. This provides a measure of expected bank credit risk premia over the five years ahead. The green line shows the average 5-year CDS spread across major UK banks. Source: Bank of England.

Figure 2: Bank term funding costs and FLS changes



The chart shows measures of long-term wholeseale funding costs for UK banks. The red line shows the spread on 5-year senior unsecured funding averaged across major UK banks. The green line shows the average 5-year spread on covered bonds for these banks Source: Bank of England.

Figure 3: FLS Timeline



This figure shows the timing of the FLS announcement, its modifications, and how they map into their corresponding empirical exercises.

Table 1: Summary statistics for participation cross-sectional analysis

	Mean	Std. Dev.	Min	Max
1 if bank participates, 0 otherwise	0.461	0.5017	0	1
Average Drawing / Initial allowance	0.0154	0.0620	0	0.512
Maximum Drawing / Initial allowance	0.0429	0.172	0	1.492
Wholesale Funding Ratio	0.163	0.239	0	0.991
Log Total Assets	7.526	2.691	3.749	14.250
Capital Ratio	0.175	0.0585	0.0740	0.452
Return on Assets	0.0521	1.838	-13.386	2.716
Cash Ratio	0.0459	0.0541	0	0.276

This table provides summary statistics for the variables used in the cross-sectional analysis. The bank balance sheet numbers are calculated as of 2012Q1.

Table 2: Wholesale funding exposure and propensity to participate

	(1)	(2)	(3)	(4)	(4)	(5)
Dependent variable:	**1 if bank participates, 0 otherwise**		**Average Drawing / Initial allowance**		**Maximum Drawing / Initial allowance**	
Wholesale Funding Ratio	-6.903***	-8.592***	-0.0583*	-0.0568**	-0.141*	-0.133***
	(2.392)	(-3.25)	(0.034)	(-2.23)	(0.0791)	(-2.84)
Log Total Assets	0.475***	0.607***	0.000915	0.0009001	0.0000734	-0.000869
	(0.143)	(0.159	(0.00165)	(0.00203)	(0.00921)	(0.00462)
Capital Ratio		5.825		0.215**		0.664**
		-1.62		-2.36		-2.64
Cash Ratio		-1.948		-0.0452		-0.028
		(-0.50)		(-0.54)		(-0.17)
N	71	67	71	67	71	67
R2	0.198	0.230	0.0182	0.743	0.0182	0.812

This table shows the econometric results for bank participation as a function of bank reliance on wholesale funding. The dependent variable for columns (1) and (2) is a dummy for whether the bank registers to participate in the Funding for Lending Scheme (FLS). In columns (3) and (4) it is the average amount the bank draws during the FLS, normalized to their initial allowance. Columns (5) and (6) use the maximum amount the bank draws, normalized to their initial allowance. The primary independent variable of interest in the Wholesale Funding Ratio. For each dependent variable, the model is run once controlling only for bank size and again with controls for their capital and cash ratios. All independent variable are calculated as of 2012Q1. The estimates use White robust standard errors. * = p<.1, ** = p<.05, *** = p<.01

Table 3: Summary statistics for baseline sample

Variable	Mean	Std. Dev.	Min	Max
Bank-level variables				
%(Wholesale)	0.32	0.16	0.00	0.65
FLS Drawing	0.02	0.03	0.00	1.49
Initial Allowance	0.02	0.01	0.01	0.04
Log Total Assets	12.17	1.97	3.75	14.27
Return on Assets	0.25	0.74	-5.77	5.57
Cash Ratio	0.06	0.03	0.00	0.20
Capital Ratio	0.19	0.04	0.06	0.32
Aggregate variables				
Eurozone CDS principal component	4.01	3.25	-1.01	8.00
Mortgage-level variables				
LTV	63.33	22.48	1.00	100.00
LTI	2.82	1.67	0.01	417.26
Mortgage Term	266.03	92.42	12.00	588.00
Log Loan Value	11.63	0.69	6.91	14.76
Borrower Age	39.01	9.69	18.00	91.00
First Time Buyer	0.23	0.42	0.00	1.00
Home Mover	0.43	0.49	0.00	1.00
Impaired	0.00	0.05	0.00	1.00
Brokered	0.49	0.50	0.00	1.00

This table provides summary statistics for the variables used in the baseline analysis. The bank balance sheet numbers are calculated as of 2012Q1.

Table 4: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Spread	Spread	Spread	Spread	Spread	Draw x Post FLS	Spread	Spread
Model:	OLS	OLS	OLS	OLS	OLS	IV Stage 1	IV Stage 2	IV Stage 2
Instrument:							Initial allowance x Post	
%(Wholesale)$_i$ x Post$_t$	-0.746***	-1.596**	-1.565**	-1.380***	-2.581***	0.0472	-2.115***	
	(0.147)	(0.616)	(0.593)	(0.481)	(0.670)	(0.0392)	(0.316)	
%(Wholesale)$_i$ x Euro$_t$				0.0593	0.0621	0.000812	0.0706	
				(0.0471)	(0.0474)	(0.000205)	(0.0474)	
Initial Allow.$_i$ x Post$_t$					-22.673***	2.176***		
					(7.152)	(0.340)		
FLS Drawing$_i$ x Post$_t$							-10.420***	-6.345***
							(2.395)	(1.366)
%(Whole)$_i$ x Post$_t$								-1.783***
x User								(0.208)
%(Whole)$_i$ x Post$_t$								-1.206***
x N-User								(0.298)
Bank x Post controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mortgage controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Bank-product FEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Product-Month FEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	415671	415671	415671	415671	415671	415671	415671	415671
R2		0.685	0.692	0.692		0.828		
Kleibergen-Paap							41.046	29.351

