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Abstract

This paper uses a large firm-level data set of UK companies and information on their pre-crisis lending relationships to identify the causal links from changes in credit supply to the real economy following the 2008 financial crisis. Controlling for demand in the product market, we find that the contraction in credit supply reduced labour productivity, wages and the capital intensity of production at the firm level. Firms experiencing adverse credit shocks were also more likely to fail, other things equal. We find that these effects are robust, statistically significant and economically large, but only when instruments based on pre-crisis banking relationships are used. We show that banking relationships were conditionally randomly assigned and were strong predictors of credit supply, such that any bias in our estimates is likely to be small.

Key words: Credit shock, financial frictions, productivity puzzle, firm-level data.

JEL classification: D21, D22, D24, G21.

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1 Introduction

The financial crisis of 2008 was associated with falls in corporate lending, business investment, labour productivity and real wages in the United Kingdom. What were the causal links between these events? Did firms retrench because they could not get financing? Or did they become pessimistic about demand for their products, and demand both less financing and fewer factors of production as a result? Empirical research in this area is only just beginning to adequately address the issue of causation.

This paper provides new evidence on the impact of the credit supply shock on corporate outcomes in the UK. Our aim is to identify the impact of the reduction in credit supply following the 2007/8 financial crisis on labour productivity, investment behaviour and average pay. We employ a new identification strategy that exploits information on pre-crisis lending relationships within a large firm-level dataset of UK companies.

In the UK, firms are required to register the identity of any party (a 'chargeholder') that has a claim on the firm's assets as collateral for a loan. We construct a proxy for pre-crisis banking relationships by identifying UK banks among these chargeholders. We show that these relationships are persistent, and that they help to predict the amount firms borrow after the crisis.

We exploit the stickiness of these relationships, together with the fact that different banks tightened credit conditions to different degrees, to generate exogenous variation in credit supply at the firm level. We then use this instrument for credit supply to quantify the impact of a change in total debt on firm outcomes, controlling for demand conditions in the product market.

Our identifying assumption is that banking relationships are only correlated with firm outcomes through their effect on credit supply, conditional on the control variables in our model. This puts our paper in a similar vein to Chodorow-Reich (2014), Edgerton (2012), Paravisini et al. (2015), Amiti and Weinstein (2013) and Bentolila et al. (2013).

This assumption will be violated if the banks which cut lending most during the crisis lent to firms which performed systematically worse, controlling for other observables. This could be due to reverse causation (bad firms harming their banks) or common causation (bad or risky decisions in several parts of the bank). We provide narrative evidence that the main cause of variability in banks' performance after the crisis was not their corporate lending decisions. Our identification strategy also allows us to report standard tests of overidentifying restrictions, which are typically not rejected. Our parameter estimates are statistically significant and economically large, but only when we address the endogeneity of credit volumes with two-stage least squares. OLS estimates are typically much smaller in absolute value, and statistically less significant. We show that this is consistent with the presence of large credit demand shocks at the firm level.

We find that firms who faced a reduction in credit supply experienced larger falls in labour productivity, capital per worker and average pay. Our results suggest that a 10% contraction in borrowing caused by credit supply led, on average, to a 5-6% fall in capital per head, a 5-8% fall in labour productivity, and a 7-9% fall in average pay for the affected firms.

We argue that the increase in the shadow price of capital caused firms to substitute towards more labour-intensive forms of production, which in turn lowered labour productivity growth. The estimated impact on labour productivity is large, suggesting that lower credit supply may also have been associated with lower levels of innovation and technological development. Average pay also fell further in firms more exposed to the credit shock, and in similar proportion to labour productivity, even though these firms were hiring labour in the same markets as less exposed firms. This observation lends support to rent- or risk-sharing theories of wage determination (Van Reenen (1996)) - in other words, firms were able to share some of their idiosyncratic productivity shocks with their workers. We also find that firms facing adverse credit supply shocks were more likely to fail.

Relative to the existing literature, our study makes two principal contributions. First, our paper is the first to our knowledge to look at the effect of the credit shock on labour productivity and wages in the cross section, both of which have been puzzlingly weak in the United Kingdom and a number of other economies after the financial crisis. Second, our paper is the first to use bank relationships to study the credit shock in the United Kingdom, an economy which is both heavily dependent on banks and which suffered a relatively large credit shock.

The remainder of this paper is structured as follows. In Section 2 we discuss how our work relates to existing studies of the impact of credit on corporate outcomes. Section 3 provides a brief overview of the behaviour of key macroeconomic variables in the UK since the 2008 crisis, and the structure of the UK banking system and corporate sector. Section 4 presents the dataset used in our analysis. Section 5 sets out our empirical methodology. Section 6 presents our results and compare them to existing estimates from the literature. Section 7 discusses the economic significance of our results. Section 8 concludes.

2 Existing literature

There are a number of studies that examine the importance of bank shocks on the provision of the supply of loans.

Kashyap and Stein (2000) show that the transmission of monetary policy is stronger for banks with less liquid balance sheets and that this affects their lending behaviour. While they do not discuss the economic impact of this contraction in lending, others have argued that this behaviour can lead to a slower growth. Peek and Rosengren (1997) find that Japanese banks cut lending in the US following deterioration in their parent banks' capital positions. They then go on to show in a follow up paper using regional data that this in turn affected US construction activity (Peek and Rosengren (2000)). Gilchrist and Zakrajsek (2012) use aggregate data to identify the impact of a credit shock on the macroeconomy. They find that shocks to the excess bond premium leads to elevated risk aversion in the financial sector, and in turn to a contraction in lending supply and economic activity.

A key challenge for research in this area is to disentangle firms' demand for credit from banks' supply of loans. Microdata can help address this concern.

Khwaja and Mian (2008) use matched firm-bank data to quantify the impact of a bank liquidity shock on the provision of loans to firms. More recently, others have taken this a step further: using matched firm-bank data to track the effect on overall economic activity. In particular, several papers have used exposure to different lenders just before the 2008 financial crisis, as a means of generating cross-sectional variation in credit supply during the crisis.

For the US, Greenstone and Mas (2012) use geographic variation in the pre-crisis market share of different banks across the US, along with variation in the credit crunch across banks, and finds that US counties in which poorly performing banks had bigger market shares saw fewer new loans, less employment and fewer business start-ups during the crisis. Edgerton (2012) uses data on lending relationships for a sample of equipment finance loans to identify the impact of restricting supply of credit to firms. He finds that variation across lenders accounted for around 17% of the decline in aggregate equipment financing, or about one-third of the total decline in financing of small businesses. Flannery et al. (2013) find that US firms which had relationships with banks with higher non-performing real estate loans borrowed less and invested less following the crisis. Chodorow-Reich (2014) measures banking relationships by identifying the lead arrangers for a given firm's syndicated loans. Having found that bank-firm pairs are sticky, he uses 'distressed' lenders as a proxy for restricted credit supply. He finds that employment fell more sharply during the crisis among the clients of less healthy lenders, particularly when those clients were small firms. The withdrawal of credit can explain roughly one-third of the employment decline in the sample in the year following the Lehman collapse.

Outside of the US, Amiti and Weinstein (2013) use matched Japanese bank-firm data over the period 1990-2010 to decompose loan movements into bank, firm, industry and common shocks. They find that idiosyncratic bank shocks have a large effect on investment. Paravisini et al. (2015) estimate the elasticity of exports to credit using matched Peruvian customs and firm-level bank credit data. They compare changes in exports of the same product and to the same destination in order to account for non-credit determinants of exports. They then compare the outcomes of firms borrowing from different banks that were differently affected by the 2008 financial crisis. Their results suggest that the reduction in credit reduced exports by raising the variable cost of production. Bentolila et al. (2013) merge the Spanish credit register with balance sheet data and find that Spanish firms who entered the crisis with relationships to weak banks experienced larger falls in employment.

Ongena et al. (2013) examine how corporate outcomes of firms that are dependent on credit differed from those that are not credit constrained. They focus on firms located in Eastern Europe and Asia, since the region was not initially affected by the global financial crisis. Their identification strategy relies on distinguishing between 3 types of banks according to whether they are domestic, foreign-owned or able to borrow on international wholesale markets. They find that banks with access to international wholesale funding cut back their lending by more than domestic banks; and that firms dependent on credit from these banks had lower returns on asset growth and lower revenue growth.

In summary, there is a small but growing literature using bank relationships to study the effect of credit supply on corporate outcomes, principally borrowing, investment and employment. Our paper is the first to do so in the UK, and to look at labour productivity, capital investment and average pay.

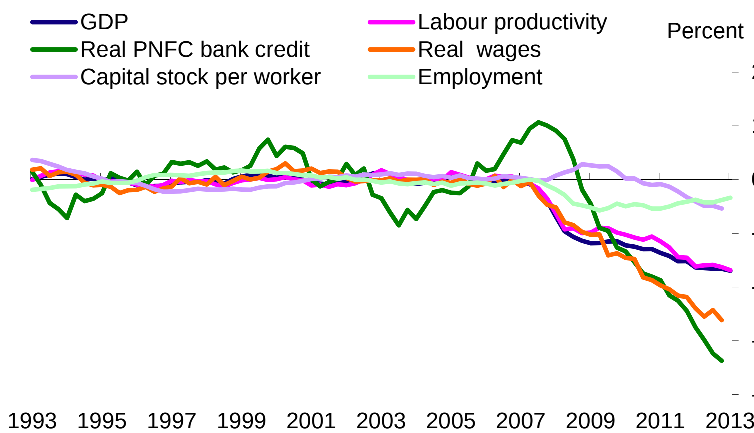
3 Macroeconomic context

The UK experienced a deep and prolonged economic recession following the 2008 financial crisis.

Labour productivity fell significantly in the aftermath of the crisis, and has stagnated since. This weakness in productivity has been puzzling, and is associated with surprisingly strong employment rather than weak output. It has been weaker in the UK than in many other advanced economies. A detailed discussed of the UK's productivity performance since the crisis is provided in Barnett et al. (2014).

The weakness in labour productivity has also coincided with a sharp fall in UK corporate borrowing and real wages (Figure 1). A key outstanding question is the extent to which the disruption in credit supply witnessed in the aftermath of the 2008 crisis has been a cause of the weakness in investment, productivity and wages.

