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Highlights

- We document heterogeneous firm employment responses to monetary policy.
- Employment at younger, more-levered firms is most sensitive to policy.
- This heterogeneity can be explained by financial constraints.
- The homes of firm directors are a key source of corporate collateral.
- Younger, levered firms exposed to collateral shocks drive the employment response.

Employment and the Residential Collateral Channel of Monetary Policy*

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Abstract

Using micro-data covering private and public UK firms, we document heterogeneous responses to monetary policy; finding that employment at younger, more-levered firms is most sensitive. This heterogeneity is consistent with firm-level financial constraints. To show this, we exploit the fact that the homes of company directors are a key source of corporate collateral, but many directors live in a different region to their firm, allowing specifications controlling for demand. Younger, more-levered firms exposed to collateral fluctuations drive the employment response, showing a residential collateral channel in the transmission of monetary policy to firms. *JEL Codes:* D22, E52, R30

Key words: Firm heterogeneity, Financial Constraints, SME Financing, Monetary Policy Transmission.

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1 Introduction

There is substantial evidence that different types of firms react heterogeneously in response to a change in monetary policy. Dimensions such as size, age, leverage, and liquidity all seem to determine how a firm reacts to a change in interest rates.[1] As these dimensions are typically proxies for financial constraints, the inference that has been drawn in the literature is that the firm-level heterogeneity arises from financial frictions. And, as a result, monetary policy transmits to activity through altering financial constraints at the firm-level. However, this inference is indirect and the evidence is inconclusive. The recent literature has highlighted disagreement over the relative responsiveness of different types of firms and whether the heterogeneity is explained by financial factors at all.[2]

Compounding the inconclusive empirical evidence are two competing mechanisms. First, *ceteribus paribus*, a financially constrained firm can have a dampened response to demand shocks as it cannot easily adjust its borrowing (Farre-Mensa and Ljungqvist 2016; Ottonello and Winberry 2020). Second, the tightness of any financial constraint typically depends on the value of the firm's net worth or collateral (Kiyotaki and Moore, 1997; Bernanke, Gertler and Gilchrist, 1999). These values change in response to aggregate shocks which alters the available financing for a constrained firm and, in turn, can amplify the firm's reaction to the shock.

To fully evaluate the role played by financial frictions *two* sources of empirical variation are needed: (i) a proxy for firms' financial constraints, which has been the main focus of the literature, and (ii) an estimate of how much these constraints change in response to monetary policy. The key novelty of this paper is to propose an empirical strategy that proxies the second object at the firm-level.

Using a unique firm-level dataset for the UK, covering both private and public firms, combined with a high-frequency measure of monetary policy surprises from Gerko and Rey (2017), we proceed in two steps. First, we show that employment at younger- and more-levered firms is most sensitive to monetary policy shocks, with these firms accounting for half of the peak aggregate employment response. Second, we turn to the question of interest: is this pattern of heterogeneity explained by financial constraints?

To answer this question, our empirical strategy focuses on the homes of company directors. This strategy relies on four key features of the data. First, directors' homes are a key source of collateral for the corporate sector, and changes in their value have a material effect on firm investment and employment (Bahaj, Foulis and Pinter 2020). Second, monetary policy has a significant effect on house prices (Jarocinski and Smets 2008; Jorda, Schularick and Taylor 2015). Third, we document significant spatial heterogeneity in the response of property prices to monetary policy. Fourth, many company directors live in a different region to their firm, allowing us to employ a variety of specifications to isolate the effect of monetary policy on firms' residential collateral values, from the effect on local demand.

Our main result is that the heterogeneity in firm responses depends *both* on firm-level financial constraints and the shifts in these constraints induced by monetary policy. First, within the younger and more-levered group, employment responds significantly more for firms whose directors live in local areas where house prices are most sensitive to monetary policy. Second, house price sensitivity is insufficient alone to generate heterogeneity: for older and less-levered firms, the sensitivity of director real estate values is irrelevant. These patterns are replicated for corporate debt, working capital, and fixed assets, consistent with a residential collateral channel of monetary policy. A simple counterfactual exercise, omitting general equilibrium effects, shows that the two-year aggregate employment response would be dampened by 13% if all directors lived in areas with the lowest sensitivity of house prices to monetary policy, suggesting the aggregate importance of this channel.

These results are consistent with monetary policy propagating through financial frictions (Kiyotaki and Moore, 1997; Bernanke, Gertler and Gilchrist, 1999). Two key components of the theory are: (i) firms face financing constraints that (ii) are eased by positive asset price movements. We seek to identify both components in the data: (i) to the extent that younger and more-levered firms face more severe financing constraints than older and less levered firms, our firm-level variation proxies the first component of the theory; (ii) the spatial variation in the price sensitivity of residential collateral to monetary policy shocks proxies the second component. Consistent with theory, collateral sensitivity only affects the response of firms

[1] See Gertler and Gilchrist (1994), Cloyne et al. (2018), Ottonello and Winberry (2020) and Jeenas (2018).

[2] Compare, for example, the findings of Ottonello and Winberry (2020) and Jeenas (2018), and see Crouzet and Mehrotra (2020) on the overall relevance of financial factors.

that appear to be constrained.

Disentangling firms' response to fluctuations in their collateral values, from their response to fluctuations in local demand, is crucial for interpreting our evidence on the monetary transmission mechanism. Our strategy using directors' real estate, and the fact that many directors live in different regions from their business activities, allows us to include region x time fixed effects, thereby controlling for the linear effect of local demand on firms' behaviour. However, firms may have heterogeneous sensitivity to (monetary-policy-induced) changes in local demand. We address this in a number of ways. First, we alter our research design by considering only those firms whose directors live more than 30 miles away from the firm's headquarters, thereby attenuating geographical spillovers of local demand. Second, we focus on firms in the tradeable goods sector, which should be less sensitive to demand in the local region (Mian and Sufi, 2014). In both cases, our results are very similar to the baseline. Third, we detect homogeneous effects of monetary policy on corporate sales in the short-run. Fourth, we show that real estate sensitivity only matters when directors' homes exceed 15% of the firm's assets in value, therefore representing a meaningful source of collateral. It is not clear why local demand would only affect this group of firms.

We also rule out alternative mechanisms explaining our results, such as directors' characteristics determining where they choose to live and how they run their firm; or behavioural responses to higher house prices. Taken together, our findings are consistent with financial frictions playing a key role in the firm-level response to monetary policy. Companies with worse credit access either pre-pay their wage bills or fund other factors that are complementary to labour using loans secured against their directors' housing. Following an expansionary monetary policy shock, collateral values rise, allowing constrained firms to expand their borrowing and pay for factors of production that ultimately increase employment.

Related Literature Our findings are related to the empirical literature on the sensitivity of firms to business cycle fluctuations (Moscarini and Postel-Vinay, 2012; Fort et al., 2013; Crouzet and Mehrotra, 2020; Decker et al., 2020). Compared to these papers, we study the sensitivity of firms conditional on a monetary policy shock and propose a research design which uncovers the role of balance sheet constraints. Moreover, we draw on a recent body of work emphasising the importance of real estate collateral for firm activity (Chaney, Sraer and Thesmar, 2012; Adelino, Schoar and Severino, 2015; Bahaj, Foulis and Pinter, 2020). The most related paper from this literature is Bahaj, Foulis and Pinter (2020), which focuses on how changes in directors' home values, from any source, affect firm investment. Building on the importance of directors' housing as a source of firm collateral, our paper exploits novel regional variation in house price sensitivities, and heterogeneity in the reliance on asset-based borrowing, to evaluate the role of asset prices and financial frictions in the transmission of monetary policy to firm employment. Another strand of the literature has focused on the prevalence of cash-flow based borrowing constraints. For U.S. publicly listed firms, borrowing is mainly secured on cash flows, however smaller listed firms still rely disproportionally on collateral-based borrowing (Lian and Ma 2021). Collateral-based borrowing is also highly prevalent among SMEs, which dominate our sample. As SMEs account for 60% of employment across the OECD, the borrowing constraints they face are material for aggregate dynamics.

An influential strand of research has shown that a change in house prices affects local demand (Mian and Sufi, 2014), that local demand shocks are transmitted through firm balance sheets (Giroud and Mueller, 2017), and alter pricing behaviour (Stroebel and Vavra, 2019). We complement these studies by showing that, over and above any local demand channel, monetary policy-induced changes in house prices have an additional effect on firms' employment and investment decisions through their impact on residential collateral values.

Our paper contributes to the empirical literature on the role played by financing constraints in the transmission of monetary policy to firms (Gertler and Gilchrist, 1994; Ottonello and Winberry, 2020; Jeenas, 2018; Cloyne et al., 2018). Our work differs from these papers in three important dimensions. First, in contrast to most existing firm-level studies on monetary transmission, our data covers private as well as public companies, and is dominated by SMEs, which are the firms most likely to be affected by financial constraints. Second, we focus primarily on the effects of monetary policy on *employment,* rather *investment,* as SMEs explain the majority of job creation and job destruction in the economy. Third, we use proxies not just for financial constraints, but also for *shifts* in these constraints due to monetary policy. In this regard, our paper is complementary to Vats (2021), who uses regional variation in creditor rights among US states to show that a relaxation of financial constraints makes a firm more sensitive to monetary policy.

