Zombie Lending to U.S. Firms*

Giovanni Favara
Federal Reserve Board

Camelia Minoiu
Federal Reserve Bank of Atlanta

Ander Perez-Orive
Federal Reserve Board

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We show that U.S. banks do not engage in zombie lending to firms of deteriorating profitability, irrespective of capital levels and exposure to such firms. In contrast, unregulated financial intermediaries do, originating more and cheaper loans to these firms. We establish these results using supervisory data on firm-bank relationships, syndicated lending data for banks and nonbanks, and an empirical setting with quasi-random shocks to firm profitability. Although credit migrates from banks to nonbanks, zombie firms file for bankruptcy at an elevated rate, suggesting that nonbanks' zombie lending does not enhance the survival rate of distressed and unprofitable firms.

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*Contact: Giovanni Favara (giovanni.favara@frb.gov), Camelia Minoiu (camelia.minoiu@atl.frb.org), Ander Perez-Orive (ander.perez-orive@frb.gov). We thank Viral Acharya (discussant), Diana Bonfim (discussant), Anton Braun, Mark Jensen, John Kandrac, Artashes Karapetyan (discussant), Mico Loretan, Xu Lu, Indrajit Mitra, Veronika Penciakova, Sam Rosen, Leslie Shen Sheng (discussant), Dominik Supera, Edison Yu (discussant), Larry Wall, Jialan Wang, Tao Zha, and participants at the Federal Reserve System Credit Risk Conference, Fischer-Shain Center for Financial Services inaugural conference at Temple University, SFA Annual Meeting, Federal Reserve Day-ahead Conference on Financial Markets and Institutions, AEA meetings, IBEFA summer meetings, XIII Workshop on Institutions, Individual Behavior, and Economic Outcomes, and seminar participants at the International Monetary Fund, European Central Bank, Atlanta Fed, and Swiss National Bank for useful discussions and comments. Quinn Danielson, Yuritzy Ramos, and Makena Schwinn provided excellent research assistance. We are grateful to Tom Heintjes and David Jenkins for editorial suggestions. The views and conclusions are those of the authors and do not indicate concurrence by the Federal Reserve Board, the Federal Reserve Bank of Atlanta, the Federal Reserve System, or their staff.

1 Introduction

Zombie firms are unproductive businesses that survive on cheap credit despite generating insufficient profits to service their debt. These firms may have negative consequences for the real economy, as they tie up resources that could be used by more productive enterprises.[1]

A widely held view in the literature is that zombie firms are kept alive by bank lending policies, which are distorted by regulatory incentives. For example, Peek and Rosengren (2005), Caballero *et al.* (2008), Acharya *et al.* (2019), and Blattner *et al.* (2022) find evidence that undercapitalized banks (in Japan in the early 1990s and in Europe in the early 2010s) evergreened loans to unprofitable firms to delay the recognition of loan losses and avoid regulatory actions. According to this view, capital requirements pose a trade-off: higher requirements reduce banks' risk-shifting incentives but also increase the costs of recognizing loan losses, creating incentives to evergreen loans (Acharya *et al.*, 2021). An alternative view is that zombie lending—that is, lending to zombie firms—is a feature of financial intermediation. In recent theories of relationship lending, the provision of cheap credit to underperforming firms dominates liquidation decisions because it increases the likelihood of debt repayment. In these theories, evergreening depends on lenders' exposure to underperforming firms and is unrelated to lenders' capital positions or regulatory requirements (see Hu and Varas (2021) and Faria-e-Castro *et al.* (2024)).

The purpose of this paper is to shed new light on these alternative views. We use data on banks and nonbank financial institutions (NBFIs) in the United States to test whether bank capital positions and capital regulatory requirements, or lack thereof, foster zombie lending. Our three main findings are as follows: (1) U.S. banks, irrespective of capital levels and exposure to zombie firms, tighten lending terms to these firms; (2) NBFIs offset banks' reduced exposure, offering larger or cheaper loans to zombie firms relative to banks; and (3) despite the financial support of NBFIs, zombie firms exit the market through bankruptcy

[1] See Acharya *et al.* (2022) and Albuquerque and Iyer (2023) for reviews of recent research on zombie firms and zombie lending.

at a higher rate than other financially distressed firms. These results suggest that capital requirements at U.S. banks do not encourage zombie lending but may foster credit migration from regulated to unregulated financial institutions. Even so, NBFIs' lending does not appear to boost the survival rate of zombie firms. To the best of our knowledge, this is the first paper to study zombie lending in the United States by comparing banks and NBFIs.

There are two main challenges associated with studying zombie lending. The first one is that borrower and lender characteristics may be jointly determined due to endogenous matching (see, e.g., Chodorow-Reich (2014); Schwert (2018)). Endogenous firm-lender matching makes it difficult to determine whether zombie status affects or is affected by lenders' decisions. The second challenge is that there is no single definition of zombie firms, with researchers combining measures of profitability, leverage, and reliance on cheap credit to identify such firms. To address these challenges, we exploit a quasi-random shock to firm profitability that is exogenous to the financial sector, and we use two definitions of zombie firms, both of which reflect the different approaches taken in the literature.

We begin the analysis with the universe of bank loans to private and listed firms in the supervisory Federal Reserve Y-14 data set. These data have the most comprehensive coverage of U.S. firms with a banking relationship and offer detailed information on bank loans and firm balance sheets (starting in 2012).[2] To examine zombie lending by NBFIs, we use Refinitiv's DealScan, a data set of syndicated loans with information on loan terms and syndicated lending activities of both banks and nonbank lenders.

In the baseline analysis, we use these data to identify financially distressed firms in a preexisting bank relationship. These firms have high leverage and struggle to service debt interest payments. In contrast to the standard approach in the literature, we do not require that financially distressed firms receive subsidized credit—that is, credit at rates below those paid by the most creditworthy firms. Instead, we let the data reveal if lenders provide

[2]The Federal Reserve Y-14 is a data collection effort originating with the Dodd-Frank Act that gathers detailed information on the lending activities of large commercial banks with at least $50 billion in total consolidated assets (at end-2019).

additional or cheaper credit to these distressed firms that become unviable after a negative profitability shock. In this paper, we refer to distressed firms that are hit by a profitability shock as "zombie candidate firms" and to zombie candidate firms that receive subsidized credit as "zombie firms."

Our baseline analysis exploits the sharp and sustained decline in the global price of crude oil during the 2014–2015 period as an exogenous shock to firm profitability. For our purposes, this empirical setting is well suited for a few reasons. First, the oil price is influenced by global economic activity and, thus, exogenous to firms' fundamentals or lender balance sheets. Second, the oil shock originates outside the financial sector, helping us identify firms that transition into zombie status for reasons that are independent of lending decisions and endogenous firm-lender matching. Third, it allows us to study lending to financially distressed firms that might default absent additional credit. In contrast to previous studies—which focus on shocks to banks' capital positions—our empirical design exploits variation in lenders' capital and exposure to financially distressed firms, both measured before the realization of firms' profitability shock. In addition, this setting allows us to compare lending by regulated banks and other financial intermediaries that are not subject to regulatory capital requirements.

Our first set of results is that after the 2014–2015 oil price shock, U.S. banks do not engage in zombie lending, irrespective of capital levels and exposure to zombie candidate firms. In a difference-in-differences setting, we estimate that banks, on average, reduce their loan exposures to ex-ante distressed firms in the oil sector. Banks also charge distressed firms in the oil sector higher interest rates, likely as a premium for exposure to higher credit risk. Importantly, these results do not vary with banks' capital positions nor with bank exposure to these firms. Whereas large U.S. banks are generally well capitalized, they are subject to annual stress tests that determine banks' capital needs under adverse economic conditions. Banks with lower capital buffers may follow a policy of forbearance to delay the default of troubled firms. In our analysis, we find no evidence of a link between post-stress capital buffers and lending to firms that acquire zombie candidate status.

We show that these baseline results are not driven by several confounding factors, including the possibilities that zombie candidate firms demand relatively less bank credit and that banks with lower capital buffers and high firm exposure face more opportunities to scale back exposures to these firms by modifying lending terms through loan renegotiations or renewals. In addition, we address the potential concern that banks that cut lending to zombie firms were those that had to raise relatively more equity to comply with capital requirements after the Global Financial Crisis (GFC). We assuage this concern by using a regression framework that allows for time-varying bank-specific shocks. In addition, we document that low-capital, high-exposure banks were less likely than other banks to increase capital ratios during the 2014–2017 period.

Our second main result is that—in contrast to banks—NBFIs *increase* their exposure to distressed firms after the oil shock. NBFIs have a higher propensity than banks to grant new loans to zombie candidate firms, and they also charge them lower spreads and offer larger loans. Furthermore, the data suggest a link between bank capital and NBFI lending. We estimate that NBFIs' lending shares to zombie firms are higher in syndicated loan deals with *lower* capital levels of participating banks. Nonbanks' lending shares are also higher after the strengthening of bank capital regulations under the Dodd-Frank Act in 2014. This evidence suggests that bank capital regulation might lead to zombie lending by fostering credit migration to less regulated financial institutions.[3]

The third main finding is that our baseline results hold outside the 2014–2015 oil shock analysis and do not depend on how we define zombie firms. We use one standard definition that identifies zombie firms as financially distressed and unprofitable businesses and a second definition that, in addition, requires, following Acharya *et al.* (2019), that these firms receive subsidized credit. In a regression framework that compares lending by the same lender to multiple firms in the same industry, location, and quarter, we estimate that banks—

[3]See, for example, Aiyar *et al.* (2014), Buchak *et al.* (2018), Kim *et al.* (2018), Irani *et al.* (2021), Chernenko *et al.* (2022), Bednarek *et al.* (2023), and Sundaresan and Xiao (2024).

irrespective of capital levels and firm exposure—lend less and under more restrictive terms to firms that transition into zombie status relative to other firms. Meanwhile, zombie candidate firms are more likely to receive more loans at better terms from NBFIs.

Our final result is that zombie firms are twice as likely to file for bankruptcy compared with firms that are financially distressed yet viable. This finding is consistent with the view that the efficiency of the U.S. bankruptcy system may facilitate a quick resolution of contract disputes, reducing lenders' incentives to evergreen loans (Ponticelli and Alencar, 2016; McGowan *et al.*, 2018; Becker and Ivashina, 2021). This finding also suggests that the credit migration from banks to nonbanks has limited spillovers to the real economy.

Taken together, our evidence indicates that U.S. capital regulation does not appear to distort banks' incentives to evergreen loans to zombie firms. At the same time, lack of capital regulation appears to promote zombie lending by NBFIs. Our findings support theories of financial intermediation in which capital regulation mitigates risk-shifting incentives but also promotes credit migration to less regulated financial institutions.

Contribution to the literature. A number of papers have studied zombie lending in the context of the Japanese banking crisis of the 1990s (Peek and Rosengren, 2005; Caballero *et al.*, 2008; Giannetti and Simonov, 2013) and the European debt crisis in 2011 (Acharya *et al.*, 2019; Blattner *et al.*, 2022). These papers show that low-capital banks lend to zombie firms to avoid writing bad loans off their balance sheets. They also show that zombie lending by these banks leads to credit misallocation and lower aggregate productivity.[4] We contribute to this literature by showing that U.S. banks, irrespective of their capital positions and exposure, do not engage in zombie lending, and although unregulated financial intermediaries do, such lending has limited implications for the economy. Our paper is also related to Faria-e-Castro *et al.* (2024), who study U.S. banks' incentives to evergreen term loans as a function

[4]To various degrees, zombie lending has been documented in other developed and emerging economies; see, e.g., McGowan *et al.* (2018), Banerjee and Hofmann (2018), Kulkarni *et al.* (2019), Chopra *et al.* (2021), Bonfim *et al.* (2020), Schmidt *et al.* (2020), Altman *et al.* (2021), De Martiis and Peter (2021), Banerjee and Hofmann (2022), Albuquerque and Iyer (2023), Amundsen *et al.* (2023), and Wang *et al.* (2024).

of their exposures to borrowers near default. Instead, our analysis studies zombie lending behavior of banks with a focus on their capital positions, in addition to firm exposure, and compares lending outcomes at regulated versus unregulated financial intermediaries.

