Does a Creditor-Friendly Bankruptcy Reform Decrease Firms' Access to Equity Capital? Evidence from a Quasi-Natural Experiment

Aaraadhya Srivastava[1]

Abstract

We argue that creditor-friendly bankruptcy reforms, such as India's 2016 Insolvency and Bankruptcy Code (IBC), increase the *ex-ante* probability of bankruptcy, leading risk-averse equity investors to reduce investments in firms more exposed to the reform. To test this hypothesis, we exploit between-firm variation in the 'propensity to default' for identification and employ the difference-in-differences (DiD) technique, leveraging the IBC reform as an exogenous policy shock. Our findings reveal a decline in equity inflows for financially distressed firms relative to their non-distressed counterparts in the post-reform period. These results are robust to firm covariate matching, alternative dependent variable specifications, and a placebo test using a fake reform year. Consistent with our hypothesis, the treatment effect is concentrated in standalone firms, which lack access to internal capital markets and therefore face higher bankruptcy risk post-reform. Consequently, we document a reduction in capital expenditure by standalone treated firms relative to their control counterparts in the post-reform period, with no such effects observed for group-affiliated firms. These findings provide policymakers with valuable insights into the unintended consequences of creditor-friendly bankruptcy reforms, offering lessons for future policy initiatives.

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[1] Economics and Public Policy Area, Indian Institute of Management Raipur, Atal Nagar P.O. – Kurru, Abhanpur, Chhattisgarh – 493661, India. Email: 22fpm001@iimraipur.ac.in

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1. Introduction

How does bankruptcy risk affect firms' access to equity capital? Bankruptcy risk is an important factor in an equity holder's decision to supply capital to firms because, in the event of bankruptcy, a firm's assets are transferred from equity holders to debt holders (Rajan and Zingales, 1995). As a result, equity holders lose control over the firm's assets and incur financial losses. However, this transfer occurs only when creditors' rights are protected and well-enforced within a given legal system (La Porta et al., 1998). Therefore, creditors' rights provide the legal foundation for bankruptcy risk to affect equity holders' investment decisions. For instance, when creditors have weak legal claims over a firm's assets, equity holders may discount or assign less importance to bankruptcy risk in their investment decisions. Conversely, if a reform strengthens creditors' legal claims on firms' assets, bankruptcy risk becomes more real for equity holders. Given that bankruptcy entails both the loss of control over a firm's assets and accompanying financial losses for equity holders, they may reduce their supply of equity capital to firms following the enactment of such a reform. This suggests that stronger creditor rights should reduce firms' access to equity capital. Despite this, much of the existing literature has focused on the *ex-post* consequences of creditors' rights on firms' activities, governance, and use of credit.[2] However the potential effects of creditors' rights on equity capital remain understudied. This paper seeks to address this gap.

[2] For a brief review of the literature on creditors' rights, please see La Porta et al. (1997); La Porta et al. (1998); Levine (1998), Levine (1999); Dahiya et al. (2003); Beck et al. (2005); Qian and Strahan (2007); Djankov et al. (2007); Acharya and Subramanian (2009); Visaria (2009); Haselmann et al. (2010); Acharya et al. (2011); Goodman and Levitin (2014); Fauceglia (2015); Ponticelli and Alencar (2016); Gopalan et al., (2016); Mann (2018); Tantri (2020); Bose et al. (2021); Alok et al. (2022) among others.

From a theoretical perspective, the *ex-ante* impact of stronger creditors' rights on firms' equity capital is *a priori* ambiguous. For instance, in the event of bankruptcy, firms' assets are transferred from equity holders to debt holders (Rajan and Zingales, 1995). To protect their firm from bankruptcy and the accompanying loss of control, shareholders may infuse additional capital into the firms. Moreover, the potential loss of control may discipline controlling shareholders (referred to as 'promoters' in the Indian context) and mitigate principal-principal agency conflicts between promoters and external shareholders (Young et al., 2018). This dynamic could encourage outside shareholders to invest more in promoter-controlled firms.

Conversely, stronger creditors' rights, *ceteris paribus*, increase the *ex-ante* likelihood of bankruptcy. Shareholders, as residual claimants of their firms' liquidation value, may fear partial or complete write-offs of their investments due to bankruptcy-related liquidation or reorganization. This could dissuade risk-averse shareholders from investing in firms more exposed to higher creditors' rights.

Furthermore, stronger creditors' rights exacerbate agency conflicts between shareholders and creditors. These conflicts arise from the asymmetry in their payoffs. While shareholders face a convex payoff structure and receive non-contractible private benefits from their firm, creditors have a concave payoff arrangement with their debtors (Kariya, 2021). In this regard, an increase in lenders' rights might lead creditors to sub-optimally liquidate firms with positive continuation value (Aghion et al., 1992; Shleifer and Vishny, 1992; Acharya et al., 2011; Assunção et al., 2014; Acharya and Thakor, 2016). Related studies highlight that managers of bankrupt firms often struggle to find re-employment at comparable wages (Gilson, 1989; Eckbo et al., 2016). Consequently, career concerns may drive managers to pursue low-risk, value-decreasing projects under stronger creditors' rights (Acharya et al., 2011), potentially deterring wealth-maximizing shareholders from investing in such firms. Given the theoretical conflict regarding the *ex-ante* impact of creditors' rights on equity capital, we leverage the enactment of a creditor-friendly reform in India to shed light on this issue.

The 2016 Indian bankruptcy law provides the economic setting for our study. In the financial year[3] 2016, the Government of India (GoI) introduced the Insolvency and Bankruptcy Code (IBC) to establish a unified framework for resolving corporate insolvencies in a timely manner[4] (Gopalan et al., 2016; Bose et al., 2021; Singh et al., 2021; Jadiyappa and Shrivastav, 2022; Singh et al., 2022; Agarwal and Singhvi, 2023; Jadiyappa and Kakani, 2023; Singh et al., 2023; Srivastava, 2024; Singh et al., 2024; Jadiyappa and Hickman, 2025). Before the IBC, bankruptcy proceedings in India were managed by multiple entities, including the Debt Recovery Tribunals (DRTs), the Board for Industrial and Financial Reconstruction (BIFR), and traditional courts (Jadiyappa and Kakani, 2023). This fragmented system led to inefficiencies, delaying resolutions and weakening the enforcement of creditor rights. For instance, prior to the IBC, the average bankruptcy resolution process spanned approximately 10 years, with creditors recovering just 12% of asset value (Chakrabarti et al., 2008).

The legal complexities of the pre-IBC period undermined creditor rights, discouraging key stakeholders such as managers, shareholders, and lenders, from making sound financial and operational choices (Bose et al., 2021). To address these shortcomings, Indian regulators introduced the IBC to enhance creditor protection. The code enforces a structured and time-sensitive insolvency resolution mechanism, setting a 330-day deadline under Section 12(3). These reforms significantly expedited case resolutions, reducing the average settlement period

[3] A financial year (FY) in India spans a period of 12 months from April 01st of the current year to March 31st of the subsequent year.
[4] For instance, in the case of *"Committee of Creditors of Essar Steel India Limited Vs Satish Kumar Gupta & Ors. [Civil Appeal No. 8766-67 OF 2019]"*, the Supreme Court of India (nation's apex judicial authority) ruled that any case of corporate insolvency must ordinarily be completed within 330 days from the commencement of corporate insolvency resolution process.

to 415 days[5] and lowering *ex-post* direct bankruptcy costs to 1% of the liquidation value (Annual Report of the Insolvency and Bankruptcy Board of India, 2019–20). Consequently, post-IBC, creditors have recovered assets more efficiently than in the pre-IBC period. Thus, it is reasonable to characterize the IBC reform as creditor-friendly in nature.

We examine our hypothesis using data from Indian manufacturing firms[6] over the period 2011-2020. To determine causality between creditors' rights and equity capital, we employ the difference-in-differences (DiD) method, premised on the implementation of the IBC reform in 2016. However, since the IBC was rolled out nationwide, a significant empirical challenge arises from the absence of a natural 'control' group. Furthermore, the IBC doesn't provide any economically meaningful threshold in terms of the loan value for its application. Therefore, we cannot rely on discontinuities arising from the law itself to identify the treatment and control firms.

To address the issue of unavailability of a natural control group, we follow Bose et al. (2021) and exploit cross-sectional heterogeneity in the reform's impact to categorize the sample into the treatment and control groups. Specifically, we use information on between-firm variation in 'propensity to default' for the pre-IBC period to identify our treatment and control firms. 'Propensity to default' is measured using firms' Altman Z-score (hereafter, Z-score). Firms with an average pre-IBC Z-score below 1.81 are categorized as 'distressed' (Altman, 1968; Singh et al., 2021; Srivastava, 2024; Jadiyappa and Hickman, 2025). For our analysis, we classify distressed firms as treated and non-distressed firms as control. This

[5] The corporate insolvency resolution process (CIRP) must be completed within 330 days. However, cases originating under the Sick Industries Companies Act (SICA) before the implementation of the IBC have faced substantial delays after being admitted under the new framework. As noted in the 2020 Quarterly Newsletter of the Insolvency and Bankruptcy Board of India, 72.46% of cases that resulted in liquidation were either previously under SICA or classified as defunct. Furthermore, judicial discretion under Article 14 of the Indian Constitution has contributed to delays in the timely resolution of CIRP. The report can be accessed at "ibbi.gov.in/uploads/publication/7e9b78bf6eba5254d788c8323055224f.pdf."

[6] We restrict our sample to manufacturing firms because Altman's Z-score, which we use to identify treatment and control firms within our sample, is more suitable for detecting financial distress among manufacturing firms compared to non-manufacturing firms (Altman, 1968).

classification is supported by prior studies in the domain of law and finance, which argue that bankruptcy reforms impact financially distressed firms more compared to their stronger counterparts (Vig, 2013; Rodano et al., 2016; Bose et al., 2021). Because we classify the sample firms into the treatment and control groups on the basis of their pre-reform variation in the 'propensity to default,' any forward-looking bias in our identification strategy is ruled out. We source data for our tests from the Centre for Monitoring Indian Economy's (CMIE) Prowess database.

The intuition behind our identification strategy is based on the premise that a low Z-score corresponds to a high risk of bankruptcy. A lower Z-score signals weaker financial health and a higher risk of default, reflecting challenges such as inadequate liquidity, diminished profitability, high leverage, and inefficient asset utilization (Altman, 1968). Although the Z-score is not a strict probability estimate, firms with a low Z-score are financially less secure (MacKie-Mason, 1990). Specifically, firms with a Z-score of below 1.81 are more likely to declare bankruptcy within two years (Altman, 1968). For example, Altman (1968) demonstrated that the Z-score could retrospectively predict 94% of bankruptcies within a year and 72% of bankruptcies within two years. The Z-score was later retested by Altman et al. (1977) with similarly robust results. Based on this, we classify firms with an average pre-IBC Z-score below 1.81 as 'distressed.' If the IBC reform increased the *ex-ante* likelihood of corporate bankruptcy and distressed firms are inherently more prone to default, it is reasonable to categorize distressed firms as 'treated.' Our classification scheme is consistent with prior studies, such as Vig (2013), Rodano et al. (2016), Bose et al. (2021), and Jadiyappa and Hickman (2025), which argue that financially distressed firms are more exposed to bankruptcy reforms than financially stronger firms.