Figure 1: UK macroeconomic data, relative to trend



Source: ONS; authors calculations. Notes: The chart shows the percentage point difference of each data series relative to its pre-crisis trend. Apart from GDP, all measures refer to the private sector. Real PNFC bank credit refers to the stock of private non-financial corporation (PNFC) loans, excluding the commercial real estate (CRE) sector, deflated using the GDP deflator. Trends are calculated as the log linear trend between 1993 and 2007 Q2.

3.1 Corporate access to credit in the UK

Firms in the United Kingdom are highly dependent on banks as a source of debt finance. The top 6 banks account for 70% of the stock of lending to UK firms (Bank of England (2013)). In fact, only about 250 firms have access to the public bond market and they account for only 12% of private employment. In addition, it is typically only larger firms that have access to equity markets with only a very small fraction of small firms gaining access.[1]

In their reliance on banks instead of bond and equity markets, UK firms are much closer to continental European firms than US firms; bank loans account for about three-quarters of euro area corporate debt, about two-thirds in the UK, and about one-quarter in the US (Pattani et al. (2011)).

Broadly speaking, the reason for firms' reliance on bank lending is because the cost of getting a rating from a credit reference agency is too high for many small firms – a prerequisite required by investors who rely on these scores as a signalling device about the riskiness and viability of the firm. As a result, firms without an external rating have greater difficulty raising equity from capital markets[2]. In other words, variation in the credit supplied by banks is, for most UK firms, coterminous with variation in the overall level of credit available to them.

3.2 The UK banking industry

The major UK banks - Barclays, HSBC, Lloyds Banking Group and the Royal Bank of Scotland (RBS) - had very different experiences during the recent financial crisis. Lloyds TSB and HBoS (Halifax Bank of Scotland) merged to form the Lloyds Banking Group (LBG). LBG, together with RBS, were subsequently part-nationalised by the UK government after a £50 billion capital injection in October 2008. Figure 2 shows the premia on credit default swaps on the senior unsecured bonds of the big four

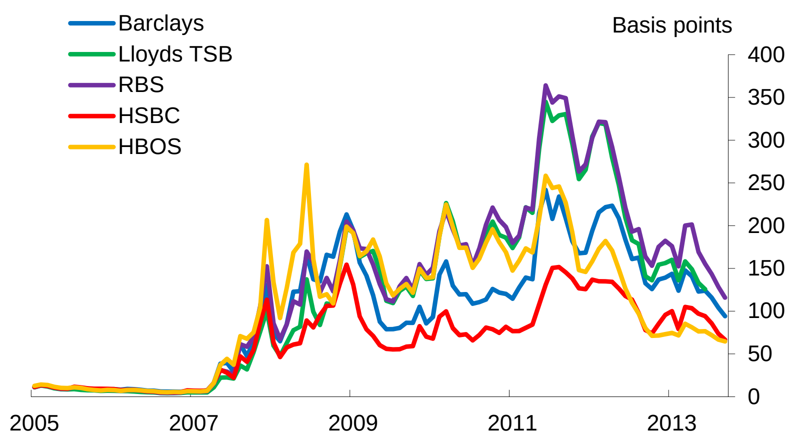
[1] A 2012 Department of Business Innovation and Skills (BIS) report found that 'a minority [of SMEs] use equity finance' and the proportions are 'too small to show on a graph' (van der Schans (2012)). Instead, they find that half of all SME's have used financial institutions to obtain finance. The remaining half did not seek funding or used other means not listed in their survey. Overall, BIS estimates suggest that in 2014 SMEs, which make up 97% of all firms in the UK, raised only £1.1 billion from private external equity compared to nearly £40 billion from bank lending British Business Bank (2014).

[2] More recently, firms have been able to raise equity from non-traditional sources (e.g. crowd funding) but those markets are tiny in comparison to traditional capital markets, and were not very relevant for the period in question anyway.

UK banks, a measure that is highly correlated with their funding costs. These were all very similar and at record lows before the crisis, but became high and dispersed after.

A necessary condition for our identification strategy is that this dispersion was not in large part caused by systematic differences in the health of UK banks' corporate loan books. Official narrative accounts of the failures of HBoS and RBS (FSA (2011) and PCBS (2013)) support this idea, laying the blame instead on trading book losses and reliance on wholesale funding. The key exception to this is the particularly large losses made in RBS' and HBoS' commercial real estate (CRE) portfolios.

Figure 2: UK bank CDS spreads

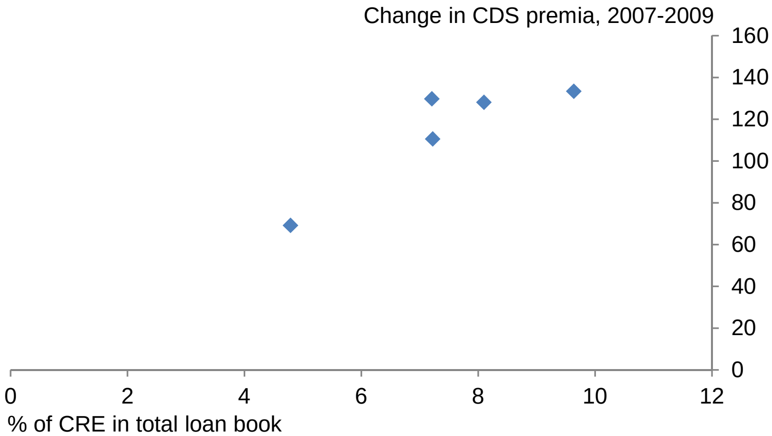


Source: MarkIT; authors calculations.

Commercial property played an important role during the financial crisis. The rapid pickup in debt tied to commercial property investments prior to the crisis contributed to the rapid acceleration in prices, and the subsequent falls in prices led to a sharp rise in non-performing loans. While debt write-offs on CRE loans picked up to around 2% after the crisis, this is likely to significantly understate the scale of non-performing CRE loans on bank's balance sheets. Recent Bank of England work suggests the median rate of forbearance on CRE loans across banks reached 20%, with the worst performing bank portfolio reaching nearly 50% (Benford and Burrows (2013)).

Figure 3 compares the share of UK CRE loans for the big four UK banks in 2007 with the change in funding costs, proxied by the change in CDS premia between 2007 and 2009. There are only five data points, but the positive correlation supports the notion that a banks' exposure to CRE was correlated with the subsequent pickup in its funding costs. For this reason, and in line with Bentolila et al. (2013), we exclude CRE firms from our sample.

Figure 3: Bank exposure to CRE and change in CDS premia



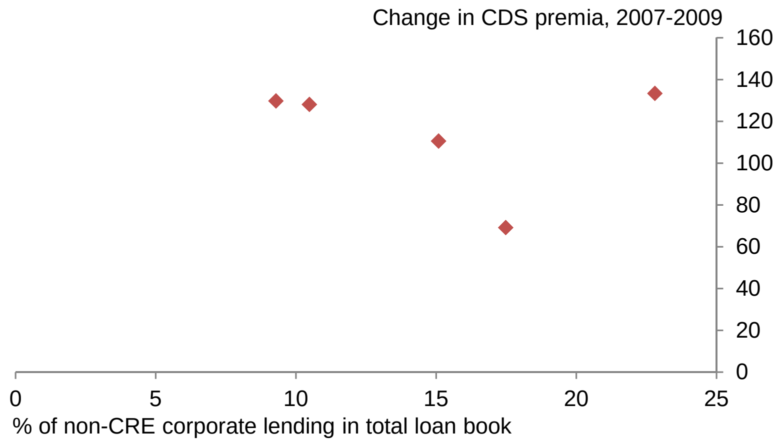
Source: Published annual accounts for Barclays, HSBC, Lloyds, HBOS and RBS in 2007; MarkIT; authors calculations. Notes: The proportion of commercial real estate (CRE) lending from each bank is calculated as the stock of loans and advances to the UK non-residential property sector relative to the total loan book (covering all countries). The non-residential property sector here includes lending to the construction sector - to ensure consistency in reporting across banks. Data for HSBC are for for total European CRE lending as a proportion of the total loan book (published UK data were not available).

Figure 4 compares the change in funding costs with each banks exposure to the non-CRE corporate sector. As shown, no obvious relationship stands out. This supports the idea that banks non-CRE corporate loan book did not cause the variation in funding costs and balance sheet health in the immediate aftermath of the crisis.

3.3 Credit supply following the financial crisis

By 2008, UK banks were under intense pressure to recapitalise. To help alleviate funding pressure, some initiatives were set up by the government and the Bank of

Figure 4: Bank exposure to non-CRE PNFCs and change in CDS premia



Source: Published annual accounts for Barclays, HSBC, Lloyds, HBOS and RBS in 2007; MarkIT; authors calculations. Notes: The proportion of private non-financial corporation (PNFC) lending from each bank is calculated as the stock of loans and advances to all industry sectors (excluding finance commercial real estate (CRE)) relative to the total loan book (covering all countries). Data for HSBC are for for total European PNFC lending as a proportion of the total loan book (published UK data were not available). Data for Lloyds include loans to financial services (which were grouped together with business services in their annual published accounts).

England (e.g. the Special Liquidity Scheme (SLS) and the Credit Guarantee Scheme (CGS) in April and October 2008 respectively). However, banks themselves had started to issue equity in order to improve their capital position. The top 4 UK banks issued £58.9 billion and £60 billion in equity in 2008 and 2009 respectively, but issued none in the years before and after that. Because the banks suffered shocks for different underlying reasons, the timing and quantities issued varied by bank. For example, HSBC issued no equity in 2008, but did so in 2009; Barclays issued equity in 2008 but not in 2009; and RBS issued equity in both years[3].

As a result, there was immense pressure on banks to cut back on lending, in particular during the acute period of 2008 and 2009. By 2008, lending growth to UK non-financial corporations had slowed down dramatically. Data from the Bank of England suggest that annual (3-month on 3-month) growth rate in corporate lending fell by 20 percentage points between 2007 and 2008, after it had been growing at an average

[3]This is important for our identifying assumption as not all the banks failed and when they did suffer serious problems, these were not occurring in unison.

rate of roughly 10% a year in the previous decade.

