Resolving Zombie Lending with Collateral Reform

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Abstract

Zombie lending, defined as lending to otherwise insolvent borrowers, misallocates resources and hinders economic growth. This paper exploits a 2002 collateral reform in India as a natural experiment to show that improving the process of resolving bad loans can reduce the share of credit and capital allocated to zombie borrowers. Post-reform credit to distressed borrowers contracts due to a decline in continued lending to zombie borrowers, which subsequently cut investment. Credit to healthy firms increases that then expand investment. Allocative efficiency improves by 18.7%, with 94% of the improvement attributable to credit reallocation by lenders from zombie to non-zombie borrowers.

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The flow of credit to unproductive firms misallocates resources and impairs economic growth. A prominent example of such misallocation of credit is "zombie lending" — that is, continued subsidized lending to otherwise insolvent firms — as seen in Japan in the 1990s (Caballero et al., 2008), more recently in Spain and Portugal (Gopinath et al., 2017; Blattner et al., 2019; Acharya et al., 2019), and in developing economies such as India (Rajan, 2018) and China (Li and Ponticelli, 2019). While the literature has extensively studied the causes of zombie lending, an emerging literature has focused on how to reduce zombie lending (Bonfim et al., 2018).

Poorly designed insolvency frameworks can exacerbate the zombie lending motives of banks and increase the share of capital sunk in zombie firms (McGowan et al., 2017). Improvements in creditor rights can reduce zombie lending. I examine whether improving creditor rights improves allocative efficiency of credit flows by cutting credit to weak (zombie) borrowers and expanding credit to more productive firms, enabling more productive capital allocation. Using the introduction of a 2002 collateral reform in India as a natural experiment, this paper uses firm-level data combined with a new dataset on collateralized loans to examine whether improved creditor rights can correct preexisting lending distortions in the economy.

In 2002, India introduced a collateral reform that allowed secured creditors to circumvent the lengthy judicial process and seize the collateral of defaulting firms. Judicial delays before the reform forced lenders to "evergreen" or extend loans at subsidized rates once the borrowers became distressed. This phenomenon, otherwise known as zombie lending, keeps credit flowing to unproductive borrowers at the expense of more productive firms. The principal analysis in this paper traces the impact of the collateral reform on these preexisting zombie borrowers. I produce three main findings. First, lenders cut secured credit to zombie borrowers and expand secured credit to healthy firms. Second, due to the credit redistribution, zombie borrowers cut capital spending and streamline their operations, whereas healthy non-zombie firms increase investment. Third, overall allocative efficiency and productivity improve due to the reallocation of credit and capital. This paper highlights an important mechanism of credit reallocation from zombie to non-zombie firms that improves allocative efficiency and overall productivity.

The first step of the analysis documents the distributive effect of the collateral reform on

credit. Secured credit of distressed firms declines post-reform, whereas secured credit to healthier firms increases. This decline in secured credit to distressed borrowers is attributable primarily to a contraction in zombie lending. Zombie borrowers are defined as distressed firms that borrow at subsidized interest rates, as in Caballero et al. (2008). I trace the impact of the reform on lending to firms that received zombie credit pre-reform. Credit to zombie borrowers declines by INR 31 million, whereas credit to non-zombie borrowers increases by INR 15 million. For identification, I follow Vig (2013) and split firms into treatment and control groups based on ex-ante collateralizability (asset-tangibility) and use a difference-in-difference-in-differences (DDD) specification that compares the zombie versus non-zombie differential for treatment and control groups. Secured borrowing of zombie borrowers relative to non-zombie firms declines by INR 45 million, representing a 74 percent fall relative to the pre-reform period.

The DDD estimates difference out trends common to both zombie and non-zombie firms, and the control group accounts for the counterfactual estimate of the zombie versus non-zombie difference. Any additional threats to the design arise if time-varying, firm-specific shocks are correlated with the asset-tangibility measure and are not common to both zombie and non-zombie firms. Several robustness tests address this threat. First, there is no divergence in credit allocation and capital spending of the treatment and control groups pre-reform, lending credibility to the parallel trend assumption. Second, effects are similar using an alternative identification strategy that takes firms that borrow from non-banking financial companies (NBFCs) — not covered under the reform — as the control group. Third, placebo tests using unsecured loans, which are not covered by the collateral reform, show no similar effects. Fourth, zombie lending also declines after the introduction of specialized debt recovery tribunals in the 1990s, confirming that these effects are not specific to the boom period of the 2000s.

Why should the collateral reform reduce credit to zombie borrowers? Zombie lending arises due to limited liability when lenders become reluctant to recognize bad loans and gamble for resurrection either by risk-shifting or asset substitution (Bruche and Llobet, 2014). With limited liability, lenders do not face the downside of continuing bad loans but benefit from the upside and continue to lend to insolvent borrowers at subsidized rates. Similarly, loan provi-

sioning requirements can compel weakly capitalized lenders to continue lending to otherwise insolvent borrowers (Peek and Rosengren, 2005). Post-reform lender payoffs from liquidation increase, and at the margin, lenders prefer liquidating rather than continue lending to zombie borrowers. I confirm this hypothesis by showing that even within distressed firms, zombie firms — distressed firms that receive subsidized credit — receive lower credit post-reform, compared to the distressed firms that *do not* receive such subsidized credit pre-reform.

To examine whether it is the contraction in loan supply (bank lending channel) or a decrease in borrower demand for credit that drives the decline in zombie lending, I turn to a new dataset on collateralized loans. Using firm fixed-effect specification, I show that for the same firm, collateralized credit is lower at the bank with which the firm has a zombie *relationship* before the reform, relative to a non-zombie relationship bank. The estimated decline of INR 39 million in credit is due to differences in the zombie relationship status as the within-firm comparison fully absorbs firm-specific changes in credit demand. This point estimate is not statistically different from the aggregate firm-level effect, indicating that supply-side effects entirely drive the decline in zombie lending. Zombie firms are also 3.5 percent less likely to repay the loans originated pre-reform, suggesting that the supply-side effect dominates for these distressed borrowers who cannot repay their loans post-reform. The loan-level data also allows me to show that the bias in firm-level regressions is inconsistent with a demand-driven hypothesis.

The second main result shows that the credit reallocation also affects firms' investments. Zombie firms, which see credit cuts, cannot switch into other forms of debt or equity. Instead, they cut capital spending by INR 26 million. In comparison, healthier non-zombie borrowers increase investment by INR 10 million. Capital spending of zombie borrowers declines by INR 38 million compared to non-zombie firms (DDD estimate), representing a 56 percent decline relative to the pre-reform period. One concern may be that the reduced form estimates are biased by other effects of the collateral reform such as demand-side factors (Vig, 2013) or other supply-side factors (Bhue et al., 2018). To assuage these concerns, I directly estimate the passthrough of the credit reallocation from zombie to non-zombie borrowers using an instrumental variable specification. I estimate an INR 27 million decline in zombie investment and a corresponding

INR 7.92 increase in non-zombie investment, close to the reduced-form estimates.

Having already established that the reallocation is due to a supply-side bank-lending I examine the mechanism for credit redistribution *to* healthier firms. Credit to non-zombie borrowers can increase due to (i) direct effects as lender willingness to lend increases owing to higher lender payoffs post-reform (Levine, 1998), (ii) reallocation effects arising from freeing up of lenders' balance sheets as they cut lending to preexisting zombie borrowers, (iii) indirect industry spillovers leading to increased demand for credit by non-zombie borrowers for capital spending due to zombie decongestion in their industries (Caballero et al., 2008). Though stronger creditor rights increase credit access for some marginal borrowers (unrated and lower-rated non-zombie firms), reallocation occurs primarily through the bank-lending channel. The bank-lending channel also dominates the indirect industry-demand channel, further strengthening the argument that supply-side effects drive credit reallocation.

In the last step, I examine the impact on allocative efficiency and aggregate productivity. Zombie firms receive subsidized credit pre-reform compared to non-zombie firms, creating a wedge or distortion compared to their first-best allocation. When firms equate their marginal product of capital with the marginal cost of capital, the zombie borrower's marginal product of capital with access to subsidized credit will be lower than non-zombie firms. Indeed, the pre-reform marginal product of capital for zombie firms is 0.490 compared to 2.110 for healthier non-zombie firms suggesting that moving a unit of capital from zombie to non-zombie firms can increase overall productivity almost 4.31 times. Hence, there are reasons to expect the credit redistribution and subsequent capital investment improved allocative efficiency and aggregate productivity. I use an accounting decomposition based on Osotimehin (2019) and a corresponding implementation by Blattner et al. (2019) in an instrumental variable framework to estimate the credit reallocation passthrough to aggregate productivity. Osotimehin (2019) decomposes aggregate productivity growth into changes in technical efficiency, within-sector allocative efficiency, between-sector allocative efficiency, and firm entry and exit. Aggregate productivity improves entirely due to an 18.70 percent increase in allocative efficiency with negligible effects from the remaining components. Using a simulation exercise, I show that 93.9

percent of this improvement is due to the lender supply-side credit reallocation from zombie to non-zombie borrowers. Counterfactual simulations rule out alternative mechanisms such as demand-driven effects, better credit access of marginal borrowers due to stronger creditor rights post-reform, or industry-level spillovers due to zombie decongestion (indirect channel).

Finally, I show that my baseline findings are robust to alternate hypotheses. Reduction in the insurance value of default post-reform (Bolton and Rosenthal, 2002; Vig, 2013)[1], alternate supply-side channels such as the liquidation bias of arm's length lenders compared to relationship lenders (Bhue et al., 2018; Goyal et al., 2019), state-ownership of banks (Banerjee et al., 2005), general equilibrium effects either through the credit markets (Lilienfeld-Toal et al., 2012), or through the labor markets (Biais and Mariotti, 2006) cannot explain my findings.

The main contribution of this paper is that it shows that collateral reforms can reduce zombie lending, a problem prevalent across the world (Banerjee and Hofmann, 2018). While zombie lending can help firms tide over temporary shocks (Schivardi et al., 2018), it can also adversely affect overall firm productivity by misallocating resources (Blattner et al., 2019) and hindering the process of creative destruction (Caballero et al., 2008). Attention has now shifted to finding ways to reduce zombie lending. Li and Ponticelli (2019) find that the introduction of specialized bankruptcy courts in China reduced the share of labor and capital sunk in zombie intensive industries. Bank recapitalization can also lower zombie lending (Giannetti and Simonov, 2013) when insolvency regimes are strong (Andrews and Petroulakis, 2019), but can perversely increase zombie lending if such recapitalizations are not sufficiently large (Acharya et al., 2019). Unconventional supervision, such as surprise on-site inspection of banks in Portugal, is successful in curtailing zombie lending (Bonfim et al., 2018). In developing countries, regulatory capture (Boot and Thakor, 1993) can limit efficacy of supervision and fiscal limitations can constrain bank recapitalization (Patel, 2020). I show that even without bank recapitalization or unconventional supervision, a broadly targeted collateral reform reduces zombie lending.

This paper also links the vast creditor rights literature to the above literature on zombie lending and highlights channels through which aggregate productivity improvements occur.

[1]See Section VIII for a full discussion of the contribution of this paper in the context of the findings in Vig (2013) and Bhue et al. (2018).

Strengthening creditor rights can improve real outcomes (Iverson, 2018; Alencar and Ponticelli, 2016) by reallocating labor and capital to healthier firms (Bian, 2018; Li and Ponticelli, 2019). While previous literature highlights the reallocation from the liquidation of distressed firms and the subsequent redeployment of labor and capital sunk in distressed firms to healthier firms, I highlight an additional reallocation mechanism that differs from these previous papers in a nuanced but important way. Improved recovery rates from liquidation post-reform incentivize lenders to cut credit to zombie borrowers, freeing up lender funds previously earmarked for zombie borrowers. While the seminal papers in the creditor rights literature highlight improved credit access as lender payoffs' increase (La Porta et al., 1997), recent work shows that credit access can decline if borrowers preemptively cut credit due to the increased liquidation threat (Gropp et al., 1997; Bolton and Rosenthal, 2002; Vig, 2013) or general equilibrium effects that price smaller borrowers out of credit markets (Lilienfeld-Toal et al., 2012). Although excessive creditor rights can prevent some borrowers from participating in the credit markets altogether, as the recent papers and often policymakers argue, excessive credit to some borrowers can also hinder productivity (Aghion et al., 2019). This paper's second contribution is that it highlights an additional reallocation channel through which credit is redirected to healthier firms that, absent the reform, would have been further sunk into zombie borrowers, thereby improving credit distribution and overall allocative efficiency and productivity.

While zombie lending in Japan in the 1990s (Peek and Rosengren, 2005), in Europe (Acharya et al., 2019), and more recently in China (Li and Ponticelli, 2019) has received much attention in the academic literature, zombie lending in India, the fifth-largest economy globally, has been less documented. India is also in the midst of nearly a decade-long zombie lending crisis starting 2008 (Chari et al., 2020; Chopra et al., 2020). Kulkarni et al. (2018) find only muted effects of India's more recent bankruptcy reform in 2016 on reducing zombie lending. In comparison, the 2002 collateral reform effectively curtails zombie lending, underscoring the need to establish what works in the Indian context. My third contribution is: I show policies such as the collateral reform can reduce zombie lending in India and improve aggregate productivity, entirely

through changes in allocative efficiency despite limited effects on firm-level productivity.[2]

The paper is organized as follows. Section I describes the institutional details. Section II provides the theoretical motivation for the reduction in zombie lending post-reform. Section III describes the data. Section IV describes the impact of the collateral reform on credit distribution. Section V examines the effects on capital investment. Section VI explains the mechanism driving reallocation and Section VII estimates the impact on aggregate productivity. Section VIII discusses alternative explanations. Section IX concludes.

I Institutional Details

Historically, enforcement of creditor rights in India was accompanied with significant judicial delay. In 1993, the government introduced the Debt Recovery Tribunals (DRTs) based on the recommendations of the Narasimham Committee I. These were quasi-legal institutions that streamlined the legal process and allowed speedy adjudication and swift execution of judgements (Visaria, 2009). With time, inadequate infrastructure and a shortage of recovery personnel ended up clogging the DRTs with excessive cases and eventually the DRTs too became ineffective. Several loopholes in the law allowed firms to indefinitely stall the judicial process. For example, defaulters could simultaneously file cases using an alternate route such as the Board for Industrial and Financial Reconstruction (BIFR) — which was meant for delinquent but bankrupt firms — as a way to delay legal actions taken by banks to recover their debt.

Subsequently, based on the recommendations of the Narasimham Committee II and the Andhyarujina Committee, the Indian government enacted the Securitization and Reconstruction of Financial Assets and Enforcement of Security Interests Act of 2002 (SARFAESI). The law was promulgated as an ordinance on June 21, 2002. This collateral reform allowed secured creditors to recover their non-performing assets (NPAs) by taking possession, managing, and selling securities without the intervention of a court or tribunal. Secured creditors could circumvent the lengthy judicial process and seize the assets securing their loans. A secured cred-

[2]Several recent papers analyze alternative outcomes of the collateral reform (Tantri, 2020; Alok et al., 2016; Bhue et al., 2018; Vig, 2013). See multiple analysis of reforms such as the banking deregulation [Strahan et al. (2003); Huang (2008); Beck et al. (2010) and references therein] in the US context and subsequent analysis of the drivers of aggregate productivity (Bai et al., 2018).

itor could start the recovery process by filing a notice on an NPA loan. If the loan was not repaid within 60 days from the date of notice, the creditor was allowed to take possession of the secured assets. The law only applied to secured borrowers and not to unsecured borrowers and covered both preexisting and new contracts. Further, the collateral reform only applied to banks and financial institutions and not NBFCs. However, this did not initially stop the NBFCs from seizing assets of defaulting firms under SARFAESI. After a long legal battle, the Supreme Court of India clarified that NBFCs could not seize collateral under SARFAESI. Since 2016, certain NBFCs are now also covered under the SARFAESI (The Economic Times, 2016). While the law was promulgated on June 21, 2002, discussions in the media started as early as 1999 (Vig, 2013). Although there was some initial uncertainty regarding the constitutional validity of the SARFAESI, the Supreme Court of India declared SARFAESI to be constitutionally valid in April 2004.[3]

Despite the initial uncertainty, the reform had an almost immediate impact on NPAs. Lenders started increasingly using SARFAESI as a recovery channel. Figure I Panel A shows that auction notices increased beginning the first quarter of 2003, indicating that cases were being filed under SARFAESI.[4] The Reserve Bank of India (RBI) estimated that the reform allowed banks to recover around INR 5 billion by June 2003, within a year of the reform (Reserve Bank of India, 2003). The reform also fixed an important loophole through which defaulters delayed the judicial process by filing simultaneously at the BIFR, and the number of cases filed under BIFR fell drastically post-reform (Panel B). The number of cases filed at the SARFAESI jumped to 50,000 in 2004–05 and reached 200,000 in 2015–2016 (Panel C), and the SARFAESI almost immediately became the predominant route for lenders to recover their loans (Panel D).[5]

Overall bank and firm health improved post-reform (Figure II). Bank health improved, and accretions to non-performing assets fell from 1.2 percent of net advances in 1999-–2000 to 0.4 percent in 2002-–2003 (Panel A). Though firms' secured debt to assets declined (Panel B), overall firm health improved. The percentage of distressed borrowers (defined as firms unable

[3]On 8 April 2004, in Mardia Chemicals Ltd. v. ICICI Bank, the Supreme Court of India declared the SARAFESI Act to be constitutionally valid.

[4]The data has been scraped from watchoutinvestors.com and is available starting 2003.

[5]Data in panels C and D are from RBI and only available post the fiscal year 2003–04.

to service their debt) fell from 30 percent to 14 percent, and the percentage of unprofitable borrowers (those with negative profits) fell from 37 percent to 22 percent (Panel C).

While the SARFAESI had an impact when it was enacted, various interpretations and reinterpretations of the law have rendered it less effective over time. For example, creditors could file at the DRT if there was a shortfall between creditor obligations and collateral value. Initially, borrowers had no rights to appeal but upon subsequent reinterpretation of the law, borrowers could appeal against secured creditor notices further weakening the law (Dvara Research, 2011). There has also been a sharp increase in NPAs following the global financial crisis. While India was relatively well insulated from the crisis in the beginning, there has been a steady increase in the NPAs of Indian banks since 2008 due to spillover effects starting 2007 (see Acharya et al., 2019). As the balance sheets of the banks themselves deteriorated after the crisis, banks may also have become less willing to recognize bad loans on their balance sheets. Given the confounding effects of the global crisis, I end my analysis in 2006.

Despite complaints by creditors that the limited appeal rights under SARFAESI were subject to abuse, SARFAESI was viewed as the most effective among all options even up until 2016. A report by the by the Planning Commission of India based on a high level committee headed by Raghuram Rajan (Planning Commission, 2008) noted that the business community viewed the SARFAESI as "a law that works" even as late as 2008. In 2016, the Insolvency and Bankruptcy Code (IBC) replaced overlapping provisions contained in various previous laws but the SARFAESI continues to be used. Even in 2014–2015, of all the channels available to lenders, nearly 90 percent was still recovered through SARFAESI (Figure I Panel D).

II Theoretical Motivation

This section motivates why we should expect to see a decline in zombie lending post-reform. To understand this, we need to first understand lenders' incentives to continue zombie lending before the reform. Existing research motivates zombie lending in four ways: (i) risk-shifting by weakly capitalized banks due to limited liability (Eisdorfer, 2008; Bruche and Llobet, 2014), (ii) to avoid loan provisioning requirements (Peek and Rosengren, 2005), (iii) agency problems

at state-owned banks (Banerjee et al., 2008), and (iv) manager myopia (Stein, 1989; Rajan, 1994; Flanagan and Purnanandam, 2020). I focus on the risk-shifting hypothesis and briefly discuss the logic for the remaining three cases, (ii)–(iv), which follows similar reasoning.

The first way zombie lending can arise is due to limited liability. Weakly capitalized banks have an incentive to continue lending to insolvent borrowers in order to hide losses and gamble for resurrection, even when such lending is socially inefficient. The following example illustrates this distorted lending incentive. Consider a risk-neutral bank. The bank's assets consist of safe assets worth INR 100 billion and a risky asset. The bank's liabilities amount to INR 130 billion. Liquidation value of the risky asset is INR 50 billion. Suppose the risky asset is a positive net present value (NPV) project at the time the loan was initially originated. Now suppose the lender updates its information after an exogenous event and the expected payoff from the loan changes. The asset is now expected to payoff INR 100 billion with a probability of 40 percent or INR 0 with a probability of 60 percent, in the next period. Liquidating the asset will pay off INR 20 billion (=$100 + 50 - 130$). Without limited liability, the payoff is a lower INR 10 billion (=$100 + 0.4 * 100 + 0.6 * 0 - 130$) and hence the bank will choose to liquidate. With limited liability, however, equity-holders do not face the downside. The expected payoff to equity holders is INR 28 billion [=$0.4 \times (100 + 100 - 130)$]. Since this is greater than the payoff from liquidation, the bank chooses not to liquidate. Thus in the presence of limited liability, the bank may choose to roll-over risky loans even when doing so is socially inefficient. Since liquidation is more efficient than continuation, we refer to rolling over of such loans as "zombie lending". Post-reform, these incentives change; payoffs from liquidation are now higher. If the liquidation value is sufficiently high (exceeds INR 58 billion), equity-holders choose to liquidate rather than roll-over zombie loans. The reform, thus, causes a decline in zombie lending, at the margin. When a bank is weakly capitalized, the payoffs from liquidation may not be enough to prevent the bank's decision to roll-over a loan.

For a weakly-capitalized bank with liabilities higher than INR 130 billion, the higher payoff of INR 58 billion post-reform may be insufficient to compel the bank to liquidate the loan. Thus, we expect to see heterogeneity in the effect of the collateral reform based on bank capitalization.

