Front-line Courts As State Capacity: Evidence From India

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Well-functioning courts are fundamental to building state capacity. Yet, they are severely underinvested with potential implications for economic development. Using a novel panel data on front-line district courts in India, constructed using the universe of legal case records, and an event study research design, I show that changes in judge staffing levels substantially affect judicial capacity: Each additional judge per court resolves 200 pending cases and increases courts' case backlog resolution rate by 10 percent. In a context with high levels of backlog, judicial capacity improvement releases assets, including banks' lending capital stuck in litigation, for productive uses. Moreover, local formal sector firms experience higher productivity through working capital expansion and lower interest expenditures. Cost-benefit analysis suggests that tax revenue and economic gains are 6 and 30 times higher, respectively, than the personnel cost of hiring an additional judge. (*JEL* O16, O43, K41, G21)

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1 Introduction

Courts play a central role in enforcing contracts and property rights, which support the development of formal sector, investment, and economic growth (La Porta et al. 1998; Djankov et al. 2003). Long lags in dispute resolution due to congested courts can increase uncertainty and transaction costs that impede effective contracting and weaken *de facto* property rights (Johnson et al. 2002; Laeven and Woodruff 2007; Ponticelli and Alencar 2016; Sadka et al. 2018). Despite this, courts are chronically underinvested in developing countries, reflected in the low judge-population ratio and the enormous pending case backlog that are many times higher compared to high income countries. For example, India has 5 times fewer judges per capita and 10 times higher backlog per available judge relative to similar county courts in the United States.[1] Estimating the returns to augmenting judicial capacity is therefore a first order question for both research and policy.

This paper studies the economic impact of changes in judge staffing-levels in district courts in India by leveraging a first of its kind court-level panel data. District courts in India are front-line courts that are citizen-facing, have the largest caseload (44 million cases), substantial vacancies (25%), and have the highest pending backlog (over 7 times higher than any other type of courts). I show that increasing judge staffing-levels in such courts substantially reduces pending case backlog and enhances the productivity of local formal sector firms, with indications of broad-based improvements in the local economy. Improved judicial capacity helps release and recirculate valuable assets, such as bank credit, which are otherwise stuck in litigation for long periods of time. The overall economic returns are large and rapid, occurring within a short timescale of 2-3 years from the time of staffing-level increase.

For causal identification, I leverage plausibly exogenous variation in the timing of judge staffing-level changes across 195 district courts between 2010 and 2018. The staffing variation results from a combination of recruitments, retirements, rotation of judges between district courts, and their interaction with one another. These changes at the district court-level are driven by centralized policies on retirement at 60 years of age, sporadic and often failed recruitment drives, and frequent rotation of judges between district courts, which are executed at the state-level, a level above the district courts themselves. As a result, the net effect on judge staffing levels at any given point in time and in any given court is plausibly exogenous.[2] I measure the timing variation using the case-level time-stamps from

[1] All judicial statistics in this paper are based on data from the National Judicial Data Grid for India and respective state and federal court websites for the United States. See Online Appendix Appendix A.1 for details on national-level summary statistics and cross-country comparison.

[2] This is also confirmed by informal interviews with several state-level and district-level judges in India,

the universe of legal cases in this period and estimate the effects by employing a stacked event study design (Cengiz et al. 2019). This design accounts for dynamic and heterogenous treatment effects (Sant'Anna and Zhao 2020, Sun and Abraham 2021) and is particularly well-suited to the setting of this study where the changes in staffing-levels occur more than once and are bi-directional, comparing courts experiencing a positive (or a negative) change with those experiencing no change. I show that the timings of these changes are unrelated to existing court backlog and other dynamic socio-economic and political conditions, including investments/budgetary allocation by government or the private sector. Further, I find no significant trends in the prior period across key outcomes and potential confounders as a support for the assumptions for causal identification. I also estimate the impact by employing generalized difference in difference (DiD) research designs (Schmidheiny and Siegloch 2020; Freyaldenhoven et al. 2021; Dube et al. 2022) to find qualitatively similar results.

Given the jurisdiction of district courts and the types of cases that are litigated in such courts, there are two likely economic mechanisms: (a) debt contract enforcement, which is particularly relevant for the recovery of bank capital stuck under litigation, affecting local credit supply, and (b) protection of property from thefts and embezzlement that enable economic agents to safeguard their stock of raw material, inventory, and capital goods. I estimate the productivity implications using a balanced panel sample of locally registered, tax-paying firms as well as using broader, district-level economic outcomes. Local judicial capacity can particularly affect formal sector firms because they often borrow from local branches of banks for their working capital needs (Nguyen 2019; Breza and Kinnan 2021; Bau and Matray 2023) and seek protection from property and financial crimes (Bandiera 2003).[3] Further, banks are required to use the formal judicial system to resolve any debt recovery disputes and thus, their lending decisions could be tied to how well-functioning their local court is. In addition, banks already have a lot of their lending capital under various on-going litigation, which get released once courts order settlement. These may consequently affect local firms' access to and cost of credit in the presence of frictions in the financial markets (Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012; Bau and Matray

which I describe in detail in Section 2. In particular, the bureaucracy of the judiciary is unique wherein many different institutions - the judiciary, the executive, or the elected representatives - have to coordinate to implement the judge staffing policies. This is unlike the much studied context of general administration bureaucrats where elected representatives play a central role (Iyer and Mani 2012; Khan et al. 2019). This further adds to the uncertainty in the timing of these changes.

[3]All firm-level data are from CMIE Prowess database, 2018, which is a representative sample of the formal sector, including the universe of listed firms, in India. I merge firms' annual balance sheet data with the court-level dataset by mapping the firms' district of registration to the corresponding court's jurisdiction. This mapping also follows the code of legal procedure that defines the location of dispute resolution. I also complement firm-level analyses by employing various sources of district-level data to examine district-level economic outcomes that I describe in detail in Section 3.

2023; Bazzi et al. 2023). Finally, an improvement in the crime environment through better protection of property could further improve firms' productivity gains.

I document three key results. First, I find a significant effect on court-level outcomes when there is a net increase in judge staffing-levels relative to when there are no changes. These include a persistent effect on reducing vacancies with two additional judges per court. Correspondingly, I note a sustained increase in the number of case resolutions by 200 cases per judge, and an increase in the court-level backlog reduction rate (disposal rate) by 20 percent (2-3 percentage points) each year following the change. These effects are immediate and sustain over the long run. On the other hand, negative staffing-level changes have roughly half the effect size in reducing the staffing levels, and thus have commensurately smaller effects on disposal rate.[4]

Second, local firm-level productivity improves following net increases in staffing-levels and decreases following net reductions. Specifically, firm-level wage bill and profits respond significantly: the average wage bill increases by around 5% in the long run when more judges are added. The effect on profit is over 40%, reflecting both productivity and accounting improvements (such as through a reduction in interest expenditures). On the other hand, a decrease in the number of judges has a negative effect: wage bill contracts by around 2% and profits drop by 20%. Since the net negative change in staffing-levels is lower in magnitude by a half, the effects on productivity measures are symmetric per-judge. These effects are significant economically since the sample of firms are among those contributing a large share of value addition and employment in India. Further, these effects appear with a lag, consistent with a natural lag where the firms' optimization follows changes in credit market and monitoring costs.

Third and related to the economic mechanism, I note an immediate increase in the resolution of bank-related cases in courts following net judge addition. At the market (district)-level, I find an increase in aggregate lending by banks to industrial borrowers. These also correspond with an increase in firms' working capital and a reduction in interest expenditure. Further, I note similar credit and productivity effects among smaller firms with lower ex-ante debt exposure (measured using leverage ratio), suggesting extensive margin increases in bank lending to smaller firms with previously low-levels of borrowing. Moreover, the credit effects are much stronger in explaining firms' productivity gains than alternate mechanisms such as improved protection of property from crime.

I take a number of precautions and perform different robustness checks to verify the

[4]This non-symmetry likely arises from different realizations of staffing changes resulting from the interplay between recruitment, rotation, and retirement. Recruitments often are lumpy in contrast to retirements, which depend on the age of senior-most judges.

results. First, I use a balanced panel of incumbent firms to ensure internal validity, which could be threatened by endogenous entry or exit of firms. Second, I verify that the firm-level results using this balanced panel are not driven by changes in the composition of the population of firms that could change the competition environment in the districts. Changes in the composition could generate an upward bias in the estimation of the regression coefficients, which could lead to erroneous inferences of positive treatment effects. In contrast, I find suggestive evidence that improved judicial staffing results in greater firm entry, increasing the total number of firms within the district. This could imply better business dynamism and increased competition. As a result, changing composition of firms could instead generate a downward bias, underestimating the impact. Third, I find that the effects are also seen among the subset of firms with no legal cases across the entire study period. This supports the fact that the estimates capture beyond any immediate effects due to case resolution for the litigating firms. Finally, I note that the effects are only observed among local firms and not among firms in the neighboring districts where the district court has no jurisdiction. More broadly, I find suggestive positive effects on district night light intensity following net addition and negative effects following net reductions in staffing-levels.[5]

These findings highlight substantial economic gains generated by strengthening staffing levels in the front-line judiciary. A back of the envelope calculation of the benefit-cost ratio shows large returns. I measure benefits accruing to the sample firms (through taxable corporate profit) and their employees (through taxable wages), and the personnel costs of an additional judge following the recommendations from the Second National Judicial Pay Commission. The calculations suggests that adding one more judge can generate over 6 times net tax revenue, considering even the most conservative estimates. The social return is orders of magnitude higher. These estimates are likely a lower bound considering that an improvement in judicial capacity could generate many other benefits not examined in this analysis and the cost estimates are higher than actual expenditures.

This paper makes several contributions. First, the findings in this paper underscore the importance of general courts of law for local economic development through an expansion of formal sector economic activity. In this regard, this paper provides evidence on the micro-foundations connecting legal institutions and economic growth (La Porta et al. 1998; Djankov et al. 2003; Johnson et al. 2002; Laeven and Woodruff 2007; Nunn 2007) and complements the literature on courts and development (Chemin 2009a,b, 2012; Ponticelli and Alencar 2016; Amirapu 2017; Kondylis and Stein 2018; Boehm and Oberfield 2020; Mattsson and Mobarak

[5]I also find similar effects qualitatively when using the larger, unbalanced panel of firm-level data. However, I find that data for many variables are missing non-randomly - that is, data reporting is correlated with judge staffing changes. Since the unbalanced panel could produce unbiased estimates of the causal effect with no insights on the direction of the bias, I abstain from using it for the main analysis.

2023). Specifically, I use a unique, at-scale court-level panel dataset constructed from the universe of legal records, which has hitherto not been exploited in any context. This data and the context of the study allow me to leverage a more credible identifying variation in the timing of personnel capacity changes to examine the fundamental role played by local courts in shaping market transactions and facilitate access to resources for economic production.

Second, this paper documents the role of local, front-line courts in the development of financial sector in a context of weak institutions. On the one hand, faster and efficient debt recovery has been a focus of many economic policies often leading to the creation of specialized courts that have been extensively studied (Visaria 2009; von Lilienfeld-Toal et al. 2012; Lichand and Soares 2014; Ponticelli and Alencar 2016; Müller 2022). On the other hand, general courts of law are a core component of the state and the final authority on contract enforcement, and we know very little about how these institutions work. This paper adds to the literature on the importance of financial sector for economic development (Rajan and Zingales 1998; Burgess and Pande 2005; Castellanos et al. 2018; Breza and Kinnan 2021; Bau and Matray 2023; Bazzi et al. 2023) by showing that local courts facilitate credit circulation in the presence of financial market frictions (Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012; Castellanos et al. 2018; Rigol and Roth 2021).

Third, this paper demonstrates that investment in front-line courts generates large and rapid returns, strengthening state capacity (Besley and Persson 2009). This contributes to the evidence on program implementation and bureaucrat performance (Dal Bó et al. 2013; Muralidharan and Sundararaman 2013; Coviello et al. 2015; Khan et al. 2015; Muralidharan et al. 2016; Lewis-Faupel et al. 2016; Neggers 2018; Banerjee et al. 2020; Dasgupta and Kapur 2020; Ganimian et al. 2021; Fenizia 2022; Narasimhan and Weaver 2023; Mattsson and Mobarak 2023) by studying staffing constraints in the judiciary. This paper provides the first estimates of the benefit-cost ratio of improving local judicial capacity by using direct measures of economic outcomes - wage bill and firm profitability.[6]

The rest of the paper is organized as follows. I document the context on judicial organization structure and how this interacts with local credit markets in Section 2. I describe the data in Section 3, empirical strategy and threats to identification in Section 4, and summarize the results with respect to an economic framework in Section 5 and Section 6. I discuss the broader implication in Section 7 and conclude in Section 8.

[6]Among the existing literature, Ganimian et al. (2021) compute a benefit-cost ratio, albeit using strong assumptions linking childhood learning and health outcomes to lifetime increase in wages among treated pre-school children.

2 Context

The judiciary in India is a three tier unitary system: district courts, where the bulk of cases begin, report to state-level High Courts, which are overseen by the Supreme Court of India. High Courts and the Supreme Court are appellate courts, with the exception of constitutional disputes or disputes concerning interstate commerce. In this paper, I examine the functioning of district-level general courts of law, which are often the first interface of the judicial system. Specifically, I study the District and Sessions Court, hereinafter called district court, which are similar to county courts in many common law countries (see Figure A.3 for an example of a legal case in such courts). These are courts of first instance for many types of legal disputes, across civil (for e.g., property or debt-related disputes), criminal (ranging from violent crimes to lower-order property and financial crimes), and commercial (for e.g., enforcing regulatory laws, contractual disputes) issues. There is one district court per administrative district, which also correspond to the geographic location of the dispute.