The validity of our identification strategy relies on the assumption that distressed firms in the pre-IBC period remain distressed in the post-IBC period. However, profit-maximizing firms may optimize their financial behavior in response to the IBC reform, potentially transitioning from distressed to non-distressed status after its implementation. In such cases, our identification strategy would fail to isolate the impact of the IBC reform on firms' equity capital. This failure would result from the convergence in financial conditions between the treatment and control firms, potentially leading rational, wealth-maximizing shareholders to view distressed and non-distressed firms, conditional on covariates, as substitutes in the post-IBC period. To address these concerns, we conduct a first-stage test to confirm that the average Z-scores in the pre-IBC and post-IBC periods are correlated. We find results consistent with our expectations. Specifically, we document that financial distress between firms is correlated between the pre-IBC and post-IBC periods. Thus, it is logical to categorize distressed firms in the pre-IBC period as treated. This gives us the confidence to proceed with our DiD analysis.

We initiate our analysis by examining the impact of the IBC reform on the paid-up[7] equity shares of firms. Our primary discovery is that the strengthening of creditors' rights, due to the passage of the IBC reform in 2016, resulted in distressed (treated) firms in our sample having 4.6% fewer equity shares compared to non-distressed (control) firms in the post-IBC period. We interpret this result as evidence that creditor-friendly reforms decrease firms' access to equity capital, driven by an increased threat of liquidation. The threat of liquidation decreases the probability of investors recouping their investments in the event of default, discouraging risk-averse investors from investing in firms more exposed to bankruptcy risk (Rajan and Zingales, 1995). Consistent with these arguments, we document that the 2016 enactment of the IBC reform reduced treatment firms' access to equity capital relative to control firms in the post-IBC period.

[7] Paid-up equity capital is the amount of money that a firm has received from shareholders in exchange for shares of stock. It's a key component of a firm's equity and is recorded on its balance sheet.

To validate the assumptions of the DiD approach, we test whether our results are simply a continuation of pre-existing differences between the treatment and control firms (Bertrand et al., 2004). Our tests do not detect any pre-existing trends in the variable of interest, which further strengthens our confidence in the inference regarding the impact of the IBC reform on the equity capital of firms. Thus, the enactment of the IBC reform in India decreased access to equity capital for treated firms relative to control firms in the post-IBC period.

At this stage, a reader might suspect that decrease in the issuance of equity shares by treated firms occurred without a corresponding decrease in their equity capital, potentially due to share repurchases (also called 'buybacks') or face value consolidation. These corporate events merely reduce the number of paid-up equity shares but don't affect the paid-up equity capital of firms. To address this concern, we estimate the impact of the IBC reform on shareholders' funds using the same identification strategy as earlier in an Entropy-DiD framework. Consistent with our previous results, we find that the IBC reform decreased shareholders' funds of treated firms by 3.4% in the DiD sense. Therefore, we conclude that our baseline results are not merely driven by corporate events, but rather reflect a genuine decrease in equity inflows for treatment firms compared to their control counterparts in the post-IBC period.

We conduct a couple of auxiliary tests to assess the robustness of our results. *First*, a discerning reader might argue that our findings primarily stem from (potential) selection bias in the construction of the treatment and control groups, leading to systematic differences in the behavior of the dependent variable between two groups in the post-IBC period. This concern is not unfounded, given that our identification strategy is based on classifying firms as treated if they had an average Z-score of less than 1.81 in the pre-IBC period. To mitigate this concern, we incorporate entropy balancing in our estimations to rule out systematic differences between the treatment and control groups as the source of our empirical results.

Second, a reader might posit that our results are driven by some pre-existing trend in our dependent variable, i.e., equity shares of our sample firms. For instance, the Indian regulators introduced various regulations around the implementation of the IBC, including the Indian Companies Act of 2013 and mandatory corporate social responsibility (CSR) spending. As a result, one might argue that our DiD regression captures the influence of these regulations on equity inflows rather than the direct effect of the IBC. Although there is no *a priori* rationale suggesting that these regulatory interventions prior to the IBC would have a differential impact on distressed (treatment) versus non-distressed (control) firms, we conduct a placebo test to rule out such possibilities. For this test, we select a fake promulgation year (i.e., 2013) for the IBC reform during the pre-reform period (i.e., 2011 to 2015). If our results are attributable to the IBC reform, there should be no change in equity inflows of treatment firms compared to control firms in the pre-IBC period. Otherwise, any post-IBC decline in equity inflows for our treated firms vis-à-vis control firms cannot be causally attributed to the implementation of the IBC reform. From a statistical perspective, we expect the coefficient on the variable of our interest (i.e., the DiD variable) to be insignificant for the fake promulgation year. We document results in alignment with our expectations. Thus, our findings are attributable to the IBC reform rather than any prior trend in our dependent variable, further enhancing the credibility of our main conclusions.

To shed light on the mechanism through which the IBC reform decreases firms' access to equity capital, we conduct a supplementary test. In this article, we argue that the promulgation of the IBC reform in 2016 increased the threat of liquidation, thereby deterring shareholders from investing in firms more exposed to the reform. Consistent with this argument, we document that the equity inflows of distressed firms decline relative to those of non-distressed firms in the post-IBC period. A corollary of our argument is that the post-IBC decrease in equity inflows should be more pronounced in firms that are exogenously more financially constrained. A unique feature of the Indian corporate landscape, i.e., the presence of business groups, allows us to test this corollary.

According to Khanna and Palepu (2000), business groups are collections of legally independent firms linked through various formal and informal mechanisms, such as shared ownership, interlocked directorates, and familial ties. These groups typically operate in markets where institutional weaknesses, such as underdeveloped financial markets and weak legal systems, create inefficiencies. Business groups serve as mechanisms to mitigate these market failures by providing internal capital markets, reducing transaction costs, and facilitating resource allocation among affiliated firms (Deloof and Jegers, 1996; Gopalan et al., 2007). Consequently, firms affiliated with business groups (hereafter referred to as 'group firms') benefit from access to internal capital markets (Thapa et al., 2020). This access can shield distressed group firms from bankruptcy-led liquidation or reorganization, thereby preserving investors' capital. In contrast, firms unaffiliated with business groups (hereafter referred to as 'standalone firms') lack the insulation provided by internal capital markets. As a result, financial distress increases their risk of default, heightening equity investors' concerns about the threat of liquidation.

To the extent that group membership is exogenous to the IBC reform, we can exploit between-firm variation in group membership to uncover whether financial constraint is the mechanism through which the IBC reform affects firms' equity capital. If our argument is valid, rational equity investors, conditional on covariates, would not alter their investments in group firms in the post-IBC period. Therefore, we posit that the observed post-IBC decline in equity inflows for treatment firms compared to control firms is entirely concentrated in standalone firms.

To test this assertion, we divide our sample into standalone and group firms and re-estimate our regressions separately for each subgroup. Within each subgroup, we identify the treatment and control firms based on the same identification strategy discussed above. Specifically, firms with an average pre-IBC Z-score below 1.81 are classified as distressed (treated), while those with an average pre-IBC Z-score of 1.81 or higher are classified as non-distressed (control). After identifying the treatment and control firms within the standalone and group samples, we proceed to re-estimate our regressions.

Our results align with our hypothesis. Specifically, we find no effect of the IBC reform on equity inflows of treated firms in the group sample when compared to the corresponding control group. Conversely, the effect of the IBC reform on equity inflows is evident in the standalone sample. Specifically, equity inflows of standalone treated firms decline by 4% in the DiD sense. These results imply that financial constraint is the mechanism through which the IBC reform affects equity inflows of treated firms. Furthermore, we find that the capital expenditure of standalone treated firms declines by 3.3% compared to standalone control firms, while no such effect is observed for the group sample in the post-IBC period. Thus, reduction in equity inflows can be interpreted as a channel through which the IBC reform incentivizes financially constrained firms to disinvest and raise resources to buffer against the contraction in the supply of external capital.

We contribute to the literature on the welfare consequences of creditor-friendly bankruptcy reforms by arguing that such reforms may have more drawbacks than advantages, particularly by reducing firms' access to equity capital. Previous research emphasizing the benefits of creditor-friendly reforms suggests that stricter bankruptcy codes discipline firm managers, thereby increasing investment and reducing the cost of debt (Bolton and Scharfstein, 1996). For example, prior research suggests that stronger creditor rights expand credit availability (Bose et al., 2021), reduce borrowing costs (Bose et al., 2021), lessen the impact of uncertainty on corporate policies (Favara et al., 2021), boost research and development (R&D) expenditures (Mann, 2018), and reduce creditors' demand for accounting conservatism (Jadiyappa and Hickman, 2025). Conversely, studies highlighting the costs of such reforms identify several negative outcomes, such as decreased firm-level innovation (Acharya & Subramanian, 2009), inefficient liquidations (Acharya et al., 2011), diminished corporate risk-taking (Acharya et al., 2011), reduced dependence on collateralized debt (Vig, 2013), and negative effects on corporate investment (Favara et al., 2017).

Our study closely aligns with the literature on the negative implications of creditors' rights and demonstrates that stronger creditors' rights instill fear among equity investors of partial or complete write-offs of their investments due to bankruptcy-driven liquidation or reorganization. This deters equity investors from investing in firms that are more prone to default in the post-reform period. While Chakraborty et al. (2024) document an increase in firms' access to equity capital in the post-IBC period, attributing their findings to the mitigation of principal-principal agency frictions between promoters and outside shareholders, we argue that laws strengthening creditor rights increase the threat of liquidation and deter equity inflows, particularly for financially constrained firms during the post-regulation period. Thus, we extend the law and finance literature by examining the *ex-post* impact of a creditor-friendly bankruptcy reform, specifically the IBC, on equity capital, a category that has been previously understudied in the bankruptcy literature.

Furthermore, we exploit a unique feature of the Indian corporate landscape, the presence of business groups, to demonstrate that the negative effects of stronger creditors' rights on firms' access to equity capital are largely concentrated in standalone firms, with no significant effects on group-affiliated firms. Given that standalone firms lack access to the internal capital markets of business groups (Thapa et al., 2020), we posit that standalone firms are, *ceteris paribus*, more constrained in their access to finance compared to group-affiliated firms.

Consequently, we are perhaps the first to demonstrate that financial constraints serve as the mechanism through which bankruptcy reforms impact firms' access to equity capital.

Lastly, we document that the decline in equity inflows for standalone firms, caused by the enactment of a creditor-friendly bankruptcy reform, namely the IBC imposes constraints on their ability to invest, leading to a decline in capital expenditure. This positions our study as one of the first to uncover financial constraints as the channel through which declines in equity inflows, triggered by a creditor-friendly bankruptcy reform such as the IBC, affect real investments. In summary, our work advances the understanding of how creditor-friendly bankruptcy reforms impact firms' access to equity capital and their real investments.

The remainder of the paper is structured as follows: Section 2 discusses the 2016 IBC reform in India. Section 3 outlines the sample and methodology. Section 4 presents a discussion on the empirical results. Section 5 includes supplementary tests to explore the mechanisms underlying the impact of the IBC reform on firms' equity capital. Finally, we conclude in Section 6.