By 2010, the UK banking system had stabilized and funding positions had improved (although they still needed to build capital to move towards full Basel 3 compliance). Most of this was done organically by seeking to increase the proportion of their books funded by deposits.

This narrative chimes with the results presented in Section 6. We find that the effects are strongest for 2008 and 2009 (although the instrument is weaker in 2009). Our preliminary investigations found that by 2010 the results start to fade and the instrument fails to hold. While this may be due to firms switching banks or ceasing to trade, it is also consistent with the timing of capital issuance outlined above.

3.4 The link between credit and the real economy

Why should shocks to the availability of bank credit affect a firm's level of investment, productivity and average pay?

Consider a firm that produces gross output with two factors of production, capital K and labour L. These are combined with a technology, indexed by A, to produce a good or service, Y. For the sake of simplicity, using a Cobb-Douglas technology yields:

$$Y = AK^{\alpha}L^{1-\alpha}$$

Firms require credit for a number of reasons. They might borrow to finance investment today, spreading the cost over future years and leveraging against future profit streams. They might also rely on credit as a source of working capital, given that sales revenues often arrive with a lag.

Suppose that the cost of credit increases or, equivalently, the availability of credit to firms decreases. The effective cost of borrowing increases, raising the relative shadow price of inputs which are financed by credit. All else equal, this will lead firms to re-engineer their production processes away from credit financed inputs - most likely away from K and towards L. This can be shown in the following example. Assuming firms' profits are given by:

$$\pi = pAK^{\alpha}L^{1-\alpha} - wL - rK$$

where p is the price of output, w is the wage rate and r the cost of capital.

Assuming firms choose their level of capital with the aim of maximising profits, a positive shock to the cost of borrowing will lead to a reduction in the capital to labour ratio.

$$\frac{K}{L} = \left(\frac{\alpha pA}{r}\right)^{\frac{1}{1-\alpha}}$$

This process of adjustment is unlikely to be immediate, and frictions in reallocating resources within and across companies may mean that, in the short run at least, factor proportions may differ from their long-run levels. Depending on the speed of reallocation, the amount of output produced will be lower in the short run. It will also affect measured labour productivity, or output per head, as the ratio of capital, and perhaps other intermediates, to labour inputs will have fallen. This is illustrated by the following expression:

$$\frac{Y}{L} = A\left(\frac{Ap\alpha}{r}\right)^{\frac{\alpha}{1-\alpha}}$$

The effect on labour productivity could be even larger if lower levels of credit availability have a detrimental impact on technology A, either directly (Levine and Warusawitharana (2014)) or because new capital embodies improved technology.

The impact on labour inputs is less straightforward. It may be that the reallocation from capital to labour leads to upward pressure on wages and employment, as labour demand increases. Alternatively, it may be that constraints to working capital make it harder to pay workers, preventing firms from borrowing against future orders or invoice payments. This may lead to downward pressure on wages if firms share this additional cost pressure across workers. In addition, firms may pass on lower labour productivity on to wages, as the marginal product of labour is lower (Van Reenen (1996)).

This highly stylised model suggests that a credit supply shock should lead to a:

- decrease in labour productivity
- decrease in the capital to labour ratio within firms
- decrease in wages, if firms pass through the cost shock to workers

4 Data

Our dataset is compiled from information taken from the Bureau Van Dijk FAME database. This service extracts information from UK companies annual accounts that are submitted to Companies House - the official government data repository for all limited companies. Companies House is responsible for incorporating and dissolving companies, and as such, all limited companies in the UK are required to register with Companies House. The BvD database contains information on around 1.2m registered UK companies.

Companies House is a register for incorporated companies only: it excludes sole proprietorships and partnerships.[4] The Business Population Estimates (BPE)[5] is the only official data source in the UK that includes information on all firms, regardless of incorporation status. It captures information on turnover and employment size.[6] The

[4]Sole proprietorships and partnerships and firms that are run by either one or multiple self-employed people respectively.

[5]See ONS (2013) for further details

[6]The BPE incorporates data from the government business register, the Inter-departmental Business Register (IDBR), the ONS Labour Force Survey (LFS) and HMRC self-assessment data. Company data is obtained from the Inter-Departmental Business Register (IDBR), which contains all registered businesses obtained from Companies House. For partnerships and sole proprietorships,

BPE reports that there were just under 4.7 million enterprises in 2007 in the UK. Of those, the vast majority– three-quarters– were sole proprietorships and partnerships, which are not incorporated.[7] Almost all the remaining 1.2 million were small firms (less than 50 employees), however they accounted for only one-third of employment and turnover. In contrast, firms with more than 500 employees made up less than 0.1% of all enterprises but accounted for almost half of all employment and turnover.

4.1 Sample selection

The aim of this study is to examine the impact on companies of a contraction in the supply of credit from banks, measured through changes in companies' total borrowings. The key explanatory variable for our analysis is therefore the level of total debt held by individual companies, which we define as the total amount of overdrafts, short term loans and long term debt.

Since we are interested in examining the effects from the financial crisis, we select a cohort of firms that had positive levels of total debt in 2007. We then track the behaviour of these firms after the crisis. The total sample consists of around 85,000 firms.

The reporting criteria for Companies House, and therefore the BvD, varies by firm size.[8] Large companies are required to send their full balance sheet and profit and loss accounts to Companies House; medium sized companies can send abbreviated profit and loss accounts; and small companies can send abbreviated balance sheet data and are not required to send profit and loss information. As a result, there will be missing information on smaller firms.

in addition to IDBR, LFS has been used to obtain the estimates of unregistered businesses.

[7] These are self-employed individuals who were paying no employees other than themselves. They make up 17% of total employment.

[8] In 2007, to be classified as a small (medium) company by Companies House, a firm had to meet at least two of the following criteria for two consecutive years: (i) annual turnover was less than £5.6 million (£22.8 million), (ii) the balance sheet total was less than £2.8 (£11.4) million, or (iii) there were less than 50 (250) employees.

This raises two important issues when considering the sample for our analysis.

First, the panel is unbalanced. Not all firms submit information for each variable in each period. This will depend on whether they meet the appropriate reporting thresholds as well as whether they cease trading in subsequent years. To address this, we construct both balanced and unbalanced samples. The balanced sample contains firms that report the variables of interest in both years. The unbalanced sample contains all firms in either year.

Second, since there are various accounting exemptions available for SMEs, they will be under-represented in our sample relative to the population of companies.

To estimate how many firms are being excluded from our sample we use the SME Finance Monitor. The SME Finance Monitor is the largest survey of SME bank finance in the UK[9]. In 2011 it surveyed 5000 firms with up to 250 employees and a turnover of less than £25 million between March - May 2011. Their results are representative of the population and are stratified by size, sector and region. The report highlights that almost half of all SME's have never used any external funding between 2006 - 2011, and very small firms make up the largest component of this. We use this survey along with the BPE to estimate the number of firms that have accessed external funding.

Table 1 compares the number of small, medium and large firms across three sources. Columns 1 and 2 provide the breakdown in the BPE population dataset. As shown, around 97% or 1.2 million active companies are classified as small. Columns 3 and 4 combine these data with estimates from the SME Finance Monitor to give an estimate of the number of firms with bank debt. This suggests that around half of all small firms have bank debt, or around 690,000 firms, but this still represents around 96% of companies with bank debt.

Columns 5 and 6 show the number of companies in our sample. The reporting exemptions mean that, of the original sample of 86,378 firms, the final sample for analysis contains only 13,444 firms. Within this there are 4,150 small firms, making

[9]For details on the survey see BDRC Continental (2011)

up 31% of our sample and representing less than 1% of the estimated total number of small firms with bank debt. In contrast, we have 3,712 large firms, making up 28% of our sample, and representing nearly two thirds of the total number of large firms.

This implies that there are a large number of firms missing from our sample who may have debt, but have not submitted detailed accounts to BvD. These companies are more likely to be small, and are significant in number. This means our estimates will be unbiased with respect to the population that our sample represents, but will not be representative of the wider economy. We return to the issue of aggregation in Section 7.

Table 1: Total number of firms by size

	Population		Firms with debt		Our sample	
	Number (1)	% (2)	Number (3)	% (4)	Number (5)	% (6)
1-49	1,186,115	97.32	689,247	96.30	4,150	30.87
50-249	26,690	2.19	20,551	2.87	5,582	41.52
250+	5,920	0.49	5,920	0.83	3,712	27.61
Total	1,218,725	100.00	715,719	100.00	13,444	100.00

Source: ONS; SME Finance Monitor 2011; authors calculations. Notes: Columns (1) and (2) are taken from 2007 ONS Business Population Estimates. Columns (3) and (4) are an estimate of the total number of firms who have bank debt by firm size, based on estimates of the proportion of firms with bank debt from the 2011 SME Finance Monitor. Columns (5) and (6) show the number of firms in the final sample, consistent with the results shown in column (1) of Table 6.

4.2 Variable description and summary statistics

Table 2 compares summary statistics for key variables in our sample of firms in 2007. These include:

- Total debt: The total amount of overdrafts, short-term loans and long-term debt in a given year.
- Capital and capital per worker: The total amount of tangible assets, and the total amount per employee.

- Turnover per head: Total turnover divided by the number of employees. This is our preferred measure of labour productivity, discussed in more detail below.
- Average pay: Total remuneration divided by the number of employees. This is our measure of avereage wages.
- Employment: Total number of employees in a given year.

Measuring labour productivity from firm-level annual accounts is not straight forward. Ideally one would want a measure of gross value added (GVA) per employee.[10] GVA for each firm could be calculated as either the sum of (i) profits and wages or as (ii) turnover minus non-labour intermediate costs.

We experimented with both approaches, and both have drawbacks. The two main issues with (i) are lower reporting rates and the presence of negative values (since firms reporting a loss could have negative GVA) which introduces difficulties when comparing percentage changes over time. The main issue with (ii) is how best to measure non-labour costs, which aren't well reported in annual accounts. In any case, the correlation between estimates of GVA per head and turnover per head were high in our sample.[11]. For the remainder of this paper, results for labour productivity refer to turnover per head.

The reporting rates vary significantly for each variable. Employment and average pay have the lowest reporting rates. And, as is typically the case in firm-level data, the distributions of all our variables are heavily skewed to the left. To ensure our results are not disrupted by outliers, and as is now customary in this literature, we use symmetric growth rates for our calculations. This is discussed in further detail in Section 5.