Due to separation of powers, the judiciary has to coordinate with both the executive and the legislature for its effective functioning. While the judiciary alone manages its organization structure and sets internal policies, it relies on the executive for budgetary approvals and funding, and the legislature for laws, including amendments to procedural codes. Coordination failures underpin many of the constraints in expanding judicial capacity.

One such key constraint that I examine in this paper is inadequate judge staffing levels that the judiciary alone is unable to address. The number of judges relative to India's population is perhaps one of the most critical constraints. On average, there are 20 authorized judge posts per million. In contrast, there are close to 100 judges per million in the United States and close to 200 per million in the European Union. This ratio is further reduced when we account for the extent of vacancies.

The total number of judge posts in a district is determined jointly by the respective state high court and the state-level executive (through budget allocation). There is no clear rule on how the number of judge posts is determined. Periodic reports by the Law Commission of India, an executive body under the central government Ministry of Law and Justice (particularly, the Law Commission Report No.245), point out that this is relatively ad hoc without any specific calculus. Typically, the numbers are determined at the time of district formation and depend on the district population count from the most recent decadal census. These numbers are rarely updated over a shorter time scale, including the scale of the study time period. Figure A.1 (Panel A) shows a strong, albeit imperfect correlation

between district population and the number of judge posts.[7]

The judiciary also faces persistent vacancies. About a quarter of judge posts in district courts are vacant, which have continued or worsened over the years (Panel A Figure A.1). Though vacancies are natural as judges reach retirement age, they persist or worsen if recruitment does not catch up with the extent of turnover. Addressing vacancies in district courts requires close coordination between the judiciary and the state-level executive, particularly to organize and implement recruitment drives. These are implemented sporadically, with varying success rates.

Personnel policies such as judge tenure and assignment to courts are handled by the state-level high courts. District judges are state officials appointed by the corresponding high court. They are senior legal professionals, who are either inducted from the local bar council or promoted from sub-district courts. A few are directly hired through competitive exams. They typically serve 10-15 years before retiring, unless promoted to the state high court, if at all. These judges serve a short tenure in any given court - 2-3 years, and are either rotated (reassigned) to a different district court or retire from the court where they turn 60 years in age during their tenure.

The specific assignment process for allocating judges is based on a seniority-first serial dictatorship mechanism, subject to non-repeat and no home district assignment constraints. Judges are asked to list 3-4 rank-ordered district court locations for their next posting subsequent to completing their tenure at their existing location. The assignment process is as follows: first, the senior-most judge is assigned their top ranked location, followed by the second senior-most judge (as long as it does not conflict with the more senior judge), and so on. In case of conflict, the assignment moves down the ranking order of the more junior judge. Finally, newly recruited judges are assigned randomly to courts with vacancy, subject to the home district constraint.

Thus, three personnel policies - recruitments, retirements, and rotation between courts - determine the net effect on the judge staffing levels in a given court at a given point in time. This interaction could result in either positive, negative, or no net changes in the number of judges in a particular year for a court. Figure A.2 presents a schematic to show this dynamic and how this affects judge staffing levels in a court over time.

[7]The Law Commission Report No. 245 recommends an algorithm to determine the required number of judges in a court using data on existing workload and historical rates of case resolution. However, applying this rule to the data as well as discussions with key stakeholders suggest that these recommendations are rarely followed.

2A Courts and Bank Credit Circulation

Financial sector enterprises such as banks rely on district courts for executing debt contracts by enabling last resort recovery. A large majority of cases listed in district courts are summons requiring the appearance of the defaulting party in response to complaint filed by the aggrieved, including complaints by banks against individuals or firms for non-payment of debt. The quantum of total lending under default, when aggregated over multiple borrowers and loans, is substantial. In a specific example of total recoveries from pre-trial mediations facilitated by courts amounted to USD 240,000 per district in a single session held across district courts in India.[8] Judicial staffing support could expand the quanta of recoveries by facilitating faster settlement, which could improve liquidity within local financial institutions.

Banks mainly lend to borrowers through their local branches. This is done to minimize adverse selection and moral hazard where the branch-level officials play a key role in verifying borrower identity, credit needs, and repayment ability through periodic site visits and inspections. Not only was this confirmed during qualitative interviews with a sample of bank managers and their legal counsels but literature has also documented this to be a standard practice in the banking industry worldwide (for example, Nguyen 2019 describes a similar lending system in the US and Bazzi et al. 2023 in the context of Brazil). This co-location requirement with the borrower is important in the context of this paper irrespective of whether the borrower is a firm or a private individual. For enterprise borrowers, this coincides with their registered office, whereas in the case of individuals, this corresponds to their verifiable residential location. Cross-district borrowing relationships are not common, and plausibly does not occur at all, suggesting deeper frictions in the financial markets as also discussed in Bau and Matray 2023. This makes the local contract enforcement environment critical for credit markets to function efficiently.

2B Courts and Law Enforcement

The district courts are general courts of law, with jurisdiction over criminal disputes as well. Capacity of these courts are also important for containing crime, which in turn could affect economic productivity. While police play a more direct role in containing violent crimes, the bulk of crimes are typically non-violent, and concern property crimes such as thefts, where the functioning of courts are more important by imposing fines and helping recover stolen property. Relatedly, a large bulk of criminal cases in district courts are what are known as "summary trial" cases. A few examples of these according to the Code of Criminal Procedure are (a) "Offense of theft, under section 379, section 380 or section 381 of the Indian

[8]Data on recovery accessed from https://nalsa.gov.in/statistics.

Penal Code, 1860, where the value of the property that has been stolen does not exceed two thousand rupees.", (b) "Offenses relating to receiving or retaining stolen property, under section 411 of the Indian Penal Code, 1860, where the value of the property does not exceed two thousand rupees.", and (c) "Offenses relating to assisting in the concealment or disposal of a stolen property, under section 414 of the Indian Penal Code, 1860, where the value of such property does not exceed two thousand rupees." The monetary value may be updated from time to time through amendments to procedural law, but the main import is that a large bulk of criminal cases pending in district courts pertain to protection of property from thefts and embezzlement. This likely has implications on monitoring costs for local firms in securing and protecting their property, particularly movable property like raw material and inventory that could be pilfered.

3 Data and Sample Construction

Court-level Variables I assemble 6 million public legal case records from the E-Courts database, spanning the universe of all legal cases filed or pending for resolution between 2010 and 2018, from a sample of 195 district courts across 15 states in India (Panel B Figure A.1).[9] Each record details the case meta-data including detailed timestamp information over the case lifecycle. Additional details in the meta-data includes the courtroom number and the judge designation where a case has been assigned, as well as litigant and case type details.

Leveraging the fact that the data represents the universe of legal cases between 2010 and 2018, I enumerate judges within a court over the study period based on annual workflow observed for a given courtroom generated from the timestamp information. I define annual workflow as follows: I record a courtroom as active (i.e., with a judge) for a given calendar year if I observe newly filed cases in that year assigned to that courtroom. A court registrar assigns new cases to all incumbent judges (who have assigned courtrooms) immediately after filing and verification of a plaint. When an incumbent judge moves (either due to rotation or retirement) with no replacement, that specific courtroom remains vacant and no new cases are assigned to the courtroom. The existing workload at the time of vacancy is transferred to other remaining judges in the court.

Using this algorithm, I generate the number of judges in a district court for each year in the study period. I also calculate vacancy rate as the relative shortfall in the number of judges in a given calendar year relative to the maximum number of observed judges in the court within the study period. Using annual judge count, I define a positive staffing change

[9]These districts were selected to ensure an overlap with the location of registered formal sector firms across industrial districts and are representative of other similar districts.

event as years when the number of judges increases relative to the previous year. Similarly, a negative change event is defined as the year when the number of judges declines relative to the previous year. From this definition, a court could experience multiple positive or negative change events, or none at all.[10]

Lastly, I construct court-level annual performance variable - rate of backlog resolution or disposal rate, as the percentage of total workload, which includes pending and new legal cases, that are resolved in a calendar year. This measure is strongly correlated with other possible measures of court performance such as case duration or appeal rates (see Table A.1 for pairwise correlations between the different measures). I also focus on case disposal rate and case-specific outcomes for legal cases involving banks.[11]

Firm-level Variables I use CMIE-Prowess dataset that includes annual balance sheet data of the universe of listed firms and a representative sample of unlisted but registered formal sector firms. This dataset has three important features: First, it contains detailed identifying information of firms, including firm name and registered office location, which allows me to match with the court-level panel dataset by location. Second, these firms report production and accounting data every year in their balance sheets, which is useful given the annual time-scale of the identifying variation. Third, the formal-sector firms account for $\approx$ 40% of sales, 60% of VAT, and 87% of exports (Economic Survey, 2018), and therefore captures a large share of value addition in the economy.

There are two important sectors of firms relevant for the analysis. First is the non-financial sector, including manufacturing, services, and trade, engaged in value added production. Of the 49202 firms in the CMIE-Prowess database in 2018, 9032 non-financial sector firms are located with their registered office in 157 of the 195 sample court districts. Remaining 38 district courts result in no match. Using this firm-level data, I also compute other measures of economic outcomes, including the total number of firms as well as the number of new incorporations per district during the study period. This enables me to examine

[10] An ideal dataset would be the personnel records of all judges serving in the sample courts over time. However, such a dataset does not exist. I verify that constructing staffing levels from the case-records generate similar aggregate estimates as reported in official reports that document staffing and vacancy measures at the state or national levels (see Panel C Figure A.1). Since the e-courts system directly records daily court proceedings on a digital platform, it minimizes bureaucrat intervention in entering data. This addresses the data quality issues with bureaucrat-reported administrative data to a certain extent (Singh 2020; Muralidharan et al. 2021).

[11] Court workload includes both pending as well as new cases, which is around 20000 cases per district court. Resolved cases also include those that are dismissed without full trial or a final judgement order. Disposal rate is a relevant metric of judicial capacity relative to average or other moments of case duration that necessarily have a selection component in what cases are resolved. Focusing on disposal rate is also important from the point of view of the volume of tied-up factors of production. While case duration may matter for individual litigant directly involved with the judicial system, annual performance indicators such as the disposal rate measures the extent of congestion and is more appropriate metric of institutional capacity.

impacts over extensive margin as well as analyze compositional changes in the set of firms over time. To measure productivity improvements on the intensive margin and preserve the internal validity for causal inference, I restrict the analysis sample to a balanced panel of 393 incumbent, non-financial firms, incorporated before 2010. I use profit, sales revenue, wage bill, the value of capital goods (plants and machinery), and raw material expenditures as key outcomes. To examine one of the main mechanisms on improved credit access, I use working capital to measure the financing of firms' operating costs and total interest expenditure across borrowings.

Second sector that is pertinent to the economic mechanism is the banking sector. I use district-level annual banking statistics by India's central bank (Reserve Bank of India or RBI), focusing on total lending to the industrial sector as the key variable. This dataset only contains total number of loans and amount outstanding, with no differentiation between paid and unpaid/defaulted debt. As a result, I only focus on the number and not the value of credit at the district level. I verify the value and cost of credit at the firm-level.

Other district-level outcomes To explore effects on local crime, I use district-level reported crime statistics by National Crime Records Bureau (NCRB). Crime reporting is classified into serious crimes such as murders and homicides (violent crime), and other crimes, which mainly include small-valued thefts and property crimes.

Lastly, I use nightlights data - Visible and Infrared Imaging Suite (VIIRS) Annual VNL V2.1 by the Earth Observation Group, and compute the pixel average within the district boundary as a proxy for overall district-level economic development.

3A Summary Statistics

Panel A of Table A.2 presents summary statistics for the court variables. On average, there are 18 judge posts per district court, with 23 percent vacancy. Over 2010-2018, courts experience 1.62 positive staffing changes with 2 judges added on average and 3.6 negative staffing changes with 3 judges removed on average. Of 195 courts in the sample, 158 experience at least one positive event whereas 37 courts experience no net judge addition over the study duration. On the other hand, every court experiences at least one negative event during the study period.

Average court-level backlog disposal rate is 14 percent of total workload and average case duration is 420 days (right-tailed distribution with a standard deviation of 570 days). I focus on disposal rate to measure court-level performance, which avoids selection concerns by including all cases in contrast to case duration that only includes resolved cases.

Panels B and C describe district and local firm-level outcomes. On average, banks issue over 9000 loans per year to the industrial sector, with about USD 4.2 million (INR 310

million) in circulation (outstanding amount) within the sample districts. The sample of firms include large firms, with USD 103 million (INR 8.4 billion) in average sales revenue and USD 4.5 million (INR 371 million) in average profits. All financial variables are reported in million Indian Rupees and are adjusted for inflation using Consumer Price Index (base year = 2015).

4 Research Design and Empirical Strategy

As detailed in Section 2, judge staffing levels in a court change frequently due to addition and/or removal of judges resulting from recruitments, periodic rotations/reassignments, and retirements. While judges are not randomly assigned to courts, the different policies on staffing affect the net changes in the staffing-levels in a given court at a given time. Thus, central to my identification strategy is that the *timing* of the judge staffing-level changes in district courts is plausibly random.