2. The IBC Reform of 2016

Before the Insolvency and Bankruptcy Code (IBC) was introduced, the corporate insolvency resolution system in India was fragmented, involving numerous laws and considerable jurisdictional overlap (Branch and Khizer, 2016). This fragmented approach resulted in inefficiency and ineffectiveness in enforcing creditor rights. For example, prior to the IBC, bankruptcy cases in India took about 10 years to resolve, with creditors recovering only 12% of the asset value (Chakrabarti et al., 2008). This inefficiency not only weakened creditor confidence in timely debt recovery but also encouraged debtors to engage in asset substitution, believing that creditors would face difficulties in seizing assets automatically in case of default. Consequently, the pre-IBC era was marked by weak creditor rights.

To tackle these issues, the Indian government committed in 2014 to establish a new legal framework. During his budget speech on July 10, 2014, the then Union Finance Minister Arun Jaitley emphasized the need for an "Entrepreneur-friendly legal bankruptcy framework." In line with this commitment to strengthen creditor rights, the government formed the Bankruptcy Law Reforms Committee (BLRC) on August 22, 2014, headed by Dr. Vishwanath Anand. Following the committee's recommendations and public consultations, the Government of India (GoI) announced on February 28, 2015, its intention to introduce a comprehensive bankruptcy code in the fiscal year 2015–16, aiming to meet international standards and enhance judicial capacity.

With a strong parliamentary majority, the government's determination to bring about a substantial shift in the bankruptcy legal framework was clear (Jadiyappa and Kakani, 2023). As a result, businesses and their stakeholders began factoring in the anticipated strengthening of creditors' rights into their financial and operational calculus as early as the 2015-16 fiscal year. Consequently, the financial year ending on March 31st, 2016 is considered as the event year (Bose et al., 2021), and the period from 2016 to 2020 is recognized as the post-IBC era. The legislative process for the IBC bill concluded with its approval by the Lower House of Parliament (Lok Sabha) on May 5, 2016, and by the Upper House (Rajya Sabha) on May 12, 2016. Finally, on May 28, 2016, the bill received presidential assent, officially enacting the IBC into a law.[8]

The IBC reform marked a transition from a debtor-in-possession model to a creditor-in-control framework (Singh et al., 2023). Under the IBC, once a case is admitted to the National Company Law Tribunal (NCLT), the existing board of directors is suspended. Control of the firm's management is then transferred to an insolvency professional (IP) appointed by the

[8] The Act is available for access at https://www.mca.gov.in/Ministry/pdf/TheInsolvencyandBankruptcyofIndia.pdf

committee of creditors (CoC). According to Section 12(1) of the IBC, corporate insolvency cases admitted to the NCLT must be resolved within 180 days from the date of admission. However, Section 12(2) allows for the possibility of a 90-day extension if the resolution professional obtains a 2/3rd majority approval from the committee of creditors.

An amendment to the IBC in 2019 introduced Section 12(3), delineating a more stringent 330-days limit for resolving a corporate insolvency case. This time-constrained process yielded a markedly improved recovery rate of 46.3%, a stark contrast to the 17.4% recorded under the SARFAESI[9] Act of 2002 (Report on Trend and Progress of Banking in India 2019-20). As per the annual report of the Insolvency and Bankruptcy Board of India (IBBI) for the financial year 2019-20, an official body incorporated under the IBC law of 2016, the average duration of the corporate insolvency resolution process (CIRP) under the IBC was documented at 415 days. This accomplishment prompted the Economic Survey (published by the Department of Economic Affairs in the Ministry of Finance, Government of India) to assert in 2023 that "the total amount recovered by *Scheduled Commercial Banks* (SCBs) under IBC has been the highest compared to other channels such as Lok Adalat's, SARFAESI Act, and *Debt Recovery Tribunals* (DRTs) in this post-IBC period."[10]

Consequently, the introduction of the IBC substantively fortified creditors' rights and compelled Indian enterprises to actively mitigate their bankruptcy risk. Thus, the promulgation

[9] SARFAESI stands for Securitization and Reconstruction of Financial Assets and the Enforcement of Security Interest.

[10] The DRT Act of 1993 necessitated the involvement of a tribunal or court to facilitate creditors in pursuing their claims against companies on the verge of default. In contrast, despite the SARFAESI Act of 2002 granting creditors the authority to take possession of defaulting firms' assets, they were required to navigate through the DRT system in situations where the assets were insufficient to cover the claims, as outlined by Bose et al. (2021). Furthermore, in the case of "*Encore Asset Reconstruction Company Pvt. Ltd. Vs Ms. Charu Sandeep Desi & Ors. Company Appeal (AT) (Insolvency) No. 719 of 2018*", the National Company Law Appellate Tribunal (NCLAT) ruled that provisions of the IBC prevail over provisions of any other comparable law, such as the SARFAESI Act of 2002. This further enhanced the authority and hence, the effectiveness of the IBC law relative to other comparable laws.

of the IBC law provides us with a natural laboratory to empirically test our assertion regarding the impact of creditors' rights on the firm's access to equity capital.

3. Data and Methodology

Our sample consists of data on manufacturing firms in India. We limit our sample to manufacturing firms because the Z-score, our variable for identifying the treatment and control firms in our empirical framework, is primarily designed for and more applicable to manufacturing enterprises than to non-manufacturing firms (Altman, 1968).

We obtain information on the financial accounting statements of firms from the Prowess database, overseen by the Centre for Monitoring Indian Economy (CMIE), covering the years 2011 to 2020. Several notable studies in the past, which used India as their context, have used this database for their empirical investigations (for instance, see Vig, 2013; Manchiraju and Rajgopal, 2017; Tantri, 2020; Bose et al., 2021; Rajgopal and Tantri, 2023).

Our study period of ten years is segmented into the pre-IBC period (2011-2015) and the post-IBC period (2016-2020) for the purpose of our analysis. To minimize the influence of outliers on our regression estimates, we winsorize variables at the 2.5% level on both tails of the distribution.[11] The data is organized on a firm-year basis to facilitate panel regression analysis. The data collection process results in a final sample of 11,780 firm-year observations on 1,178 unique firms.

The sample is highly diverse, encompassing 311 clusters of National Industrial Classification (NIC)[12] activities. The NIC activity with the highest concentration of firms is

[11] To choose the level of winsorization at the 2.5% level, we heed the advice of Bose et al. (2021) and perform focused visual inspection of the distribution of our sample values. Our results are robust to winsorization at the 5% level. These results are not tabulated to conserve space.
[12] The National Industrial Classification (NIC) is a standardized system developed by the Government of India to classify and categorize business establishments and economic activities. Derived from the International Standard Industrial Classification (ISIC), it is widely used to organize industry-related data, facilitate economic analysis,

"Manufacture of allopathic pharmaceutical preparations" (NIC code: 21002), which includes 35 firms (or 350 firm-year observations), accounting for only 2.97% of the sample. Consequently, we can be confident that no particular industrial activity disproportionately influences our empirical results. The description of variables used in this study is available in Table 1 of Section 3.1.

3.1. Description of Variables

The IBC policy was implemented on a nationwide basis in 2016. Therefore, for our DiD analysis, we don't have a natural control group for comparison. To address this issue, we exploit the cross-sectional heterogeneity in the reform's impact to identify our treatment and control firms. We classify sample firms into the treatment and control groups on the basis of their 'propensity to default' in the pre-IBC period. Because we use pre-reform data for identification, our classification of sample into the treatment and control groups is robust to any forward-looking bias induced by the IBC reform.

'Propensity to default' refers to the likelihood that a firm might fail to meet its repayment obligations to creditors. The implementation of the IBC in 2016 enhanced creditors' rights within India, incentivizing aggrieved creditors to file insolvency petitions against defaulting firms. This shift to a creditor-in-control system increased the *ex-ante* probability of bankruptcy for defaulting firms in the post-IBC period. As a result, firms with a higher risk of default are more likely to face bankruptcy-driven liquidation or reorganization compared to those with a lower risk post-IBC.

A firm may default on its repayment obligations due to reduced cash flows, which may stem from a loss of sales, decreased profits, or difficulty in turning over inventory. Therefore,

and inform policymaking. Following Topalova and Khandelwal (2011) and Aggarwal (2024), we use NIC codes to group firms into industrial clusters for our empirical analysis.

to quantify the 'propensity to default,' we use the Altman Z-score[13], a financial metric designed to assess bankruptcy risk (Altman, 1968). Firms with a pre-IBC average Z-score below 1.81 are classified as 'distressed,' while those with higher scores are categorized as 'non-distressed' (Altman, 1968; Singh et al., 2021; Srivastava, 2024; Jadiyappa and Hickman, 2025). In our analysis, distressed firms represent the treatment group, while non-distressed firms form the control group. This classification is supported by prior studies in the domain of law and finance, which argue that bankruptcy reforms impact financially distressed firms more compared to their stronger counterparts (Vig, 2013; Rodano et al., 2016; Bose et al., 2021; Jadiyappa and Hickman, 2025).

To identify treatment firms in our regressions, we construct a binary variable, *Treatment*, which takes the value of 1 for firms in the treatment group and 0 for firms in the control group. Our sample of 11,780 firm-year observations includes 4,800 firm-year observations from 480 unique firms in the treatment group and 6,980 firm-year observations from 698 unique firms in the control group. Thus, 480 firms in our sample, out of a total of 1,178 firms, have an average pre-IBC Z-score below 1.81, while the remaining 698 firms have an average pre-IBC Z-score of 1.81 or higher. Specifically, treatment firms have an average pre-IBC Z-score of 1.378 (std. dev. = 0.412), while control firms have an average pre-IBC Z-score of 2.573 (std. dev. = 0.801). Description of other variables can be found in Table 1. Table 2 documents summary statistics on the variables used in this study.

[13] Z-score = [3.3*(EBIT × TA^{-1}) + 1.0*(Sales × TA^{-1}) + 1.4*(Retained Earnings × TA^{-1}) + 1.2*(Working Capital × TA^{-1})], where EBIT stands for Earnings Before Interest and Taxes and TA stands for Total Assets. Altman (1968) includes a fifth term in the computation of the Z-score, i.e., the ratio of the market value of equity to book value of debt. However, since our study focuses precisely on the capital structure of firms and directly uses equity capital as the dependent variable in our regressions, we omit this fifth term. This approach aligns with the recommendation of MacKie-Mason (1990), who advises excluding the ratio of market value of equity to book value of debt when the focus is on analyzing firms' capital structure.

[INSERT Table 1 HERE]

[INSERT Table 2 HERE]

It is pertinent to note that our identification strategy, as discussed above, hinges on the assumption that firms classified as distressed in the pre-IBC period remain distressed in the post-IBC period. However, profit-maximizing firms may adapt their financial behavior in response to the IBC reform, potentially transitioning from distressed to non-distressed status after its implementation. Such transitions would compromise our identification strategy, as they would blur the distinction between the treatment and control firms, making it difficult to isolate the reform's impact on equity capital. Specifically, if financial conditions converge between the treatment and control firms, rational wealth-maximizing shareholders may perceive distressed and non-distressed firms, conditional on covariates, as substitutes in the post-IBC period.