Costly loan provisioning requirements may also make undercapitalized banks unwilling to liquidate delinquent firms and continue zombie lending (Peek and Rosengren, 2005). Zombie lending can also arise at state-owned banks since anti-corruption laws subject loan officers at state-owned banks to extreme personal downside risk, who may then prefer evergreening loans (Banerjee et al., 2008). Managerial myopia may also make managers less reluctant to recognize bad loans due to reputational costs, even when it destroys long-term shareholder value (Rajan, 1994; Flanagan and Purnanandam, 2020). Post-reform, zombie lending decreases in all these cases when the liquidation payoff exceed the private benefits of zombie lending.

III Data

This paper uses data primarily from Prowess$_{dx}$, a database maintained by the Centre for Monitoring Indian Economy (CMIE). Prowess$_{dx}$ provides data on annual financial statements for Indian firms and includes both listed and unlisted firms. I exclude government-owned firms and foreign entities and focus on March 1996–March 2006 from the March 2016 vintage. Table B1 describes the variables. There are 47,414 firm-year observations, of which 45 percent belong to listed firms. All data is as of the fiscal year ending on March 31st. Data on collateralized loans are from the Ministry of Corporate Affairs (MCA). I describe the zombie measure below and details on data construction for other variables are in Section A1.

Zombie definition: To define zombie credit, I build on Caballero et al. (2008). Caballero et al. (2008) define a zombie firm as one whose interest payment is lower than the interest payment for credit-worthy firms. The proxy for interest payment for credit-worthy firms is the prime lending rate for the most credit-worthy firms. Fukuda and Nakamura (2013) point to several weaknesses of this measure. One, the Caballero et al. (2008) definition does not take into account the evergreening of loans. Two, during periods of weak demand, banks could offer interest rates below their prime lending rates. Three, the interest payments data is from the annual financial statements, and a small interest amount could simply reflect low firm leverage. Hence, I modify the Caballero et al. (2008) definition to address the above criticisms. A firm in my sample is classified as a zombie if it satisfies all of the following conditions: (i) interest

rate of the firm is below the minimum prime lending rate, (ii) ICR is less than 1, (iii) leverage (total external debt to total assets) is greater than 0.20, (iv) change in debt is greater than zero, and (v) the firm itself is not high-rated. The first item is analogous to the baseline definition in Caballero et al. (2008). The prime lending rate is the lowest among the prime lending rates of State Bank of India (the largest public sector bank in India), ICICI (one of the largest private sector banks in India), and IDBI (a development bank). The second and third items address the concern that interest payments may be low because the firm has low leverage. Hence the second item accounts for interest payments relative to its profitability, and the third item directly includes criteria for leverage. The fourth item accounts for evergreening, that is, that the lender was rolling over or extending credit. The fifth item ensures that I do not inadvertently classify creditworthy high-rated firms that receive low-interest rate loans as zombie borrowers.

IV Distributional Effect of the Collateral Reform on Credit

This section documents the distributive impact on credit post-reform. Section IV.A details the empirical strategy and the empirical results are described in Section IV.B. I start by analyzing the distributive effect on distressed versus healthy borrowers (Section IV.B.1). Since there is reason to expect effects on on zombie lending post-reform (see Section II), I proceed to determine if this credit redistribution is driven by a decline in zombie lending (Section IV.B.2). In Section IV.C, I use loan-level data to establish that the bank supply-side channel drives this redistribution away from zombie borrowers.

IV.A Identification Strategy

The first step is to analyze the reforms' distributive effect on credit. I start by analyzing the impact on distressed and healthy borrowers. Since the reform applied to all firms nationwide, I generate variation in treatment by exploiting ex-ante collateralizability of assets following Vig (2013). Since only tangible assets of a firm can effectively be collateralized in India, I classify firms with high asset tangibility as the treatment group and as the control group otherwise using the definition of tangibility from Rajan and Zingales (1995). The empirical strategy uses

a difference-in-difference-in-differences (DDD) framework and compares distressed borrowers to healthy firms — analogous to a simple difference-in-differences (DD) specification — of the treatment group to the same DD estimate of the control group. The key exogeneity assumption is that the control group provides an unbiased benchmark of how the distressed and healthy borrowers would have differed had there been no reform. The empirical specification is:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \eta \times \text{Distressed}_i \times \text{Post}_t + \nu \times \text{Treatment}_i \times \text{Post}_t \\ + \phi \times \text{Distressed}_i \times \text{Treatment}_i \times \text{Post}_t + \beta \times X_{it} + \epsilon_{it}, \end{aligned} \tag{1}$$

where i indexes firms, t indexes time, and α_i and γ_t denote firm and year fixed effects, respectively. The period is 1997–2006. Post is 1 if year is greater than 2002. Distressed firms have an average interest coverage ratio (ICR) in 2000 and 2001 that is less than 1. Treatment firms are firms that have above-median tangibility in 2001. Remaining terms in the regression are absorbed by firm and year fixed effects. y_{it} denotes secured borrowing and is the change in secured debt between $t-1$ and t (a flow variable). Standard errors are clustered at the firm-level. Control variables include firm-level time-varying measures of firm profitability and sales. More robust specifications also include industry-year fixed effects. The estimate of interest, ϕ, shows the differential effect on distressed versus healthy borrowers for the treatment group relative to the control group.

To facilitate transparent examination of the parallel trends assumption, I plot the coefficients of the DDD specification over time (ϕ_τ) below in event study plots.

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \sum_\tau \eta_\tau \times 1_\tau \times \text{Distressed}_i + \sum_\tau \nu_\tau \times 1_\tau \times \text{Treatment}_i \\ + \sum_\tau \phi_\tau \times 1_\tau \times \text{Distressed}_i \times \text{Treatment}_i + \epsilon_{it}, \end{aligned} \tag{2}$$

where τ ranges from 1997 to 2006, and 1_τ equals one if the year is τ. $\tau = 0$ is the year of reform, and all coefficients are normalized relative to $\tau = -1$. The coefficient of interest is ϕ_τ, which measures the difference (conditional on controls) in outcome y between distressed and healthy

borrowers for the treatment group relative to the control group τ years post-reform.

To examine the components driving the DDD estimates, I plot the event study plots separately for the sub-samples of control and treatment firms and examine the underlying parallel trends assumption. The specification is:

$$y_{it} = \alpha_i + \gamma_t + \sum_{\tau} \eta_\tau \times (1_\tau \times \text{Distressed}_i) + \epsilon_{it}, \tag{3}$$

where τ is from 1997–2006, 1_τ equals one if the year is τ and η_τ is the coefficient of interest. $\tau = 0$ is the year the reform is announced, and all coefficients are normalized relative to $\tau = -1$. The coefficient of interest, η_τ, measures the difference, conditional on controls, in outcome y between distressed and healthy borrowers τ years post-reform.

I also look at the sub-samples of distressed and healthy firms with:

$$y_{it} = \alpha_i + \gamma_t + \sum_{\tau} \eta_\tau \times (1_\tau \times \text{Treatment}_i) + \epsilon_{it}, \tag{4}$$

where τ ranges from 1997 to 2006, 1_τ equals one if the year is τ and η_τ is the coefficient of interest. $\tau = 0$ is the year the reform is announced, and all coefficients are normalized relative to $\tau = -1$. The coefficient of interest, η_τ, measures the difference, conditional on controls, in outcome y between control and treatment firms τ years after the reform for the distressed (or healthy) firms. I plot this separately for the sub-samples of distressed and healthy firms. Robust standard errors are clustered at the firm level in all the above event study plots.

To determine whether the decline in borrowing is driven by zombie lending motives, I split the sample firms into zombie and non-zombie firms based on whether they were receiving subsidized credit prior to the reform. The specification is:

$$\begin{aligned} y_{it} = \alpha_i + \gamma_t + \eta \times \text{Zombie}_i \times \text{Post}_t + \nu \times \text{Treatment}_i \times \text{Post}_t \\ + \phi \times \text{Zombie}_i \times \text{Treatment}_i \times \text{Post}_t + \beta \times X_{it} + \epsilon_{it}, \end{aligned} \tag{5}$$

where i indexes firms, t indexes time, and α_i and γ_t are firm and year fixed effects, respectively.

A firm receives zombie credit if it satisfies all of the following conditions: (i) interest rate of the firm is below the minimum prime lending rate, (ii) ICR is less than 1, (iii) leverage is greater than 0.20, (iv) change in debt is greater than zero, and (v) the firm itself is not high-rated. $Zombie = 1$ in the above equation if a firm receives zombie credit in any year between 1997–2002. Other terms are as defined before. ϕ, the coefficient of interest, compares the outcome variable, y_{it}, for zombie firms relative to non-zombie firms in the treatment group relative to the control group. Standard errors are clustered at the firm level. I replace the distressed indicator with an indicator for zombie in Equations 2, 3, and 4 to analyze the time dynamics and examine the parallel trends assumptions.

I next use a new data set on collateralized loans to set up a within-firm specification exploiting within-firm variation in the spirit of Khwaja and Mian (2008) as follows:

$$y_{ibt} = \alpha_{it} + \delta_{bt} + \eta \times \text{Zombie Relationship}_{ib} \times \text{Post}_t + \epsilon_{ibt}, \tag{6}$$

where i indexes firms, t indexes time, and b is the bank that the firm borrows from. α_{it}, δ_{bt} are firm-year and bank-year fixed effects respectively. Post_t is an indicator variables for the post-reform period. $\text{Zombie Relationship}_{ib}$ is a zombie relationship measure at the bank-firm level. A bank-firm pair is classified as a zombie relationship if the firm is a zombie (as defined in the baseline zombie measure) between 1997–2002 and receives a loan from the bank in the same period. I look at both the extensive and intensive margin with the dependent variable y_{ibt} as (i) an indicator variable for whether a firm borrows from a given bank b at time t or (ii) the amount borrowed by firm i from bank b at time t. The firm-year fixed effects, α_{it}, control for the firm-level changes in credit. Thus, the specification compares the amount of credit from banks that a firm has a zombie relationship relative to the credit from a bank that has a non-zombie relationship for the *same* firm. Standard errors are clustered at the firm and bank level.

I also examine the heterogeneity by lender characteristics with the specification:

$$\begin{aligned} y_{ibt} =& \alpha_{it} + \delta_{bt} + \eta \times \text{Zombie Relationship}_{ib} \times \text{Post}_t + \delta_{bt} + \\ & \gamma \times \text{Zombie Relationship}_{ib} \times \text{Post}_t \times \textit{Lender Characteristic}_b + \epsilon_{ibt}, \end{aligned} \tag{7}$$

where i indexes firms, t indexes time, and b is the bank that the firm borrows from. α_{it} and δ_{bt} are firm-year and bank-year fixed effects respectively. The lender characteristics I examine are: (i) whether a firms borrowed from a healthy bank prior to the reform, defined as a firm with below median non-performing assets ratio in 2001, (ii) whether a firm borrowed from state-owned banks, and (iii) whether a firm borrowed from a publicly listed private sector bank. The remaining variables are as defined in Equation 6.

The collateralized loan data also tracks whether a loan has been fully paid off (classified as "satisfied"). To examine whether zombie borrowers preemptively pay back their debt, I subset to loans originated in the pre-period between 1997-2002 and then estimate whether a loan has been satisfied post-reform. The specification is:

$$1_{ibv} = \alpha_i + \delta_{bv} + \eta \times \textit{Zombie Relationship}_{ib} \times \text{Post}_t + \epsilon_{ibv}, \tag{8}$$

where i indexes firms, and b is the bank the firm borrows from in period (vintage) v for the subset of loans originated in 1997–2002. The dependent variable is 1 if the loan has an entry in the date for "satisfaction of charge" column in the database. α_i is the firms fixed-effect and δ_{bv} is a fixed effect for loans of vintage v (originated in year v by bank b. δ_{bv} controls for vintage effects determined by the year (v) in which a loan was originated by a particular bank (b). Standard errors are clustered at the firm and bank level. $\textit{Zombie Relationship}_{ib}$ is as defined above. η measures whether a zombie firm is more (or less) likely to repay a loan post-reform.

IV.B Results

IV.B.1 Effect on Credit of Distressed and Healthy Firms

I first examine the impact on secured borrowing, a flow variable defined as the year-to-year change in secured debt. Positive (negative) values of secured borrowing depicts an increase (decrease) in the stock of secured debt. On average, secured borrowing of distressed borrowers declined from INR 51 million in the pre-reform period to INR 31 million in the post-reform (Table I), and secured borrowing of healthy borrowers increased from INR 31 million to INR 53 million. Table II shows this more formally. Secured borrowing of distressed firms declines by INR 26 million post-reform (column 1), whereas it increases by INR 16 million for healthy borrowers (column 2). Using the combined sample, the DD specification in column 3 shows a relative decline of INR 47 million for distressed borrowers (columns 3 and 4).

While the DD estimates difference out any time-varying differences common to both groups, estimates could be biased if any other macro factors differentially affected healthy and distressed firms. Hence, I use the DDD specification. To transparently examine the identifying variation, I build-up to the DDD estimate by separating the effects at distressed and healthy firms (columns 5–6). Secured borrowing of distressed firms in the treatment group declined by INR 33 million (column 5), whereas healthy firms see an increase of INR 12 million (column 6).

Table II columns 7–8 present the preferred DDD estimates using Equation 1. Distressed borrowers see an INR 44 million (DDD estimate) decline in secured borrowing, representing an 87 percent decline relative to the pre-reform average of INR 51 million (column 7). The DDD estimate captures the decline for distressed borrowers relative to the healthy borrowers for the treatment group compared to the control group. For ease of exposition, I refer to this as the relative decline for the distressed borrowers, henceforth. Column 8 shows a similar relative decline of INR 42 million with industry-year fixed effects and time-varying firm-level controls.

Figure III show the event study estimates. All coefficients are normalized relative to 2001. The bars show the 95 percent confidence intervals. Post-reform, secured debt for distressed firms (dashed blue line) declines and increases for healthy firms (solid red line). Panel B shows

the impact on secured borrowing of distressed and healthy firms for the treatment (solid red line) and control groups (dashed blue line). The decline is driven by the treatment group with no impact on the control group. Panel C shows the dynamics of the DDD specification using Equation 2. Effects increase in later years as the initial uncertainty regarding the reform's constitutional validity was settled in 2004 (Section I). Importantly, the parallel trends assumption in the DDD specification cannot be rejected.

Results are robust to using alternative measures of the dependent variable and alternative definitions of firm distress in Table B2 I also show results are robust to the choice of functional form of the dependent variable using the non-linear changes-in-changes method in Athey and Imbens (2006). See Section A2 for further detail.

IV.B.2 Effects Attributable to Zombie Lending

Is the credit decline attributable to reductions in zombie lending? Section II hypothesizes that lender payoffs increase post-reform making them unwilling to continue lending to zombie borrowers. Indeed, the summary statistics in Panel B in Table I show that secured borrowing declines for zombie borrowers, whereas it increases for non-zombie firms. In the aggregate, the fraction of zombie borrowers fell from 5.5 percent in 2002 to 3.5 percent in 2006 (dashed blue line in Figure IV Panel A) with similar trends when restricting to firms that receive subsidized credit (solid red line in Pane A) or when weighted by assets (Panel B).

Table III traces the post-reform credit of borrowers classified as zombies pre-reform. Secured credit of zombie borrowers declines by INR 31 million (column 1) compared to an increase of INR 15 million for non-zombie firms (column 2) with a relative decline of INR 46 million in column 3 (INR 43 million in column 4 with additional controls). Exploiting ex-ante tangibility in columns 5–7 (Table III), we see similar effects. Secured borrowing of zombie borrowers declines by INR 41 million (column 5) and increases by INR 12 million for non-distressed non-zombies (column 7).[6] The preferred DDD specification (using Equation 5) shows that secured credit of zombie borrowers fell by INR 44 million relative to non-zombie firms, repre-

[6]Effects are noisier in column 6 as it also includes distressed firms that lenders may also not be willing to lend to post-reform. Hence, I exclude distressed firms in column 7 to see the impact on the healthier non-zombie firms.

senting a 72 percent decline compared to the pre-reform average of INR 61 million (Table III, column 8). Column 9 shows a similar decline of INR 45 million with additional controls.

Figure V shows the event study plots. Panel A shows that zombie firms' secured borrowing declines (dashed blue line), but increases for non-zombie firms (solid red line). Panel B shows that the differential effect on secured borrowing for zombie versus non-zombie firms is driven by the treatment group (solid red line) with no effect on the control group (dashed blue line). Panel C shows the event study plots for the DDD specification allowing us to examine the time-dynamics and the implicit parallel trends assumption. Secured borrowing declines post-reform with larger effects in later years as the initial uncertainty in the reform's constitutional validity was resolved (Section I). Table B3 examines the pre-reform zombie characteristics and shows that while zombie firms are more likely to be in the manufacturing sector, older, lower-rated, the treatment and control groups do not differ along these dimensions, though zombie firms in the treatment group are more likely to be publicly listed. Further, Table B4 shows that distressed firms are more likely to receive credit, and zombie firms have higher access to credit pre-reform as hypothesized in Section II. While this could potentially confound results, identification only requires no divergence in the *trends* for the treatment group absent the treatment. The pre-reform trends (Panel C, Figure V) are similar, assuring us that we cannot reject the parallel trends assumption.

To further highlight that the credit redistribution is due to these preexisting zombie relationships and not due to differences in firm distress, I restrict to just distressed firms in Table B5. Zombie firms see a decline of INR 41 million (column 9, Table B5) *even* relative to distressed non-zombie firms. Nearly 97 percent of the total relative decline in secured debt (INR 42 in column 8,Table II) is attributable to lower zombie lending (see Section A2 for further discussion).

Section A2 discusses why the baseline zombie measure is preferred, but shows that results are robust to several alternative zombie definitions (Figure C1). I also show robustness with alternative identification strategies (Table B6, Section A2): (i) using firms linked to non-banking financial companies (NBFCs) as the control group, and (ii) exploiting preexisting differences in court efficiency across states. As a placebo test, I also show that the reform has no impact on

unsecured borrowing since the reform only applied to collateralized borrowing (Table B7).

IV.C WITHIN-FIRM VARIATION USING COLLATERALIZED LOAN DATA

This paper argues that credit to zombie borrowers declines post-reform due to a decline in zombie lending. Two different mechanisms can explain this decline. First, lenders cut credit post-reform as motivated in Section II (lender supply-driven channel). Alternatively, zombie borrowers preemptively cut credit post-reform due to the higher liquidation threat (firm demand-driven channel as in Bolton and Rosenthal, 2002; Vig, 2013). To examine which of the two mechanisms dominate, I turn to new collateralized loan-level data and exploit within-firm variation in the spirit of Khwaja and Mian (2008).

Zombie *relationship*, defined at the bank-firm level, measures whether a firm is a zombie (as defined in the baseline firm-level zombie measure) *and* whether a given bank lends to a zombie firm in the pre-reform period. Table B8 shows the summary statistics of the variables in the collateralized loan-level data. The average loan volume for a bank-firm pair is INR 48 million. Armed with the bank-firm level zombie relationship measure, I use Equation 6 to track whether a firm is more likely to receive a loan post-reform from the same bank. Columns 1–2 in Table IV do not include the firm×year fixed effect and show that a zombie relationship bank was 9 percent less likely to lend and cut the volume of lending by INR 13 million. The preferred specification in column 3 includes the firm×year fixed effect and shows that a bank with a zombie relationship with a firm is 32 percent less likely to lend post-reform than another bank with which the *same* firm does not have a zombie relationship. Similarly, the volume of lending declines by INR 39 million. The specification includes firm-year fixed effects and thus controls for demand-side factors that can affect credit at the firm-level.

Khwaja and Mian (2008) show that under certain assumptions (specifically if lending is additively separable over bank health and firm characteristics), the difference in the point estimates between the regressions including and excluding the firm×time fixed effects can help us measure the amount of bias induced by the endogenous matching of borrowers and lenders. Comparing the point estimate of INR 13.14 million in column 2 with the INR 39 million in

column 4 indicates that the bias is positive. Intuitively, banks that are less likely to cut zombie lending are matched to zombie firms. We provide evidence for this below by examining heterogeneity in effects across banks. In contrast, a demand-side hypothesis would suggest a negative bias as the firm-level demand pullback would mean that the specification without the firm×year fixed effect should be higher in magnitude (more negative) relative to the estimate with the fixed effect. Although fixed effects allow us to get unbiased estimates, they ignore general equilibrium response by firms, for example, if firms switch to borrowing from other banks, or there are crowding-out effects. Hence, the firm-level estimates are important and the point estimate of the aggregate firm-level effect in Table III is not statistically significant from firm×year fixed effect regression indicating that firms are not able to borrow from other banks and undo the cut in zombie lending. This is not surprising since 91.65 percent of the firms in our sample are single-relationship firms and do not borrow from multiple banks.

We next explore the reason for the endogeneous matching between banks and firms as indicated by the positive bias documented above. Table B9 examines the characteristics of zombie bank-firm relationships in the pre-reform period. Distressed banks are 2 percent more likely to have zombie relationships with firms (column 1), consistent with a risk-shifting story arising from limited liability of banks as in Section II. State-ownership adds another layer of complexity to banks' lending decisions. Anti-corruption laws can subject loan officers at state-owned banks to extreme personal downside risk (Banerjee et al., 2004) and may incentivize loan-officers to evergreen loans (Banerjee et al., 2008). Column 2, however, suggests that the state-owned banks do not drive pre-reform zombie lending. Zombie lending may also arise when managers are reluctant to recognize bad loans and hide poor performance, even if it destroys long-term shareholder value. Such managerial myopia is likely to be more acute at publicly-listed private sector banks (Rajan, 1994; Flanagan and Purnanandam, 2020). However, Column 3 suggests that the pre-reform zombie lending is not attributable to such managerial myopia. Column 5 shows that controlling for firm fixed effects yields similar results and within-firm zombie lending relationship is 2 percent higher for distressed banks.