Our finding that NBFIs engage in zombie lending adds to the literature that sees capital regulation as reducing banks' balance sheet capacity at the advantage of unregulated nonbanks (Irani *et al.*, 2021; Bednarek *et al.*, 2023). However, the higher bankruptcy rate of zombie firms in our data, compared to distressed firms, suggests that the credit migration from regulated to unregulated financial institutions that we document has limited implications for the macro-economy. Our firms' bankruptcy results support another strand of the zombie firm literature that argues that efficient bankruptcy systems encourage creditors to resolve bad loans through insolvency, reducing lenders' incentives to evergreen loans (Andrews and Petroulakis, 2019; Kulkarni, 2020; Becker and Ivashina, 2021; Li and Ponticelli, 2022).

Our paper also speaks to the literature relating low interest rates to capital misallocation and economic stagnation (Gopinath *et al.*, 2017; Aghion *et al.*, 2019; Caggese and Pérez-Orive, 2022). Our regression samples include the post-GFC period, which features low interest rates and accommodative credit conditions. Even so, we do not observe a meaningful rise in zombie firms during this period and find no evidence that U.S. banks allocate capital to underperforming firms in a low interest rate environment.

2 Data

Our analysis requires detailed information on firm-lender credit relationships and firm balance sheets. We draw such information from two main data sources: the Y-14 data set, which provides comprehensive supervisory data on commercial and industrial (C&I) loans from large U.S. banks, and DealScan, a dataset with detailed information on the lending activities of banks and nonbanks in the syndicated loan market. These and other data sources are described in this section.

Federal Reserve's Y-14 Since 2012, the FR Y-14Q Schedule H.1 (Corporate Wholesale Risk) collects quarterly data on C&I loans with commitment amounts above $1 million from bank holding companies (BHCs) that are subject to the Federal Reserve stress tests.[5] The number of reporting banks in Y-14 fluctuates between 31 and 36 over time. The data account for nearly three-fourths of total C&I lending (Bidder *et al.*, 2021; Favara *et al.*, 2021) and about 85% of total banking-sector assets (Frame *et al.*, 2023). A key advantage of these data is the extensive coverage of private firms that borrow from reporting banks. In any given year, we observe about 70,000 firms, of which about 3% are publicly listed.

We use the Y-14 data in two ways. First, we rely on extensive firm balance sheet information (as reported by the banks) to classify firms as zombie candidates or zombie firms based on alternative definitions (defined in Section 3.2). Second, we use information on individual loan agreements to study the terms of bank lending to such firms.

We use two alternative measures of bank capital. The first one is the "post-stress capital ratio" defined as the minimum common equity Tier 1 capital (CET1) ratio estimated under the adverse scenario (formally, the "Supervisory Severely Adverse" scenario) of the Dodd-Frank Act Stress Test.[6] Stress tests assess banking sector resilience to hypothetical severe economic downturns and determine the minimum CET1 capital ratio that participating banks must hold to cushion losses without becoming insolvent. The second measure is the BHC-level regulatory CET1 capital ratio from the Consolidated Financial Statements for Holding Companies, FR Y-9C. From these data we also use total bank equity to normalize bank loan commitments and obtain a measure of bank exposure to individual borrowers.

Table 1 reports summary statistics for firm-bank-quarter observations in the baseline regression sample between 2012 and 2017. The volume of committed credit in each firm-bank

[5]These stress tests include the Comprehensive Capital Analysis and Review and the Dodd-Frank Act Stress Test. See link for more information on the Y-14 data. The data used in this study were downloaded on May 11, 2021.

[6]These data are publicly available on the Federal Reserve's stress test page starting in 2014.

pair has a mean of \$33.9 million and a large standard deviation (\$86.3 million), reflecting in part the significant degree of heterogeneity in firm size and leverage in our data. The average interest rate on outstanding loans is 2.8%. The average firm in our sample is relatively large, but there is high dispersion, with total assets ranging from \$5 million at the 10th percentile of its distribution to \$5.7 billion at the 90th percentile. Leverage, interest coverage ratio (ICR), and sales growth—our main criteria to identify distressed and zombie firms in subsequent sections—exhibit significant heterogeneity across firms and years.

Refinitiv LPC DealScan As the coverage of the Y-14 data is limited to banks, we complement the firm-bank analysis with data on syndicated loans from Refinitiv Loan Pricing Corporation (LPC) DealScan, which provides information on the syndicated lending participations of both banks and NBFIs. Syndicated loans represent a sizeable portion of commercial lending, accounting for about one-quarter of aggregate banking system C&I loans in the Y-14 data and one-third of C&I loans on the balance sheets of large U.S. banks (Ivashina and Scharfstein, 2010). We use the sample of loans originated in 2010 or later to focus the analysis on the post-GFC period, which features strengthened bank capital regulation.

Syndicated loans are typically structured as deals comprising multiple loan tranches. For each loan deal, we observe the characteristics of individual loan tranches (e.g., tranches typically differ in terms of loan amounts and type—credit line vs. term loan), the identities of the borrower and of all lenders in the syndicate, and the loan shares that individual lenders contribute to each loan tranche. We use the "lender type" variable and additional matching approaches to classify lenders as banks or nonbanks. Furthermore, the dataset includes information on interest rate spreads, which together with other loan pricing terms, are determined at the deal level (Ivashina, 2005). These data allow us to study differences in lending policies to firms across bank and nonbank lenders, how these policies vary with the level of bank capital, and across loan tranches and deals. In Section A-I, we provide details on the processing of DealScan data and the classification of lenders into banks and NBFIs.

Compustat and Call Reports We double-match syndicated loans with balance sheet information for firms and banks. As most borrowers in the syndicated loan market are public firms, we obtain quarterly firm balance sheets from Compustat via the crosswalk in the DealScan-Compustat Linking Database (Chava and Roberts, 2008). The Compustat-DealScan match yields approximately 10,500 loan deals to nearly 5,000 firms over 2010–2019. The balance sheet data from Compustat are used to identify zombie firms and control for firm balance sheet characteristics in the empirical analysis. In addition, we conduct a string match of bank names across DealScan and the Call Report. Section A-I describes the matching approaches involving DealScan, Compustat, and the Call Report and the characteristics of the final samples.[7]

Firm Bankruptcy Data We use data on bankruptcy filings from the S&P Capital IQ U.S. Bankruptcy Tracker to measure ex-post firm performance and market exit. Data coverage includes listed firms, private firms with public debt at the time of the bankruptcy filing above $2 million, and private firms with total assets or liabilities at the time of the bankruptcy filing above $10 million. We supplement these data with several additional bankruptcy events for public firms recorded in the Mergent FISD data set. To examine firm exit via bankruptcy, we construct a regression sample that retains, from the universe of borrowing firms in the Y-14 data, those firms that meet the criteria for being included in the S&P Capital IQ Bankruptcy Tracker.[8] The regression sample comprises about 14,500 firms per year and a total of 310 bankruptcy events over the sample period.

[7]Summary statistics for the DealScan regression samples are shown in Table A1. The probability of a new loan origination in the bank-lender sample in any given year is 11% and average loan spreads (usually over the London Interbank Offered Rate, or LIBOR) are 227 basis points. The typical loan syndicate has 14 participants and the share of nonbank lenders across loan tranches is 5.5%. Note also that the average firm in DealScan is significantly larger than in the Y-14 sample, with total assets of $54 million at the 10th percentile and $26.1 billion at the 90th percentile.

[8]Specifically, we ensure that the firms have public debt of at least $2 million and total assets of at least $10 million. Because we do not observe the amount of public debt of Y-14 firms, we approximate it as the difference between total debt and total bank debt across Y-14 reporting banks. Since this calculation omits bank debt from non-reporting banks, we likely overestimate the amount of public debt, and, hence, the number of firms eligible to be included in the S&P Capital IQ Bankruptcy Tracker.

3 Zombie Lending by Banks

3.1 Oil Price Shock Analysis

We begin our analysis of zombie lending with a quasi-natural experiment. We select firms that are financially distressed and exploit the sharp and sustained decline in the global price of crude oil in 2014 and 2015 as a quasi-random shock to the profitability of oil sector firms. We then estimate whether bank lending to financially distressed firms in the oil sector changes as their profitability deteriorates after the oil shock. We examine the terms of lending by NBFIs in Section 4.

2014–2015 Oil Price Shock The global price of crude oil experienced a sudden and sizable drop between mid-2014 and early 2015, resulting in one of the largest declines in the price of oil in recent history (see Figure A1). This price decline was largely unanticipated and driven by a combination of factors, including excess supply and declining global demand for oil (Baumeister and Kilian, 2016; Prest, 2018). The cumulative price drop of roughly 70% between 2014 and 2015 put severe stress on oil-producing firms and delayed their investments in alternative drilling techniques, stifling the growth prospects of these firms. Baumeister and Kilian (2016) report that the oil price decline triggered a significant decline in investment spending in the oil sector.

Empirical Specification To test for differences in bank lending policies to ex-ante distressed firms with a preexisting bank relationship that transition into zombie candidate status after the oil shock, we use the following regression model estimated with data at the firm-bank-quarter level:

$$\begin{aligned} \textit{Lending outcome}_{bit} =& \beta_1(\textit{Distressed}_i \times \textit{Oil Sector}_i \times \textit{Post}_t) + \lambda^c \textit{Lower level controls} + \\ &+ (\lambda^x + \lambda^p \textit{Post}_t) \times X_{it} + \gamma_{jt} + \eta_{st} + \delta_{bt} + \psi_{bi} + \epsilon_{bit} \end{aligned} \tag{1}$$

where the lending outcome of interest between bank b and firm i (in industry j and state s) in quarter t is the (log of) total loan commitments and the weighted average loan interest rate. $Distressed_i$ is a dummy variable for firms that are in financial distress before the oil shock; that is, firms with a debt-to-asset ratio above the cross-sectional median and an ICR below one, during the 2013–2014 period. $Post_t$ is an indicator variable that takes value 1 for the three-year period following the oil shock (2015–2017) and value 0 in the three years before the shock (2012–2014).

The regression model includes all the double interactions and individual variables of the triple DiD term in *Lower level controls*; it also includes a vector of firm-specific controls X_{it} such as size (log-assets), cash holdings (cash/assets), and tangibility ratio (tangible assets/assets), which enter the specification individually and interacted with the $Post_t$ dummy. $Oil\ Sector_i$ is a dummy variable taking value one for firms exposed to the oil price decline, that is, firms that operate in the broad sectors of oil and gas (O&G) extraction, drilling, and support activities for related operations, based on their granular NAICS industry classification.[9]

We saturate the regression model in equation (1) with an array of fixed effects. To start with, we include industry×quarter and state×quarter fixed effects (γ_{jt} and η_{st}) to control for time-varying unobserved demand shocks that are common to all firms in a given industry or state. As many of the firms in our analysis are private and operate locally, these fixed effects control for local shocks that cannot be diversified away. We also include bank×quarter fixed effects (δ_{bt}) to control for time-varying unobserved heterogeneity across banks (such as quarterly shocks to banks' funding costs or capital position). Finally, we add firm×bank fixed effects (ψ_{bi}) to control for unobserved factors that are specific to a firm-bank

[9] More specifically, we classify firms as oil sector firms if they have one of the following NAICS codes: O&G Extraction (code 211), Drilling O&G Wells (code 213111), Support Activities for O&G Operations (code 213112), Natural Gas Distribution (code 2212), Pipeline Transportation (code 486), O&G Pipeline and Related Structures Construction (code 23712), Mining & O&G Field Machinery Manufacturing (code 33313), Petroleum Refineries (code 32411), Other Petroleum and Coal Products Manufacturing (code 32419), Petrochemical & Industrial Gas Manufacturing (codes 32511, 32512), and Petroleum & Petroleum Products Merchant Wholesalers (code 4247).

relationship, such as banks' private or soft information on borrower creditworthiness, and for endogenous firm-bank matching (Chodorow-Reich, 2014; Schwert, 2018) which may be driven by bank specialization in particular borrower activities and geographies (Paravisini *et al.*, 2023; Blickle *et al.*, 2023).