To address these potential issues, we conduct a first-stage test to examine whether financial distress, as measured by Z-scores, is correlated between the pre-IBC and post-IBC periods. Specifically, we estimate a regression in which the inverse of the firm-wise average post-IBC Z-score is regressed on the inverse of the firm-wise average pre-IBC Z-score, along with a vector of covariates such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, as described in Table 1. We use the inverse of the firm-wise average Z-scores because an increase in the Z-score indicates improved financial health, whereas a decrease indicates worsening financial health. By inverting the Z-scores, we enable a marginal increase in both the dependent and independent variables to represent a deterioration in firms' financial health, thereby facilitating easier interpretation of the regression results.

Because the dependent and independent variables are based on the inversed firm-wise averages of Z-scores, it is not possible to include firm fixed effects. Using firm fixed effects would result in perfect collinearity between the explanatory variable (the inversed firm-wise average pre-IBC Z-score) and the fixed effects, making it impossible to estimate the average effect of the regressor on the dependent variable. Instead, we include industry fixed effects, which account for industry-specific, time-invariant factors that might influence our regression estimates. Moreover, we incorporate year fixed effects to control for within-year macroeconomic shocks. To further address the possibility that the observed correlation between the dependent and independent variables is driven by time-varying, industry-specific shocks, we also include industry × year fixed effects. While this regression approach does not establish causality between the inversed firm-wise average pre-IBC and post-IBC Z-scores, it allows us to assess whether a correlation exists between the two periods.

Our primary interest lies in identifying this correlation, rather than causation. The presence of such a correlation would suggest that firms distressed in the pre-IBC period remain distressed in the post-IBC period relative to non-distressed firms in our sample. This finding would validate the robustness of our scheme for classifying sample firms into the treatment and control groups, even after the implementation of the IBC reform.

The results align with our expectations, showing a strong correlation between the inverse firm-wise averages of Z-scores between the two periods. Specifically, we document a regression coefficient of 0.786 (robust standard error = 0.011, t-statistic = 67.50) on the inverse firm-wise average pre-IBC Z-score, which is significant at the 1% level.[14] For example, the pre-IBC average Z-score for treated (control) firms was 1.378 (2.573), with a standard deviation of 0.412 (0.801). This indicates that treated firms, on average, were at a higher risk

[14] The adjusted R-squared of the regression is 0.6528 (65.28%), which is significant at the 1% level (F-statistic = 1,069.73).

of default compared to control firms during the pre-IBC period. However, the promulgation of the IBC law in 2016 did not affect the average Z-scores of the treated and control groups. Specifically, the post-IBC average Z-score for treated (control) firms was 1.429 (2.358), with a standard deviation of 0.510 (0.854). In other words, treated firms continued to exhibit financial distress relative to control firms. These findings validate our categorization of pre-IBC distressed firms as treated and provide the necessary confidence to proceed with our DiD analysis.

3.2. Empirical Strategy

We utilize the DiD technique to evaluate the causal impact of higher creditors' rights, specifically the enactment of the IBC reform in 2016, on firms' access to equity capital. This method is widely used in quasi-experimental studies to establish causal associations in policy-related research (Araujo et al., 2012; Vig, 2013; Bose et al., 2021). It involves comparing groups affected by the policy to those unaffected. Specifically, the DiD technique examines differences in the outcome variable before and after the policy's implementation to measure its impact on outcomes such as equity capital.

However, these findings may be influenced by other factors that are time-variant or invariant, as well as observable or unobservable, making it challenging to draw causal inferences. To address this limitation, the DiD method divides firms into the treatment and control groups, with control firms serving as a counterfactual for treatment firms. The differences in outcome variables are calculated for both groups before and after the policy, and the difference between these differences is taken. This final calculation gives us the DiD estimate.

A prerequisite for conducting the DiD analysis is that the exogenous shock, such as the introduction of the IBC reform in this context, should essentially categorize firms into the treatment and control groups in a manner similar to 'random assignment' (Atanasov and Black, 2016, 2021). In our empirical framework, firms in the sample are not randomly allocated into the treatment and control groups. Instead, the classification is based on their average pre-IBC Z-score. One approach to address this concern is to enhance the balance of covariates between the treatment and control firms through matching (Atanasov and Black, 2021). Therefore, we incorporate entropy balancing into our analysis (Entropy-DiD).[15]

In the context of the DiD analysis, entropy balancing is a technique used to create balanced samples that account for covariate imbalances between the treatment and control groups. The goal is to make the distribution of covariates similar between the groups before applying the DiD methodology. The entropy balancing process can be formalized through an optimization problem, where the objective is to find weights for control group observations such that the weighted sample balances the covariates of the control group with those of the treatment group. Thus, entropy balancing can be formally expressed as

$$min_w \sum_{i \in control} w_i \, log(w_i)$$

Subject to

$$\sum_{i \in control} w_i x_i = \sum_{i \in treatment} x_i$$

$$\sum_{i \in control} w_i = 1$$

[15] We prefer entropy balancing over propensity score matching (PSM) because it directly achieves covariate balance between the treatment and control groups without requiring iterative matching. While PSM assumes that matched samples sufficiently balance covariates and often results in information loss due to unmatched units being excluded, entropy balancing employs a reweighting method that minimizes imbalance by adjusting weights for the control group to align with the moments of the treatment group's covariates. This approach ensures balance not only in means but also in higher moments, such as variances, leading to more precise adjustments for confounding effects (Hainmueller, 2012). Moreover, entropy balancing is computationally efficient and avoids reliance on arbitrary choices, such as the caliper width, which can affect PSM outcomes. PSM's reliance on the propensity score as a singular covariate adjustment mechanism can also introduce bias if the score is misspecified (King and Nielsen, 2019). In contrast, entropy balancing achieves covariate balance without depending on a model for treatment assignment, making it a reliable and flexible tool for estimating causal effects in observational studies.

$w_i \geq 0 \ \forall i \in control$ (1)

Here, i is an index for firms, t is an index for years, $w = (w_1, w_2, \ldots, w_n)$ are the weights for the control group observations and x represents firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*. The first constraint ensures that the weighted mean of covariates in the control group equals the mean of covariates in the treatment group, the second constraint normalizes the weights to sum to 1, and the third constraint ensures non-negative weights. We present, in Table 3, data on mean, variance, and skewness of covariates by the treatment and control groups before and after entropy balancing.

[INSERT Table 3 HERE]

Table 3 shows that after entropy balancing, the first three moments of the covariates are identical for the treatment and control groups. This indicates that entropy balancing effectively aligns the distribution of covariates across the groups, thereby mitigating potential biases in the DiD estimation. This gives us the confidence to proceed with our DiD analysis.

Subsequently, we assess the parallel trend assumption necessary for the DiD analysis. To validate the parallel trend assumption, we adopt the approach used by Autor et al. (2003) and examine the yearly changes in our dependent variable, *Equity Shares*, for both the treatment and control firms, before and after the exogenous shock, i.e., the implementation of the IBC reform in 2016. The method assumes that if the two groups exhibit parallel trends, there should be no significant difference in the dependent variable during the pre-regulation period. To assess this, we plot the yearly trend in regression coefficients between the treatment and control firms obtained using the following regression specification

$$Equity\ Shares^i_t = \alpha + \sum^{+5}_{p=-4} \beta_p D_p \times Treatment^i + (Firm\ and\ Industry \times Year)\ fixed\ effects + \varepsilon^i_t \quad (2)$$

The data is organized at the firm-year level, where i indexes firms and t indexes years. D_p is an indicator variable whose value is 1 for the year 'p' and 0 otherwise. For instance, if 2015 (the last year of the pre-IBC period) is the reference year (omitted group), then D_{-4} is 1 for observations in 2011 and 0 otherwise, while D_{+5} is 1 for observations in 2020 and 0 otherwise.

We incorporate firm and industry × year fixed effects in our regression. Firm fixed effects absorb the influence of firm-specific time-invariant factors on regression estimates, whereas industry × year fixed effects mitigate the effect of any time-varying industry-level shocks that could have been correlated with the promulgation of the IBC reform.[16] Furthermore, we match the treatment and control firms on a vector of firm-level time-varying covariates *X*: *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*. This specification allows us to assess whether there are significant differences in *Equity Shares* between the treatment and control firms in the pre- and post-IBC periods.

β_p estimates the difference in *Equity Shares* between the treatment and control firms for the year 'p.' For our identification strategy to be valid, coefficients (β) on D_{-4}, D_{-3}, D_{-2}, and D_{-1} are expected to be statistically indifferent from 0. However, we expect the coefficient on D_p for $p > 0$ (i.e., D_1, D_2, D_3, D_4, and D_5) to be positive or negative and statistically significant. This would imply that the promulgation of the IBC reform in 2016 led treatment firms in our sample to suffer a decline in their access to equity capital compared to our control firms in the post-IBC period. Table 4 documents the result of estimating specification (2).

[16] The standard DiD analysis nullifies the impact of any possible omitted variables arising out of "pretreatment time-invariant differences between the treatment and control groups and aggregate time trends" (Alok et al., 2022: 10). Consistent with this argument, we do not separately include year fixed effects in our analysis.

[INSERT Table 4 HERE]

Table 4 demonstrates the absence of any pre-existing trend in the dependent variable, *Equity Shares*, between the treatment and control groups. Columns 1 and 2 reveal that the coefficients on the interaction term $D_p \times Treatment$ are statistically insignificant for the years prior to the introduction of the IBC reform (2011-2015, with 2015 serving as the base year for comparison). In contrast, during the post-IBC period (2016-2020), the coefficients remain statistically insignificant for the first two years (2016 and 2017) but become statistically significant at varying levels in the years after 2017. Moreover, the coefficient on the interaction term becomes increasingly negative in the period after the IBC reform, indicating a monotonic decline.

Our findings are robust to the use of entropy balancing to match the treatment and control firms, as shown in specification (1). For instance, qualitatively similar results are observed in columns 1 and 2, despite column 2 specifically matching the treatment and control groups. Taken together, the results from estimating specification (2) confirm not only that the parallel trends assumption holds for our sample but also provide evidence supporting the hypothesis that the implementation of the IBC reform led to a decline in the equity capital of treated firms relative to control firms.

Figure 1 illustrates the coefficients on the interaction term, along with their 95% confidence intervals, for the four years prior to the 2016 enactment of the IBC reform and the five years after it. During the pre-IBC period, there is no significant difference in the dependent variable, *Equity Shares*, between the treatment and control firms. However, in the post-IBC period, the coefficients become monotonically negative and statistically significant three years after the enactment, indicating a marked decrease in equity inflows for treatment firms compared to control firms post-IBC.

[INSERT Figure 1 HERE]

The absence of any pre-existing trend in the behavior of our dependent variable *Equity Shares* between the treatment and control firms in the pre-IBC period gives us the confidence to proceed with the DiD analysis.

We use the following panel specification for the empirical test of our hypothesis:

$$Equity\ Shares^{i}_{t} = \alpha + \beta_1\ Reg_t \times Treatment^{i} + (Firm\ and\ Industry \times Year)\ fixed\ effects + \varepsilon^{i}_{t} \quad (3)$$

Where, *Equity Shares* is the dependent variable for firm i in year t, *Reg* is a binary variable that assumes the value of 1 for the post-IBC period and 0 for the pre-IBC period, and ε is the residual. In specification (3), *Reg* × *Treatment* is our variable of interest.