Table IV, columns 5–10 shows the impact on zombie lending post-reform for different

lender characteristics using Equation 7. Column 5 shows that firms with zombie relationships with distressed banks are 36 percent less likely to make a loan post-reform. Distressed banks are also less likely to lend to firms with zombie relationship status pre-reform, though 13 percent less likely to do so compared to healthier banks. However, the impact on the volume of lending is similar for distressed and healthy banks (column 6). Banks trade off the gains from liquidation against the gains from evergreening. If banks are undercapitalized, the gains from liquidation may not be sufficient to incentivize banks to stop evergreening loans (see Section II). Columns 7–8 examine the effect across state-owned versus private sector banks and find no differential effect. Similarly, columns 9–10 shows that effects are not different for publicly-listed private firms.[7] These results are consistent with our finding that the distressed banks drove pre-reform zombie lending, not state-owned or publicly listed banks. Together, these results explain the positive bias in the point estimate with and without the firm$\times$year fixed effect in columns 2 and 4. Zombie firms are matched with distressed banks pre-reform and these are precisely the banks that are less likely to cut loans post-reform.

The MCA data also allows me to track whether a loan has been satisfied or fully paid by the borrower. I restrict to loans originated in the pre-reform period and examine whether the loan is more likely to be satisfied post-reform. Column 11 shows that a zombie borrower is 4 percent *less* likely to satisfy a loan in the post-reform period, 2003–2007. Thus, the decline in the borrowing of zombie borrowers is not attributable to a demand-side cutback in borrowing by firms by repaying loans, as documented in Vig, 2013. Section VIII presents additional robustness tests to alternative hypothesis.

V The Effect of the Collateral Reform on Capital Investment

Having established the significant distributional effect of the collateral reform on credit to zombie and non-zombie firms, I now show that the credit reallocation affects investment. I show the

[7] These heterogeneity specifications may be underpowered since only 8.35 percent of banks have multiple lending relationships. The empirical specification compares for the same firm, the impact on a zombie lending relationship with a distressed bank relative to a healthy bank's zombie lending relationship. In unreported results, I confirm using a less stringent specification that accounts for single bank relationships that the decline in lending is muted only for distressed banks but not state-owned or publicly-listed private sector banks. Results available upon request.

reduced form effects and then estimate the pass-through from credit reallocation established in the previous section to capital investment.

V.A Identification Strategy

To examine the reduced form impact on investment, I estimate Equations 1–5 with the dependent variable as capital expenditure from $t-1$ to t. These estimates capture the impact of (plausibly due to the lending cut) on firms' capital spending.

To examine the pass-through of the credit reallocation in Section IV to investment, I use the following instrumental variable design. I estimate the effect of the credit reallocation due to the collateral reform on firm investment using the specification:

$$y_{it} = \alpha_i + \gamma_t + \nu \times \widehat{\text{Secured borrowing}}_{it} + \epsilon_{it}, \tag{9}$$

for firm i in period t and outcome variable y_{it}. The outcome variable is capital expenditure for firm i in period t. α_i and γ_t are firm and time fixed effects. I instrument for secured borrowing in two ways. First, I instrument with the baseline firm-level zombie measure using the specification in Equation 5.

$$\begin{aligned}\text{Secured borrowing}_{it} = \alpha_i + \gamma_t + \eta \times \text{Distressed}_i \times \text{Post}_t + \nu \times \text{Treatment}_i \times \text{Post}_t \\ + \phi \times \text{Distressed}_i \times \text{Treatment}_i \times \text{Post}_t + \beta \times X_{it} + \epsilon_{it},\end{aligned} \tag{10}$$

I also use an alternate strategy that exploits the within-firm variation in zombie relationship based on the loan-level data:

$$\text{Secured borrowing}_{it} = \alpha_i + \gamma_t + \eta \times \text{Zombie Relationship}_i + \epsilon_{it}, \tag{11}$$

where Zombie Relationship$_i$ is the aggregate exposure to zombie relationships and is 1 if any of the bank-firm pairs for a firm is a zombie relationship in the pre-reform period as defined in Equation 6 (Zombie Relationship$_{ib} = 1$).

V.B RESULTS

Table V examines the impact of the collateral reform on the capital expenditure of firms. Column 1 shows that capital spending (investment) of zombie firms declined by INR 24 million whereas capital spending of the healthier non-zombie firms increased by INR 18 million (column 2). In a relative sense, investment of zombie firms declined by INR 35 million (column 4). Column 5 shows that zombie borrowers in the treatment group cut investments by INR 26 million, whereas non-zombie firms in the treatment group increase investment by INR 10 million (column 6). Using the preferred specification, columns 7–8 show that the capital expenditures of zombie borrowers relative to non-zombie firms for the treatment group declined by INR 38 million, representing a 56 percent decline relative to the pre-reform average. Figure VI confirms that the parallel trend assumptions for the specifications in Equation 5 cannot be rejected.

I then use an instrumental variable design to estimate the pass-through of the credit allocation described in Section IV using Equation 9. The first instrument is based on the baseline firm-level zombie measure as shown in Equation 10. Column 9 in Table V shows that an INR 1 increase in secured borrowing leads to a INR 0.77 increase in capital expenditure. To get a sense of the magnitude of the impact from credit reallocation on investment, the INR 41 million decline in credit to zombie borrowers (column 5, Table III) translates to INR 32 million (=0.77*41) decline in capital investment. Similarly, the INR 12 (column 7, Table III) million increase in lending to healthy (non-zombie) firms translates to INR 9.24 (=0.77*12) million increase in capital investment. In a relative sense, this amounts to INR 35 million (=0.77*45 based on column 9, Table III) decline in investment of zombie firms relative to healthier (non-zombie) firms. This relative estimate is very close to the reduced form estimate of INR 38 million (column 8, Table V) decline in investment for zombie firms relative to non-zombie firms, further validating our reduced form estimates.

One concern could be that the reduced form estimates in columns 1–8 in Table V capture the entire effect of the reform and could also be biased by the demand-side factors documented in Vig (2013) or the other supply-side effects documented in Bhue et al. (2018). The findings in Column 9 assuage this concern by directly capturing the effect of the credit reallocation due to

the decline in credit to zombie firms relative to non-zombie firms. Note, in Section IV I argued that using loan-level data and within-firm variation in zombie lending yields very similar estimates of the decline in zombie credit (compare estimates in column 4 in Table IV to column 9 in Table III) and addresses concerns that my estimates are picking up demand-side pullback in credit by zombie firms. Nonetheless, I repeat the instrumental variable estimation in Equation 9 using a firm-level zombie measure based on the within-firm (across bank) variation in zombie relationship. The first stage specification is in Equation 10. Repeating the exercise in the previous paragraph using the coefficient of the pass-through estimate of 0.66 (column 10, Table V) shows that zombie investment declined by INR 27 (=0.66*41) million compared to an increase in investment of non-zombies by INR 7.92 (=0.66*12 million). These estimates are very close to the reduced form estimates in column 5 and 6 in Table V. In a relative sense, this is a decline of INR 29.7 (=0.66*45) million and translates to an economically meaningful 43 percent decline in capital investment relative to the pre-reform average.

What investments do these zombie firms cut? A subset of the firms (1,288) has data on individual projects. I split the projects into core and non-core projects in columns 1–2 in Table B10. For core projects, the project industry code matches the industry code of the firm. Columns 1–2 show that the completion rate of core and non-core projects. Zombie firms were 47 percent more likely to complete core projects (column 9), and the coefficients are significantly different from each other, suggesting that zombie firms switched focus to core projects. Ersahin et al. (2019) find that creditors force firms that violate their loan covenants to refocus their operations. Similarly, plausibly lenders slashed zombie credit post-reform, forcing focus on core projects and streamline their operations.

For completeness, Section A3 in the Appendix also examines the impact of the collateral reform on labor outcomes. Similar to the analysis in this section, I find that zombie firms cut back on labor (Table B11) but effects are somewhat muted and hence I focus on the capital investment as the main channel for productivity improvements in Section VII.

VI Mechanisms For Credit Redistribution

I now discuss the mechanisms for credit redistribution. Section IV established that bank supply-side factors led to a decline in zombie lending (Section VIII provides further evidence against demand-side credit cutback to zombie firms). I now examine the mechanisms for redistribution to non-zombie firms. I hypothesize three distinct channels through which credit to non-zombie firms increases: a direct effect, a reallocation effect due to lenders freeing up bank capital sunk in zombie firms, and an indirect effect through the zombie decongestion in industries. The empirical specifications and results are discussed below.

In the first direct effect, lenders increase credit to marginal borrowers due to improved creditor rights post-reform as extensively documented in previous literature (La Porta et al., 1997). Indeed, credit to unrated and lower-rated non-zombie firms increases post-reform (Table B12).[8] However, we see no differential effect along the remaining dimensions (young vs. old, listed vs. unlisted, large vs.small) suggesting that this channel is not the primary driver of credit redistribution.

In the second reallocation effect, lenders used the capital freed from zombie borrowers and reallocate it to non-zombie firms. The third indirect effect is through industry spillovers wherein the zombie decongestion [as in Caballero et al. (2008)] in industries due to reductions in resources such as capital and labor allows non-zombie firms to increase input, increasing their credit demand and plausibly explaining the credit increase to non-zombie borrowers.

To examine these two mechanisms — reallocation through a bank lending channel and reallocation through the industry channel — I examine the reallocation on non-zombie firms relative to zombies more exposed to each channel using the specification:

$$\begin{aligned} y_{ibjt} = \alpha_i + \gamma_t + \eta \times \text{Non-zombie}_i \times \text{Post}_t + \nu \times \text{Channel Exposure} \times \text{Post}_t \\ + \phi \times \text{Non-zombie}_i \times \text{Channel Exposure} \times \text{Post}_t + \epsilon_{it}, \end{aligned} \tag{12}$$

where i indexes firms, t indexes time, j indexes the industry the firm belongs to, and b is the lead

[8] This effect could also be due to the small number of high rated firms (only 8 percent of the sample).

bank for the firm. α_i and γ_t are firm and year fixed effects, respectively. Post_t and Non-zombie_i are indicator variables for the post-reform period and for whether a firm is a zombie, respectively. The channels I explore are bank exposure and industry exposure. To calculate bank exposure I link each firm to the lead bank in the Prowess database and then aggregate to the bank-level.[9] Bank exposure is based on the fraction of zombie firms in a bank's portfolio. If this value is in the top two quintiles, then bank-exposure is 1. Industry exposure captures the industries that witnessed zombie decongestion and is 1 for industries with average asset tangibility of firms in 2000 above the median value. Standard errors are clustered at the level of randomization, that is, at the bank or industry-level depending on the exposure measure used.

Table VI, column 1 shows that non-zombie borrowers increase borrowing by INR 32 million from banks with high ex-ante exposure to zombie firms. Credit reallocation from zombie borrowers to non-zombie firms is higher in industries that witness the greatest zombie decongestion (column 2). Consistent with this demand-driven channel for non-zombies, Table B10 shows that capital expenditure of non-zombie firms in exposed industries was higher. However, the credit reallocation channel dominates the indirect channel (column 3). Prior literature highlights post-liquidation redeployment of credit, labor, and capital to healthier firms (Bian, 2018; Ponticelli and Alecnar, 2018). In contrast, I highlight how better lender payoffs post-reform make it less lucrative for lenders to continue zombie lending and it is this redirecting of credit earmarked for zombie firms that drive credit and capital reallocation. Indeed, Table B4 shows that pre-reform, a distressed firm was 14 percent more likely to receive zombie credit the next year (column 1). Credit to zombie firms was INR 42 million higher credit (column 3), particularly for more collateralized firms (columns 2 and 4).

[9] I use an exposure measure based on the firm-level Prowess data since we need the stock of credit to zombie firms at the bank-level as opposed to the flow of credit captured by the MCA data. Each firm is assigned to the lead bank (the bank that appears first in the firm's annual report). For 55 percent of the firms, the bank names appear in alphabetical order. Dropping these firms from the sample yields qualitatively similar effects, though are noisier. Results available on request.

VII Measuring the Effect of Credit Redistribution on Aggregate Productivity

Zombie lending provides subsidized credit to a set of borrowers relative to healthier firms. When firms equate their marginal product of capital to the marginal costs of capital, this subsidized credit ends up creating wedges or distortions in firms' first-order conditions. Plausibly, when lenders cut zombie credit post-reform and redirect it to healthier firms that then increase capital investments, overall allocative efficiency and productivity also improve. Hence, I next estimate and decompose the effect of collateral reform on aggregate productivity. Section VII.A describes the aggregate productivity decomposition from Osotimehin (2019) that forms the central part of the analysis. Section VII.B describes the empirical strategy that allows me to estimate the passthrough from the credit allocation to the aggregate productivity using this decomposition. I conclude with simulation exercises to determine how much of the improvement can be attributed to credit reallocation from zombie to non-zombie borrowers through the supply-side bank lending channel.

VII.A Aggregate productivity decomposition and capital wedges

This section builds on the literature on misallocation in Restuccia and Rogerson (2008) and Hsieh and Klenow (2009) who estimate wedges in firms' first-order conditions to determine capital distortions. I follow previous literature and model the misallocation as wedges on the prices of inputs (Bau and Matray, 2020; Blattner et al., 2019). Assuming a Cobb-Douglas technology, firms' marginal productivity for capital is:[10]

$$MRPK_{it} := \alpha_s \frac{p_{it} Y_{it}}{K_{it}} = \mu^K_{st} P_{st} (1 + \tilde{\tau}^K_{it}) \tag{14}$$

[10] I focus only on capital reallocation due to the limited effect of the reform on labor. For completeness, marginal productivity for labor is analogously defined as:

$$MRPL_{it} := \beta_s \frac{p_{it} Y_{it}}{L_{it}} = \mu^L_{st} P_{st} (1 + \tau^L_{it}) \tag{13}$$

β_s is the elasticity of labor in sector s. L_{it} is the number of employees.

α_s is the elasticity of capital in sector s. $p_{it}Y_{it}$ is value-added, K_{it} is the capital stock. The equation indicates that inputs may be inefficiently allocated across firms as a result of the distortions or input wedges, $(1+\tilde{\tau}_{it}^K)$. The term μ_{st}^K is the mark-up or the output wedge arising from the direct effect on a firm's output and price, for example if the firm has market power. As in Bau and Matray (2020), we abstract away from differentiating between the input and output wedge and instead focus on the combined wedge or distortion defined as $(1+\tau_{it}^K) = \mu_{st}^K P_{st}(1+\tilde{\tau}_{it}^K)$.[11]

To determine the impact on allocative efficiency, I rely on the decomposition in Osotimehin (2019), which is based on an extension of the growth accounting framework in Solow (1957). Osotimehin (2019) decomposes aggregate productivity growth into changes in technical efficiency (ΔTE), changes in within-sector allocative efficiency ($\Delta \text{AE}_{within}$), changes in between-sector allocative efficiency ($\Delta \text{AE}_{between}$), and changes in entry and exit (EX):

$$\Delta \ln \text{TFP} = \Delta \text{TE} + \Delta \text{AE}_{within} + \Delta \text{AE}_{between} + \Delta EX \tag{15}$$

The within-sector changes in allocation, $\Delta \text{AE}_{within}$, forms the most important of the analysis. It reflects the effects of capital distortions on aggregate productivity and is the weighted average of firm-level changes in distortions. The impact on aggregate productivity depends only on the relative marginal productivities, which allows us to estimate the change in allocative efficiency without explicitly measuring the shadow cost of capital. Instead to accurately estimate the impact on allocative efficiency, we only need to determine the growth in MRPK:

$$\Delta \text{AE}_{within,s} \simeq \frac{\alpha_s}{1-\gamma_s\theta_s} \sum_{i \in C_{st}} \frac{\Delta MRPK_{it}}{MRPK_{it-1}} \left[(1-\beta_s\theta_s) \frac{K_{it}}{\sum\limits_{i \in C_{st}} K_{it}} - \frac{p_{it}Y_{it}}{\sum\limits_{i \in C_{st}} p_{it}Y_{it}} \right] \tag{16}$$

$$\Delta \text{AE}_{within} = \sum_{s=1}^{S} \frac{1}{1-\gamma_s\rho} \left[\frac{P_{st}Y_{st}}{P_tY_t} - \rho\epsilon_t^K \frac{K_{st}}{K_t} \right] \Delta \text{AE}_{within,s} \tag{17}$$

[11] This abstraction is only needed for the exploratory analysis where I examine the reduced-form estimate of impact of the reform on capital wedges or distortion. For the main aggregate productivity and allocative efficiency decomposition, I only need to determine the changes in marginal productivity.

where C_{st} the set of continuing firms in sector s at time t. Assuming constant returns to scale, I set $\gamma_s = \gamma = 1$. I also set $\theta_s = \theta = 0.66$ and $\rho = 0.5$, corresponding to an elasticity of substitution of 3 within sectors and an elasticity of substitution of 2 between sectors. $\frac{\Delta MRPK_{it}}{MRPK_{it-1}}$ is the growth in marginal productivity of capital. $\frac{K_{it}}{\sum_{i \in C_{st}} K_{it}}$ is the share of capital of a firm relative to the sector-level capital. $\frac{p_{it}Y_{it}}{\sum_{i \in C_{st}} p_{it}Y_{it}}$ is similarly the firm-level value added share relative to the sector. $\frac{P_{st}Y_{st}}{P_tY_t}$ is the sectoral value share added and $\frac{K_{st}}{K_t}$ is the sectoral share of capital. ϵ_t^K is a function of the sectoral capital elasticity and the elasticity of substitution between sectors, defined as:

$$\epsilon_t^K = \frac{\alpha^Y - \rho\alpha^L}{1 - \rho(\alpha^Y + \beta^K)} \tag{18}$$

where $\alpha^Y = \sum_s \alpha_s \frac{P_{st}Y_{st}}{P_tY_t}$, $\beta^K = \sum_s \beta_s \frac{P_{st}Y_{st}}{P_tY_t}$, and $\alpha^L = \sum_s \alpha_s \frac{L_{st}}{L_t}$.

The between-sector allocative efficiency change in Equation 15 is calculated as:

$$\Delta \text{AE}_{between} = \sum_{s=1}^{s} \frac{1}{1 - \gamma_s\rho} \left(\epsilon_t^K (1 - \beta_s\rho) \frac{K_{st}}{K_t} - \alpha_s \frac{P_{st}Y_{st}}{P_tY_t} \right) \frac{\Delta MRPK_{st}}{MRPK_{st-1}} \tag{19}$$

Since there is only a minimal effect of the collateral reform on exit and entry of firms (see Table B13 and Appendix B2), I set ΔEX in Equation 15 to zero.[12]

The aggregate technology efficiency in Equation 15 is a function of firm-level TFP changes ΔTE_s, the sector-level change in technical efficiency is:

$$\Delta TE_s \simeq \frac{1}{1 - \gamma_s\theta_s} \sum_{i \in C_{st}} \frac{\Delta A_{it}}{A_{it-1}} \left[\frac{p_{it-1}Y_{it-1}}{\sum_{i \in C_{st}} p_{it-1}Y_{it-1}} - \alpha_s\theta_s \frac{K_{it-1}}{\sum_{i \in C_{st}} K_{it-1}} \right] \tag{20}$$

To get the aggregate productivity changes, we need to aggregate across sectors as in Equation 17 with $\Delta \text{AE}_{within,s}$ replaced with ΔTE_s. $\frac{\Delta A_{it}}{A_{it-1}}$ is the growth in firm-level TFP and remaining terms are as defined previously.

[12] Blattner et al. (2019) and Osotimehin (2019) also find that the contribution of this term to aggregate productivity is negligible in the context of France and Portugal, respectively.

VII.B EMPIRICAL STRATEGY

I now describe the empirical strategy to estimate the impact of the reform on allocative efficiency due to the credit reallocation away from zombie firms to non-zombie firms.[13] Since we only need to estimate the firm-level changes in marginal productivity to estimate the overall impact on allocative efficiency (as shown in Equation 19), I first estimate the passthrough of credit to growth in firm-level marginal productivities for the post-reform period using:

$$\frac{\Delta MRPK_{ijt}}{MRPK_{ijt-1}} = \alpha_{jt} + \beta_1 \times \text{Secured Borrowing}_{ijt} + \beta_2 \times \text{Pre-reform Capital Wedge}_i \times \text{Secured Borrowing}_{it} + \epsilon_{ijt} \quad (21)$$

for firm i in industry j in period t. α_{jt} is the industry-year fixed effect. The sample is restricted to the post-reform period since we are interested in the credit pass-through during this period. Standard errors are clustered at the firm level. Pre-reform Capital Wedge$_i$ is the average MRPK for a firm in pre-reform period. $MRPK_{ijt}$ is the marginal productivity of capital and calculated as described in Section A1. Building on the analysis in the Section IV, I instrument for secured borrowing with the non-dynamic version of the regression in Equation 5 as the sample is restricted to the post-reform period. The first stage specification is:

$$\text{Secured Borrowing}_{ijt} = \alpha_{jt} + \eta \times \text{Zombie}_i + \nu \times \text{Treatment}_i + \phi \times \text{Zombie}_i \times \text{Treatment}_i + \epsilon_{ijt} \quad (22)$$

I obtain the partial equilibrium effect of the collateral reform on aggregate productivity by estimating $\frac{\Delta MRPK_{ijt}}{MRPK_{ijt-1}}$ using $\widehat{\beta}_1$ and $\widehat{\beta}_2$ estimated from Equation 21 and credit to zombie and

[13] This part of the analysis is similar to Blattner et al. (2019) who examines an opposite credit reallocation away from healthy firms to firms that underreport their losses (analogous to zombie firms).

non-zombie firms estimated in Table III:

$$\frac{\Delta\widehat{MRPK}_{ijt}}{MRPK_{ijt-1}} = \widehat{\beta}_1 \times \widehat{\text{Secured Borrowing}}_{ijt} + \widehat{\beta}_2 \times \text{Pre-reform Capital Wedge}_i \times \widehat{\text{Secured Borrowing}}_{it} \quad (23)$$

Finally, to estimate how much of the total reallocation effect (through the bank-lending channel) can be attributed to the credit reallocation from zombie to non-zombie firms I use a simulation exercise, the details of which I leave to the next subsection.