Our paper is also related to the theoretical literature on the interactions between the macroeconomy, asset prices and financial markets (Kiyotaki and Moore, 1997; Bernanke, Gertler and Gilchrist, 1999; Liu, Wang and Zha, 2013). Our firm-level evidence complements these findings and highlights the importance of financial frictions for the aggregate response of firms to monetary policy. Finally, our paper relates to the empirical corporate finance literature on the use of personal assets, including housing, as collateral for small firms (e.g. Avery, Bostic and Samolyk, 1998 and Berkowitz and White, 2004). Our contribution is to use the heterogeneous regional impact of monetary policy on the value of this housing collateral to identify the residential collateral channel of monetary policy.

2 Data and Methodology

In this section we describe the dataset, report descriptive statistics, and explain our strategy for identifying the impact of monetary policy shocks on firms.

2.1 Firm-level Data

All UK companies must file annual accounts with Companies House, a government agency. We access this data via Bureau van Dijk (BvD), a commercial data provider. This dataset covers around 1.5 million unique company accounts every year, although reporting requirements vary by firm characteristics such as size. The dataset has a number of features that make it particularly well-suited for our analysis. First, it contains key variables of interest; *Number of Employees*, *Firm Age*, and *Firm Leverage*.[3] Second, while it covers large listed firms, the dataset is dominated by small and medium-sized private firms; precisely the firms most likely to be affected by financial frictions (Dinlersoz et al., 2018). Third, firms from all sectors of the economy are covered, in particular, both manufacturing (Crouzet and Mehrotra, 2020) and non-manufacturing firms. Finally, it also contains detailed information on who runs the firm – *the directors* – including their name, date of birth, appointment and resignation dates, whether they're a shareholder, and, crucially, their usual residential address. This last piece of information allows us to measure the sensitivity of the director's home value to monetary policy shocks.

BvD is a live database, meaning that historical information, including on who directors are and where they live, is lost over time. To circumvent this limitation we merge together 25 historical vintages of the BvD database, following the methodology of Bahaj, Foulis and Pinter (2020). In the UK, firms typically file their accounts annually. However, their filing dates, and hence the sequence of shocks experienced during their accounting window, are distributed throughout the year (Online Appendix A.3).

2.2 Sample and Firm Descriptive Statistics

Our sample comprises private limited and public quoted firms for whom the UK Companies Acts apply. We exclude firms that operate in the financial, public or non-profit sectors and we also exclude firms that have a parent with an ownership stake greater than 50% to correctly account for the firm's financial position and avoid double counting (see Online Appendix A.1). We further restrict to firms that report the lag of firm age, number of employees, and leverage; common proxies for firm-level financial frictions used in the recent literature (e.g. Dinlersoz et al., 2018). Our sample period covers firms that file accounts from May 1997 (when the Bank of England was granted operational independence and the Monetary Policy Committee was established) and extends until the end of 2016. Throughout in our employment regressions we consider firms who report employment growth over a five year horizon, from employment in the lagged accounts to four accounts hence.[4] Thus, our baseline estimates are conditional on firm survival (Online Appendix N considers firm exit).

Table 1 presents summary statistics for the largest sample used in our firm-level regressions, covering 192,223 observations on 37,944 unique firms. The upper panel shows the sample is dominated by small

[3]Firm age is years since incorporation; firm leverage is the ratio of total liabilities to total assets.

[4]The focus on employment eliminates a large number of small entities that are either not required to report employment or have no employees. However, as shown in Online Appendix A.1, our sample covers a substantial share of aggregate employment and tracks its business cycle dynamics well. Moreover, we show that non-reporting firms tend to be younger and more-levered, suggesting the heterogeneity in monetary policy responses would be even stronger if these firms were included in our analysis.

firms, with the median firm having 48 employees, and an interquartile range running from 8 to 115 employees. However, the sample also contains a tail of large firms, as highlighted by the mean firm having 496 employees and £84 million in total assets. There is an even dispersion of firm age and leverage, with interquartile ranges running from 7-29 years, and 42-81%, respectively.

Several proxies for financial constraints have been proposed by the literature including size (Gertler and Gilchrist, 1994), age (Hadlock and Pierce, 2010)[5] and leverage (Ottonello and Winberry, 2020; Jeenas, 2018), with the latter directly mapping into firm net-worth – a key state variable governing access to external finance in models of financial frictions (Bernanke, Gertler and Gilchrist, 1999; Kiyotaki and Moore, 1997). As shown by Figure 1, however, these proxies are correlated, with firms tending to reduce their leverage and increase their size as they age (conditional on survival). This highlights the importance of conditioning on firm age when assessing how firm size and leverage affect a firm's behaviour.

2.3 Identification of Monetary Policy Shocks

We use the series of Gerko and Rey (2017) to measure exogenous fluctuations in UK monetary policy. This series uses high frequency market reactions, around Bank of England policy releases over the period 2000-2015, as an external instrument in a structural vector autoregression (proxy-SVAR) model covering UK aggregate data. Our VAR specification is identical to Gerko and Rey (2017), with the single exception that we include aggregate employment (in logs) as an extra variable, to obtain a comparable aggregate benchmark for the employment response to a monetary policy shock. We use the identified monetary policy shock series from the VAR in our firm-level regressions. An advantage of this approach is that it extends the identified monetary policy series back to the period before the high frequency market data was available, hence extending our firm-level sample.[6]

The VAR contains the 5-year bond yield as a measure of the monetary policy stance. Our sample overlaps with the zero lower bound period, limiting the ability of the central bank to influence short rates. The longer term rate captures news about the expected future path of policy. This is also the reason Gerko and Rey (2017) include the minutes of the meeting and the publication of the Inflation Report, when information about future rates are disclosed, as policy announcements. Hence, our policy shock also embeds a forward guidance component. We show the robustness of our results to excluding the zero lower bound period and controlling for central bank non-monetary information revelation in Section 5.

Online Appendix C provides further details on the construction of our shock series, and presents impulse responses to the monetary policy shock that emerges from the VAR. The pattern of responses is in line with the monetary policy literature.

2.4 Estimation of Firm-Level Responses

Let $EMP_{i,t}$ be firm i's number of employees for accounting period t. As described, our firm-level data is effectively annual so t refers to the firm's accounting year, and we use the index $m \in \{1, \ldots, 12\}$ to denote months within the accounting year, with $m = 12$ the month the firm files its accounts (not necessarily December). We use the index s to denote month-year pairs in the time domain, which are common to all firms, regardless of their filing date.

Our baseline linear specification is specified as a local projection (Jorda, 2005) and is an extension of the model discussed in Ramey (2016) into a panel instrumental variable setting:

$$ln(EMP_{t+h,i}) - ln(EMP_{t-1,i}) = \sum_{g=1}^{G} \alpha_g^h \times Dg_{i,t-1} + \sum_{g=1}^{G} \beta_g^h \times Dg_{i,t-1} \times \Delta r_t + v_{i,t}^h \tag{1}$$

where $h \in \{0, \ldots, 4\}$ indexes a set of regressions at different horizons, running from 0 to 4 years. The term Δr_t is the change in the average 5-year interest rate over the firm's accounting year.[7] We instrument

[5] Financially constraints are more likely to bind on younger firms as they: (i) have less history, so less information is available to external creditors, (ii) often rely on a few key personnel, potentially exacerbating moral hazard problems, and (iii) tend to grow faster and are more likely to be reliant on external finance.

[6] This is a common approach in the proxy SVAR literature: see Gertler and Karadi (2015).

[7] Precisely, $\Delta r_t = 1/12(\sum_m (r_{m,t} - r_{m,t-1}))$, where $r_{m,t}$ is the average of daily observations of the 5 year gilt yield in month m of firm accounting period t.

the interest rate changes with the series $\sum_{m=1}^{12} e_{m,t}$, where the term $e_{m,t}$ denotes the monetary policy shock for month m of accounting year t, as extracted from the VAR described in Section 2.3. To allow for heterogeneous responses, the term $Dg_{i,t-1}$ is a dummy variable that takes a value of 1 when firm i is part of a particular group of firms in period $t-1$, and 0 otherwise. The impulse response to an interest rate change for a particular group is then given by the vector of coefficient estimates $\{\beta_g^h\}_{h=0}^4$. We re-scale all impulse responses so they can be interpreted as the response to a shock that raises the interest rate by 25bp on average over the firm's accounting year (i.e. $\Delta r_t = 25bp$).

By including time fixed effects, we can also compute relative impulse responses:

$$ln(EMP_{t+h,i}) - ln(EMP_{t-1,i}) = \delta_{j,s}^h + \gamma_{l,s}^h + \sum_{g=1}^{G} \tilde{\alpha}_g^h \times Dg_{i,t-1} + \sum_{g=1}^{G-1} \tilde{\beta}_g^h \times Dg_{i,t-1} \times \Delta r_t + v_{i,t}^h \quad (2)$$

In this context, $\delta_{j,s}^h$ is a dummy that takes a value of 1 for firms operating in (SIC-1) industry j that file their accounts in month, year s, and 0 otherwise.[8] This means we are comparing firms within industry in the same month and year and are thus eliminating the role of industry-specific sensitivities to monetary policy. Similarly, $\gamma_{l,s}^h$ is a dummy that takes a value of 1 for firms that operate in (NUTS-1) region l and file their accounts in month, year s, and 0 otherwise. Hence, we are comparing two firms in the same region, at the same time, subject to the same local economic conditions. In our baseline specification, the firm's region l corresponds to the location of its headquarters (following Chaney, Sraer and Thesmar 2012). Note also that NUTS-1 regions are relatively large: there are 10 in England and Wales and they correspond to large regional economic areas equivalent to a larger CBSA in the U.S. (London is a single NUTS-1 region, for example). We use these coarser regional fixed effects in the baseline to accommodate region x month fixed effects. We consider specifications that have finer geographic fixed effects, and address the potential for firms to operate in multiple locations, in Section 5.