To omit the possibility that our results are obtained on grounds of systematic cross-sectional differences, we also control for firm fixed effects. Because standard DiD analysis nullifies the impact of any possible omitted variables arising out of "pretreatment time-invariant differences between the treatment and control groups and aggregate time trends" (Alok et al., 2022: 10), we don't include year fixed effects separately in our regressions. Nevertheless, there is a chance that the enactment of the IBC reform was correlated with varying trends over time across different industries. To tackle this concern, we incorporate industry × year fixed effects into our specification. This non-parametric approach, which does not rely on specific parameters, helps control for time-varying shocks specific to each industry. By doing so, our regression estimates are derived from the variation within each firm and within each industry around the time of the implementation of the IBC reform in 2016. Consequently, the inclusion of industry × year fixed effects in our regression accounts for and neutralizes any time-varying differences specific to industries that might be correlated with the enactment of the IBC.

In the preceding specification (3), note that we do not separately include coefficients for *Reg* and *Treatment*. This is because the inclusion of firm fixed effects in our specification will completely absorb the firm-invariant variable *Treatment*. Similarly, the coefficient on *Reg* will be completely absorbed due to the inclusion of industry × year fixed effects in our specification. Nevertheless, the inclusion of firm and industry × year fixed effects won't absorb the coefficient β_1 on our variable of interest *Reg* × *Treatment*. This is because *Reg* × *Treatment* is a product of two binary variables, i.e., *Reg* and *Treatment*. On the one hand, *Reg* is invariant within a year but it varies across firms. On the other hand, *Treatment* is invariant within a firm but it varies across years. Therefore, a product of *Reg* and *Treatment*, i.e., *Reg* × *Treatment* will benefit from the within firm variability of *Reg* and within year variability of *Treatment*. Consequently, the inclusion of firm and industry × year fixed effects in our panel specification (3) won't omit the coefficient β_1 due to within-firm and within-year variability of *Reg* × *Treatment*. Based on this discussion, it becomes evident that our coefficient of interest is the DiD operator β_1.

A positive (negative) and statistically significant β_1 would indicate that the strengthening of creditors' rights due to the enactment of the IBC reform in 2016 increased (decreased) equity inflows for our treatment firms relative to control firms in the post-IBC period.

We estimate specification (3) using fixed effects regression analysis. Furthermore, we cluster standard errors at the firm level to account for conditional heteroscedasticity.

4. Empirical Results

4.1. Analysis of Descriptive Statistics

Table 5 presents descriptive statistics on the variables utilized in this study.

[INSERT Table 5 HERE]

Table 5 highlights significant variation in key variables. For instance, the average number of *Equity Shares* for the control group increased from 15.529 before the IBC reform to 15.786 after its implementation. This rise in the average was accompanied by greater variability in the data, with the standard deviation increasing from 1.632 in the pre-IBC period to 1.693 post-IBC.

In comparison, treatment firms consistently held a higher average number of *Equity Shares* in both periods. Specifically, their average rose from 16.088 pre-IBC to 16.292 post-IBC. Notably, treatment firms exhibited less variability than the control group. For example, the standard deviation for treatment firms increased from a low of 1.601 pre-IBC to 1.675 post-IBC.

Table 6 documents pairwise correlation among variables.

[INSERT Table 6 HERE]

Table 6 shows that none of the variables in this study exhibit a high correlation with one another. To formally assess multicollinearity, we calculate the variance inflation factor (VIF) for each firm-year covariate, including *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*. This is done by regressing each variable on the remaining firm-year covariates while incorporating firm, year, and industry × year fixed effects to account for unobserved heterogeneity. Moreover, we cluster standard errors at the firm level to address conditional heteroscedasticity. The VIF for each variable is then computed as the inverse of (1 – R^2 from each regression). The results are presented in Table 7.

[INSERT Table 7 HERE]

Table 7 provides insights into the VIF of the variables in our analysis. According to Curto and Pinto (2011), a VIF threshold of 10 is recommended. Consistent with their guidance, all variables in our study exhibit VIF values below this threshold. Moreover, we observe a mean VIF of just 3.56, indicating that multicollinearity is not a concern.

4.2. Empirical Results and Discussion

We perform a DiD analysis to examine our intuition regarding the impact of the IBC reform on firms' access to equity capital. The results of our empirical analysis are presented in Table 8.

Table 8 consists of two columns, which differ primarily in how they account for firm-year covariates. In column 1, treatment and control firms are not matched on the vector of firm-year covariates *x* specified in equation (1). In contrast, column 2 explicitly matches treatment and control firms on the same vector using entropy balancing.

We observe from both columns that the coefficient on our variable of interest, *Reg* × *Treatment* (i.e., β_1), is negative and statistically significant at varying levels. This indicates that our estimate of β_1 remains robust even when treatment and control firms are matched using entropy balancing. We focus on the estimates in column 2, as this specification is the most rigorous and has the highest adjusted R-squared value of 0.9367.

[INSERT Table 8 HERE]

Please note that our dependent variable is in log form but our regressor is not. Therefore, to interpret coefficient β_1 on *Reg* × *Treatment* in percentage terms, we compute β_1^{st} power of *e* (~ 2.718). This effort gives us the percentage change in the equity shares of our treatment firms relative to control firms for the post-IBC period.

The coefficient on *Reg* × *Treatment* in column 2 indicates that firms in the treatment group have 4.6% fewer equity shares (calculated as $1 - e^{-0.048} = 0.046$) compared to control firms after the IBC reform came into effect. We contrast this decrease with the unconditional rise in the average equity shares of treatment firms between the pre and post-IBC periods to highlight the role of the IBC reform in limiting access to equity capital for treatment firms in the post-IBC period. This comparison is valid due to the presence of a parallel trend in our dependent variable, i.e., *Equity Shares*, between the treatment and control firms in the pre-IBC period, as illustrated in Figure 1 of Section 3.2.

The statistics presented in Table 5 shows that equity shares for the treatment group increased from 16.088 to 16.292, reflecting a 22.6% rise [$e^{(16.292 - 16.088)} - 1 = 0.226$]. However, the DiD analysis reveals that treatment firms have 4.6% fewer equity shares compared to control firms in the post-IBC period. This indicates that while the treatment group experienced an absolute increase of 22.6% in equity shares over time, this increase was smaller than the trend one might expect in the absence of the IBC reform. We interpret this fact to suggest that the IBC reform negatively affected treatment firms' ability to raise equity capital in the post-IBC period.

We attribute our finding to the heightened threat of liquidation introduced by the IBC reform. In response, risk-averse equity investors, aiming to maximize risk-adjusted returns, reduce their investments in distressed firms due to concerns about the total or partial loss of their capital (Rajan and Zingales, 1995). Consistent with these arguments, we observe that firms more exposed to the risk of default in the post-IBC period experience reduced access to equity capital compared to their counterparts after the reform's implementation.

In Section 4.3, for the purpose of robustness, we analyze the impact of the IBC reform on an alternative specification of the dependent variable (i.e., shareholders' funds) using the same identification strategy as before, within an Entropy-DiD framework.

4.3. Equity Capital as the Dependent Variable

In Section 4.2, we document that the promulgation of the IBC reform in 2016 led treatment firms in our sample to issue less equity shares compared to control firms in the post-IBC period. However, a reader might wonder whether the decrease in the issue of equity shares occurred without a corresponding decrease in the equity capital of firms, possibly due to corporate actions like the buyback of equity shares or face value consolidation. These events merely decrease the count of paid-up equity shares but don't affect the equity capital of firms.

To alleviate this concern, we estimate the effect of the IBC reform on shareholders' funds using the same identification approach as before, within an Entropy-DiD framework. In other words, we re-estimate regression specification (3) using *Equity Capital* as our dependent variable. The variable *Equity Capital* is described in Table 1, while summary and descriptive information on *Equity Capital* is available in Table 2 and Table 5 respectively. We present the result of our estimation in Table 9.

[INSERT Table 9 HERE]

Notice that while our dependent variable is in log form, our regressor is not. To interpret the coefficient β_1 on *Reg* × *Treatment* in percentage terms, we calculate e (~ 2.718) raised to the power of β_1. This gives us the percentage change in the equity capital of our treatment firms relative to control firms for the post-IBC period.

Table 9 consists of two columns, which differ primarily in how they account for firm-year covariates. In column 1, treatment and control firms are not matched on the vector of firm-year covariates x specified in equation (1). In contrast, column 2 explicitly matches treatment and control firms on the same vector using entropy balancing. We observe from both columns that the coefficient on our variable of interest, *Reg* × *Treatment* (i.e., β_1), is negative and statistically significant at varying levels. This implies that our estimate of β_1 is robust to matching the treatment and control firms using entropy balance. We focus on estimates in column 2, as this specification is the most rigorous and has the highest adjusted R-squared value of 0.9596.

In column 2, the coefficient on *Reg* × *Treatment* indicates that equity inflows of treatment firms decrease by an economically meaningful 3.4% (calculated as $1 - e^{-0.035} = 0.034$) in the DiD sense. These results are consistent with the facts documented in Table 8. Thus, our baseline results are robust to use of an alternative measure of the dependent variable.

In Section 4.4, we perform a placebo test to assess whether our results are a mechanical continuation of some pre-existing trend in our dependent variable.

4.4. Placebo Test

To ensure that our results are robust, we conduct a placebo test with a fake promulgation year for the IBC reform during the pre-IBC period (i.e., 2011 to 2015). If the observed decrease in equity inflows for our treatment firms relative to control firms is on account of higher creditors' rights (i.e., the promulgation of the IBC reform in 2016), then in the pre-IBC period there should be no difference in equity flows between our treatment and control groups. Otherwise, the post-IBC decrease in firms' equity inflows cannot be causally attributed to the implementation of the IBC reform. Statistically, the coefficient on the variable of interest in specification (3), i.e., *Reg* × *Treatment* should be insignificant for the fake reform year.

For the purpose of this placebo test, suppose 2013 is the fake reform year. This implies that 2011 and 2012 are considered as the pre-IBC period, while 2013, 2014, and 2015 are considered as post-IBC years. This creates an unbalanced comparison, as the post-IBC period spans three years, while the pre-IBC period covers only two. To address this, we drop firm-year observations for 2013. This adjustment ensures a balanced comparison by aligning two years of pre-IBC data with two years of post-IBC data. Thus, we have 4,712 firm-year observations on the variables used in our study for the period 2011 to 2015. Description of these variables is available in Table 1 of Section 3.1.

Subsequently, we classify our sample into the treatment and control groups based on the identification approach outlined in Section 3.1. Specifically, the treatment group consists of firms that had an average Z-score of below 1.81 in the pre-IBC period. In contrast, the control group includes firms that had an average pre-IBC Z-score of at least 1.81. For the treatment (control) group, we have 1,996 (2,716) firm-year observations on the variables used in the study.

To ensure that the allocation of our sample firms to the treatment and control groups resemble 'random assignment,' we incorporate entropy balancing (as exemplified in specification 1) into our analysis. Thereafter, we re-estimate specification (3) on our matched treatment and control firms in the DiD framework with 2013 as the fake reform year. The results of the placebo test are presented in Table 10.

[INSERT Table 10 HERE]

Table 10 has two columns, which differ in a manner similar to their differences in Tables 8 and 9. Our results show that the interaction coefficients (DiD) are insignificant in columns 1 and 2, indicating that any pre-existing trend in the pre-IBC period do not drive our baseline results. For instance, the estimate obtained on the variable *Reg* × *Treatment* in column 2 is 0.018 (std. error = 0.025, t-statistic = 0.74), which is insignificant. Thus, the re-estimation of empirical model (3) with the fake reform year confirms our intuition that our results are obtained due to the passage of the IBC reform in 2016 and not some prior trend in our dependent variable. This enhances the credibility of our main findings. Therefore, we conclude that our DiD results as documented in Table 8 are valid.

5. Supplementary Tests

5.1. Standalone versus Group Firms

To explore the mechanism through which the IBC reform reduces firms' access to equity capital, we conduct a supplementary test. This article posits that the introduction of the IBC reform in 2016 heightened the threat of liquidation, discouraging shareholders from investing in firms more exposed to the reform. Supporting this view, we find that distressed firms experience a sharper decline in their equity inflows relative to non-distressed firms during the post-IBC period.

A key implication of our argument is that the decline in equity inflows post-IBC should be most pronounced in firms that are exogenously more financially constrained. The unique structure of the Indian corporate landscape, characterized by the presence of business groups, provides an opportunity to test this hypothesis.

Business groups comprise legally independent firms interconnected through formal and informal mechanisms, such as shared ownership, interlocked directorates, and familial ties (Khanna and Palepu, 2000). These groups often operate in environments characterized by institutional deficiencies, such as underdeveloped financial markets and weak legal systems. By providing internal capital markets, business groups help mitigate these inefficiencies, reduce transaction costs, and facilitate resource allocation among member firms (Deloof and Jegers, 1996; Gopalan et al., 2007; Thapa et al., 2020). Consequently, equity investors in group-affiliated firms are less likely to perceive creditor-friendly bankruptcy reforms, such as the IBC, as a threat and are therefore less inclined to reduce their capital supply to such firms. In contrast, standalone firms in financial distress lack the buffer provided by internal capital markets, which heightens their risk of default in the presence of stronger creditors' rights, amplifying equity investors' fears of liquidation.

Provided that group membership is exogenous to the IBC reform, it allows us to test whether financial constraint is the channel through which the reform impacts equity inflows. If our hypothesis is correct, rational equity investors would not adjust their investments in group firms following the IBC reform. Consequently, the observed post-IBC decline in equity inflows for treated versus control firms should be entirely concentrated in standalone firms.

To test this assertion, we divide our sample into standalone and group subsets. We have 6,940 firm-year observations on standalone firms and 4,840 firm-year observations on group firms. Following the identification strategy outlined in Section 3.1, we classify firms with an average pre-IBC Z-score below 1.81 as treated and others as control, thereby identifying the treatment and control groups separately within the group and standalone subsamples.[17] We then re-estimate our empirical specification (3) in an Entropy-DiD framework on these subsamples to determine whether the post-IBC decrease in firms' access to equity capital is confined to standalone firms or extends to group firms as well. We document the result of re-estimating specification (3) on group and standalone samples in Table 11.

[INSERT Table 11 HERE]

Columns 1 and 2 show that treated firms within the standalone sample issue fewer equity shares than control group firms within the same sample in the post-IBC period. For example, the coefficient on *Reg* × *Treatment* is negative and statistically significant at the 5% level in both columns. We focus on the estimates in column 2 because it represents a more rigorous specification, with a higher adjusted R-squared of 0.9290. To interpret the regression coefficients as elasticities, we compute the exponential (e, approximately 2.718) raised to the power of the coefficient (β_1). This calculation provides the percentage change in equity shares for standalone treatment firms relative to standalone control firms in the post-IBC period.

The coefficient on *Reg* × *Treatment* in column 2 is -0.041 (robust standard error = 0.021), indicating that standalone treated firms issued 4% fewer equity shares (calculated as $1 - e^{-0.041}$

[17] It is important to note that within the standalone sample, we have 2,510 firm-year observations for treated firms and 4,430 for control firms. In contrast, within the group sample, we have 2,290 firm-year observations for treated firms and 2,550 for control firms.

= 0.04) than standalone control firms in the post-IBC period. This suggests that the passage of the IBC reform in 2016 caused distressed firms within our sample of standalone firms to experience a decline in equity inflows in the DiD sense.

On the other hand, columns 3 and 4 show that treated firms within our group sample did not experience any statistically significant change, either increase or decrease, in their equity inflows compared to control firms within the same sample. For example, the coefficient on *Reg* × *Treatment* in both columns is negative but not statistically significant.

Thus, our subsample analysis provides results consistent with our arguments. Specifically, we find that the IBC-induced decline in equity inflows is more pronounced among standalone firms than group firms. We interpret these findings as evidence that group firms benefit from access to internal capital markets, which shield them from bankruptcy-led liquidation or reorganization. In contrast, standalone firms lack such a failsafe, leading to a decline in their equity capital.[18]

At this point, it remains unclear whether the reduced access to equity capital is disadvantageous for standalone firms. On the one hand, standalone firms may substitute reduced equity inflows with increased borrowings. In such cases, there would be no adverse effect on their ability to invest in risky but positive net present value (NPV) projects. Consequently, despite restricted access to equity capital, their operating performance would remain unchanged or even improve.

On the other hand, if the reduced access to equity capital stems from an increased threat of liquidation, as evidenced by our mechanism analysis in this section, firms may face constraints in their ability to access equity capital and consciously avoid debt capital to avoid

[18] Our subsample analysis results remain robust when using Equity Capital as the dependent variable. However, we do not present these results here to conserve space.

premature liquidation. In such a scenario, firms would be limited in their ability to invest in positive NPV projects, leading to a decline in their operating performance.

To address this uncertainty and uncover the real effects of the IBC reform, we investigate in Section 5.2 whether decreased access to equity capital results in lower real investments by treatment firms compared to control firms within the standalone sample during the post-regulation period.

5.2. Real Effects

We now explore how treated firms within the standalone sample respond to reduced equity inflows in the post-IBC period. As previously noted, equity inflows decrease following the enactment of the IBC reform in 2016, with this effect being more pronounced among standalone firms. We posit that our results are attributable to an increased threat of liquidation in the post-IBC period, which had a greater impact on financially constrained firms.

However, standalone firms have the option to substitute the reduced availability of equity with increased leverage to fund real investments in the post-IBC period. This substitution arises from the higher efficiency and effectiveness of the IBC reform, which improves expected recovery rates for creditors, mitigates costs associated with adverse selection and moral hazard, and incentivizes creditors to supply more credit at lower costs than before (Benmelech and Bergman, 2011; Gopalan et al., 2016; Rodano et al., 2016; Ersahin, 2020; Alok et al., 2022). Consequently, firms are also incentivized to avail themselves of low-cost credit to fund real investments, which, in turn, increases returns on these investments and enhances shareholders' wealth. Such a substitution would suggest that creditor-friendly bankruptcy reform prompts firm managers to pivot their capital structure from equity to debt, ultimately increasing returns for equity holders.

On the other hand, treated firms within our standalone sample face a dilemma stemming from the increased threat of liquidation and financial constraints. This raises their risk of default, which, coupled with stronger creditors' rights in the post-IBC period, may deter them from accessing leverage after the implementation of the IBC reform. Consequently, standalone treated firms not only experience a decrease in their access to equity capital but also fail to take advantage of the increased availability of credit in the post-IBC period, leading to a subsequent reduction in their real investments. Thus, from an *ex-ante* theoretical perspective, the impact of the IBC-induced reduction in access to equity capital for financially constrained firms on their real investments is *a priori* unclear.[19]

We attempt to resolve the above dilemma by estimating the below regression specification (4), within an Entropy-DiD framework, using natural logarithm of firm's property, plant, and equipment (PP&E) as the dependent variable.[20] We estimate regression specification (4) separately for standalone and group firms.

$$log(PP\&E)^i_t = \alpha + \beta_1\, Reg_t \times Treatment^i + \beta_2\, Reg_t \times Equity\ Shares^i_t + \beta_3\, Treatment^i \times Equity\ Shares^i_t + \beta_4\, Reg_t \times Treatment^i \times Equity\ Shares^i_t + (Firm\ and\ Industry \times Year)\ fixed\ effects + \varepsilon^i_t \quad (4)$$

In specification (4), the variable of interest is *Reg* × *Treatment* × *Equity Shares*. The coefficient on this variable, β_4, captures the post-IBC difference in the change of our dependent

[19] Group firms have access to internal capital markets, which explains why we find no effect of the IBC reform on their access to equity capital (Section 5.1). Consequently, we anticipate no effect of the IBC reform on the real investments of group firms in the post-IBC period. We present these results alongside results from standalone firms in Table 12.

[20] We have data on 11,780 firm-year observations for PP&E. Out of this total, 6,940 firm-year observations correspond to standalone firms, while 4,840 firm-year observations pertain to group firms. Within the sample of standalone (group) firms, 2,510 (2,290) firm-year observations are classified as treated, and 4,430 (2,550) firm-year observations constitute the control sample. On average, standalone treated firms have a *log(PP&E)* of 6.119, with a standard deviation of 1.398, whereas standalone control firms have lower fixed asset holdings compared to their treated counterparts, with a *log(PP&E)* of 5.869 and a standard deviation of 1.366. Similarly, group control firms have lower fixed asset holdings than their treated counterparts. Specifically, group treated firms have a *log(PP&E)* of 7.481 with a standard deviation of 1.718, while group control firms have a *log(PP&E)* of 7.204 with a standard deviation of 1.577.

variable, *log(PP&E)*, between the treatment and control groups, attributed to the IBC-induced decrease in equity inflows for treated firms, where the decrease is interpreted in the DiD sense.

Before estimating regression specification (4), it is essential to test whether the parallel trends assumption, which underpins the validity of DiD results, holds for the sample data. The parallel trends assumption implies that, in the absence of the IBC shock, the dependent variable, *log(PP&E)*, would have evolved similarly for the treatment and control firms.

Importantly, our interest here lies not in the overall difference in the evolution of *log(PP&E)* between the treatment and control firms but in whether equity inflows affected the dependent variable differently, depending on whether a firm belonged to the treatment or control group. If equity inflows indeed influenced *log(PP&E)* differently across the two groups prior to the IBC, the coefficient on *Reg* × *Treatment* × *Equity Shares* in specification (4) cannot be interpreted as a consequence of the IBC reform.

To address this concern, we conduct a modified version of the Lemmon and Roberts (2010) test on the pre-IBC data (data for years 2011-2015), as described below.

$$\Delta log(PP\&E)^{i}_{t} = \gamma_0 + \gamma_1\, Treatment^{i} + \gamma_2\, Equity\ Shares^{i}_{t} + \gamma_3\, Treatment^{i} \times Equity\ Shares^{i}_{t} + \varepsilon^{i}_{t} \quad (5)$$

Here, Δ represents the first-difference operator. The above specification (5) is estimated using the pooled OLS method. This approach is chosen because the regressor (*Treatment*) is a binary variable, which takes the value 1 for firms in the treatment group and 0 for firms in the control group. Consequently, *Treatment* would be fully absorbed by firm fixed effects. To avoid this issue, we employ the pooled ordinary least squares (OLS) method. We present the result of estimating specification (5), separately for standalone and group firms, in Table 12.