I employ a heterogeneity-robust event study design to account for the multiplicity and bi-directionality of the staffing-level changes that I describe in detail in this section. I use positive staffing-level changes to draw inferences on the causal effect of judicial staffing improvements and negative changes for the effect of staffing-level reductions. I include unit and state-year fixed effects to address space and time-invariant potential confounders. These fixed effects would also absorb any political and/or business cycle confounders that could bias the causal effect estimation.

4A Stacked Difference in Differences Event Study

With a one time, albeit staggered, change in district courts' staffing levels (number of judges), the causal effect parameter could be estimated using recent dynamic difference in difference estimators that correctly account for dynamic treatment effects and treatment effect heterogeneity across groups and cohorts (Sant'Anna and Zhao 2020, Sun and Abraham 2021). However, in the context of this paper, district courts experience multiple staffing changes, and in opposing directions, over the study period. My preferred empirical strategy takes into account this multiplicity of events, occurring in different years across district courts, by stacking separate datasets generated for each district-event (this strategy follows Cengiz et al. 2019 that examines the effect of multiple minimum wage revisions on employment distribution in the context of the United States). The dataset for an event e within a district d is centered around one period prior to the event with relative annual event-time bins and an effect window of 4 years in lead and lag. I bin the end points by clubbing all the years in the

dataset outside this effect window. Binning of the endpoints accounts for any plausible effects outside the effect window, thus also capturing long-run effects of staffing-level changes. I append all such district-by-event datasets to generate a stacked dataset for analysis, with each event indexed by an event number. Districts with no staffing changes are included in the stack once.

In order to distinguish a positive staffing-level change from a negative change, I modify the standard stacked event study design by including both directions of staffing changes within the same specification as shown in Equation 1. Specifically, I create respective binary variables - Pos_{de} and Neg_{de} - and interact these with the event time bins.

$$\begin{aligned} y_{it} &= \sum_{j=-4-, j\neq -1}^{4+} \beta_j^{+} \mathbb{1}\{|t - T_{d,e}| = j\} \text{ x } Pos_{d,e} + \sum_{j=-4-, j\neq -1}^{4+} \beta_j^{-} \mathbb{1}\{|t - T_{d,e}| = j\} \text{ x } Neg_{d,e} \\ &+ \alpha_i + \alpha_e + \alpha_{st} + \epsilon_{it} \end{aligned} \tag{1}$$

where y_{it} is the outcome of either the court or local firm, indexed by i. The specification accounts for unit fixed effect (i.e. district or firm fixed effect), event fixed effect, and state-year fixed effect. The choice of the effect window (from $t-4$ to $t+4$) incorporates the maximal tenure length of a judge in a court - a typical judge spends 2-3 years in one district court - before being reassigned to a different court. Any new vacancy takes about 3-4 years to be converted into an open position for recruitment. Thus, the effect window incorporates any immediate impact of staffing-level changes - for example, on fast moving outcomes such as court-level performance measures - as well as delayed impact that would require persistence of staffing levels to generate market-level or general equilibrium effects within the jurisdiction of the courts.

The treated groups are courts with a net positive or a net negative change occurring in a specific calendar year (for e.g., a change occurring in calendar year $T_{d,e} = 2013$) relative to the previous year. The control group is the set of districts that don't experience any positive or negative change in the same year but could in the future. Since there are multiple events, the control group also includes the same district experiencing another positive and/or negative change in the future. 37 districts never experience positive staffing-level change (never-treated for net addition) whereas every district experiences a negative change at least once within the study period. The coefficients of interest are $\beta_{j\geq 0}^{+}, \beta_{j\geq 0}^{-}$ - coefficients on the event-time bins interacted with the positive or negative change dummies, normalized relative to $t = -1$ (the year prior to the corresponding event), representing the dynamic treatment effect of judge staffing changes. $\beta_{j<0}^{+}, \beta_{j<0}^{-}$, i.e. the coefficients on the interacted terms during the

pre-period enable testing for any significant pre-trends. For inference, I use two-way cluster robust standard errors, clustering by both district and event (Bertrand et al. 2004, Abadie et al. 2017).[12]

This estimation strategy, which modifies the standard stacked-event study specification, addresses potential SUTVA violation where a positive event may counteract a negative event happening elsewhere at the same time (for example, if a judge assigned to a court resulting in a positive staffing-level change is due to their departure from a different court, which experiences a negative change, this would lead to SUTVA violation in a standard event-study specification). The estimator in Equation 1 overcomes this challenge by including both positive and negative event dummies interacted with the event bins in addition to stacking all events per court. I test the estimator using simulated data with a known treatment effect to verify that the estimator recovers treatment effects without bias (see Figure A.7).

Threats to Causal Identification An ideal research design would require random assignment of judges to courts and random assignment of individual cases to judges. In the absence of this ideal variation, I exploit the next best variation, which leverages the timing of net changes in the judge staffing levels in a court conditional on unit-level and state-year fixed effects. Causal identification with this strategy requires the following assumptions: (a) exogeneity of timing, and (b) parallel trends, as the stacked estimator accounts for heterogeneous as well as dynamic treatment effects.

The main threat for causal inference is if the timing of staffing level changes are correlated, or even driven by local economic conditions. For example, if a district is identified as a priority region by the federal or state-level government to attract investments or relegated from such policy targeting due to changing political considerations, then the timing could be endogenous if such priorities also affect the judicial staffing levels. Alternatively, local business owners could lobby the government to assign more or fewer judges to their local courts in anticipation of growing their businesses. Further, judges themselves could lobby to be assigned to district courts that are more lucrative.

The context and results from empirical tests provide support for the identifying assumptions. First, the interplay between three different personnel policies (recruitment, retirements, and reassignments) concerning judges in district courts could have different consequences on the judge staffing levels at any point in time, generating plausible exogeneity in the timings of these staffing changes. Many senior judicial officers I spoke to said that lack of adequate candidates during recruitment and assignment drives, and routine retire-

[12]Clustering this way follows the research design where the "treatment" is assigned at a district-year level. For robustness, I also cluster by state and event in order to account for any spatial correlation between districts arising from state-level policies.

ments that occur as existing judges reach seniority as among the main challenges facing the judiciary. They said that these challenges have been persistent and is rarely influenced by elected representatives or changing local conditions. Consequently, if recruitment and/or reassignment into a court add fewer judges than their turnover either due to retirement or reassignment away, then the district court would experience a negative staffing change. Similarly, if recruitment and/or reassignment add more judges than their turnover from retirement or reassignment away, then the court would experience a positive staffing change. Finally, it is also possible that these forces cancel each other, resulting in no net change to the court staffing levels.

Second, I carry out several empirical exercises in the spirit of balance tests and check for differential trends in the periods prior to staffing level changes. To do this, I leverage multiple rounds of decadal population census, quinquennial economic census, and electoral data in the decade prior to the study period to test whether any of these could predict which districts are likely to experience judicial staffing-level changes in the future. I employ a long differences specification where I regress long-run changes in judge staffing levels (i.e. between 2010 and 2018) on decadal changes in population, number of establishments, employment in manufacturing, demographic composition (caste, literacy, and urbanization), and electoral outcomes as important determinants (i.e. as RHS variables). Table 2 presents the results. To aid easier interpretation of the coefficients, all dependent and independent variables are transformed into % changes relative to their baseline values (i.e., relative to the earliest period of data availability before the start of the study period). None of the individual coefficients are statistically significant nor do they jointly do well in predicting which districts are likely to experience staffing-level changes.

Lastly, I check whether the timing of staffing-level changes coincide with concurrent or past investments by the government and the private sector (for example, for infrastructure and/or new greenfield projects) in the sample districts. Any significant prior-period or immediate correlation could both violate the exogeneity assumption for identification and also affect inference whether the estimated treatment effects are truly due to staffing shocks or other concurrent changes. I find no significant pre-trends in either government or large private sector investments relative to the timing of staffing level change. If anything, the results suggest that the investments could increase much later in the long run as a consequence of improved capacity in front-line courts (see Figure A.8).

Generalized Difference in Differences Two important concerns still remain unaddressed with the above strategy: (a) absence of a never-treated group for negative events, and (b) potential interference between events within the study period. To address these con-

cerns, I supplement the main empirical strategy with a more generalized event study design by using the number of judges as a continuous-valued "treatment" by including leads and lags of the explanatory variable (following Freyaldenhoven et al. 2021) described in Equation 2 below. I bin the end points and normalize the event study coefficients relative to the year prior to the event(s) as before (and consistent with the generalized design as per Schmidheiny and Siegloch (2020)). The binning also relaxes the assumption of no treatment effects outside the effect window.

$$y_{it} = \sum_{j=-3}^{3} \delta_j \Delta x_{i,t-j} + \delta_4 x_{i,t-4} + \delta_{-4}(-x_{i,t+3}) + \alpha_i + \alpha_{st} + \xi_{it} \tag{2}$$

Δ is the first difference operator and the effect window spans 4 years in the lead and 4 years in the lag as in Equation 1. x_{it} is the number of judges in district i in year t. y_{it} is the unit-level outcome variable, where i refers to district when outcomes are at the district-level, or a firm when the outcomes are at the firm-level. The specification includes unit fixed effect and state-year fixed effect. I normalize using $t = -1$ such that the coefficients δ_j are relative to δ_{-1}. $x_{i,t-4}$ and $1 - x_{i,t+3}$ serve as the endpoints. For inference, I cluster standard errors by district.

This strategy trade-offs its advantages for more restrictive set of identifying assumptions: (a) parallel trends between districts with one more judge in a given year relative to others with no changes, (b) parallel trends between courts experiencing different level-changes (for example, courts experiencing net addition of 1 judge is assumed to be trending similarly to those experiencing a net addition of 2 judges in the counterfactual scenario), and (c) homogenous treatment effects. However, the estimates from this design provide a per-judge effect, complementing the extensive margin effects estimated by the event study specification in Equation 1. This also addresses any concerns stemming from the construction of events in Equation 1.

5 Results: Reduced Form Effects

I start with documenting the effects on immediate and longer term court-level outcomes in terms of staffing as well as backlog disposal rate. Next, I discuss the reduced form effects on local firms before interpreting the results through the lens of a conceptual framework on economic mechanisms.

5A Judge Headcount and Vacancy Rate

Panels A and B Figure 1 present the regression coefficients on the interacted event dummies from Equation 1 using both positive and negative change indicators when the outcome variable is judge headcount (Panel A) or inverse vacancy rate (Panel B) - (100-vacancy in %). Three features of these graphs are noteworthy: (a) an immediate increase/decrease in headcount and inverse vacancy rate following the changes, (b) persistence over a 4-year horizon, and (c) lack of any statistically or economically significant effects in the time periods prior to the staffing change. On average, the positive events increase the number of judges by ≈ 2 over a baseline level of 15 judges ($p < 0.001$ immediately, $p = 0.002$ 3 years from the staffing change, and $p = 0.13$ in the long run), increasing the staffing levels by over 13% and reducing vacancy rates by over 15 percentage points. Negative events decrease the number of judges by ≈ 1 ($p < 0.001$ immediately, $p < 0.001$ 3 years from the staffing change, and $p = 0.155$ in the long run), implying a 5.5% decrease in staffing levels. The coefficients indicate economically meaningful persistence where the staffing levels continue to be higher (or lower) by around 10 (5) percent 3-4 years following the events, albeit with a gradual decay given the frequency of turnovers. The asymmetry between positive and negative changes is plausible in a context where recruitment drives are often sporadic and lumpy whereas vacancy generation is relatively steady as incumbent judges reach seniority.

Table A.3 presents the estimates on positive (Columns 1 and 2) and negative (Columns 4 and 5) change events over time in a tabular format. These effects on judge staffing levels can be seen across different subsamples of district courts (see Table A.4 by subsets of districts based on their population). Finally, the estimates continue to be significant when I cluster the standard errors by state and event to account for any spatial correlation between district courts arising from reassignment of judges from one district to another.

5B Court Performance

Panel C Figure 1 plots the regression coefficients as per Equation 1 with annual court-level backlog disposal rate as the dependent variable. I find that disposal rate increases by ≈ 2 percentage points over a baseline of 12.62% following positive staffing changes ($p = 0.004$ immediately, $p = 0.047$ 3 years from the staffing change, and $p = 0.019$ in the long run). Each additional judge resolves 200 cases in a context where the average annual judge-level workload is ≈ 2000 cases.[13]

[13]I also confirm these numbers by estimating the specification using number of resolved cases as the dependent variable in Table A.5. I focus on disposal rate as the key measure as it measures backlog resolution in terms of percentage reduction in the existing workload of legal cases.

On the other hand, disposal rate does not respond significantly following a negative change. The estimated decline is ≈ 0.57 percentage points ($p = 0.003$ immediately but most likely due to improved precision, $p = 0.35$ 3 years from the staffing change, and $p = 0.98$ in the long run). The lack of a significant negative result following negative changes could be driven by the fact that fewer number of judges turnover relative to those added and that existing workload is shared among other judges in the court.

Columns 3 and 6 of Table A.3 present the event study estimates using disposal rate as outcome in a tabular format for net increase and net decrease in judge staffing, respectively. Importantly, the point estimates in the periods prior to the staffing changes are both statistically and economically insignificant, supporting the parallel trends assumption.

I note treatment effect heterogeneity by underlying district population (which also corresponds to the size of the court). Mid-sized and smaller districts experience larger improvements in disposal rate following net judge additions whereas the negative effects of net removal are mainly observed in large districts (see Table A.6).