As a precursor to estimating the impact on allocative efficiency, in supplementary analyses I determine the reduced form effects on firm-level capital wedges, MRPK with:

$$Y_{it} = \alpha_i + \gamma_{jt} + \beta_0 \times \text{Post}_t \times \text{Non-zombie}_i + \beta_1 \times \text{Post}_t \times \text{Non-zombie}_i \times \text{Industry-Exposure}_j + \epsilon_{ijt}, \quad (24)$$

where i indexes firms, t indexes time, j indexes the industry in which the firm operates. Industry-Exposure$_j$ and Post$_t$ are indicators for high asset tangibility zombie industries and the post-reform period as defined in Table B1. The dependent variable of interest is capital wedge, MRPK, and firm-level productivity (TFP) as described in Section A1. β_1, the coefficient of interest, measures the relative non-zombie versus zombie impact on the outcome variable in more exposed industries relative to less. Standard errors are clustered at the industry level.

VII.C Results

The average marginal product of zombie firms' capital is 0.490 in the pre-reform period (Table I). In comparison, the average marginal product of capital is an order of magnitude higher for healthier non-zombie firms at 2.110, suggesting that reallocating a unit of capital from zombie to non-zombie firms would increase output almost 4.31 times. Why do non-zombie firms have higher marginal products relative to zombie firms in the pre-period? Consider the canonical example of misallocation in Hsieh and Klenow (2009) and Restuccia and Rogerson (2008)

where only some firms receive subsidized credit. In an economy with two firms with identical technologies, and with one firm receiving subsidized credit (in my case, the zombie firm) and the other a healthy firm (non-zombie firm) that can borrow only at non-subsidized/higher interest rates. If both firms equate their marginal product of capital with the interest rate, the marginal product of capital of the zombie borrower with access to subsidized credit will be lower than the marginal product of capital for the non-zombie firm. As Hsieh and Klenow (2009) and Restuccia and Rogerson (2008) argue, this indicates capital misallocation since a social planner could improve aggregate output by reallocating capital from the zombie firm with the lower marginal product to the non-zombie firm with the higher marginal product. Note, such reallocation can improve allocative efficiency only if there a preexisting misallocation or a dispersion in distortions to begin with. When firms are unable to achieve their first-best outcome, this introduces wedges or distortions. In line with the marginal product, capital wedge is much smaller for zombie firms (2.96) than non-zombie firms (11.14) in the pre-reform period (Table I). TFP, firm-level productivity (A_i), is similar for both zombie and non-zombie firms, which is not surprising since prior literature has found that firm-level TFP and wedges are not necessarily correlated (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Nishida et al., 2017; Blattner et al., 2019).

To motivate the allocative efficiency and productivity analysis, I examine the reduced-form effect of the reform on the capital wedges, MRPK, and firm-level TFP in industries congested by zombie firms in Table VII using Equation 24. I focus on the relative impact of the non-zombie versus zombie firms as this relative effect matters for allocative efficiency (and not the average change in firm-level productivities).[14] The capital wedge declined by 4.203 on average for non-zombie firms relative to zombie borrowers in industries that witnessed a zombie decongestion (Column 1). The collateral reform decreased the difference in wedges between the zombie and non-zombie firms, especially in industries more exposed to the reform, which

[14] I focus on the relative or distributional impact since there is no reason to expect effects on the average level of MRPK, capital wedges, and TFP. The policy did not directly affect the production technology or firm-level productivity and hence we don't see an impact on the levels in Table I. The credit redistribution documented in previous sections corrects for the distortions in the *distribution* of MRPK or capital wedges and hence I focus on the distributional aspects of the reform.

could potentially improve allocative efficiency. Columns 2 and 3 indicate the effect is more concentrated in firms linked to high-exposure banks (-4.665, s.e.=1.605) relative to low exposure banks (-2.635, s.e.=1.743), though the estimates are not statistically distinguishable from each other. The marginal product of capital for non-zombie borrowers relative to zombie borrowers declines (Column 4). There is no statistically significant impact on firm-level TFP (column 5).

In the primary analysis, I estimate the impact on aggregate productivity using Equation 15. To calculate the allocative efficiency (Equations 16 and 17), I need to determine MRPK growth. I determine the passthrough of the credit reallocation to MRPK growth using Equations 21 and 23. Columns 6 and 7 in Table VII examines the passthrough of credit allocation to firm-level MRPK growth as specified in Equation 21. The F-statistic of the first-stage in both columns is above the threshold of 5. There is no overall impact of credit access on growth in the marginal productivity of capital. Column 7 shows that marginal productivity of capital declines with credit increase, but only for firms with a high pre-reform capital wedge (a proxy for financially constrained firms). Firms with a high pre-reform capital wedge see a 16.5 percent decrease in their marginal productivity when credit increases by INR 1 million. This is consistent with the credit reallocation from the zombie (low MRPK) to non-zombie (high MRPK) firms that allows non-zombie firms to increase their inputs (capital) closer to their first-best allocation, lowering their MRPK and indicates that the reform possibly improved allocative efficiency.

I obtain the partial equilibrium effect of the collateral reform on aggregate productivity by estimating $\frac{\Delta MRPK_{ijt}}{MRPK_{ijt-1}}$ using β_1 and β_2 estimated from Equation 21 and using the credit to zombie and non-zombie firms estimated in Table III. Table VIII shows the results of this aggregation. I collapse the data to the pre- and post-period to estimate the aggregate change in allocative efficiency. Using the passthrough due to the credit allocation post-reform as indicated by Equation 23, I find that the the collateral reform improved allocative efficiency by 18.70 percent (Panel A). The total (actual) allocative efficiency growth during this period obtained by plugging in the raw data in Equation 16 is 29.32 percent post-reform, possibly because the period also coincided with an overall global economic boom. Thus, nearly 63.7 percent of the allocative efficiency improvement during the period is due to the collateral reform (=18.70/29.32).

These estimates are also indicative of the impact on overall productivity (Equation 15) since between-sector allocative efficiency has a smaller -4.7 percent effect as estimated with Equation 19 and ΔTE and ΔEX term are set to zero given the muted effects on TFP (column 5, Table VII) and firms' exit and entry (Section B2).

Section IV and VI point to the bank supply-side channel and the resulting credit reallocation as the primary driver of allocative efficiency improvements. The bank-lending channel hypothesizes that post-reform, banks free up funds that would have been previously sunk in zombie firms and redirect the credit to healthier non-zombie borrowers. I use a simulation exercise to estimate how much of the aggregate change in allocative efficiency post-reform is due to the credit reallocation from zombie to non-zombie borrowers through the bank-lending channel. I first aggregate total credit to zombie firms (due to the reform). We know lending increased for non-zombie firms but cannot identify the non-zombie firms that see increased credit due to the credit reallocation. Hence, I take 10,000 random draws of the non-zombie firms such that the total credit decline for zombie firms is equal to the total credit increase for non-zombie firms. The 10,000 different permutations of allocations to non-zombie borrowers gives a range of possible allocative efficiency estimates. Panel B, Table VIII summarizes the results of this simulation. On average, allocative efficiency increases by 17.56 percent, accounting for nearly 93.90 percent of the total estimated effect of 18.70 percent. If all of the credit freed up from the zombie borrowers is allocated to the firms with the lowest (highest) marginal product of capital, we would see smaller (larger) allocative efficiency improvements. Thus, the change in allocative efficiency ranges from 6.29 percent (when credit is reallocated to non-zombie firms with lower marginal product of capital) to 18.92 percent (when credit is reallocated to non-zombie firms with higher marginal product of capital). Thus, the zombie to non-zombie credit redistribution improved allocative efficiency post-reform between 33.63–101.17 percent.

The direct and the indirect channel (hypothesized in Section VI) have only a limited impact on allocative efficiency. Since the direct channel improves credit access of marginal borrowers post-reform, I re-estimate the baseline simulation excluding the marginal borrowers (as proxied by young, unlisted, low-rated, and small firms). I find allocative efficiency changes by 17.09

percent, close to the estimates in Panel B (17.56 percent). I also find a limited effect of the indirect channel wherein non-zombie firms increase investment as the zombie decongestion frees up resources (such as capital) sunk in zombie firms. I estimate allocative efficiency change by modifying the baseline simulation such that the aggregate capital investment decline of zombie borrowers equals the corresponding increase for non-zombie borrowers. Panel C shows that the indirect channel has only a -0.5 percent effect.

Demand-driven cutbacks in credit by zombie borrowers, as hypothesized in Vig (2013), do not account for the allocative efficiency improvements. Column 1, Table B14 confirms the baseline result in Vig (2013). On average, firms in the treatment group see a decline in secured borrowing. Columns 2 and 3 are analogous to columns 6, and 7 in Table VII, except secured borrowing is instrumented with the non-dynamic version of column 1 (an indicator for high tangibility). The first stage F-statistic in both columns is above the threshold of 5. Unlike column 6 in Table VII, column 3 in Table B14 indicates only a noisy effect (coeff=-0.163, s.e.=0.0976) on MRPK growth for firms with high pre-reform capital wedges (constrained firms). The passthrough of the credit reduction to MRPK growth implies a decline in allocative efficiency (-15.73 percent), contrary to the zombie to non-zombie credit reallocation simulation above.

An alternative bank-lending channel is as follows. Zombie firms preemptively cut credit [demand-driven channel in Vig (2013)], freeing up bank capital, and lenders redirect credit to marginal borrowers (direct effect of improved creditor rights post-reform). I repeat the baseline simulation and set the aggregate credit decline the treatment group to equal the aggregate credit increase to marginal borrowers (as proxied by young/low-rated/unlisted/small borrowers) and estimate only a -0.15 percent change in allocative efficiency (Panel C).

Overall the results in this section indicate that the collateral reform had a significant effect on allocative efficiency and overall productivity. Several simulation exercises show that the bank supply-side credit reallocation from zombie to non-zombie borrowers is the primary driver of these aggregate productivity improvements.

VIII Alternative Channels

I conclude by exploring alternative explanations for the observed patterns of credit. Alternative channels such as a reduction in insurance value of default post-reform (Bolton and Rosenthal, 2002; Vig, 2013), alternative supply-side channels such as the liquidation bias of arm's length lenders compared to relationship lenders (Bhue et al., 2018; Goyal et al., 2019), state-ownership of banks (Banerjee et al., 2005), general equilibrium effects either through the credit markets (Lilienfeld-Toal et al., 2012), or through the labor markets (Biais and Mariotti, 2006) cannot explain my findings. I focus on robustness to the insurance channel and relationship channel documented based on the same reform I study and leave the discussion of the remaining possible channels to Section A3.

The insurance channel (Gropp et al., 1997; Bolton and Rosenthal, 2002) hypothesizes that borrowers can preemptively cut credit post-reform due to higher liquidation threat or decline in insurance value of default. In particular, Vig (2013) shows that high-tangibility firms cut credit and their profitability improves. At face value, my results may not seem entirely novel in light of Vig (2013). Indeed, treated firms (high-tangibility) see an overall decline in lending as in Vig (2013) (column 1, Table B14).[15] However, I document that while zombie borrowers see a decrease in lending, non-zombie borrowers see an increase (columns 6–7, Table II), and the *relative* difference between the zombie and non-zombie borrowers increases (columns 8–9, Table II). My contribution is not merely documenting heterogeneity in credit to zombie and non-zombie borrowers. Instead, I hypothesize that lenders cut credit supply to zombie borrowers, allowing them to reallocate credit to healthier firms. Once a lender takes a borrower to liquidation (or restructures the loan), the sequence of events is as follows: the lender first recognizes a loan as non-performing (at which time the lender will need to provision for the loan losses) and then proceeds to liquidate the assets securing the loan. Pre-reform, lenders are reluctant to recognize non-performing assets because of limited liability and loan provision-

[15] In the baseline specification in this paper, I use the change in secured debt (a flow variable) as the dependent variable, which allows for easy interpretation of the rupee magnitude of the reform's effect and feeds directly into the simulation exercise in Section VII. The results in column 1 in Table B14 are consistent with Vig (2013) and addresses concerns that the results in my paper are driven by these differences.

ing requirements. Instead, lenders evergreen loans to zombie borrowers in the off-chance the borrower recovers in the future, essentially gambling for resurrection. The improved recovery rates from liquidation post-reform incentivize the lender to recognize a loan to a zombie borrower as non-performing and seize the collateral securing the loan and start liquidation. This freeing up of bank capital sunk in continued credit to zombie firms drives credit reallocation.

I summarize the previous results supporting this supply-driven decline in zombie lending and also provide additional evidence below. The loan-level data (Section IV.C) helps establish the supply-side channel to a large extent. Within-firm analysis allows us to pinpoint the zombie lending mechanism by shutting down effects from firms' demand. By exploiting differences in zombie lending relationships for a bank-firm pair within the same firm, I can establish that bank-level differences in relationships (within firms) show similar effects.

Another argument is perhaps both demand-side and supply-side effects drive the results, and it is difficult to say which effect dominates. Khwaja and Mian (2008) show that the difference in the point estimates between the regressions including and excluding the firm$\times$time fixed effects allows us to measure the amount and direction of bias (discussed in Section IV.C). As shown in Table IV, columns 1–4, the bias is positive, indicating that banks that are less likely to cut zombie lending are matched to zombie firms. Suppose demand-side factors were biasing the results [consistent with Vig (2013)]. In that case, we should see a negative bias as the firm-level demand pullback would mean that the specification without the firm$\times$year fixed effect should be higher in magnitude (more negative) relative to the estimate with the fixed effects. Hence, the credit cuts to zombie borrowers is predominantly due to lender supply-side factors.

While exploiting within-firm variation in zombie lending allows us to control for demand-side factors affecting firm zombie borrowing, one could still argue that the particular bank-firm relationship is special. Plausibly, unobservable to the econometrician, there is something special about the relationship that leads to the zombie lending pre-reform and it is these unobservable characteristics of the relationship that results in different behavior by the banks and the firm in the zombie relationship (say if the borrower wants to "preserve" the relationship and preemptively pays back post-reform). While this argument is unlikely (but plausible), it cannot

explain why we find bank-level differences in zombie lending wherein healthy banks are more likely to cut zombie lending compared to distressed banks (column 5, Table IV) after controlling for demand side effects with the firm×time fixed effect. As I argue, bank-level differences arise due to banks' differences in motivation to zombie lend post-reform, too.

The within-firm variation in zombie lending also addresses concerns that results are driven by changes in the relationship versus arm's length lending post–reform. Bhue et al. (2018) use a similar setting and show that relationship lending declines post-reform because strong creditor rights increase the willingness of arm's length lenders to provide credit to firms (supply-side factor). Alternatively, in line with the liquidation bias channel above, Goyal et al. (2019) use a cross-country setting to show that firms switch from arm's length lenders to relationship lenders (who are less likely to liquidate) when creditor rights strengthen. The within-firm analysis using firm-level data, however, shows that even when I restrict to relationship bank-firm pairs, the lending by the zombie relationship bank declines relative to lending by the non-zombie relationship bank. These results address concerns that my results capture the direct impact of firms switching to arm's length borrowing (or vice versa).

Further, zombie borrowers are genuinely distressed borrowers who cannot repay their loans and not the borrowers in Vig (2013) who can preemptively cut credit exposure by repaying their debt post-reform. Using the MCA loan-level data, I trace whether a loan originated pre-reform is repaid post-reform in 2002–2007. Zombie borrowers are *less* likely — not more likely as Vig (2013) would suggest — to repay loans, despite the higher liquidation threat post-reform (Column 11, Table IV and also discussed in Section IV.C).

Lending further credence to the notion that these are severely impaired borrowers who do not (and likely cannot) willingly cutback credit, I show that unlike the borrowers in Vig (2013), zombie borrowers do not hoard cash (column 5, Table B7) and cannot undo the credit cuts by switching to other forms of credit, such as unsecured debt (Table B7, column 1) contrary to the hypothesis in Vig (2013).[16] Zombie borrowers are also not able to raise equity (column 2). Neither can they issue commercial paper or increase trade credit (Table B7, columns 3–4).

[16] Vig (2013) also finds limited effect on unsecured lending in contradiction to his theoretical framework, but attributes the lack of effect to the underdeveloped unsecured credit market in India.

Note, the above results do not contradict the findings in Vig (2013). Likely both the demand-side factors documented in Vig (2013) and the supply-side factors documented in this paper are at play. Instead, this paper emphasizes that the credit impact on the severely impaired zombie borrowers is driven by supply-side factors. I show that it is this bank lending channel that drives credit redistribution and improves allocative efficiency of capital. Section VII documents that nearly 94 percent of the credit effect on allocative efficiency is due to the bank supply-side credit reallocation from zombie to non-zombie borrowers. The demand-side channel cannot account for these aggregate productivity improvements.

Finally, I argue that the particular collateral reform I study is not special and strengthening creditor rights should reduce zombie lending on the margin, absent additional frictions.

External Validity: Evidence on zombie lending from two alternate changes in creditor rights in India supports this hypothesis. In Table B6, columns 1–2, I find a similar decline in borrowing using the staggered implementation of the DRTs in the 1990s as a measure of strengthened creditor rights. DRTs strengthened legal enforcement by reducing judicial delays (Lilienfeld-Toal et al., 2012). This result addresses concerns that the effects documented in this paper are specific only to the 2002 collateral reform studied.[17] A subsequent paper by Kulkarni et al. (2018) also shows that zombie lending declines after the 2016 bankruptcy reform in India.[18]

[17]The implementation of the DRTs is not the main identification strategy because, in effect, the creditor rights through the DRTs were weak. These quasi-legal courts quickly became ineffective as cases piled up at the DRTs. Significant loopholes, such as defaulters simultaneously filing at the BIFR indefinitely delayed the recovery process making the DRTs ineffective (see Section I for further institutional details).

[18]Kulkarni et al. (2018) find that the decline in zombie lending is muted due to the weakly capitalized banking system. My findings suggest that the collateral reform had far more substantial effects on zombie lending than the more recent 2016 insolvency law, though I too find slightly lower impact on zombie lending at distressed banks (column 5, Table IV). The differences in effects for the two reforms are not attributable to differences in health of the banking sector during these two periods as the ratio of non-performing assets to total assets (a measure of bank distress) pre-reform was 10.4 percent in March 2002 compared to a similar 7.5 percent before the insolvency law in March 2016. The difference in effect likely arises from the need for collective action in the insolvency law instead of individual recovery action for the collateral reform. The collateral reform allows a lender to enforce a specific contract or foreclose the specific collateral securing a loan. In contrast, the insolvency law requires that all creditors to collectively assess a firm's viability and then proceed either with restructuring or liquidating the firm, making the insolvency bankruptcy code less effective than the collateral reform studied in this paper.

IX Conclusion

The macro-development literature finds that financial frictions prevent optimal allocation of resources (Midrigan and Xu, 2014), especially in developing countries such as India and China (Hsieh and Klenow, 2009; Bau and Matray, 2020). Prominent frictions in emerging markets are debtor-friendly laws that make the resolution of distressed assets difficult. Realizing this, developing countries such as Brazil and China have recently introduced new bankruptcy laws increasing the legal protection of creditors. This paper highlights a critical channel, not studied in the literature, through which strengthening creditor rights can positively affect the allocation of credit and capital. The 2002 collateral reform in India is also interesting because the rhetoric at the time focused on the slowdown in secured credit growth following the reform (Chakravarty, 2003). This slowdown is puzzling because India is not a creditor-friendly country, and commentators argued that such a draconian reform makes creditor rights excessive. Especially in countries such as India, where creditor rights are initially weak, we should expect to initially see benign growth in credit reflecting massive churn, as credit gets reallocated from unproductive to productive firms. Despite the immediate pullback in credit post-reform, particularly by poorly performing firms, creditor-friendly laws can restore the economy to health.

Though broadly effective in cutting zombie lending, the collateral reform had only a modest impact on distressed banks. Bank recapitalization (Andrews and Petroulakis, 2019) or regulatory interventions (Kulkarni et al., 2018) may need to accompany such reforms to force even reluctant banks to cut zombie lending. I leave the examination of the effect of such policies on zombie lending to future research. These issues became even more pertinent as the COVID-19 pandemic has forced governments across the world to pass measures — such as government-guaranteed loans (Baudino, 2020) — that could potentially increase zombie loans. India, too, suspended the initiation of new insolvency proceedings in August 2020 (Bloomberg, 2020). While these schemes ensure the survival of temporarily distressed firms, they can also lead to a surge in zombie lending (Chari et al., 2020). It is even more pertinent we establish policies that effectively tackle zombie lending and ensure quick recovery post-crises.