We refer to the impulse responses arising from the two specifications in Equations 1 and 2 as the level and relative effect respectively. In terms of the former, the fact $e_{m,t}$ is an identified structural shock with ample time series variation, means that omitting time fixed effects does not bias the estimates of how monetary policy affects a firm in a particular group g. There is no missing intercept problem that typically arises from a fixed effect absorbing general equilibrium feedback loops. Hence, we can conduct aggregation exercises and consider which types of firms explain most of the aggregate employment response to monetary policy. However, through inclusion of region x time and industry x time fixed effects, the specification in Equation 2 is better placed to disentangle the mechanisms that explain the heterogeneity. Furthermore, this specification is better suited for conducting inference over differences between groups because estimation uncertainty over the average effect of the shock is absorbed. In particular, the term $\tilde{\beta}_g^h$ captures the response of group g relative to the Gth group.

Last, further details are in order on the setup of our baseline specification. When constructing cumulative growth rates, $log(EMP_{i,t+h}) - log(EMP_{i,t-1})$, we (i) omit observations in the 1st and 99th percentiles of observations in order to prevent outliers distorting the results, (ii) omit observations where any accounting period in the window between $t-1$ and $t+h$ is not one year, (iii) rectangularise the sample such that for any observation to be included $ln(EMP_{t+4,i}) - ln(EMP_{t-1,i})$ must be reported, and (iv) when using alternative left hand side variables from employment, recast all nominal variables in real terms by dividing through by the seasonally adjusted UK consumer price index. We compute standard errors using the methodology of Driscoll and Kraay (1998). This accounts for the serial correlation at the firm-level that is standard in local projections as well as arbitrary cross-sectional dependence between firms both contemporaneously and through time. Note that, while our monetary policy shock is generated, it is used as a *generated instrument*, so does not suffer from generated regressor inference problems (see Wooldridge, 2002, page 117).

3 Average and Heterogeneous Monetary Policy Effects

This section first presents the average firm-level employment response to monetary policy shocks, then explores heterogeneity by firm age, leverage, and size.

[8] There are few observations in SIC-1 industry group 0 so we combine them with group 1.

3.1 Average Firm-Level Effect of Monetary Policy

Figure 2 shows the average firm-level employment response to a contractionary monetary policy shock that raises the interest rate by 25bp on average over the firm's accounting year, together with 90% confidence intervals. The shock results in a 0.3% decline in firm-level employment during the first year, although this effect is not statistically different from 0. The fall in employment continues with the mean response reaching a trough of -1% after 2 years, followed by recovery. Online Appendix E shows that we obtain similar results when considering a number of alternative specifications of the baseline regression, including not requiring firms to report employment over the four-year horizon; including financial variables as controls; or including firm fixed effects. Considering the aggregate employment response, we also find similar effects when using administrative data to weight our regressions by firm size. The peak employment response is greater than we find using the VAR, however this appears to reflect the VAR's longer sample period: we find similar magnitudes if we estimate a local projection on aggregate employment over the same period as the firm-level data.

3.2 Heterogeneous Firm-Level Effects of Monetary Policy

Having established that the effect of a monetary policy shock on the average firm is reasonable and can be reconciled with the aggregate employment response, we turn to heterogeneity in firm-level responses. Figure 3 shows the two-year employment response cut by alternative firm age, leverage, and size groups (for the full dynamic responses see Online Appendix F). The top row shows that younger firms respond the most to monetary policy, with the employment contraction being over 2% for firms under five years old, but under 0.5% for firms over thirty years old. In between these two extremes, the effect of monetary policy on employment decreases near monotonically in firm age. The next row shows that the upper three quintiles of firms by leverage (approximately those with a leverage ratio above 50%) respond in a relatively homogeneous fashion but the difference relative to the lower two quintiles is sharp, with the latter characterised by far smaller and, for the lowest quintile, insignificant effects. The last row shows that a size split, in contrast, leads to non-monotonic, less clear-cut estimates. For instance, the largest response is recorded for firms with between 500 and 2000 employees, whereas the smallest adjustments are for the largest and smallest firms. Thus, the definition of a large firm, whether above 250 employees, as in the UK, or the top 1% of assets, as in Crouzet and Mehrotra (2020), will determine whether large or small firms respond more to monetary policy. Indeed the academic literature is mixed on how firm size affects business cycle responsiveness.[9]

In the remainder of the analysis, we focus on heterogeneity by firm age and leverage, as in contrast to firm size, they govern the responsiveness to monetary policy in a way that is not dependent on the specific threshold used. Moreover, in Online Appendix O we show that our main results, combining firm heterogeneity and collateral sensitivity, are robust to controlling for firm size. To explore the marginal contribution of age and leverage, we split firms along two dimensions: (i) more/less levered, cutting at the median leverage ratio in a given year; and (ii) younger/older firms, cutting at 15 years old, around the median in our sample. The result of this double cut by age and leverage is presented in Figure 4. Figure 4a shows results omitting time fixed effects (specification 1), whilst Figure 4b includes industry x month and region x month fixed effects, and shows results relative to older, less levered firms (specification 2). There are three key takeaways, with all differences statistically significant: (1) the first column of Figures 4a and 4b shows that higher leverage firms contract employment by more when they are also younger; (2) conversely, the first row of Figures 4a and 4b shows that the employment response of younger firms is greater when they have higher leverage; (3) the most sensitive group, with a peak employment contraction of almost 2%, is younger, higher levered firms.

Aggregate Contribution of Younger, More Levered Firms Using these results, and the employment shares of each group in our regression sample, we can back-out the share of the total employment response to monetary policy that is contributed by younger, more levered firms. This calculation finds that younger, more levered firms contribute half of the peak employment response, despite accounting for only a quarter of employment; by contrast, older, less levered firms account for 10% of the peak response, despite a larger employment share. To calculate contributions to the aggregate employment response, we use administrative

[9]See, e.g. Moscarini and Postel-Vinay (2012); Kudlyak and Sanchez (2017); Crouzet and Mehrotra (2020).

employment data to: (1) reweight the regression; and (2) calculate the aggregate employment share of each group, with the findings for both exercises similar to our baseline sample (see Online Appendix A.2). The implication of these two results is that younger, higher leverage firms also contribute around half of the *aggregate* employment response of incumbent firms to monetary policy shocks.

4 Explaining Heterogeneous Responses

The prior section showed that the largest employment response to monetary policy shocks is among younger, more levered firms. Given that the prior literature (discussed in Section 2.2) has proposed firm age and leverage as proxies for financial constraints, a natural question to ask is whether this pattern of firm-level heterogeneity is consistent with financial constraints? We tackle this question here, first providing clear theoretical predictions from a simple model of financially constrained firms. We then explain our identification strategy, which uses personal real estate collateral, and allows us to cleanly test these predictions empirically.

4.1 Theoretical Considerations

We draw on the existing literature and informally lay out the theoretical framework for our empirical analysis. Online Appendix D provides a formal setting for our arguments by considering a firm that chooses how much labour to hire subject to the need to obtain external finance to prepay wages. How financial constraints alter the response of employment to monetary policy is ambiguous. There are two competing mechanisms. First, *ceteribus paribus*, a firm facing financial constraints should be less sensitive to shocks to demand (see Farre-Mensa and Ljungqvist, 2016 and Ottonello and Winberry, 2020). For example, a firm that has exhausted its borrowing capacity cannot react to a positive demand shock by hiring more workers as it cannot finance the increase in its wage bill. Second, by raising asset prices and hence the value of collateral available to the firm, expansionary monetary policy shocks could ease the degree to which firms are financially constrained. This is the heart of the financial accelerator mechanism (Bernanke, Gertler and Gilchrist, 1999). If the asset price response is sufficiently large, the additional financing could result in constrained firms experiencing a larger employment response than unconstrained firms. For unconstrained firms, this asset price response is irrelevant.

This allows us to sharpen our empirical predictions. If financial constraints are relevant for explaining the heterogeneous firm-level response to monetary policy, then: (i) across unconstrained firms, heterogeneity in the sensitivity of collateral values to monetary policy shocks will not generate heterogeneous employment responses; (ii) across constrained firms by contrast, heterogeneous collateral value sensitivity will translate into heterogeneous employment responses.[10]

4.2 Exposure to Collateral Value Fluctuations

Testing these theoretical predictions requires a firm-level measure of the sensitivity of collateral values to monetary policy shocks. To do this, we focus on the residential collateral of company directors. This is for four key reasons. First, as we will outline in the following paragraphs, the houses of company directors are a major source of collateral for firms. Second, monetary policy has a significant effect on house prices (Jarocinski and Smets, 2008; Jorda, Schularick and Taylor, 2015). Third, we document that monetary policy has a heterogeneous impact on house prices across local areas, providing a key source of variation in the sensitivity of collateral values. Fourth, many directors live in a different region or far away from their firm, allowing us to use a number of identification strategies to isolate the impact of monetary policy on a firm's collateral value, from the impact on its business opportunities. In particular, we can control for the impact monetary policy has on local demand, either via house prices, or through other channels.

The houses of company directors offer a substantial pool of collateral for firms to draw on. Aggregating across all the company directors in the UK, we estimate that the combined value of their housing was worth around £1.5 Trillion in 2014 (around 80% of UK GDP), around four times more than the buildings owned

[10]Moreover, an expansionary monetary policy shock will reduce a constrained firm's cost of finance above and beyond the fall in the risk free interest rate, consistent with Anderson and Cesa-Bianchi (2018).

by firms (i.e. corporate real estate). This reflects the fact that there are almost 3 million company directors in the UK, and the average value of their houses is substantially above the national average (Bahaj, Foulis and Pinter, 2020).[11] Note, this figure is for all firms in the UK, not just the firms that report sufficient information to appear in our sample. As we discuss in Online Appendix A.5, changes in director home values have a material effect on firm employment: an increase of £147k in the combined home values of a firm's directors results in a net increase of one employee after three years.