[10] An alternative would be to construct estimates of Total Factor Productivity (TFP). However, this not a trivial task in firm level data due to the variability in production function specifications across firms.

[11] A simple regression of the change in turnover per head on the change in productivity, controlling for 2 digit industries, yielded a coefficient of 0.8 for 2008 and 0.9 for 2009 and both were statistically significant at the 1% level.

Table 2: Summary statistics, 2007

Variable	Mean	Median	Min	Max	Obs
Total debt (£'000s)	36,755	345	0	753,000,000	86,378
	[2,609,447]				
Capital (£'000s)	14,497	261	0	31,400,000	78,461
	[303,909]				
Capital per worker (£'000s)	328	17	0	408,474	28,296
	[5,025]				
Turnover per head (£'000s)	347	120	-8	230,680	21,410
	[2,549]				
Average pay (£'000s)	34	28	0	3410	28,102
	[43]				
Employment	555	67	1	507,480	29,131
	[6,014]				
Total sample size					86,378

Notes: Total debt is the total amount of long term debt and short term overdrafts. Capital is the total amount of tangible assets. Capital per worker is the total amount of tangible assets divided by the number of employees. Turnover per head is the total turnover divided by number of employees. Average pay is total remuneration divided by number of employees. Employment is the number of employees. Standard deviations are shown in square brackets.

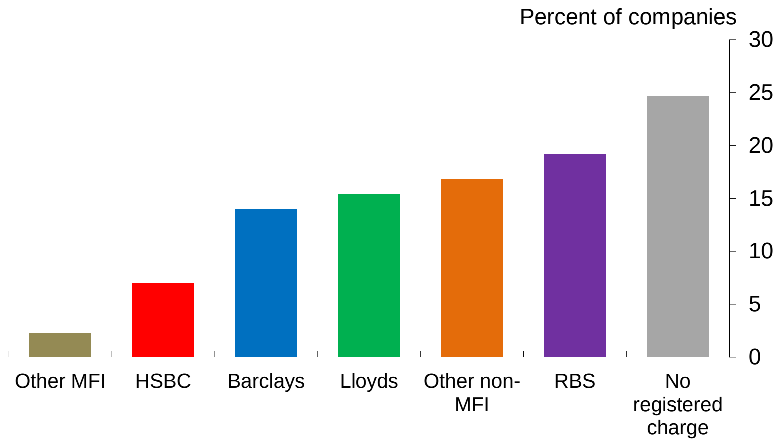
Table 11 (in Appendix B) compares an equivalently sized random sample of firms that do not report information on total debt. As shown, this second sample of firms is on average much smaller, both in terms of total capital and employment. They also have much lower reporting rates across the set of variables, suggesting that a large proportion are exempt from reporting more detailed financial information on account of their size. Given the distinct differences between firms reporting positive levels of debt and those without debt, one must exercise caution when generalising from our sample to all firms in the UK.

Table 12 (also in Appendix B) shows the mean of the change from 2007 for key variables in 2008 and 2009. There is a lot of variation across banks, but also across years.

4.3 Chargeholder information

Our identification strategy relies on information regarding pre-crisis relationships between companies and individual banks. To get this we extract information on registered charges from the BvD FAME database. Figure 5 shows the proportion of our sample with a charge registered to each bank in 2007.

Figure 5: Registered charge by bank in 2007



Notes: The chart shows the percentage of companies in the final sample for analysis with a charge registered to each institution, consistent with the estimation in column (1) of Table 4.

A charge is the security a company gives for a loan and must be registered at Companies House (the UK business registry) within 21 days. There are two types of charge: a fixed charge is a charge or mortgage secured on particular property; and a floating charge can be against all the company's assets, such as stock in trade, plant and machinery and vehicles. The BvD FAME database captures information on persons entitled to an outstanding charge raised at Companies House. It also includes information on when the charge was created and when it ended (when the loan matured). The way in which we encode this information for our econometric analysis is set out in Section 5.

Figure 6 shows the proportion of companies that started with an outstanding charge in 2007 with one of the four major UK banks and tracks whether or not they still had an outstanding charge to that particular institution in subsequent years. By the end of the sample period around 90% of companies still had an outstanding charge registered to the same institution as they did in 2007.

Figure 7 shows the proportion of companies that started off with an outstanding charge to one of the four major UK banks in 2007 and tracks whether or not a new charge was registered with a different institution. The chart shows that by 2011 only around 5% of companies had raised a charge with a different institution to the one they had an outstanding charge with in 2007. This suggests banking relationships - or at least the proxy in our dataset - appear to be very sticky.

5 Empirical approach and identification strategy

5.1 The baseline regression specification

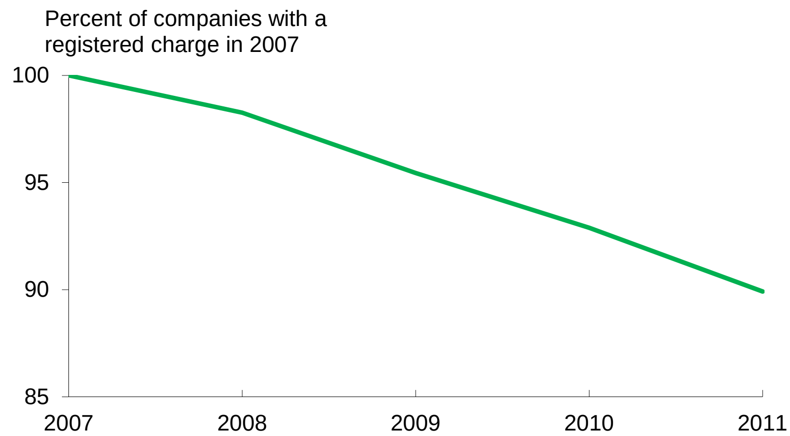
Our aim is to quantify the effect of a credit supply shock on various aspects of firm performance and behaviour, principally labour productivity, capital per worker, wages and firm survival.

The first question to address is how to quantify the size of the shock itself. If a firm suffers a negative credit supply shock, the amount it borrows will tend to fall. One natural and convenient choice of metric is therefore the amount of debt a firm has borrowed. Our baseline equation is thus

$$\Delta y_{it} = \beta_{0t} + \beta_{1t}\Delta d_{it} + \beta_{2t}x_{it} + \epsilon_{it} \tag{1}$$

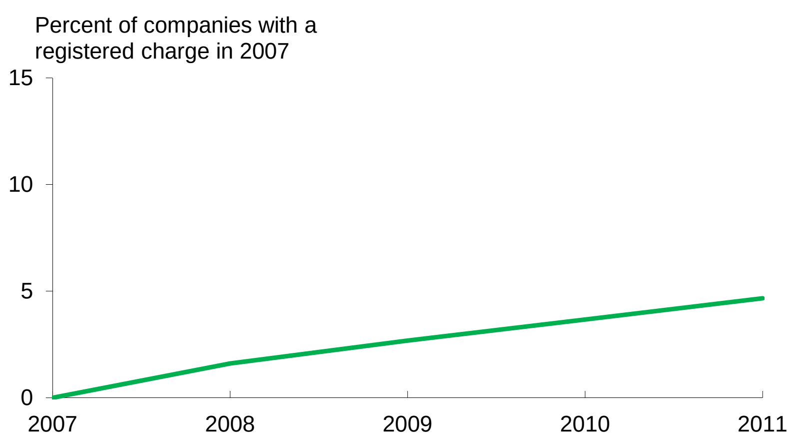
where i indexes firms, t is time, d is the stock of a firm's debt, y is the response variable of interest and x is a vector of observable firm characteristics. The vector of observables x is chosen to make the residual variation in pre-crisis firm characteristics uncorrelated with our instrumental variable, and comprises the following variables: 2-digit industry sector; whether the firm is a subsidiary, a parent company or a

Figure 6: Average loan durations



Notes: This chart shows the proportion of companies with an outstanding charge to the same institution to that in 2007. It is based on the full sample of 86,378 companies with positive levels of debt in 2007. This analysis only includes firms with charges linked to Barclays, HSBC, Lloyds and RBS.

Figure 7: New loans with other banks



Notes: This chart shows the proportion of companies with an outstanding charge in 2007 that have registered a new charge with a different institution over time. It is based on the full sample of 86,378 companies with positive levels of debt in 2007. This analysis only includes firms with charges linked to Barclays, HSBC, Lloyds and RBS.

standalone firm; the log level of turnover in 2007, and firm age in 2007 at time t.[12][13]

The amount a firm borrows will be driven by both the supply of and demand for credit. For example, a firm might reduce borrowing because of a reduction in credit supply, but also because it might want to dispose of physical capital or otherwise alter its capital structure. In each case, the correlation between credit and investment will be different. So a simple OLS regression of, say, investment on the change in debt will typically deliver biased estimates of the effect of a credit supply shock.

To see the determinants of the OLS bias, consider a very simple model of borrowing, d, output, y, and credit supply (perfectly elastic at interest rate r) at the firm level.

$$d = \alpha_1 y + \alpha_2 r + \epsilon_d \qquad \text{(Credit demand)}$$

$$r = \epsilon_s \qquad \text{(Credit supply)}$$

$$y = \gamma_1 \epsilon_s + \epsilon_y \qquad \text{(Output)}$$

In terms of the exogenous shocks of the model, realised output and borrowing are given by

$$\begin{aligned} d =& \alpha_1 \left(\gamma_1 \epsilon_s + \epsilon_y\right) + \alpha_2 \epsilon_s + \epsilon_d \\ =& \left(\alpha_1 \gamma_1 + \alpha_2\right) \epsilon_s + \alpha_1 \epsilon_y + \epsilon_d \\ y =& \gamma_1 \epsilon_s + \epsilon_y \end{aligned}$$

If we were simply to regress output on credit volumes, the expected value of our OLS

[12]We add a value of 1 to the level of turnover in order to include firms with zero turnover.