Robustness: First Stage One would be concerned if any mechanical correlations between coding of the staffing change events and the disposal rate could drive the estimated effects. Recall that the staffing-level change is only measured using filing of new litigation, and thus should have little mechanical correlation with case resolutions or backlog from past years. Additionally, I estimate the effects of judicial staffing changes on court performance using Equation 2, which does not rely on event construction and uses leads and lags of continuous valued changes in the number of judges as key explanatory variables.

Figure A.10 presents the results from this specification in a graphical format. I find that existing workload and court performance are not correlated with the current or future judge staffing-level changes, further supporting the parallel trends and exogeneity of timing assumptions. Also reassuring is the result on the demand for litigation (number of new filings) that does not change significantly following staffing-level changes. Importantly, current and past changes in staffing-levels are only consequential for current and future disposal rates, which are qualitatively similar to the results from the event study design. These findings are also supported by local projection DiD estimation based on a sequence of first difference regression specifications following Dube et al. (2022) reported in Panel B Figure A.10.

5C Local Firms' Productivity

Figure 2 and Figure 3 depict the event study graphs for sales, wage bill, profit, capital, and raw material expenditures for the balanced panel sample following a net increase and a net decrease in the number of judges, respectively. Three key features of these graphs are: (a)

a gradual increase (or decrease) in the outcome following staffing change, (b) effects visible in the long-term, and (c) statistically and economically insignificant prior period estimates. The gradual and long-run nature are consistent with the fact that these firms represent an average, formal sector firm in the district, and not just those with legal cases in the court. These effects take time to appear as they are channeled through market mechanisms. This also suggests that the effects are unlikely due to specific legal cases being resolved in these courts and more indicative of improvements overall institutional capacity.

Table A.7 and Table A.8 present the results in a tabular format corresponding to each of the figures, respectively. Wage bill gradually increases by around 5% in the long run ($p = 0.037$) but is small immediately ($p = 0.93$). The improvement is visible starting year 3 from the staffing change ($p = 0.095$). Similarly, sales revenue grows by 2% in the long run (($p < 0.001$), with effects showing starting year 3 from the staffing change ($p = 0.016$). The effect on profit is 40% over the period ($p < 0.001$ in the long run but $p = 0.26$ immediately and $p = 0.002$ 3 years from the staffing change). The results for capital goods, i.e., the value of plant and machinery, are not statistically significant even though the point estimates are large and in the same direction as wage bill or sales. Lastly, expenditures on raw material used for production also increases, with a persistence over the long run ($p < 0.001$).

Since the sample firms are large in terms of these outcomes at baseline, these effects are economically meaningful. The relatively large effect on profit is consistent with the fact that the profit numbers are smaller relative to wage bill or sales revenue, and that increases in profits are also driven by a reduction in expenditures such as interest payments and accounting expenses.

The effects of negative staffing changes are commensurate with the magnitude of the change. In the long run, wage bill and sales revenue contract by about 2% each ($p = 0.82$ immediately, $p = 0.085$ 3 years from the staffing change, and $p = 0.003$ in the long run for wages and $p = 0.46$ immediately, $p = 0.06$ 3 years from the staffing change, and $p = 0.006$ in the long run for sales), respectively. Profits contract by 20% ($p = 0.36$ immediately, $p = 0.05$ 3 years from the staffing change, and $p = 0.003$ in the long run). The value of plants and machinery as well as expenditure on raw material also decrease but the point estimates are imprecise. Normalizing effects per judge suggest that the changes in firm productivity outcomes are symmetric with respect to staffing variations.

Robustness: Firm-level Outcomes One concern is whether the above results reflect biased estimates due to firm sample construction to create a balanced panel. That is, if the outcomes of the analysis sample are correlated with the changing composition of the population of firms in the district (particularly those that are not in the balanced panel),

the productivity effect could be estimated with a bias. So, even if using a balanced panel keeps the sample composition fixed for internal validity, the sample construction itself could be introducing bias due to endogenously changing environment over time. This raises three questions: (a) how would this affect the direction of the bias, (b) whether this should be considered as an outcome (for example, a change in market competition can indeed be considered an outcome), and (c) interpreting welfare effects in the presence of such a bias.

I address this concern in three different ways: First, I examine the effect of judge staffing-level changes on new firm incorporations (firm entry) and total number of firms in the district. This itself could indicate a more broad-based impact of judicial capacity, answering (b) above, and the direction of effects would help shed light on (a) and (c). I find increased firm entry and an increase in total number of firms in the districts around positive staffing events. This plausibly creates a more competitive environment for businesses and introduces dynamism. If this were so, the estimates from the main analysis would more likely be downward biased, providing a lower bound for the true effect. Second, I estimate the effects using the full sample of unbalanced firms, which are qualitatively similar (see Table A.9). Third and related to the second approach, I check if missingness of data is correlated with staffing-level changes. I find a decrease in the extent the missing data consequent to improved judicial staffing and greater missing entries following net decreases (but importantly, with no pre-trends; see Table A.10). This suggests that firms are more likely to report data (or less likely to evade reporting) when there are more judges in their local courts and vice versa. Together with the fact that there are more firms operating in the district following net judge addition, increased reporting by other incumbent firms further supports plausible increased competition and downward bias in the estimated productivity effects from the balanced sample. This also implies that using unbalanced panel of firms is not a feasible strategy to estimate the causal effects, since missing data is not random.

A second concern is whether the effects are mechanical due to litigating nature of firms. If this is the case, then the effects could be due to gains from resolution of ongoing litigation in courts rather than through real economic channels. There are two reasons that suggest that the effects are much broader, reflecting the role of courts in facilitating local economic transactions: (a) the main sample of incumbent, non-financial firms are not litigation intensive but financial sector firms such as banks are, which I study as part of the mechanism in Section 6, and (b) the effects persist even among firms with no legal case data in the sample courts over the entire study period (see Table A.11).

A third concern is about more general spurious correlation, such as those arising from concurrent local macro-economic shocks not captured in state-year fixed effects. To address this, I check whether the effects are restricted to firms within a court's jurisdiction and

not experienced among similar firms in the neighboring districts, which may also experience these unobserved shocks. Table A.12 documents the results for positive staffing-level changes, showing that the point estimates are statistically and economically insignificant. This also supports the likelihood of the firm-level estimates reflecting a lower bound of the true effect of improved judicial capacity within the local courts.

Lastly, the results are also qualitatively similar when using complementary econometric specifications (Equation 2 and and local projection DID as in Dube et al. 2022), suggesting plausible real effects of local courts on economic activities and development outcomes (Figure A.12).

5D Other District-level Outcomes

Two pieces of evidence suggest that the effect of judicial staffing-level changes are broad-based: (a) changes in firm entry (new incorporations) highlight potential extensive margin improvements in the number of formal sector firms in a district, and (b) improvement in district-level measures or proxies of GDP, such as nighttime light intensity, which would incorporate the informal sector.

On firm entry, I note an increase in new incorporations and an increase in the overall number of firms in the district (Cols 1-2 Table 3) following staffing expansions. On the other hand, a net negative change has a minimal effect on the firm composition.

On GDP growth, I find suggestive evidence of increases in nightlight intensity following positive changes (intensity increases by about 6%) and a decrease (by about 3%) following negative changes (Cols 3 and 6 Table 3). Albeit noisy ($p = 0.315$ in the long run), this analysis complements the results from the formal sector analysis under the assumption that the nightlight data would capture informal and household sector outcomes and investments in infrastructure.

6 Mechanisms

District courts facilitate a large number of economic transactions, such as those engaging much of the financial sector. Formal sector enterprises, including banks, are required by law to file disputes in their local court before initiating further actions to enforce contracts (for example, before initiating any liquidation proceedings). Thus, well-functioning courts that can resolve such disputes in a timely fashion are essential to the workflow of banks and more broadly, the formal sector.

Using the individual legal case records, I document that: (a) banks are litigation intensive - there are many more cases per bank relative to any other firm type, (b) about 50% of all

commercial banks in India have at least one ongoing legal case during the study period in the sample courts, and (c) banks are the complainants in an overwhelming majority of the cases (see Figure 4). Further, the value of assets under litigation involving debt recovery are many orders of magnitude larger than other dispute types (such as insurance claims or alimony). Typically, such disputes are settled in favor of the lenders, where judges facilitate a settlement to enable partial or complete recovery.[14]

Local Credit Supply I begin by examining the resolution of legal cases in the sample courts where a bank is one of the litigant, pertaining to recovery of unpaid debt. The average disposal rate of these cases in the sample court is similar in magnitude to the overall disposal rate, which also increases by 2 percentage points per year following positive staffing-level changes. The median duration of such cases decreases when judge vacancy is resolved (see Panel A Figure 5). It is possible that the banks could recirculate the increased liquidity as additional credit to industrial borrowers.

In the absence of bank branch-level lending data, I examine district-level aggregate lending to industrial borrowers to estimate the effect of judge staffing-level changes on local credit supply towards industrial production. Since bank's lending response to improved judicial capacity depends on the extent of pending cases, I weight the regression specification in Equation 1 by the number of pending cases involving banks at the start of the study period. Panel B Figure 5 presents the event study graphs using total number of bank loans to industrial borrowers in a district as the outcome variable.[15] Total lending to industrial borrowers increases between 6-8% over the long run following an increase in the number of judges ($p = 0.07$ in year 3 and $p = 0.11$ year 4 and beyond), with private sector banks playing a bigger role (private lending increases by over 12% in the long run, $p = 0.016$).

This credit mechanism does not preclude the possibility of changes in borrower default behavior and strategic lending, which could also respond to changes in the number of judges. To shed light on this, I develop an economic framework that I discuss below, which provides specific hypotheses to suggest that both liquidity and forward-looking behavior could be at play.

Local Markets, Access to Credit, And Firms' Production Decisions There are two key ingredients in this framework linking local judicial capacity with firm productivity.

[14]Based on parsing judgements from a random subsample of cases involving banks, I found that over 83% of the credit related disputes have outcomes in favor of the banks. This was also confirmed based on unstructured interviews with retired and incumbent judges of district courts.

[15]The credit data also includes total outstanding loan amount at the district level but this includes new loans as well as defaulted loans including NPAs. Thus, it is unclear a-priori how this measure would map to improved lending as an outcome. Unfortunately, there is no NPA or collections data at the district-level.

First relates to access to credit via credit markets and repayment behavior (following Besley and Coate 1995; Banerjee and Duflo 2010). Second is about firms' optimization problem.

Starting with the credit model, I assume that firms need external finance for operations, which has some stochastic probability of success. A lender (e.g., bank) bases their lending decisions on whether repayment can be enforced through courts, in addition to borrower's wealth (to liquidate in the event of default). Lending takes place only if the lender's expected return is greater than the market return. Upon completion of the contract period, the borrower either repays or evades. Evasion leads to default, which initiates debt recovery litigation. This recovery process is costly for both lender and borrower, and is a decreasing function of court's effectiveness in contract enforcement. Some borrowers may choose to litigate if their payoff is better with litigation - for example, if litigation enables the borrowers to renegotiate a reduced interest rate or alter other repayment terms. Other borrowers may choose to settle with the lender to avoid litigation. I model this interaction as a lender-borrower game with a sub-game perfect Nash equilibrium that requires a wealth cut-off for lending. Comparative statics with respect to exogenous variation in judicial capacity suggests that the lender would lend to smaller borrowers and lower interest rates for all levels of borrowing with better courts.

The second part of the framework concerns firms' problem where production also incurs monitoring costs and is subject to credit constraints. Firms would re-optimize their production decisions following changes in access to credit. In addition to the credit channel, improved courts could also directly benefit firms' through lower monitoring costs, for example, from those incurred in protecting property. This suggests that firms would expand production from increased credit as well as incur lower expenditures, both of which would positively impact their productivity and balance sheet outcomes.

I discuss the framework in detail in Appendix A.3. Important implications from this are: (a) there are extensive margin changes determining who a bank lends to - better judicial capacity expands credit access, (b) lowering of the price of credit (interest rate on loans) - better judicial capacity lowers interest rates for all levels of borrowing, (c) productivity benefits for firms through increased credit access and lower monitoring costs, and (d) growth of smaller, credit-constrained firms.

Firm-level Working Capital and Interest Expenditures To examine firm-level credit use and the cost of credit, I examine annual working capital and interest expenditures. Working capital reflects the extent of cash available to meet operating expenses, which I

use as a proxy for borrowing in the absence of reliable firm-level borrowing data.[16] Interest expenditure reflects both the price of credit as well as the total expenditure on credit (total borrowing costs).

Empirically, I note an increase in firms' working capital and a decrease in interest expenditure following positive staffing-level changes that persist over the long run (Panel C Figure 5). The immediate effects on working capital and interest expenditure is consistent with the plausible role of liquidity in local credit markets. Working capital increases by 39% ($p < 0.001$) that persists in the long run ($p = 0.021$). Interest expenditures decline by 8% immediately and also persist ($p < 0.001$ over the long run). The result on interest expenditure includes both price effect (reduction in the cost of borrowing) as well as quantity effect (increased expenditure as a result of additional borrowing). The net effect is negative, where the reduction in the cost of borrowing offsets any increase in additional borrowing.