Firm funding can be secured directly on this residential collateral, or it can be used, explicitly or implicitly, to support a *personal guarantee,* that is, a legal commitment by directors to repay the firm's debt in the case of default.[12] As we show in Online Appendix B.3, using recent Bank of England survey evidence, personal guarantees are very common in the UK, with 51% of bank lending to SMEs and mid-sized corporates by volume (43% by value) secured by these means, thereby breaking limited liability for these companies. The firms in this survey represent around three-quarters of aggregate UK company employment, showing the importance of personal collateral for the aggregate economy. This is not just a UK-specific phenomenon: in Online Appendix B.3, we review the international literature to show that housing is a key source of collateral for business loans in other countries too, including in the United States, where approximately half of small business loans have personal assets as collateral (Bathala, Bowlin and Dukes 2006; Meisenzahl 2014).

As we further show in Online Appendix B.2, the use of personal real estate is particularly pronounced among younger, higher leverage firms; precisely the ones that are driving the employment response to monetary policy. Almost two-thirds of bank borrowing by younger, higher leverage firms is secured by personal guarantees, compared to only a third for older, lower leverage firms. These patterns may reflect the relative availability of different types of real estate collateral across firms (see Online Appendix A.7). To corroborate these figures, in Online Appendix B.2 we use results from a second survey on SME finances (the 2004, 2008 waves of the UK Survey of SME Finances). This survey confirms the importance of personal assets as a source of collateral for SMEs, and for younger, higher leverage firms in particular, also finding around two thirds of this group rely on personal collateral. Moreover, with this survey we are able to decompose the form of personal security, with around 40% of lending to younger, higher leverage firms secured directly on residential property, and around 30% secured by personal guarantees.

4.3 House Price Sensitivity to Monetary Policy

To measure the sensitivity of residential collateral to monetary policy we run simple projections of house price indices on monetary policy shocks. To enable precise measurement of this collateral sensitivity, and to fully exploit the geographical variation in the data, we use granular house prices series from the Land Registry's monthly repeat sales index, which covers 172 *local areas* in England and Wales over 1995-2016 (Scotland is omitted as granular house price indicies do not exist prior to 2004). These smaller areas correspond to local authority districts or the non-urban parts of counties. As discussed in Section 2.4, to accommodate firm region x month fixed effects, firm regions are defined using the broader NUTS-1 defintion, which encompasses these local areas. These fixed effects will be sufficient to absorb the regional impact of monetary policy on firms, so long as this impact, e.g. due to local demand, is relatively broad. In Section 5 we show our results are robust to including more granular fixed effects, at the level of these local areas.

These regressions produce the average house price responsiveness to monetary policy shocks for each local area, which we refer to as the *house price beta.* We then rank local areas by their cumulative house price responsiveness after two years, the typical peak response. As robustness, in Online Appendix O, we also use local area refusal rates of planning applications as a proxy for housing supply elasticities: areas with inelastic housing supply should experience greater house price increases following expansionary monetary policy shocks. Online Appendix A.4 provides further detail on both measures and displays a heat map, which confirms substantial spatial heterogeneity in both metrics. At the firm-level, we average the estimated responses across the local areas where each of the firm's current directors live to compute a *director* house price beta. In contrast, the *firm* house price beta refers to the beta in the local area where the firm is

[11]The UK is not an outlier in terms of self-employment rates or the contribution of small and medium size businesses to aggregate activity (see OECD (2021), page 267).

[12]Whilst personal guarantees can be explicitly secured by directors' houses, even if they are not, the house will implicitly back the guarantee, as it will be valued during the underwriting process and can be seized by court order if the director defaults on a guarantee. Online Appendix B.1 provides further details on the legal background regarding personal guarantees, and the properties that can make them attractive for firms.

headquartered. From Section 4.4 onwards, we restrict the regression sample to firms where the director house price beta is non-missing. This leaves us with 133,078 firm-year observations on 27,718 unique firms. These firms account for 84% of the employment of the sample in Section 2.2, and have very similar characteristics (see Online Appendix A.6).

Our research design will be invalid if director house price betas matter for a firm's response to monetary policy shocks for a reason other than capturing the sensitivity of an important source of collateral to the firm. Table 2 presents medians for firm and director characteristics, grouped by betas (see Online Appendix A.6 for further statistics on directors). Directors in high versus low beta areas have a similar age and level of experience, and display a similarly high degree of spatial separation from their firm, living around 8 miles away, with around 60% living in a different local area to their firm (for comparison, 30% live in a different NUTS-1 region to their firm). This is in line with the average UK commuting distance for all employed workers of 8.8 miles (the UK National Travel Survey, 2014). However, across the whole sample, there is a tail of approximately a fifth of directors who live more than 30 miles away from their firm, which provides more extreme spatial heterogeneity. Directors located in high beta areas tend to own more valuable houses and run somewhat younger, smaller firms. However, as we can use granular fixed effects to implement a research design that essentially compares two firms operating in the same local area, but which have directors living in areas with differing house price sensitivities, the source of variation that matters is not the director beta *per se*, but the director beta *relative* to the firm's beta. The final three columns in Table 2 show that firm and director-level characteristics are very similar for directors living in higher vs lower beta areas than their firm. Moreover, in Section 5, we show that our results are robust to controlling for a broad set of director characteristics

4.4 Employment Responses by Residential Collateral Sensitivity

For ease of exposition, in Figure 5a, we focus on the groups on the main diagonal of Figures 4a-4b, namely younger firms with higher leverage (Figure 5a: top row) and older firms with lower leverage (Figure 5a: bottom row), as these two groups are distinguished by the two key characteristics that drive heterogeneity in employment responses. In Figure 5a, we further split these two groups depending on whether the firm's directors live in a local area with high (top tertile, left column) or low (bottom tertile, right column) house price sensitivity to monetary policy shocks. In Figure 5b, we report estimates from the relative effects specification with time fixed effects (specification 2), using the older, lower levered firms with directors living in low house price sensitive local areas as the baseline group.

Three main findings emerge from Figure 5. First, our key result: among younger, more levered firms (the top row in Figures 5a–5b), the employment contraction is greater for firms when their directors live in high beta rather than low beta local areas (left vs right columns). This is consistent with financially constrained firms contracting their employment by more when the value of their housing collateral is more sensitive to monetary policy. The difference is material: the peak employment contraction is around 1-1.5 percentage points greater for firms with directors in high beta regions, whilst equality of the impulse responses in the top row of Figure 5b is rejected at the 1% level for the 2 and 3-year horizons. Second, among the group of older less-levered firms, house price sensitivity is irrelevant: the bottom left and bottom right panels in Figure 5a exhibit very similar dynamics (the bottom left chart in Figure 5b formally shows no statistical difference). This is consistent with heterogeneity in collateral value changes having no impact on unconstrained firms. Third, even within the group of firms whose directors live in higher house price sensitivity areas (left column), the employment response of younger, more levered firms is large and very significant whereas the response of older, less-levered firms is small and insignificant. Taken together, the findings are consistent with monetary policy transmission working, in part, through altering the value of collateral available to financially constrained firms.

In Online Appendix A.5, we corroborate this evidence by showing that the relative magnitudes of the impulse responses are consistent with existing evidence on the response of firm employment to an unconditional increase in directors' house values and the response of house prices to monetary policy.

Contribution of House Price Movements to the Aggregate Response To assess the aggregate importance of real estate prices for firm employment responses, we repeat the exercise of Section 3.2, using administrative data to provide representative weights for the regression. Figure 6a shows that, if anything, the director beta plays a bigger role in this weighted regression. The peak employment response at the 2- and 3- year horizons for younger, more-levered firms is 2 percentage points higher for firms with directors in the most house price sensitive areas, resulting in an estimated coefficient that is twice as high. As shown in Figure 6b, the 2 percentage point difference in peak responses remains (in fact, is slightly larger) once region x time and industry x time fixed effects are included, suggesting the differential responses in Figure 6a are due to collateral sensitivity.