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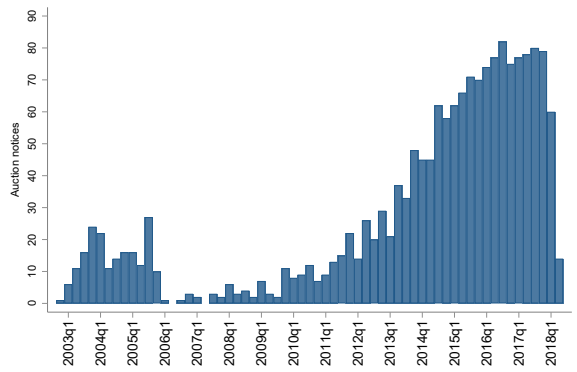
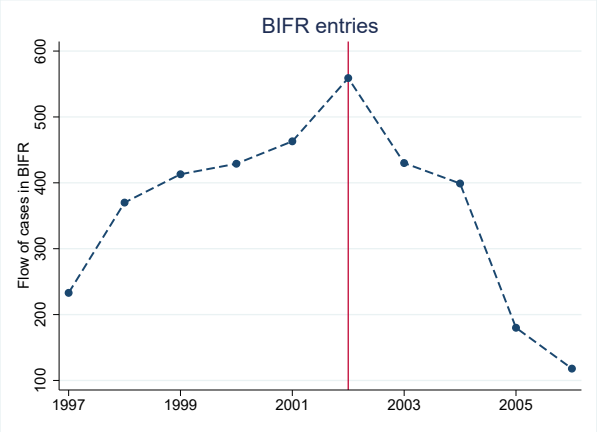
Figure I
Effectiveness of the Collateral Reform

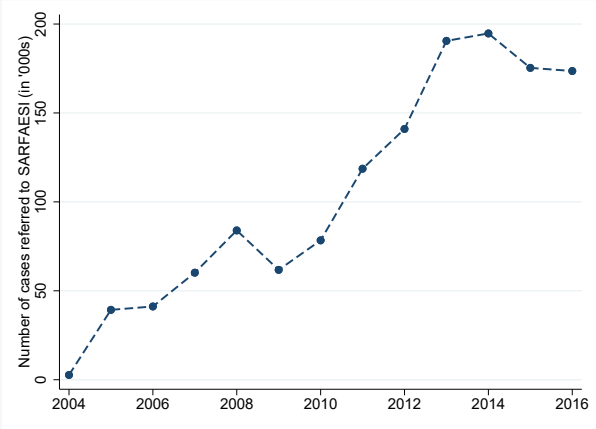
Panel A shows the quarterly auction/possession notices issued by banks under the collateral reform (SARFAESI). Data is from wachoutinvestors.com and is for the 2002 Q4 to 2018 Q2 period. Panel B shows the number of cases filed under the Board for Industrial and Financial Reconstruction (BIFR). Data is hand-collected from the BIFR website for 1997–2007. Panel C shows the number of cases referred to under the SARFAESI. Panel D shows the total amount recovered through SARFAESI (LHS-axis) and of all the avenues available for recovery, the amount recovered through SARFAESI as a percentage of the alternate avenues (Lok Adalats, Debt recovery Tribunals and through SARFAESI). Note, this is not the recovery rate. Data for panels C and D is from the Reserve Bank of India (RBI) for 2004–2016 and is available only starting 2004.



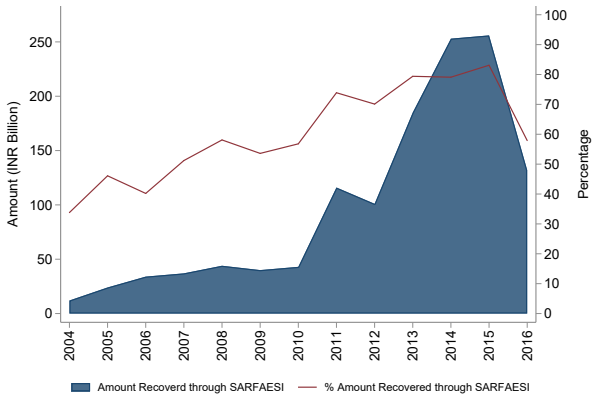
(A) QUARTERLY AUCTION NOTICES



(B) FIXING THE BIFR LOOPHOLE



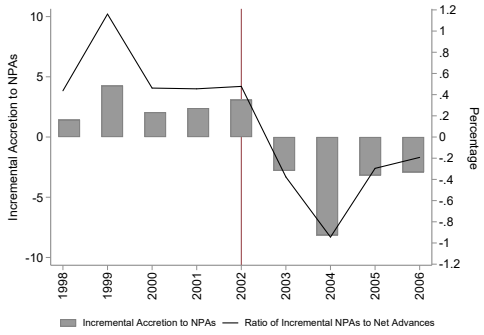
(C) NUMBER OF CASES REFERRED



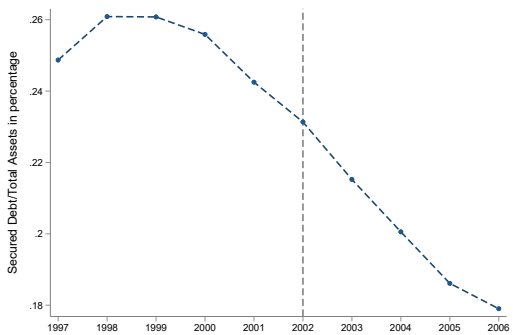
(D) RECOVERY THROUGH THE SARFAESI CHANNEL

Figure II
Aggregate Impact of the Collateral Reform

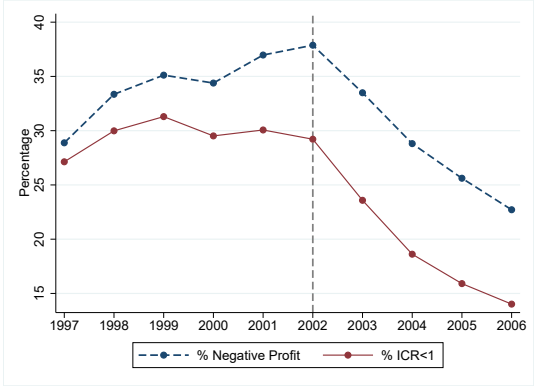
Panel A shows the incremental additions in non-performing assets (NPAs) on the left-hand-side vertical axis and the ratio of incremental NPAs to gross advances on the right-hand-side vertical axis. Data is from the Reserve Bank of India (RBI) and collected from IndiaStat. Data is at the annual level and as of March of each year for 1998–2006. Panel B shows the percentage of secured debt to total assets of firms. Panel C shows the percentage of firms with negative profit and the percentage of firms with interest coverage ratio (ICR) below 1. ICR is the ratio of earnings before interest and taxes to total interest expense. Return on assets is the ratio of earnings before interest, taxes, depreciation, and amortization to total assets. Data for Panel B and C is from Prowess$_{dx}$ for 1997-2006.



(A) NPAs



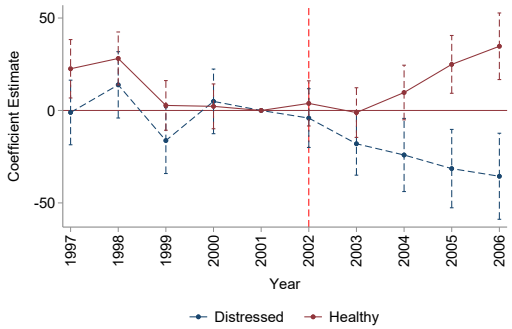
(B) Firm Debt



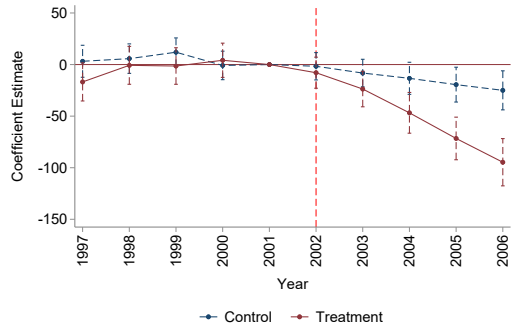
(C) Firm Health

Figure III
Impact of the Collateral Reform on Secured Borrowing

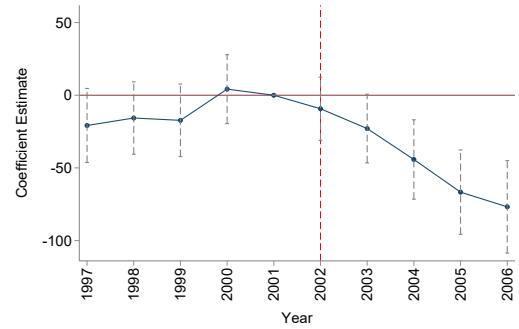
The graphs below show event study plots, as explained in Section IV. The dependent variable is secured borrowing. Secured borrowing is the change in secured debt between the current period and the previous period. Panel A plots the event study coefficients separately for each sub-sample of distressed and healthy firms from Equation 4. The dashed blue line (solid red line) is for the sub-sample of distressed (healthy) firms. Panel B plots the event study coefficients for each sub-sample of treatment (dashed blue line) and control firms (solid red line) from Equation 3. Panel C plots the event study coefficients from Equation 2 for the full sample. Standard errors are clustered at the firm level. The gray bars represent the 5 percent confidence interval. Remaining variables are defined in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.



(A) DISTRESSED VS. HEALTHY



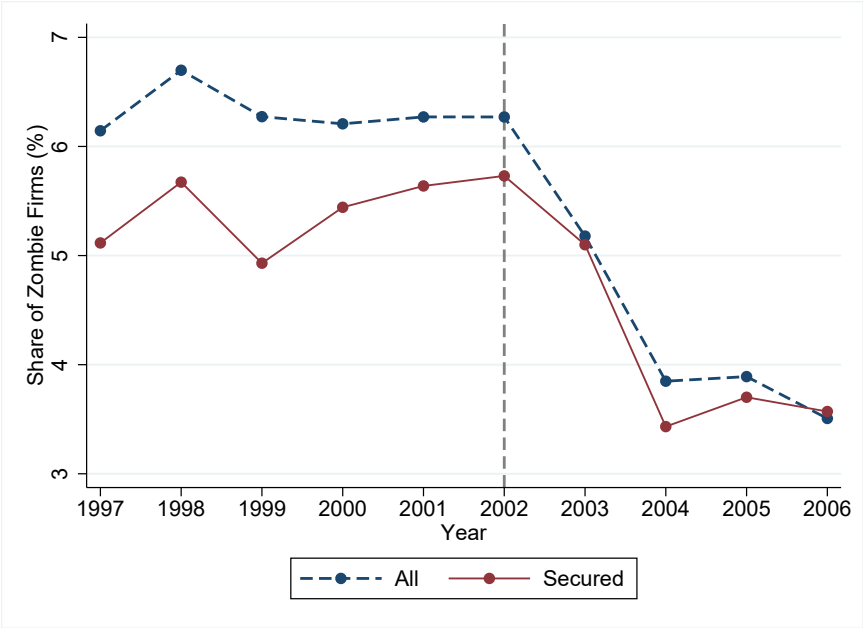
(B) BY TREATMENT AND CONTROL



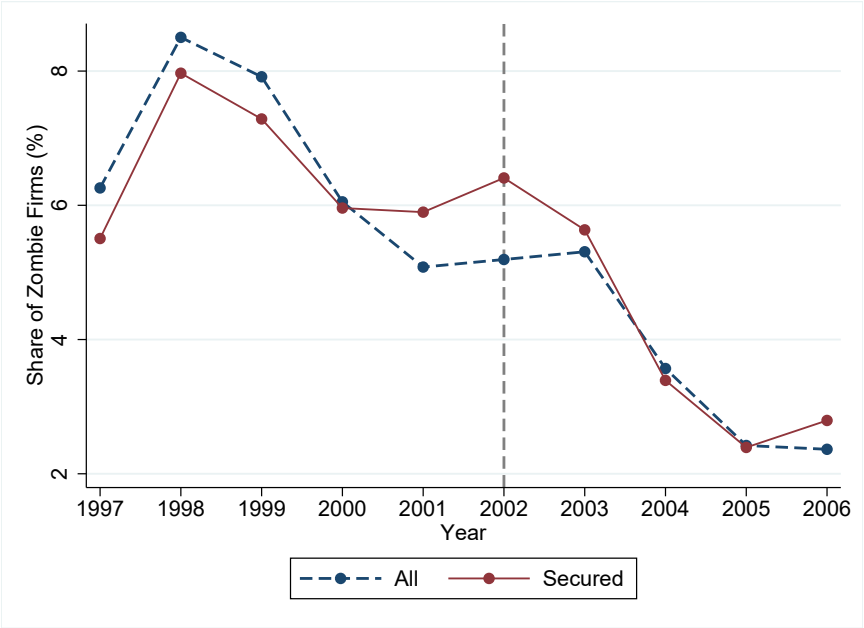
(C) TRIPLE DIFFERENCE

Figure IV
Percentage of Firms Receiving Subsidized Credit

The graphs below show the percentage of firms classified as zombie firms relative to the total number of firms. Panel A plots the raw numbers, and Panel B plots the asset-weighted percentage by total assets. A firm receives zombie credit if it satisfies all of the following conditions: (i) interest rate of the firm is below the minimum prime lending rate, (ii) interest coverage ratio is less than 1, (iii) leverage (total external debt to total assets) is greater than 0.20, (iv) change in debt is greater than zero, and (v) the firm itself is not high-rated. Since the collateral law only applies to secured debt; the alternate zombie classification (red solid line in both panels) modifies condition (iv) to secured borrowings is greater than zero (solid red line in each graph). Data is from Prowess$_{dx}$ for 1997–2006.



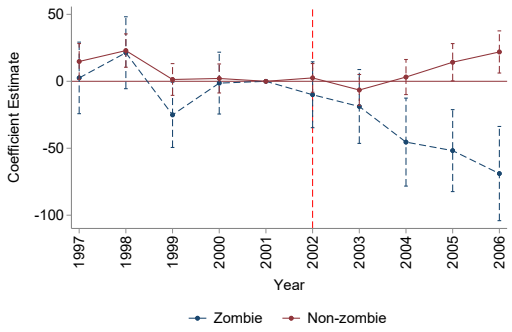
(A) RAW



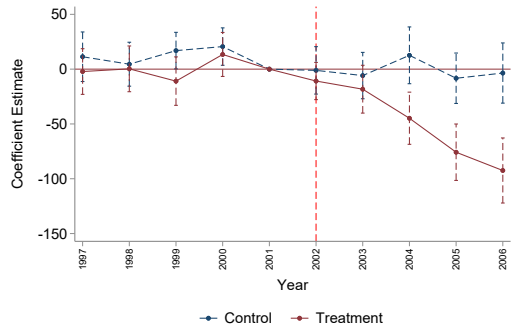
(B) ASSET-WEIGHTED

Figure V
Impact on Zombies

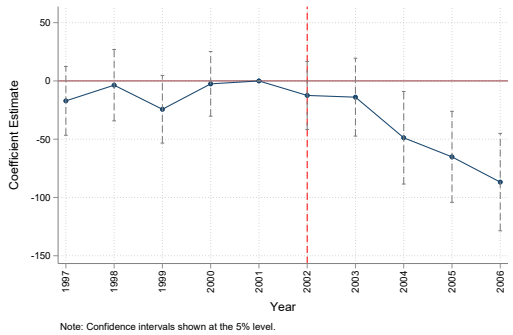
The graphs below show event study plots, as explained in Section IV. The dependent variable is secured borrowing. Secured borrowing is the change in secured debt between the current period and the previous period. Panel A plots the event study coefficients separately for each sub-sample of zombie and non-zombie firms from Equation 4 (with the distressed indicator replaced a zombie indicator). The dashed blue line (solid red line) is for the sub-sample of zombie (non-zombie) firms. Panel B plots the event study coefficients for each sub-sample of treatment (dashed blue line) and control firms (solid red line) from Equation 3 (with the distressed indicator replaced a zombie indicator). Panel C plots the event study coefficients from Equation 2 (with the distressed indicator replaced a zombie indicator) for the full sample. Standard errors are clustered at the firm level. The gray bars represent the 5 percent confidence interval. Remaining variables are defined in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.



(A) Zombie vs. Non-Zombie



(B) By treatment and control



(C) Triple Difference

Figure VI
Impact of the Collateral Reform on Capital Expenditures

The graph below examines the distributive impact of the collateral reform on capital expenditure for zombie versus non-zombie borrowers. The coefficient estimate of the difference-in-difference-in-differences specification in Equation 2 are shown. The dependent variable in is capital expenditure. Capital expenditure is non-negative difference in gross fixed assets between the current period and the previous period. Standard errors are clustered at the firm level. The gray bars represent the 5 percent confidence interval. Remaining variables are defined in Table B1. Data is from Prowess$_{dx}$ for 1997-2006.

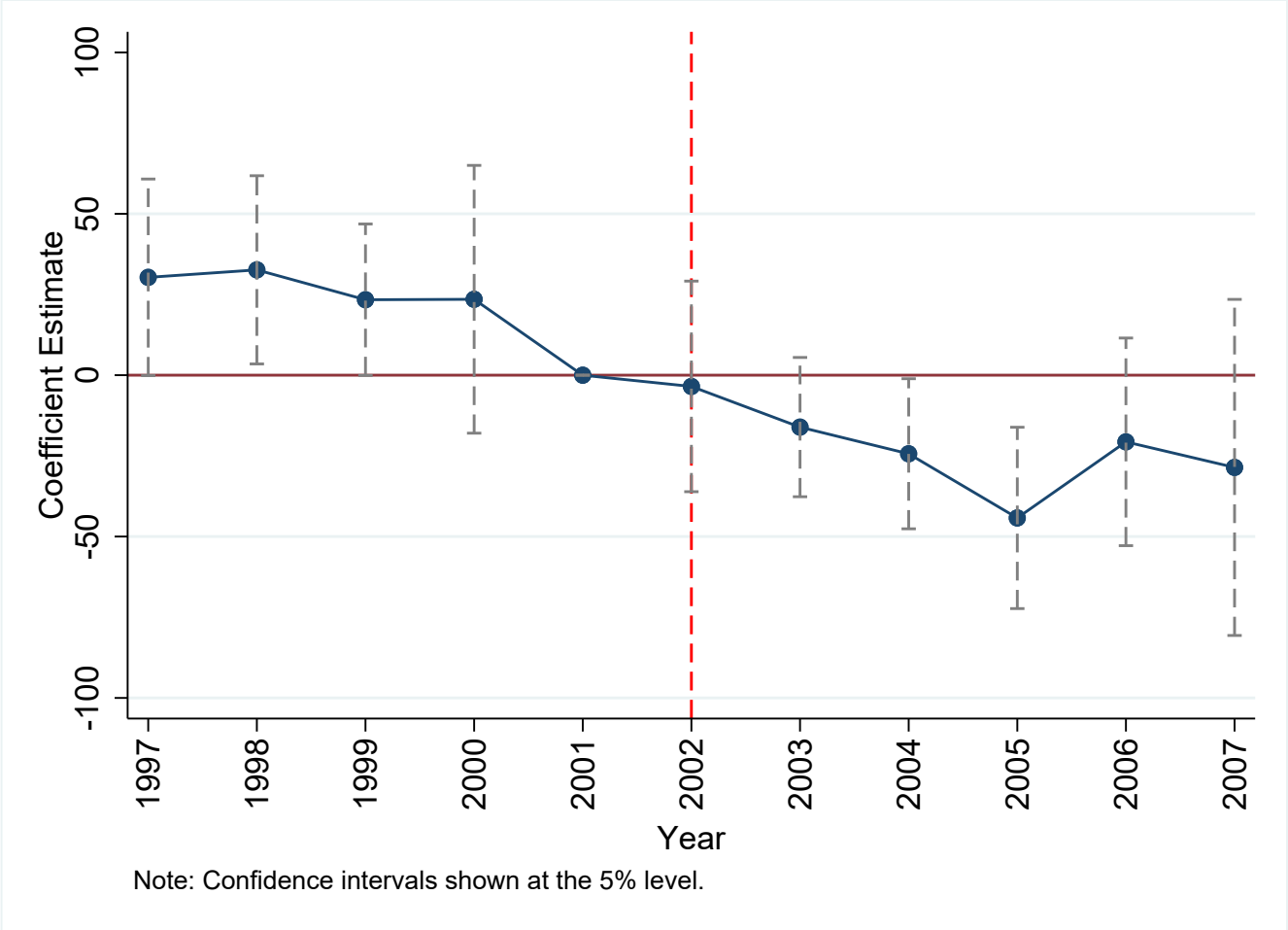


Table I
Descriptive Statistics

This table shows the summary statistics of all the variables. The means and standard deviations are shown in columns 1 and 2. Panel A (B), Columns 3 and 4 show the average for distressed borrowers (zombie firms) in the periods before and after the reform. Panel A (B), Column 5 shows the whether the the pre- (2002 and before) and post-reform (after 2002) periods for the distressed borrowers (zombie firms) are statistically different (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). Panel A (B), Columns 6 and 7 show the average for healthy borrowers (non-zombie firms) in the period before and after the reform. Column 8 shows whether the two are statistically different. Remaining variables are defined in Table B1. Number of observation refer to the data for which all variables below (excluding MRPK, capital wedge, and TFP are available).

Panel A

	All		Distressed			Healthy		
	Mean	SD	Pre	Post	Diff.	Pre	Post	Diff.
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Secured borrowing^{+}	41.11	183.10	50.77	31.26	(***)	31.02	52.48	(***)
Unsecured borrowing^{+}	2.96	16.61	1.59	4.18	(***)	2.06	4.69	(***)
Capital expenditure^{+}	78.01	249.30	57.94	53.79		78.00	102.30	
Log(Sales)	5.32	2.40	4.80	4.86		5.42	5.76	
$\frac{\text{EBITDA}}{\text{Total Assets}}$	0.12	0.41	-0.02	0.11	(***)	0.17	0.17	(***)
MRPK	2.080	31.74	0.520	0.830	(**)	2.470	3.200	(**)
Capital Wedge	12.52	255.4	3.030	5.420	(***)	13.02	21.71	(***)
TFP	2.070	0.950	2.030	2.050		2.090	2.090	
Observations	47,414		15,150			32,264		

Panel B

	All		Zombies			Non-Zombies		
	Mean	SD	Pre	Post	Diff.	Pre	Post	Diff.
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Secured borrowing^{+}	41.11	183.10	60.64	36.22	(***)	32.46	48.04	(***)
Unsecured borrowing^{+}	2.96	16.61	1.69	5.22	(***)	1.95	4.42	(***)
Capital expenditure^{+}	78.01	249.30	68.47	59.21	(*)	71.90	93.17	(*)
Log(Sales)	5.32	2.40	4.70	4.88	(***)	5.32	5.60	(***)
$\frac{\text{EBITDA}}{\text{Total Assets}}$	0.12	0.41	-0.02	0.11	(***)	0.14	0.16	(***)
MRPK	2.080	31.74	0.490	0.810		2.110	2.810	
Capital Wedge	12.52	255.4	2.960	4.890		11.14	19.03	
TFP	2.070	0.950	2.020	2.080	(**)	2.080	2.080	(**)
Observations	47,414		8,130			39,284		

$^{+}$ *INR million*; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table II
Impact of the Collateral Reform on Borrowing

This table examines the impact of the collateral reform on secured borrowing for distressed and healthy firms. The dependent variable in all columns is secured borrowing. Post, Distressed, and Treatment are the indicator variables for the post-reform period, distressed borrowers, and treatment firms, respectively. Fixed effects are as indicated. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Sample of firms in each column is as indicated. Columns 1 and 5 subset to only distressed firms, columns 2 and 6 subset to only healthy firms, and remaining columns include all firms. Baseline mean is calculated for the distressed (healthy) firms in the period before the reform in columns 1, 3–5, and 7–9 (columns 2 and 6). Remaining variable definitions are in Table B1.