The conceptual framework generates additional hypotheses relating to firm-size that can be tested in the data, namely smaller, credit-constrained firms benefit from improved judicial capacity, and witness an increase in credit access and productivity effects. I use firm-level baseline data on total asset value as well as the extent of debt-exposure (leverage) to classify firms into size bins (above and below median) to examine these additional hypotheses. Specifically, I estimate the event study specification in Equation 1 among subsamples of firms that are small and have below-median leverage. Figure 6 shows that smaller firms are more likely to appear as defendants in legal cases when there are more judges. These firms also experience greater working capital infusion, face lower interest expenditure, and record higher profits.

Lastly, these effects on credit behavior are not symmetric with respect to negative changes in judge staffing levels. This lack of a symmetric negative effect is plausible in the presence of natural lags in recognizing defaults and filing of debt recovery litigation in courts. So, even if borrower defaults go up following negative staffing-level changes, I do not detect its effect on court-level outcomes or district-level lending outcomes (Col 3-4 in Table A.13) within the time-period of this study.

7 Discussion

Decomposition of Firms' Productivity and Plausible Mechanisms Since courts enforce not just contracts but also property rights and rule of law, an improvement in judicial capacity could also translate into local development outcomes through these other channels.

[16]Borrowing data is not consistently reported by all firms within the study period and hence, I rely on working capital as an indicator for their ability to finance operating expenses. Working capital mainly consists of excess cash, including borrowings, net of committed payments due within the accounting year.

These alternate channels are indeed important. For example, I find that positive judicial staffing-level changes help contain both serious/violent crimes (homicides, and those causing bodily injuries) as well as minor property crimes (thefts). Panel C Figure 6 shows reductions in both types of crimes following a net increase in the number of judges. With the caveat that I am unable to distinguish whether these changes are due to reporting or true occurrence of crimes, these results suggest that local courts plausibly also improve security of persons and property. The effects on firm productivity are also consistent with the mechanism of lower monitoring costs.

How do these channels compare with access to credit to facilitate the observed growth of local formal sector firms? For example, if firms are infra-marginal in their need to safeguard their property, then improved local courts through the crime channel may not translate to huge effects on their profits. Similarly, if credit constraints are not binding, then an expansion in local judicial capacity should not affect firm profits.

To assess the relative importance of each of these channels, I decompose firms' profits and sales into that arising from credit access (working capital and interest expenditures) and monitoring costs (local crime rates) using a distributed lags model. I decompose profit and sales separately to isolate gains from lower expenses, especially because interest expenditures incorporate both price and quantity mechanisms. Decomposing sales would provide insights on productivity-related improvements from these mechanisms. I include lagged values of firm-level dependent variables, firm fixed effects, and flexible controls for district-year and industry-year interactions to account for many of the time-varying unobserved drivers of firm productivity.

Columns 1 and 2 of Table 4 decompose profit whereas columns 3 and 4 decompose sales. Note that crime data varies at the district-year level, and therefore, they are absorbed by the district-year fixed effects in Columns 2 and 4. Profit decomposition suggests a significant positive association with working capital and a negative association with interest expenditure. The coefficients remain stable even after including additional district-year and industry-year fixed effects. The positive coefficient on working capital suggests that many of these firms are credit constrained and that an expansion in credit access improves firm profitability. On the other hand, because interest payments are accounted as an expenditure in the calculation of annual profits, the negative coefficient implies that profit increases with a lowering of interest expenditures. Sales revenue is positively associated with working capital as well as interest expenditure. The positive correlation between interest expenditure and sales could reflect quantity mechanism where higher interest expenditure increases productivity through increased borrowing to finance a higher scale of operations. The associations between profits or sales with crime variables, while in the expected direction, are much more noisy.

Benefits and Costs of Reducing Judge Vacancy This paper suggests that investing in improving judicial staffing in front-line, district courts is important for local firm productivity and subsequently, overall economic development. Leveraging the fact that the firms in the study sample are tax-paying firms and employ labor with taxable income, this investment could generate large returns, both from the perspective of public budget surplus as well as increases in social returns.

In Table 5, I document sources, computations, and assumptions to generate a back-of-the-envelope benefit-cost ratio from adding one more judge to district courts. On the benefits side, I use the median values of profits and wage bills among the sample firms to compute increases in firm-level surplus and salaried income. For tax revenue implications, I calculate additional corporate and income tax generated following the increase in corporate profit and wage bill. On the expenditure side, I calculate the cost of an additional judge using the median proposed salary in the Second National Judicial Pay Commission. I further inflate the salary to account for fringe costs including annual increments, benefits and allowances towards retirement, transport, and housing costs. The actual salaries and benefits would be lower than this figure depending on the extent of adoption of these recommendations by each state.

I compute the discounted net present value (NPV) of the stream of benefits - increases in profits, wage bills, tax revenue - and the stream of costs - the average expenditure per judge, over a 5 year horizon, using 5% discount rate in the base calculation. I arrive at the confidence intervals by bootstrapping the NPV calculations using the estimated coefficients and their standard errors. This computation shows that the benefits are orders of magnitude larger than the costs. For the public budget, the ratio implies revenues that are over 6 times larger than expenditure on average (with the 90% confidence interval including a ratio of 4.81 and 8.75), whereas the social returns are over 30 times the cost (with the 90% confidence interval including 25.6 and 46.15). Even the most conservative estimates (when using a higher discount rate and the lower bound of the cost-benefit estimate within the confidence interval) suggest that the returns to investing in district judicial staffing is high and more than pays for itself.

8 Conclusion

Well-functioning front-line judiciary is a core component of state capacity and important for local economic development. The current status-quo in India and much of the developing world underscores a problem of large backlogs of legal disputes and staffing constraints. Reducing vacancy by adding more judges is a highly cost-effective intervention as seen in

Indian context, which spurs the productivity of local formal sector firms.

In a context with credit supply constraints and where large amounts of capital are stuck in litigation due to loan defaults, even a marginal increase in judicial capacity frees up a meaningful magnitude of frozen capital. Local firms become more productive when credit supply constraints are resolved by expanding their working capital and lowering interest expenditures. There are also indications for broad-based economic development. Importantly, the benefits accrue relatively quickly, suggesting that this is a relatively low-hanging fruit in terms of policy.

These findings complement the existing narrative in the literature on the importance of courts in bankruptcy enforcement (Ponticelli and Alencar 2016; Müller 2022) and the rule of law (La Porta et al. 1998; Johnson et al. 2002). The new insight is that front-line courts play a transactional role in local credit markets through routine debt contract enforcement, enabling credit recirculation. This is salient in a context where a large share of the loan defaults are from unsecured or policy-directed lending, which have no recourse to bankruptcy proceedings. Small-loan default cases are prevalent among front-line courts across the world.[17]

While this paper does not delve into the subsequent actions of financial institutions in response to changes in judicial capacity on credit misallocation specifically, one could think of capital recovered from the backlog of litigation as reducing misallocation. Further research is needed to examine whether lenders extend credit to firms with higher marginal product of capital or higher TFP and how this interacts with the local judicial capacity. Examining how functioning of district courts interact with banks' lending decisions across different borrower types can potentially shed light on important mechanisms behind capital misallocation. Availability of data such as judge biographies, loan-level data, and high frequency data on the productivity of the household informal sector would greatly help answer these follow up questions on the role of courts in finance and development.

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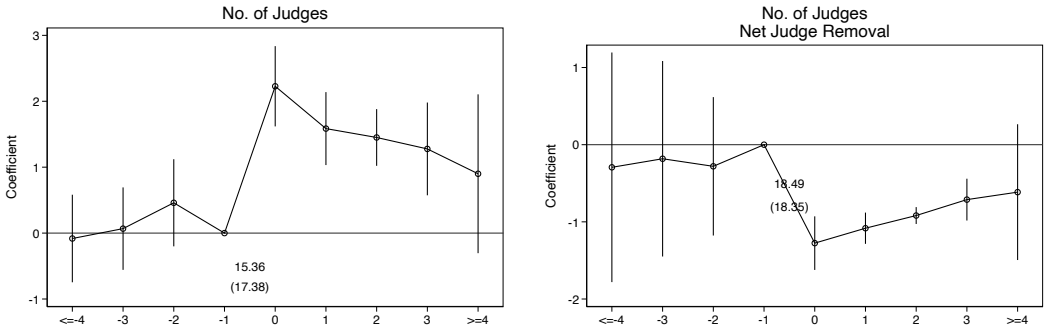
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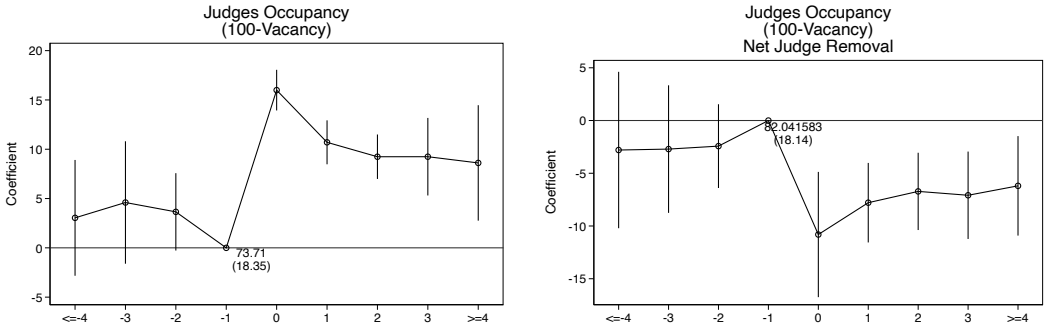
9 Figures