Our regression framework in equation (1) is akin to a triple DiD setting that compares bank lending outcomes before and after the oil shock (first difference) to distressed versus nondistressed firms (second difference) and, among distressed firms, between firms in the oil sector versus other sectors (third difference). Distressed firms in the oil sector are likely to acquire zombie candidate status after the oil shock as their ability to generate profits deteriorates and they may become economically unviable. The identifying assumption is that the transition from distressed to zombie candidate status is induced solely by the oil price shock and not by past bank lending practices nor by unobserved bank and firm characteristics that determine their matching in the loan market.

The coefficient of interest is β_1, which measures the effect of zombie candidate status on bank lending decisions. A negative β_1 for loan quantities and a positive one for loan pricing would suggest that banks refrain, on average, from lending to distressed firms because these firms become less profitable (hence riskier) after the oil shock.

A key finding of the literature on zombie lending is that lending decisions are distorted by banks' weak capital positions, and more so when banks are highly exposed to a particular borrower (Acharya *et al.*, 2019; Bonfim *et al.*, 2020; Blattner *et al.*, 2022; Faria-e-Castro *et al.*, 2024). The typical finding is that undercapitalized banks direct additional credit toward low-quality firms to avoid loan defaults, as these defaults would deplete their capital buffers. In addition, the incentives of undercapitalized banks to continue lending to a zombie firm are stronger the larger the impact of a firm's default on bank equity. To estimate the effects of banks' capital and firm exposure on lending to zombie firms, we split banks in our sample into those that have low capital ratios and high firm exposure (as a share of bank equity) using the cross-sectional distributions of these two variables *before* the oil shock. Accordingly, we define a *low-capital high-exposure bank* dummy variable for banks that simultaneously have a high (above-median) exposure and low (bottom quartile) post-stress CET1 capital ratio at the end of 2014, and estimate a modified version of equation (1):

$$\begin{aligned} \textit{Lending outcome}_{bit} = & \sum_{\tau=1,2} \beta_\tau(\textit{Distressed}_i \times \textit{Oil Sector}_i \times \textit{Post}_t) \times \textit{Bank Type}_{b,\tau} + \\ & + \sum_{\tau=1,2} \lambda^b(\textit{Lower level controls}) \times \textit{Bank Type}_{b,\tau} + \\ & + (\lambda^x + \lambda^p \textit{Post}_t) \times X_{it} + \gamma_{jt} + \eta_{st} + \delta_{bt} + \psi_{bi} + \epsilon_{bit}, \end{aligned} \tag{2}$$

where $\tau = 1$ indicates a *low-capital high-exposure* bank and $\tau = 2$ indicates *other* bank. In additional specifications, we explore whether low-capital high-exposure bank effects are driven by either the bank's level of capital or its exposure to zombie firms. A positive β_1 for loan quantities at low-capital banks suggests that such banks extend more credit than other banks to distressed borrowers after the oil shock, in line with the predictions of the zombie lending literature. We also report p-values of two-sided t-tests of the null hypothesis (H_o) of coefficient equality $\beta_1 = \beta_2$ against the alternative hypothesis H_a that $\beta_1 \neq \beta_2$. Rejecting the null hypothesis of coefficient equality across bank types suggests a lack of evidence for the zombie lending hypothesis.

Results Panel A of Table 2 presents the estimation results for the specification in equation (1).[10] The estimates in column 1 indicate that, following the oil price decline, distressed firms in the oil sector—that is, firms likely to transition into zombie candidate status—experience a reduction in loan amounts of 13.4% higher than other firms. In addition, the estimates in column 2 show that banks grant these firms more expensive loans (at interest rates close to 24 bps higher on average) than other firms, likely as a compensation for the higher credit risk. These results suggest that banks pare back their exposures to distressed firms that are adversely affected by the oil shock, even if these firms and banks are in a preexisting

[10]In Table 2 we only show the main coefficients of interest. Table A2 additionally reports all the estimated coefficients for lower-level terms and firm-level controls in levels and interacted with Post.

relationship.

In Panel B of Table 2, we test for differences in lending terms to zombie candidate firms between *low-capital high-exposure* banks and other banks. These estimates by bank type show that lending terms to zombie candidates are not statistically different across the two groups of banks. Both *low-capital high-exposure* and other banks reduce loan exposures to distressed oil firms by 12-14% and charge them loan rates that are higher by 21 to 32 bps after the oil shock.

Table 3 breaks down the effects on lending terms of bank capital and bank exposure to firms. Regression estimates reveal that neither banks with high firm exposure (panel A) nor banks with low capital buffers (panel B) offer advantageous lending terms to zombie candidate firms. Tests of coefficient equality across bank groups indicate statistically insignificant differences in lending outcomes for high versus low-exposure banks (panel A). In panel B, estimates are relatively higher for low-capital banks, and we reject the null of coefficient equality across bank groups, which indicates that low-capital banks tighten lending terms to zombie firms even more than high-capital banks. In fact, low-capital banks reduce loan exposures by 21.4% and increase interest rates by close to 35 bps compared to 8.2% and 17 bps for high-capital banks.

These results suggest that low-capital banks with significant exposure to zombie candidates do not evergreen loans nor do they provide cheaper loans to ex-ante distressed firms after the oil shock. These findings are, therefore, inconsistent with the notion of zombie lending in our sample of Y-14 reporting banks.

Ruling Out Potential Confounding Effects We present three sets of tests to allay potential concerns that our baseline results may be driven by other confounding factors. One concern is that banks with low capital ratios had to cut lending or raise capital more than other banks to comply with the phase-in of new regulatory capital minimums starting in 2015 and stress tests. This adjustment could have curtailed banks' ability to lend to zombie candidate firms for reasons that are unrelated to firms' risk. Our regression analysis partly addresses this concern by including bank×quarter fixed effects, which control for time-varying bank-specific shocks. In addition, Figure A2 presents visual evidence to further assuage this concern. The figure plots the dispersion of capital (CET1) ratios before the oil shock (2013) and at the end of the post-oil shock period (2017) for the banks in our sample. Contrary to the view that low-capital, high-exposure banks had to make relatively larger adjustments to their capital ratios post-GFC regulation, the scatter plot indicates that these banks adjusted capital ratios by *less* than other banks during the phase-in of the new regulatory regime.

A second concern is that our results are driven by credit demand. For instance, ex-ante financially distressed firms may reduce loan demand relatively more from *low-capital high-exposure* banks than from other banks after the oil shock, leading to a downward bias in these banks' exposures in the post period. This concern is in part mitigated by the fact that *low-capital high-exposure* banks not only reduce their lending exposures (quantity effect) but also charge distressed firms higher loan rates (price effect) after the oil shock (Table 2). Two additional results further increase our confidence that we identify supply-driven effects. As seen in Table A3, our baseline results continue to hold when we use very granular fixed effects, such as state×industry×quarter fixed effects (panel A) and state×industry×size-group×quarter fixed effects (panel B). The latter fixed effects control for common loan demand shocks to firms of similar size or in the same location and industry and are valid alternative demand controls for the traditional Khwaja-Mian-type firm×time fixed effects (Khwaja and Mian, 2008; Degryse *et al.*, 2019). Furthermore, as shown in Table A4, we estimate that after the oil shock, ex-ante distressed oil firms do not change their credit line utilization on average (columns 1 and 3) nor do they do so more vis-a-vis a certain type of bank (column 2). To the extent that credit line utilizations reflect changes in credit demand, this evidence reinforces the view that our baseline results are not driven by demand factors.

Third, we address the potential concern that the results are driven by *low-capital high-*

exposure banks being more inclined to renegotiate loans and modify contractual terms. This possibility would arise if such banks offered on average shorter-maturity loans and could confound our baseline results insofar as we examine the sample of outstanding loans, where modifications in loan terms stem mainly from loan renegotiations. To alleviate this concern, in Table A5 we show our baseline findings hold when we limit the sample to newly originated loans. Furthermore, low-capital, high-exposure banks grant longer-maturity loans than other banks.[11]

Additional Checks In Table A6 we show that levels of statistical significance for our main estimates remain virtually unchanged for different choices of double-clustering of the standard errors (e.g. on firm-bank and quarter or on firm and quarter). In addition, Table A7 shows that our baseline results are robust to controlling for differential pre-shock trends in lending outcomes for distressed firms in the oil sector and firms in the comparison group. Following Autor *et al.* (2024), these regressions control for a pretrends term that interacts the change in the dependent variable over the pre-shock period with a linear time trend. Finally, as seen in Table A8, our baseline results are robust to using the BHC-level regulatory CET1 ratio (as opposed to the post-stress CET1 ratio) measured at the end of 2013—that is, before the phase-in period of new capital requirements and the first Dodd-Frank stress tests.

3.2 External Validity

We assess the external validity of our oil shock analysis by leveraging the Y-14 data to study zombie firms and zombie lending outside the oil shock episode. We start by outlining two leading definitions of zombie firms from the literature and documenting stylized facts on these firms over the 2014–2019 period. Then, we use a regression framework to generalize the results of the previous section to the universe of firms and banks in Y-14.

[11] The outstanding loans of these banks in Y-14 have an average remaining maturity of 2.69 years compared to 2.24 years at other banks (the difference is statistically significant at a 1% level). In addition, low capital, high-exposure banks tend to grant new loans with maturities of 4.68 years, on average, compared to 4.47 years at other banks (this difference is statistically significant at a 5% level).

Alternative Definitions of Zombie Firms The literature has used three alternative approaches for detecting zombie firms. In the first approach, firms are classified as zombies if they are unviable—that is, highly leveraged, with limited debt-servicing capacity, and unprofitable (see e.g., Peek and Rosengren (2005); McGowan *et al.* (2018); Banerjee and Hofmann (2018); Schivardi *et al.* (2020); Bonfim *et al.* (2020)). A second approach selects firms as zombies if they receive subsidized credit from banks (see, e.g., Hoshi (2006); Caballero *et al.* (2008); Giannetti and Simonov (2013)). A third approach combines both requirements (Acharya *et al.*, 2018, 2019).

In light of these approaches, we use two alternative definitions of zombie firms. The first one requires that unprofitable firms be in financial distress. This classification mirrors the approach used in the oil shock analysis (Section 3.1) and selects firms in a preexisting banking relationship to be in zombie status if they (1) are highly indebted (leverage is above-median); (2) struggle to service debt interest payments (ICR is less than one), and (3) are unprofitable (have negative three-year average sales growth).[12] The second definition follows Acharya *et al.* (2019) and classifies firms in a preexisting banking relationship as zombies if they (1) have speculative-grade rating and (2) receive loans at interest rates below those offered to the most creditworthy firms.[13]

These two definitions are complementary. The first one selects zombie firms using only balance sheet information. The second one adds the requirement that firms receive loans at

[12] Studies show that sales growth is a reliable predictor of future productivity (Goyal and Yamada, 2004; Whited and Wu, 2006). Imposing this requirement also reduces the risk of incorrectly classifying temporarily unprofitable firms with good future growth prospects as zombies. In a similar vein, Schivardi *et al.* (2020) use return on assets as a gauge of profitability, while Banerjee and Hofmann (2018) require that publicly listed firms have high leverage and low Tobin's q. One drawback of using Tobin's q as an indicator of firms' growth potential is that it is available only for listed firms.

[13] Firms in Acharya *et al.* (2019) receive subsidized credit if they have interest expenses scaled by total debt below the median interest rate paid by all public AAA-rated firms; the speculative-grade rating is derived from the three-year median ICR with a 2.5 cutoff. In our implementation of the credit-subsidy definition of zombie firms, we use the following approaches. In the Y-14 data, we employ the ratings assigned to firms by their lenders and use average AAA-rated firms' interest rates on outstanding loans as thresholds for the credit-subsidy (regardless of private/public firm status). In Compustat, we follow the implementation in Acharya *et al.* (2022) for U.S. publicly-listed firms of the zombie firm definition of Acharya *et al.* (2019). To compute the credit-subsidy thresholds, we use credit ratings data from S&P, Moody's, and Fitch and calculate the median interest rate paid each year by firms rated least AA. Zombie firms are those with subsidized credit and a three-year average ICR below 2.5 (which implies a rating of BB or below).

subsidized rates from their lenders. A potential drawback of this second approach is that it is difficult to measure subsidized credit. Firms that borrow at low rates may face other lending standards that make overall borrowing less advantageous than what can be gauged by looking at loan rates only. The first definition therefore casts a wider net on the selection of firms that are candidates to acquire zombie status while allowing for the possibility that they receive additional, though not necessarily cheaper, bank credit. For simplicity, we refer to these approaches as the "balance-sheet" and "credit-subsidy" definitions of zombie firms.