[13]When firms cease to operate, the left-hand side variable is typically not recorded or recorded at zero such that the log difference is undefined. We use symmetric growth rates - as is now common in this literature - to measure Δy_{it} and Δd_{it}. For instance, taking a variable z, the change in z from 2007 to t is calculated as follows

$$\Delta z_t = \frac{z_t - z_{2007}}{0.5\left(z_t + z_{2007}\right)}$$

parameter estimate would be

$$E\left[\hat{\beta}_1^{OLS}\right] = \frac{cov(y,d)}{var(d)}$$
$$= \gamma_1 \frac{\sigma_s^2\left(\alpha_1\gamma_1+\alpha_2\right)}{\left(\alpha_1\gamma_1+\alpha_2\right)^2\sigma_s^2+\alpha_1^2\sigma_y^2+\sigma_d^2} + \frac{\alpha_1\sigma_y^2}{\left(\alpha_1\gamma_1+\alpha_2\right)^2\sigma_s^2+\alpha_1^2\sigma_y^2+\sigma_d^2}$$

The OLS estimator is biased for two reasons. First, the bias is an increasing function of $\alpha_1\sigma_y^2$ - i.e. output shocks will bias the OLS parameter upwards to the extent that borrowing is an increasing function of output and that there are output shocks in the sample. Intuitively, if credit volumes are strongly increasing in output, and output varies autonomously a great deal, the parameter estimate in an OLS regression of output on credit volumes will be biased upwards. Secondly, credit demand shocks will bias the parameter towards zero, as they will raise the variance of the right-hand side variable in the regression.

For this reason, we adopt an instrumental variables approach. For each of the firms in our sample, we have information about the identity of any legal person with a charge on the assets of the firm. When the chargeholder is a bank, we take this as evidence of a possible banking relationship between the firm and the bank. These relationships are in turn an indicator of firm-specific credit supply on account of two features of the UK banking system in the recent crisis: the exogenous differences across banks in the severity with which they were hit by the credit shock (Section 3.2), and the stickiness of banking relationships (subsection 4.3).

The banking relationship b is correlated with credit supply but uncorrelated with any of the other shocks, such that $b = \mu\epsilon_s + \epsilon_b$. In expectation, our IV estimator is then

$$
\begin{aligned}
E\left[\hat{\beta}_1^{IV}\right] =& \frac{cov(y,b)}{cov(d,b)} \\
=& \frac{E\left[\left(\gamma_1\epsilon_s + \epsilon_y\right)\left(\mu\epsilon_s + \epsilon_b\right)\right]}{E\left[\left(\left(\alpha_1\gamma_1 + \alpha_2\right)\epsilon_s + \alpha_1\epsilon_y + \epsilon_d\right)\left(\mu\epsilon_s + \epsilon_b\right)\right]} \\
=& \frac{\gamma_1\mu\sigma_s^2}{\mu\left(\alpha_1\gamma_1 + \alpha_2\right)\sigma_s^2} \\
=& \frac{\gamma_1}{\left(\alpha_1\gamma_1 + \alpha_2\right)}
\end{aligned}
$$

This coefficient provides an unbiased estimate of the effect of credit supply shocks on output normalised by their effect on borrowing.

Concretely, our regression specification is the following two-stage least squares model:

$$
\Delta d_{it} = \theta_{0t} + \theta_{1t} b_{i,2007} + \theta_{2t} x_{it} + \mu_{it} \tag{2}
$$

$$
\Delta y_{it} = \beta_{0t} + \beta_{1t} \Delta d_{it} + \beta_{2t} x_{it} + \epsilon_{it} \tag{3}
$$

where $b_{i,2007}$ is a vector of seven indicator variables for the identity of the bank with which firm i had relationship with before the crisis.

The first four indicator variables represent a firm having a relationship with exactly one of the big four banks - Barclays, HSBC, Lloyds (including HBoS) and RBS (including NatWest). In view of the relative infrequency with which other banks appear in our sample, the remaining three respectively code for a relationship with a bank outside the big four, and a relationship with a non-bank, or a relationship for which there is no assigned chargeholder. We exclude observations for which a firm has more than one banking relationship with the big 4. These firms tend to be different and we find that our exclusion restriction does not hold for that group. These constitute only 6% of firms.

Our IV results are qualitatively robust to reasonable alternatives to this scheme. For example, we get similar results when we separately code relationships with the big banks, irrespective of whether the firm has relationships with other banks.

One reasonable alternative to this specification would be to group banks according to whether they are strong or weak, and therefore more or less likely to provide credit, and then perform a difference-in-differences analysis comparing firms who have relationships with these two groups. The problem with this approach is that it is not obvious how to group banks. On one hand, banks like RBS and Lloyds became so weak that they were nationalised. On the other, nationalisation itself may have prompted a change in lending policy and actually boosted credit supply from the affected banks (see Rose and Wieladek (2014) for evidence that nationalisation affected the lending of UK banks). Furthermore, banks facing tougher funding conditions may have had more of an incentive to provide loan support or forbearance to weaker firms to avoid having to realise further losses on their balance sheet. This might have had a positive impact on measured lending. For example, latest estimates suggest the scale of forbearance across non-CRE SME borrowers is likely to have been relatively small by 2013, but could have been higher immediately after the crisis (Arrowsmith et al. (2013)).

Conversely, our two-stage least squares approach does not require us to make assumptions about the ranking of banks, but uses the variation across all banks to identify the impact of the credit shock. This methodology allows us to be agnostic about which banks restricted credit and why.

Equation (1) is essentially a time-differenced version of the specification in Paravisini et al. (2015). Our dataset is an unbalanced panel of firms, so we could in principle estimate a variant of equation (1) in levels terms rather than first differences, controlling for time-invariant unobserved heterogeneity using standard methods. This approach would, however, suffer from a number of important problems. Most obviously, the credit shock itself varied over time and affected each bank differently over our sample. Furthermore, our identifying strategy, explained in detail below, relies on using pre-crisis banking relationships as an instrument for firm-level credit supply. Over the passage of time banking relationships will change and end for a variety of reasons. This means that the coefficients of equation (2) are highly likely to be unstable over time, and we verify that this is the case in the next section.

We therefore estimate our model in terms of changes between the year 2007, before the most serious phase of the credit crisis, and each of the post-crisis years 2008 and 2009 in our sample. Beyond 2009, our identification strategy begins to fail as pre-crisis banking relationships have longer to decay, and do so non-randomly, so we do not present results for later years.

5.2 Firm survival

An important question is whether the attrition in our sample due to firms ceasing to operate is random or not with respect to the other variables in our model. It seems likely that changes in credit supply will influence firm survival. Indeed, with our dataset we can also quantify the impact of credit supply shocks on firm survival. The right hand side of our regression model only includes *time-invariant* variables observed in 2007. This means that we can evaluate the predicted change in a firm's borrowing among dead firms, in other words how much a firm with similar pre-crisis characteristics would have been expected to borrow had it survived. This is a natural metric with which to assess the impact of predicted borrowing, and in particular the contribution of bank identity to it, as a determinant of firm survival. We construct a binary cumulative failure indicator f_{it} taking the value of zero if firm i is alive in year t and 1 if the firm failed in or before year t.[14] We then run logit regressions of firm failure or survival on the predicted value of credit supply, plus the non-bank controls in equation (2)

$$f^*_{it} = \alpha_t \widehat{\Delta d_i} + \beta_t x_i + u_{it} \tag{4}$$

$$Pr(f^*_{it} > 0) = F(\alpha \widehat{\Delta d_i} + \beta x_i) \tag{5}$$

The estimated coefficient on credit supply will therefore capture the effect that credit supply, as identified with bank ID and measured in units of credit volumes, has on

[14] We create a proxy for firm death by looking at whether a firm's status (when the data were collected) is not 'active' and looking for the first year in which balance-sheet data such as assets are either zero or missing: we assume the firm failed in that year.

bank survival. A negative coefficient would mean that firms which would have been able to borrow more, had they survived, would have been less likely to fail.

In principle, a reasonable alternative would be to estimate responses for both continuous left hand side variables and firm survival jointly, e.g. through a Heckman-type model. The problem is that with our data we lack an additional exclusion restriction and would need to rely on distributional assumptions and the curvature of the inverse Mills ratio for identification. For this reason, we focus here on the question of whether factors that predict credit supply among surviving firms also predict whether a firm survives at all.

5.3 Identification

Our identifying assumption is that a firm's banking relationships are correlated with its performance, conditional on observables, but only through the effect that bank identity has on credit supply. In terms of equations 2-3, these assumptions are respectively

$$E[\Delta d_i b_i] \neq 0 \tag{6}$$

$$E[\epsilon_{it} b_i] = 0 \tag{7}$$

Section 3 explained in detail the stickiness of banking relationships, which suggests that pre-crisis relationships will be a determinant of post-crisis borrowing opportunities. Subsection 6.1 below confirms statistically that pre-crisis relationships are very strong predictors of post-crisis borrowing. Together, these pieces of evidence establish that equation (6) - the relevance of our instruments - holds.

On the question of instrument validity, equation (7), there are two principal reasons why our identifying assumption might fail. The first possibility is reverse causation, in other words did firm performance lead to changes in credit supply rather than the other way round. If the firms who had relationships with a given bank performed systematically worse (conditional on observables) than others, say because that bank had selected riskier or less promising borrowers than others, causation could run from corporate performance to bank relationships - violating our identifying assumption.

This reverse causation seems unlikely in practice. Section 3 sets out narrative evidence that the main cause of variability in banks' performance after the crisis was not their corporate lending decisions (apart from in commercial real estate (CRE)). Given that we exclude CRE firms from our sample, we are omitting the major potential source of reverse causation from our sample. The second possibility is selection on unobservables, whereby a firm's performance and the lending behaviour of its bank are influenced by a common, unobserved factor. For example, suppose a given bank took above-average risks on both sides of its balance sheet in the lead-up to the crisis. In the event of a system-wide financial shock, its lending would have contracted more than average on account of funding difficulties, and its borrowers may have performed less well than average because they were more exposed to the economic cycle. We would then observe a conditional correlation between performance and borrowing at the firm level, but it would not be causal - another violation of our identifying assumption.

We provide three pieces of evidence which suggest that selection on unobservables is not present in our sample to a significant extent.