Dependent variable:	Secured borrowing							
	Distressed firms	Healthy firms	All		Distressed firms	Healthy firms	All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-25.64***	16.02***						
	(4.064)	(2.396)						
Distressed * Post			-47.11***	-43.18***			-20.12***	-16.97***
			(4.649)	(4.506)			(5.281)	(5.043)
Treatment * Post					-32.52***	11.62**	11.80**	8.465*
					(7.007)	(5.056)	(5.104)	(4.871)
Distressed * Treatment * Post							-43.94***	-41.90***
							(8.631)	(8.205)
Baseline Mean	50.77	31.02		50.77		31.02	50.77	
No. of Obs.	15150	32264	47414	47414	15150	32264	47414	47414
R-sq.	0.420	0.342	0.367	0.383	0.422	0.345	0.367	0.383
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	N	N	Y	Y	N	N	Y
Controls	Y	Y	N	Y	Y	N	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table III
Impact of the Collateral Reform on Zombie Lending

This table examines the impact of the collateral reform on secured borrowing for zombie and non-zombie firms. The dependent variable in all columns is secured borrowing. Post, Zombie, and Treatment are the indicator variables for the post-reform period, zombie firms, and treatment firms, respectively. Fixed effects are as indicated. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Sample of firms in each column is as indicated. Baseline mean is calculated for the zombie (non-zombie) firms in the period before the reform in columns 1, 3–5, and 7–9 (columns 2 and 6). Remaining variable definitions are in Table B1.

Dependent variable:	Secured borrowing								
Sample:	Zombie firms	Non- zombie firms	All		Zombie firms	Non-zombie firms	Non-zombie excl. distress firms	All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	-31.22***	14.78***							
	(5.956)	(2.267)							
Zombie * Post			-45.97***	-43.11***				-16.57**	-12.40*
			(6.383)	(6.119)				(7.444)	(6.745)
Treatment * Post					-41.14***	2.337	11.78**	2.478	0.471
					(10.62)	(4.510)	(5.344)	(4.512)	(4.241)
Zombie * Treatment * Post								-43.52***	-45.14***
								(11.52)	(10.71)
Baseline Mean	60.64	32.46		60.64		32.46		60.64	
No. of Obs.	8130	39284	47414	47414	8130	39284	30340	47414	47414
R-sq.	0.438	0.343	0.366	0.382	0.441	0.346	0.336	0.366	0.382
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	N	N	Y	Y	N	N	N	Y
Controls	N	N	N	Y	Y	N	N	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table IV
Within-firm variation using collateralized loan-level data

This table examines the impact of the collateral reform on collateralized borrowing using the loan level data from the Ministry of Corporate Affairs. Data is at the firm-bank-year level. The dependent variable in columns 1, 3, 5, 7, and 9 is whether a firm received a loan from a given bank in a given year. The dependent variable in columns 2, 4, 6, 8, and 10 is amount of total collateralized borrowing of a firm from a given bank in a given year. Column 11 subsets to loans originated in the pre-reform period between 1997 to 2001 and the dependent variable is whether a firm repays the loan between 2002 to 2007. Post is an indicator variable for the post-reform period. Zombie relationship is 1 if a firm is classified as a zombie at the firm level and receives a loan from the given bank in the pre-reform period. Fixed effects are as indicated. The baseline mean for each dependent variable in the pre-period for the zombie relationship firms is as indicated. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Bank characteristics examined (distressed bank, state-owned bank, and publicly listed private bank) are as indicated in columns 5–10. Remaining variable definitions are in Table B1.

Lender characteristic					Distressed bank		State-owned bank		Publicly listed private bank		
Dependent variable:	$\mathbb{1}_{Loan}$	Vol.	$\mathbb{1}_{Loan}$	Vol.	$\mathbb{1}_{Loan}$	Vol.	$\mathbb{1}_{Loan}$	Vol.	$\mathbb{1}_{Loan}$	Vol.	$\mathbb{1}_{Repaid_{02--07}}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Zombie$_{Reln}$ * Post	-0.09***	-13.14***	-0.32***	-39.34***	-0.36***	-48.20***	-0.35***	-36.76**	-0.31***	-39.28**	
	(0.02)	(3.86)	(0.04)	(11.70)	(0.04)	(12.83)	(0.05)	(14.46)	(0.04)	(15.16)	
Zombie$_{Reln}$ * Bank char.					-0.01	12.86	-0.00	-18.10	-0.02	16.04	
					(0.03)	(20.79)	(0.04)	(15.99)	(0.04)	(17.89)	
Zombie$_{Reln}$ * Post * Bank char.					0.13**	14.13	0.04	14.71	-0.05	-16.35	
					(0.05)	(25.04)	(0.04)	(17.28)	(0.05)	(18.78)	
Zombie$_{Reln}$											-0.04***
											(0.01)
Baseline Mean	0.32	43.12	0.32	43.12	0.32	43.12	0.32	43.12	0.32	43.12	
No. of Obs.	31580	31580	31580	31580	31580	31580	31580	31580	31580	31580	9403
R-sq.	0.276	0.299	0.461	0.408	0.462	0.408	0.461	0.408	0.461	0.408	0.0126
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Firm-Year FE	N	N	Y	Y	Y	Y	Y	Y	Y	Y	
Bank-Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	
Firm, Bank-loan vintage year											Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table V
Impact on Investment

This table examines the impact of the collateral reform on capital expenditure for zombie and non-zombie firms. The dependent variable is capital expenditure in all columns. Capital expenditure is non-negative difference in gross fixed assets between the current period and the previous period. Post, Zombie, and Treatment are the indicator variables for the post-reform period, zombie firms, and treatment firms, respectively. Firm, year, industry-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Columns 1 and 5 subset to only zombie firms, columns 2 and 6 subset to only non-zombie firms, and remaining columns include all firms. Baseline mean is calculated for the zombie (non-zombie) firms in the period before the reform in columns 1, 3–5, and 7–10 (columns 2 and 6). In columns 9 (10), I instrument for secured debt with the Post, Zombie (Zombie Relationship), and Treatment and their interactions. Zombie relationship is 1 if a firm is classified as a zombie at the firm level and receives a loan from the given bank in the pre-reform period. Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ from CMIE in columns 1–8 and from the Ministry of Corporate Affairs (MCA) in columns 9–10 for 1997–2006.

Dependent variable:	Capital Expenditure									
	Zombie firms	Non-zombie firms	All		Zombie firms	Non-zombie firms	All			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Secured Borrowing									0.772***	0.657***
									(0.114)	(0.105)
Post	-24.19***	17.99***								
	(5.822)	(2.551)								
High-tangibility * Post					-25.87**	9.721*	10.14**	9.815*		
					(12.74)	(5.074)	(5.076)	(5.067)		
Zombie * Post			-41.45***	-34.47***			-17.24	-9.923		
			(6.327)	(6.267)			(11.19)	(9.976)		
Zombie * Post * High-tangibility							-37.97***	-38.04***		
							(13.76)	(12.67)		
Baseline Mean	68.47	71.90		68.47		71.90		68.47		
No. of Obs.	8130	39284	47414	47414	8130	39284	47414	47414	47274	17063
R-sq.	0.551	0.631	0.621	0.636	0.558	0.634	0.621	0.636	.	.
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	N	N	Y	Y	N	N	Y	Y	Y
Controls	N	N	N	Y	Y	N	N	Y	N	N
First Stage F-statistic									99.11	56.60
Data	CMIE	CMIE	CMIE	CMIE	CMIE	CMIE	CMIE	CMIE	CMIE	MCA

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VI
Reallocation Mechanisms

This table reports results for the difference-in-difference-in-differences specification in Equation 12. The dependent variable is secured borrowing. Secured borrowing is the change in secured debt between the current period and the previous period. Post, Non-zombie, Bank Exposure and Industry Exposure are the indicator variables for the post-reform period, non-zombie firms, whether a firm is linked to high exposure banks and or is in high exposure industries, respectively. Bank exposure is based on the fraction of zombie firms in a bank's portfolio. If this value is in the top two quintiles, then bank-exposure is 1. Industry Exposure is set to 1 for industries with average firm asset tangibility in 2000 is above median value. Firm, year, industry-year, and bank-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the level of randomization: at the bank-level in column 1, at the industry-level in column 2, and are two-way clustered at the bank-level and industry-level in column 3. Baseline mean is calculated for the non-zombie firms in the period before the reform in columns 1–3. Remaining variable definitions are in Table B1. Only firm-year observations for which information on the lending bank is available are included in this analysis. Data is from Prowess$_{dx}$ for 1997–2006.

Dependent variable:	Secured borrowing		
	(1)	(2)	(3)
Non-zombie * Post	-5.011		-9.688
	(11.90)		(11.21)
Non-zombie * Bank Exposure * Post	31.98**		29.14**
	(14.44)		(11.78)
Non-zombie * Industry Exposure * Post		23.18**	8.858
		(11.03)	(8.105)
Baseline Mean		32.46	
No. of Obs.	14601	14565	14510
R-sq.	0.468	0.475	0.499
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Industry-Year FE	N	Y	Y
Bank-Year FE	Y	N	Y
Controls	Y	Y	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VII
Impact on Capital Wedges, Marginal Productivity of Capital and TFP

This table reports results for the specification in Equation 24 in columns 1–5 using OLS and Equation 21 in columns 6–7 using an instrumental variable strategy. The dependent variable is capital wedge for columns 1–3, MRPK for column 4, TFP for column 5 and growth in MRPK for columns 6–7. Non-Zombie, Post and Industry Exposure are indicator variables for a non-zombie firm, the post-reform period and whether a firm is in high exposure industries, respectively. Calculation of capital wedges, MRPK (marginal product of capital) and TFP (firm-level productivity) are described in Section A1. Pre-reform Capital Wedge is the average MRPK for a firm in the pre-reform period, 1997–2001. Remaining variable definitions are in Table B1. Columns 1–5 include all firms-year observations and columns 6–7 include only the firm-year observations in the post-reform period. Industry-year fixed effects are included in all columns and firm and year fixed effects are also included in columns 1–5. Standard errors are clustered at the industry-level in columns 1–5 and at the firm-level in columns 6–7. In columns 1–5 only firm-year observations for which information on the lending bank is available are included in the analysis. Columns 6–7 use all firm-year observations for which the data is available. Data is from Prowess$_{dx}$ for 1997–2006.

Dependent Variable	Capital Wedge			MRPK	TFP	MRPK growth	
Sample:	All	High Bank Exposure	Low Bank Exposure	All		Post	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-Zombie*Industry Exposure*Post	-4.203***	-4.655***	-2.635	-0.559***	-0.0620		
	(1.295)	(1.605)	(1.743)	(0.186)	(0.0375)		
Secured Borrowing						-0.0497	-0.0382
						(0.0458)	(0.0368)
Secured Borrowing * Pre-reform Capital Wedge							-0.165**
							(0.0706)
No. of Obs.	14750	11744	3006	14750	14750	17978	17978
R-sq.	0.744	0.720	0.798	0.749	0.980	.	.
Firm FE	Y	Y	Y	Y	Y		
Year FE	Y	Y	Y	Y	Y	Y	Y
Industry-Year FE	Y	Y	Y	Y	Y	Y	Y
Type	OLS	OLS	OLS	OLS	OLS	IV	IV
First Stage F-statistic						18.045	7.319

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VIII
Effect on Allocative Efficiency and Productivity

This table reports the effects of the collateral reform on allocative efficiency. The top row in Panel A presents the estimates of the change in allocative efficiency calculated from Equations 16 and 17 using Equation 23. The bottom row in Panel A shows the actual change in allocative efficiency calculated using the raw data and Equations 16 and 17. Panel B shows the change in allocative efficiency due to the credit reallocation from zombie to non-zombie firms. It presents the results of a simulation exercises that sets the total credit to zombie firms to equal the total credit to 10,000 random draws of non-zombie firms as described in Section VII and Mean, Min, and Max refer to the average, minimum and maximum from the simulation exercise using 10,000 draws. Panel C presents robustness to the following alternative channels as described in Section VII: the indirect channel (mean from a simulation exercise), the zombie reallocation channel excluding young, unlisted, low-rated and small firms (mean from a simulation exercise), the demand channel (aggregate estimate), and the demand channel combined with the creditor rights channel (mean from a simulation exercise). Data is from Prowess$_{dx}$ for 1997–2006.

Panel A: Total Effect of Collateral Reform on Allocative Efficiency and Productivity

	Estimate
ΔAllocative Efficiency (1)	18.70%
% of total [= (1)/(2)]	63.78%
Actual from raw data (2)	29.32%

Panel B: Effect Attributable to Reallocation from Zombie to Non-zombie borrowers

	Mean	Min	Max
Δ Allocative Efficiency (3)	17.56%	6.29%	18.92%
% of est. effect = (3)/(1)	93.90%	33.63%	101.17%

Panel C: Robustness to Alternative Channels

ΔAllocative Efficiency	Mean or Estimate
Indirect channel	-0.5%
Zombie Reallocation excluding young, unlisted, low-rated, and small firms	17.09%
Demand channel	-15.73%
Demand channel + Creditor rights	-0.15%

Internet Appendix

A1 DATA

This paper uses primary data from Prowess$_{dx}$, which is maintained by the Centre for Monitoring Indian Economy (CMIE). Prowess$_{dx}$ provides data on annual financial statements for Indian firms and includes both listed and unlisted firms. I exclude government-owned firms and foreign entities and focus on the period between March 1996 to March 2006, from the March 2016 vintage. Table B1 describes all the variables used in this paper. These variables have been winsorized at the 2 percent level for analysis. There are 47,414 firm-year observations of which 45 percent belong to listed firms. Data is as of the fiscal year ending on March 31st. The fiscal year of 2003, immediately following the official date of the law (June 21st 2002), is taken as the first year the law is in effect.

The data on bankers are extracted from CMIE as a separate dataset that gives the name of the bank, which is the banker to the firm in that particular year. A firm may have more than one banker in a year. The data field "Order" stores a number that determines the order in which the banks appear in the source document (i.e., the annual report of the company). In case of multiple bankers, I retain the top banker based on "Order no." as it is assumed that firms display the name of their most important banker or the bank that has the highest exposure to the company at the top of the list. The top bank is assigned to a firm. This assignment makes a strong assumption that all increase in debt of a firm is from the top banker. Note data on bankers is only available for a subset of 11,556 firm-years. This is matched to bank-level information from the Reserve Bank of India (RBI).

Since employment data is not well populated in Prowess$_{dx}$, I also supplement with data from the Annual Survey of Industries (ASI). ASI is a survey conducted by the Ministry of Statistics and Program Implementation (MoSPI) in India and provides information on industrial units of manufacturing firms employing 10 or more workers and using electricity. Dates for DRT establishment for the analysis in the robustness checks come from Lilienfeld-Toal et al. (2012). Data on court efficiency from Alok et al. (2018) is the fraction of trials disposed in less than one year in the district/sessions court.

Zombie definition: To define zombie credit, I build on Caballero et al. (2008). Caballero et al. (2008) define a zombie firm as one whose interest payment is lower than the interest payment for the credit-worthy firms. The proxy for interest payment for the credit-worthy firms is the prime lending rate for the most credit-worthy firms. Fukuda and Nakamura (2013) point to several weaknesses of this measure. One, the Caballero et al. (2008) definition does not take into account the evergreening of loans. Two, during periods of weak demand, banks could offer interest rates below their prime lending rates. Three, interest payments data is from the annual financial statements, and a small interest amount could simply reflect low firm leverage. Hence, I modify the Caballero et al. (2008) definition to address the above criticisms. A firm in my sample is classified as a zombie if it satisfies all of the following conditions: (i) interest rate of the firm is below the minimum prime lending rate, (ii) ICR is less than 1, (iii) leverage (total external debt to total assets) is greater than 0.20, (iv) change in debt is greater than zero, and (v) the firm itself is not high-rated. The first item is analogous to the baseline definition in Caballero et al. (2008). The prime lending rate is the lowest among the prime lending rates of State Bank of India (the largest public sector bank in India), ICICI (one of the largest private

sector banks in India), and IDBI (a development bank). The second and third items address the concern that interest payments may be low because the firm has low leverage. Hence the second item accounts for interest payments relative to its profitability, and the third item directly includes criteria for leverage. The last item accounts for evergreening, that is, that the lender was rolling over or extending credit.

A1.A Description of loan-level data

For the loan-level analysis, I use a new data source on loan charges from the Ministry of Corporate Affairs (MCA). When a company obtains a loan from a bank or a financial institution against collateral, a charge is applied to the loan. This charge has to be electronically filed with the MCA by companies and limited liability partnerships. These charges are self-declared and can be either audited or unaudited, though companies are mandated to file them. This data is publicly available under the "Index of Charges" in the MCA website and allows lending firms to track the assets pledged by a firm and thus avoid double financing. Companies are required to register the charge within 20 days of the creation of the charge and are required to make the filing of the registration of the charge up to 300 days from the date of creation of the charge. In addition to the loan charges, the company also declares the loan amount and the name of the financial institution.

I obtain information on loan amount, the name of the financial institution, and the date the loan was granted from the MCA website using a Company Identification Number (CIN). I use the CIN from the Prowess data to obtain the loan-level information from the MCA website. The MCA includes even very small firms that are not captured by Prowess. Additionally, not all firms in the Prowess have a corresponding match in MCA if (i) the firms did not borrow in a given period, or (ii) if there are errors in the matching process. In the baseline, I only retain Prowess firms which were matched at least once to the MCA database. As a robustness check, I also retain all firms in the baseline in Prowess and set the amount borrowed to zero if there are no matches. To obtain bank-level information, I manually match the bank names to the data on scheduled commercial banks from the Basic Statistical Reports (BSR), available from RBI. Thus only firms that borrow from scheduled commercial banks are retained for this part of the analysis. The final cleaned data consists of matched firm-bank information on collateralized loans.

Since the data is filed manually by companies and banks, there are often instances where, due to human error, there are duplicate observations in the data. A report published by the RBI (Reserve Bank of India (2008)) notes that inaccuracies exist in the data due to filing errors. I deal with these inaccuracies in the following way. Each charge has a unique charge identification number. First, I drop duplicate observations with duplicate charge identification numbers. Second, there are instances where the charge identification number is different, but loans amounts, bank name and date are the same. Since this likely refers to duplicate entries, I take a conservative approach and drop observations where the date of taking the loan, loan amount and the CIN appears more than once. I then form a balanced panel and aggregate up to the bank-firm-year level to get a total of 31,580 observations.

A1.B Estimating marginal product of labor, capital wedges, and TFP

I estimate the TFP residuals and factor-wise edges for the subset of manufacturing non-financial companies for the time period 1988–2020 using firm's financial data from CMIE's Prowess$_{dx}$

database. I first derive a NIC 2-digit sector-wise production function for the entire time period (1988-2020) using a Cobb-Douglas production function with a Hicks-neural productivity.

$$Y_{it} = F(K_{it}, L_{it}, M_{it}) * exp(\omega_{it})$$

where F(.) is the production function and ω is log(TFP). I use a 2-stage IV GMM estimation akin to Levinsohn and Petrin (2003) to estimate a revenue production function and then obtain the labour and capital coefficients of a log-linearized production function. I rely on the annual financial statements of non-financial firms and use sales as the revenue (PY), net fixed capital as capital (K), compensation to employees as (L) and raw material expense, stores and spares as (M). Since the "number of employees" variable is not well populated in the Prowess$_{dx}$ database, I impute the average industry wage (W) by taking a simple average of industry-wide ratio of compensation to employees to number of employees with the available data. I similarly impute industry-wide interest rate (R) by averaging the ratio of interest expense and debt at the industry-level.

Marginal products of capital and labor are calculated as:

$$MRPK_{it} := \alpha_s \frac{p_{it} Y_{it}}{K_{it}}$$
$$MRPL_{it} := \alpha_s \frac{p_{it} Y_{it}}{L_{it}}$$

I use the following equation derived from the first order conditions to estimate wedges as in Bau and Matray (2020):

$$\tau_{K_i} \equiv (1 + \hat{\tau}_{K_{it}})\mu_i = \frac{\alpha_s}{R} \frac{P_{it} Y_{it}}{K_{it}}$$
$$\tau_{L_i} \equiv (1 + \hat{\tau}_{L_{it}})\mu_i = \frac{1 - \alpha_s}{w} \frac{P_{it} Y_{it}}{L_{it}}$$

where μ is the output-wedge and $(1 + \tau)$ is the factor misallocation. I take the product of factor and output wedge as the combined wedge for the baseline analysis.

TFP is obtained by predicting the first-stage residuals of the IV GMM estimation, which helps in predicting expected output as described in De Loecker and Warzynski (2012). TFP is then obtained as the difference between expected output and realized output.

A2 Additional robustness results

Alternate specifications and choice of dependent variable: One concern is that the distress measure captures borrowers with future investment opportunities, even if they are currently unable to service their debt. Column 1 in Table B2 addresses this concern by controlling for investment opportunity with Tobin's q. Results are similar to the baseline.