Figure 1: Net Addition and Removal of Judges and Court Performance

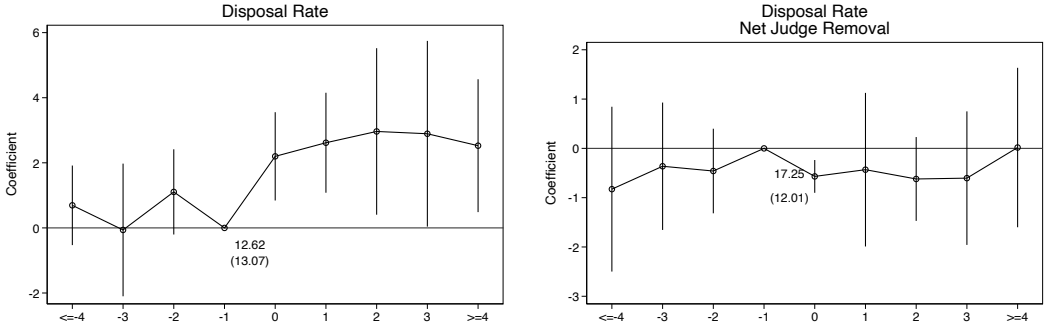
Panel A: Judge Headcount



Panel B: Inverse Vacancy Rate

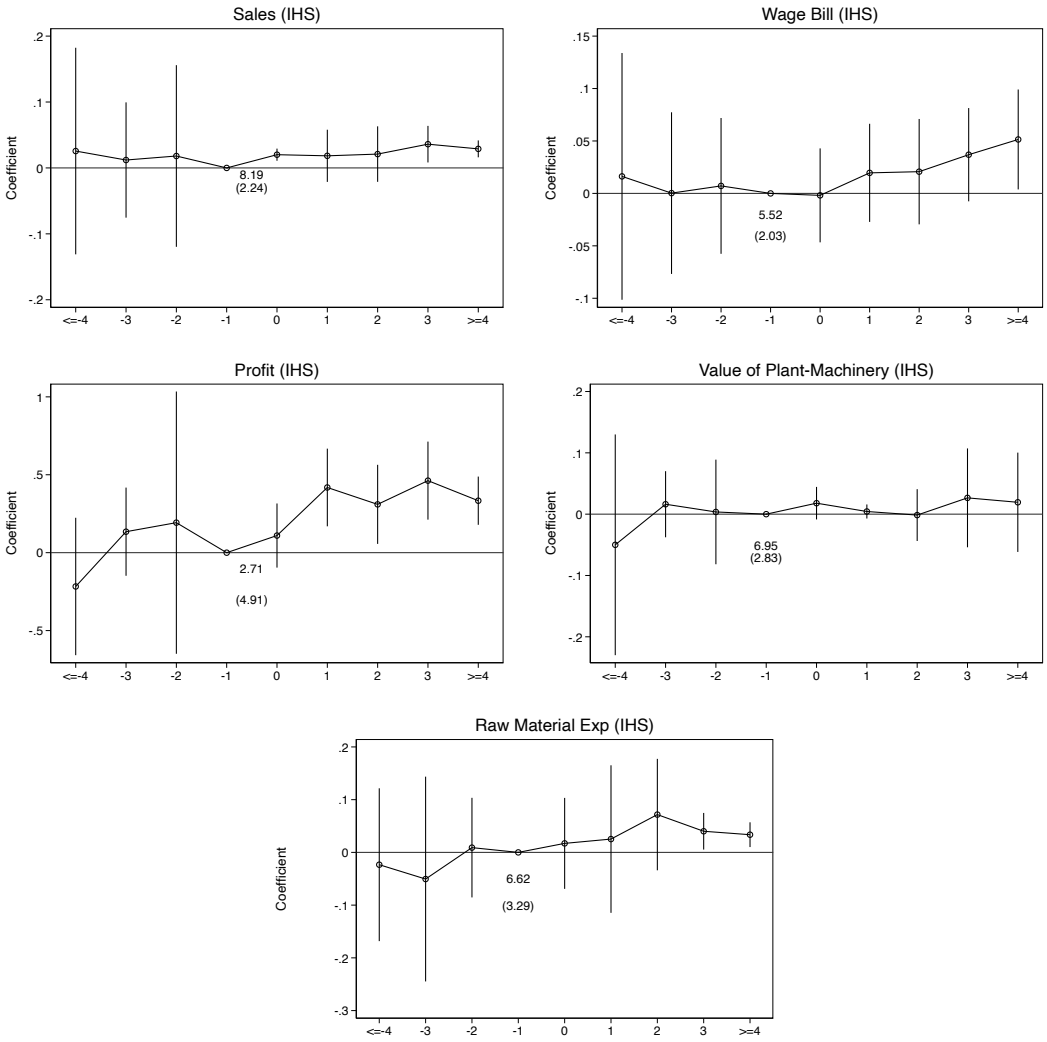


Panel C: Court-Level Disposal Rate



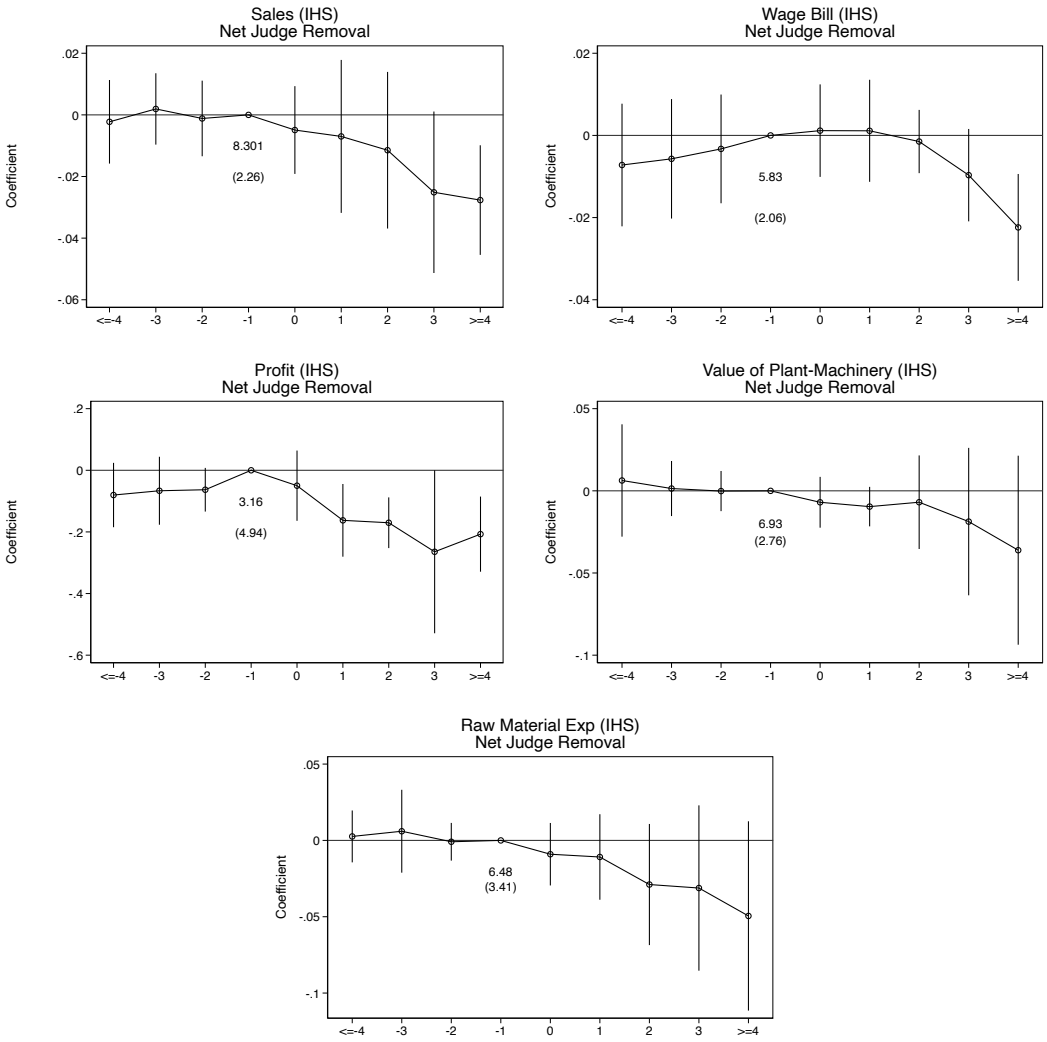
Notes: The figures plot the event study interaction coefficients for positive and negative staffing changes from estimating Equation 1 using total number of judges (Panel A), inverse vacancy rates (Panel B) and disposal rate (expressed in percentage terms in Panel C) as dependent variables, respectively. In all the figures, the end-points take into account relative event-bins outside the effect window in the data. The coefficients are all normalized to the period prior to the event. Standard errors are clustered by district and event. Error bars present 95% confidence interval. The table equivalent of these graphs is Table A.3.

Figure 2: Local Firms' Production: Net Judge Addition



Notes: The figures above plot the event studies coefficients on positive staffing change event-time interaction dummies from estimating Equation 1 for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed in the balance-sheet data. Using log transformation also yields similar results. The sample comprises of a balanced panel of incumbent firms in the district that report their annual balance sheet information over the study period, enabling the use of firm fixed effect in the specification. The first row presents the coefficients with sales revenue and wage bills as the dependent variables. The dependent variables in second row are profit, the value of capital goods (plant/machinery), and raw material expenditure, respectively. In all the figures, the end-points take into account relative event-bins outside the effect window in the data. The coefficients are all normalized to the period prior to an event and standard errors are clustered by district and event. Error bars present 95% confidence interval. The table equivalent of these graphs is Table A.7.

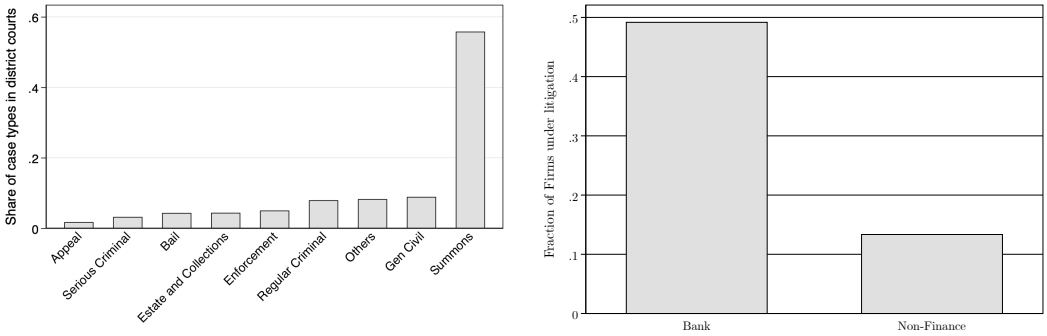
Figure 3: Local Firms' Production: Net Judge Removal



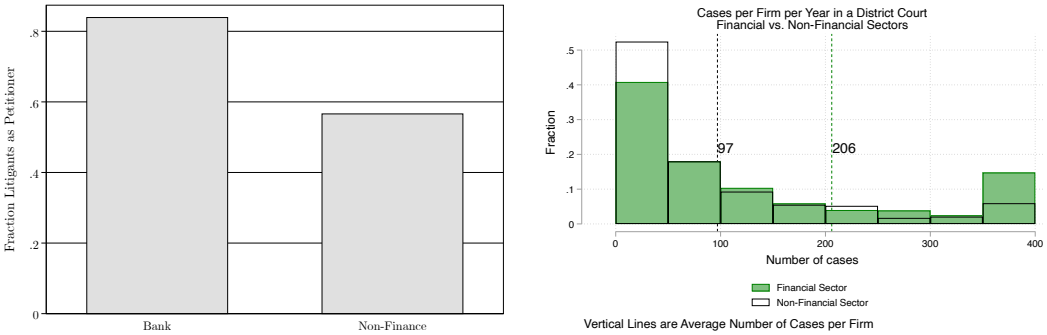
Notes: The figures above plot the event studies coefficients on negative staffing change event-time interaction dummies from estimating Equation 1 for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed in the balance-sheet data. Using log transformation also yields similar results. The sample comprises of a balanced panel of incumbent firms in the district that report their annual balance sheet information over the study period, enabling the use of firm fixed effect in the specification. The first row presents the coefficients with sales revenue and wage bills as the dependent variables. The dependent variables in second row are profit, the value of capital goods (plant/machinery), and raw material expenditure, respectively. In all the figures, the end-points take into account relative event-bins outside the effect window in the data. The coefficients are all normalized to the period prior to an event and standard errors are clustered by district and event. Error bars present 95% confidence interval. The table equivalent of these graphs is Table A.8.

Figure 4: Case-Types in District Courts

Panel A: All cases



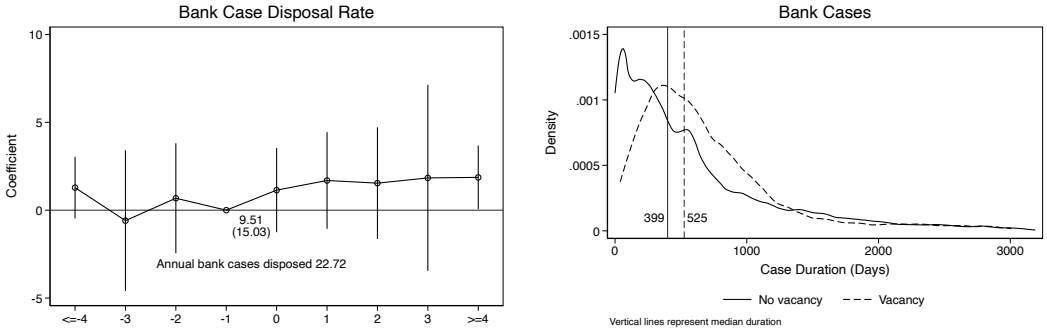
Panel B: Cases with firms as litigants



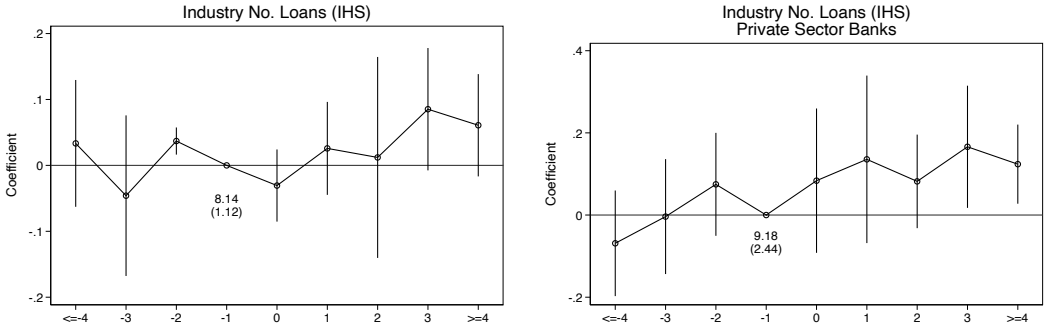
Notes: Panel A illustrates the distribution of legal case types in district courts in a typical industrial state. Left panel categorizes cases by the underlying case-type. A large majority of cases ($> 50\%$) are summon cases, which mean that these are one-sided complaints that require the defendant/respondent to appear in front of the court. In contrast, the remaining case types are more well-defined where both complainant and respondents are present. Many of these categories - "Gen Civil", "Enforcement", "Estate and Collections" also represent contractual disputes. Figure on the right summarizes the fraction of firms by banking and non-finance sectors that appear as a litigant in the sample courts. Panel B presents the distribution of firm-related cases in district courts by sector, showing the fraction of litigants from specific sectors that initiate complaints as petitioners (left) and the distribution of the number of legal cases per firm in the sample by their sector (right).