[INSERT Table 12 HERE]

The variable of interest in specification (5) is *Treatment* × *Equity Shares*, which captures the differential impact of *Equity Shares* on Δ*log(PP&E)* for the treatment and control groups during the pre-IBC period. For the parallel trend assumption to hold, the coefficient on *Treatment* × *Equity Shares* should be statistically insignificant. Consistent with our expectations, we find in columns 1 and 2 that the coefficient on *Treatment* × *Equity Shares* is 0.001 (robust standard error = 0.003) for standalone firms and 0.002 (robust standard error = 0.002) for group firms, both of which are statistically insignificant. Since the parallel trend assumption holds for our sample data with respect to *log(PP&E)*, we proceed to estimate regression specification (4) separately for standalone and group firms to examine real effects of the IBC-induced decline in equity inflows of standalone firms. We present the result of estimating specification (4) in Table 13.

[INSERT Table 13 HERE]

Columns 1 and 2 show that treated firms within the standalone sample suffered a decline in their real investments compared to control firms within the same sample in the post-IBC period. For example, the coefficient on *Reg* × *Treatment* × *Equity Shares* is negative and statistically significant at the 5% level in both columns. We focus on the estimates in column 2 because it represents a more rigorous specification, with a higher adjusted R-squared of 0.9545. To interpret the regression coefficients as elasticities, we compute the exponential (*e*, approximately 2.718) raised to the power of the coefficient (β_4). This calculation provides us the percentage change in the PP&E due to decline in equity inflows for standalone treatment firms relative to standalone control firms in the post-IBC period.

The coefficient on *Reg* × *Treatment* in column 2 is -0.034 (robust standard error = 0.017), indicating that capital expenditure for standalone treated firms declined by 3.3% (calculated as $1 - e^{-0.034} = 0.033$) compared to standalone control firms due to reduced inflow of equity capital for the former in the post-IBC period. This suggests that the passage of the IBC reform in 2016 caused distressed firms within our sample of standalone firms to suffer a decline in their real investments in the DiD sense.

On the other hand, columns 3 and 4 show that treated firms within our group sample did not experience any statistically significant change, either increase or decrease, in their capital expenditure compared to control firms within the same sample in the post-IBC period. For example, the coefficient on *Reg* × *Treatment* in both columns is positive but not statistically significant. This was anticipated because, in Section 5.1, we didn't document any change in access to equity capital for treatment firms versus control firms within the group sample.

Thus, our subsample analysis provides results consistent with our arguments. Specifically, we find that real effects of the IBC-induced decline in equity inflows are more pronounced among standalone firms than group firms. We interpret these findings as evidence that group firms benefit from access to internal capital markets, which shield them from bankruptcy-induced liquidation or reorganization. In contrast, standalone firms lack such a failsafe, prompting equity investors to invest less in standalone firms in the post-IBC period.

6. Conclusion

This paper contributes to the literature on the unintended consequences of creditor-friendly bankruptcy reforms by examining the impact of India's 2016 Insolvency and

Bankruptcy Code (IBC) reform on firms' access to equity capital. Our findings demonstrate that the IBC reform, by heightening the threat of bankruptcy-induced liquidation or reorganization, led to a significant decline in equity inflows among financially distressed firms. This adverse effect was particularly pronounced among standalone firms, which lack access to internal capital markets and are therefore more prone to adverse financial shocks.

Through rigorous econometric analysis leveraging the difference-in-differences (DiD) technique and entropy balancing, we establish the causal link between stronger creditor rights and reduced equity inflows. Auxiliary tests further validate these results, ruling out alternative explanations such as pre-existing trends or corporate actions. Moreover, the concentrated impact on standalone firms underscores financial constraint as the primary channel through which the IBC reform affects equity capital.

These findings hold important implications for policymakers. While creditor-friendly reforms aim to improve credit markets and enhance overall economic efficiency, our results reveal a potential trade-off, i.e., the decline in access to equity capital, particularly for financially constrained firms. Policymakers designing insolvency frameworks must consider mechanisms to mitigate these adverse outcomes, such as ensuring access to alternative financing options for distressed firms.

This study advances our understanding of the complex interplay between creditor rights and corporate financing decisions, providing a robust empirical foundation for future research. By illuminating the equity market implications of bankruptcy reforms, we pave the way for a more nuanced assessment of the welfare effects of creditor-friendly legal frameworks.

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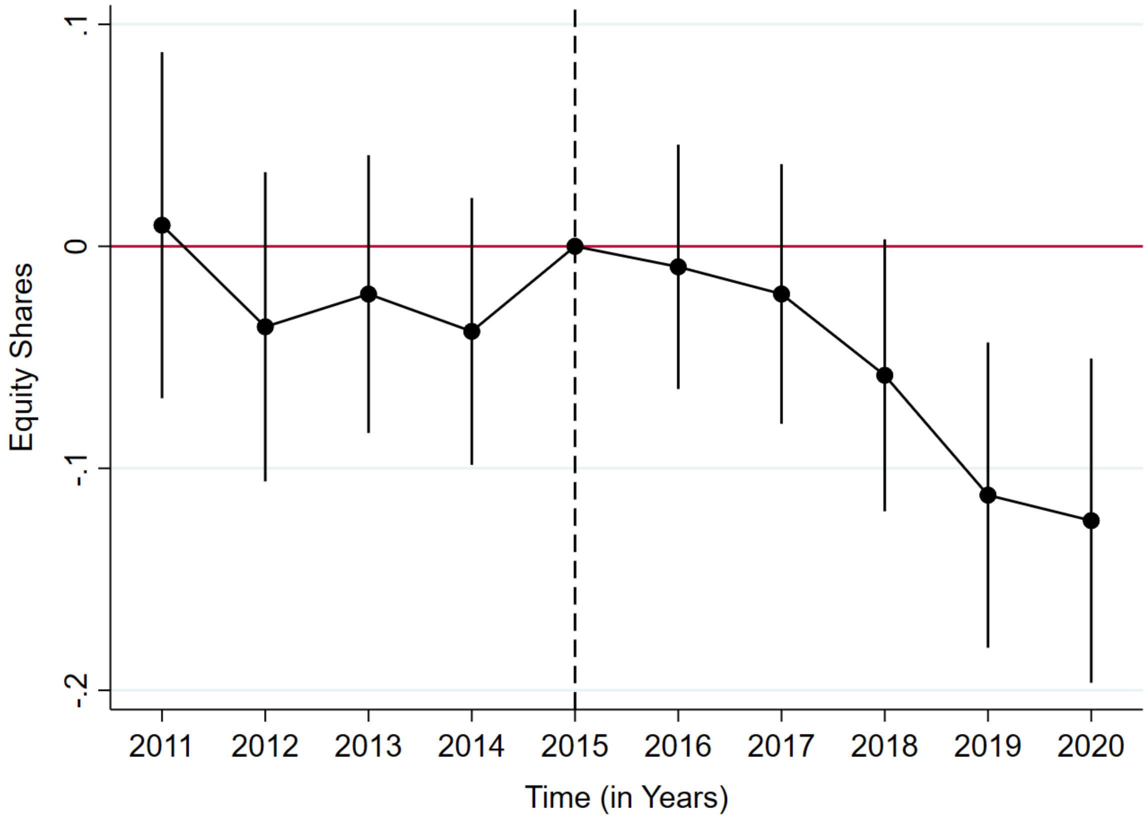
Figure 1: Illustration of the Parallel Trend Assumption



Note: This figure plots the coefficients and their 95% confidence intervals for the interaction terms between the indicator variable for each year and the indicator variable representing treatment firms. The dependent variable is the firm-year-varying Equity Shares. The base year is 2015, which is the last year of the pre-IBC sample period. The horizontal axis represents time (in years), while the vertical axis represents the coefficients on the interaction terms.

Table 1: Description of Variables

Variables	Description
Equity Shares	Natural logarithm of 'the total number of paid-up equity shares in the firm.'
Equity Capital	Natural logarithm of 'the paid-up share capital of the firm,' in INR millions. We alternatively refer to Equity Capital as Shareholders' Funds.
Growth	Time-series change in sales, computed as $(Sales_t – Sales_{t-1})/Sales_{t-1}$.
ROA	'Return on Total Assets', computed as the ratio of 'Earnings Before Interest and Taxes' (EBIT) to 'Total Assets,' in percent.
Liquidity	Surplus or deficit of 'Current Assets' over 'Current Liabilities' scaled by 'Total Assets.'
Tangibility	Fraction of 'Net Fixed Assets' in 'Total Assets.'
Size	Natural logarithm of 'Total Assets.'

Note: This table offers an overview of the variables utilized in the present study, where INR denotes Indian Rupees. To avoid situations where a firm-year observation is recorded as negative due to its absolute magnitude being less than 1, or omitted because its absolute magnitude is 0, we always increase the magnitude of a variable by 1 before calculating its natural logarithm. This approach does not alter the ordinal scale but helps to avoid unnecessary omissions or logically implausible negative observations.

Table 2: Summary Statistics

Variables	Obs	Mean	Std. dev.	Min	p25	p75	Max
Equity Shares	11,780	15.875	1.678	12.125	14.962	16.809	19.440
Equity Capital	11,780	4.274	1.173	1.704	3.568	4.992	6.886
Growth	11,780	0.120	0.221	-0.323	-0.007	0.224	0.788
ROA	11,780	6.499	5.168	0.134	2.397	9.344	20.896
Liquidity	11,780	0.176	0.178	-0.139	0.050	0.281	0.611
Tangibility	11,780	0.281	0.159	0.017	0.154	0.394	0.629
Size	11,780	7.478	1.598	4.454	6.292	8.599	11.041

Note: This table reports detailed summary statistics of the variables. It provides information on the number of firm-year observations per variable (Obs) along with their Mean, Standard Deviation (Std. dev.), Minimum (Min), 25^{th} percentile (p25), 75^{th} percentile (p75), and Maximum (Max).

Table 3: Entropy Balancing Diagnostics

Variables	Panel A: Pre-Entropy Balance					
	Treatment firms			**Control firms**		
	Mean	Variance	Skewness	Mean	Variance	Skewness
Growth	0.115	0.051	0.750	0.124	0.047	0.796
ROA	5.056	17.23	1.274	7.492	30.81	0.780
Liquidity	0.129	0.026	0.871	0.208	0.032	0.367
Tangibility	0.309	0.029	0.088	0.261	0.021	0.453
Size	7.71	2.806	0.122	7.318	2.321	0.349
Variables	**Panel B: Post-Entropy Balance**					
	Treatment firms			**Control firms**		
	Mean	Variance	Skewness	Mean	Variance	Skewness
Growth	0.115	0.051	0.750	0.115	0.051	0.750
ROA	5.056	17.23	1.274	5.056	17.23	1.274
Liquidity	0.129	0.026	0.871	0.129	0.026	0.871
Tangibility	0.309	0.029	0.088	0.309	0.029	0.088
Size	7.71	2.806	0.122	7.71	2.806	0.122

Note: This table presents the mean, variance, and skewness of firm-level covariates by the treatment and control firms before and after entropy balancing.