To quantify the aggregate importance of residential collateral sensitivity we consider how much lower the two-year aggregate employment response would be if all directors lived in a low house price sensitivity area. As house prices in low sensitivity areas still fall following a contractionary monetary policy shock, this simple exercise is not equivalent to switching off the residential collateral channel altogether. We implement this within each firm age x leverage group, using the coefficient estimates from Figure 6a, and only include the counterfactual difference where, in the fixed effects specification of Figure 6b, collateral sensitivity makes a statistically significant difference to the employment response at the 10% level. To construct the aggregate counterfactual, we use the initial aggregate employment shares of each group, estimated following the same methodology as Online Appendix A.2. This exercise results in the aggregate employment response being attenuated by 13% at the 2-year horizon, and 11% at the 1- and 3-year horizons, with the difference all driven by younger, higher leverage firms.[13] The advantage of working with the estimates from Figure 6a is that this simple counterfactual exercise does not suffer from a missing intercept problem. However, it is still a partial equilibrium exercise, as it ignores any feedback mechanism between different firms, other agents in the economy, and prices, and such omitted general equilibrium effects may amplify or dampen the overall response.[14]

5 Identification and Robustness

The previous section provided evidence consistent with monetary policy transmitting by easing firm-level financial constraints. This section shows the robustness of the documented heterogeneity to a number of threats to identification and alterations to the baseline specification. For compactness, the key results are gathered together in Table 3, with the full figures in the Online Appendix. Row 1 of Table 3 presents the equivalent numerical results from Figure 5b (the baseline specification with region x time and industry x time fixed effects). For each specification the table shows three groups of results. First, to show the combined importance of firm characteristics and collateral sensitivity, the first three columns show the equivalent of the top left panel in Figure 5b: the employment response for younger, higher leverage, high beta firms relative to older, lower leverage, low beta firms, at horizons, one, two, and three years. The third column shows that, after 3 years, the cumulative employment response is 3.3pp. lower for younger, higher leverage, high beta firms. The second group of results, in the next three columns, highlights the importance of firm characteristics, showing the relative employment response for younger, higher leverage, low beta firms (equivalent to the upper right column in Figure 5b). The third column in this group shows that, even for firms with lower sensitivity of collateral to monetary policy shocks, the three year cumulative relative employment response is 1.5pp. lower. Finally, the last three columns highlight the importance of collateral sensitivity, showing the difference in the employment response for younger, higher leverage firms with high vs low beta. At each horizon, these results show the difference in employment response between the first and second group of results in the table. To reiterate the key result, the employment response of younger, higher leverage firms is materially larger when the value of director housing collateral is more sensitive to monetary policy shocks. Specifically, the employment contraction is 1.8pp. greater after three years, with this difference statistically significant at the 1% level.

Mechanisms Rows 2-4 of Table 3 consider alternative specifications to explore the mechanisms behind the observed responses. If financial constraints explain the employment responses, we would expect the pattern to be replicated with firm borrowing. In confirmation, row 2 shows the response of firms' total debt,

[13] This thought experiment considers the employment response of surviving, incumbent firms. Online Appendix O shows similar results when relaxing the restriction that firms report 4-year employment growth. Online Appendix N separately considers the impact of monetary policy shocks on firm exit.

[14] For a discussion of the general equilibrium effects of the residential collateral channel more generally, not specifically in response to monetary policy shocks, see Bahaj, Foulis and Pinter (2019).

with younger, higher leverage firms having a significantly larger response when they also have the highest collateral sensitivity. One way borrowing can affect employment decisions is via a working capital channel, if firms need to pay workers in advance and are reliant on external finance to do so. Another way is through firm investment: if capital and labour are complements in production, a reduction in investment may also reduce firms' employment. Rows 3 and 4 present evidence consistent with both of these channels, with younger, higher leverage firms having a substantially greater response of prepaid expenses (to proxy working capital needs) and fixed assets (to proxy investment) when their housing collateral is most sensitivity to monetary policy.

Local Demand Rows 5-10 present alternative specifications designed to address a key identification challenge: disentangling fluctuations in collateral values from changes in local demand faced by the firm. As a first pass to argue against demand effects, we use turnover as a proxy for demand at the firm-level and use it as an alternative left-hand-size variable in our regression. While monetary policy shocks have a material effect on turnover across all firm groups, row 5 of Table 3 shows no meaningful heterogeneity in the responses of firms' turnover. This suggests our baseline results are not a result of heterogeneous demand following monetary policy shocks. However, turnover may be an imperfect proxy and there may be other local factors driving the firm's response. As directors can live in different regions from their firm, our research design allows us to control for region x time fixed effects which capture the linear effect of local economic conditions. In our baseline specification, the definition of the region is at the NUTS-1 level, which is coarse. Row 6 shows similar results, however, if we instead use local area x time fixed effects, where local areas correspond to the smaller geographical units that we use to compute house price sensitivities.[15]

We can also exploit the fact that some directors live far away from their firms and so their house values are less related to demand in its vicinity. The correlation between the firm and director beta drops to 40% when directors live at least 30 miles away on average (see Figure A7 in Online Appendix A). Row 7 shows results when we restrict our sample to firms where the average director lives above this 30 mile limit.[16] As an additional check, in row 8 we focus on firms that should be insensitive to demand conditions in the local region: those operating in the tradeable goods sector. In row 9 we go further, combining the prior two specifications, and restrict the sample to firms in the tradables sector whose directors live at least 30 miles away on average. Across rows 7-9 the results are very similar to the baseline.[17] Last, some firms have establishments in multiple regions. Directors could live far from the firm's headquarters, but near one of the firm's establishments. Moreover, multi-establishment firms may differ on other dimensions such as size, and respond in a systematically different way to policy. However, row 10 shows we obtain very similar results when restricting our sample to single establishment firms.

Director Characteristics House price sensitivity in the director's local area is a key variable in our analysis. However, the location of the director's home is an endogenous choice that may proxy for omitted heterogeneity. We do not have an instrument for director location. So in rows 11–17 of Table 3 we conduct a sequence of exercises designed to rule out alternative explanations for our results due to a director's location or characteristics. As a starting point, a director's income could influence their choice of location, and thus be correlated with the houuse price sensitivity in the director's local area is a key variable in our analysis. However, the location of the director's home is an endogenous choice that may proxy for omitted heterogeneity. We do not have an instrument for director location. So in rows 11–17 of Table 3 we conduct a sequence of exercises designed to rule out alternative explanations for our results due to a director's location or characteristics. As a starting point, a director's income could influence their choice of location, and thus be correlated with the house price beta. In row 11 we place firms into three equal size groups based on their directors' average income and include as controls dummies for the income group interacted with the

[15]We lack a sufficient number of observations to include local area x month fixed effects, so instead use local area x year fixed effects. Monthly effects are still picked up through the month x industry fixed effects.

[16]Due to a smaller number of observations for this subsample, and the shareholder subsamples below, for these specifications we include region x year and industry x year fixed effects for the relative responses.

[17]However, in row 9, due to the substantially reduced sample size, this difference is no longer statistically significant. The difference is statistically significant, and the magnitudes are very similar, if we consider instead the larger sample of firms in the tradables sector whose directors live at least 20 miles away.

firm age and leverage buckets, and with the monetary policy shock.[18] More generally, a director's choice of where to live could be endogenous either to monetary policy shocks or house price growth more generally. For example, directors whose firm becomes successful could move to places where house prices are growing rapidly. To address this, in row 12 we omit potentially endogenous director location switches. Specifically, we adopt a strategy analogous to Chaney, Sraer and Thesmar (2012), and fix who directors were and where they lived at the start of our sample, in 1997, and calculate their house price beta, holding this fixed for the firm throughout the rest of the sample. Even omitting moves, directors living in high-beta areas may still differ along a number of additional dimensions. Hence, in row 13 we saturate the regression with a large number of director characteristics, capturing their age, experience, and directorship history (see Online Appendix I). The three exercises in rows 11-13 all generate results in line with the baseline regressiose price beta.

To corroborate the interpretation that the heterogeneity in employment responses stems from the collateral channel on the director's house, and not, for example, their personal experiences or behavioural expectations (Malmendier and Tate, 2015), we exploit the fact that directors who are only *managers,* and not also *owners,* have much less incentive to pledge personal assets to support the firm. Hence if the collateral channel is driving our results, we would expect to see an effect only based on the house price sensitivity of those directors who are also shareholders in their firm. To avoid the beta of non-shareholder directors being correlated with the firm beta, for this sub-sample analysis we limit to firms where directors live 30 miles from the firm on average. Rows 14-15 of Table 3 show that the employment of younger, higher leverage firms responds in a statistically significant way only to the house price sensitivity of where director-shareholders live.

Bahaj, Foulis and Pinter (2020) show that housing is only a meaningful source of collateral for firms if the ratio of director house values to the firm's total assets is above 15%. Below this, firm behaviour is not sensitive to the value of director housing. If our results are explained by housing as collateral, we would expect a similar outcome in our analysis as well. In rows 16 and 17 of Table 3, we show results for two subsamples of firms: those below and above the 15% house value to asset ratio at the time the shock hits. For firms above the 15% threshold the differences by director beta are even more pronounced, and statistically significantly different at the 1% level at horizons 2-3 years (row 17). Whereas it is clear that the director beta is irrelevant for determining the sensitivities of firms below the 15% threshold to a monetary policy shock (row 16).[19] If another mechanism explained our results we would not expect this 15% threshold to be relevant.

Monetary Policy Shocks Rows 18-20 in Table 3 consider robustness to our measure of monetary policy shocks. Our sample intersects with a period when interest rates were stuck against the zero lower bound. During this period, whilst changes in short-term interest rates are less likely, monetary announcements can still result in unexpected movements in market rates, due to, for example, policymakers clarifying their reaction function, and issuing guidance on how long they intend to keep policy rates low. As robustness, row 18 shows results when we exclude this period and end our sample in 2008, with similar conclusions. More generally, before or during the zero lower bound period, one may be concerned that high frequency market reactions to monetary policy announcements reflect the revelation of private information of the central bank rather than random policy fluctuations – this would lead to upward bias if central bank tightening reflects private good news about future economic prospects. To control for the central bank's information set, we use the change in the 4-quarter ahead official forecasts for both inflation and output, from Cloyne and Hurtgen (2016), summed over the firm's accounting year. In row 19, we include the interaction of the forecasts with the age x leverage x director beta buckets as additional controls – the results are very similar. A final concern regards the weighting of the monthly monetary policy shocks over the firm's accounting period. Our baseline specification simply sums up the monthly shocks, implying equal weights. In row 20 we instead let the data speak and show an alternative, over-identified specification where each of the monthly shocks is used as a

[18] We define directors' average income as *director remuneration* divided by number of directors to get the directors' average (non-dividend) compensation at each firm. The tertiles are constructed for each year.