First, there was no significant correlation, conditional on our other observables, between bank relationships and our left-hand side variables before the crisis. If banks had been selecting firms based on their characteristics, such as level of riskiness, then we might expect to see statistically significant differences in our response variables y across banks conditional on observables characteristics *before* the financial crisis. We estimate the following equation for a series of outcome variables, denoted by y_{it}, in the period before the financial crisis. The outcome variables are gross profit, capital, employment, capital per head, turnover per head, and average pay and the equation is estimated using data from 2005 - 2006

$$\Delta y_{it} = \phi_0 + \boldsymbol{\phi_1} bank_{it} + \phi_2 x_{it} + t + e_{it}$$

where Δy_{it} is the symmetric growth rate between (2005 and 2006), $bank_{it}$ are the same set of bank indicator variables for whether the firm had a charge with one of the big four banks, another financial institution or a non-financial institution in 2007. The equation tries to capture whether some firms who had a charge with a certain bank grew faster than others before the financial crisis. We test whether the coefficients on the vector $\boldsymbol{\phi_1}$ of bank dummies are jointly equal to zero. Table 3 reports p-values from the joint F-test. The p-values show that we cannot reject results for the coefficients jointly being equal to zero on all outcome variables (although capital per head is slightly significant at the 10% level).

Table 3: Joint F-test of significance of bank dummies

$\Delta y_{i,2006-2005}$	P-value
Gross Profit	0.611
Capital	0.081
Employment	0.486
Capital per head	0.159
Average pay	0.215
Turnover per head	0.823

Notes: Table reports p-values from joint F-test of the bank indicator vector $bank_{i,2006-2005}$, where the dependent variable is the $\Delta y_{i,2006-2005}$

Second, and relatedly, in subsection 6.3 below we re-run our baseline two-stage least squares analysis but move the sample dates and selection rules two years back in time, attempting to predict response variables in 2006 and 2007 - before the most serious phase of the crisis - on the basis of banking relationships in place in 2005. We show that our instrument is not relevant before the crisis. The second stage regressions are also largely insignificant. This further supports the idea that there is no common causation driving our results.

Third, subsection 6.1 reports standard tests of overidentifying restrictions, which are typically not rejected at standard significance levels. Taken together, this evidence suggests that our instrument is both valid and relevant.

5.4 Standard errors

We calculate standard errors for our baseline regressions using a heteroscedasticity-robust estimator. At first sight, it might be warranted to cluster our estimates at the bank level. However, our instrument is already a bank-level fixed effect, making this redundant. Our first-stage equation regresses the change in debt at the firm level on the bank relationship dummies and other controls. Any variation that is common at the bank level will be absorbed by the bank dummies.

For the second-stage equation, our identifying assumption is shown in (7). This means that any residual variation at the bank level, of the sort that would require clustering of standard errors, would amount to a violation of our identification restrictions. In other words, if our standard errors are biased by not clustering, then so are the parameter estimates themselves. We provide evidence elsewhere in favour of our identifying assumptions. Furthermore, we have calculated clustered standard errors (not shown) and confirmed that they are generally very close to simple heteroscedasticity-consistent estimates.

We calculate standard errors and significance levels for the firm survival model (4)-(5) by bootstrap resampling with replacement from our full dataset. This is necessary because the samples for equations (4) and (5) are different, such that normal IV formulae for standard errors are not applicable.

6 Results

6.1 Baseline results

6.1.1 First stage regression

Table 4 sets out the results of the first stage regression (equation (2)) of the change in credit volumes on our vector of identifiers for bank relationships $b_{i,2007}$. The first columns show results from an unbalanced panel of firms while the second two columns show results for the balanced panel of firms who have data available in both years[15]. The identities of the banks themselves are anonymised.

The last two rows of the table show that the regression as a whole, as well as the joint test on θ_{1t}, is highly significant. The coefficients on $b_{i,2007}$ themselves are precisely estimated and in many cases bilaterally significantly different from each other. Coefficients on the other control variables are not reported for the sake of brevity.

6.1.2 Second stage regression

Tables 5 - 7 set out our main (second stage) results from equation 3. Each table shows all possible permutations of the time period in question (changes from 2007-2008 and 2007-2009), whether the sample is unbalanced or balanced (whether we restrict attention to observations available in both years) and for for both OLS or IV estimation of equation (3). We report the elasticity, β_{1t}, on the change in debt, Δd_{it}. We also report the Sargan-Hansen test of overidentifying restrictions, the Kleinbergen-Paap F-Statistics of weak instruments and the P-value from the Angrist and Pischke F-test of excluded instruments from the first stage of the regression.

We first present results for capital per worker in Table 5. In both years, and for both the balanced and unbalanced sample, the OLS estimates of β_{1t} are much smaller than

[15]Note that firms which cease operation are also included in our sample. They are recorded as having zero output

Table 4: First stage, baseline sample

	Unbalanced		Balanced	
	2008	2009	2008	2009
	(1)	(2)	(3)	(4)
Bank 1	-0.084***	-0.076***	-0.092***	-0.067***
	[0.02]	[0.02]	[0.02]	[0.02]
Bank 2	-0.065***	-0.086***	-0.081***	-0.084***
	[0.02]	[0.03]	[0.02]	[0.03]
Bank 3	-0.072***	-0.064***	-0.077***	-0.057***
	[0.01]	[0.02]	[0.02]	[0.02]
Bank 4	-0.054***	-0.044**	-0.057***	-0.043**
	[0.01]	[0.02]	[0.01]	[0.02]
Other financial institution	-0.042	-0.051	-0.075**	-0.031
	[0.03]	[0.03]	[0.03]	[0.04]
Non financial institution	-0.051***	-0.051***	-0.054***	-0.047**
	[0.01]	[0.02]	[0.02]	[0.02]
Controls	YES	YES	YES	YES
Observations	13,444	11,418	10,471	10,471
R-squared	0.007	0.008	0.007	0.009
F statistic	3.692	3.911	8.939	3.840
P-value of regression	0.000	0.000	0.000	0.000
P-value of all banks	0.000	0.001	0.000	0.007

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from regressions on the change in debt, $\Delta debt_{it}$, on a vector of bank dummies in 2007. $\Delta debt_{it}$ is calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2007, where t is $(2008, 2009)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2007. Balanced columns control for number of firms that have data recorded in both years. Data are restricted to firms who have a positive level of debt in 2007.

their IV counterparts (although both are also statistically significant). In view of the argumentation in Section 5, this attenuation bias from OLS is consistent with most changes in firm borrowing being due to credit demand unrelated to the firm's operating business - i.e firms managing their balance sheets. Turning to the IV estimates, the coefficient for the unbalanced and balanced samples are similar in magnitude; the effects vary somewhat by year and sample, but by less than one standard deviation. Taken together, the IV results suggest that a 10% fall in borrowing due to a credit supply shock leads to a 5-6% reduction in capital per head. This suggests that a credit supply shock has large effects on the capital intensity of production at the firm level.

The diagnostic tests reported with the regression suggest that any bias in this estimate is unlikely to be very large due to any issues with instrument validity and/or relevance, especially in 2008: the Sargan test of overidentifying restrictions (where the null is that our instruments are valid) is not rejected at standard levels for either year, while the F-statistic for the joint significance of the instruments suggests that the probability of a large weak instrument bias is low for 2008 (Stock and Yogo (2002)).

Table 6 shows the results for turnover per head, our measure of labour productivity, as the dependent variable. Once again the IV estimates of β are reasonably well-determined and much larger in absolute value than their OLS analogues although, in contrast to the capital-per-head regressions, the latter are not statistically significant. The coefficients do get somewhat smaller over time, perhaps as firms have more time to adjust their factors of production. The OLS estimates of β_{1t} are economically very small and statistically insignificant. In contrast, our IV estimates are economically large and relatively precisely estimated. One again, diagnostic suggests suggest any bias from weak or invalid instruments is unlikely to be large.

Interpreting the numbers, columns (2) and (6) shows that a credit shock that reduces a firm's level of borrowing by 10% by 2008 or 2009 reduced labour productivity by 8% and 5-6% respectively. These are of a magnitude equal or proportionally larger than the effect of credit on capital per worker, a large effect. As shown in subsection

3.4, capital per worker is one component of measured labour productivity, technology is another. It could be that lower capital investment may have been associated with lower levels of innovation and technological development.

Table 7 shows our estimate of the impact on average pay. As above, the IV estimates are much larger in absolute value than their OLS analogues, and suggest that a firm-level credit shock reduces wages in approximately the same proportion as it reduces productivity. This suggests that firms were able to share some of the costs from the credit shock with their employees. It also suggests these firms were operating in a labour market that was non-Walrasian to an important extent, such that wages were not equalised across similar employees but differed ex post according to the credit supply experienced by their employers. Comparing them to Table 6, it seems that firms in our sample cut wages by roughly the same degree as labour productivity, supporting the notion that workers are paid their marginal product. In line with the results above, the diagnostic tests suggest that our our instruments are strong and valid in 2008, but less so in 2009.

When comparing IV estimates across our three response variables, the estimated parameters are typically closer to zero in 2009 than in 2008, and our instruments are typically weaker and closer to being invalid. Consistent with this, when we extend our analysis for 2010 and 2011, we find that our results completely fade: the instrument is measured as weak and the estimates of β_1 in the second stage are poorly determined and widely dispersed.

There are four possible reasons, two econometric and two economic, as to why this is the case. First, the instruments are weaker for 2009, which may be one reason why the estimated coefficient is smaller in absolute value (i.e. it may be biased towards the OLS parameter). Second, the sample shrinks between the two years, in part because some of the firms in the 2008 sample had gone bankrupt or become inactive by 2009. In the (likely) event that some of this attrition is non-random with respect to credit supply, the effect we observe among surviving firms will be different; it is noteworthy in this regard that the difference between the estimates for 2008 and 2009 is smaller on the balanced sample. Appendix A below shows that sample attrition

Table 5: Second stage, baseline sample - Capital per head

	Unbalanced				Balanced			
	2008		2009		2008		2009	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in debt	0.119***	0.528***	0.128***	0.584***	0.122***	0.611***	0.127***	0.472**
	[0.01]	[0.14]	[0.01]	[0.20]	[0.01]	[0.14]	[0.01]	[0.21]
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	13,444	13,444	11,418	11,418	10,471	10,471	10,471	10,471
Hansen-Sargan Stat		5.590		10.810		3.687		9.868
Hansen-Sargan P-values		0.348		0.055		0.595		0.079
Kleinbergen-Paap		7.227		3.632		7.381		2.967
P-value of excl. instruments		0.000		0.001		0.000		0.007

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from the second stage regression of the change in tangible assets per head, Δy_{it}, on the change in debt, $\Delta debt_{it}$, which is instrumented using a vector of bank dummies in 2007. Changes in debt and y are calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2007, where t is $(2008, 2009)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2007. Balanced columns control for number of firms that have data recorded in both years. Firms that have become inactive or are in liquidation are included in the sample as having zero turnover. Failed firms are included as having zero turnover and zero assets. Data are restricted to firms who have a positive level of debt in 2007.