This table summarizes the main results for estimating equation (1). The analysis is done at the loan-month level. i indexes banks, t indexes month for mortgage variables and fixed effects and quarters for bank-level variables. The dependent variable is the mortgage spread over maturity-matched overnight interest swap rates. Columns (1)-(4) show OLS estimates with increasingly stringent controls and fixed effects, as described in section 4.3.2. Columns (5)-(7) control for the direct, participation effect of the FLS as described in section 4.3.1. Column (8) shows the results interacted with a dummy for if the bank participates in the FLS. The standard errors are clustered at the bank level. * = p<.1, ** = p<.05, *** = p<.01

Table 5: Alternative Explanations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Spread	Spread	Spread	Spread	Spread	Spread
Model:	IV	IV	IV	IV	IV	IV
%(Wholesale) x Post	-1.566***	-2.266***	-2.346***	-2.384***	-2.338***	-2.794***
	(0.253)	(0.355)	(0.351)	(0.374)	(0.359)	(0.384)
FLS Draw x Post	-2.054	-9.682***	-10.689***	-10.020***	-10.187***	-12.701***
	(1.316)	(2.088)	(2.835)	(2.099)	(2.352)	(2.916)
Log Fee	-0.000516***					
	(0.0000719)					
QE inflow		-14.519***				
		(6.094)				
Competitor FLS Draw x Post			-1.458			
			(2.947)			
Competitor %(Wholesale) x Post				-0.144		
				(0.250)		
FLS Draw x Post x Herfindahl					-0.144	
					(0.214)	
%(Wholesale) x Post x (LTV>75)						1.367***
						(0.374)
FLS Draw x Post x (LTV>75)						6.063**
						(2.966)
Bank x Post controls	No	Yes	Yes	Yes	Yes	Yes
Mortgage controls	No	No	Yes	Yes	Yes	Yes
Bank-product FEs	No	Yes	Yes	Yes	Yes	Yes
Product-Month FEs	No	Yes	Yes	Yes	Yes	Yes
N	67,895	415,671	384,034	384,034	413,895	415671

This table studies plausible alternative explanations for our main results, re-estimating equation (1) as detailed in Section 5.2. Column (1) controls for mortgage fees. Column (2) controls for inflows from Bank of England asset purchases during quantitative easing. Column (3) and (4) control for a bank's competitors direct and indirect channels. Column (5) controls for local banking bank concentration. Column (6) controls for changes in borrower riskiness. The standard errors are clustered at the bank level. * = p<.1,** = p<.05,*** = p<.01

Table 6: Mechanism for the main result

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Spread	Spread	Spread	Spread	Spread
Model:	IV	IV	IV	IV	IV
%(Wholesale) x Post	-2.134**				
	(0.839)				
%(Wholesale <6m) x Post		-2.232***			
		(0.648)			
%(Wholesale >=6m) x Post		0.699			
		(1.631)			
%(Wholesale Short) x Post			-2.320***	-2.186***	
			(0.653)	(0.526)	
%(Wholesale Long) x Post			2.879		
			(2.517)		
%(Wholesale Long Secured) x Post				-1.288	
				(1.119)	
%(Wholesale Long Unsecured) x Post				1.215	
				(1.366)	
%(Wholesale Sterling) x Post					-3.089**
					(1.225)
%(Wholesale Euro) x Post					-4.601***
					(0.522)
FLS Draw x Post	-3.554*	-5.794**	-10.010*	-10.031***	-10.309***
	(2.101)	(2.688)	(5.134)	(2.064)	(2.356)
Bank x Post controls	Yes	Yes	Yes	Yes	Yes
Mortgage controls	Yes	Yes	Yes	Yes	Yes
Bank-product FEs	Yes	Yes	Yes	Yes	Yes
Product-Month FEs	Yes	Yes	Yes	Yes	Yes
N	415,671	415671	415671	384,034	413,895
Kleibergen-Paap	68.934	131.956	25.970		

This table summarizes results for re-estimating equation (1), splitting out wholesale funding into its sub-components as described in Section 6. Column (1) reproduces the main result using the new dataset required for this exercise. Column (2) and (3) split wholesale funding by 6 months original maturity and 1 year remaining maturity, respectively. Column (4) then splits out long term wholesale funding into secured and unsecured. Column (5) then splits wholesale funding by currency. The standard errors are clustered at the bank level. * = p<.1, ** = p<.05, *** = p<.01

Table 7: Impact of the FLS extension

	(1)	(2)	(3)
Sample	Jul 2012 – Oct 2013	May 2013 – Oct 2014*	May 2013 – Dec 2013
WF_2012 * Post Extension	-0.240		
	(0.254)		
WF_2012 * Post Amendment		0.091	
		(.143)	
FLS Draw * Post Extension	-3.530**		
	(1.622)		
FLS Draw * Post Amendment		2.210**	24.246
		(0.947)	(20.353)
FLS Draw * Post Amendment * %(Mortgages)			-34.996**
			(16.253)
Bank * Post controls	Yes	Yes	Yes
Mortgage controls	Yes	Yes	Yes
Bank-Product FEs	Yes	Yes	Yes
Product-Month FEs	Yes	Yes	Yes
Kleibergern	25.304	21.389	
Observations	417819	467589	242379

* Excluding November and December 2013

The table summarizes results for studying the amendments and extension of the Funding for Lending Scheme as detailed in Section 7 and outlined in Figure 3. Column (1) estimates the effect of the extension of the FLS. Column (2) studies the amendment that excluded mortgages. Column (3) examines the period after it was announced mortgages would be excluded in the future, but before the exclusion began. The standard errors are clustered at the bank level. * = p<.1, ** = p<.05, *** = p<.01