[19] Firms below the 15% threshold are larger, so their employment may respond differently to monetary policy shocks. To address this, we run the same regression controlling for four buckets of firm size (annual quartiles of lagged employment), included linearly, and interacted with the monetary policy shock. Figures A54–A55 of Online Appendix I show very similar results when including these additional controls.

separate instrument. This produces very similar results to the baseline.

Further Robustness The Online Appendix subjects our main result from Figure 5 to an extensive range of robustness tests. To highlight a few key results, we consider alternative specifications controlling for: the cash flow effects of monetary policy on directors' mortgages (Online Appendix K); the bank lending channel (Online Appendix L); hiring frictions and wage rigidities (Online Appendix M); as well as alternative proxies for financial constraints and collateral sensitivity (Online Appendix O). None of these alternative specifications overturn our key findings.

6 Conclusion

This paper finds that the employment adjustment to monetary policy is large and significant for younger and more levered firms but is small and statistically negligible for older and less levered firms. This heterogeneity becomes even more pronounced for firms whose collateral value, specifically the house value of their directors, is more sensitive to monetary policy. This finding is mirrored by the response of corporate debt to monetary policy shocks.

Our results are consistent with monetary policy transmitting via asset prices, through collateral constraints, generating heterogeneous effects on firms. To arrive at this conclusion, our research design employed various strategies to ensure that we are not simply picking up monetary policy effects via demand channels. Furthermore, this interaction between residential collateral values, monetary policy and firm-level characteristics is of quantitative importance. It is large enough to explain a significant share of both the observed firm-level heterogeneity in response to monetary shocks and the aggregate employment response to monetary policy.

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Tables

Table 1: Regression Sample Summary Statistics

Full Sample Summary Statistics				
Variable	**Mean**	**Median**	**25%tile**	**75%tile**
Number of Employees	496	48	8	115
Total Assets (£'000s)	83,754	3,418	713	8,240
2-year Employment Growth (%)	6.3	0	-3.9	16
2-year Real Asset Growth (%)	8.9	6.1	-8.9	25
Age (years)	22	15	6.8	29
Leverage (% assets)	66	63	42	81
Average Wage (£'000s)	25	22	14	31
192,223 Firm-Year Observations on 37,944 Firms				

Median/Mean values by Age, Leverage, Size						
	Age		**Leverage**		**Size (Employees)**	
	0-15	**15+**	**Below**	**Above**	**1-250**	**250+**
Variable			**Median**	**Median**		
Number of Employees^a	25	66	53	43	37	528
Total Assets (£'000s)^a	2,166	4,439	3,854	2,976	2,858	33,435
2-year Employment Growth (%)^b	9.2	3.5	5.3	7.4	6.6	4.4
2-year Real Asset Growth (%)^b	12	6.3	8.8	9	9.1	7.6
Age (years)^a	6.8	29	20	11	15	20
Leverage (% assets)^a	71	55	41	81	63	61
Average Wage (£'000s)^a	21	22	22	21	22	21

a = median, b = mean

Notes: The Table presents summary statistics for the baseline regression sample (see Section 2.1). Age is defined as the number of years elapsed from the date of incorporation. Leverage is measured as the ratio of the balance sheet items "Total Liabilities" to "Total Assets". Size is measured as the "Number of Employees". Average wage is defined as the ratio of "Remuneration" to "Number of Employees". All of these variables are measured at the beginning of the firm's accounting period. Employment and Asset growth are measured as the log difference from the beginning of the firm's accounting period to the end of the subsequent accounting period, a period of two years. Asset growth is deflated using the change in the consumer price index over the same period. These growth rates are trimmed at the 1/99 percentiles. For presentation in this table, leverage and the average wage are winsorised at the 1/99% levels. The upper panel shows the statistics based on the regression sample. The lower panel splits the statistics into two groups for each of age, leverage and size.

Table 2: Summary Statistics Split by Director Beta

Summary Statistics by House Price Beta						
					Director Beta	
	Unconditional	**Director Beta**			**vs Firm Beta**	
Variable	**Median**	**Low**	**High**	**Same**	**Lower**	**Higher**
Firm-level Summary Statistics: *133,078 Firm-Year Observations on 27,718 Firms*						
Number of Employees	56	70	43	41	59	65
Total Assets (£'000s)	3,900	4,320	3,414	2,764	4,273	4,674
Age (years)	15	18	14	15	15	15
Leverage (% assets)	63	62	63	62	63	63
Director-level Summary Statistics: *270,350 Director-Year Observations on 61,612 Directors*						
Director House Value (£'000s)	411	307	473	354	460	446
Director Outside Firm Local Area (%)*	62	57	66	0	100	100
Director Distance From Firm (Miles)	8.7	7.8	8.1	3	16	15
Director Age (Years)	49	48	49	48	49	48
Experience (Years)	9.7	8.8	9.8	8.2	11	10

Notes: (I) Firm-level statistics: further restricts the baseline regression sample to firms where the average director beta is non-missing. Age is defined as the number of years elapsed from the date of incorporation. Leverage is measured as the ratio of the balance sheet items "Total Liabilities" to "Total Assets". Medians are presented. The second and third column breaks the statistics down by whether the average director beta of the director's firm (averaged across all directors at the firm) is in the top tertile (*High Beta*) or bottom tertile (*Low Beta*) in a given year. The fourth, fifth and sixth column panel breaks the statistics down by whether the average director's local area has the same house price beta as the firm (*Same*), a lower house price beta, or a higher house price beta.
(II) Director-level summary statistics: statistics cover the directors of the firms in the first part of the table and are shown only for the directors whose housing beta can be calculated. Director House Value is the value of the director's house in £000s. Director Outside Firm Local Area is a dummy variable which takes value 1 when the director lives in a different local area to their firm, and 0 otherwise. Director Distance From Firm is the distance between the director's house and the firm, in miles. Director Age is the director's age, in years. Experience is the cumulative years of experience the individual has in years, across all directorships held. The columns are broken down by betas in the same manner as the firm-level statistics.

Table 3: Identification and Robustness

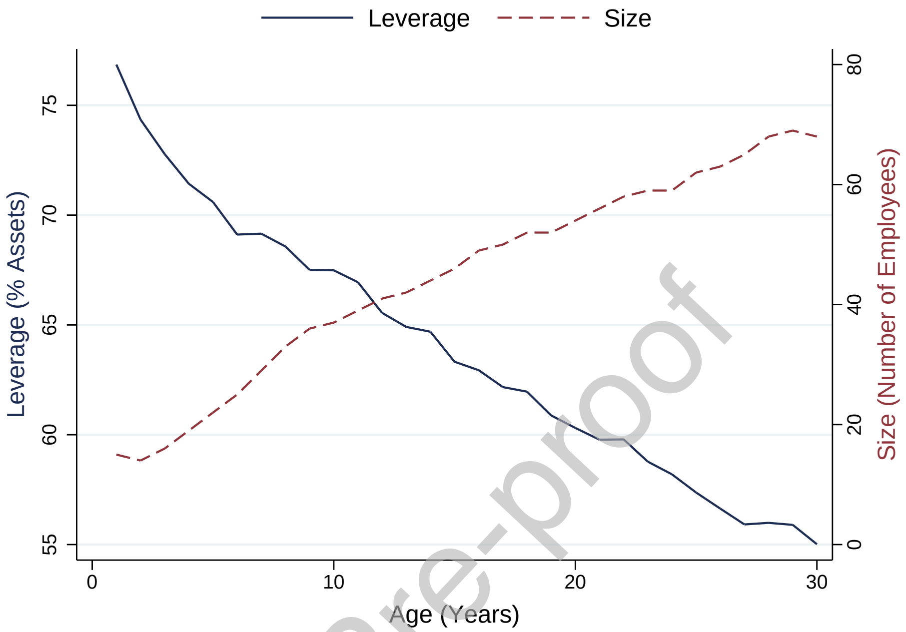
	Relative Impulse:	Younger, Higher Lev., High β vs Older, Lower Lev., Low β			Younger, Higher Lev., Low β vs Older, Lower Lev., Low β			Younger, Higher Lev., Hi β $-$ Younger, Higher Lev., Low β		
		Horizon (Years)			Horizon (Years)			Horizon (Years)		
	Specification	1	2	3	1	2	3	1	2	3
1.	Baseline (Figure 5b)	-0.0118***	-0.0221***	-0.0332***	-0.0068**	-0.0072	-0.0156***	-0.0050	-0.0149***	-0.0176***
	Mechanism									
2.	Total Debt (Figure A26)	-0.0385*	-0.0528	-0.0809**	0.0004	-0.0089	-0.0194	-0.0390**	-0.0439*	-0.0615**
3.	Prepaid Expenses (Figure A28)	-0.0228*	-0.0412***	-0.0607***	-0.0090	-0.0110	-0.0305**	-0.0138	-0.0302***	-0.0302**
4.	Fixed Assets (Figure A30)	-0.0270**	-0.0334***	-0.0380***	-0.0002	-0.0014	-0.0044	-0.0268**	-0.0319**	-0.0336*
	Local Demand									
5.	Turnover (Figure A32)	0.0047	-0.0048	-0.0177	0.0116	0.0088	0.0006	-0.0069	-0.0137*	-0.0182
6.	Granular region FEs (Figure A33)	-0.0113***	-0.0185***	-0.0298***	-0.0069*	-0.0073	-0.0156***	-0.0044	-0.0112**	-0.0141***
7.	Directors live 30-miles away (Figure A35)	-0.0213**	-0.0299*	-0.0407**	-0.0082	-0.0022	-0.0081	-0.0132	-0.0277*	-0.0326*
8.	Tradeables Firms (Figure A37)	-0.0047	-0.0195***	-0.0154	0.0130	0.0175	0.0102	-0.0177*	-0.0369***	-0.0255***
9.	Dir. 30-miles, Tradeables Firms (Figure A39)	-0.0294	-0.0393	-0.0239	-0.0023	0.0059	-0.0022	-0.0271	-0.0452	-0.0217
10.	Single Establishment (Figure A41)	-0.0149***	-0.0252***	-0.0377***	-0.0115***	-0.0087	-0.0189***	-0.0033	-0.0166**	-0.0189***
	Director Characteristics									
11.	Control for director income (Figure A43)	-0.0085	-0.0218***	-0.0401***	-0.0037	-0.0074	-0.0226***	-0.0048	-0.0143***	-0.0175**
12.	Fixed director location (Figure A45)	-0.0141**	-0.0201**	-0.0321***	-0.0106***	-0.0064*	-0.0094*	-0.0035	-0.0136**	-0.0226***
13.	Control for director char. (Figure A47)	-0.0092***	-0.0172***	-0.0283***	-0.0051	-0.0038	-0.0129***	-0.0041	-0.0133**	-0.0154***
14.	Director also shareholder (Figure A49)	-0.0304**	-0.0397*	-0.0483*	-0.0100*	0.0023	-0.0028	-0.0204	-0.0419*	-0.0454*
15.	Director not shareholder (Figure A51)	-0.0154	-0.0212	-0.0344*	-0.0038	-0.0007	-0.0124	-0.0115	-0.0205	-0.0220
16.	Housing wealth $<$ 15% of assets (Figure A53a)	-0.0002	-0.0011	-0.0054	-0.0049	-0.0060	-0.0121***	0.0046	0.0049	0.0067
17.	Housing wealth $\geq$ 15% of assets (Figure A53b)	-0.0163***	-0.0277***	-0.0416***	-0.0090***	-0.0082	-0.0172***	-0.0073*	-0.0195***	-0.0244***
	Monetary Policy Shocks									
18.	Ex-ZLB period (Figure A57)	-0.0125***	-0.0247**	-0.0389***	-0.0096**	-0.0101	-0.0175**	-0.0029	-0.0146**	-0.0214**
19.	Control for central bank forecasts (Figure A59)	-0.0080	-0.0192***	-0.0288***	-0.0067**	-0.0069***	-0.0146***	-0.0013	-0.0122*	-0.0142*
20.	Multiple Instruments (Figure A61)	-0.0059**	-0.0140***	-0.0246***	-0.0038	-0.0037	-0.0112***	-0.0022	-0.0103***	-0.0134***