Characteristics of Zombie Firms Panel A in Table A9 reports the share of zombie firms in Y-14. Regardless of the definition used, we estimate that the share of zombie firms varies between 2% and 6.5% per year during 2014–2019, suggesting a very low incidence of zombie firms in our sample of firms borrowing from large U.S. banks. These zombie shares are also significantly below those typically estimated in other countries (see, e.g., Acharya *et al.* (2022) and Banerjee and Hofmann (2022)). Only a handful of firms are classified as zombies based on both definitions. Panel B of Table A9 shows that balance-sheet zombie firms are equally likely to be detected among small- and medium-sized firms. There are even instances of zombie firms among the largest firms in the sample.

Empirical Specification To test whether the results established in the previous section hold outside the oil shock episode, we use the full sample of Y-14 data over the 2014–2019 period.[14] We regress bank lending outcomes on zombie firm status with data at the bank-firm-quarter level using the following linear regression model:

[14] The estimation sample starts in 2014 because one of the two definitions of zombie firms discussed above requires three consecutive years of sales growth data (2012–2014) to classify firms as unprofitable. The sample ends in 2019 to deliberately exclude the COVID-19 shock from the analysis. The pandemic-induced recession triggered an unprecedented policy response that likely influenced bank lending to all firms, potentially confounding the main drivers of zombie lending.

$$Lending\ outcome_{bit} = \sum_{\tau=1,2} \beta_\tau Zombie_{it} \times Bank\ Type_{b,\tau} + \\ + \lambda^x \times X_{it} + \gamma_{jt} + \eta_{st} + \delta_{bt} + \psi_{bi} + \epsilon_{bit}, \tag{3}$$

where the lending outcomes of interest are (log of) loan amounts and interest rates on loans from bank b to firm i in quarter t. $Bank\ Type_{b,\tau}$ refers to either *low-capital high-exposure* banks ($\tau = 1$) or *other* banks ($\tau = 2$). The dummy variable $Zombie_{it}$ takes value one for zombie firms and zero otherwise. As in Section 3.1, the regression model controls for firm characteristics X_{it} (size, cash holdings, and tangibility) and includes state×quarter and industry×quarter fixed effects (γ_{jt} and η_{st}), bank×quarter fixed effects (δ_{bt}), and firm×bank fixed effects (ψ_{bi}).

The coefficient of interest, β_1, is identified under the assumption that zombie status is exogenous to other firm characteristics or past bank lending decisions, or other factors, that might also influence current bank lending decisions. Thus, an important caveat is that firms' transition to zombie status may be nonrandom, unlike in the oil shock analysis. Therefore, the findings in this section should be interpreted as generalizing those of the previous section and not as establishing causal evidence of zombie lending at U.S. banks.

Results Panel A of Table 4 reports coefficient estimates of the regression model in equation (3). Estimates in columns 1–2 show that zombie candidate firms identified based on the balance-sheet definition receive, on average, statistically significantly smaller (by 6.7%) and more expensive loans (by 16 bps) than other firms. Columns 3–4 show that both *low-capital high-exposure* and other banks tighten the terms of lending to zombie candidate firms (with effects that are not statistically significantly different from each other). Columns 5–6 present similar findings when zombie firms are selected using the subsidized credit requirement.

These results confirm those reported in the oil shock analysis and suggest that, regardless of the approach used to identify zombie firms, the banks in our data neither increase their exposure to zombie firms nor offer lower rates to such firms over the 2014–2019 period. The evidence, instead, supports the view that banks lend to zombie firms at risk-adjusted terms.

4 Zombie Lending by Nonbanks

Having shown that zombie lending is not a pervasive practice at U.S. banks, we turn to studying the lending policies of nonbanks. We ask if NBFIs tighten lending standards when firms become zombie candidates, as banks do. Or, whether they offset banks' reduced exposure to zombie firms by offering more and possibly cheaper loans. Evidence in support of zombie lending by NBFIs would point to the possibility of credit migration from banks to NBFIs (Irani *et al.*, 2021; Bednarek *et al.*, 2023). We start by documenting differential lending behaviors at nonbanks compared to banks. We then study the relation between bank capital and NBFIs' participation in syndicated loans.

4.1 Oil Price Shock Analysis

Main Results We take advantage of a unique feature of the DealScan dataset, which offers detailed information on the syndicate composition of individual loans, to explore differences in lending policies to zombie candidate firms between banks and NBFIs.[15] Similar to the baseline analysis in Section 3.1, we exploit the decline in the global price of crude oil in 2014 and 2015 as a quasi-random shock to firms' profitability. Using a sample of syndicated loans granted by more than 850 lenders (banks and nonbanks in roughly equal proportions) during 2012–2017, we estimate a version of equation (2) in which the triple DiD term $Distressed_i \times Oil\ Sector_i \times Post_t$ is interacted with a dummy for nonbank lenders:

[15] Section A-I provides detailed information on the approach used to classify lenders into banks and NBFIs.

$$
\begin{aligned}
\textit{Lending outcome}_{lit} = {} & \beta_1(\textit{Distressed}_i \times \textit{Oil Sector}_i \times \textit{Post}_t) \times \textit{Nonbank lender}_l \\
& + \beta_2(\textit{Distressed}_i \times \textit{Oil Sector}_i \times \textit{Post}_t) \\
& + \lambda^c \textit{Lower level controls} + (\lambda^x + \lambda^p \textit{Post}_t) \times X_{it} \\
& + \alpha_i + \gamma_{st} + \delta_{lt} + \epsilon_{bit}
\end{aligned} \tag{4}
$$

where the outcome variables are *New Loan* and *Loan Spread*. The variable $\textit{New Loan}_{lit}$ is a dummy that takes value one if a syndicated loan is originated by lender l to a firm i in a given year t and zero in any year when the firm is active but does not obtain a new loan (similar to the approach in Chodorow-Reich (2014)). In the loan-level data, *Loan Spread* is the (all-in drawn) spread of the loan rate over the reference rate (typically LIBOR). $\textit{Distressed}_i$ is a dummy variable for firms that are in financial distress before the oil shock (with debt-to-asset ratio above the sample median and ICR below one during 2013–2014). $\textit{Oil Sector}_i$ is a dummy variable taking value one for firms in the two-digit SIC industry "Oil and Gas Extraction." $\textit{Post}_t$ is an indicator variable that takes value 1 for the three-year period following the oil shock (2015–2017) and value zero in the three years before the shock (2012–2014).

The specification includes an extensive set of fixed effects at the firm (α_i), state×time (γ_{st}), and lender×time level (δ_{lt}); the latter fixed effects are particularly important as they absorb time-varying heterogeneity in lender characteristics, including balance sheet characteristics of nonbanks, for which we do not observe financial statements. We add the same firm controls X_{it} as in the preceding specifications. Importantly, because loan pricing terms are determined at the deal level (Ivashina, 2005), differential loan pricing effects for nonbanks are estimated off of the cross-deal variation in loan spreads. In this specification, *Nonbank lender* is a dummy taking value one for those loan deals that have at least one nonbank lender.

The coefficients of interest are β_1, which captures the differential effect of NBFIs on lending outcomes to ex-ante distressed oil firms, and β_2, which represents the baseline triple-

DiD effect of an oil firm's ex-ante distressed status on lending outcomes.

Estimation results are reported in Table 5. The first two columns show that NBFIs are significantly more likely than banks to grant new loans to distressed oil firms after the oil price shock. The point estimates in columns 1 and 2 indicate that zombie candidate firms are close to 8% more likely to receive a new loan from a NBFI than they are from a bank. Turning to loan pricing, and focusing on firms borrowing from banks, estimates in columns 3–4 suggest that zombie candidate firms face loan spreads that are on average 171-182 bps higher than non-zombie candidate firms (marginally statistically significant at 12.5%). By contrast, looking at firms that borrow from nonbanks, zombie candidate firms pay spreads that are on average 140 to 234 bps lower than firms that do not transition into zombie candidate status ($234 = 1.825 - 4.167$ in column 3 and $140 = 1.712 - 3.117$ in column 4).

Next, we study fluctuations in the share of nonbank lending in syndicated loan deals in response to firms' transition into zombie candidate status after the oil shock. We estimate the following specification at the loan deal level:

$$\begin{aligned} Nonbank\ share_{it} = {} & \beta_1(Distressed_i \times Oil\ Sector_i \times Post_t) \\ & + \lambda^c Lower\ level\ controls + (\lambda^x + \lambda^p Post_t) \times X_{it} \\ & + \alpha_i + \gamma_{st} + \epsilon_{it}, \end{aligned} \tag{5}$$

where the outcome variable is the fraction of a given syndicated loan deal i that is funded by nonbank participants in year t. We control for firm characteristics X_{it} (individually and interacted with $Post$), firm fixed effects (α_i), and state×year fixed effects (γ_{st}). The parameter of interest is β_1, with a positive coefficient indicating that distressed oil firms receive higher loan shares from nonbank participants in syndicated loan deals.

The estimation results, reported in Table 6, are consistent with those in Table 5 and suggest that, following the oil price shock, ex-ante distressed oil firms are granted relatively higher loan shares by nonbank participants compared to other firms. The estimates imply that exposed firms experience, on average, an increase in the share of NBFI funding in syndicated loan deals ranging between 31.7% and 33.4%.

The results in Tables 5–6 deliver two main takeaways. First, NBFIs, unlike regulated banks, increase their exposure to firms that transition into zombie candidate status, both on the extensive and intensive margins. Second, NBFIs charge significantly lower spreads than banks to zombie candidate firms.

External Validity We check the external validity of our oil shock analysis results in DealScan by extending the sample period to 2010–2019 and using the balance-sheet and credit-subsidy definitions of zombie firms introduced in Section 3.2. We focus the analysis on the period between 2010 and 2019 because it covers the post-GFC period of stricter bank capital regulation. Here, we estimate a version of the baseline specifications where the triple DiD term is replaced with a $Zombie_{it}$ dummy variable and its interaction with the nonbank lender dummy.

Estimation results in column 1 of Table 7 show that firms that transition into zombie candidate status identified using balance sheet data only are as likely to receive new loans from banks compared to other firms, but are more likely (by 2.0% ($\approx -0.009 + 0.029$)) to be granted new loans by nonbanks than firms that do not transition to zombie candidate status. Turning to the credit-subsidy definition of zombie firms (column 3), such firms are significantly less likely to receive a new loan from banks than non-zombie firms (by 5.8%), but about as likely to receive a loan from nonbanks. Loan pricing results in column 2 suggest that balance-sheet zombie candidate firms face higher loan spreads from banks (close to 40 bps) but lower spreads from NBFIs (about 20 bps $\approx 0.399 - 0.220$) compared to other firms.

Overall, these results corroborate our main findings in the oil shock analysis.

4.2 Credit Migration to Nonbanks

What it the mechanism that enables NBFIs to offer alternative source of funding to zombie firms? To study this question, we exploit within-loan deal variation in lending shares across syndicate participants and study the response of nonbanks' loan shares to firms' transitions into zombie candidate status. We use a specification that follows Irani *et al.* (2021) and allows the share of nonbank lending to zombie firms to vary (a) before and after 2014—the beginning of bank capital reforms under the Dodd-Frank Act—and (b) with the capitalization of participating banks *in the same syndicated loan deal*. We estimate the following regression in the DealScan sample spanning the 2010–2019 period:

$$\begin{aligned} Nonbank\ share_{it} = \beta_1 Zombie_{it} + \beta_2 Zombie_{it} \times Bank\ capital_{it} + \\ + \beta_3 Bank\ capital_{it} + \lambda^x X_{it} + \lambda^z Z_{it} + \psi_t + \epsilon_{it}, \end{aligned} \tag{6}$$

where the outcome variable is the fraction of the syndicated loan deal i funded by nonbank participants in year t, and $Bank\ capital_{it}$ is the average capital ratio across bank participants in the syndicate. The unit of observation is a loan deal. As there are insufficient syndicated loan deals to credit-subsidy zombie firms to estimate equation (6), we focus on the balance-sheet definition of zombie candidate firms. We control for the same firm characteristics X_{it} as in previous specifications and additionally control for average characteristics of the banks participating in the loan deal Z_{it} (size (log-assets), deposits as a share of nondeposit liabilities, loan-to-asset ratio, and net interest margins) and year fixed effects.[16]

The parameter of interest is β_2, with a negative coefficient indicating a negative correlation between the nonbank lending share to zombie firms and average capital ratios at participating banks in syndicated loan deals.

The estimation results, reported in Table 8, broadly support the hypothesis of zombie credit migration to NBFIs after 2014. Columns 1–2 show that syndicated loan deals to zombie firms exhibit, on average, significantly higher loan participation from nonbanks after the start of banking sector capital reforms in 2014 (by 6–8 bps). In addition, bank capital plays an important role: as seen in columns 3–4, estimates of β_2 are consistently negative and statistically significant. These estimates imply that while syndicates in which banks have higher capital ratios are more likely to see a higher share of nonbank lending, the positive

[16]Using the simple average or the assets-weighted average of these bank characteristics at the deal level has no material effect on the results.

effect of bank capital on the nonbank share is significantly smaller when the borrower is a zombie firm. In other words, decreases in the average capital ratio of a syndicate's banks are less likely to lead to decreases in the nonbank share in that syndicate when the borrower is a zombie firm. When the borrower is a zombie firm, a one percentage point reduction in the average bank capital ratio in a given deal is associated with about half a percentage point decrease in the fraction of that loan that is funded by nonbanks ($0.510 = 1.137 - 0.627$ in column 1 and $0.457 = 1.113 - 0.656$ in column 2), but with more than a one percentage point decrease in the nonbank share when the borrower is not a zombie firm.

Taken together, the evidence in this section suggests that NBFIs adopt less strict lending policies than banks toward firms that transition into zombie status and that NBFIs appear to step in to fill the gap from banks' pullback from such firms. This migration of credit from banks to unregulated lenders suggests that capital regulations may have led banks to pare back risk-taking, creating opportunities for nonbanks to increase market share by supplying credit to riskier borrowers.

5 Zombie Firms and Bankruptcy

Given the credit support of NBFIs to zombie firms, a natural question is whether these firms are artificially kept alive by these lenders and exit the market through bankruptcy at a slower pace than other firms. Absent insolvency frictions, bankruptcy should be the natural exit of firms that operate in a competitive setting and are no longer economically viable. At the same time, a bankruptcy system that enables lenders to quickly resolve firm insolvency should increase lenders' incentives to restructure or liquidate zombie firms instead of pursuing zombie lending.

We relate the likelihood of balance-sheet zombie firms entering an insolvency procedure—either liquidation or restructuring—with those of distressed yet viable firms. The outcome variable is a dummy variable that takes value one for firms that file for bankruptcy in either the first or second year after they acquire zombie status. We estimate the following linear probability model in a firm-year panel:

$$Bankruptcy_{i,t|t+1} = \beta_1 Zombie_{it} + \lambda^x X_{it} + \alpha_i + \gamma_j + \delta_t + \epsilon_{it}, \tag{7}$$

where $Bankruptcy_{i,t|t+1}$ is a dummy variable that takes value one for firms filing for bankruptcy in year t or $t+1$. A positive β_1 would indicate that zombie candidate firms are more likely to exit through bankruptcy. The specification includes the firm-level controls X_{it} from previous specifications (size, cash, and tangibility) and firm, industry, and year fixed effects. The estimation period is 2014–2019.[17]

Table 9 reports the results, focusing on distressed and zombie candidate firms based on the balance-sheet definition. Coefficient estimates in column 1 indicate a positive and statistically significant relationship between distressed status and the likelihood of exit via bankruptcy over a two-year horizon. In column 2 we repeat the specification for zombie candidate firms and find that such firms are more likely to file for bankruptcy by one percentage point relative to the average probability in the full sample (0.35%). In column 3, we test whether bankruptcy filing rates conditional on distressed status are more pronounced when those firms acquire zombie candidate status. We find that zombie firms are twice as likely to exit via bankruptcy as their distressed non-zombie counterparts—the likelihood of zombie firms filing for bankruptcy is 1.23 percentage points higher relative to non-distressed firms, compared to 0.58 for distressed non-zombie firms relative to non-distressed firms.

6 Conclusion

Using supervisory commercial lending data for large U.S. banks, coupled with quasi-random shocks to firm profitability, we document that zombie lending is not a pervasive feature of U.S. banks. That is, banks do not appear to keep financially distressed firms afloat with more

[17] As shown in Table A10, the results are robust to extending the sample period to 2014–2024 to allow firms transitioning into zombie candidate status in 2018 or 2019 to file for bankruptcy within two years of acquiring this status.

or cheaper credit when these firms become economically unviable and transition into zombie candidate status. We also find that U.S. banks, irrespective of capital levels and exposure to zombie firms, offer stricter lending terms to these firms. By contrast, syndicated loan data reveals that lending by nonbanks to these firms offsets the reduction in lending from banks. Unlike regulated banks, nonbanks grow their exposure to zombie firms and offer them more advantageous terms. However, despite support from nonbank lenders, zombie candidate firms file for bankruptcy at a higher rate than other financially distressed firms.

Overall, our findings provide several novel perspectives to the zombie lending literature. First, our results contrast with the evidence from other countries that capital position influences banks' incentives to lend to zombie firms to avoid loan losses and regulatory scrutiny. Our study shows that such incentives are not at work in the U.S. banking system. Second, our findings suggest that the unregulated environment in which NBFIs operate generates zombie lending incentives. Finally, our analysis suggests that the efficient resolution of financial distress via bankruptcy in the Unites States may contribute to weaken financial intermediaries' incentives to keep insolvent firms alive.

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Table 1: Y-14 Analysis: Selected Descriptive Statistics

This table reports summary statistics of the main variables used in the baseline Y-14 analysis over 2012–2017. The summary statistics are for the loan-level data, aggregated at the firm-bank-quarter level (for firms with multiple outstanding loans in any given quarter from a given bank, we calculate weighted-average interest rates using the relative shares of outstanding loan amounts as weights). Interest rate data is available for all loans other than fully undrawn credit lines. Distressed is a dummy that takes value one for firms in distressed status (with ICR below one and above-median leverage) during 2013–2014 and zero otherwise. Oil sector is a dummy that takes value one for firms in the oil sector, as defined in Section 3.1. Low-capital, high-exposure bank is a dummy for banks with above-median lending exposure to the firm (relative to bank equity) and with post-stress CET1 capital ratio in the bottom quartile before the oil shock (both measured at end-2014). Section 2 describes the data sources in detail.

	(1) N	(2) Mean	(3) SD	(4) p10	(5) p50	(6) p90
Loan commitment amount ($mn)	879503	33.9	86.3	1.5	12.5	79.2
Interest rate (%)	677503	2.8	1.4	1.4	2.7	4.4
Firm is distressed	879503	12%	32%	0.0	0.0	1.0
Firm is in oil sector	879503	7%	25%	0.0	0.0	0.0
Low-capital high-exposure bank	797918	16%	36%	0.0	0.0	1.0
High-exposure bank	798663	53%	50%	0.0	1.0	1.0
Low-capital bank	874838	33%	47%	0.0	0.0	1.0
Firm size (log-assets)	879503	18.6	2.8	15.5	18.2	22.5
Firm cash holdings (cash/assets)	879503	10.0	13.2	0.1	5.0	26.2
Firm tangibility	879503	85.0	22.3	47.4	97.4	100.0
Firm leverage (debt/assets)	871419	34%	27%	2%	30%	67%
Firm has ICR<1	720646	16%	36%	0.0	0.0	1.0
Firm real sales growth	865417	10%	36%	-11%	5%	31%

Table 2: Oil Shock Analysis in Y-14: Bank Lending to Distressed Firms

The table shows coefficient estimates from OLS regressions that link firms' distressed status to bank lending outcomes after the oil shock in Y-14. The outcome variables are loan amount (log) and interest rate (available for all loans other than fully undrawn credit lines). The data are at the firm-bank-quarter level during 2012–2017. Post is a dummy that takes value 1 for 2015–2017 and 0 for 2012–2014. Distressed is a dummy that takes value one for firms in distressed status (with ICR below one and above-median leverage) during 2013–2014 and zero otherwise. Oil sector is a dummy that takes value one for firms in the oil sector, as defined in Section 3.1. In panel (A) we estimate the baseline DID effect of the oil price shock on bank lending outcomes to ex-ante distressed firms. In panel (B) we break down the triple DID coefficient by banks with low capital and high exposure ("Low-Cap High-Exp") versus other banks. "Low-Cap High-Exp" is a dummy for banks with above-median lending exposure to the firm (relative to bank equity) and with post-stress CET1 capital ratio in the bottom quartile before the oil shock (both measured at end-2014). Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets) in levels and interacted with Post. Industry FE are based on 2-digit NAICS classification. Lower level terms for the interactions are included but coefficients are not shown (full specifications showing all controls are reported in Table A2). Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)
	Loan amount	**Loan rate**
	(A) Baseline	
Distressed×Oil Sector×Post	-0.134***	0.238**
	(0.046)	(0.094)
Distressed×Post	-0.057***	0.023
	(0.017)	(0.024)
Observations	879,503	675,399
R^2	0.950	0.825
	(B) By Bank Status	
Distressed×Oil Sector×Post×Low Cap–High Exp Bank [1]	-0.121**	0.323***
	(0.049)	(0.091)
Distressed×Oil Sector×Post×Other Bank [2]	-0.136**	0.212*
	(0.050)	(0.112)
Observations	797,870	611,204
R^2	0.950	0.822
pvalue ttest H_0: [1] = [2]	0.737	0.259
Firm controls	Y	Y
Firm controls×Post	Y	Y
Industry×Quarter FE	Y	Y
State×Quarter FE	Y	Y
Bank×Quarter FE	Y	Y
Bank×Firm FE	Y	Y

Table 3: Oil Shock Analysis in Y-14: Bank Capital vs. Exposure to Distressed Firms

The table shows coefficient estimates from OLS regressions that link firms' distressed status to bank lending outcomes after the oil shock in Y-14 and break down the coefficients by bank exposure to the firm and bank capital. The outcome variables are loan amount (log) and loan interest rate. The data are at the firm-bank-quarter level during 2012–2017. "Post" is a dummy that takes value 1 for 2015–2017 and 0 for 2012–2014. Distressed is a dummy that takes value one for firms in distressed status (with ICR below one and above-median leverage) during 2013–2014 and zero otherwise. Oil sector is a dummy that takes value one for firms in the oil sector, as defined in Section 3.1. "High exposure" is a dummy that takes value one for banks with above-median exposure to the firm (relative to bank equity) and "Low capital" is a dummy for banks with capital ratio in the bottom quartile before the oil shock (both measured at end-2014). Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets) in levels and interacted with Post. Industry FE are based on 2-digit NAICS classification. Lower level terms for the interactions are included but coefficients are not shown. Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)
	Loan amount	**Loan rate**
	(A) By Bank Exposure	
Distressed×Oil sector×Post×High exposure [1]	–0.114**	0.228**
	(0.052)	(0.086)
Distressed×Oil sector×Post×Low exposure [2]	-0.189**	0.144
	(0.089)	(0.442)
Observations	797,784	611,165
R^2	0.950	0.822
pvalue ttest H_0: [1] = [2]	0.455	0.843
	(B) By Bank Capital	
Distressed×Oil sector×Post×Low capital [1]	-0.214***	0.349***
	(0.045)	(0.106)
Distressed×Oil sector×Post×High capital [2]	-0.082*	0.174*
	(0.044)	(0.091)
Observations	682,655	521,942
R^2	0.957	0.840
pvalue ttest H_0: [1] = [2]	0.00559	0.0719
Firm controls	Y	Y
Firm controls×Post	Y	Y
Industry×Quarter FE	Y	Y
State×Quarter FE	Y	Y
Bank×Quarter FE	Y	Y
Bank×Firm FE	Y	Y

Table 4: External Validity in Y-14 With Alternative Zombie Firm Definitions

The table shows coefficient estimates from OLS regressions that link firms' zombie status to bank lending outcomes in the full Y-14 sample and break down the coefficients by bank capital and bank exposure to the firm. Columns 1–4 refer to balance-sheet definition and columns 5–6 refer to the credit-subsidy definition of zombie firms (see Section 3.2). The outcome variables are loan amount (log) and loan interest rate. In columns 5–6, estimates on interest rates are omitted because the credit-subsidy definition uses low interest rates on bank debt as a criterion to identify zombie firms. The data are at the firm-bank-quarter level during 2014–2019 (the Y-14 data start in 2012, therefore the first observation of zombie candidate firms based on the balance-sheet definition is 2014). "Low-Cap High-Exp" is a dummy for banks with above-median lending exposure to the firm (relative to bank equity) and with post-stress CET1 capital ratio in the bottom quartile before the oil shock (both measured at end-2014). Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets) in levels and interacted with Post. Industry FE are based on 2-digit NAICS classification. Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Amount	**Loan Rate**	**Loan Amount**	**Loan Rate**	**Loan Amount**	**Loan Amount**
	(A) Balance-sheet definition				**(B) Credit-subsidy definition**	
Zombie	-0.067***	0.155***			-0.071***	
	(0.009)	(0.019)			(0.010)	
Zombie×Low Cap–High Exp Bank [1]			-0.089***	0.166***		-0.084***
			(0.025)	(0.033)		(0.022)
Zombie×Other Bank [2]			-0.063***	0.152***		-0.069***
			(0.009)	(0.017)		(0.010)
pvalue ttest H_0: [1] = [2]			0.329	0.580		0.492
Observations	832,156	625,374	832,156	625,374	1,003,084	1,003,084
R^2	0.955	0.865	0.955	0.865	0.956	0.956
Firm controls	Y	Y	Y	Y	Y	Y
State×Quarter FE	Y	Y	Y	Y	Y	Y
Industry×Quarter FE	Y	Y	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y	Y	Y

Table 5: Oil Shock Analysis in DealScan: Nonbank Lending to Distressed Firms

The table shows coefficient estimates from OLS regressions that link firms' distressed status to bank lending outcomes after the oil shock in DealScan. The outcome variables are a dummy variable for new loans ("New loan" is a dummy that takes value 1 in the year when a new loan is identified for all bank-firm pairs in the data set and 0 otherwise) and the loan (all-in drawn) spread over the reference rate. The data are at the firm-bank-year level in columns 1–2 and at the loan-tranche level in columns 3–4 over 2012–2017. "Post" is a dummy that takes value 1 for 2015–2017 and 0 for 2012–2014. "Distressed" is a dummy that takes value 1 for firms in distressed status (with ICR below one and above-median leverage) during 2013–2014. "Oil sector" is a dummy that takes value one for firms in the oil sector, as defined in Section 4.1. Loan controls in columns 3–4 include syndicate size (the number of syndicate participants, and indicator variables for term loans, lead banks, and refinancings. Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets). Lower level terms are included but coefficients are not shown. Standard errors are triple clustered at the firm, bank, and time level. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
Dependent variables:	**New Loan**		**Loan Spread (percent)**	
Distressed × Oil sector × Post × Nonbank	0.079**	0.078**	-4.167*	-3.117*
	(0.022)	(0.024)	(2.019)	(1.808)
Distressed × Oil sector × Post	-0.010	-0.009	1.825	1.712
	(0.013)	(0.013)	(1.091)	(1.065)
Observations	139,150	139,150	17,667	17,667
R^2	0.220	0.220	0.849	0.849
Firm controls	Y	Y	Y	Y
Firm controls × Post		Y		Y
Loan controls	-	-	Y	Y
Firm FE	Y	Y	Y	Y
State × Time FE	Y	Y	Y	Y
Lender × Time FE	Y	Y	Y	Y

Table 6: Oil Shock Analysis in DealScan: Nonbank Lending to Distressed Firms Across Syndicated Loan Deals

The table shows coefficient estimates from OLS regressions that link the share of NBFI lending in syndicated loan deals to firms' distressed status after the oil shock in DealScan. The outcome variable is the share of nonbank lending in a given syndicated loan deal. The data are at the loan-deal level during 2012–2017. "Post" is a dummy that takes value 1 for 2015–2017 and 0 for 2012–2014. "Distressed" is a dummy that takes value 1 for firms in distressed status (with ICR below one and above-median leverage) during 2013–2014. "Oil sector" is a dummy that takes value one for firms in the oil sector, as defined in Section 4.1. Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets). Lower level terms are included but coefficients are not shown. Standard errors are double clustered at the industry and year level. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)
Dependent variable:	**Share of nonbank lending**	
Distressed × Oil sector × Post	0.317***	0.334**
	(0.110)	(0.131)
Distressed × Post	0.064	0.061
	(0.085)	(0.081)
Observations	1,195	1,195
R^2	0.622	0.624
Firm controls	Y	Y
Firm controls × Post		Y
Firm FE	Y	Y
State × Year FE	Y	Y

Table 7: External Validity in DealScan With Alternative Zombie Firm Definitions

The table shows coefficient estimates from OLS regressions that link firms' zombie status to bank lending outcomes in the full DealScan sample and interact the "Zombie" dummy with nonbank lender dummies. Columns 1–2 use the balance-sheet definition and column 3 uses the credit-subsidy definition of zombie firms (see Section 3.2). Estimates on interest rates are omitted for the credit-subsidy definition of zombie firms because this definition uses low interest rates on bank debt as a criterion to identify zombie firms. The outcome variables are "New Loan" (a dummy variable for whether a given borrower signs a new loan in a given year with a given bank) and the loan (all-in drawn) spread over the reference rate. The sample period is 2010–2019. Loan controls include syndicate size (the number of syndicate participants) and indicator variables for term loans, lead banks, and refinancings. Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets). Industry is based on 2-digit SIC classification. Standard errors are triple clustered at the firm, lender, and quarter level. *** p<0.01, ** p<0.05, * p<0.1.

Dependent variables:	(1) **New Loan**	(2) **Loan Spread (percent)**	(3) **New Loan**
	(A) Balance-sheet definition		**(B) Credit-subsidy definition**
Zombie	-0.009	0.399**	-0.058**
	(0.006)	(0.183)	(0.023)
Zombie × Nonbank Lender	0.029***	-0.220*	0.059***
	(0.008)	(0.127)	(0.016)
Observations	217,918	26,669	225,655
R^2	0.193	0.827	0.186
Firm controls	Y	Y	Y
Loan controls	-	Y	-
Firm FE	Y	Y	Y
Industry × Year FE	Y	Y	Y
State × Year FE	Y	Y	Y
Lender × Year FE	Y	Y	Y

Table 8: DealScan Analysis: Nonbank Lending to Zombie Firms and Bank Capital Across Syndicated Loan Deals

The table shows coefficient estimates from OLS regressions that link the share of nonbank lending in syndicated loan deals to firms' zombie status (based on the balance-sheet definition) and average capitalization of bank participants. The data are at the loan-deal level over 2010–2019. The outcome variable is the share of nonbank lending in a given syndicated loan deal. "Post-2014" takes value 1 for 2014–2019 and 0 for 2010–2013. The bank capital ratio is computed as the average ratio of Tier1 capital divided by total assets across bank participants in the syndicate. Bank controls include bank size (log-assets), deposits as a share of nondeposit liabilities, loan-to-asset ratio, and net interest margins (all computed as equally-weighted averages across bank participants in the syndicate). Firm controls include firm size (log-assets), cash holdings (cash/assets) and tangibility (tangible assets/assets) and loan controls refer to loan maturity. Standard errors are double-clustered at the firm and quarter level. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
Dependent variable:	**Share of nonbank lending**			
Zombie × Post-2014	0.064*	0.082**		
	(0.034)	(0.037)		
Zombie × Capital ratio			-0.627*	-0.656**
			(0.311)	(0.299)
Bank capital ratio			1.137**	1.113**
			(0.482)	(0.472)
Zombie	-0.017	-0.034	0.106**	0.108**
	(0.017)	(0.021)	(0.051)	(0.052)
Observations	1,415	1,411	1,415	1,411
R^2	0.075	0.080	0.095	0.099
Quarter FE	Y	Y	Y	Y
Loan controls	Y	Y	Y	Y
Bank controls	Y	Y	Y	Y
Firm controls		Y		Y

Table 9: Zombie Firm' Exit Through Bankruptcy

The table shows OLS coefficient estimates from regressions that link firms' zombie candidate (or distressed) status to the probability of filing for bankruptcy (chapter 7 or chapter 11). The dependent variable is a dummy for firms that file for bankruptcy in the first or second year of distressed or zombie candidate status. The data are at the firm-year level over 2014–2019. Results in columns 1 and 2 are based on the balance-sheet definition zombie firms (see Section 3.2). The variable "Distressed" in columns 1 and 3 takes value one for the firms with ICR below one and above-median leverage (see Section 3.1). Firm controls include firm size (log-assets), cash holdings (cash/assets), and tangibility (tangible assets/assets). Industry FE are based on 2-digit NAICS classification. Two-year bankruptcy rate in the regression sample is 0.35%. Standard errors are double-clustered at the firm and year levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
	Firm Files for Bankruptcy in First or Second Year of Zombie Candidate Status		
Distressed	0.0086**		
	(0.003)		
Zombie		0.0106*	
		(0.004)	
Distressed × Zombie [1]			0.0123**
			(0.005)
Distressed × Non-zombie [2]			0.0058**
			(0.002)
pvalue t-test Ha: |1| > |2|			0.079
Observations	64,154	64,154	64,154
R^2	0.508	0.508	0.508
Firm controls	Y	Y	Y
Year FE	Y	Y	Y
Firm FE	Y	Y	Y
Industry FE	Y	Y	Y

Internet Appendix for "Zombie Lending to U.S. Firms"

A-I Data Appendix: DealScan

Initial Data Processing We start with the DealScan masterfile (downloaded from WRDS on December 18, 2022) with information on more than 300,000 loan tranches (tranches) organized in 240,000 loan deals extended since 1980. Data before the early 1990s is very sparse, therefore we follow Chava and Roberts (2008) and start the sample period in 1994. We end the sample period in 2019, consistent with the Y-14 analysis.

We apply several filters to the data. Following Elliott *et al.* (2024) and Roberts (2015), we remove loan amendments as those reflect renegotiations of loans over time as opposed to new loan originations. Thus, we retain only those loan observations that correspond to purely new credit, representing about 75% of all observations. We drop any loan deals and tranches with negative or zero amounts, we keep "closed" deals (we drop cancelled, in-process, no further info, on-hold and pre-mandate deals), we replace negative loan spreads with missing, and we drop loans in foreign currencies. In addition, we use the variable "lead role" to construct a lead bank dummy which takes value 1 for those banks with the following main syndicate roles: book-runner, administrative agent, agent, lead arranger, lead manager, manager, mandated lead arranger, mandated arranger, syndication agent, and sole lender.

We also construct dummies for term loans (versus credit lines). Term loans are identified as those loans classified according to the "tranche type" variable as term loans (types A-K), bridge loans, delayed draw term loans, demand loans, other loans, and a minority of other types of loans (such as loan style floating rate notes, loan-style floating rate CDs, Sukuks, and Wakalas). The majority of credit lines are 364-day tranches, revolvers with a maturity of either less or more than one year, and other loan types such as revolver/term loans, standby letters of credit, leases, and guarantees. A minority of other types of loans such as trade letters of credit, mezzanine tranches, limited lines, export credit, and acquisition/advance/bills/bonding/CAPEX/construction tranches are also classified as credit lines.