Columns 2–3 in Table B2 use alternative variations of the dependent variable. Column 2 normalizes the dependent variable by assets, and column 3 uses the log of secured borrowing as the dependent variable. Both estimates confirm the relative decline in borrowing for distressed borrowers, and point estimates are comparable to the baseline. The baseline uses the change in debt as the dependent variable, which allows for easy interpretation of the rupee

magnitude of the effect and tables include baseline means to get a sense of relative magnitudes. The choice of levels of secured borrowing (a flow variable) is driven by the presence of pre-trends in the debt and debt to assets (stock) variables. As Angrist and Pischke (2009) note, the parallel trends assumption can be applied to transformed data, but parallel trends in logs rule out parallel trends in levels and vice versa. Hence, the underlying assumptions required to causally interpret the estimates in columns 2–3 in Table B2 need not necessarily hold. Results are robust using an alternate non-linear changes-in-changes method introduced by Athey and Imbens (2006). This method provides a semi-parametric estimate that does not rely on functional forms assumptions and is thus robust to transformations of the dependent variable. Using the changes-in-changes method, I show that the zombie versus non-zombie estimate is negative and significant (b=-61, s.e.=7.7) for treatment firms, whereas the estimate is insignificant for control firms (b=-11, s.e.=8.37). In the DDD sense, the difference between these two estimates is also negative and significant, confirming that the results are not sensitive to the choice of the functional form of the dependent variable.

Potential shortcomings of the distress and zombie measure: The analysis in this paper focuses on identifying firms that receive subsidized credit before the reform and then traces the impact on lending to these firms. The distress measure is based on a firm's ability to service debt since the central hypothesis in this study focuses on pre-reform lending quality. Similarly, zombie borrowers are identified based on whether a firm receives subsidized credit at rates below those for the most creditworthy firms in the economy. This definition is based on Caballero et al. (2008) and Acharya et al. (2019). See Section III for further detail. Banerjee and Hofmann (2018) instead argue that a profitability based measure may be more suited to identify zombies since: (i) in a low-interest rate environment, subsidized lending rates are low, which could potentially confound results, (ii) such lending may simply reflect long-standing lending relationships. While the criticism of a low-interest-rate environment is relevant for the period after the global financial crisis, the late 1990s and early 2000s were a period of relatively high repo rates in India. Additionally, since the DDD specification compares the relative difference in lending to zombie and non-zombie firms, it differences out the effects common to both sets of firms. Any *relative* effect on the zombie versus non-zombie difference is taken care of by comparing the treatment to the control groups. Also, note the distinction between zombie credit and zombie firms. While other studies use profitability based definitions (Banerjee and Hofmann, 2018), the baseline definition based on credit is motivated by Section II that focuses on the effect of the reform on zombie lending.

Despite the suitability of using credit-based zombie measures in my analysis, several robustness tests look at alternative definitions of zombie borrowers and distressed firms. Columns 4–7 in Table B2 look at alternate definitions of distress. Results are robust to using a profitability-based distress measure (column 4). A potential concern of the distress measure is that low ICR captures young firms with high future investment opportunities. Columns 5 defines distress based on investment opportunity and effects are similar. Column 6 shows that results are also robust to defining distress based on the ICR in 2001 (as opposed to the average of 2000 and 2001, which is more stable). Does the baseline measure merely capture industry-level variation in distress? Effects are similar even when I classify firms as distressed if they have below-median ICR *within* an industry (column 7). A further potential concern is that the distress measure only captures temporary shocks to firms and lenders with long-standing relationships with borrowers may have been exploiting soft information to continue lending to these firms pre-reform, if say, they believed that those projects have positive net present value. These would, however,

bias the estimates upward since secured lending to zombie firms goes down post-reform. Results are robust even when I use a very stringent definition and classify a firm as distressed only if it has persistently low ICR throughout the pre-reform period (column 8).

Results are also robust to using alternative definitions for zombie borrowers. Figure C1 Panel A shows the DDD estimates, and grey bars represent 5 percent confidence intervals using alternative zombie definitions. I consider different combinations of the conditions in the baseline zombie definition (shown by the vertical solid red line for reference). The first two definitions address the criticism that zombie credit merely reflects macroeconomic trends in interest rates. Definition 1 and definition 2 are both based on debt serviceability and hence take into account the interest payments relative to *current profitability*. These zombie classifications also include the condition that firms continue to receive loans pre-reform, and hence they differ from the baseline distress measure. Since the collateral reform only applies to secured borrowing, definition 2 defines a zombie firm based on only its debt serviceability (ICR<1) and whether it continued to receive *secured* loans before the reform. The prime lending rate for definitions 3–7 is the prime lending rate at the State Bank of India, the largest state-owned bank in India. Definition 3 uses the baseline definition in Caballero et al. (2008) and adds the condition that the firm is not high rated. Definition 4 adds the criteria that ICR is less than 1 to definition 3. Interest payments used to calculate interest rates, may be low if firms have low leverage. Hence, definition 5 also adds that leverage should be above 20 percent to definition 4. Definition 6 adds the evergreening criteria; that is, new borrowing is greater than 0 in a given year. Definition 7 modifies definition 6 with the evergreening condition based on whether *secured* new borrowing is greater than zero in a given year. Definitions 8–10 repeat definitions 5–7, except the minimum lending rate is the minimum lending rate of the most credit-worthy public sector bank (State Bank of India), the most credit-worthy private sector bank (ICICI), and a development bank (Industrial Development Bank of India) as in the baseline. Definition 9 modified the baseline definition and adds a negative profitability criteria. Definition 9 addresses the Banerjee and Hofmann (2018) concern that zombie criteria should be based on profitability measures. Definition 10 augments the baseline to include non-zero growth in *secured* borrowing. Estimates are all significant and negative in magnitude, using all 10 alternative definitions. Absolute magnitudes are smaller (see definition 3 and arguably 4) when we leave out the condition that new borrowing should increase pre-reform, further bolstering the assumption that the effects are driven by firms that continued to receive subsidized credit before the reform.

Zombie lending before the reform: The baseline hypothesis shows that zombie lending declined after the reform because it cut continued lending to zombie borrowers post-reform. Table B4 analyzes lending during the pre-reform period. Before the reform, a distressed firm was 14 percent more likely to receive zombie credit in the next period (column 1). Lending to a firm classified as a zombie firm also increased by an average of INR 42 million in the subsequent year (column 3). These effects were also larger for firms with more collateral (columns 2 and 4). These effects are consistent with the idea that the older, publicly listed firms, that is, possibly more established firms were more likely to receive zombie credit. However, one concern is that this could potentially confound the baseline results. Identification, however, only requires that the trends in lending would be similar absent the treatment and control groups which was shown above and in Figure V.

Is it about distress or subsidized credit? Table B5 links the results in Table II to Table III. I show that even when we subset to only the distressed firms, and compare zombie firms to non-

zombie but distressed firms, effects on secured borrowing are much higher for zombie firms (which received subsidized lending pre-reform). Secured borrowing of zombie firms declines 3.6 times more than distressed but non-zombie firms (columns 1–2). Effects are similar in the pooled regression (columns 3–4) confirm and zombie borrowers see a relative decline of INR 33 million compared to distressed non-zombie firms. Column 5 shows that zombie firms see a decline in secured borrowing of INR 53 million, whereas distressed non-zombie firms see a much smaller decline of INR 16 million (column 6). The baseline zombie measure is motivated by prior literature (Caballero et al., 2008) and captures one aspect of subsidized credit based on whether a firm is receiving subsidized credit. However, this classification may miss firms that receive other forms of subsidized credit not necessarily reflected in interest rates. Hence column 7 restricts to distressed firms (have a median ICR of less than one in 2000 and 2001) that *do not* receive any additional credit. This classification may misclassify some non-zombie firms as zombies but is useful for the current exercise as it does not inadvertently include zombie firms. Using this definition, non-zombie firms do not see declines in secured borrowing. Distressed firms that do not receive subsidized credit pre-reform do not see a similar reduction. The DDD estimates in Columns 8–9 show similar effects. Comparing the DDD estimate of INR 41 million in column 9 to the DDD estimate of INR 42 in column 8 of Table II, we see that nearly 97 percent of the decline in secured debt is attributable to the decline in zombie lending.

Potential Threats to Identification: The empirical strategy uses variation in ex-ante asset tangibility to get variation in the intensity of treatment. One threat to this identification strategy is that any other macroeconomic factors differentially affecting treatment and control firms could explain the baseline results. I address this using two alternate identification strategies in Table B6, columns 3–6. As Section I describes, NBFCs were not allowed to seize collateral under the new collateral reform. Columns 3–4 exploits this in an alternative identification strategy wherein firms that borrow from banks form the treated group and firms that borrow from NBFCs form the control group. The decline in secured borrowing is driven by the firms that primarily borrow from banks, that is, whose lead lenders are non-NBFCs. The DDD estimate of INR 41 million is very similar to our baseline estimates. Since there was some initial confusion as to whether NBFCs were allowed to seize collateral under the reform, I do not use this identification as the baseline strategy. Columns 5–7 exploits prevailing court efficiency in an alternative strategy. The reform allowed borrowers to bypass the judicial courts. Hence, effects should be higher in states with more congested courts before the reform. Indeed, the decline in secured borrowing is higher in states that witness a greater increase in court efficiency. Firm operations can span more than one state and hence this is not the baseline specification.

Are borrowers able to switch into other forms of debt or equity? Borrowers are not able to undo the decline in credit by switching to other forms of credit, such as unsecured debt (Table B7, column 1). Zombie borrowers are also not able to raise equity (column 2). Neither are they able to issue commercial paper or increase their trade credit (Table B7, columns 3–4). The null effect on unsecured borrowing also helps address another potential threat to identification: the baseline DDD estimate is confounded if other factors (such as lending) that differentially affect treatment and control firms also changed at the same time. The results on unsecured borrowing can be viewed as a placebo test showing that there is no differential impact on unsecured borrowing since the collateral reform only pertains to collateralized borrowing and should only affect secured borrowing. This also supports the credit supply hypothesis because as Vig (2013) theoretically shows, a demand-side effect would imply that borrowers switch to

unsecured debt.[19]

Impact on firm entry and exits: In Caballero et al. (2008) show how zombie presence can dampen creative destruction keeping alive severely impaired borrowers and preventing the birth of new firms. Analogously a reduction zombie firms can encourage the creative destruction process, that is, firm death and entry. Table B13 shows the impact on firm births and deaths at the industry-level for industries that witnessed greater zombie decongestion. Firm birth is based on the year of incorporation from the Prowess$_{dx}$ database and firm death is coded when a firm leaves the database. We see negligible effect on the entry of new firms (column 1) and on firm deaths (column 2) in industries with greater exposure. Since the collateral reform allowed lenders to directly seize the collateral securing loans and not necessarily accelerate firm liquidations, we see muted effects on this extensive margin.

A3 Alternative channels

I explore alternative explanations for the observed patterns of credit.

Credit Inelasticity and General Equilibrium Effects: Lilienfeld-Toal et al. (2012) argue that when the supply of credit is inelastic, stronger creditor rights can increase equilibrium interest rates and reallocate credit from poor to wealthy borrowers. If zombie borrowers are also poorer borrowers, as in Lilienfeld-Toal et al. (2012), such general equilibrium effects can explain our results. However, the baseline results are robust to such an alternative explanation. The baseline compares zombie borrowers to non-zombie firms in the treatment group, that is, firms with more collateralizable assets (wealthier firms as in Lilienfeld-Toal et al. (2012)). Section IV shows that zombie firms in the treatment group (firms with more collateralizable assets or wealthier firms) see a more significant decline in borrowing. Additionally, zombie borrowers, which see a credit cut after the reform are not the "poorer" firms, but are the older, publicly listed firms. Further, Section IV shows that these zombie firms were *more* likely to get credit in the weak enforcement regime (before the reform), inconsistent with the idea that the zombie definition somehow captures the "poorer" borrowers defined in Lilienfeld-Toal et al. (2012).

Labor market channel: Before addressing the alternative hypothesis operating through the labor markets, Table B11 documents the effect of the reform on labor. Employment data is not well-populated in CMIE. Hence, I supplement with ASI with the caveat that this data only includes manufacturing firms. Columns 5–6 in Table B11 show that $\Delta Employment$ declined by seven employees relative to a pre-reform period average of 0.089. My results, however, show a significant decline in employment of zombie firms since these firms become financially constrained as lenders pare back credit, consistent with a credit supply-side hypothesis. These zombie firms do not switch from capital-intensive to labor-intensive industries post-reform, as documented in Alok et al. (2018). Additionally, non-zombie firms in exposed industries see 7 employees added per year relative to an average decline of 5 employees before the reform (Table B11, columns 7–8). Columns 1–4 in Table B11 repeat the analysis with CMIE data, but estimates are noisy due to data-quality issues.

I next examine an alternative mechanism operating through labor markets as in Biais and Mariotti (2006). In their model, large firms are less credit-constrained compared to small firms.

[19] The empirical results in Vig (2013) also show no impact on unsecured borrowing. My results on unsecured lending should be interpreted with caution because, as Vig (2013) argues, the muted impact on unsecured lending can be attributed to by the limited unsecured lending market in India at the time.

Stronger enforcement expands credit access disproportionately more for small firms, who then increase their demand for labor. With an upward-sloping supply curve, the wage rates increase. In their model, credit supply is infinitely elastic, and hence the cost of capital does not change. However, credit access to large firms does not change while the wage rate rises, and so the profits of the large firms decline. Large firms shrink, whereas small firms expand, and credit access reflects this. At face value, since zombie firms correspond to the older, listed firms, the above hypothesis can explain our results. Section IV, however, controls for this and the balance tests for zombie firms show that zombie and non-zombie firms do not differ in treatment and control groups. To test the Biais and Mariotti (2006) theory further, I examine the impact on wage rates. Table B7 column 6 indicates that there was no impact on the wage rates of the treatment firms inconsistent with the Biais and Mariotti (2006) hypothesis that wage rates increase after the reform. Of course, this requires us to assume that labor is inelastic. However, the effects on employment documented above are consistent with Biais and Mariotti (2006) since the zombie firms (older, public firms) lose labor relative to the non-zombie firms. The effects in Biais and Mariotti (2006) operate through the industry channel, which is inconsistent with Column 4 (Table VI) that shows that the bank lending channel dominates (as hypothesized in this paper).

Table B1
Variable Descriptions

Data Item	Variable	Definition
Item 1	Current Portion of Secured Debt	From Prowess
Item 2	Current Portion of unsecured debt	From Prowess
Item 3	Secured Debt (Secured by tangible assets)	From Prowess
Item 4	Unsecured Debt (Not secured by tangible assets)	From Prowess
Item 5	Number of Employees	From Prowess
Item 6	Total Sales	From Prowess
Item 7	Long-term Borrowings	From Prowess
Item 8	Total Assets (Book Value of Assets)	From Prowess
Item 9	Plant and Machinery	From Prowess
Item 10	Land and Building	From Prowess
Item 11	Capital Work in Progress	From Prowess
Item 12	Other Fixed Assets	From Prowess
Item 13	Cash and Bank Balance	From Prowess
Item 14	Marketable Securities	From Prowess
Item 15	Specific Assets	Derived from Prowess (Item 9 + Item 12)
Item 16	Non-specific Assets	Derived from Prowess (Item 10 + Item 13 + Item 14)
Item 17	Total Debt	Derived from Prowess (Item 6 + Item 7 or Item 3 + Item 4)
Item 18	Secured Borrowings	Derived from Prowess (max(0, Item 3 - (lagged Item 3 - Item 1)))
Item 19	Unsecured Borrowings	Derived from Prowess (max(0, Item 4 - (lagged Item 4 - Item 2)))
Item 20	Gross Fixed Assets	Derived from Prowess (Item 9 + Item 10 + Item 11 + Item 12)
Item 21	CapEx	Derived from Prowess max (0, Item 20 - Lagged Item 20))
Item 22	Tangibility	Derived from Prowess Specific assets / (Specific + Non - specific assets)
Item 23	Interest Rate Expense	From Prowess
Item 24	Prime Lending Rate for Long-term Loans	From SBI
Item 25	Lending Rate for Short-term Loans	From RBI/Prowess
Item 26	Interest Coverage Ratio (ICR)	Derived from Prowess (EBIT/Interest Expense)
Item 27	Interest Rate	Derived from Prowess Interest Rate Expense/(Secured debt + unsecured debt)
Item 28	Log(MPK)	Derived from Prowess Log(Item 6/Item 20)
Item 29	Tobin's q	Derived from Prowess (Market Value of Assets/Book Value of Assets)
Item 30	Post	An indicator equal to 1 is year is greater than 2002. The fiscal year March 2003, immediately following the official date of the law (June 21st 2002), is taken as the year the law is in effect.

Table B1 (contd.)
Variable Descriptions

Data Item	Variable	Definition
Item 31	Distressed Borrowers	Firms with an ICR of less than 1 in 2000 and 2001
Item 32	Healthy Borrowers	Firms with an ICR of greater than 1 in 2000 and 2001
Item 33	Zombie Firms	Firms that have received subsidised credit in pre-collateral reform period in 1997–2002. A firm receives subsidised credit if: (i) interest rate of firm is below minimum prime lending rate (ii) ICR is less than 1 (iii) leverage (total debt to total assets) is greater then 0.20 (iv) change in debt is greater than zero (v) firm is not high rated (“Highest Safety" in CMIE")
Item 34	Non-zombie Firms	Firms that have not received subsidised credit in pre-collateral reform period in 1997–2002.
Item 35	Treatment Firms	Firms with asset tangibility ratio in 2001 above the median tangibility of all firms
Item 36	Control Firms	Firms with asset tangibility ratio in 2001 below the median tangibility of all firms
Item 37	Zombie relationship	Zombie relationship is 1 if a firm is classified as a zombie at the firm level and receives a loan from the given bank in the pre-reform period. Calculated using data from Ministry of Corporate Affairs (MCA).
Item 38	Distressed Bank	Equal to 1 if the distressed asset ratio is in the top quintile.
Item 39	State-owned bank	Equal to 1 for whether a bank is state-owned.
Item 40	Publicly listed private sector bank	Equal to for whether a bank is publicly listed and whether it is a private sector bank.
Item 41	Bank Exposure	Based on the fraction of zombie firms in a bank’s portfolio. If this value is in the top two quintiles, then bank-exposure is 1.
Item 42	Industry Exposure	Equal to 1 for industries with average asset tangibility of firms in 2000 above the median

Table B1
Variable Descriptions

(contd.)

Data Item	Variable	Definition
Item 43	MRPK	Marginal Productivity of Capital (MRPK) is calculated as the sectoral elasticity of capital multiplied by the sales to capital. Details of the calculation are described in Section A1.B.
Item 44	Capital Wedge	Capital Wedge is the ratio between MRPK and industry-wide interest rate. Details of the calculation are described in Section A1.B.
Item 45	TFP	Total Factor Productivity is the portion of output not explained by the amounts of inputs used in a production function. Details of the calculation are described in Section A1.B.

Table B2
Robustness to Alternate Specifications and Definitions of Quality

This table reports results for the difference-in-difference-in-differences specification in Equation 1. The dependent variable is secured borrowing for column 1, 4–8. Secured borrowing is the change in secured debt between the current period and the previous period. The dependent variable for column 2 is secured borrowing normalized by total assets and the dependent variable for column 3 is log(1 + secured borrowing). Post, Distressed, and Treatment are the indicator variables for the post-reform period, distressed borrowers, and treatment firms, respectively. Firm and year fixed effects are included in all columns. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Column 1 includes Tobin's q as control variable, columns 4–8 defines distressed borrowers as firms with the below median return on assets in 2001, as firms with the below median Tobin's q in 2001, as firms with the below median interest coverage ratio in 2001, as firms with the below median interest coverage ratio in 2001 within an industry, and as firms with interest coverage ratio less than 1 in the entire pre-reform period from 1997 to 2001, respectively. Baseline mean is calculated for the zombie firms in the period before the reform in columns 1–8. Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.

	Alternate Specifications			Alternate definitions of distressed borrowers				
	Tobin's Q	$\frac{\text{Secured Borrowings}}{\text{Assets}}$	Ln(1+ Secured Borrowings)	ROA	Tobin's Q	ICR in 2001	Within Industry	ICR$\leq$1 Persists
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distressed * Treatment * Post	-50.58***	-0.0147***	-0.248**	-24.88***	-54.49***	-22.95***	-35.60***	-29.11***
	(18.96)	(0.00571)	(0.100)	(7.943)	(13.86)	(8.101)	(8.237)	(11.06)
Baseline mean				50.77				
No. of Obs.	21080	42506	35385	47870	47870	47870	47870	47870
R-sq.	0.362	0.257	0.650	0.355	0.355	0.356	0.356	0.355
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3
Balance Tests for Zombie Firms

This table examines characteristics of zombie firms before the reform. Zombie and Treatment are the indicator variables for the zombie firms, and treatment firms, respectively. The dependent variable is an indicator for whether a firm is a manufacturing firm (column 1), log of firm age (column 2), an indicator for whether a firm is publicly listed (column 3), an indicator for whether a firm is large defined as above median in 2001 (column 4), and whether a firm is high rated/"highest safety" in CMIE (column 5). Standard errors are clustered at the firm level. Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ as of 2001.

	Manufacturing	Age	Listed	Large	High Rated
	(1)	(2)	(3)	(4)	(5)
Zombie	0.0578**	0.178***	0.0208	0.00473	-0.0604***
	(0.0276)	(0.0430)	(0.0287)	(0.0290)	(0.0147)
Zombie * Treatment	-0.0134	-0.00248	0.0902**	-0.0405	-0.0286
	(0.0346)	(0.0538)	(0.0359)	(0.0363)	(0.0185)
Baseline mean			60.64		
No. of Obs.	5852	5852	5852	5852	5852
R-sq.	0.0677	0.0338	0.0378	0.0161	0.0161

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4
Pre-reform Zombie Lending

This table reports examines zombie lending in the period before the reform, 1997–2002. In columns 1–2, the dependent variable is whether a firm is classified as a zombie in the current year (Zombie_t). The dependent variable for columns 3–4 is secured borrowing Secured borrowing is the change in secured debt between the current period and the previous period. The baseline zombie definition is as in Table B1. Zombie_{t-1} is whether a firm is classified as a zombie in the prior year. Distressed_{t-1} is whether a firm is classified as a distressed firm in the prior year. Treatment is the indicator variable for the post-reform period. Year fixed effects are included in all columns. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Remaining variable definitions are in Table B1. Data is from Prowess_{dx} for 1997–2002.