$^{\star}p < 0.10, ^{\star\star}p < 0.05, ^{\star\star\star}p < 0.01.$

Notes: The table shows firm-level responses to a 25bp contractionary monetary policy shock for the alternative specifications detailed in Section 5. The dependent variable, except for rows 2-5, is the cumulative growth rate in log points of Employment from t $-$ 1 to t + 1, t+2, and t+3, respectively, where t is the date of the monetary policy shock. All results follow specification 2, including industry-month and NUTS1-month fixed effects, except where specified in Section 5. Younger is defined as less than 15 years old, and higher leverage is defined as above the median leverage by year. For each specification the table indicates the corresponding figure in the Online Appendix.

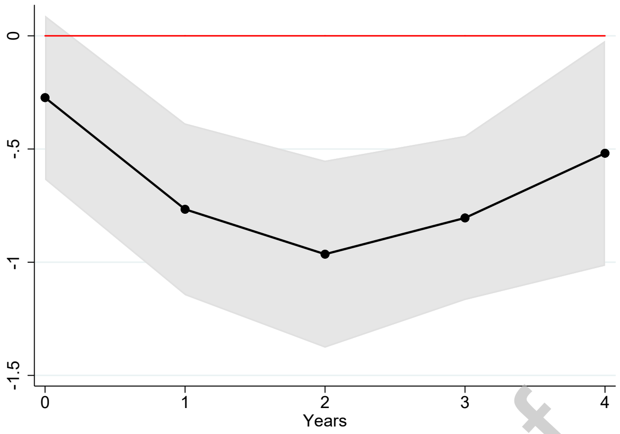
Figures

Figure 1: Firm Leverage and Size Over the Life-Cycle



Notes: the figure shows the median leverage (measured as the ratio of total liabilities to total assets) and the median firm size (measured as number of employees) for firms of each age group from 1 to 30. Age is measured as number of years since incorporation.

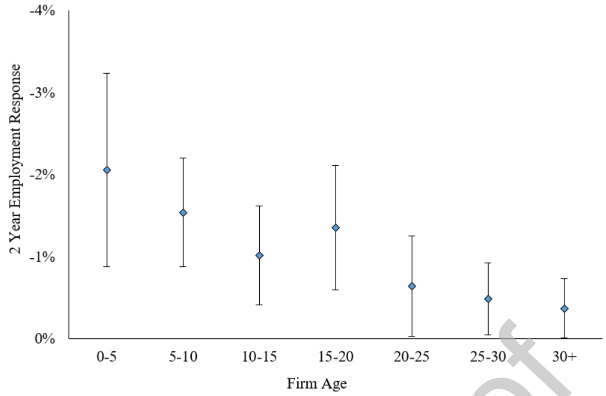
Figure 2: The Linear Effect of Monetary Policy on Firms



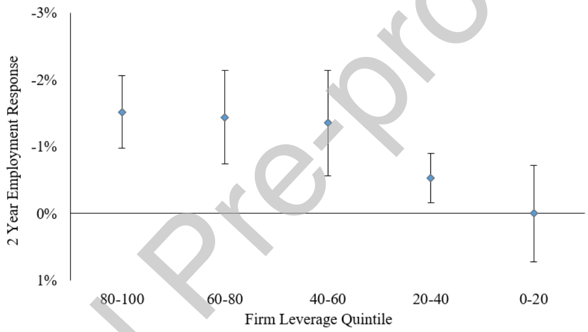
Notes: Firm-level responses to a 25bp contractionary monetary policy shock. Black lines are point estimates. Grey shaded area is the 90% confidence interval. The dependent variable is the cumulative growth rate in log points of employment from $t-1$ to $t+h$ where t is the date of the monetary policy shock and h is the x-axis.

Figure 3: Employment Responses to a Contractionary Monetary Shock by Age, Leverage, Size Groups - 2 year horizon

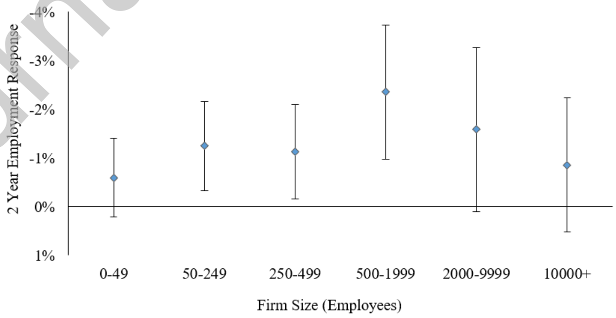
(a) Age



(b) Leverage



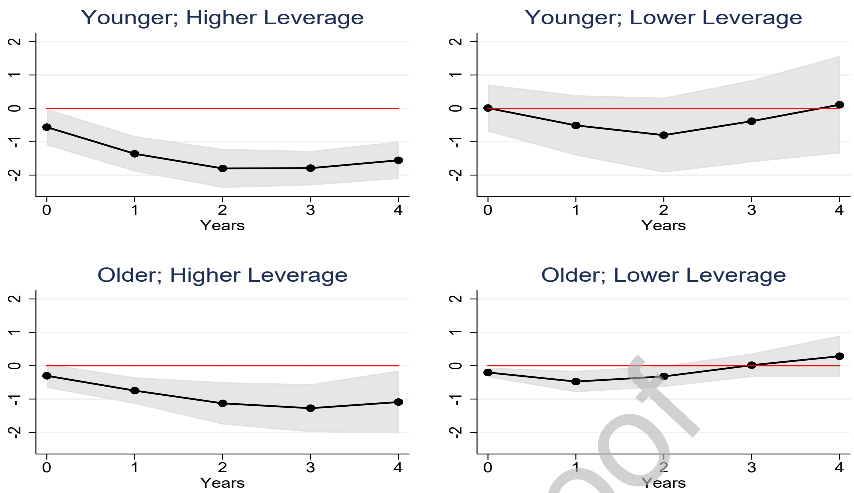
(c) Size



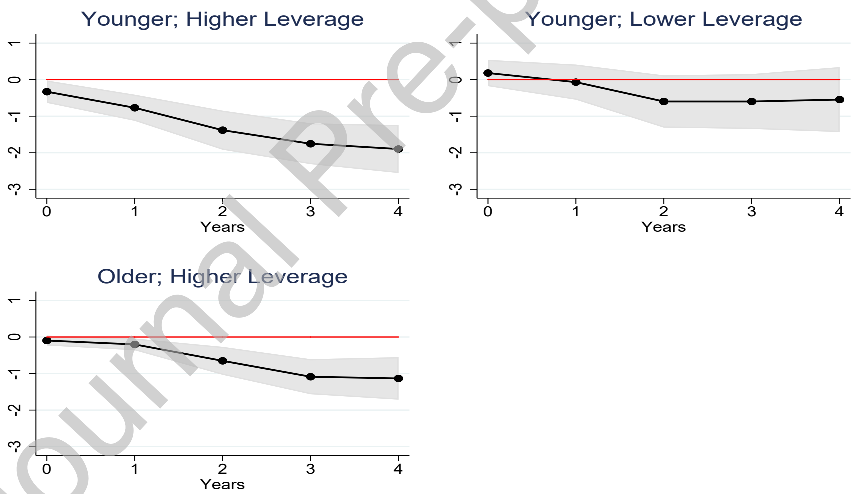
Notes: The candle chart summarises the point estimates (together with the 90% confidence interval) corresponding to the effect of a 25bp contractionary monetary policy shock for different groups of firms, sorted by age (top panel), leverage (middle panel) and size (bottom panel), as estimated by Equation 1.