Table 6: Second stage, baseline sample - Turnover per head

	Unbalanced				Balanced			
	2008		2009		2008		2009	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in debt	0.005	0.826***	0.003	0.533***	0.003	0.798***	0.009	0.603***
	[0.01]	[0.16]	[0.01]	[0.17]	[0.01]	[0.15]	[0.01]	[0.20]
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	13,444	13,444	11,418	11,418	10,471	10,471	10,471	10,471
Hansen-Sargan Stat		4.033		8.429		5.089		8.855
Hansen-Sargan P-values		0.545		0.134		0.405		0.115
Kleinbergen-Paap		7.227		3.632		7.381		2.967
P-value of excl. instruments		1.02e-07		0.00133		6.75e-08		0.00677

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from the second stage regression of the change in turnover per head, Δy_{it}, on the change in debt, $\Delta debt_{it}$, which is instrumented using a vector of bank dummies in 2007. Changes in debt and y are calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2007, where t is $(2008, 2009)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2007. Balanced columns control for number of firms that have data recorded in both years. Firms that have become inactive or are in liquidation are included in the sample as having zero turnover. Failed firms are included as having zero turnover and zero assets. Data are restricted to firms who have a positive level of debt in 2007.

Table 7: Second stage, baseline sample - Average pay

	Unbalanced				Balanced			
	2008		2009		2008		2009	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in debt	0.004	0.851***	-0.007	0.670***	-0.000	0.858***	-0.002	0.764***
	[0.00]	[0.15]	[0.00]	[0.18]	[0.00]	[0.15]	[0.00]	[0.22]
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Observations	13,444	13,444	11,418	11,418	10,471	10,471	10,471	10,471
Hansen-Sargan Stat		3.305		12.55		5.496		11.73
Hansen-Sargan P-values		0.653		0.0279		0.358		0.0387
Kleinbergen-Paap		7.227		3.632		7.381		2.967
P-value of excl. instruments		1.02e-07		0.00133		6.75e-08		0.00677

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from the second stage regression of the change in total remuneration per head, Δy_{it}, on the change in debt, $\Delta debt_{it}$, which is instrumented using a vector of bank dummies in 2007. Changes in debt and y are calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2007, where t is $(2008, 2009)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2007. Balanced columns control for number of firms that have data recorded in both years. Firms that have become inactive or are in liquidation are included in the sample as having zero turnover. Failed firms are included as having zero turnover and zero assets. Data are restricted to firms who have a positive level of debt in 2007.

may invalidate our Hansen-Sargan statistics, leading to spurious rejections of the null of valid instruments. Third, the effect of a persistent credit shock on productivity may fade over time, as firms have more time to reorganise production or finance expenditures through other means. Fourthly, the dispersion of credit supply across banks may have fallen in those latter years. The top 4 UK banks issued £120 billion in equity in 2008 - 2009, but not after that. As a result, the pressure on banks to cut back on lending, may have eased in 2010 and 2011 as their capital positions started to improve.

6.2 Firm survival

Table 8 sets out our estimates of the parameter α in equation (5), the effect of a reduction in credit supply on the probability of firm death. The results are negative and significant for all years under consideration. A negative parameter means that higher predicted credit supply (the amount a firm's borrowing would have changed, conditional on survival, based on its banking relationships and other observables) increases the probability that the firm survived up until the period in question. This suggests that a widespread contraction in credit supply will tend to increase corporate insolvencies across the economy, an effect we quantify in Section 7

Table 8: Firm survival logit regression, baseline sample

	(1)	(2)
	2008	2009
Predicted debt	-6.26***	-5.27***
	[1.08]	[0.82]
Controls	YES	YES
Observations	82,624	82,709

Notes: The table shows the estimated coefficients for α in equation (5).

6.3 Placebo test

Our identifying strategy works by exploiting variation across banks in the changes in the terms on which they supplied credit during the financial crisis. This variation was likely to have been relatively large during the period of funding and credit market turbulence, asset-price volatility and bank nationalisation of 2008-9. Conversely, if our instruments are valid and banking relationships are randomly assigned with respect to corporate outcomes, our instruments are more likely to be only weakly relevant at a time of tranquil market conditions. But if there is also endogenous variation in bank relationships that is relevant for credit supply and corporate performance, violating our identifying assumptions, then our instruments could turn out to be relevant when they should not be.

With this in mind, we re-run the regressions above but substitute 2005 in place of 2007 for our sample selection rule[16] and as the base year against which changes in debt, labour productivity and so on are measured.

Table 14 (in Appendix B) presents the results from the coefficient on the $\Delta debt_{t-2005}$, analogous to the first row of the balanced regressions seen in Tables 5-7. In contrast to our baseline results, the F-statistics for the first-stage regressions and the associated IV estimates of β are typically insignificant at standard levels. The only exception to this is when turnover per head is the dependent variable and the change is measured between 2005 and 2007, in which case the coefficient on change in debt is significant at the 10% level. This could be a false positive, or could suggest that the market turbulence experienced in the second half of 2007 may already have been having an effect on the real economy. However, in no case are our instruments relevant in 2006, a time of uniformly easy funding conditions across UK banks.

Both the failure of the placebo test and the lack of evidence of pre-selection of firms by banks before the crisis, provides greater confidence that our instrument is picking up credit supply shocks - and nothing else - in our baseline results for 2008 and 2009.

[16] We select firms in the Bureau Van Dijk database who had a positive level of debt in 2005 rather than 2007

7 Economic magnitude

This section provides a set of simple illustrations to demonstrate the economic significance of our results, by translating our microeconometric findings into macroeconomic numbers. These calculations, although very broad bush, imply that the impact of the credit shock on the macroeconomy may have been substantial.

Turning first to our baseline estimates. Table 9 sets out some simple steps to examine the possible contribution of the credit supply shock induced by the financial crisis on capital per worker, labour productivity and average pay.

To begin with, the first row looks at the deviation in the corporate debt stock, relative to its pre-crisis trend. Recent research by the Bank of England suggests that the vast majority of the initial fall in aggregate lending was due to credit supply shocks, rather than decreased demand (Barnett and Thomas (2014)). For the sake of illustration, lets suppose that all of the fall in the corporate debt stock by 2009 was due to restricted credit supply, and half of the fall by 2013. The intuition being that banks drove the immediate contraction in credit in the aftermath of the crisis, and as demand conditions worsened firms will have demanded less credit. We multiply this by the proportion of firms with debt and the relevant elasticity estimate from our regressions, to derive some simple predictions of the aggregate impact of the credit shock.

These estimates suggest that the credit supply shock could have lowered the level of labour productivity by 1-2% by 2009 and 5-8% by 2013. This compares to an aggregate shortfall of -10% by 2009 and -17% by 2013. Barnett et al. (2014) argue that by the end of 2013, structural factors - including reduced investment in physical and intangible capital, and impaired resource allocation - could explain around 6-9 pp of the deviation relative to pre-crisis trend. Our results are in a similar ball park, and suggest that the credit supply shock could have been a major underlying cause behind these factors.

These estimates predict that the level of capital per worker would be 1-2% by 2009 and 5-6% by 2013. One serious consideration to bear in mind here is comparability of the data. The measure of capital used in our data is the level of tangible assets among non-CRE PNFCs. The aggregate capital stock includes a wider variety of assets across all PNFCs. Nonetheless, they suggest that by 2013 a large part of the 6% deviation in capital per worker may have been driven by reduced credit supply.

These estimates also suggest that the credit shock would have contributed to lower levels of average pay - by around 2% in 2009, and 7-9% in 2013 or up to a third of the deviation in real wages relative to their pre-crisis trend.

Table 9: Economic significance, baseline regressions

Calculation step	Capital per head %	Turnover per head %	Average pay %
Corporate debt relative to pre-crisis trend, 2009 (a)	-5	-5	-5
Proportion of fall in debt due to credit supply, 2009 (b)	100	100	100
Corporate debt relative to pre-crisis trend, 2013 (c)	-34	-34	-34
Proportion of fall in debt due to credit supply, 2013 (d)	50	50	50
Impact of 1% change in credit supply (e)	0.5 to 0.6	0.5 to 0.8	0.7 to 0.9
Proportion of firms with bank debt (f)	60	60	60
Predicted aggregate impact, 2009 (a)x(b)x(e)x(f)	-1 to -2	-1 to -2	-2
Actual deviation relative to pre-crisis trend, 2009	3	-10	-10
Predicted aggregate impact, 2013 (c)x(d)x(e)x(f)	-5 to -6	-5 to -8	-7 to -9
Actual deviation relative to pre-crisis trend, 2013	-6	-17	-27

Source: ONS; authors calculations.

Turning to firm survival. In a logistic regression, such as that defined by equations

(5), the slope parameter α implies that a one-unit increase in the explanatory variable increases the odds of the event by a factor e^{α}. So our estimate of the parameter α in equation (5) of around -5 suggests that a credit supply shock that would have reduced borrowing by 10% would raise the odds of bankruptcy by about $e^{-5x10\%} \approx 60\%$.

Corporate liquidations in England and Wales rose by around 60% between 2007 and 2009, while the stock of corporate debt fell by 7.5%. Assuming again that the fall in corporate debt over this period represented a shock to credit supply, then multiplying these numbers with the proportion of firms with bank debt suggests that the credit shock may have accounted for almost half of the pick-up in company liquidations. This calculation is illustrated in Table 10.

The estimates provided above are purely illustrative, but suggest the size of our parameter estimates in Section 6 are not only statistically significant but also economically large.