Table 4: Test of the Parallel Trend Assumption

Variable	Coefficient	Equity Shares (1)	Equity Shares (2)
$D_{-4} \times Treatment$	β_{-4}	-0.000 (0.045)	0.009 (0.047)
$D_{-3} \times Treatment$	β_{-3}	-0.044 (0.042)	-0.036 (0.042)
$D_{-2} \times Treatment$	β_{-2}	-0.033 (0.037)	-0.021 (0.038)
$D_{-1} \times Treatment$	β_{-1}	-0.057 (0.035)	-0.038 (0.036)
$\boldsymbol{D_0 \times Treatment}$	$\boldsymbol{\beta_0}$	**Omitted (2015 serves as the reference year)**	**Omitted (2015 serves as the reference year)**
$D_{+1} \times Treatment$	β_{+1}	-0.004 (0.032)	-0.009 (0.033)
$D_{+2} \times Treatment$	β_{+2}	-0.030 (0.035)	-0.021 (0.035)
$D_{+3} \times Treatment$	β_{+3}	-0.067* (0.037)	-0.058* (0.037)
$D_{+4} \times Treatment$	β_{+4}	-0.100** (0.040)	-0.112*** (0.041)
$D_{+5} \times Treatment$	β_{+5}	-0.107** (0.043)	-0.123*** (0.044)
Constant	α	15.918*** (0.010)	16.139*** (0.012)
Entropy Balanced		No	Yes
Firm Effects		Yes	Yes
Industry × Year Effects		Yes	Yes
Observations		11,780 firm-year observations	11,780 firm-year observations
F-statistic		1.60***	1.76***
Adjusted R-squared		0.9313	0.9368

Note: This table documents the result of estimating specification (2): $Equity\ Shares^i_t = \alpha + \sum_{p=-4}^{+5} \beta_p D_p \times Treatment^i + (Firm\ and\ Industry \times Year)\ fixed\ effects + \varepsilon^i_t$. Column 1 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 matches the treatment and control groups on these covariates. This indicates that our results from estimating specification (2) are robust to matching the treatment and control groups. Robust standard errors are enclosed within parentheses. ***, **, and * indicates $p \leq 0.01$, 0.05, and 0.10 respectively.

Table 5: Univariate Analysis

Variables	Panel A: Treatment firms				Panel B: Control firms			
	Pre-Regulation		Post-Regulation		Pre-Regulation		Post-Regulation	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Equity Shares	16.088	1.601	16.292	1.675	15.529	1.632	15.786	1.693
Equity Capital	4.444	1.127	4.586	1.148	4.027	1.157	4.189	1.170
Growth	0.154	0.236	0.076	0.207	0.180	0.224	0.068	0.195
ROA	4.858	4.053	5.254	4.237	7.610	5.721	7.373	5.372
Liquidity	0.110	0.158	0.149	0.165	0.185	0.174	0.231	0.182
Tangibility	0.322	0.173	0.297	0.170	0.270	0.148	0.253	0.146
Size	7.502	1.648	7.917	1.675	7.052	1.481	7.583	1.519

Note: This table enumerates the pre and post-IBC mean and standard deviation of winsorized variables used in this study for both the treatment and control groups. Table 1 reports the description of variables, while Table 2 enumerates detailed summary statistics on these variables.

Table 6: Pairwise Correlation Matrix

Variables	Equity Shares	Growth	ROA	Liquidity	Tangibility	Size
Equity Shares	1.000					
Growth	-0.026	1.000				
ROA	0.064	0.168	1.000			
Liquidity	-0.148	-0.098	0.264	1.000		
Tangibility	0.122	0.049	-0.029	-0.477	1.000	
Size	0.623	-0.036	0.204	-0.176	0.068	1.000

Note: This table summarizes the pairwise correlation among variables used in this study. *Equity Shares* and *Equity Capital* represent the same construct, namely the amount of paid-up capital available with a firm. As a result, they are highly correlated, with a correlation coefficient of 0.845.

Table 7: Variance Inflation Factor (VIF)

Variables	VIF	1/VIF
Growth	1.34	0.747
ROA	3.15	0.317
Liquidity	2.25	0.443
Tangibility	4.23	0.236
Size	6.84	0.146
Mean VIF	**3.56**	

Note: This table documents the variance inflation factor (VIF) of variables used in the study. For a description of these variables, refer to Table 1.

Table 8: Baseline Results

Variables	Equity Shares (1)	Equity Shares (2)
Reg* × *Treatment	**-0.035*** **(0.019)**	**-0.048**** **(0.020)**
Constant	15.906*** (0.006)	16.131*** (0.006)
Entropy Balanced	No	Yes
Firm Effects	Yes	Yes
Industry × *Year Effects*	Yes	Yes
Observations	11,780 firm-year observations	11,780 firm-year observations
F-statistic	3.14***	5.62***
Adjusted R-squared	0.9312	0.9367

Note: This table presents our baseline estimates from the regression of *Equity Shares* on *Reg* × *Treatment*. These estimates assess the differential impact of the IBC reform on equity inflows of treatment versus control firms in the post-IBC period. Column 1 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 matches the treatment and control groups on these covariates. Bold values represent the estimates of interest in each column. *, **, and *** implies significance at the 10%, 5%, and 1% levels respectively. Parenthesis enclose robust standard errors.

Table 9: Equity Capital as the Dependent Variable

Variables	Equity Capital (1)	Equity Capital (2)
Reg* × *Treatment	**-0.029***** **(0.011)**	**-0.035***** **(0.011)**
Constant	4.301*** (0.003)	4.478*** (0.003)
Entropy Balanced	No	Yes
Firm Effects	Yes	Yes
Industry × *Year Effects*	Yes	Yes
Observations	11,780 firm-year observations	11,780 firm-year observations
F-statistic	7.13***	9.80***
Adjusted R-squared	0.9545	0.9596

Note: This table presents our estimates from the regression of *Equity Capital* on *Reg* × *Treatment*. These estimates assess the differential impact of the IBC reform on equity inflows of treatment versus control firms in the post-IBC period. Column 1 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 matches the treatment and control groups on these covariates. Bold values represent the estimates of interest in each column. *, **, and *** implies significance at the 10%, 5%, and 1% levels respectively. Parenthesis enclose robust standard errors.

Table 10: Placebo Test

Variables	Equity Shares (1)	Equity Shares (2)
Reg* × *Treatment	**0.021** **(0.025)**	**0.018** **(0.025)**
Constant	15.766*** (0.007)	15.960*** (0.008)
Entropy Balanced	No	Yes
Firm Effects	Yes	Yes
Industry × Year Effects	Yes	Yes
Observations	4,712 firm-year observations	4,712 firm-year observations
F-statistic	0.72	0.54
Adjusted R-squared	0.9527	0.9569

Note: This table presents our placebo test estimates from the regression of *Equity Shares* on *Reg* × *Treatment*. These estimates assess the differential impact of the IBC reform on equity inflows of treatment versus control firms in the post-IBC period. Column 1 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 matches the treatment and control groups on these covariates. Bold values represent the estimates of interest in each column. *, **, and *** implies significance at the 10%, 5%, and 1% levels respectively. Parenthesis enclose robust standard errors.

Table 11: Standalone versus Group Firms

	Standalone Firms		Group Firms	
Variables	Equity Shares (1)	Equity Shares (2)	Equity Shares (3)	Equity Shares (4)
Reg* × *Treatment	**-0.049**** **(0.021)**	**-0.041**** **(0.021)**	**-0.000** **(0.037)**	**-0.038** **(0.041)**
Constant	15.320*** (0.006)	15.468*** (0.006)	16.764*** (0.012)	16.993*** (0.014)
Entropy Balanced	No	Yes	No	Yes
Firm Effects	Yes	Yes	Yes	Yes
Industry × Year Effects	Yes	Yes	Yes	Yes
Observations	6,940 firm-year observations	6,940 firm-year observations	4,840 firm-year observations	4,840 firm-year observations
F-statistic	5.52**	3.84**	0.00	0.88
Adjusted R-squared	0.9169	0.9290	0.9186	0.9198

Note: This table presents our placebo test estimates from the regression of *Equity Shares* on *Reg* × *Treatment* separately for standalone and group firms. These estimates help us distinguish the differential impact of the IBC reform on equity inflows of treatment versus control firms within the standalone and group samples in the post-IBC period. Column 1 and 3 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 and 4 matches the treatment and control groups on these covariates. Bold values represent the estimates of interest in each column. *, **, and *** implies significance at the 10%, 5%, and 1% levels respectively. Parenthesis enclose robust standard errors.

Table 12: Parallel Trends Assumption Test for Real Effects Analysis

	Standalone	**Group**
Variables	*Δlog(PP&E)* (1)	*Δlog(PP&E)* (2)
Treatment	-0.035 (0.056)	-0.059 (0.048)
Equity Shares	0.000 (0.002)	-0.000 (0.001)
Treatment* × *Equity Shares	**0.001** **(0.003)**	**0.002** **(0.002)**
Constant	0.099*** (0.030)	0.126*** (0.031)
Observations	3,470 firm-year observations	3,470 firm-year observations
F-statistic	1.82	4.95***
R-squared	0.001	0.005

Note**:** This table report estimates for the Lemmon and Roberts (2010) parallel trend assumption test necessary for validity of the DiD results. Bold values represent the estimates of interest in each column. *** indicates significance at the 1% level. Parenthesis enclose robust standard errors.

Table 13: Real Effects

	Standalone Firms		Group Firms	
Variables	log(PP&E) (1)	log(PP&E) (2)	log(PP&E) (3)	log(PP&E) (4)
Reg × Treatment	0.478 (0.301)	0.330 (0.274)	-0.362 (0.287)	-0.271 (0.284)
Reg × Equity Shares	0.037*** (0.012)	0.021** (0.010)	-0.025** (0.011)	-0.019* (0.010)
Treatment × Equity Shares	0.189*** (0.054)	0.204*** (0.057)	0.096*** (0.019)	0.098*** (0.019)
Reg × Treatment × Equity Shares	**-0.042**** **(0.019)**	**-0.034**** **(0.017)**	**0.016** **(0.016)**	**0.012** **(0.016)**
Constant	4.629*** (0.318)	4.501*** (0.420)	6.860*** (0.186)	6.921*** (0.209)
Entropy Balanced	No	Yes	No	Yes
Firm Effects	Yes	Yes	Yes	Yes
Industry × Year Effects	Yes	Yes	Yes	Yes
Observations	6,940 firm-year observations	6,940 firm-year observations	4,840 firm-year observations	4,840 firm-year observations
F-statistic	21.45***	20.42***	10.55***	9.25***
Adjusted R-squared	0.9410	0.9545	0.9518	0.9567

Note: This table reports the estimates of the impact that the IBC reform-induced decline in equity inflows had on firms' capital expenditure, separately for standalone and group firms. Column 1 and 3 does not match the treatment and control groups on firm-year covariates, such as *Growth*, *ROA*, *Liquidity*, *Tangibility*, and *Size*, whereas Column 2 and 4 matches the treatment and control groups on these covariates. Bold values represent the estimates of interest in each column. *, **, and *** implies significance at the 10%, 5%, and 1% levels respectively. Parenthesis enclose robust standard errors.