Figure 4: The Effects of Monetary Policy on Employment by Age and Leverage

(a) Level Effects



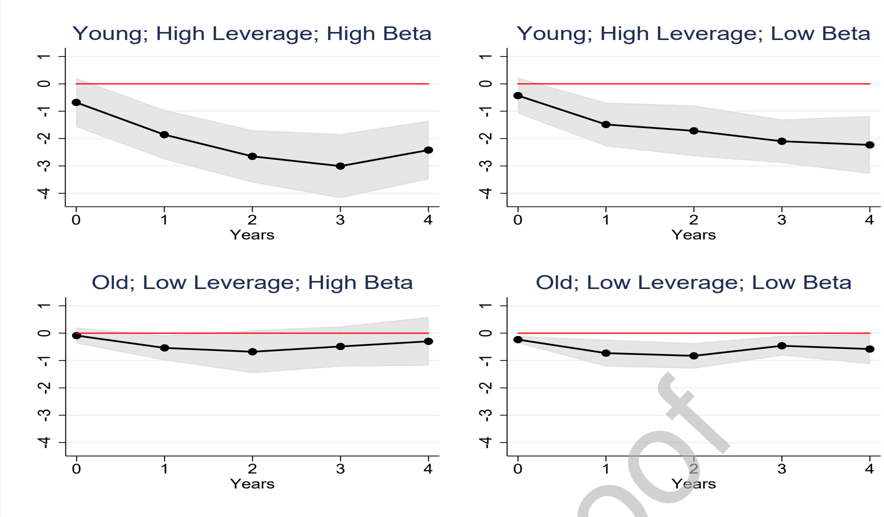
(b) Relative Effects



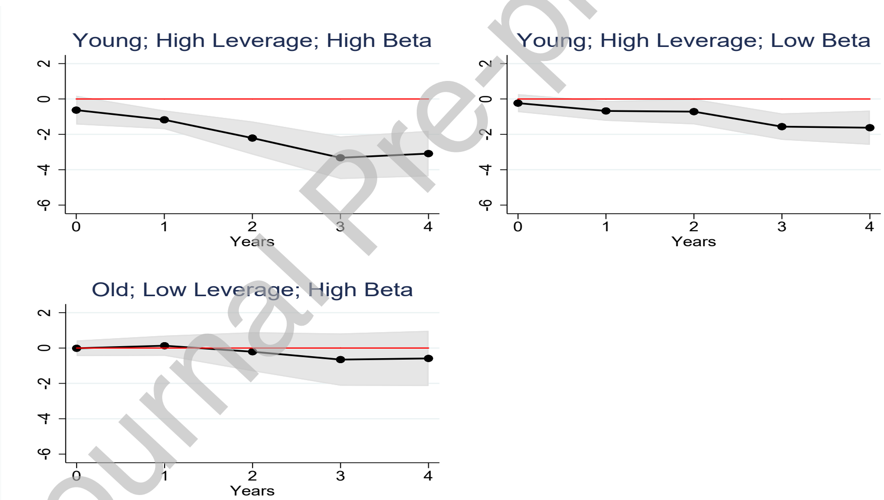
Notes: The figure shows firm-level responses to a 25bp contractionary monetary policy shock. Black lines are point estimates. Grey shaded areas are 90% confidence intervals. The dependent variable is the cumulative growth rate in log points of Employment from $t-1$ to $t+h$ where t is the date of the monetary policy shock and h is the x-axis. Panels a and b show the results for specifications 1 and 2, respectively. All the responses in Panel b are relative to the group of older and less levered firms (omitted given the inclusion of industry-month and NUTS1-month fixed effects). Younger is defined as less than 15 years old, and higher leverage is defined as above the median leverage by year.

Figure 5: The Effects of Monetary Policy on Employment by Age, Leverage and Director Beta

(a) Level Effects



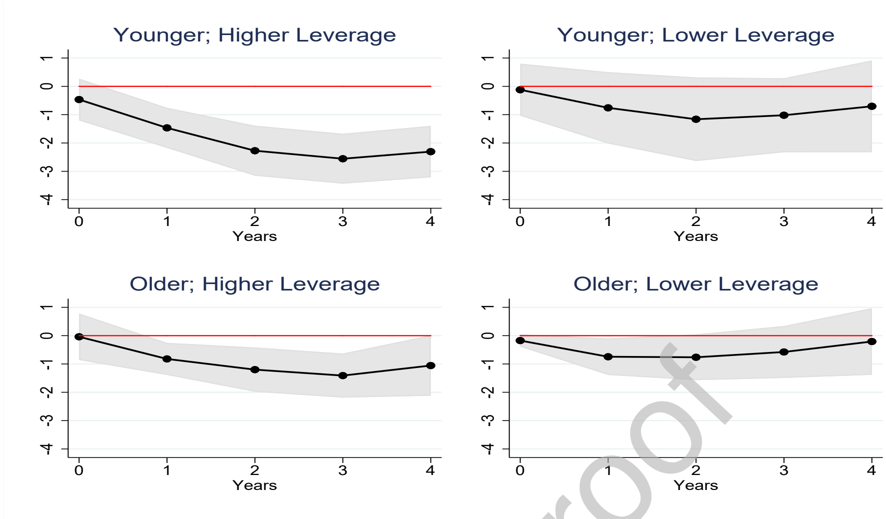
(b) Relative Effects



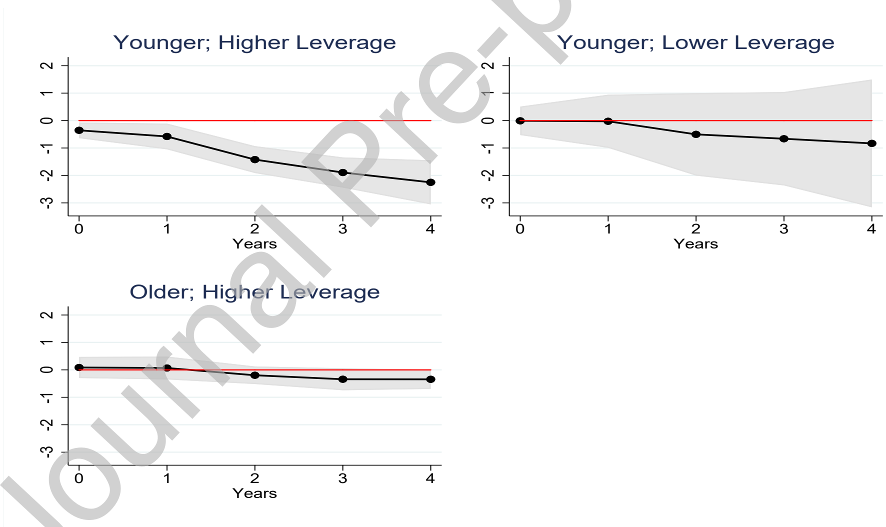
Notes: The figure shows firm-level responses to a 25bp contractionary monetary policy shock. Black lines are point estimates. Grey shaded areas are 90% confidence intervals. The dependent variable is the cumulative growth rate in log points of Employment from $t-1$ to $t+h$ where t is the date of the monetary policy shock and h is the x-axis. Panels a and b show the results for specifications 1 and 2, respectively. All the responses in Panel b are relative to the group of older and less levered firms with directors in low beta areas (omitted given the inclusion of industry-month and NUTS1-month fixed effects). Younger is defined as less than 15 years old, and higher leverage is defined as above the median leverage by year.

Figure 6: The Effects of Monetary Policy on Employment by Age, Leverage and Director Beta, Employment Weighted

(a) Level Effects



(b) Relative Effects



Notes: The figure shows firm-level responses to a 25bp contractionary monetary policy shock. Black lines are point estimates. Grey shaded areas are 90% confidence intervals. The dependent variable is the cumulative growth rate in log points of Employment from $t-1$ to $t+h$ where t is the date of the monetary policy shock and h is the x-axis. Panels a and b show the results for specifications 1 and 2, respectively. All the responses in Panel *b* are relative to the group of older and less levered firms with directors in low beta areas (omitted given the inclusion of industry-month and NUTS1-month fixed effects). Younger is defined as less than 15 years old, and higher leverage is defined as above the median leverage by year. Both regressions weight the firm observations by lagged employment, to align their weight in the sample with their contribution to aggregate employment. Administrative microdata is taken from the *Business Structure Database* from the *Office For National Statistics* (ONS), covering total employment by companies in the same industries as the baseline regression. The share of total employment for these companies accounted for by different firm size groups is calculated for the following employment bins: 1-9,10-49,50-99,100-149,150-199,200-249,250+. The share of observations in our baseline regression sample accounted for by each firm size bin is also calculated. Weights are then constructed for each size bin using these two set of numbers, to ensure that the weight placed on each group in our regression matches their contribution to aggregate employment. This work was produced using statistical data from ONS. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates.