Towards win-win: Evidence from commercial dispute resolution in India

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August 26, 2025

Abstract

Courts are central in contract enforcement, and yet we know very little about how they function and their impact in the context of low state capacity. We leverage random assignment of commercial and contractual cases to judges in newly created commercial courts in India to examine the impact of mediation-led settlement on litigating firms' profits. The resolution of a case through mutual reconciliation ("settlement") rather than a full-length trial is negatively correlated with its duration and its pending status. We estimate a large, positive effect on the profitability of plaintiff firms and negative effects among defendant firms. However, the negative effects among defendants are driven by the timing of suing rather than from settlement. We find suggestive evidence that settlement, in fact, stems the loss, suggesting a Pareto improvement.

1 Introduction

The judiciary is an important branch of the state that plays a central role in enforcing the rule of law, contracts, and property rights to enable the efficient functioning of markets. In dynamic business environments, disputes naturally arise due to disagreements on contractual terms, payment delays or defaults, and other disagreements. For courts to be effective, disputes need to be resolved within a reasonable time, which depends not only on the courts but also on the litigants and their representing lawyers in enabling a faster resolution.

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Recent research has shown that judicial capacities vary vastly over space and time, and any constraints on judicial capacity have important ramifications for firm productivity, job creation, and overall economic development (Ponticelli and Alencar 2016; Coviello et al. 2018; Chemin 2020; Amirapu 2021; Kondylis and Stein 2023; Rao 2024). In addition to staffing levels and vacancy rates among judges in courts (Ponticelli and Alencar 2016; Rao 2024, 2025), timely resolution of cases is also affected by litigants' response to court summons for hearings and their ability to resolve disputes through mutual reconciliation within the framework of the law (Sadka et al. 2024).

This paper examines the impact of resolving commercial and contractual disputes through mutual reconciliation (which we will refer as settlement throughout the paper) in recently introduced commercial courts in India on litigating firms' profits. To study this, we use data from the universe of legal cases in two large, newly created exclusive commercial courts, and exploit exogenous variation generated by random assignment of cases to judges. Random assignment allows us to isolate whether a case is settled due to being assigned to a settlement-prone judge or not (similar to judge leniency used in labor economics, for example Arnold et al. 2018).[1] We instrument whether a case is settled using judge-specific settlement propensity measured as a jack-knife leave-out average settlement rate across cases from others litigants assigned to the same judge. We measure the causal effect on firms engaged in litigation using the jackknife settlement propensity before and after the case was resolved (either through settlement or through a full trial in the case of the counterfactual), separately for firms engaged as a plaintiff or as a defendant. We use a balanced panel of quarterly balance sheet data on profit and loss from CMIE Prowessdx database for our sample firms, which enable us to combine the jack-knife approach with Difference in Differences (DiD) design to estimate the causal effect parameter.

Commercial courts were created under the federal Commercial Courts Act, 2015, with the objective of bringing faster resolution to commercial and contractual dispute litigation. Specifically, the reform focused on mandatory mediation to encourage settlement rather than resolution through full trial. Dedicated commercial courts were established in four commercial centers in India - Delhi, Mumbai, Bengaluru, and Kolkata. These metropolitan areas are also regions where most firms are located (these 4 regions together account for over 50% of all the formal sector enterprises in India). We use the universe of case-level data from Delhi and Bengaluru commercial courts that were publicly available for analysis for this study.[2]

First, we document several empirical facts pertaining to the set of commercial litigation in these courts. We find that a large fraction (around 80%) of plaintiffs are banks/financial enterprises and non-financial sector firms. Only a small proportion of the plaintiffs are individuals or informal businesses lacking firm identifiers. Half the cases are new cases filed by the plaintiff ("original suit") whereas a quarter pertain to the execution of past court orders. 10% are appeals.

[1] Random assignment of cases have recently been built into the Court Information System or CIS as part of the ongoing efforts to digitize and improve the business process workflow in courts across India. This automatic random assignment started in early 2020.

[2] We contacted the remaining 2 commercial courts for access to case-level data but we were unable to access it.

Second, we find systematic correlation between resolution of a case by settlement and it's shorter duration or its pending status in the court. Conditional on court, registration date, and case-type fixed effects, a case that is settled through mutual reconciliation is 16 percentage points less likely to remain pending towards the end of the study period and is resolved 52 days earlier than similar cases that are not settled and undergo full trial. The correlation between these case-level outcomes and the leave-out settlement average is also significant, suggesting that a case is significantly less likely to remain pending if it is assigned to a settlement-prone judge.

Third, the probability that a case itself is settled through mutual reconciliation is largely determined by the judge assigned to the case. Judges vary in their settlement propensity; some judges are better at helping negotiate an outcome that is agreeable to both plaintiff and defendant whereas others are not. We find that a case assigned to a settlement-prone judge is over 70 percentage points more likely to be settled. This probability is similar whether we examine cases where the sample firm appears as a plaintiff or when they appear as a defendant. The settlement behavior is particularly significant among firms that are banks or financial institutions. When banks appear as plaintiffs, they are 10 percentage points *more* likely to settle. In contrast, when they appear as a defendant, they are 24 percentage points *less* likely to settle.

Finally, we examine the causal effects of being assigned a settlement-prone judge on firms' profits after the resolution of their case. We examine the profits of plaintiffs or defendants assigned to settlement-prone judges using jack-knife leave-out average settlement rate before and after a case is decided, following the standard judge leniency design in understanding the outcomes of bail decisions in criminal cases (Kling 2006; Arnold et al. 2018). Since we have a balanced panel of quarterly data on firms' profits, we contribute to the literature on judge-leniency designs by combining random assignment of cases with panel data on litigant outcomes in a modified judge-leniency-DiD research design. We find large effects on quarterly profits among plaintiff firms assigned a settlement-prone judge following the resolution of their case in the court. We find negative effects among defendant firms assigned to settlement-prone judges, but this stems from the timing of filing the suit rather than from settlement. The effect continues for plaintiff firms even when we restrict the analysis sample to quarters after a case is initially filed in the court, whereas the effect reverses for defendant firms, further suggesting that the negative effects seen in the simple DiD regression arises from not accounting for timing of filing the case.

This paper makes several contributions to the literature. First, it contributes to the growing literature on courts and development by documenting *what* affects judicial efficiency and subsequently firm productivity. This is among the first set of papers in this literature to use judge-specific characteristics, such as settlement propensity, to resolve a contractual dispute in a timely manner as opposed to procedural or legal reforms that are often complicated and require support through legislation. The results suggest a win-win outcome, where settlement reduces both case duration and its pendency status, improving the efficiency of dispute resolution. Both plaintiff and defendant firms experience positive productivity benefits from improved court efficiency. This is consistent with a rich theoretical and empirical literature on court efficiency and economic growth, suggesting

that firms and economic agents shift to a second-best equilibrium in response to judicial inefficiency (Djankov et al. 2003; Nunn 2007; Visaria 2009; Coviello et al. 2015, 2018; Chemin 2020; Kondylis and Stein 2023; Mehmood and Ali 2024; Liu et al. 2022). This paper provides experimental evidence from random assignment of cases to settlement-prone judges, showing the inverse of this relationship: when courts function better, the resulting outcome is Pareto superior. This paper highlights the importance of mutually agreed-upon settlement of the dispute, facilitated within the legal framework by a judge, as a solution to the problem of court inefficiency.

Second, this paper contributes to finance and banking literature in developing economies. We show that banks are among the biggest users of courts for contractual disputes concerning their lending operations. Thus, court efficiency specifically matters for the financial sector (Visaria 2009; Lilienfeld-Toal et al. 2012; Rao 2024) beyond sectors that are relationship-intensive (Nunn 2007; Ponticelli and Alencar 2016). Banks are strategic when it comes to settling cases in courts. As plaintiffs, they are mostly similar in their propensity to settle a case when assigned to a settlement-prone judge compared to any other type of firms. In contrast, they are less likely to settle a case as a defendant. This could be because agents representing a bank may have differential incentives to settle depending upon their role in a dispute. As a plaintiff, bank disputes are typically the enforcement of credit contracts. As a defendant, the disputes are generally counter-suits filed by borrowers to prevent banks from liquidating assets. Consequently, banks are potentially less likely to agree with settlement conditions in such situations.

Third, this paper is related to and consistent with Sadka et al. (2024), showing that litigants often are over-optimistic in the outcome of their case. Mediation process addresses some of the key challenges stemming from information asymmetry, enabling firms to resolve their dispute through mutually agreeable settlement terms, facilitated by judges. Settlement reduces the duration of a case while also releasing factors of production from dispute that could be put to better use. This paper extends the scope and conclusion by Sadka et al. (2024), who studied labor disputes, to all commercial disputes, including those by financial institutions in the process of debt recovery.

2 Background

Prior to 2015, commercial litigation in India was handled by general civil courts, which were often overburdened with cases across different dispute types. This resulted in significant delays in adjudicating commercial matters, undermining contractual reliability and increasing transaction costs for firms operating in the country (Ghosh, 2018).

In response to these inefficiencies, the Commercial Courts Act, 2015 was enacted by the Government of India to establish dedicated courts for the adjudication of commercial disputes. The Act introduced Commercial Courts at the district level, Commercial Divisions in High Courts with original jurisdiction, and Commercial Appellate Divisions for appeals. According to Section 2(1)(c) of the Act, the scope of a "commercial dispute" includes a broad set of issues such as breach

of contract, shareholder disputes, intellectual property rights, and admiralty matters, provided the monetary value involved exceeds *Rs.*1 crore (approximately USD 120,000).

The legislation aimed to streamline dispute resolution through procedural innovations. A key feature is mandatory mediation, which must be undertaken before a commercial suit is filed — except in cases requiring urgent interim relief. If mediation fails, the formal trial process begins with structured phases: filing of pleadings, disclosure and inspection of documents, issue framing, examination of witnesses, and final arguments. The judgment is required to be delivered within 90 days of conclusion of final arguments. Despite procedural streamlining, the examination phase remains the most time-consuming, often extending up to six months, where the entire process typically takes about a year(The Economic Times, 2021).

Another relevant innovation under the Act was the introduction of random allocation of cases to judges through a digital Case Information System (CIS 3.2). Since 2019, this system has been operational in the dedicated Commercial Courts of Delhi, Mumbai, Bengaluru, and Kolkata. By automatically assigning cases without human intervention, the system minimizes scope for "forum shopping" or bias in judicial assignment, enhancing transparency and fairness in the process. We use the universe of randomly assigned cases in the courts of Delhi and Bengaluru for this study.[3]

3 Empirical Design

We leverage random assignment of contract and commercial dispute cases to judges in two exclusive commercial courts in India. This new randomized case assignment system (see Figure A1 for an example of the assignment process) that started in late 2019-2020 in these courts replaced an old system, which was a black-box that followed the discretion of court's principal administrative judge in assigning cases.

Following a large literature in empirical legal studies and labor economics such as (Kling, 2006; Arnold et al., 2018) and many others, our empirical design combines an instrumental variable design with difference in differences (DiD) design, where the endogenous, case-specific settlement outcome is instrumented by a jackknife leave-out settlement average using settlement outcomes of cases by other firms presided by the same judge. Since judges vary in their ability to mediate a settlement between litigating firms without a full trial, cases assigned to judges who encourage settlement could experience better outcomes through mutually agreed-upon resolution to the dispute.

We construct the instrument as below, following the standard leave-out judge leniency instrument as constructed in Arnold et al. 2018:

$$z_{cj} = \left(\frac{1}{n_j - n_{ij}}\right)\left(\sum_{k=0}^{n_j} \text{Settled}_{ik} - \sum_{c=0}^{n_{ij}} \text{Settled}_{ic}\right) \tag{1}$$

where Settled_{ik} represents a dummy variable for each case k with a judge j that takes value 1 if that case is settled. Similarly, Settled_{ic} is a dummy variable for firm i's case c with judge j that takes value 1 if it is settled. n_j and n_{ij} are the

[3]Case-level data from Mumbai and Kolkata were not available for the study.

total number of cases assigned to judge j and the number of firm i's cases assigned to judge j, respectively. z_{cj} is the leave-out average proportion of cases assigned to judge j that are settled. As our case-level data spans only 3 years (2020-2023) following the start of random assignment in 2020, we only compute the leave-out settlement rate at judge level as opposed to judge-year level to preserve statistical power.

Following this, our estimate the following empirical specification as the "first stage" of contract enforcement in courts:

$$\text{Case Outcome}_{cij} = \delta_s + \delta_m + \delta_l + \beta z_{cj} + \varepsilon_{cij} \tag{2}$$

where $\text{Case Outcome}_{cij}$ of case c of firm i assigned to judge j includes whether the case is resolved through settlement. It can also mean other outcomes including duration and pending status. The specifications include city location s fixed effect, registration month m fixed effect, and case type l fixed effect. City location and registration month are stratifying variables used in the randomization process and thus, follows the design. We include case type l fixed effect as there may be an imbalance in the flow of cases of different types over time, which may not follow a random process. However, conditional on case-type, the randomized assignment process ensures that the judge characteristics are orthogonal to the potential outcomes of the case or the litigants. We cluster the standard errors by judge, which is the unit of treatment variation.

We examine the outcomes of the litigating firms, separately for plaintiff and defending firms, for whom we have quarterly balance sheet panel data for multiple quarters before filing of their case, during when case is ongoing, and for multiple quarters after the case is resolved. Taking advantage of this panel structure, and the reduced form leave-out instrument, we estimate the effects of settlement on firm productivity - measured as standard deviation units from baseline (prior to case filing) profit as our main outcome of interest. We execute this as a generalized (continuous-valued) DiD design using the leave-out settlement average before and after the quarter the corresponding case is resolved. Our estimating equation is as follows:

$$\text{Profit}_{it} = \phi_s + \phi_m + \phi_i + \phi_t + \gamma z_{cj} \text{ x } Post_t + \alpha Post_t + \varepsilon_{cij} \tag{3}$$

where i denotes the litigating firm with profit reported in quarter t. The rest of the subscripts are as defined as before. In addition to city location and registration month fixed effects, we include firm and reporting quarter fixed effects as in standard DiD specifications. We cluster the standard errors by the assigned judge.

Using Equation 3, we compare the outcomes of firms assigned to a higher-intensity settlement-prone judge to those assigned to a lower-intensity settlement-prone judge before and after their corresponding case is resolved. We also include firms whose cases continue to remain pending until the end of the study period as never-treated.

For causal inference, we mainly invoke assumptions used in staggered DiD designs as the dates on which a settlement order is passed for specific litigants are staggered. Random assignment of cases ensures that the judge leave-out settlement-propensity is exogenous to case and litigant-specific potential outcomes.

Since we estimate our main models - Equation 2 and Equation 3 - as reduced-form specifications using OLS estimators rather than estimating the local average treatment effect (LATE) parameter using two-stage-least-squares (2SLS) estimator, the estimated coefficients should be interpreted as the average treatment effect on the treated (ATT). Consequently, we make weaker assumptions than requiring exclusion restrictions (i.e., the outcome stems from settlement alone and not due other mechanisms), or monotonicity (i.e., if judge A is lenient than judge B on case 1, then A should also be lenient than B on case 2) to hold. However, we test for the relevance and the predictive power of the instrument by estimating Equation 2 to enable us to infer about the causal effect of settlement in a specific contractual case on its litigants' outcomes.

Although we do not estimate a LATE parameter, we address concerns about exclusion restriction. Settlement resulting from case resolution is similar to sentencing or bail outcome, which are extensively studied in labor economics. For exclusion restriction to hold even with the random assignment of cases to judges, we need to assume that judges affect both plaintiff and defendant outcomes only through settlement and not through other channels. While this is a fundamentally untestable assumption, we argue that any other type of resolution, including full trial, takes longer duration than settlement. Furthermore, the Commercial Courts Act, 2015, requires mediation to be the first step in the dispute resolution process. This implies that a settlement at this stage would limit any other potential channels from even occurring to affect the litigants' outcomes. However, we do caveat that this relies on a fundamentally untestable assumption and that we rely on the context to provide support towards the validity of this assumption.

Following our choice of reduced-form model, we also do not require monotonicity conditions to hold. Recent literature (Bhuller and Sigstad 2022; Sigstad 2023; Frandsen et al. 2023) has questioned the validity of judge leniency designs in studies examining the consequences of sentencing or bail decisions, which implicitly impose a single dimension of decision-maker behavior (such as acquittal or bail approval) while in reality, the decisions could be multi-dimensional that challenge the monotonicity assumption. Settlement as a decision is significantly different from sentencing or bail decisions in one important factor: settlement arises from negotiations that generates a win-win outcome as opposed to decisions like sentencing/bail or even a contractual dispute following a full trial, which creates winners and losers. With the caveat that it is impossible to observe any judges' internal ranking on case outcomes, the nature of settlement suggests that if a less settlement-prone judge helps settle a case, then a more settlement-prone judge will also be able to settle the same case if it was assigned to them.

From the perspective of estimating Equation 2 using a reduced-form staggered DiD design, random assignment of cases to judges ensures that the judge settlement propensity instrument z_{cj} is orthogonal and independent of potential outcomes of both the case and its litigants. This also addresses the problem of selection bias arising from selection to different treatment intensities (Callaway et al. 2024). This reduces concerns about negative weights although estimation using Equation 3 still assumes homogeneous treatment effects. We present the raw means and event study specification to address concerns about dynamic treatment effects.

3.1 Exogeneity Due to Random Assignment

Table 1 presents the results of tests that support random assignment of cases to judges in the commercial courts sample. Columns 1-3 report regression coefficients on different types of litigation - whether it is a bank vs. bank, bank vs. firm, firm vs. firm, and so on - and firm characteristics (when we are able to identity and match the firm with firm-level data) in determining the identity of the specific judge assigned (dependent variable is judge id). Col 1 includes the entire case-level data in the commercial court sample. Columns 2-3 are restricted to the matched firm-level sample of plaintiffs and defendants, respectively. Columns 4-5 report results from a similar exercise using the plaintiff and defendant data where the dependent variable is the leave-out settlement rate. Since we can only construct the settlement instrument using the matched firm data, we carry out the tests in the analysis samples for matched plaintiff and defendant firm with case-level data. While some individual case and firm-level characteristics are statistically significant, we are unable to reject the joint null of any case or litigant-specific characteristic determining the leave-out instrument.

4 Data

This paper combines two primary data sources: (i) firm-level financial data from the Prowess dataset curated by the Centre for Monitoring Indian Economy (CMIE), and (ii) case-level records from two commercial courts in India - Delhi and Bengaluru. This section describes each dataset, outlines how we construct the analysis dataset (firm–court panel), and details our various classification algorithms to generate variables of interest.

4.1 Firm-Level Data

We use firm-level panel data from the Prowess database, a comprehensive source maintained by CMIE that includes financial statements for over 40,000 Indian firms. The dataset covers publicly listed, unlisted, and privately held companies and includes annual and quarterly information on profit and loss statements, balance sheets, cash flows, and firm characteristics such as industry affiliation, ownership, and incorporation year. Our primary firm-level outcome variable is quarterly profit (income net of expenses).

4.2 Judicial Data and Entity-Type Classification

We compile a case-level dataset of the universe of commercial court filings in Delhi and Bengaluru commercial courts since the start of random assignment of cases. These data contain detailed records for each case, including plaintiff and defendant names, filing and resolution dates, and basic metadata such as the judge name and case type.[4]

To structure the data for empirical analysis, we develop a rule-based text classification algorithm to categorize both plaintiffs and defendants into one of three

[4]We also have the universe of all case-level data from the inception of these courts but we do not include cases that were not randomly assigned to judges in our analysis.

mutually exclusive entity types: Individuals, Firms, and Banks. The algorithm first converts all party names to uppercase for standardization.

Banks are identified using keywords such as BANK, FINANCE, CREDIT. Firms are identified using a large set of regular expressions and keywords commonly associated with companies and organizations (e.g., LTD, LLP, PVT, TECH, FOUNDATION, SOCIETY, TRUST, etc.). All remaining entities are classified as Individuals, assumed to be natural persons not associated with any institutional identifier.

This classification is applied separately to plaintiffs and defendants. We then construct a categorical variable for each case indicating the entity-type pair involved in the dispute (e.g., Firm–Firm, Bank–Individual). This approach enables scalable classification of cases by actor type.

4.3 Matched Panel Construction

We link commercial court cases to firms in the Prowess database by matching the names of plaintiffs and defendants with firm names recorded in Prowess. To make the names comparable, we first convert all names to uppercase letters and remove extra characters like spaces and punctuation. This step ensures that minor formatting differences (e.g., "ABC Ltd." vs. "abc ltd") do not prevent a match.

We then use a string-matching procedure to compare the cleaned court names with firm names in Prowess. The match is based on exact or near-exact string matches, allowing for minor spelling or formatting differences. We keep only those cases where the match quality is high—specifically, where the string similarity score exceeds 90%. This ensures that we are linking firms to court cases with a high degree of confidence.

Using this approach, we construct two matched samples: one where the firm appears as the plaintiff and one where the firm appears as the defendant in a court case.

4.4 Summary of Entity-Type Pairs

Panel A of Table 2 presents the distribution of entity-type pairs across the full case-level sample and matched plaintiff and defendant samples. In the full sample, the most common case types involve Firm–Firm (29.9%) and Bank–Individual (20.3%) disputes, followed by Firm–Individual (14.7%) and Bank–Firm (12.7%). Cases involving Individual–Firm (10.4%) also appear with moderate frequency, while purely individual or bank-to-bank disputes are rare.

The matched plaintiff sample is overrepresented by bank-individual (44.0%) and bank-firm cases (38.0%), while firm-firm (11.8%) or firm-individual (5.92%) cases are under-represented. In contrast, the defendant-matched panel contains a higher share of banks or firms, which occurs by construction since Prowess data contains details only on formal sector firms, including banks. One key reason why these differences are stark is due to data constraints on litigant outcomes. Quarterly or high frequency data are mainly available for the formal sector firms, including banks, and hence such firms are over-represented in the matched firm-case dataset used in the analysis. We thus caveat our interpretation of the results keeping these differences in representation in mind.

4.5 Case Type Composition

We classify each case into four mutually exclusive categories based on the case number prefix in data: Execution (ComEX), Appeals (ComAA), Original/New Suits (ComOS), and a residual Other category. Table A1 reports the average shares of these case types across the full sample and the two matched samples. In the full sample, nearly half (48%) of all cases are original suits, followed by execution proceedings (23%) and other case types (19%). Appeals constitute a smaller share (10%). The analysis sample for plaintiff firms maintains this ordering qualitatively, where original suits make the largest share, followed by execution.

4.6 Firm Characteristics

Table A1 presents summary statistics for firms in the full Prowess dataset and for firms matched as plaintiffs and defendants in commercial court cases.

Firms in the matched plaintiff sample are, on average, slightly older (33.2 years) than both the general Prowess population (30.2 years) and firms in the defendant sample (31.5 years). This suggests that firms initiating litigation tend to be more established.

Sectoral composition also differs across samples. Plaintiffs are more likely to be in trade and retail (26.5%) compared to the full sample (18.6%) and defendants (15.7%). In contrast, manufacturing firms are underrepresented among plaintiffs (28%) and defendants (29.1%) relative to the full sample (37.3%). The share of firms in services is relatively stable across samples around 17–20%.

A notable distinction lies in ownership structure: the share of publicly listed firms is high in all samples, at around 54–61%, with the highest proportion among defendants (61.0%), suggesting that the outcome data on firm profit mainly represent large, formal sector firms.

4.7 Analysis Sample

We carry out our analysis separately for firms appearing as plaintiff and firms appearing as defendants. The plaintiff sample has 1490 cases from 3413 total cases in the case-level data where a plaintiff is classified as a firm (either a bank/financial firm or a non-financial firm). The defendant sample has 248 cases from 502 total cases in the case-level data where a defendant is classified as a firm. Within the plaintiff sample, over 85% plaintiff cases map to 34 unique financial sector firms and the remaining map to 119 unique non-financial firms. In the defendant sample, 53% cases map to 41 unique financial sector firms and the remaining 47% cases map to 83 non-financial sector firms. The firm-level observations include quarterly data from 2011-2024 for 34 financial firms and 119 non-financial firms in the plaintiff dataset and 41 financial firms and 83 non-financial firms in the defendant dataset.

Due to the compositional differences between the case-level data and the matched plaintiff and defendant firm-level data used for analysis, we interpret the causal estimates as applicable to this specific sample of litigating firms from the formal sector for whom we have corresponding outcome data.

5 Results

We carry out all our analysis separately for plaintiff and defendant firms as the behavior response to contract enforcement vastly vary by their role in a litigation. In our matched data for plaintiff firms, 10% of the firms have only one case. This masks substantial heterogeneity by sector. The median non-financial sector firms like those in manufacturing or trade have 1 case in total (average number of cases is 1.46 due to right skew; the maximum number of cases per firm in this sector is 5). In contrast, firms from the financial sector including banks have more cases per firm. The median number of cases among financial sector firms is 72, with a maximum of 471 cases per firm.

Among the defendant sample, 25% of the firms have only one case, with the median being 2 cases per firm. The median non-financial sector defending firm has 1 case (average is 1.55 and maximum is 7 cases per firm). Financial sector defending firms have an average 3.6 cases per firm (median 3 and maximum 25 cases per firm). We account for these different distribution of cases per firm when analyzing firm-level outcomes that we describe in detail below.

5.1 Leave-out settlement instrument

We calculate the instrument following the construction process described in Equation 1 for each dataset. In the plaintiff data, we calculate the leave-out average rate of settlement across all cases assigned to a specific judge over the study duration, after leaving out cases pertaining to the specific plaintiff firm. Similarly, we calculate the leave-out average settlement rate by judge in the defendant dataset, after leaving out cases pertaining to the specific defending firm.

We note that the distribution of judge settlement propensity (the leave-out instrument) exhibits large variation (see Figure 1). Some judges are more settlement-prone than others. Second, this distribution varies between plaintiff and defendant samples. Since these samples are generated from on merging with firm-level balance sheet data, we interpret this difference in settlement propensity to be a result of selection of firms into plaintiff or defendant samples rather than the same judge having two different settlement instruments.

5.2 Case-level outcomes

We carry out descriptive, correlational analysis to examine the association between settlement of a case with other case outcomes including it's pending status and duration to resolution. Table 3 presents these correlations, separately for plaintiff and defendant samples. Overall, we find that when a case is settled through mutual reconciliation, it is around 20 percentage points less likely to be associated with pending status. This is similar across both samples. We also note that a settled case is associated with lower duration from the time of filing. On average, plaintiff cases are settled about 52 days earlier than cases that are not settled and go through full trial. Among defendants, this association is even larger: settled cases experience 137 fewer days in court relative to other cases.

The reduced form effects of being assigned a settlement-prone judge is also in the similar direction (although loses statistical precision for case duration). A

more settlement-prone judge is over 30 percentage points less likely to keep plaintiff firms' cases pending until the end of the study period, and is also more likely to resolve the case sooner than judges who are less settlement prone (see Table 4). For defending firms, these numbers are even higher.

Next, we examine the "first stage" relationship between the judge leniency instrument (leave-out settlement rate) and the case-level settlement dummy. We find that being assigned a settlement-prone judge is strongly correlated with a case being settled in both datasets. Plaintiff firms' cases are 72 percentage points more likely to be settled (Col 1 Table 5) and defendant firms' cases are 79 percentage points more likely to be settled (Col 3 Table 5). The instrument has a substantial explanatory power, with F-statistic > 150 in the plaintiff dataset. This statistic is smaller in the defendant dataset, presumably due to smaller matched sample.

Finally, we examine the interaction between the judge instrument and whether the litigating firm is a financial sector firm or not. This, on its own, is an interesting exercise to examine whether the settlement rate varies by a firm's identity. Columns 2 and 4 Table 5 presents the results from this interaction specification. We find that financial plaintiff firms are 10 percentage points more likely to settle on their own, which doubles when assigned to a settlement-prone judge. Surprisingly, this interaction effect is negative in the defendant sample, where financial defendant firms assigned to settlement-prone judges are 24 percentage points less likely to settle. This suggests potentially differential bargaining power by the identity of the firms. Legal professionals and scholars in India suggest that financial sector firms appear as defendants when borrowers file counter suits to prevent liquidation or restructuring in debt recovery. In such cases, financial firms are less likely to settle as that would mean withdrawing their liquidation or asset restructuring processes. In contrast, it is more likely when such firms appear as plaintiff as they are more willing to settle renegotiating contractual terms, such as waiving parts of interest dues or extent repayment period, in debt recovery proceedings.

5.3 Firm-Level Productivity Effects

When examining firm-level effects, we need to take into account the fact that many firms have multiple cases. Thus, in order to study firm-level effects, we transform the case-level data into wide format and then merge it with firm-level quarterly profit data. This transformation allows us to examine settlement across all cases involving the said firm either as a plaintiff or defendant.

We define a firm as "treated" (i.e., resolve their dispute through settlement) if the earliest case (in terms of date of filing) is resolved through settlement. The timing of treatment is the decision date of the settlement order. Thus, we define post-treatment period as all quarters following the decision date of the settlement of the earliest disputes filed in the courts. For firms with only one case, this corresponds to the outcome of that case. For firms with multiple cases, the intervention corresponds to the earliest filed case is that settled. This naturally generates a staggered treatment design with pre and post periods in the quarterly profits. The never-treated group includes all the firms if none of their cases are resolved through settlement or if all their cases continue to remain pending. We test for robustness using alternate definitions of treatment, including using timing of the last filed case that is settled.

Figure 2 depicts the raw trends in firms' quarterly profits before and after judges' decision by treatment groups, conditional on the timing of filing the dispute. The profit variable is transformed into a standardized measure, as z-score relative to each firm's long-term average. This transformation also helps account for zeros in profit and is normally distributed by construction. We note that the profits of plaintiff firms increase after judges' decision for the group with settled cases relative to group with cases that are either not settled or continue to be pending at the end of the study period. In contrast, we note the opposite result among defending firms. However, a closer inspection of the figure should reveal that the divergence in profits among defending firms occurs before the decision, suggesting the need for a more careful analysis.

We report the DiD reduced form estimates of the effect of being assigned a settlement-prone judge after the date of decision among plaintiff and defendant firms in Table 6. Columns 1 and 2 present the results from the plaintiff dataset whereas Columns 3 and 4 present the results from defendant dataset. Furthermore, Columns 1 and 3 include the entire study period, i.e., 2011-2024, whereas Columns 2 and 4 include periods only after the filing of the first case in the court. The results for plaintiff firms are clear and robust. The quarterly profit of plaintiff firms with settled cases increases by over 0.5 standard deviation units after the decision relative to firms without a settled case or those with cases still pending. For defending firms, the effect on quarterly profits is negative when using the entire study period, including quarters before filing of their case. However, limiting the analysis time period to quarters after filing of the first case results in a positive, but imprecise coefficient.

Table A2 presents robustness against the definition of settlement when firms have multiple cases. We find qualitatively similar effects even when we examine the consequences of settlement among the last filed case. Columns 1 and 2 of Table A3 reports results from "placebo" test to examine the consequences of filing a case (as opposed to decision) in the court on the quarterly profits separately for plaintiff and defendant firms before the case is decided. We find smaller and statistically insignificant effect among plaintiff firms and a significant negative effect among defending firms. After including all time periods and using two separate events - one for filing and one for decision - in the DiD design (Columns 3 and 4 Table A3) supports our claim further. We find no effects upon filing but find significant positive effects on plaintiff firms' profits after decision. Among defendant firms, Column 4 clearly shows that the negative effect is entirely driven by the effects of filing the case and not from decision to settle or not.

6 Discussion: Doctrinal Legal Analysis

This section examines the implications of the findings through the lens of the statutory provisions within the legal system in India to draw policy implications.

6.1 Doctrinal Legal Analysis

The Commercial Courts Act, 2015, was an important legislative intervention aimed at streamlining commercial dispute resolution in India. The economic findings directly validate the Act's intended outcomes. The Act's primary objective was

to achieve faster resolution for commercial and contractual disputes. The study demonstrates that resolution through mutual reconciliation (settlement) is "negatively correlated with its duration and its pending status". Specifically, a settled case is "16 percentage points less likely to remain pending and is resolved 52 days ahead of similar cases that are not resolved through settlement". For defendant firms, this effect is even more pronounced, with settled cases experiencing "137 fewer days in court". These findings are consistent with the legislative intent behind establishing dedicated commercial courts and emphasizing settlement. From a legal standpoint, the reduction in case duration and pendency aligns with constitutional mandates for speedy justice and the efficiency principles underlying procedural laws such as the Code of Civil Procedure, 1908.

6.2 Mandatory Mediation

A critical feature of the Commercial Courts Act is mandatory mediation, which must be undertaken before a commercial suit is filed. The study's findings directly support the effectiveness of this mechanism. The positive effect on the profitability of plaintiff, and the stemming of losses for defendant firms through settlement, underscore mediation's role in achieving beneficial outcomes for litigants. This offers good reasons for exploring mandatory mediation provisions in other legislation as well, especially legislation dealing with commerce and business. The findings also provide a basis for defending these provisions against constitutional challenges to such requirements by arguing that they impede access to court.

The mandatory nature of Section 12A of the act has had implications on the interpretation of India's constitution after the Patil Automation case in the Supreme Court of India.[5] Therein, the court dismissed the arguments against the mandatory nature of the provision. The findings in this paper substantiate the court's reasoning by demonstrating better delivery of justice through settlements. The only exception under the rule in Section 12A are cases where urgency requires an interim relief. The critical issue here is that law does not define what is meant by "urgent", as used in the statute book. The results in this study demonstrate a case for the courts to give a very strict reading of what is statutorily meant by "urgent". A narrow reading should limit non-application of Section 12A to only few cases, thereby, incentivizing settlement in most cases.

There is a larger debate on this provision for mandatory mediation as well. The arguments against mandating mediation are premised on the fact that mediation is essentially a consent-based system, and thus, mandating it is contradictory to the philosophy of mediation. However, there is support from other literature such as work by Professor Frank Sander[6] [7], where he and his coauthors group case referrals to mediation into two classes, categorical and discretionary. In categorical cases, it is argued that the judge has no discretion and mediation is a must. Some authors have critiqued this approach, arguing that the benefits of mandatory mediation are not backed by empirical evidence.[8] This study counters the

[5]Patil Automation (P) Ltd. v. Rakheja Engineers (P) Ltd., 2022 SCC OnLine SC 1028

[6]Frank E. A. Sander, H. William Allen & Debra Hensler, Judicial (Mis)use of ADR? A Debate, 27 U. TOL. L. REV. 885, 886 (1996)

[7]Frank E. A. Sander, Another View of Mandatory Mediation, DISP. RESOL. MAG., Winter 2007, at 16

[8]Roselle L. Wissler, Court-Connected Mediation in General Civil Cases: What We Know

apprehensions expressed in these arguments by providing empirical evidence in support of mediation.

6.3 Random Assignment and Judicial Impartiality

The Courts randomly allocate cases to judges through a digital Case Information System (CIS 3.2) to minimize forum shopping or bias and enhance transparency. This system is part of a larger effort to introduce transparency and objectivity in the Indian judicial system at the district level. The basis of this measure is that every case should have a similar outcome irrespective of the judge who is running the court.

The integrity of random assignment ensures that the "judge settlement propensity is exogenous to case and litigant-specific potential outcomes". The findings however, indicate that the realization of settlement as an outcome has different probabilities in different court rooms. The finding that "judges vary in their settlement propensity" and that a case assigned to a "settlement-prone judge is over 70 percentage points more likely to be settled" suggests that effective mediation is a distinct judicial skill, not merely an incidental function. Legally, this has important implications for judicial training, performance evaluation, and potentially even judicial appointments in commercial courts. It supports the development of specialized judicial education programs focused on negotiation, conflict resolution, and mediation techniques for commercial court judges.

Even though judicial training academies and institutes exist in every state in India, and judicial officers are provided training after their induction into service, specific modules to increase the settlement propensity are potentially lacking in these training programs. The systemic problems in the judicial training philosophy and practice are immense and are documented extensively by the Supreme Court itself in the *All India Judges' Association Case*.[9] The case led to creation of the state-level judicial academies but still the induction programs do not happen for years after the judicial officers are appointed.[10]

6.4 Way Forward

The doctrinal analysis together with the empirical findings provides two clear pathways for policy scale-up. First, we now have empirical evidence in the context of commercial dispute resolution in India that suggests that mediation increases the efficiency of dispute resolution process when measured in terms of case duration or pending status. This can be used to resolve petitions challenging the constitutional validity of the mandatory mediation provision within the Commercial Courts Act, 2015, and serves as an example of how commercial dispute resolution codes can be designed in common law legal systems.

Second, this paper documents a wide variation in the extent of mediation abilities among commercial court judges. While this enabled causal identification

from Empirical Research, 17 OHIO ST. J. ON DISP. RESOL. 641, 695 (2002)

[9] All India Judges' Association v. Union of India, 1992 AIR 165

[10] Oberoi, G. (2018). Limitations of Induction Trainings Offered to Magistrates by State Judicial Educators in India. Athens JL, 4, 301.

for this paper, this variation has implications for judicial organizational structure in terms of training judges in the art of negotiating settlement through mediation.

7 Conclusion

To conclude, this paper is among the first to examine the causal effects of court-mediated settlement on litigating firms' welfare in contract enforcement and commercial dispute litigation. Random assignment of cases to judges in commercial courts in India has enabled causal inference by introducing exogenous variation in judge "leniency" or settlement-propensity to determine the final case outcome of commercial and contractual cases. Randomization also introduces independence between any of the litigating firms' identity and judge identity, minimizing concerns of forum-shopping where litigants may game the assignment system to get a favorable judge for their case.

We find that settling a case before undergoing full trial is beneficial both for case-level metrics such as reduced duration and lower pending status as well as for litigant welfare measured as quarterly profit. Whereas the profit effects are unequivocally positive for plaintiff firms, the effects on defending firms are a bit more subtle. These firms first experience a decline in their profit, which follows after the filing of their first case in the court. However, the trend reverses when the defending firms settle their case through mutual reconciliation.

Additional research is needed to interpret the policy implication of this finding on the long-run welfare of litigants and the broader economy. Should the role of courts be to facilitate settlement rather than trial? If so, why couldn't the litigants have settled the dispute themselves before filing the case in the court in the first place? To some extent, this could reflect overoptimism among litigants in obtaining a decision in their favor through full trial, which a judge can correct. Indeed, Sadka et al. (2024) shows that this is a possibility in the context of labor disputes in Mexican labor courts.

Another explanation is that litigants prefer formal approval to the resolution of their dispute, which is provided by courts. Mediation enables them to resolve their dispute faster *and* with a formal, state-supported mandate. This renders an interpretation that the state still plays an important role in contract enforcement even when the litigants themselves may resolve their dispute through mutual reconciliation because it could minimize future disagreements.

In general, court-facilitated mediation is a powerful solution to reduce the duration of legal cases in courts and reduce pending backlog. This generates a win-win for the litigating firms, signaling a Pareto rather than Kaldor-Hicks improvement.

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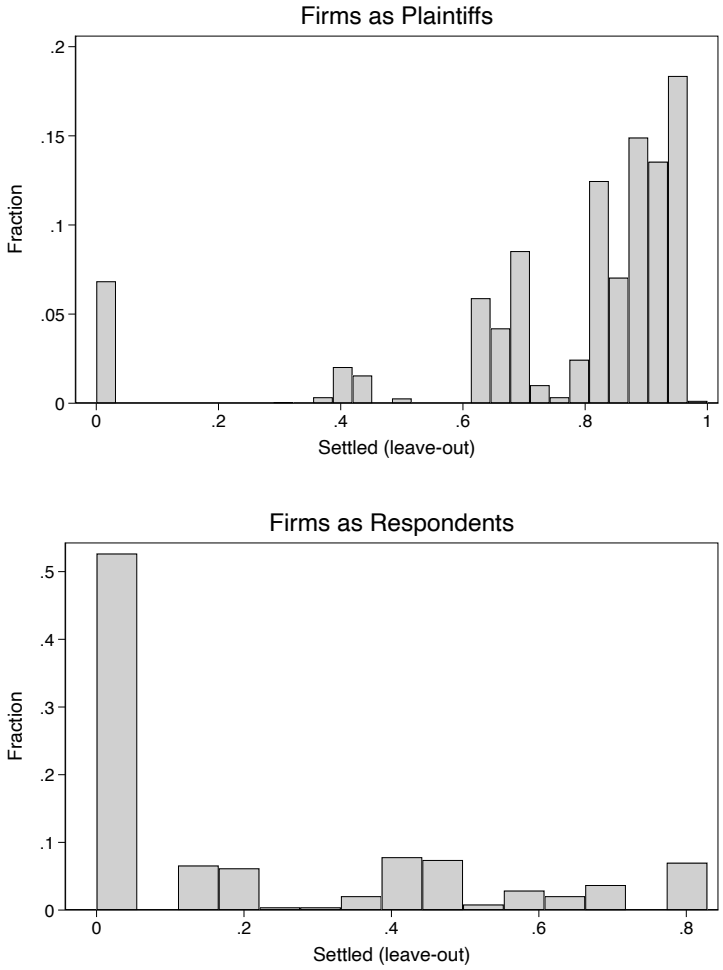
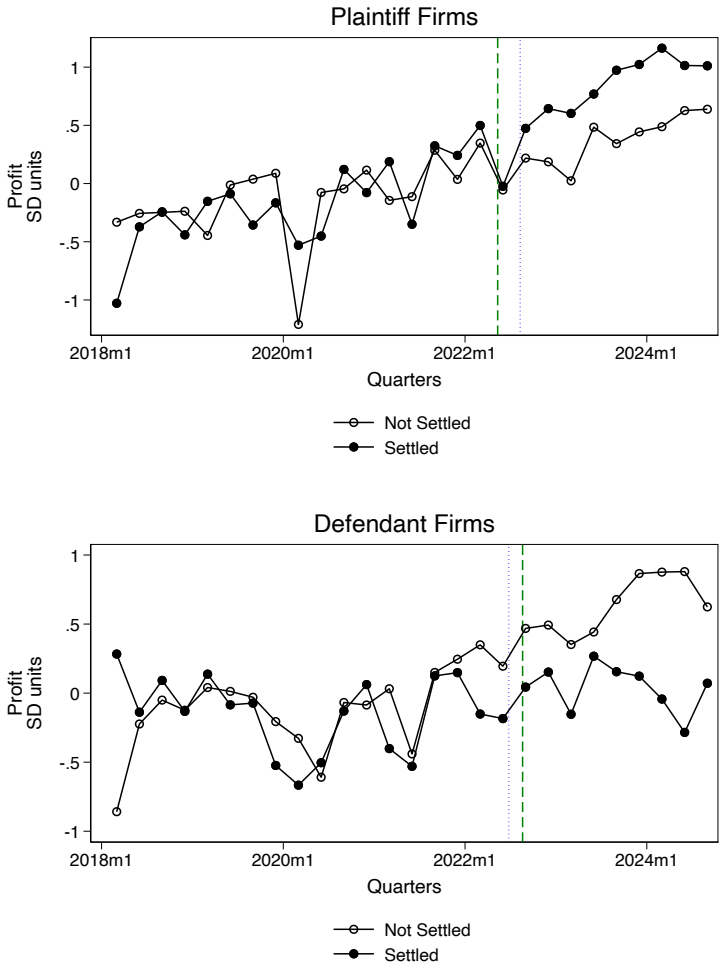
8 Figures

Figure 1: Settlement Propensity by Assigned Judges



Notes: Distribution of leave-out settlement average by judge based on cases classified as including firms as plaintiff (top) or as defendants (bottom).

Figure 2: Raw Means of Firm-level Quarterly Profit



Notes: The figures above present quarterly profit, measured in standard deviation units relative to the long run average firm-specific profit, separately for plaintiff and defendant samples. Dashed vertical lines denote the average decision month of cases with no settlement and dotted vertical lines denote the average decision month of cases that are settled.

9 Tables

Table 1: Balance Table

Dep Var	Judge ID			Leave-out Settled	
	(1) All	(2) Plaintiff	(3) Defendants	(4) Plaintiff	(5) Defendants
Bank–Bank Case	9.62	15.70	. †	-0.16	. †
	(18.49)	(22.58)	(.)	(0.12)	(.)
Bank–Firm Case	1.71	5.95	-7.57	-0.03	-0.18*
	(2.53)	(3.65)	(12.60)	(0.02)	(0.10)
Bank–Individual Case	1.24	5.62	15.94	-0.02	0.13**
	(2.55)	(3.59)	(12.31)	(0.02)	(0.06)
Firm–Bank Case	4.73	26.98***	7.88	-0.20*	0.01
	(3.36)	(8.13)	(7.41)	(0.12)	(0.03)
Firm–Firm Case	3.07**	2.60	-6.44	-0.02	0.07
	(1.41)	(4.64)	(8.65)	(0.03)	(0.06)
Age	–	0.00	0.02	-0.00**	0.00
	–	(0.03)	(0.08)	(0.00)	(0.00)
Non-Finance Firm	–	4.86	-3.68	-0.06	0.00
	–	(3.23)	(5.94)	(0.04)	(0.04)
Bank	–	-4.26*	-6.85	0.04*	-0.12*
	–	(2.41)	(12.37)	(0.02)	(0.06)
Observations	6,081	1,489	247	1,450	216
F-stat	0.98	3.64	1.92	0.96	1.12
Joint p-value	0.435	0.003	0.095	0.485	0.380
City Fixed Effects	✓	✓	✓	✓	✓
Registration-Month FE	✓	✓	✓	✓	✓
Case Type FE	✓	✓	✓	✓	✓

Notes: Robust standard errors in parentheses. The dependent variable in Columns (1)–(3) is `judge_id`; in Columns (4)–(5) it is the leave-out mean of settlement. All regressions include fixed effects for the city, registration month-year, and commercial case type (execution, appeals, or original suit). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$, † omitted due to no variation.

Table 2: Summary Statistics and Sample Composition

Panel A: Distribution of Litigant-Type Pairs

	(1) Full Case Sample	(2) Plaintiff	(3) Defendant
Bank–Bank	0.04	0.09	0.20
Bank–Firm	12.67	37.97	2.19
Bank–Individual	20.30	44.04	0.00
Firm–Bank	0.99	0.15	8.76
Firm–Firm	29.87	11.84	57.77
Firm–Individual	14.74	5.92	0.00
Individual–Bank	1.19	0.00	11.55
Individual–Firm	10.41	0.00	19.52
Individual–Individual	9.78	0.00	0.00
Cases	16,098	3,413	502

Panel B: Firm Characteristics Summary

	(1) Full Prowess Sample	(2) Plaintiff	(3) Defendant
Age	30.165	33.190	31.506
	(16.495)	(19.463)	(18.283)
Manufacturing	0.373	0.280	0.291
	(0.484)	(0.450)	(0.455)
Trade and Retail	0.186	0.265	0.157
	(0.389)	(0.443)	(0.365)
Services	0.166	0.194	0.198
	(0.372)	(0.397)	(0.399)
Publicly Listed	0.544	0.545	0.610
	(0.498)	(0.499)	(0.489)
Observations (Firms)	40,786	211	172

Notes: Panel A reports percentages of litigant-type pairs in the case-level data. Col 1 presents the distribution across the universe of commercial cases from the two courts in our study. Col 2 presents the distribution among a subset of these cases where we identify the plaintiff as a firm (either a bank/financial firm or non-financial firms) in Prowess. Col 3 presents the distribution among another subset where we identify the defendants as a firm (bank/financial or non-financial firms) in Prowess. Panel B presents the characteristics of firms matched to the litigants in the case-level data, reporting the means with standard deviations in parentheses. Col 1 represents the distribution of characteristics across the full sample in Prowess database. Cols 2 and 3 present the distribution of these characteristics among firms found in the case-level data, depending on whether the firm appears as a plaintiff or as a defendant, respectively.

Table 3: Correlation Between Case Settlement, Duration, and Pending Status

	(1) Pending Plaintiff	(2) Duration (Days) Plaintiff	(3) Pending Respondent	(4) Duration (Days) Respondent
Case Settled	-0.163***	-51.81***	-0.230**	-136.9***
	(0.0596)	(19.09)	(0.106)	(36.81)
Observations	1490	1440	248	229
No. Judges	43	43	37	37
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Case-Type FE	Y	Y	Y	Y
Control Mean	0.126	437.7	0.0865	406.8
Control SD	0.333	399.9	0.282	371.4
Adj R-Squared	0.197	0.745	0.216	0.787
F-stat	7.456	7.363	4.701	13.83

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas.

Table 4: Leave-out Settlement, Duration, and Pending Status

	(1) Pending Plaintiff	(2) Duration (Days) Plaintiff	(3) Pending Defendant	(4) Duration (Days) Defendant
Settled (leave-out)	-0.344**	-17.40	-0.738**	-94.95
	(0.161)	(70.55)	(0.322)	(136.0)
Observations	1470	1420	235	216
No. Judges	30	30	30	30
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Case-Type FE	Y	Y	Y	Y
Control Mean	0	678.0	0	443.8
Control SD	0	438.5	0	413.1
Adj R-Squared	0.188	0.696	0.315	0.763
F-stat	4.540	0.0608	5.269	0.488

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas.

Table 5: Probability of Settlement by Settlement-Propensity of Assigned Judge

	(1) Case Settled Plaintiff	(2) Case Settled Plaintiff	(3) Case Settled Defendant	(4) Case Settled Defendant
Settled (leave-out)	0.722***	0.605***	0.789***	0.851***
	(0.0541)	(0.0885)	(0.124)	(0.116)
Financial Firm x Settled (leave-out)		0.103		-0.243**
		(0.0751)		(0.103)
Financial Firm		0.106**		0.0351
		(0.0399)		(0.0868)
Observations (cases)	1470	1470	235	235
No. Judges	30	30	30	30
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Case-Type FE	Y	Y	Y	Y
Control Mean	0	0	0.00781	0.0244
Control SD	0	0	0.0884	0.156
Adj R-Squared	0.368	0.381	0.384	0.382
F-stat	178.1	114.9	40.30	34.52

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas.

Table 6: Causal Effects of Settling Legal Cases on Firms

	(1)	(2)	(3)	(4)
	Profit (SD units) Plaintiff	Profit (SD units) Plaintiff Post Filing Periods Only	Profit (SD units) Defendant	Profit (SD units) Defendant Post Filing Periods Only
Post Decision=1	-0.536**	-0.401*	-0.135	-0.332
	(0.233)	(0.221)	(0.234)	(0.194)
Post Decision=1 × Leave-out Settled	0.865***	0.727***	-1.109***	0.193
	(0.251)	(0.220)	(0.382)	(0.463)
	{p=0.003}	{p=0.002}	{p=0.011}	{p=0.68}
Observations	2910	939	2051	665
No. Judges	19	19	21	21
No. Firms	71	67	56	51
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Mean Dep Var	0	0	0	0
SD Dep Var	0.988	0.988	0.987	0.987
Adj R-Squared	0.146	0.305	0.0458	0.312

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas matched with firm-level quarterly balance sheet data. Columns 1 and 3 include firm-level data from 2011 (prior to the Commercial Courts Act, 2015), with post period denoted as quarters following the decision date of a case (decision date for counterfactual is the date when the case is resolved through full trial or continues to be pending outcome). In the event when firms have multiple cases, we use the earliest resolved case as the reference. Columns 2 and 4 subsets the data to include time periods (quarters) only after the date when a case is filed, with post period defined as previously. Standard errors are clustered by assigned judge. Bootstrapped p-values in {.}.

Online Appendix

A Data and Variable Construction

A.1 Data Sources and Preparation

The analysis combines three primary data sources: (1) case records from Delhi and Karnataka courts, (2) judicial assignment records, and (3) corporate financial statements from the Prowess IQ database. We implemented a multi-stage cleaning and merging process to construct the final analytic dataset.

A.2 Case Record Processing

The raw case data (`Delhi_Kar_CC`) underwent extensive cleaning to standardize party names and case outcomes. We removed non-alphabetic characters, standardized legal entity suffixes (e.g., converting "LIMITED" to "LTD"), and created indicators for corporate parties using a dictionary of common business terms supplemented by manual review. Case dispositions were categorized into six mutually exclusive outcomes: ALLOWED, CONTESTED, DISMISSED, ORDERED, SET ASIDE, and SETTLED, with spelling variations normalized through automated and manual corrections.

A.3 Judge Data Merging

We matched cases to judges using court identifiers and hearing dates, ensuring temporal alignment with judicial tenures. The merge accounted for judge transfers between courts by verifying assignment periods against official records. For Karnataka courts, we implemented a court-by-court matching procedure (courts L32-L39) before combining results, while Delhi cases were appended after processing. This yielded a judge-case panel covering all observed dispositions.

A.4 Firm Identification and Matching

Corporate parties were identified using a combination of:

- Regular expressions for common business suffixes
- Manual review of entity names
- Fuzzy string matching (Jaro-Winkler similarity > 0.9) with corporate registries

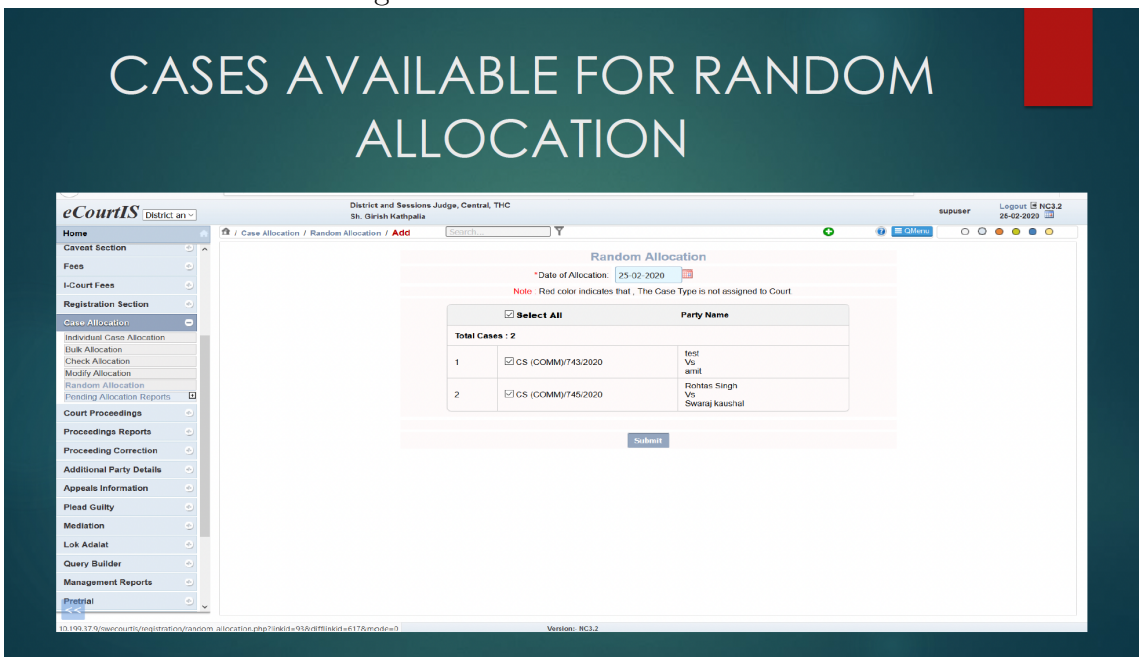
The matching process successfully linked 476 unique firms (249 petitioners and 227 respondents) to their financial records. We verified matches through manual checks of a random sample (10% of matched cases), finding 98% accuracy in entity identification.

A.5 Financial Data Integration

Balance sheet information was merged using unique company identifiers (`co_code`), with quarterly financials aligned to case timing. We retained only exact matches between legal records and financial data, yielding eight complete datasets (petitioners/respondents $\times$ income/expenses/assets/capital). Financial variables were inflation-adjusted using RBI price indices and winsorized at the 1st/99th percentiles to mitigate outlier effects.

B Sample Size

Figure A1: Random Allocation



Notes: Screenshot from `eCourtIS 3.0`

Table A1: Case Type Summary Statistics

Distribution of Case Types

	(1) Full Sample	(2) Petitioner	(3) Respondent
Execution	0.232	0.208	0.086
	(0.422)	(0.406)	(0.280)
Appeals	0.099	0.059	0.070
	(0.299)	(0.235)	(0.255)
Original/New Suit	0.482	0.660	0.325
	(0.500)	(0.474)	(0.469)
Other	0.187	0.073	0.520
	(0.390)	(0.260)	(0.500)
Observations	16,098	3,417	502

Notes: Panel A reports percentages of litigant-type pairs in the data. Col 1 presents the distribution across the universe of commercial cases from the two courts in our study. Col 2 presents the distribution among a subset of these cases where we identify the plaintiff as a firm (either a bank/financial firm or non-financial firms). Col 3 presents the distribution among another subset where we identify the respondents as a firm (bank/financial or non-financial firms). Panel B shows mean values with standard deviations in parentheses. Panel C presents firm characteristics.Standard deviations in parentheses

Table A2: Robustness: Causal effects on firms using last case filed

	(1)	(2)	(3)	(4)
	Profit (SD units) Plaintiff	Profit (SD units) Plaintiff Post Filing Periods Only	Profit (SD units) Defendant	Profit (SD units) Defendant Post Filing Periods Only
Post Decision=1	-0.102	-0.347	-0.0150	-0.286
	(0.196)	(0.216)	(0.185)	(0.222)
Post Decision=1 × Leave-out Settled	0.422	0.598**	-1.235**	0.420
	(0.249)	(0.241)	(0.471)	(0.602)
	{p=0.084}	{p=0.059}	{p=0.012}	{p=0.503}
Observations	2910	939	2051	665
No. Judges	18	18	23	23
No. Firms	71	67	56	51
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Mean Dep Var	0	0	0	0
SD Dep Var	0.988	0.988	0.987	0.987
Adj R-Squared	0.138	0.299	0.0429	0.308

Standard errors in parentheses

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas matched with firm-level quarterly balance sheet data. Columns 1 and 3 include firm-level data from 2011 (prior to the Commercial Courts Act, 2015), with post period denoted as quarters following the decision date of a case (decision date for counterfactual is the date when the case is resolved through full trial or continues to be pending outcome). In the event when firms have multiple cases, we use the last resolved case as the reference. Columns 2 and 4 subsets the data to include time periods (quarters) only after the date when a case is filed, with post period defined as previously. Standard errors are clustered by assigned judge. Bootstrapped p-values in {.}.

Table A3: Firm-Level Effects: Events as Filing and Decision of a Case

	(1) Profit (SD units) Plaintiff Placebo	(2) Profit (SD units) Defendants Placebo	(3) Profit (SD units) Plaintiff All Periods	(4) Profit (SD units) Defendants All Periods
Post Filing Before Decision=1	-0.202	0.512**	-0.344*	0.414**
	(0.195)	(0.195)	(0.192)	(0.176)
Post Filing Before Decision=1 × Leave-out Settled	0.219	-1.578***	0.390	-1.681***
	(0.252)	(0.483)	(0.266)	(0.414)
	{p=0.49}	{p=0.002}	{p=0.14}	{p<0.001}
Post Decision=1			-0.336	-0.380*
			(0.260)	(0.209)
Post Decision=1 × Leave-out Settled			0.588*	0.362
			(0.321)	(0.389)
			{p=0.12}	{p=0.485}
Observations	2363	1661	2910	2051
No. Judges	19	21	19	21
No. Firms	70	56	71	56
City FE	Y	Y	Y	Y
Registration-Month FE	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Mean Dep Var	0	0	0	0
SD Dep Var	0.988	0.987	0.988	0.987
Adj R-Squared	0.0676	0.0657	0.148	0.0582

Standard errors in parentheses
* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Notes: Sample includes cases that were randomly assigned to judges in commercial courts in the two cities/metropolitan areas matched with firm-level quarterly balance sheet data prior to the case decision. The time periods include quarters from 2011 (prior to the Commercial Courts Act, 2015), with post period denoted as quarters following the filing date of a case (filing date for counterfactual is fixed by construction). We only include firm-level data before the resolution of their case in order to observe if there are "trends" in the outcome prior to case resolution either through settlement or full trial. Standard errors are clustered by assigned judge. Bootstrapped p-values in {.}.