Classifying Lenders as Nonbanks The starting point for separating lenders into banks versus nonbanks is the variable "lender institution type." This string variable identifies lender type for the lenders in each syndicate. Lender types include the following categories:

- **Banks**: Investment bank, U.S. bank, Western European bank, European bank, East. Europe or Russian Bank, Canadian bank, African bank, Asia-Pacific bank, Middle Eastern Bank, Latin America & Caribbean bank, Australian/New Zealand bank, Japanese bank, Multinational investment bank, Multinational investment bank & Financial services, Mortgage bank, Farm credit bank, Thrifts and S&L associations, Export-import bank, Credit union, Government sponsored lender, and Foreign bank;

- **Nonbanks**: Finance Company, Corporation, Asset manager, CDO, Business development company, Insurance company, Specialty Lender, Development Finance Institution, Diversified financial services company, Hedge fund, Diversified (vulture) fund, Institutional Investor (CDO, Hedge fund, Insurance company, Prime fund, and Other), Business corporation, Law firm, Leasing company, Mutual fund, Pension fund, Private equity, and Trust company.

The assignment of lenders to the bank/nonbank category is straightforward for single-lender loans and for loans in which all lenders are banks. In the case of loan deals with at least one nonbank lender, it is not possible to carry out an automatic assignment because there is no one-to-one mapping between the syndicate lenders and the types listed in "lender institution type" variable. In these cases, we extract the lenders and assign them a bank/nonbank classification as follows. First, all lenders that have bank-related word stems in the name (such as bank, banc, banque, banco, banca, bancorp, banken, or bankia), all thrifts and S&L associations, credit cooperatives, and financial institutions that primarily provide farm credit, are classified as banks. We drop international financial institutions such as the World Bank (International Finance Corporation).

Second, we classify as nonbanks those lenders with stems in the name such as financing, financial, business, credit, investor, investment, insurance, capital, advisors, asset, management, income, trust, prime, senior, secured, debt, leveraged, lease, leasing, loanco, floating, services, high yield, insurance, infrastructure, opportunities, funding trust, trustee, agency, equity, guarantee, securities, funding, and fund. Lastly, we carefully inspect all the nonbanks identified with this approach and make manual corrections or additional classifications for unassigned lenders based on web searches and research of individual lenders. This approach yields a share of nonbank lenders across loan tranches over the sample period 2010–2019 of 5.5% (as reported in Table A1), which is comparable to the 5.7% share reported by Elliott *et al.* (2024) for the 1990–2019 period.

Matching Borrowers to Compustat After classifying all lenders as banks and nonbanks, we keep only U.S. borrowers and lenders as reported in the lender and borrower country variables. Then, as described in Section 2, we link the borrowers in DealScan (using the borrower name or the borrower parent's name) with their balance sheet characteristics using the DealScan-Compustat Linking Database from Chava and Roberts (2008) (dated

August 31, 2012), which contains matches through 2017. In Compustat, we focus on nonfinancial firms by excluding utilities (SIC codes 4900–4999), financial firms (SIC codes 6000-6999), and public administration firms (SIC codes 9000-9999). Zombie status is assigned to firms using data on ICR, leverage, and sales growth on a four-quarter rolling window.

Matching Borrowers to Call Report To obtain bank balance sheet data from the U.S. Call Report, we follow Dell'Ariccia *et al.* (2018) and clean and uniformize the names of bank lenders in DealScan and the Call Report and conduct a string match on bank name. We inspect the matches manually to resolve uncertain and multiple matches using additional information on city-state location, web searches, and the NIC National Information Center (link). We augment this initial match with the cross-walk linking DealScan lenders with RSSD IDs from Keil (2018) (available on link) and covering the period between 1982 and 2016. We are able to match 468 banks in DealScan with RSSD IDs in the Call Report over the 2010–2019 sample period.

For the analysis in Tables 6 and 8, we use the loan share that is contributed by individual lenders (available in the "lender share" variable). We use the loan shares as reported in DealScan when available. When the loan share is missing, we follow Ivashina and Scharfstein (2010) and focus on the loan contributions of lead banks. When a loan has more than one lead bank, we assume each lead bank extends the loan pro rata as in Giannetti and Laeven (2012a,b). For single-lender loans, 100% of the loan amount is assigned to the lender.

Final Sample Characteristics Over the sample period 2010–2019, the final DealScan sample with Compustat-matched borrowers has 10,606 loan deals (structured in 16,741 loan tranches) extended by 1,040 lenders to 4,924 borrowers identified by GVKEY ID. In the deal-level analyses, the regression sample of approximately 1,415 loan deals is significantly smaller than the starting sample because it fulfills additional requirements for the construction of the nonbank loan share and participating banks' average capital positions. These requirements are as follows: (a) loan deals must be syndicated as opposed to single-lender loans (which results in dropping about 20% of loans), (b) loan deals must include at least one nonbank lender (which results in dropping an additional 77% of loans); and (c) participating banks are matched to Call Reports.

A-II Additional Figures and Tables

Figure A1: Oil Price, 2012–2019

Notes: The figure plots the Spot Crude Oil Price West Texas Intermediate (WTI) (FRED series WTISPLC retrieved on December 13, 2021) on a monthly basis during 2012–2019. Source: Federal Reserve Bank of St. Louis.

Figure A2: Increase in Bank Capital, 2013–2017

Notes: The figure plots the 2013 and 2017 BHC-level regulatory CET1 capital ratios for the banks in the regression sample (column 1 of Table 2) along with the 45-degree line. Source: Consolidated Financial Statements for Holding Companies, FR Y-9C.

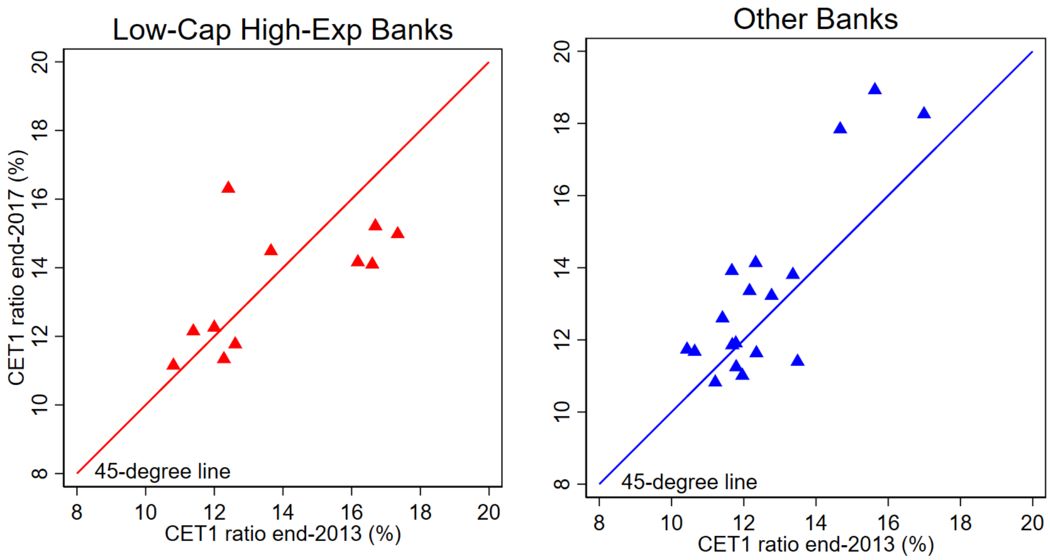

Table A1: DealScan Analysis: Selected Descriptive Statistics

This table reports summary statistics of the main variables used in the DealScan analysis over the sample period 2010–2019 for regressions at the loan tranche level (panel A) and deal level (panel B), respectively. Section 2 describes the data sources in detail and Section A-I provides information on the construction of the DealScan samples.

	(1)	(2)	(3)	(4)	(5)	(6)
	(A) DealScan tranche-level analysis 2010–2019					
	N	Mean	SD	p10	p50	p90
New loan	683000	11%	31%	0.0	0.0	1.0
Loan (all-in drawn) spread (percent)	26669	2.27	1.33	1.13	2.00	4.00
Nonbank lender	26669	5.5%	22.8%	0.0	0.0	0.0
Firm size ($bn)	26669	13.42	35.57	0.54	3.39	26.12
Firm size (log-assets)	26669	8.19	1.60	6.29	8.13	10.17
Firm liquidity (cash/assets)	26669	9%	10%	1%	6%	20%
Firm tangibility	26669	26%	23%	4%	18%	65%
Term loan	26669	40%	49%	0%	0%	100%
Syndicate size (# participants)	26669	14	9	4	11	27
Lead bank	26669	32%	47%	0%	0%	100%
Deal is refinancing	26669	67%	47%	0%	100%	100%
	(B) DealScan deal-level analysis 2010–2019					
	N	Mean	SD	p10	p50	p90
Nonbank loan share	1415	3%	16%	0.0	0.0	0.0
Zombie (balance-sheet definition)	1415	6.5%	24.7%	0.0	0.0	0.0
Bank capital ratio (Tier 1 capital/Asset)	1415	11%	5%	8%	9%	21%
Firm size (log-assets)	1415	8.2	1.8	6.0	8.2	10.6
Firm liquidity (cash/assets)	1413	10%	11%	1%	6%	22%
Firm tangibility	1411	27%	24%	4%	18%	66%
Bank size (log-assets)	1415	19.6	2.4	15.4	20.6	21.3
Bank deposits/liabilities	1415	68%	13%	58%	64%	93%
Bank loan/asset ratio	1415	56%	10%	44%	54%	72%
Bank net interest margins	1415	1%	1%	1%	1%	3%
Loan maturity (years)	1415	4.2	1.8	1.0	5.0	5.0

Table A2: Oil Shock Analysis: Bank Lending to Distressed Firms—Full set of controls

This table replicates baseline Table 2 and reports the estimates coefficients for lower level terms and additional controls. p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
Dependent variables:	**Loan Amount**	**Loan Rate**	**Loan Amount**	**Loan Rate**
Distressed×Oil Sector×Post	-0.134***	0.238**		
	(0.046)	(0.094)		
Distressed×Oil Sector×Post×Low Cap–High Exp Bank [1]			-0.121**	0.323***
			(0.049)	(0.091)
Distressed×Oil Sector×Post×Other Bank [2]			-0.136**	0.212*
			(0.050)	(0.112)
Distressed×Post	-0.057***	0.023		
	(0.017)	(0.024)		
Distressed	0.018*	-0.040*		
	(0.010)	(0.022)		
Distressed×Oil Sector	-0.063**	0.067		
	(0.024)	(0.040)		
Oil Sector×Post	-0.081***	0.109***		
	(-0.023)	(0.036)		
Distressed×Post×Zombie Bank			-0.063**	0.045
			(0.027)	(0.032)
Distressed×Post×Other Bank			-0.057***	0.011
			(0.015)	(0.020)
Oil Sector×Post×Zombie Bank			-0.079***	0.055
			(0.028)	(0.054)
Oil Sector×Post×Other Bank			-0.057**	0.067
			(0.026)	(0.039)
Distressed×Oil Sector×Zombie Bank			0.030	-0.079
			(0.041)	(0.088)
Distressed×Oil Sector×Other Bank			0.029	-0.052
			(0.030)	(0.051)
Distressed×Zombie Bank			0.132**	-0.173
			(0.054)	(0.149)
Distressed×Other Bank			0.018	-0.029
			(0.010)	(0.021)
Non-distressed×Zombie Bank			0.105*	-0.089
			(0.056)	(0.162)
Firm size (log-assets)	0.029***	-0.007	0.030***	-0.008
	(0.010)	(0.005)	(0.010)	(0.005)
Firm cash	-0.002***	-0.002*	-0.002***	-0.002*
	(0.000)	(0.001)	(0.000)	(0.001)
Firm tangibility	-0.001**	-0.002***	-0.001***	-0.002***
	(0.001)	(0.000)	(0.001)	(0.000)
Firm size×Post	0.019***	-0.009	0.018***	-0.009
	(0.003)	(0.007)	(0.003)	(0.007)
Firm cash×Post	-0.001***	-0.002**	-0.001***	-0.002**
	(0.000)	(0.001)	(0.000)	(0.001)
Firm tangibility×Post	0.000**	0.001	0.000**	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
pvalue ttest H_0: [1] = [2]			0.737	0.259
Observations	879,503	675,399	797,870	611,204
R^2	0.950	0.825	0.950	0.822
Firm controls	Y	Y	Y	Y
Firm controls×Post	Y	Y	Y	Y
State×Quarter FE	Y	Y	Y	Y
Industry×Quarter FE	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y