Figure 5: Credit Mechanism

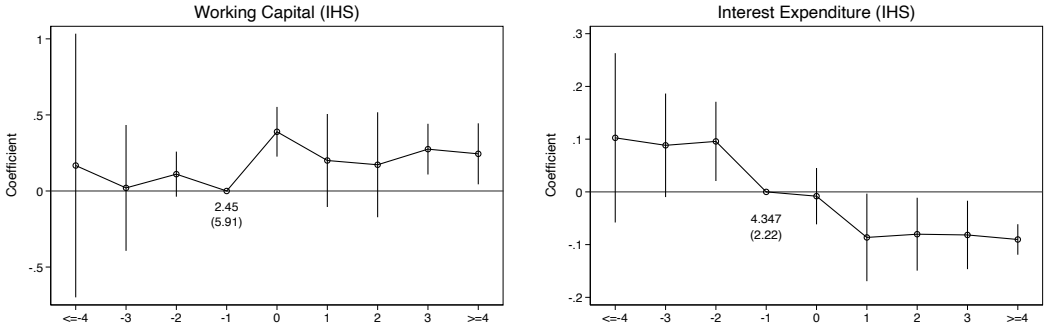
Panel A: Resolution of Banks' Cases in Courts



Panel B: District-Level Lending

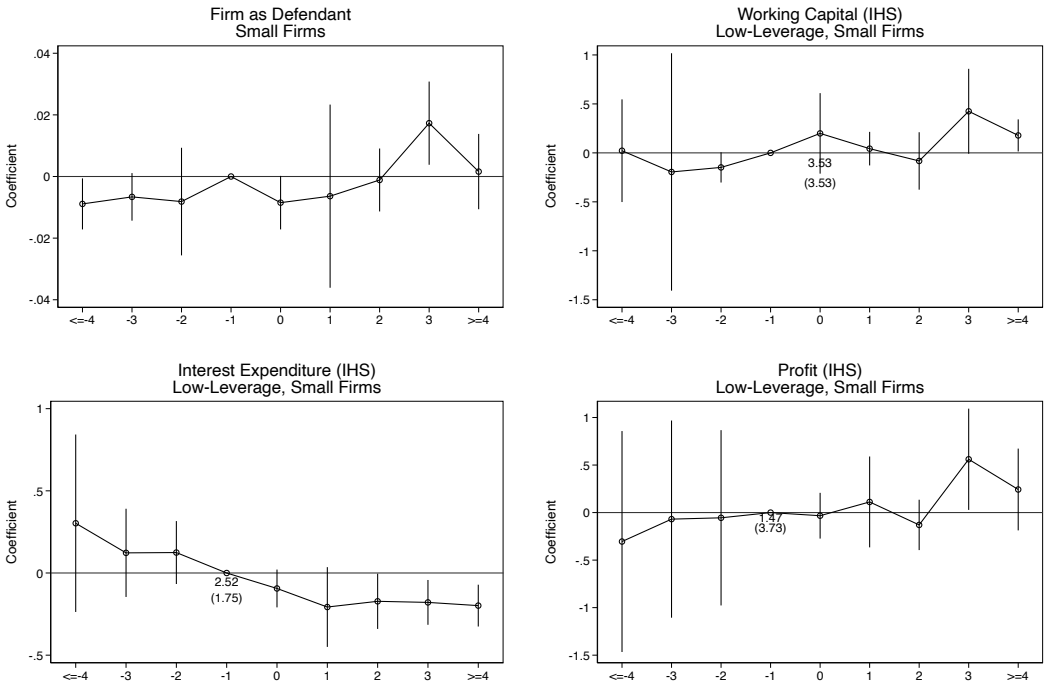


Panel C: Firm-level Working Capital and Interest Expenditure - All Sample Firms



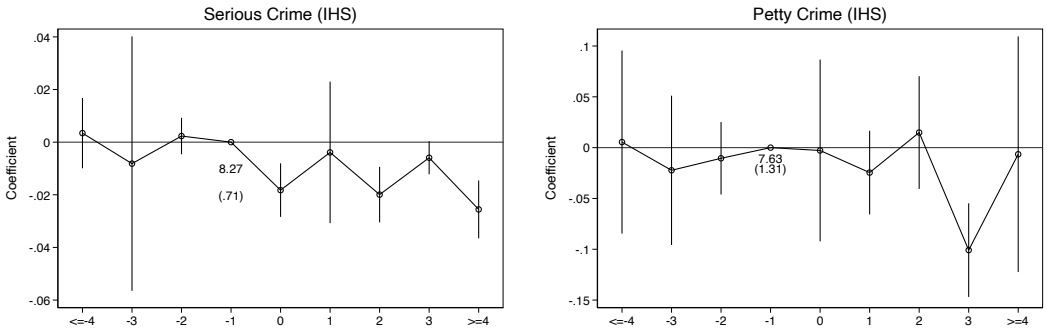
Notes: Panel A presents what happens to banks' legal cases in courts following net judge addition. Left panel shows that the case backlog reduces by 2 percentage points. Right panel shows reduction in median case duration when judge vacancy is resolved. Panel B presents effects of positive staffing-level changes on overall district-level lending by all banks branches, including those of private sector banks (right), to industrial borrowers. Panel C presents effects of positive changes on working capital and interest expenditure for all firms. The table equivalent of the firm-level graphs is Table A.7 and Table A.8, respectively (Col 3, 6, 7). The table equivalent of the district-level bank lending outcome is in Table A.13 (Col 1 and 5, respectively). District-level regressions on bank lending are weighted by the number of bank-related legal cases at the start of the study period. Error bars present 95% confidence interval.

Figure 6: Improved Judicial Capacity and Access to Credit



Notes: In clock-wise order starting from top-left: (a) dependent variable in the event study is a dummy variable taking value 1 when a small firm (below median ex-ante asset size) is found as a defendant in the legal case data, (b) dependent variable is the annual working capital reported by small firms with below-median ex-ante leverage (leverage defined as debt-equity ratio), (c) the dependent variable is annual interest expenditure by small, below-median-leveraged firms as in (b), and (d) dependent variable is the annual profit of the firms in (b)-(c). The event studies are all around the timing of net addition of judges. Error bars present 95% confidence interval.

Figure 7: Alternate Explanations: Crime as a Mechanism
Panel A: District-Level Crime Outcomes



Notes: Panel A presents positive staffing effects on overall reported crime outcomes in the district, separated by serious and petty crimes. Panel B documents the effect on raw material expenditure. Error bars present 95% confidence interval. The table equivalent of the firm-level graphs is Table A.7 and Table A.8, respectively (Col 3).

10 Tables

Table 1: Summary Statistics

	(1)					
	No. of Units	Observations	Mean	Std Dev	Min	Max
Panel A: Court Variables						
Total Judge Posts	195	1755	18	19	1	108
100-Vacancy(%)	195	1723	77	21	10	100
No. Net Judge Increases	195	195	1.621	1.153	0	6
Δ Judge (+ve) (per event)	158	158	2.31	2.54	1	24
No. Net Judge Decreases	195	195	3.6	1.66	1	8
Δ Judge (-ve) (per event)	195	195	3.67	3.97	1	33
Disposal Rate (%)	195	1755	14	12	0	86
Case Duration (days)	195	5706852	420	570	0	4022
Panel B: District Outcomes						
No. Industry Loans	192	1719	9188.2	15602.58	30	188456
Outstanding Amount (real terms, million INR)	192	1719	310.3	1130.19	0.023	15569.2
Serious Crimes	195	1744	3258	3474	16	36377
Other IPC Crimes	195	1744	1624	2371	0	26170
Nightlights Intensity	192	1344	1.3	3.78	0.05	62.07
Panel C: Sample Firms						
Wage Bill (in real terms, million INR)	393	3537	640.9	939.2	0	4645.76
Plant value (real terms, million INR)	393	3537	3867.6	7052.8	0	36506.9
Raw Mat Exp (real terms, million INR)	393	3537	3687.3	5797.7	0	28694.6
Revenue from Sales (real terms, million INR)	393	3537	8421.6	12085.3	0	59319.2
Accounting Profits (in real terms, million INR)	393	3537	371.2	811.5	-1897.1	3388.14
Working Cap (in real terms, million INR)	393	3537	537	1873.3	-5611.1	7099.9
Interest Exp (in real terms, million INR)	393	3537	231.5	460.9	0	2933.6

Notes: Panel A summarizes the court-level variables computed from case-level disaggregated data. Panel B summarizes district-level outcomes including bank lending to industries, local reported crime, and satellite nightlight intensity. Panel C summarizes firm-level variables for incumbent firms in the main firm-level analysis sample, i.e., the balanced panel of firms. All monetary variables are measured in INR million as reported in Prowess database, in real terms using 2015 as the base year.

Table 2: Balance Table: A Long-Differenced Prediction of Judge Staffing Changes

	(1) Δ Judges	(2) Δ Judges	(3) Δ Vacancy	(4) Δ Vacancy
Δ Pop	-0.597	-0.564	0.387	0.353
	(0.742)	(0.688)	(0.604)	(0.578)
Δ # HH	0.349	0.377	-0.282	-0.309
	(0.422)	(0.523)	(0.313)	(0.365)
Δ SC Pop	-0.0138	-0.00937	-0.00447	-0.0108
	(0.0647)	(0.0759)	(0.0467)	(0.0546)
Δ Lit Pop	0.140	0.0706	-0.0647	0.00732
	(0.225)	(0.140)	(0.190)	(0.156)
Δ Urban Pop	-0.0482	-0.0550	0.0494	0.0569
	(0.0543)	(0.0545)	(0.0469)	(0.0471)
Δ All Emp	-0.0184	-0.0203	0.00872	0.0108
	(0.0377)	(0.0363)	(0.0299)	(0.0285)
Δ Manuf Emp	0.0126	0.0142	-0.00562	-0.00726
	(0.0299)	(0.0285)	(0.0240)	(0.0226)
Δ Candidates		0.0176		-0.0206
		(0.0182)		(0.0170)
Δ Elec Turnout		0.157		-0.157
		(0.416)		(0.324)
Δ Winner Vote Share		0.130		-0.162
		(0.386)		(0.244)
Observations	194	194	194	194
State FE	X	X	X	X
Joint P-value	0.890		0.810	
Joint P-value (electoral)		0.324		0.194

Notes: This table uses a long difference specification, regressing long-differenced judicial staffing measures - the number of judges as well as judge vacancy rates - on lagged long-differenced district-level measures from population and economic census including population, demographic composition, urbanization, employment including manufacturing employment, and electoral outcomes. All the variables are measured in terms of percentage changes from the baseline period. A more typical approach to generating balance table using pair-wise regressions between baseline outcomes and judicial staffing changes, as followed in RCTs, also do not yield any statistical or economically meaningful correlation coefficients on the staffing variable.

Table 3: District-level Firm Incorporations, Total Number of Firms, and Nightlights

	Net Judge Addition			Net Judge Removal		
	(1) New Incorp.	(2) Total Firms	(3) Avg. Nightlights (IHS)	(4) New Incorp.	(5) Total Firms	(6) Avg. Nightlights (IHS)
Event x <=-4	-1.274	-8.789	-0.105	0.0650	-0.167	0.0315
	(1.009)	(7.129)	(0.0751)	(0.168)	(2.483)	(0.0322)
Event x -3	-0.212	-4.672	-0.0570	0.0671	-0.231	0.0201
	(0.366)	(2.838)	(0.0491)	(0.139)	(0.599)	(0.0213)
Event x -2	-0.168	-1.555	0.00240	0.144	0.0383	-0.0136
	(0.201)	(1.827)	(0.00753)	(0.201)	(0.650)	(0.0288)
Event x 0	0.286***	1.549	0.00893	-0.0289	-0.702	-0.00139
	(0.0709)	(1.659)	(0.0165)	(0.0695)	(1.145)	(0.0166)
Event x 1	0.286**	3.387*	0.0234	-0.0184	-0.857	-0.0203
	(0.117)	(1.875)	(0.0275)	(0.0309)	(1.438)	(0.0207)
Event x 2	0.520***	6.808	0.0353	-0.0840	-2.370	-0.0127
	(0.0856)	(4.003)	(0.0392)	(0.116)	(1.961)	(0.0178)
Event x 3	0.466***	7.635	0.0369	0.0482	-1.705	-0.00840
	(0.142)	(4.751)	(0.0386)	(0.0551)	(1.704)	(0.0169)
Event x >=4	0.644***	9.972	0.0584	-0.0711	-2.483	-0.0382
	(0.196)	(6.544)	(0.0559)	(0.0996)	(2.944)	(0.0399)
Observations	4806	7497	6993	4806	7497	6993
No. Districts	95	155	192	95	155	192
Control Mean	1.8	22.2	0.96	1.9	48.3	1.55

Notes: This table presents the estimates from Equation 1 using new firm incorporation and total number of firms in a district in a given year, including those not in the main analysis balanced panel. For nightlights reported in Columns 3 and 6, I use VIIRS annual average nightlights data from Colorado Mines Earth Observatory from 2012-2018. I use district GIS shapefiles to compute the average nightlight intensity within the polygon for each year in the data. The empirical specification includes district and state-year fixed effects. Standard errors are clustered by district and event.

Table 4: Decomposition - Firm Profits

	(1) Profit	(2) Profit	(3) Sales	(4) Sales
Working Cap	0.147***	0.138***	0.0110***	0.0103*
	(0.0299)	(0.0347)	(0.00320)	(0.00526)
Interest Exp	-0.590***	-0.749***	0.163***	0.191***
	(0.172)	(0.219)	(0.0245)	(0.0335)
Lesser Crime	-0.209		-0.00904	
	(0.231)		(0.0232)	
All Crime	-0.169		0.0280	
	(0.785)		(0.0752)	
Profit t-1	-0.00816	-0.0683***		
	(0.0179)	(0.0231)		
Profit t-2	-0.0342*	-0.0194		
	(0.0179)	(0.0218)		
Sales t-1			0.00268	0.00416
			(0.00380)	(0.00484)
Sales t-2			-0.00283	-0.00803
			(0.00394)	(0.00522)
Observations	2702	2095	2372	1785
No. Firms	368	298	363	295
Firm FE	X	X	X	X
Interaction	State-Year FE	District-Year FE Industry-Year FE	State-Year FE	District-Year FE Industry-Year FE

Standard errors in parentheses

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

Notes: This table presents a firm fixed effect regression of inverse hyberbolic sine-transformed variables - profit or sales revenue (dep var) on working capital, interest expenditure, local crime (all crime and lesser crimes) and lagged dependent variables. Following firm-level profit maximizing problem in Appendix A.3, profit should be positively correlated with working capital, whereas negatively correlated with costs induced by local crime. The sign on the coefficient on the interest expenditure depends on the dependent variable. Since profit is defined as total income net of total costs, interest expenditure should be negatively correlated since it is incorporated under costs. In contrast, when sales is the dependent variable, the correlation with interest expenditure is ex-ante ambiguous. A positive correlation with interest expenditure is indicative of positive productivity effect from increased working capital (and an associated higher total interest payment through quantity mechanism). In contrast, high cost of credit (through higher interest rates) could also increase the interest expenditure but this would negatively impact productivity (through price mechanism). Columns 1 and 3 control for state-year dummies to non-parametrically account for macro-economic changes at the state-level in addition to firm fixed effect. Columns 2 and 4 introduce district-year and additionally, industry-year dummies respectively. Since crime variables vary only at the district-year level, these are absorbed by the district-year dummies.