Table 10: Economic significance, firm survival

Calculation step	Percent
Change in corporate debt, 2007 to 2009 (a)	-8
Impact of 1% change in credit supply on probability of bankruptcy (b)	6
Proportion of firms with bank debt (c)	60
Predicted pickup in liquidations, (a)x(b)x(c)	27
Actual pickup in liquidations, 2007 to 2009	59

Source: ONS; authors calculations.

8 Conclusion

This paper provides new evidence on the impact of the credit supply shock caused by the financial crisis of 2008 on corporate outcomes in the UK.

We find that firms facing a reduction in credit supply experienced greater falls in labour productivity and capital per worker. We argue that this is due to an increase in the shadow price of capital causing firms to substitute towards more labour-intensive technologies in production. Our results suggest that a 10% contraction in credit supply led, on average, to a 5-6% fall in capital per worker and a 5-8% in labour productivity. The estimated impact on labour productivity is large, suggesting lower capital investment may have been associated with lower levels of innovation and technological development. However further research is needed in this area.

Our results also suggest average pay fell further in firms more exposed to the credit shock, and in similar proportion to labour productivity, even though these firms were hiring labour in the same markets as less exposed firms. We find that a 10% fall in credit supply led, on average, to a 7-9% fall in average pay for the firms affected. This suggests firms were able to share the impact of the credit shock with workers, through lower wage growth.

We find that firms facing adverse credit supply shocks were more likely to fail. Our results predict that a 10% decrease in credit supply would increase the probability of bankruptcy by 60%.

These parameter estimates are both statistically significant and economically large. They suggest that the credit supply shock caused by the recent financial crisis may explain around 5-8 percentage points of the 17% shortfall in labour productivity relative to its pre-crisis trend by 2013, half of the shortfall in wages, and nearly half of the pickup in company liquidations between 2007 and 2009.

Our identification strategy relies on pre-crisis banking relationships that decay non-randomly over time. A key limitation of this empirical design is that we are unable to say how persistent these effects might be. Although we are only able to identify the impact of the contraction in credit supply in 2008 and 2009, it may be that either the shock was more persistent or the effects on the corporate sector longer lasting.

On the intensive margin, it could be that firms are somehow permanently scarred by temporary credit shocks, or it could be that they are able to catch up to the counterfactual pre-crisis trend path once the credit shock abates. And on the extensive margin, this study does not tell us how firm entry is affected by credit supply, so we cannot say what happens to the factors that become unemployed when firms fail, either during or after the period of crisis. Barnett et al. (2014) suggests impaired capital allocation between firms may have been an important driver of the weakness in productivity. There are also likely to have been factors, not covered in this study, helping to keep unproductive firms alive after the crisis, including forbearance by banks and the tax authorities, and low levels of interest rates (Barnett et al. (2014)).

Overall, the durability of the productivity slowdown since the crisis make this an important avenue for future research.

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A Attrition and overidentification tests

To the extent that credit supply affects firm survival, it may also affect the results of the standard tests of overidentification restrictions used to assess instrument validity. Standard tests of overidentifying restrictions work by looking how far from zero our sample analogues $b'\hat{\epsilon}$ of the population moment conditions $E\left[\epsilon_i b_i\right]=0$ are. However, if there is nonrandom attrition in our sample, tests based on such overidentifying restrictions are unlikely to work. In particular, if the disturbances in the observation equation ϵ are correlated with those in the selection equation u, and our instrument d is a determinant of the latent selection variable f^* (this is a necessary condition for identification), then in general our instrument will be correlated with the disturbances in the observation equation, conditional on them being observed, i.e.

$$E\left[b'\epsilon|f^*>0\right]\neq 0$$

To see this, assume that the moment condition $E[b'\epsilon]=0$ holds in the population. However, what we observe is instead the sample analogue of $E\left[b'\epsilon|f^*>0\right]$

$$\begin{aligned}
E\left[b'\epsilon|f^*>0\right] =& E_b\left[E\left[b'\epsilon|f^*>0,b\right]\right]\\
=& E_b\left[b'E\left[\epsilon|f^*>0,b\right]\right]\\
=& E_b\left[b'E\left[\epsilon|\alpha_t\widehat{\Delta d_i}+\beta_t x_i+u_{it}>0,b\right]\right]\\
=& E_b\left[b'E\left[\epsilon|u_{it}>-\alpha_t\widehat{\Delta d_i}-\beta_t x_i,b\right]\right]
\end{aligned}$$

Suppose for the sake of illustration that u and ϵ are both mean zero, and jointly normally distributed with covariance $\sigma_{u\epsilon}$. Then

$$\begin{aligned}
E_b\left[b'E\left[\epsilon|u_{it}>-\alpha_t\widehat{\Delta d_i}-\beta_t x_i,b\right]\right] =& E_b\left[b'\frac{\sigma_{u\epsilon}}{\sigma_\epsilon}\frac{\phi\left(\frac{-\alpha_t\widehat{\Delta d_i}-\beta_t x_i}{\sigma_\epsilon}\right)}{1-\Phi\left(\frac{-\alpha_t\widehat{\Delta d_i}-\beta_t x_i}{\sigma_\epsilon}\right)}\right]\\
\neq& 0
\end{aligned}$$

So in general, even if our identifying restrictions hold, our instruments will be correlated with the residuals in the second-stage equations for the continuous variables. The intuition is that our instruments determine selection, so they will be systematically related to the unobserved variables u in the selection equation *among surviving firms*. The latter will be related systematically to the unobserved variables ϵ to the extent that firms which are more likely to survive are also more likely to invest, hire labour, and so forth. This means that our instruments can be correlated with the residuals of the second-stage equation, invalidating tests of validity based on overidentifying restrictions. We may therefore reject the null of validity more frequently than indicated by the significance level of the test.

B Additional tables

Table 11: Summary statistics for a different sample of firms without debt, 2007

Variable	Mean	Median	Min	Max	Obs
Capital (£'000s)	502	8	0	235,0106	55,579
	[15,277]				
Capital per worker (£'000s)	264	4	0	441,080	2,774
	[8,480]				
Turnover per head (£'000s)	435	110	-520	204,620	2,489
	[4,474]				
Average pay (£'000s)	38	29	0	908	3,023
	[46]				
Employment	109	13	1	25057	3422
	[669]				
Total sample size					86,378

Notes: See notes under Table 2. Standard deviations are shown in square brackets.

Table 12: Descriptive statistics for level change of dependent variables from 2007

	Bank 1		Bank 2		Bank 3		Bank 4	
	2008	2009	2008	2009	2008	2009	2008	2009
Total debt	2257	3489	1339	3639	2425	684	537	460
	[80,962]	[142,757]	[120,290]	[179,122]	[50,835]	[52,103]	[155,392]	[101,028]
Capital per head	-6	-36	0	-49	2	-23	-57	-48
	[491]	[1,048]	[1,135]	[1,423]	[1,330]	[862]	[2,583]	[814]
Turnover per head	21.76	-43.06	-3.47	-16.62	-3.69	-18.51	5.57	-19.33
	[885.37]	[2,075.06]	[750.28]	[728.69]	[898.18]	[773.70]	[507.49]	[777.14]
Average pay	1.26	0.05	-0.21	-0.23	0.62	1.47	0.76	-1.01
	[40.78]	[21.85]	[21.25]	[23.81]	[22.63]	[58.34]	[32.37]	[21.42]

Notes: Table shows the mean values for the change in debt, capital per head, turnover per head and remuneration per head from 2007-2008 and 2007-2009 by the biggest four banks in the UK. Standard deviations are shown in square brackets.

Table 13: First stage, placebo regressions

	Unbalanced		Balanced	
	2006	2007	2006	2007
	(1)	(2)	(3)	(4)
Bank 1	-0.021	-0.049**	-0.020	-0.046**
	[0.02]	[0.02]	[0.02]	[0.02]
Bank 2	0.019	-0.024	0.010	-0.030
	[0.02]	[0.03]	[0.02]	[0.03]
Bank 3	-0.001	-0.023	0.006	-0.021
	[0.02]	[0.02]	[0.02]	[0.02]
Bank 4	-0.013	-0.030	0.000	-0.026
	[0.01]	[0.02]	[0.02]	[0.02]
Other financial institution	0.001	-0.098***	-0.004	-0.103***
	[0.03]	[0.04]	[0.03]	[0.04]
Non financial institution	-0.015	-0.025	-0.008	-0.025
	[0.02]	[0.02]	[0.02]	[0.02]
Controls	YES	YES	YES	YES
Observations	13,862	11,266	10,532	10,532
F statistic	1.152	2.392	0.914	2.451
P-value of regression	0.269	8.74e-05	0.590	5.39e-05
P-value of all banks	0.655	0.127	0.849	0.139

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from regressions on the change in debt, $\Delta debt_{it}$, on a vector of bank dummies in 2005. $\Delta debt_{it}$ is calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2005, where t is $(2006, 2007)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2005. Balanced columns control for number of firms that have data recorded in both years. Data are restricted to firms who have a positive level of debt in 2005.

Table 14: Second stage, placebo regressions

Δy_{t-2005}:	2006		2007	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Capital per head	0.137***	0.503	0.148***	-0.161
	[0.01]	[0.44]	[0.01]	[0.31]
Turnover per head	-0.011	0.239	0.004	0.525*
	[0.01]	[0.34]	[0.01]	[0.28]
Average pay	-0.006	0.290	-0.008*	0.100
	[0.01]	[0.31]	[0.00]	[0.20]
Controls	YES	YES	YES	YES
Observations	10,421	10,421	10,421	10,421
P-value of instruments		0.791		0.163
Kleinbergen-Paap		0.954		2.492

Notes: *Significant at 10% level **significant at 5% level *** significant at 1% level. P-values in square brackets. Table shows results from the second stage regressions of the change in Δy_{it}, on the change in debt, $\Delta debt_{it}$, which is instrumented using a vector of bank dummies in 2005. Changes in debt and y are calculated using symmetric growth rates . Each column represents a cross-sectional regression for the change in time t relative to 2005, where t is $(2006, 2007)$. Regressions control for industry sector, firm entity type, age and log level of turnover in 2005. Balanced columns control for number of firms that have data recorded in both years. Firms that have become inactive or are in liquidation are included in the sample as having zero turnover. Failed firms are included as having zero turnover and zero assets. Data are restricted to firms who have a positive level of debt in 2005.