Table A3: Oil Shock Analysis: Finer Fixed Effects

This table shows that the baseline results in Table 2 are including more granular fixed effects to account for common shocks affecting all firms in a given industry, geography, and of size group. In panel (A) we include interacted state×industry×quarter FE; in panel (B) we include interacted state×industry×size group×quarter FE (where the size groups are defined based on total assets in the following groups: below $50mn, $50–$250mn, $250mn–$1000mn, $1000mn–$5000mn, and above $5000mn, following Chodorow-Reich *et al.* (2022)). The outcome variables are loan amount (log) and interest rate. The data are at the firm-bank-quarter level over 2012–2017. All variables as in baseline Table 2. Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	Loan Amount	**Loan Rate**	**Loan Amount**	**Loan Rate**
	(A) state×industry×quarter FE			
Distressed×Oil Sector×Post	-0.134***	0.226**		
	(0.045)	(0.089)		
Distressed×Oil Sector×Post×Other Bank [2]			-0.136**	0.196*
			(0.050)	(0.108)
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]			-0.123**	0.319***
			(0.049)	(0.089)
pvalue ttest H_0: [1] = [2]			0.771	0.211
Observations	877,482	673,126	795,726	608,869
R^2	0.952	0.834	0.952	0.831
Firm controls	Y	Y	Y	Y
Firm controls×Post	Y	Y	Y	Y
State×Industry×Quarter FE	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y
	(B) state×industry×size-group×quarter FE			
Distressed×Oil Sector×Post	-0.135***	0.234**		
	(0.043)	(0.091)		
Distressed×Oil Sector×Post×Other Bank [2]			-0.134**	0.198*
			(0.048)	(0.109)
Distressed×Oil Sector×Post× LowCap-HighExp Bank [1]			-0.132**	0.328***
			(0.048)	(0.091)
pvalue ttest H_0: [1] = [2]			0.959	0.206
Observations	864,730	661,520	783,592	597,707
R^2	0.956	0.844	0.956	0.842
Firm controls	Y	Y	Y	Y
Firm controls×Post	Y	Y	Y	Y
State×Industry×Size-Group×Quarter FE	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y

Table A4: Oil Shock Analysis: Credit Line Utilization of Distressed Firms

This table examines changes in credit line utilization rates of ex-ante distressed firms after the oil shock relative to other firms. The outcome variable is the credit line utilization rate, defined as the ratio between the utilized amount and the committed amount across a firms' outstanding bank credit lines. The data are at the firm-bank-quarter level (columns 1–2) or at the firm-quarter level (column 3) during 2012–2017. All variables as in baseline Table 2. Standard errors are double-clustered at the firm and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
	Credit line utilization rate		
	(A) Bank-firm-quarter		**(B) Firm-quarter**
Distressed×Oil Sector×Post	0.032		0.012
	(0.024)		(0.021)
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]		0.014	
		(0.037)	
Distressed×Oil Sector×Post ×Other Bank [2]		0.040	
		(0.024)	
pvalue ttest H_0: [1] = [2]		0.431	
Observations	699,218	640,492	506,003
R^2	0.765	0.761	0.743
Firm controls	Y	Y	Y
Firm controls×Post	Y	Y	Y
State×Quarter FE	Y	Y	Y
Industry×Quarter FE	Y	Y	Y
Firm FE	Y	Y	Y
Bank×Quarter FE	Y	Y	-
Bank×Firm FE	Y	Y	-

Table A5: Oil Shock Analysis: New Loan Originations

This table shows that the baseline results in Table 2 are robust to estimating the main specifications on the sample of new loans only. The outcome variables are loan amount (log) and interest rate. The data are at the firm-bank-quarter level over 2012–2017. All variables as in baseline Table 2. Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)
	Loan amount	**Loan rate**
	(A) Baseline	
Distressed×Oil Sector×Post	-0.239***	0.279**
	(0.068)	(0.105)
Distressed×Post	-0.021	-0.044
	(0.029)	(0.042)
Observations	77,203	58,879
R^2	0.952	0.853
	(B) By Bank Status	
Distressed×Oil Sector×Post×Low Cap–High Exp Bank [1]	-0.187**	0.410**
	(0.089)	(0.149)
Distressed×Oil Sector×Post×Other Bank [2]	-0.213**	0.281**
	(0.083)	(0.124)
pvalue ttest H_0: [1] = [2]	0.797	0.381
Observations	70,341	53,738
R^2	0.953	0.854
Firm controls	Y	Y
Firm controls×Post	Y	Y
State×Quarter FE	Y	Y
Industry×Quarter FE	Y	Y
Bank×Quarter FE	Y	Y
Bank×Firm FE	Y	Y

Table A6: Oil Shock Analysis: Alternative Clustering of Standard Errors

This table replicates panel A of baseline Table 2 (where standard errors are triple-clustered at the firm, bank and quarter levels) with alternative clustering approaches for the standard errors. In panel A, the standard errors are double clustered at the firm-bank and quarter levels. In panel B, the standard errors are double clustered at the firm and quarter level. All variables as in baseline Table 2. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	Loan Amount	**Loan Rate**	**Loan Amount**	**Loan Rate**
	(A) Double cluster on firm-bank and quarter			
Distressed×Oil Sector×Post	-0.134***	0.238***		
	(0.034)	(0.078)		
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]			-0.121**	0.323***
			(0.043)	(0.092)
Distressed×Oil Sector×Post×Other Bank [2]			-0.136***	0.212**
			(0.037)	(0.089)
pvalue ttest H_0: [1] = [2]			0.744	0.218
Observations	879,503	675,399	797,870	611,204
R^2	0.950	0.825	0.950	0.822
	(B) Double cluster on firm and quarter			
Distressed×Oil Sector×Post	-0.134***	0.238**		
	(0.046)	(0.094)		
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]			-0.121**	0.323***
			(0.055)	(0.106)
Distressed×Oil Sector×Post×Other Bank [2]			-0.136**	0.212**
			(0.049)	(0.102)
pvalue ttest H_0: [1] = [2]			0.762	0.206
Observations	879,503	675,399	797,870	611,204
R^2	0.950	0.825	0.950	0.822
Firm controls	Y	Y	Y	Y
Firm controls×Post	Y	Y	Y	Y
State×Quarter FE	Y	Y	Y	Y
Industry×Quarter FE	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y

Table A7: Oil Shock Analysis: Control for Pretrends

This table shows that the baseline results in Table 2 are robust to controlling for a firm-bank-specific linear pretrend. The pretrend control variable is the log-diff growth rate in loan amounts, the difference in loan rates, and in probabilities of default at the bank-firm level over the pre-oil shock period (2012–2014) multiplied by a linear time trend. The outcome variables are loan amount (log) and interest rate. The data are at the firm-bank-quarter level over 2012–2017. All variables as in baseline Table 2. Standard errors are triple-clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)	(4)
	Loan Amount	**Loan Rate**	**Loan Amount**	**Loan Rate**
Distressed×Oil Sector×Post	-0.101**	0.255***		
	(0.039)	(0.090)		
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]			-0.116***	0.295***
			(0.039)	(0.091)
Distressed×Oil Sector×Post×Other Bank [2]			-0.093**	0.245**
			(0.042)	(0.102)
Pretrend Control	0.028***	0.025***	0.027***	0.024***
	(0.005)	(0.006)	(0.005)	(0.006)
pvalue ttest H_0: [1] = [2]			0.482	0.563
Observations	844,375	554,562	797,269	524,739
R^2	0.955	0.843	0.955	0.841
Firm controls	Y	Y	Y	Y
Firm controls×Post	Y	Y	Y	Y
State×Quarter FE	Y	Y	Y	Y
Industry×Quarter FE	Y	Y	Y	Y
Bank×Quarter FE	Y	Y	Y	Y
Bank×Firm FE	Y	Y	Y	Y

Table A8: Oil Shock Analysis: Alternative Bank Capital Measure

This table shows that the baseline results in panel B of Tables 2 and 3 are robust to using BHC-level regulatory CET1 ratio as a measure of capital instead of post-stress CET1 capital. The BHC-level CET1 ratio is measured at end-2013. The outcome variables are loan amount (log) and interest rate. The data are at the firm-bank-quarter level over 2012–2017. All variables as in baseline Table 2. Standard errors are triple clustered at the firm, bank, and quarter levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)
	Loan Amount	**Loan Rate**
	(A) LowCap-HighExp Bank vs. Other Bank	
Distressed×Oil Sector×Post×LowCap-HighExp Bank [1]	-0.109**	0.247***
	(0.052)	(0.087)
Distressed×Oil Sector×Post×Other Bank [2]	-0.177**	0.176
	(0.068)	(0.237)
pvalue ttest H_0: [1] = [2]	0.373	0.742
Observations	797,784	611,165
R^2	0.950	0.822
	(B) Low vs. High Capital Bank	
Distressed×Oil Sector×Post×LowCap [1]	-0.123**	0.270**
	(0.047)	(0.099)
Distressed×Oil Sector×Post×HighCap [2]	-0.181**	-0.024
	(0.079)	(0.117)
pvalue ttest H_0: [1] = [2]	0.445	0.0248
Observations	797,784	611,165
R-squared	0.950	0.822
Firm controls	Y	Y
Firm controls×Post	Y	Y
State×Quarter FE	Y	Y
Industry×Quarter FE	Y	Y
Bank×Quarter FE	Y	Y
Bank×Firm FE	Y	Y

Table A9: Prevalence of U.S. Zombie Firms—Alternative Definitions

This table reports the share of zombie firms during 2014–2019 based on the balance-sheet and credit-subsidy definitions (panel A) and, for the balance-sheet definition, by size group using firm size cutoffs from Chodorow-Reich *et al.* (2022) (panel B). The two alternative definitions are discussed in Section 3.2. Column 3 reports the total number of firms with balance sheet information in the Y-14 sample.

	(1)	(2)	(3)	(4)
	(A) Share of zombies			
	Balance-sheet definition	**Credit-subsidy definition**	**# firms in Y-14 sample**	
2014	3.9%	3.3%	76216	
2015	4.3%	1.9%	77293	
2016	5.0%	2.7%	80830	
2017	5.8%	3.7%	80151	
2018	6.3%	4.4%	77693	
2019	6.5%	2.9%	72603	
	(B) Share of zombies by firm size Balance-sheet definition			
Assets ($mn)	**[0–50]**	**[50–250]**	**[50–1000]**	**[1000–]**
2014	4.2%	3.1%	4.0%	3.5%
2015	4.6%	3.7%	4.1%	3.9%
2016	5.3%	4.0%	5.5%	4.7%
2017	5.8%	5.6%	6.4%	6.4%
2018	6.3%	6.3%	6.3%	6.2%
2019	7.0%	5.6%	6.0%	5.8%

Table A10: Zombie Firm' Exit Through Bankruptcy—Extended Sample

The table shows that baseline results in Table 9 are robust to extending the sample period by an additional two years, to 2014–2021, to capture those firms in zombie candidate status in 2018 or 2019 that filed for bankruptcy in 2020 or 2021. It reports OLS coefficient estimates from regressions that link firms' zombie and distressed status to the probability of filing for bankruptcy. The dependent variable is a dummy for firms that file for bankruptcy in the first or second year of zombie candidate (or distressed) status. Firm controls include firm size (log-assets), cash holdings (cash/assets), and tangibility (tangible assets/assets). Industry FE are based on 2-digit NAICS classification. All variables as in Table 9. Standard errors are double-clustered at the firm and year levels. *** p<0.01, ** p<0.05, * p<0.1.

	(1)	(2)	(3)
	Firm Files for Bankruptcy in First or Second Year of Zombie Candidate Status		
Distressed	0.0082***		
	(0.002)		
Zombie		0.0098***	
		(0.003)	
Distressed × Zombie [1]			0.0114***
			(0.003)
Distressed × Non-zombie [2]			0.0055**
			(0.002)
pvalue t-test Ha: |1| > |2|			0.079
Observations	90,438	90,438	90,438
R^2	0.500	0.500	0.501
Firm controls	Y	Y	Y
Year FE	Y	Y	Y
Firm FE	Y	Y	Y
Industry FE	Y	Y	Y