Dependent variable:	Zombie_t		$\text{Secured borrowing}_t$	
	(1)	(2)	(3)	(4)
Zombie_{t-1}			41.95***	14.27**
			(7.591)	(7.073)
Distressed_{t-1}	0.139***	0.102***		
	(0.00612)	(0.00521)		
Zombie_{t-1} * Treatment		0.278***		40.41***
		(0.0207)		(12.32)
No. of Obs.	22148	22148	22148	22148
R squared	0.0763	0.120	0.0638	0.0644
Year FE	Y	Y	Y	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B5
Is it about distress or subsidized credit?

This table examines the impact of the collateral reform on secured borrowing for the sub-sample of distressed firms. The dependent variable in all columns is secured borrowing. Secured borrowing is the change in secured debt between the current period and the previous period. Post, Zombie, and Treatment are the indicator variables for the post-reform period, zombie firms, and treatment firms, respectively. Firm, year, industry-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Columns 1 and 5 subset to only zombie borrowers, columns 2 and 6 subset to only non-zombie distressed firms, column 7 to only firms who did not receive new borrowing and had ICR less than 1, and remaining columns include all distressed firms. Baseline mean is calculated for the zombie (distressed non-zombie) firms in the period before the reform in columns 1, 3–5, and 8–9 (columns 2 and 6–7). Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.

Dependent variable:	Secured Borrowing (Sample=Distressed firms)								
	Zombie firms	Distressed non-zombie firms	All		Zombie firms	Distressed non-zombie firms	Distressed non-zombie firms with no subsidized credit	All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	-44.87***	-12.63***							
	(7.364)	(4.233)							
Zombie * Post			-32.29***	-34.56***				-3.773	-4.191
			(8.512)	(8.144)				(9.422)	(8.830)
Treatment * Post					-53.27***	-15.75**	5.756	-15.88**	-12.66*
					(12.39)	(7.964)	(11.95)	(7.961)	(7.564)
Zombie * Treatment * Post								-37.14**	-40.52***
								(14.73)	(13.87)
Baseline Mean	60.64	50.77		60.64		50.77		60.64	
No. of Obs.	6206	8944	15150	15150	6206	8944	3809	15150	15150
R-sq.	0.444	0.394	0.421	0.455	0.450	0.395	0.309	0.423	0.456
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	N	N	Y	N	N	N	N	Y
Controls	N	N	N	Y	N	N	N	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B6
External Validity and Alternate Identification Strategies

The dependent variable in all columns is secured borrowing. Secured borrowing is the change in secured debt between the current period and the previous period. Columns 1–2 report results for an external validity test using the staggered introduction of the Debt Recovery Tribunals (DRTs). Columns 3–4 use non-banking financial corporations (NBFCs) as the control group for whom the collateral did not apply. Columns 5–7 use an alternative strategy that exploits prevailing court efficiency. Post, DRT, and Bank are indicator variables for the post-reform period, whether the DRT is in effect, and whether a firm's lead lender is a bank (as opposed to an NBFC). Court efficiency is 1 for the top tercile. Firm, year, industry-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Period of analysis is 1991–2002 in columns 1–2 and for 1997–2006 in remaining columns. Dependent variable for columns 1–2 is winsorized at the 5 percent level due to noisiness of data in the earlier years and at the 2 percent level in the remaining columns. Dates for DRT establishment is from Lilienfeld-Toal et al. (2012). Data on court efficiency is from Alok et al. (2018). Remaining variable definitions are in Table B1. Remaining data is from Prowess$_{dx}$.

Dependent variable:	Secured Borrowing					
	External validity		Alternate identification			
	(1)	(2)	(3)	(4)	(5)	(6)
Zombie * DRT	-19.78***	-19.25***				
	(6.769)	(6.855)				
Zombie * Bank * Post			-41.09***	-40.51***		
			(11.23)	(14.49)		
Zombie * Court efficiency * Post					-43.14**	-44.48**
					(17.89)	(17.91)
Baseline Mean	55.30		60.64			
No. of Obs.	21859	21859	28156	28156	47348	47348
R-sq.	0.372	0.405	0.356	0.375	0.370	0.384
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	Y	N	Y	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B7
Impact of the Reform on Unsecured Borrowing, Equity Issuance, Commercial Paper Issuance, Trade Credit, Cash Hoarding, and Wages

The table reports results for the difference-in-difference-in-differences specification in Equation 1. The dependent variable is unsecured borrowing in columns 1 and log of equity in column 2. Unsecured borrowing is the change in unsecured debt between the current period and the previous period. Equity is the difference between net worth and retained profits. The dependent variable for column 3 is commercial paper and the dependent variable for column 4 is trade credit. Trade credit includes trade payable (not including from groups and subsidiaries), acceptances, and advances from customers. The dependent variable for column 5 is cash hoarding (cash to total assets) and the dependent variable for column 6 is wage bill. Post and Treatment are the indicator variables for the post-reform period and treatment firms, respectively. Firm, year, industry-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Baseline mean is calculated for the zombie firms in the period before the reform in columns 1–5. Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.

Dependent variable:	Unsecured borr.	Ln(equity)	CP	Trade credit	$\frac{\text{Cash}}{\text{Assets}}$	Wage bill
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment * Post	1.268***	-0.169***	-6.198*	17.90	-0.000871	-10.10
	(0.453)	(0.0213)	(3.210)	(66.44)	(0.00198)	(13.89)
Zombie * Post	1.198	-0.270***	1.847	-114.2**	-0.00279	-33.70**
	(0.854)	(0.0540)	(1.523)	(54.08)	(0.00487)	(13.18)
Zombie * Treatment * Post	-1.446	-0.0575	5.081	27.13	0.00440	34.92
	(1.109)	(0.0725)	(3.430)	(78.16)	(0.00536)	(22.58)
Baseline Mean	1.69	4.67	9.17	297.56	0.05	737.01
No. of Obs.	47274	47274	47274	47274	47274	47274
R-sq.	0.448	0.928	0.312	0.811	0.582	0.912
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Industry-Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B8
Collateralized loan-level data: Summary statistics

This table shows the summary statistics of all the main variables from the loan level data from the Ministry of Corporate Affairs (MCA). The data is collapsed to the firm-bank-year level. The means and standard deviations are shown in columns 1 and 2. Columns 3 and 4 show the average for zombie relationship bank-firm pairs in the periods before and after the reform, that is 2002. Column 5 shows the t-statistic on the difference between the pre- and post-reform periods for the zombie relationship bank-firm pairs. Columns 6 and 7 show the average for the non-zombie relationship bank-firm pairs in the period before and after the reform. Column 8 shows the t-statistic for the difference between the two. Summary statistics are shown for whether a firm received a loan from a given bank in a given year ($\mathbb{1}_{Loan}$), and for total collateralized borrowing of a firm from a given bank in a given year (Total volume). Zombie relationship is 1 if a firm is classified as a zombie at the firm level and receives a loan from the given bank in the pre-reform period.

	All		Zombie$_{Reln}$ loans			Non-Zombie$_{Reln}$ loans		
	Mean	SD	Pre	Post	t-stat on diff.	Pre	Post	t-stat on diff.
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}_{Loan}$	0.300	0.460	0.320	0.0900	(-18.68***)	0.380	0.280	(-17.23***)
Total volume (in million)	47.65	122.6	43.12	18.69	(-7.32***)	53.38	47.53	(-3.72***)
Observations	31,580		3,872			27,708		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B9
Collateralized loan-level data: Characteristics of bank-firm zombie relationships

This table examines the bank characteristics of the banks that make zombie loan to firms using the loan level data from the Ministry of Corporate Affairs (MCA). The data is collapsed to the firm-bank-year level. We use all the unique firm-bank pairs in the pre-reform period between 1997–2001. Distressed bank uses bank-level non-performing loans data from the Basic Statistical Returns dataset. The distressed asset ratio is the ratio of all non-standard credit to total credit in 2001. Distressed bank is an indicator equal to 1 if the distressed asset ratio is in the top quintile. State-owned bank is an indicator for whether a bank is state-owned. Publicly listed private sector bank is an indicator for whether a bank is publicly listed and whether it is a private sector bank. Zombie relationship is 1 if a firm is classified as a zombie at the firm level and receives a loan from the given bank in the pre-reform period between 1997–2001. Firm fixed effects are included are as indicated in the table. Standard errors are clustered at the firm and bank level.

Dependent variable:	$Zombie_{Reln} = 1$				
	(1)	(2)	(3)	(4)	(5)
Distressed bank	0.0181**			0.0178**	0.0189**
	(0.00777)			(0.00801)	(0.00762)
State-owned bank		0.0107		0.00735	-0.00466
		(0.00879)		(0.00903)	(0.00773)
Publicly listed private bank			0.00475	0.00770	0.0107
			(0.00727)	(0.00737)	(0.00677)
No. of Obs.	6518	6518	6518	6518	6518
R-sq.	0.000830	0.000227	0.0000655	0.00108	0.827
Firm FE	N	N	N	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B10
Impact of Collateral Reform CapEx and CapEx Spillovers

The table below examines the effect on project completion (columns 1–2) and the spillover effects on non-zombie firms (column 3–4). Post, Non-zombie, Treatment, and Industry Exposure are the indicator variables for the post-reform period, non-zombie firms, treatment, and whether a firm is linked to high exposure industries, respectively. Columns 1–2 reports results for the triple difference specification in Equation 5. The dependent variable in columns 1–2 is an indicator variable for whether the project was completed. Core (non-core) projects refer to projects where the project industry is the same (differs) as the firm industry. Column 1 subsets to core projects and column 2 subsets to non-core projects. Columns 3–4 shows the results of the specification in Equation 12 for the high industry exposure channel. The dependent variable in columns 3–4 is capital expenditure. Capital expenditure is non-negative difference in gross fixed assets between the current period and the previous period. Industry Exposure is set to 1 for industries with average firm asset tangibility in 2000 is above median value. Fixed effects and control variables are as indicated. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Data is from Prowess$_{dx}$ and Capex$_{dx}$ for the 1997-2006 period.

Dependent variable:	Project Completed = 1		Capital Expenditure	
	Core projects	Non-core projects	All	
	(1)	(2)	(3)	(4)
Zombie * Post	-0.382***	0.288		
	(0.162)	(0.351)		
Treatment * Post	-0.0388	-0.134		
	(0.168)	(0.160)		
Zombie * Treatment * Post	0.468***	-0.501		
	(0.213)	(0.460)		
Non-Zombie * Post			8.393	-1.778
			(7.814)	(7.439)
Industry Exposure * Post			-23.30**	
			(9.891)	
Non-Zombie * Industry Exposure * Post			44.15***	47.28***
			(10.70)	(10.30)
No. of Obs.	2267	1661	47488	47442
R-sq.	0.551	0.631	0.646	0.008
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry-Year FE	N	Y	N	Y
Controls	N	N	N	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B11
Impact of Collateral Reform on Employment and the Effect of Zombie Distortions

The table reports results for the difference-in-difference-in-differences specification in Equation 1 for columns 1–2 and 5–6, and in Equation **??** for the remaining columns. The dependent variable is the employment change from the current period relative to the previous period. Post, Distressed, and Treatment are the indicator variables for the post-reform period, distressed borrowers, and treatment firms, respectively. Firm, year, industry-year fixed effects are included as indicated in each column. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm level. Baseline mean is calculated for the distressed (non-zombie) firms in the period before the reform in columns 1–2, and 5–6 (columns 3–4 and 7–8). Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ in columns 1–4 and from from Annual Survey of Industries (ASI) in columns 5–8. for 1997–2006.

Dependent variable:	Change in Employment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-Zombie * Post			0.000475	-0.00124			7.968***	6.778***
			(0.00185)	(0.00177)			(1.739)	(1.719)
Distressed * Post	-0.00316**	-0.00176			-5.431***	-3.891***		
	(0.00140)	(0.00127)			(1.491)	(1.480)		
Industry Exposure * Post			-0.00415**	-0.0000808			-6.800	7.899
			(0.00207)	(0.00575)			(4.493)	(11.57)
Treatment * Post	-0.000877	-0.000642			4.566**	3.484**		
	(0.00110)	(0.00113)			(1.773)	(1.758)		
Distressed * Treatment * Post	-0.000819	-0.000706			-7.299**	-6.193*		
	(0.00183)	(0.00183)			(3.364)	(3.320)		
Non-Zombie * Industry Exposure * Post			0.00302	0.00403*			7.539**	6.728*
			(0.00231)	(0.00223)			(3.724)	(3.625)
Baseline Mean	0.180		0.720		0.089		-5.07	
No. of Obs.	51939	51939	51939	51939	113424	113424	113424	113424
R-sq.	0.125	0.137	0.125	0.137	0.131	0.141	0.130	0.141
Firm FE	Y	Y	N	N	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	Y	N	Y	N	Y	N	Y
Controls	N	Y	N	Y	N	Y	N	Y
Dataset	CMIE	CMIE	CMIE	CMIE	ASI	ASI	ASI	ASI

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B12
Heterogeneity in credit to non-zombie firms

This table examines heterogeneity of the impact of the reform on secured borrowing of non-zombie firms for different firm characteristics. Treatment and non-zombie are indicators for treatment firms and Non-zombie firms, respectively. The characteristics are listed in the column header and are whether, a firm is young (age in 2001 less than 5 years) in column 1, whether a firm is publicly listed in column 2, whether a firm is large (above median assets in 2001) in column 3, and whether a firm is high rated ("highest rates in CMIE") in column 4. Standard errors are clustered at the firm level. Baseline mean is calculated for the non-zombie firms in the period before the reform in columns 1–4. Remaining variable definitions are in Table B1. Data is from Prowess$_{dx}$ for 1997–2006.

Dependent Variable:	Secured Borrowing			
Characteristic:	Young	Listed	Large	High Rated
	(1)	(2)	(3)	(4)
Characteristic * Post	1.747	-2.993	-4.776	-0.341
	(4.439)	(3.833)	(5.181)	(3.790)
Treatment * Post	11.19**	18.54**	13.87***	28.55***
	(4.924)	(8.365)	(4.830)	(8.746)
Characteristic * Treatment * Post	-9.897	-4.296	1.558	-25.83**
	(7.192)	(10.32)	(6.832)	(11.31)
Baseline mean	32.46			
No. of Obs.	39284	39284	39284	39284
R-sq.	0.366	0.366	0.367	0.366
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry-Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B13
Impact on Firm Births and Deaths

This table presents the number of firm births and deaths post the collateral reform. The dependent variables are the total firm births and firms deaths in a year at the industry-level. Firm birth is based on the year of incorporation from the Prowess$_{dx}$ database and firm death is coded when a firm leaves the database. Post and Industry Exposure are indicator variables for the post-reform period and whether a firm is in high exposure industries, respectively. Industry and year fixed effects are included in all columns. Bootstrapped standard errors are reported. Data is from Prowess$_{dx}$ for 1997–2006.

	(1) Number of firm births	(2) Number of firm deaths
Industry Exposure*Post	-0.175	-3.294
	(0.107)	(2.268)
No. of Obs.	662	729
R-sq.	0.251	0.743
Industry FE	Y	Y
Year FE	Y	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B14
Effects Attributable to the Insurance Channel in Vig (2013)

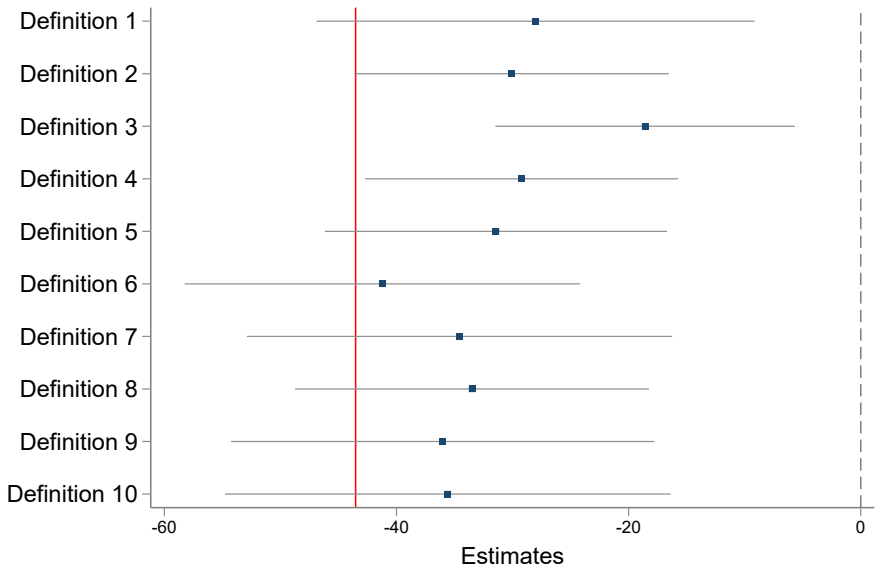
This table examines the impact of the collateral reform on secured borrowing for treatment firms in column 1. Post and Treatment are the indicator variables for the post-reform period and treatment firms, respectively. Column 2–3 present the results using an instrumental variable specification as in Equation 21, with secured debt instrumented with the treatment indicator. The dependent variable in columns 2–3 is capital growth in MRPK for columns 6–7 with MRPK calculation described in Section A1. Pre-reform Capital Wedge is the average MRPK for a firm in the pre-reform period, 1997–2002. Fixed effects and sample subsets are as indicated. Firm-level controls included are log of sales and profitability. Standard errors are clustered at the firm-level. Data is from Prowess$_{dx}$ for 1997–2006. Remaining variable definitions are in Table B1.

Dependent Variable	Secured Borr	MRPK growth	
Sample:	All	Post	
	(1)	(2)	(3)
Post * Treatment	-9.659**		
	(3.931)		
Secured Borrowing		0.0106	-0.0256
		(0.0543)	(0.0633)
Secured Borrowing * Pre-reform Capital Wedge			-0.163*
			(0.0976)
No. of Obs.	47274	17978	17978
R-sq.	0.380	.	.
Firm FE	Y		
Year FE	Y	Y	Y
Industry-Year FE	Y	Y	Y
Type	OLS	IV	IV
First Stage F-statistic		28.11	5.068

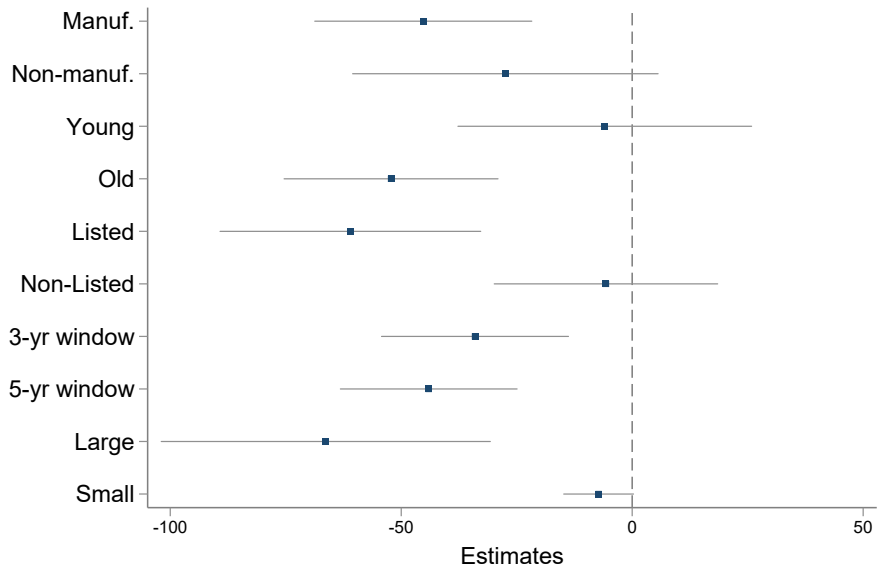
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure C1
Alternative Definitions and Heterogeneity of Zombie Lending Effects

The graphs below show the coefficients for the difference-in-difference-in-differences (DDD) estimate in Equation 2 with the dependent variable secured borrowing for different zombie definitions (Panel A) and varying subsets of firms (Panel B). All regressions include firm and year fixed effects. Standard errors are clustered at the firm level. 5 percent confidence intervals are shown in gray. The solid red line represents the baseline DDD estimate in Section IV. The dashed gray line is at 0. In the baseline a firm is classified as a zombie if it receives zombie credit in 1997–2002. A firm receives zombie credit if it satisfies all of the following conditions: (i) interest rate of the firm is below the minimum prime lending rate, (ii) ICR is less than 1, (iii) leverage (total external debt to total assets) is greater than 0.20, (iv) change in debt is greater than zero, and (v) the firm itself is not high-rated. Zombie definitions use variations of the baseline criteria. Definition 1 includes condition (ii), (iv). Definition 2, in addition, modifies condition (iv) to include nonzero growth in secured debt. Definition 3 includes (i) and (v). Definition 4 augments definition 3 with (i). Definition 5 augments definition 4 with (iii). Definition 6 augments definition 4 with (iv). Definition 7 is the same as definition 6, but with *secured borrowing* greater than zero in a year. The minimum prime lending rate for definitions 5–7 is the prime lending rate at the largest state-owned bank, State Bank of India. The minimum prime lending rate for remaining definitions is the minimum prime lending rate at State Bank of India, ICICI (a private sector banks), and IDBI (a development bank). Definition 9 is the baseline definition but also includes a profitability condition that return on assets is less than 0. Definition 10 is the baseline with the modified condition that new secured borrowing is greater than zero in a year. Panel B shows the DDD estimates for the subsets of firms (from top to bottom): (a) manufacturing, (b) non-manufacturing, (c) young firms defined as firms with age less than or equal to 5 years in 2001, (d) old firms defined as firms with age greater than 5 years in 2001, (e) listed firms, (f) non-listed firms, (g) restricting to 3 years post-reform, (h) restricting to 2 years post-reform, (i) large firms (size above median in 2001), and (j) small firms (size below median in 2001). Remaining variables are defined in Table B1.



(A) Alternative zombie definitions



(B) Heterogeneity of effect