Do Creditor Rights Reduce Tunneling? Evidence From India's Bankruptcy Law Reforms*

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Abstract

We study the impact of a bankruptcy reform that improved creditor rights on the tunneling of funds through related party transactions (RPTs) among group firms in India. The reforms introduced measures to facilitate debt recovery by creditors and promote legal efficiency in handling corporate insolvencies. We employ a triple-difference estimation strategy comparing RPTs between 'treated' firms, defined as financially distressed firms with continued cheap bank credit, compared to other financially distressed firms, before and after reform, and in Indian states where debt recovery court systems were less efficient relative to other states. We find that improved creditor rights reduced RPT outflows, specifically in the form of loans and investments. Our findings suggest that creditor empowerment led firms to repay bank debt by reducing RPTs as well as by reducing dividend payouts. We find no evidence of any real impact on the treated firms in the form of firm financial ratios or on sales, salaries paid, and investments.

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1 Introduction

Can improved creditor rights diminish "tunneling", the tendency for managers to expropriate a firm's resources for private gains at the expense of shareholders? Tunneling is notoriously hard to detect, as managers can use their decision-making powers to expropriate funds through mispriced transactions in assets, goods, and loans that take place on preferential terms to other firms within their control.[1] Tunneling can lead to financial distress, increase future bankruptcy probability, and diminish countries' growth prospects through increased misallocation if firms' financial malaise transfers to their creditors.

In this paper, we shine a light on tunneling behavior using a novel empirical design and a bankruptcy policy implementation in India. Particularly, we focus on the Insolvency and Bankruptcy Code (IBC) of 2016, which effectively enabled all creditors to initiate bankruptcy process, increased expected recoveries for creditors, and drastically reduced insiders' post-filing payoffs (the managers lost control of the firm immediately upon filing, for example). We examine the impact of the reform on related party transaction (RPT) *outflows* among financially weak firms in corporate groups; we exclude the financially healthy firms since the bankruptcy reform does not directly affect them. In this setting, we argue that RPT outflows among group firms are a useful proxy for tunneling behaviour.

Our main finding is that financial RPTs, as distinct from operational RPTs, fell due to the reform. Related party loan outflows and asset outflows formed the main components of the decline. We provide evidence to suggest that creditor empowerment following IBC forced borrowers to repay bank credit by cutting down on RPT outflows. However, we do not find evidence for managers bringing money in through other sources, including through RPT inflows. Because the results show a fall in RPT *outflows*, we rule out the possibility of otherwise well-meaning managers trying to prop up weak firms through RPT *inflows*.

Our empirical design uses a triple-difference approach. We compare financially distressed firms with continued access to low-cost credit ('treated' firms henceforth) with a control group of other financially distressed firms. In a regime with weak creditor rights, banks may delay recognizing financial distress to avoid provisioning requirements (Peek and Rosengren (2005)). Distressed firms can remain on bank balance sheets as 'evergreened' loans for long periods.[2] The treated firms are more affected because higher expected recovery rates reduced banks' willingness to evergreen further.

Indeed, insiders taking advantage of weak banks was a major concern in India before

[1] For example, a manager in Firm A may give out underpriced loans to Firm B where they have greater cash flow rights and give themselves dividends in Firm B.

[2] Evergreening refers to the continued extension of credit, typically by banks, to financially weak firms. Banks can be induced to do so due to political interference (in publicly-owned banks) or if the bank's balance sheet weakness renders it unable to absorb losses due to the firm's failure. In fact, we show that in the pre-period, financial RPT outflows among our 'treated' firms were higher in case of firms exposed to weak banks (Appendix Table A2).

IBC. Non- performing loans in some banks were as high as 20 percent due to cumulative losses on large infrastructure lending coupled with a prolonged regulatory forbearance following the global financial crisis in 2008. Banks with limited capital buffers were therefore more likely to evergreen loans (as in Peek and Rosengren (2005)), which further increased the incentives among insiders to tunnel resources out of financially weak firms. We restrict the control group to not include healthy firms both because the law did not directly affect them, and to avoid contamination due to time-varying unobserved firm-level factors that may differ across distressed and healthy firms. The second difference is from comparing after versus before the reform.

The third difference is used to invalidate other nationwide policies that were implemented around the same time as the IBC law. These policies include a major indirect tax reform, implementation of an inflation-targeting monetary policy regime, and a demonetization policy that removed large denomination banknotes from circulation. Each of these policies could potentially impact the treated firms differently relative to the control group. We exploit the variation in judicial efficacy in debt recovery tribunals across states in India before the IBC reform as the third difference in the same spirit as Alok et al. (2022). Since parallel courts were established under the IBC to explicitly deal with corporate insolvency issues, they were more efficient than the court system that preceded them. This resulted in a larger increase in efficiency in states with previously less efficient debt recovery courts. Importantly, the efficiency of the earlier court regime is not related to policies other than the IBC. We verify that parallel trends hold prior to the reform, providing empirical validation for our identification strategy.

The estimated effects are statistically significant and economically meaningful. Total financial RPTs among group firms fell by 91 percent more in low court efficiency states after the reform, relative to high court efficiency states. In a similar sense, related party loan outflows fell by 87 percent and asset purchases from related parties fell by 56 percent. Operational RPTs, which include purchases of goods and non-financial services from related parties, were unaffected by the reform.

The focus on RPT outflows, coupled with the empirical design, gives us a unique vantage point to highlight the impact of the law on tunneling, which remains notoriously difficult to measure.[3] To be sure, not all RPT outflows are tunneling; our contention is that *changes* to RPT outflows among the treated firms relative to the control group after the IBC, in low versus high court efficiency states, reflects *changes* in tunneling. The fact that it is related party loan outflows that fell among the treated firms strongly points to a fall in tunneling (Jiang et al. (2010)). It is difficult to justify alternate explanations that do not involve tunneling for why a financially distressed firm with evergreened credit reduces RPT loan outflows in our setting.

[3]Improvements in RPT disclosure regulations do not necessarily improve tunneling detection. Creditors, investors, and regulators often resort to forensic audits to detect tunneling.

Next, we find that credit, measured by the change in total borrowings to total assets, falls after IBC for the treated firms in low efficiency states. The fall is driven by bank credit. There is however no impact on interest rates on the loans. This is consistent with both errant borrowers cutting down on RPTs due to fear of bankruptcy after IBC, and a creditor-led channel of disciplining, where creditors decrease supply of credit.

We test if the fall in RPTs is due to creditor-led or borrower-led (as in Vig (2013)) disciplining mechanisms. We do this by estimating the impact separately for firms borrowing predominantly from healthy versus less healthy banks. Less healthy banks will likely be unable to impart discipline, given that they cannot credibly threaten borrowers with bankruptcy (Peek and Rosengren (2005)). Our results show that financial RPTs fall even for firms borrowing from less healthy banks, indicating that borrowers are cutting down on RPTs. We also find that the estimated fall in *loan* RPTs is statistically significant only for firms borrowing from healthy banks. Healthy banks can force firms to cut down on mis-priced loan RPTs. Banks generally have a comparative advantage in identifying mis-pricing in credit contracts relative to mis-pricing in other types of financial and operational contracts.

In the same vein, are firms finding other sources of funds to repay credit and avoid bankruptcies? We test if firms raise external finance, increase financial incomes, generate RPT inflows, or reduced dividend payouts. We find no impact on firms' external finances and on financial incomes. Similarly, there is no impact on financial and operational RPT inflows, so the beneficiaries of prior RPTs do not seem to return these funds. Interestingly, there is a fall in dividends paid out. Put together, the evidence suggests that firms use internal funds to repay bank credit. Finally, we explore effects on real outcomes by examining firm performance measures. We find no improvements in financial indicators such as return on assets, return on equity, or on profits. Similarly, there are no real effects in the form of an improvement in sales, salaries paid to employees, or on real investments.

Collectively, our evidence is consistent with creditor right improvements due to IBC curtailing the practice of corporates tunneling funds via related party loans and asset purchases, funded with evergreened loans from banks.[4] We conclude that, after the implementation of IBC, the threat of bankruptcy and subsequent loss of control forces firms to repay banks by cutting down on RPTs and dividend payments. It is noteworthy in the context of literature that identifies related party loans and loan guarantees as a prominent form of tunneling (Gopalan et al. (2007); Berkman et al. (2009); Fisman and Wang (2010); Jiang et al. (2010)). Channeling credit from such distressed firms to healthy firms would improve allocation of capital, potentially leading to aggregate economic gains.

We contribute to three different strands of literature. First, our study relates to the

[4]This is unlinked to another phenomenon known as "indirect evergreening" documented in the literature (Kashyap et al. (2023)). Corporate groups sometimes borrow on the balance of healthy firms to lend onwards to financially distressed firms within the group to keep the recipient afloat. We posit that in our case the distressed firm's credit was evergreened directly by the lenders.

literature on tunneling. Our approach exploits RPT data to capture tunneling activity, which is challenging to pin down (Bertrand et al. (2002)). RPT flows can be used to more directly examine transactions between firms of a corporate group that may be used for expropriation (Cheung et al. (2006)). Further, we highlight the importance of creditor rights as a crucial driver of tunneling behavior.

Second, the paper relates to the literature on the impacts of stronger creditor rights. Improved creditor rights can empower them to enforce contracts and promote the expansion of credit. Haselmann et al. (2010) find that legislation improving creditor rights through collateral reform increases the supply of credit and encourages firms towards more debt financing. Vig (2013) suggests that the relationship between stronger creditor rights and credit expansion need not be positive. He explores an earlier Indian reform aimed at strengthening creditor rights and shows that additional creditor protection imposes extra costs on borrowers leading to a decrease in the use of secured debt by firms. Ponticelli and Alencar (2016) find that firms operating in Brazilian municipalities with less congested courts experience a larger increase in loan use and performance measures after a bankruptcy reform that involved improvement in creditor rights. Like these papers, our paper demonstrates the impacts on loan evergreening and tunneling.

Third, we underscore the importance of reforms that promote allocative efficiency by facilitating the ability of banks to monitor the financial health of their borrowers. The literature on misallocation in developing countries highlights factor market imperfections as a key determinant (Hsieh and Klenow (2009); Restuccia and Rogerson (2013, 2017)). We suggest that creditor empowerment can mitigate misallocation of credit to non-performing firms, with implications for aggregate gains. As such, we reinforce the argument that legal institutions and corporate governance are central to economic development (Johnson et al. (2000)).

The rest of the paper is organized as follows. Section 2 reviews the Insolvency and Bankruptcy Code and discusses the channels through which it impacts creditor rights and tunneling; Section 3 describes the empirical methodology; Section 4 describes the data; Section 5 presents the results and mechanisms; Section 6 concludes.

2 Institutional Background and Mechanisms

2.1 Institutional Background

The Insolvency and Bankruptcy Code (IBC) of 2016, heralded a comprehensive reform of the Indian insolvency and bankruptcy regime, sweeping away obsolete and contradictory laws that complicated judicial decision making. In some instances, insolvency laws were debtor-friendly, aimed at minimizing employment losses and limiting the claims of creditors. In others, legislation was creditor-friendly, but failed to empower creditors ad-

equately and tackle insolvency directly.[5] An important aim of the IBC, therefore, was to codify bankruptcy laws and to generate significant improvements in creditor rights and time-bound asset recovery.

Reforms to the court system. Recognizing that contradictory laws in the previous bankruptcy regime were being used strategically by debtors to delay judicial processes, the IBC mandated that all corporate insolvency cases be addressed within a strict 180-day window. Additionally, a parallel set of courts was established to explicitly handle corporate insolvency. These National Company Law Tribunals (NCLTs) comprised judges with expertise in banking and/or insolvency resolution and liquidation. Once the case is admitted by the NCLT, certified "insolvency professionals" take over the ailing firm to develop the best implementable strategy towards resolution or liquidation. The overall aim was, therefore, to improve and hasten the corporate bankruptcy process.

The IBC also removed several discretionary powers from the judiciary. It mandated that petitions filed be automatically admitted, and that courts automatically approve any resolution or liquidation plan negotiated between debtors and creditors. By eliminating judicial discretion, the IBC sought to reduce the time taken to resolve stressed assets and encourage creditors to approach the NCLTs relative to the inefficient Debt Recovery Tribunals (DRTs) that previously dealt with bankruptcy.

Stylized evidence supports the notion that establishing NCLTs improved the efficiency of recovery and resolution for creditors. The Reserve Bank of India's 'Report on Trend and Progress of Banking in India' in December 2020 points to a sharp increase in the recovery of non-performing assets under the IBC relative to cases under the earlier DRTs and SARFAESI Act. In March 2019, while overall recovery of non-performing assets was 16.3 percent, the recoveries under DRTs, SARFAESI, and IBC were 3.9, 15, and 45.7 percent respectively. In March 2020, the overall recovery increased to 23.2 percent. The recoveries under DRTs, SARFAESI, and IBC stood at 4.1, 26.7, and 45.5 percents respectively.

2.2 Mechanisms – IBC as a Deterrent to Tunneling

Impact on borrowers. The contradictory laws prior to 2016 allowed firms to become "strategically insolvent", i.e. strategically manipulate cash flows and declare default. An Indian newspaper Business Standard article, reproduced on the Insolvency and Bankruptcy Board of India website, summarizes the ideas:[6]

[5]The Sick Industrial Companies (Special Provisions) Act, 1985, is an example of a debtor-friendly legislation. Creditor-friendly laws include the Recovery of Debt Due to Banks and Financial Institutions (RDDBFI) Act, 1993 and the Securitization and Reconstruction of Financial Assets and Enforcement of Security Interests (SARFAESI) Act, 2002.

[6]Link to the article (accessed on January 24, 2024): https://ibbi.gov.in/webadmin/pdf/media/2018/Jul/https___www.business-standard.com_article_printer-friendly-version_article_id=118060900745_1.pdf.

'Perverse incentives for promoter-managers are higher when the company is in the "twilight zone" where there is acute financial distress and insolvency is imminent. At that stage there is a clear information asymmetry between promoter-managers and external stakeholders such as banks. Being aware of incipient stress, promoter-managers are prone to indulging in desperate practices such as asset stripping, off-market related-party transactions and creation of fresh encumbrances for friendly lenders to divert assets from the bankruptcy estate.'

In the academic literature, Gopalan et al. (2023) provide evidence suggesting that managers manipulate the firm's cash flow downwards in the years preceding bankruptcy declaration. They note that several firms with weak balance sheets and continued access to low-cost credit did not require their inside shareholders to add their own equity.

The IBC has three provisions that tackle and deter strategic cash flow manipulation on the borrowers' side. First, upon default, managers lose control over the firm – when a case is admitted by an NCLT, control rights pass to a resolution professional. Second, the RPTs of the firm in the two years prior are subject to forensic audit. If suspect transactions are detected, punitive action can follow and creditors can claw back the value of these transactions from beneficiaries, including management or firms in the same corporate group. Individuals' assets can be liquidated to recoup obligations arising from illegal RPTs. And, finally, there is increased liability for responsible individuals since they cannot bid for the assets of other companies going through the insolvency process.[7] So, we expect the IBC to exert a disciplining effect on managers' propensity to tunnel.

Impact on creditors. The absence of a strong bankruptcy regime prior to 2016 restricted the ability of creditors to recover stressed loans. Many of these loans, especially among large corporates, stemmed from evergreening during the era of forbearance. Indeed, Kulkarni et al. (2023) use the administrative Central Repository of Information on Large Creditors (CRILC) database and find that banks were unwilling to recognize stressed loans as non-performing loans.

The IBC expanded creditor rights in the following ways. First, it put creditors at the forefront of the recovery process by replacing the firm's managers with a resolution professional once the insolvency case is admitted by the court. Second, it markedly expanded creditor coverage, enabling both financial and operational creditors to pursue insolvent firms in court for defaults over INR 10 million. Previous legislation, notably SARFAESI, only enabled financial creditors with secured tangible assets to initiate bankruptcy proceedings. The advent of the IBC resulted in a significant number of cases being filed by operational creditors. Nayak and Regy (2022) report that, as of June 2018, 65 percent of the 6,668 insolvency cases nationwide were filed by operational creditors. These provi-

[7] Responsible individuals under the IBC include promoters (a sub-class of shareholders) of defaulting firms, directors, their related entities, or anyone whose accounts are delinquent for more than a year, individuals who default on their personal guarantees, and those imprisoned for more than two years.

sions empowered creditors to recover stressed loans and cease stressed loan evergreening. We examine these channels empirically in subsequent sections.

3 Empirical Methodology

We identify the impact of the IBC on RPT outflows using a triple-difference strategy. First, we define "treated firms", which are firms that are operational in spite of weak balance sheets with the help of low-cost credit. We are motivated by evidence, both in India and internationally, that weak recovery prospects induce banks to evergreen bad loans to avoid provisioning requirements (Peek and Rosengren (2005); Tantri (2021)). In the case of India, cumulative losses on large infrastructure lending before the global financial crisis of 2008 and the prolonged regulatory forbearance policy resulted in large and unhealthy borrowers which were kept alive through evergreening. The assumption is that such firms are the most exposed to the IBC because creditor incentives to take them to court are the strongest. The control group comprises other financially distressed firms; we exclude healthy firms because they may be very different from our treated firms, and because they are not directly affected by the IBC law. The control group firms are those with insufficient cashflows to meet interest payment obligations (interest rate coverage ratio < 1), but are not sustained with evergreened loans.

Our indicator for treated firms equals one if a firm meets all of the following five criteria in any of the years between 2014-16: i. has insufficient cashflows to meet credit costs (interest rate coverage ratio < 1); ii. credit costs are below the prime lending rates of the largest public sector bank (State Bank of India); iii. saw an increase in borrowings between 2014 and 2016; iv. the debt to assets ratio is above 20 percent; and v. debt is not AAA rated.[8] The first three criteria narrow down the unhealthy firms that are sustained by continued low-cost borrowing. The fourth criteria is added to narrow down on firms that have high debt levels. The fifth criterion ensures that information missing to an econometrician, but available to rating agencies does not classify some firms incorrectly as treated.

Second, we examine the difference in RPT outflows before and after the IBC implementation in May 2016. Third, to account for other major policies implemented around the same time as the IBC, we exploit variation across Indian states in the intensity of treatment.[9] We capture this by considering how ineffective existing DRTs were before the reform as a third difference.[10] Intuitively, IBC treatment intensity should be highest

[8] We conduct robustness to classification criteria in Table A8 in the appendix.

[9] Three other major policies were implemented during our sample period. First, a new flexible inflation targeting monetary regime was adopted during October 2016. Second, a demonetization policy which removed 86 percent of the notes in circulation was implemented in November 2016. Third, a new goods and service taxes regime that removed differential tax rates across states was adopted during July 2017.

[10] Alok et al. (2022) use a similar strategy to identify the impact of the 2002 SARFAESI law on

in those states where DRTs are least efficient, meaning that the creation of a parallel system of NCLTs should have the most impact. We measure DRT effectiveness as the ratio of loan recoveries to the pending amount. A higher value indicates that a court is better at recovery because it disposes cases in a time-bound manner or can exploit other operational efficiencies. Nayak and Regy (2022) report data on pendency at NCLTs, defined as the number of cases filed but not yet disposed of by the court. We verify that DRT effectiveness is positively related to pendency with a correlation of 0.3. In other words, greater DRT inefficiency is associated with lower pendency in the relevant NCLT.

We consider a state to be treated if its DRT has low recovery ex-ante – it has a below-median ratio of loan recovery to pending amount. We then examine differential changes in RPT outflows before and after the reform, in the treated states relative to other states. The equation to be estimated is:

$$\log(y_{i(s)t}) = \beta_1 \text{Treated}_i \times \text{Post}_t + \beta_2 \text{Treated}_i \times \text{Post}_t \times \text{Low Recovery}_s + \delta X_{it} + \gamma_f + \gamma_s \times \gamma_t + \gamma_d \times \gamma_t + \epsilon_{it} \quad (1)$$

where y_{it} is the dependent variable defined at the firm-year level; Treated_i indicates whether firm i was in the treated category during 2014-16; Post_t indicates years 2017-19; Low Recovery_s indicates whether the firm is registered in a state which has DRT court with low recovery; and X_{it} are firm-year controls which include firm age in years and age square, log of total assets and its square, log of cash-in-hand to control for the consequences of a large demonetization policy, tangible assets to total assets ratio, and firm profitability proxied by the ratio of EBITDA to total assets.[11] The coefficients $\gamma_j,\ j \in \{f = \text{firm}, s = \text{state}, t = \text{time}, d = \text{industry}\}$, indicate various fixed effects. The fixed effects absorb industry- and state-specific shocks such as productivity shocks and other local shocks that may bias the main estimate. ϵ_{it} is the idiosyncratic error term.

4 Data and Summary Statistics

We make use of four primary data sources. The first is the Prowess-Dx dataset that contains firms' detailed financial statements including related party transactions.[12] We

firms' mix of capital and labor used in production. They assume that states with slower courts before 2002 are more affected by the SARFAESI law. Drawing from Amirapu (2021), they classify states with below-median scores on the fraction of cases disposed off within one year as 'low-efficiency' states.

[11] The demonetization policy, announced at 8 pm local time on November 8, 2016, canceled as a legal tender 86 percent of banknotes in circulation as of the following midnight. The cash-in-hand measure controls for the possibility that firms with higher cash in hand could drive financial RPT outflows after 2016.

[12] Related parties include insider individuals and their family members, promoters and their family members, shareholders, immediate to ultimate holding companies, and subsidiaries, among others.

limit our analysis to firms that are part of a corporate group and to the period 2014-19. Tunneling is typically linked to corporate groups where the network of companies within the group facilitates the movement of funds and where ownership structure opacity potentially helps conceal such transactions. We focus on the period from 2014 to exploit the regulation under the Companies Act in 2013 that mandated RPT disclosures. And we do not focus on the period after 2019 since these data were affected by the COVID-19 pandemic.

Missing RPT observations are replaced with a zero if a firm reported other RPT information in that year and we disregard cases where all RPTs are reported as missing – the assumption is that if a firm reports one type of RPT, it must have disclosed all of them in that year. This addresses the concern that a given missing observation indicates strategic non-compliance as opposed to it being zero. We also restrict our analysis to RPT outflows since they are more comprehensively captured by the Prowess-Dx dataset. RPT outflows are defined as outflows of cash. For instance, asset outflows refer to purchases of assets resulting in an outflow of cash from the firm.

The second data source is the data on loan originations for the universe of secured loans available from the Ministry of Corporate Affairs. The data is collected when a borrower registers the right over collateral in favor of a lender when the loan is in effect. The secured lenders have a strong incentive to ensure registration – they lose the right to liquidate the collateral on default by the borrower if the collateral is not registered in this way. Past research has shown that secured loans are over 75 percent of all loans in India (Bhue et al. (2015)) and that, in this particular dataset, the loans to listed firms cover over 50 percent of all private commercial credit (Chopra et al. (2021)).

The third and fourth datasets provide information about the quality of legal infrastructure. From a creditor perspective, a court is judged by how much it can help recover. We, therefore, construct a measure of DRT effectiveness by dividing the recoveries from these courts by total dues. The data on these variables is available from Indiastat. We also use data on congestion in civil courts, which have little to do with bankruptcy law, to conduct placebo tests. Court congestion data are available from Boehm and Oberfield (2020).

Our final sample has 222 groups. Of these, 45 groups have at least one 'treated' firm. On the firms side, there are 440 unique firms are in the sample; 80 unique firms are classified as treated. Appendix Table A1 summarises the data. Roughly 34 percent of the observations report loan RPT outflows, compared to 17 percent and 18 percent respectively for asset and investment outflows. Related party loans formed the highest share of total financial RPTs at 60 percent, followed by 21 percent for asset RPTs, and 19 percent for investment RPTs. The RPT flows were a significant share of total assets, with the mean loan RPT outflow being some 4.3 percent of total assets. The shares of asset and investment RPT outflows are significantly smaller.

The baseline approach to classify treated firms leads to 14 percent of the firms being classified as treated. However, these are large firms. For example, as of 2016, the treated firms accounted for nearly 38 percent, 33 percent, and 28 percent, respectively, of the total borrowings, bank borrowings, and total assets in the sample.

5 Results, Mechanisms, and Real Effects

Baseline results. Table 1 reports the results from estimating Equation 1. There is a statistically significant fall in the total financial RPTs in treated firms in low recovery states after the implementation of IBC (column one). The coefficient size implies that total RPT outflows fell by 91 percent following the IBC in the states with low DRT court recovery. Interestingly, we find an increase in financial RPT outflows among the treated firms in the high DRT recovery states; the treated $\times$ post coefficient is positive and statistically significant. Other policies that were implemented around the same time as the IBC law could incentivize the treated firms to conduct more tunneling. Nevertheless, the positive coefficient is smaller in magnitude compare to the negative triple interaction coefficient, so financial RPT fell after IBC in states with low recovery in the pre-period.[13]

We decompose the fall in financial RPTs into its components in columns two to four. The fall in financial RPTs is driven by fall in related party loans (column two) and asset flows (column three), not investment flows (column four). The fall in loan RPTs can be explained by the fact that banks and other creditors have a comparative advantage in identifying the mis-pricing of credit products. They are less adept at assessing non-financial products since their pricing can be industry-specific. Moreover, loan RPTs and related party loan guarantees are known to be predominant ways to tunnel resources from firms (Asia-Pacific Office of the CFA Institute Centre for Financial Market Integrity (2009); Jiang et al. (2010)). Our evidence corroborates this in the context of India.

Figure 1 illustrates the decomposition of the baseline estimate into year-by-year effects. The triple interaction effects are negative but are not statistically significant in the pre-IBC years (2014 and 2015). This reinforces our identification strategy – there are no significant pre-trends in differential financial RPT outflows between treated firms relative to control firms, in states with less efficient courts prior to the reform. The coefficient of interest is strongly negative in the first two years after the policy. In the third year, the effect size is smaller and is less precisely estimated. This might indicate that the deterrence brought in by IBC is limited to the large borrowers (as indicated by policy reports such as Standing Committee on Finance (2021)) who are the subject of this paper due to its focus on group firms. Alternatively, the new court system could be catching

[13] In a difference-in-differencespecification, we show in Appendix Table A10 that financial RPTs fell on average in treated firms after the IBC. While the treated $\times$ post coefficient is negative in this regression, it is imprecisely estimated.

up to its predecessor in terms of judicial inefficiency and delay, driven possibly by understaffed courts, which is against the spirit and letter of the IBC legislation (Insolvency and Bankruptcy Board of India (2022)).

Placebo regressions and robustness. While the benchmark results are robust to specifications with, and without, controls and fixed effects, spurious correlations can still generate the same results. Appendix Table A4 reports the results of a suite of robustness checks. First, we run a placebo regression, restricting the sample to before 2016 and replacing the post indicator with a 'pseudo treatment period dummy with 2015 as the treatment year. The estimated triple-interaction coefficient is not significantly different from zero.

We then test to see if there is a fall in financial RPT outflows for standalone firms. Column two in Table A4 indicates no such effect. Finally, we ask if the results hold for an alternate 'treatment intensity' variable. In the baseline model, the ex-ante efficiency of DRT courts is used as a measure of treatment intensity. We replace this with a measure of congestion in civil courts. Since we argue that the IBC worked by improving creditor rights, what should matter for impacts is the congestion or inefficiency in debt recovery courts, not civil courts engaged in broader legal issues. Table A4 confirms that our results are not influenced by firms' ability to access civil courts.

We evaluate the impact of the law on operational RPTs. Such RPTs are less likely to be used for sustained tunneling from unhealthy firms (financial RPTs arguably are easier to undertake). Furthermore, even if tunneling is conducted with operational RPTs through mis-priced transactions, it is unlikely that creditors will be able to detect mis-pricing that can be industry and time-specific. In any case, we do not expect to see any impact of the IBC law on operational RPT outflows – as can be seen (in column four), the advent of the IBC did not induce any fall in these transactions.

Another concern is that the main results are biased by zero-valued financial RPTs. An emerging literature argues that taking logs in the presence of zeros can lead to very high coefficient magnitudes in difference-in-difference estimations (Chen and Roth (2024); Mullahy and Norton (2024)). We address this in two ways and the results are in Appendix Table A3. First, we split the outcomes into intensive and extensive margins. For the intensive margin, we drop zero RPT values by taking log(financial RPT); for the extensive margin, we use a dummy for positive financial RPTs. In columns one and two of Appendix Table A3, we find that the effect is statistically significant only for the extensive margin. The coefficient remains negative for the intensive margin, but it is not statistically significant. Second, we run a pseudo-Poisson maximum likelihood estimator on the level of financial RPTs with zeros as part of the sample. This estimator is robust to the biases created by zero values. We find that the estimated coefficient (in column four) is negative and now statistically significant.

Further, we evaluate the robustness of our baseline result to alternate criteria used

to identify treated firms (Appendix Table A8) and for outliers in the RPT data by winsorizing the data at 5% (Appendix Table A9). The results are robust to broader definitions of treated firms that do not restrict treatment based on credit ratings or on an increase in debt during the pre-period. Similarly, outliers in the RPT data do not bias the main results in an important way. We then test the validity of pre-period parallel trends for other relevant variables (Appendix Figure A2). Across the other variables, the impact is visible only after the enactment of the IBC law, which strengthens the causal interpretation of our results.

5.1 Mechanisms

We next consider the mechanisms through which the IBC reduced tunneling. There are two potential forces at play. First, emboldened banks and creditors can restrict the scope for tunneling by refusing to extend loans to treated firms. This is the creditor-led disciplining mechanism. Second, claw back provisions within the IBC to recover funds from personal assets may encourage self-discipline by managers to avoid bankruptcy. This is the borrower-led disciplining mechanism.

We proceed with the analysis as follows. First, we confirm, using our baseline specification, that borrowings indeed fell after the implementation of IBC. As we argue above, this could be driven by supply or demand factors. The second set of tests aim to hone in on which of these factors are in play.

Table 2 considers the impact of the IBC on firms' borrowings and the cost of borrowing using Equation 1. In columns one and two, the dependent variable is the change in the stock of total firm borrowing as a ratio of total assets. In columns three and four, the dependent variable is the change in the stock of bank borrowing as a ratio of total assets. The triple-interaction coefficient in column one is negative and significant at the 10 percent level. The effect is stronger, in terms of both coefficient magnitude and precision (five percent level of significance) in the first year following the IBC. Columns three and four show that the fall is driven primarily by bank borrowings. The coefficients are negative and statistically significant at the five percent level in the first year following the IBC reform.

The results so far in Table 2 establish a fall in credit. Whether it is creditor-led or borrower-led, the results imply that greater creditor empowerment leads to a fall in credit usage for the treated firms. Creditors can initiate, or credibly threaten to initiate, insolvency proceedings against defaulters, which would lead to the loss of the debtor's control over the firm.

Next, we test if the interest cost goes up, which serves as one test for creditor-led disciplining. The treated firms had low-interest debt in the pre-period, possibly because the lenders were unwilling to charge interest rates commensurate with the risk to avoid

bankruptcy. With higher expected recoveries after the IBC, the lender will be less sensitive to a firm's bankruptcy. The fall in credit is, however, not driven by an increase in the interest rate charged by banks, as seen in columns five and six.

Are firms finding other sources to supplement funds from a fall in RPTs to fund credit repayment? We test if the treated firms experienced higher inflows or lower outflows in Table 3. In particular, we test for funds generated from all financial activities (column one), non-borrowing financial activities (column two), financial income (column three), financial RPT inflows (column four), operational RPT inflows (column five), and dividend payments (column six). We find no impact on firms raising money externally in the form of equity or debt, and there is no evidence of prior RPTs being returned by the beneficiaries in the form of RPT inflows (whether as financial or operational RPTs) after IBC.

Interestingly, there is a statistically significant fall in dividend payouts. Dividend payouts themselves can be a form of tunneling, and our evidence suggests that the IBC had a mitigating effect on dividend payouts from these distressed firms.citation?.

Discussion – tunneling vs propping. A number of papers suggest that weak firms in groups are propped up with funds accessed from related parties (Jian and Wong (2010); Kashyap et al. (2023)). However, in our sample, the fact that there is no increase in RPT inflows shows that other firms in the group are unlikely to be propping treated firms.

Finally, we refine our test for creditor-led versus borrower-led channels by exploiting information on firm links to unhealthy banks in the MCA data. Following Kulkarni et al. (2023), a bank is classified to be 'weak' if its Common Equity Tier 1 capital adequacy ratio according to Basel III norms was in the bottom tercile across all banks; the weak bank dummy is assigned a value of zero if a bank is the top tercile; banks in the middle tercile are assigned a missing value. A firm is said to be linked to a weak bank if its weighted exposure between 2006-2015 to any weak bank was above the median of the firm-bank distribution.

The hypothesis is as follows. Creditor-led channel is relevant only for banks that can absorb bankruptcy losses, i.e., the banks that are well capitalized. Recent research in the context of IBC law shows that the impact on the supply of loans is limited to healthy banks (Kulkarni et al. (2023)). From this perspective, a decline in financial RPTs and borrowings for firms connected to weak banks implies that the firm is willingly cutting down their exposure to such banks – a borrower-led channel.

We follow Kashyap et al. (2023) to construct a firm-bank-year loans panel dataset from 2006 to 2015 using the MCA data. We then construct a dummy for firm exposures to weak banks (where weak banks are defined as those in the lowest tercile of capitalization) that takes the value one if a firm had above median exposure to any weak bank.

The results are in Table 4. We find no significant difference in the declines in financial

RPTs (columns one and two), borrowings (columns three and four), or dividend payouts (columns five and six) for firms connected to weak banks relative to firms that are not connected to weak banks. These results support the argument that borrowers willingly repay credit due to the fear of costs imposed by bankruptcy.

Taken together, the results in Tables 2, 3, and 4 support the notion that creditor empowerment played an important role in deterring tunneling activity. We conclude that the treated firms cut down on related party transactions and dividend payouts to repay bank credit.

5.2 Other Outcomes

We test for shareholder-led disciplining by considering firm director and firm-level outcomes using Equation 1. These results are reported in Appendix Table A5. In columns one to three, we consider director remuneration, entry, and exit of acting directors (where acting is defined as drawing remuneration). We do not find any change in these variables, which is not surprising given that IBC did not lead to the empowerment of shareholders. Surprisingly, we do not find any improvements in the treated firm's financial outcomes as measured by return on equity (ROE), return on assets (ROA) or profits. (Appendix Table A6).

Next, we test if the IBC had any impact on the treated firms' real outcomes. The results are in Appendix Table A7. In particular, we look at the impact on firms' sales (column one), salaries (column two), and investments (column three). We do not find any meaningful differences between the treated and control firms in terms of the financial ratios and other real effects.

6 Conclusion

In this paper, we draw on a natural experiment – the introduction of a legal reform in India – to show that strengthened creditor rights can reduce tunneling activity by a firm's managers. We use data on related party transactions among group firms, along with a triple-difference estimation strategy exploiting institutional variation in debt recovery court efficiency across states in India, to show that the advent of the Insolvency and Bankruptcy Code empowering creditors resulted in decreased tunneling. Notably, we find evidence of borrower-led disciplining, whereby firms pay off outstanding loans to avoid the costs of bankruptcy initiated by more empowered creditors.

Our findings highlight the important role of strong judicial institutions in improving resource allocation and promoting economic development. An efficient court system that focuses on hastening bankruptcy resolution and the recovery of non-performing assets is critical in this process.

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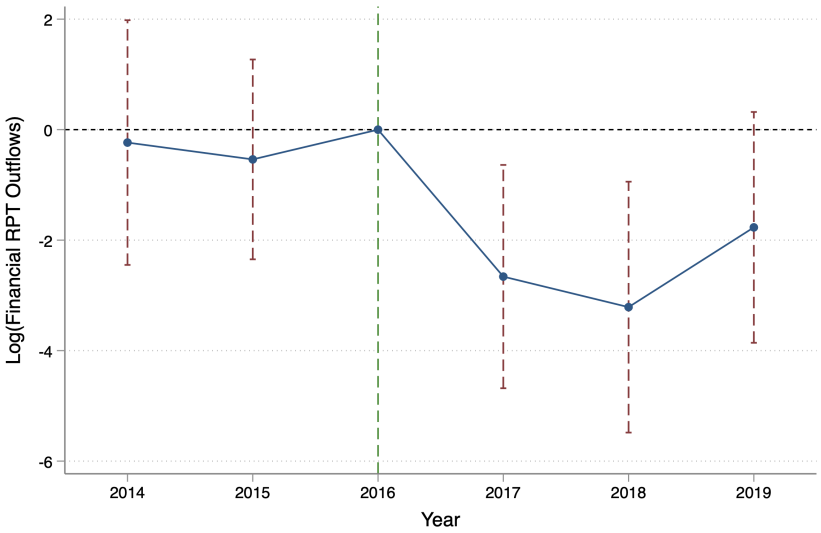
Table 1: Results - Financial RPT Outflows and IBC

	(1) All Fin. RPTs	(2) Loans	(3) Assets	(4) Investment
Treated=1 × Post=1	1.16**	1.22**	0.32	0.71**
	(0.53)	(0.49)	(0.23)	(0.34)
Treated=1 × Post=1 × Low Recovery=1	-2.41***	-2.01***	-0.86**	-0.77
	(0.71)	(0.61)	(0.36)	(0.61)
Constant	3.79*	3.18	2.33	-0.21
	(2.18)	(2.16)	(2.43)	(1.81)
Observations	2,031	2,031	2,031	2,031
R^2	0.725	0.714	0.661	0.662

Notes: This table reports the results from estimating Equation 1 with the logs of total financial RPTs (column one), loan RPTs (column two), asset RPTs (column three), and investment RPTs (column four) as dependent variables. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Figure 1: Effect of IBC Law Over Time



Notes: This figure reports the effect of the IBC law over time. The dependent variable is the log of total financial RPT outflows at the firm-year level. Each point represents the triple interaction coefficient treated × year × low recovery; the dashed lines around each coefficient represent 95 percent confidence intervals. The dashed green vertical line corresponds to the year of IBC law implementation. The dynamic effects for other relevant variables are presented in Figure A2 in the appendix.

Table 2: Firm borrowings and Interest Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Borrowings		Bank Borrowings		Interest Rate	
Treated=1 × Post=1 × Low Recovery=1	-1.41*		-1.21**		16.66	
	(0.74)		(0.62)		(12.77)	
Treated=1 × year=2014 × Low Recovery=1		0.25		0.28		6.37
		(0.53)		(0.43)		(18.44)
Treated=1 × year=2015 × Low Recovery=1		-0.03		-0.02		29.20
		(0.36)		(0.28)		(21.68)
Treated=1 × year=2017 × Low Recovery=1		-1.45**		-1.10**		18.49
		(0.63)		(0.53)		(14.10)
Treated=1 × year=2018 × Low Recovery=1		-1.20		-1.18*		17.10
		(0.77)		(0.62)		(16.12)
Treated=1 × year=2019 × Low Recovery=1		-1.36		-1.12		50.29
		(0.86)		(0.73)		(41.26)
Observations	1,970	1,970	1,970	1,970	1,184	1,184
R^2	0.674	0.675	0.788	0.789	0.621	0.627

Notes: This table reports the results from estimating Equation 1 with (each as a ratio of total assets) the change in total borrowings (columns one and two), the change in bank borrowings (columns three and four), both expressed as a ratio of total assets, as dependent variables. In columns five and six, the dependent variable is the long term interest rate. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Alternate Funding Sources

	(1) Cash Inflow (All Financing)	(2) Cash Inflow (Non-Debt Financing)	(3) Financial Income	(4) Financial RPT Inflow	(5) Operational RPT Inflow	(6) Dividend Payouts
Treated=1 × Post=1 × Low Recovery=1	-0.19	-0.17	-0.45	-0.10	-0.70	-0.67**
	(0.79)	(0.71)	(0.35)	(0.86)	(0.56)	(0.33)
Observations	2,031	2,028	2,031	2,031	2,031	2,031
R^2	0.709	0.545	0.891	0.651	0.833	0.833

Notes: This table reports the results from testing alternate sources of funds accessed by firms' managers. We estimate Equation 1 with the logs of all financing-related cash inflows (column one), debt-financing cash inflows (column two), income from financial activities (column three), inflows from financial RPTs (column four), inflows from operational RPTs (column five), and dividend payouts (column six). In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Results – Demand Vs. Supply Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
	Weak Bank Exposure?					
	Yes	No	Yes	No	Yes	No
	Financial RPTs		Loan RPTs		Dividend Payouts	
Treated=1 × Post=1 × Low Recovery=1	-3.62***	-1.68	-2.45*	-2.52*	-1.41*	-1.06*
	(1.22)	(1.62)	(1.28)	(1.33)	(0.83)	(0.58)
Observations	553	634	553	634	553	634
R^2	0.823	0.782	0.827	0.786	0.887	0.867

Notes: This table reports the results from estimating the role of demand (i.e., borrower) versus supply (i.e., lender) forces by using firms' links to weak banks. Following Kulkarni et al. (2023), a bank is classified to be 'weak' if its Common Equity Tier 1 capital adequacy ratio according to Basel III norms was in the bottom tercile across all banks; the weak bank dummy is assigned a value of zero if a bank is the top tercile; banks in the middle tercile are assigned a missing value. A firm is said to be linked to a weak bank if its weighted exposure between 2006-2015 to any weak bank was above the median of the firm-bank distribution.

We estimate Equation 1 with the logs of financial RPT outflows (columns one and two), loan RPT outflows (columns three and four), and dividend payouts (columns five and six). In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

7 Online Appendix

Table A1: Pre-Period Summary Statistics - Related Party Transactions

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: Features of the RPT Data (Pre-Period)					
Dummy, Loan RPT > 0	0.352	0.478	0	1	1,168
Dummy, Asset RPT > 0	0.188	0.39	0	1	1,168
Dummy, Investment RPT > 0	0.18	0.384	0	1	1,168
		As a Fraction of Financial RPTs			
Share, Loan RPTs	0.586	0.457	0	1	572
Share, Asset RPTs	0.227	0.403	0	1	572
Share, Investment RPTs	0.188	0.352	0	1	572
		As a Fraction of Total Assets			
Share, Loan RPTs	0.038	0.183	0	4.474	1,168
Share, Asset RPTs	0.006	0.052	0	1.179	1,168
Share, Investment RPTs	0.007	0.038	0	0.612	1,168
Panel B: Summary Statistics (Full Sample)					
Count, Firms (by year)	338.83	59.86	257	407	6
Count, Groups (by year)	57.67	7.84	48	69	6
Treated Dummy	0.14	0.347	0	1	2,033
Low Recovery Dummy	0.558	0.497	0	1	2,033
Legal Congestion Dummy	0.538	0.499	0	1	2,033
Firm Age	25.504	22.374	1	156	2,033
Total Assets (Log)	8.577	1.875	1.065	13.96	2,033
Tangibility	0.334	0.275	0	1.044	2,033
EBITDA	0.048	0.104	-0.672	0.542	2,033
Cash in Hand (Log)	0.859	0.899	0	5.455	2,033
Financial RPT Outflows (Log)	2.317	3.125	0	12.699	2,033
Loan RPT Outflows (Log)	1.721	2.86	0	12.695	2,033
Asset RPT Outflows (Log)	0.567	1.625	0	9.375	2,033
Investment RPT Outflows (Log)	0.805	2.124	0	10.912	2,033
Growth, All Borrowings	-0.074	2.621	-101.632	1.709	1,987
Growth, Bank Borrowings	-0.052	1.922	-84.947	1.755	1,987
Interest Rate (%)	19.083	51.39	0	977.811	1,349
Cash Inflow, Financing (Log)	5.894	2.506	0	12.998	1,553
Cash Inflow, Non-Credit Financing (Log)	1.445	2.647	0	10.647	1,437
Income, Financial Services (Log)	3.119	2.135	0.095	10.904	1,796
Inflow, Financial RPTs (Log)	2.65	3.132	0	12.796	2,033
Inflow, Operational RPTs (Log)	2.747	2.976	0	11.917	2,033
Dividend Payouts (Log)	0.638	1.583	-0.223	9.689	2,033

Notes: This table reports features of the RPT data during 2014-16 (Panel A) and the summary statistics of the important variables in the full sample (Panel B). In Panel A, the first three rows summarize the extensive margin of components of financial RPT, i.e., the probability that a firm-year observation is positive. Rows four to six report the same components of financial RPT as a share of total financial RPTs, which is a sum of the three components. The last three rows summarize the same financial RPT components as a share of firms' total assets.

Table A2: Tunneling – Pre-Period

	(1)	(2)
Treated=1 × Weak Bank Exposure=1	2.23**	-2.25
	(0.91)	(1.59)
Treated=1 × Weak Bank Exposure=1 × Asset Quintile=1		1.64
		(2.27)
Treated=1 × Weak Bank Exposure=1 × Asset Quintile=2		4.06*
		(2.09)
Treated=1 × Weak Bank Exposure=1 × Asset Quintile=4		3.88*
		(2.20)
Treated=1 × Weak Bank Exposure=1 × Asset Quintile=5		4.78**
		(2.09)
Observations	791	791
R^2	0.391	0.518

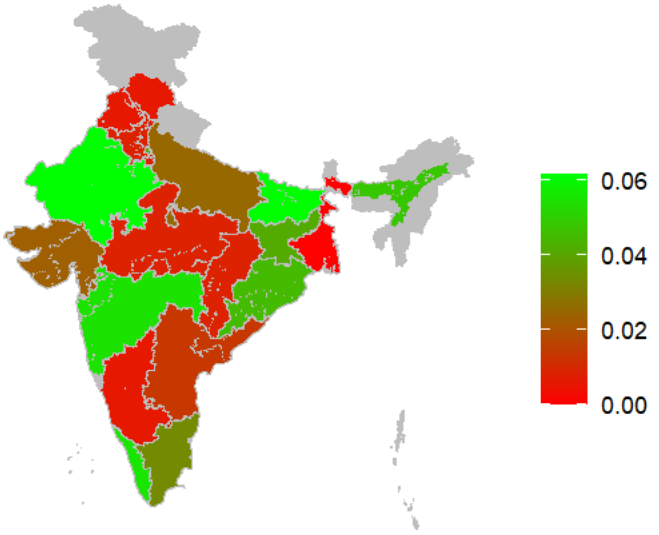
Notes: This table tests whether treated firms were more likely to conduct financial RPT in the pre-period if they were exposed to a weak bank. The weak bank indicator takes a value of one if a firm's exposure to a weak bank was higher-than-median exposure across firm-bank links. We use the loan-level data from the Ministry of Corporate Affairs to construct firm-bank exposures between 2006 to 2016 (Kashyap et al. (2023)). A bank is considered weak if it is in the bottom tercile of the banks' capital ratio distribution between 2014-16; banks in the top tercile are considered healthy (Kulkarni et al. (2023)). Column one reports the results from regressing the financial RPTs against the treatment dummy and the weak bank exposure indicator. Column two reports the results from a regression where the treated × weak bank exposure is further interacted with the firm's pre-period asset quintile (the third quintile is taken as the base). The specification includes industry, state, and year fixed effects; controls include age, age squared, cash in hand, tangibility, and profitability. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: Results - Margins of Fall in RPTs

	(1) Baseline	(2) Int. Marg	(3) Ext. Marg	(4) PPML
Treated=1 × Post=1 × Low Recovery=1	-2.41***	-2.03	-0.31***	-2.67**
	(0.71)	(1.56)	(0.11)	(1.12)
Observations	2,031	760	2,031	1,444
R^2	0.725	0.773	0.665	

Notes: This table decomposes the benchmark result in Table 1 into extensive and intensive margins as well as tests whether results hold when zero-value robust econometric techniques are used. In column one, we reproduce the baseline result. In column two, we do not add one to financial RPT before taking log. This represents changes along the intensive margin since only the positive financial RPTs are considered. In column three, the dependent variable is a dummy that takes a value of one if the financial RPT is positive. In column four, we conduct a pseudo-Poisson maximum likelihood estimation a zero-value robust procedure recommended in recent research (Chen and Roth (2024); Mullahy and Norton (2024)) with the level of financial RPT as the dependent variable. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Figure A1: Recovery Effectiveness of DRT Courts Across States



Notes: This figure reports the effectiveness of Debt Recovery Tribunals (DRTs) across Indian states. The effectiveness is measured as the total recoveries to total dues. The data is taken from IndiaStat. Gray shades indicate states for which the data is not available.

Table A4: Placebo Regressions

	(1) Treat Year = 2015	(2) Standalone Firms	(3) Legal Congestion	(4) Operational RPTs
Treated=1 × Post2015=1 × Low Recovery=1	-0.19			
	(0.96)			
Treated=1 × Post=1 × Low Recovery=1		0.24		-0.09
		(0.38)		(0.54)
Treated=1 × Post=1 × Civil Congestion=1			-0.49	
			(0.77)	
Observations	1,150	7,266	2,031	2,031
R^2	0.800	0.669	0.722	0.842

Notes: This table reports the results from placebo regressions which validate our baseline results. In columns one through three, the dependent variable is financial RPTs. In column one, we test if policies other than IBC, which were implemented in 2015, drive our main results. In column two, we use our baseline specification to test if the IBC law impacted financial RPTs among a sample of non-group (i.e., standalone) firms. In column three, we test if court congestion in civil courts (which are not directly related to IBC law) is important. In column four, we test if the IBC law impacted operational RPTs in our sample in the baseline regression. All columns include firm, industry-year, and state-year fixed effects. Controls include age, age squared, logs of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A5: Results - Directors' Outcomes

	(1) Director Remuneration	(2) New Directors	(3) Director Exits
Treated=1 × Post=1 × Low Recovery=1	-0.56	0.88	0.29
	(1.17)	(0.59)	(0.53)
Observations	1,878	1,878	1,878
R^2	0.801	0.473	0.491

Notes: This table reports the results from estimating Equation 1 for three directors' outcome related variables. In the first column, the dependent variable is the log of the total remuneration paid to the company's directors. In the second and third columns, the outcomes are the total number of new and exiting acting directors. A director is defined as an acting director if they were paid a remuneration. A director is defined as new/exiting if they are not observed in the preceding/following three years. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A6: Results - Firm Financial Outcomes

	(1) RoE	(2) RoA	(3) Profits
Treated=1 × Post=1 × Low Recovery=1	24.09	-0.01	-0.01
	(35.96)	(0.02)	(0.02)
Observations	2,031	2,031	2,031
R^2	0.397	0.884	0.882

Notes: This table reports the results from estimating Equation 1 for firms' financial outcomes. In columns one through three, the dependent variables are, respectively, return to equity, return to assets, and retained profits to total assets ratio. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A7: Results - Real Effects of IBC

	(1) Sales	(2) Salaries	(3) Investments
Treated=1 × Post=1 × Low Recovery=1	0.10	0.04	-0.44
	(0.31)	(0.17)	(0.56)
Observations	1,968	1,961	1,779
R^2	0.932	0.972	0.812

Notes: This table reports the results from estimating Equation 1 with real outcomes. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A8: Robustness to Treated Firms Classification

	(1)	(2)
	Treated = 1 if	
	ICR < 1 + Low Cost	ICR < 1 + Low Cost + Increasing Debt
Treated=1 × Post=1 × Low Recovery=1	-0.89*	
	(0.49)	
Treated=1 × Post=1 × Low Recovery=1		-0.98*
		(0.53)
Observations	2,373	2,373
R^2	0.720	0.720

Notes: This table reports the results testing the robustness of the baseline results in column two of Table 1 to alternate methods of defining the treated set of firms. In the baseline, a firm is classified as treated if it meets all of the following five criteria in any of the years between 2014-16: i. has insufficient cashflows to meet credit costs (interest rate coverage ratio < 1); ii. credit costs are below the prime lending rates of the largest public sector bank (State Bank of India); iii. saw an increase in borrowings between 2014 and 2016; iv. the debt to assets ratio is above 20 percent; and v. debt is not AAA rated. In this table, we define broader measures that impose fewer restrictions. In column one, the firms that meet conditions i and ii are classified as treated; in column two, firms that meet conditions i, ii, and iii are classified as treated. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A9: Robustness to Winsorizing the RPT Data

	(1) Baseline	(2) Loans	(3) Assets	(4) Investment
Treated=1 × Post=1 × Low Recovery=1	-1.69***	-1.38***	-0.14	-0.21
	(0.57)	(0.47)	(0.10)	(0.14)
Observations	2,031	2,031	2,031	2,031
R^2	0.694	0.677	0.645	0.648

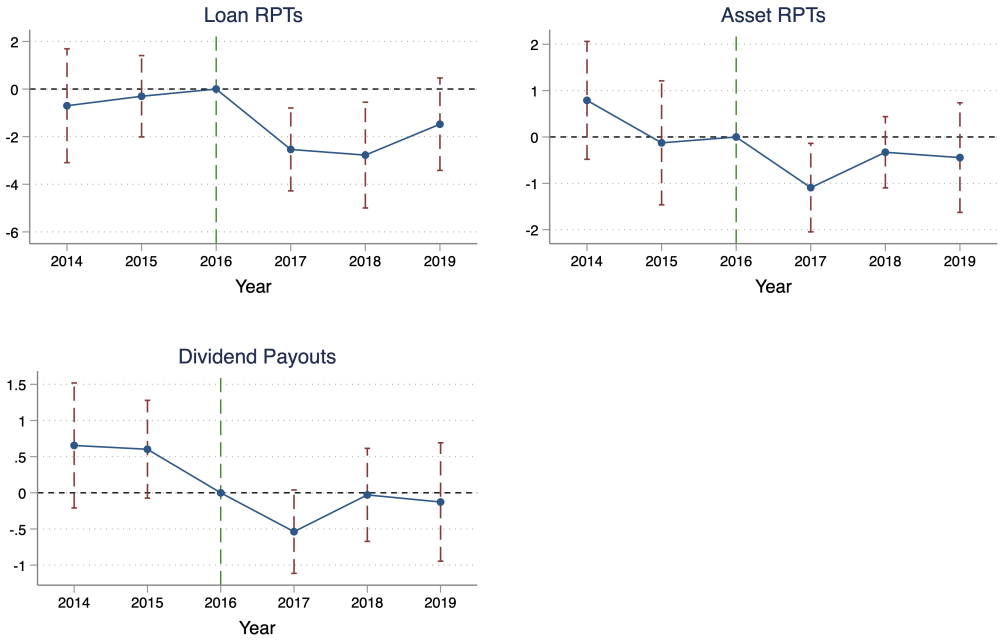
Notes: This table reports the results testing the robustness of the baseline results in Table 1 to outliers in the RPT data as seen in the summary statistics Table A1. We winsorize each variable by replacing 5% of observations on each tail. In each specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the triple interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Table A10: Results - Difference in Differences

	(1) No FE	(2) Firm FE	(3) + Controls	(4) Baseline
Treated=1 × Post=1	-0.45	-0.58	-0.53	-0.53
	(0.39)	(0.38)	(0.35)	(0.37)
Observations	2031	2031	2031	2031
R^2	0.010	0.631	0.649	0.649

Notes: This table reports the results from estimating a difference-in-differences model where the log financial RPT is regressed on an interaction term between the treatment dummy and the post dummy. Column one reports results from a regression without fixed effects or controls; in column two, firm fixed effects are included; in column three, firm fixed effects and controls are included; in column four, we estimate a regression with all the fixed effects and controls as in the baseline regression. In the baseline specification, firm, industry-year, and state-year fixed effects are included; controls include age, age squared, log of total assets and its square, cash in hand, tangibility, and profitability. Only the main coefficient of interest – the double interaction coefficient – is reported for each regression. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.

Figure A2: Effect of IBC Law Over Time – All Relevant Variables



Notes: This figure reports the effect of the IBC law over time for relevant variables (the same test for the main variable, total financial RPTs, is presented in Figure 1). Each point represents the triple interaction coefficient treated × year × low recovery; the dashed lines around each coefficient represent 95 percent confidence intervals. The dashed green vertical line corresponds to the year of IBC law implementation.