Table 5: Cost-benefit Calculation

Parameter	Value	Units	Source
No. Firms per District	6	Number	Sample
Median Profit	79.21	Million INR	Sample
Median Wage Bill	240.74	Million INR	Sample
Corporate Tax Rate	22	Percent	Sec115BAA Taxation Laws Amendment Ordinance (2019)
Effective Income Tax Rate	7.3	Percent	LiveMint
Annual Per Judge Salary + Other costs	3.33	Million INR	Second National Judicial Pay Commission
Benefit-Cost (Tax Revenue) ($\delta = 0.05$)	6.64 [1.21]	Ratio	Calculation Bootstrapped SE
Benefit-Cost (Social) ($\delta = 0.05$)	35.12 [6.3]	Ratio	Calculation Bootstrapped SE
Benefit-Cost (Tax Revenue) ($\delta = 0.03$)	7.16 [1.28]	Ratio	Calculation Bootstrapped SE
Benefit-Cost (Social) ($\delta = 0.03$)	37.93 [6.685]	Ratio	Calculation Bootstrapped SE
Benefit-Cost (Tax Revenue) ($\delta = 0.1$)	5.52 [1.052]	Ratio	Calculation Bootstrapped SE
Benefit-Cost (Social) ($\delta = 0.1$)	29.16 [5.47]	Ratio	Calculation Bootstrapped SE

Notes: I focus on the event of positive staffing change to compute benefit-cost ratios. I calculate effective income tax incidence on salaried individual tax payer using average reported annual income of INR 690,000 and the applicable progressive tax slab on this reported income: income upto INR 500,000 is exempt and the remaining INR 190,000 is taxed at 20%. This gives an effective average tax incidence of 7.3%. Corporate tax rate of 22% is the rate applicable on reported corporate income for domestic companies. Bootstrapped standard errors from 1000,000 random draws are reported in square brackets below the cost-benefit estimates. Figure A.17 shows the distribution of the benefit-cost ratio following the bootstrapping procedure.

Online Appendix
For Online Publication Only

A.1 Additional details on the context

District courts across India have over 18 million legal cases pending for 3 or more years as on 1st July 2023. This translates to 1059 pending cases per judge (the total sanctioned judgeships for district courts is 22677 of which only about 17000 are non-vacant positions). While the US has a slightly different structure of the judiciary, I examine the extent of backlog in both federal as well as state-level frontline courts. US federal district courts have 0.128 million cases pending over 3 years. With 677 federal district judges, this translates to 189 pending cases per judge. Among states, I consider top five most populous states: California, Texas, Florida, New York, and Pennsylvania. California has 39 million population (12% of US population) and about 0.8 million pending over 3 years, which implies 400 pending cases per judge across 2000 judges in California superior courts.[1] Statutory county courts of Texas have about 0.6 million legal cases pending in total. With about 9% US population, 765 active judges, and 4947 assigned judges (including retired judges), this translates to 121 pending case per judge. Florida and New York states have close to 100% clearance rates, with no pending cases over 3 years. Lastly, Pennsylvania with 13 million population (4% of US population) has 44046 cases pending over 3 years across 458 judges, translating to 96 pending cases per judge. This exercise reveals substantial heterogeneity within the US, but even with these differences, most states strive to keep their backlogs low with a specific attention to resolving pending backlog within 3 years. Comparing the backlog of cases per judge between district courts in India with that of relevant frontline courts in the US, the magnitude in India is about 10 times more severe. See Figure A.18 for a cross-country comparison using data by the World Bank on duration of contractual trials in frontline courts and GDP per capita. Unfortunately, no dataset exists that provides information on pending case backlog per judge across cuntries.[2]

Availability of adequate number of judges is among the key constraints in resolving case backlog in courts. There are fewer than 20 judge posts per million population in India in contrast to 70 per million recommended by the United Nations. This ratio worsens after

[1] As per the reports, there are 10 million cases pending in total across all superior courts in California. While there is no breakdown by years pending, about 90% cases are shown as resolved within 24 months. Using this, I assumed 0.8 million pending over 3 years.

[2] Data on US federal courts from uscourts.gov, on California courts from courts.ca.gov, on Texas courts from txcourts.gov, on Florida courts from flcourts.gov, on New York courts from nycourts.gov, on Pennsylvania courts from pacourts.us.

taking into account the extent of vacancies. Staff vacancies and its sporadic redressal is a fundamental problem in bureaucratic organizations worldwide, and is particularly acute in India. For example, vacancy rates are close to 10% across superior courts in California, USA, and over 25% across district courts in India.[3]

A.2 Data Construction

A.2.A. Matching firms with trial data

I follow the steps below to match firms with registered trials in the e-courts database:[4]

1. Identify the set of trials involving firms on either sides of the litigation (i.e. either as a plaintiff/petitioner, or as a defendant/respondent, or as both) using specific naming conventions followed by firms during registration. Common patterns include firm names starting with variants of "M/S", ending with variants of "Ltd", and so on. This results in 1.2 million trials, or 20% of the trial dataset being identified as those involving firms.

2. Create a set of unique firms appearing in above dataset. I note that same firm could appear as a litigant in more than one district. Procedural laws pertaining to civil and criminal procedures determine where a specific litigation can be filed based on the issue under litigation.

3. Map firm names as they appear in the trial data in step 2 with firm names as they appear in Prowess dataset using common patterns with the aid of regular expressions. This also accounts for extra spaces, punctuation marks, as well as common spelling errors such as interchanging of vowels. Further, I also account for abbreviations. For example, "State Bank of India" appears in the trial dataset as "State Bank of India", "SBI", S.B.I", and similar variants. I map all these different spellings to the same entity "State Bank of India".

4. Remove matches where firm names are used as landmark in the addresses of litigants. To do this, I detect prefix words such as "opposite to" "above", "below", "near", and "behind" followed by a firm name.

[3] I accessed aggregate court statistics from the National Judicial Data Grid for all of India and personnel statistics from the India Justice Reports. US district and county courts (also called superior courts in some states) are comparable to district courts in India with similar case types and nature of disputes.

[4] Note that the firms can be engaged in litigation in any district other than their registered office location. Specifically, banking firms have ongoing trials in the court corresponding to the jurisdiction of the borrower. For matching, therefore, I employ a nested approach following above heuristics. I only retain one-to-one match between a firm and a trial.

5. Create primary key as the standardized name, from step 3 to match with both trial as well as Prowess datasets.

6. When more than one firm match with a case, that is when there are multiple entities involved as either petitioners or respondents, I select one matched firm at random. These many-to-one matches are about 5% of the matches.

A.3 A model of credit market with enforcement costs

A.3.A. Credit Market

I follow and extend the credit contract model in Banerjee and Duflo (2010) to include probability of litigation at a given rate of trial resolution in the corresponding district court. Specifically, I consider a lender-borrower sequential game with lender's choice to enforce debt contract through litigation. This is similar to the role of social sanctions in the group liability model discussed in Besley and Coate (1995). The solution to the game provides an optimal contract that details the interest rate schedule and a wealth threshold for lending.

At the start, borrower needs to invest, K, in a project which returns $f(K)$. Their exogenous wealth endowment is W. They need an additional $K_B = K - K_M$ from the lender to start the project, where K_M is the amount they raise from the market, with market return ϕ. Borrower repays RK_B at the end of the contract period, where $R = 1 + r > 1$ incorporates the interest rate r. The project succeeds with probability s, upon which the borrower decides to repay or evade. Evasion is costly as the borrower incurs an evasion cost ηK_B leading to a payoff $f(K) - \eta K_B$. The lender loses the entire principal, $-K_B$. Repayment results in $f(K) - RK_B$ as payoff to the borrower and the lender payoff is RK_B. On the other hand, the borrower automatically defaults if her project fails, in which case the lender can choose to litigate to monetize borrower's assets to recover their loan. This game is depicted in Figure A.14. Litigation is costly and lender incurs a cost, $C_L(\gamma) > 0$, $\frac{\partial C_L}{\partial \gamma} < 0$, as a function of judicial capacity, γ. The borrower can also choose to litigate with costs, $C_B(\gamma) > 0$, $\frac{\partial C_B}{\partial \gamma} < 0$, or settle out of court. Once the lender chooses to litigate and borrower accepts, lender wins with a very high probability. The intuition behind the relationship behind enforcement costs and judicial capacity can be explained by the fact that the litigants need to spend on travel, logistics, and lawyer fees over the duration of the trial, which is longer when the judicial capacity is lower.[5]

When borrower's project fails, they litigate only if the value of their assets net litigation

[5]Introducing a probability of winning, $p >> 1-p$ does not add much to the exposition and for tractability, I skip this stochastic component.

costs is positive. At the same time, the lender seeks to liquidate part of the borrower's assets, δW, to recover the loan, where δ is the depreciation rate. Lender earns a payoff of $\Gamma\delta W - C_L(\gamma)$ under litigation, where $\Gamma < 1$ is the fraction of the disputed amount that the court is able to help recover. The borrower earns a payoff $\Gamma\delta W - E[C_B(\gamma)]$, where their litigation cost $C_B(\gamma)$ is unknown ex-ante. Therefore, the condition for the borrower to accept litigation instead of opting to settle, given project failure, is

$$\Gamma\delta W - E[C_B(\gamma)] > -\delta W \implies W > \frac{E[C_B(\gamma)]}{(1-\Gamma)\delta} = \tilde{W} \tag{1}$$

This gives a distribution of borrowers, $1 - F(\tilde{W})$, likely to litigate, where $F(.)$ is their size distribution (wealth endowment). Using backward induction, litigation under project failure would be the lender's dominant strategy if

$$\begin{aligned}(1 - F(\tilde{W}))(\Gamma\delta W - C_L(\gamma)) + F(\tilde{W})\delta W > -K_B \\ \implies W > \frac{(1 - F(\tilde{W}))C_L(\gamma) - K_B}{((1 - F(\tilde{W}))\Gamma + F(\tilde{W}))\delta} = W^*\end{aligned} \tag{2}$$

This gives a minimum wealth threshold, W^*, for lending. Under project success, the borrower can choose to default if they can successfully evade. However, default gives rise to the possibility of litigation. In this situation, borrower will litigate if

$$\begin{aligned}f(K) - \Gamma RK_B - E[C_B(\gamma)] > f(K) - RK_B \\ \implies RK_B > \frac{E[C_B(\gamma)]}{(1-\Gamma)} = \delta\tilde{W}\end{aligned} \tag{3}$$

K_B mainly depends on the project and has an ex-ante distribution given by CDF, $G(.)$. R is fixed by the lender. This gives a distribution of firms willing to litigate under default as $1 - G(\tilde{W})$. Therefore, by backward induction, litigation will be lender's weakly dominant strategy if

$$\begin{aligned}(1 - G(\tilde{W}))(\Gamma RK_B - C_L(\gamma)) + G(\tilde{W})RK_B \geq -K_B \\ \implies R \geq \frac{(1 - G(\tilde{W}))C_L(\gamma) - K_B}{((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B}\end{aligned} \tag{4}$$

The possibility of default and costly litigation makes the lender account for these costs in the credit contract, by including a wealth threshold for borrowing, W^* and setting the interest rate schedule. The returns from lending to ensure adequate recovery of loan under default gives the following schedule:

$$R = \frac{(1 - G(\tilde{W}))C_L(\gamma) - K_B}{((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B} \tag{5}$$

The contract design thus generates a set of borrowers that will $\{default, litigate\}$ and another set that will either $\{default, settle\}$ or $\{repay\}$ based on their ex-ante wealth $\tilde{W}$ and project size K_B. Finally, lender's participation constraint is given by

$$s\Big(G(\tilde{W})RK_B + (1 - G(\tilde{W}))(\Gamma RK_B - C_L(\gamma))\Big) + \tag{6}$$
$$(1 - s)\Big((1 - F(\tilde{W}))(\Gamma\delta W - C_L(\gamma)) + F(\tilde{W})\delta W\Big) \geq \phi K_B$$

The timing of the game where the lender and borrower decide on their strategies are depicted as an extensive form game in Figure A.14.

Proposition 1: Litigation response from borrower As judicial capacity, γ, increases, the wealth threshold for litigation decreases. That is, $\frac{\partial \tilde{W}}{\partial \gamma} < 0$.

Proof for Proposition 1: Differentiating (1) with respect to γ gives $\frac{\partial \tilde{W}}{\partial \gamma} \propto \frac{\partial C_B(\gamma)}{\partial \gamma} < 0$.

Constraints (2) and (5) define the credit contract. Additionally $R \geq \phi$ else the lender would rather invest in external markets than engaging in lending. This gives the relationship between returns - R, borrowing - K_B, and the wealth threshold for lending - W^*, as depicted in Figure A.14.

Proposition 2: Credit market response to judicial capacity As judicial capacity, γ, increases, the credit market response varies as follows:

1. Effect on W^* is negative. That is, an increase in judicial capacity lowers the threshold of wealth required for lending.

2. Effect on R is negative for each level of borrowing. That is, the interest curve shifts inward.

3. Borrowing becomes cheaper, which expands total borrowing, particularly at lower levels of wealth W.

Proof for Proposition 2: Differentiating (2) and (5) with respect to γ yields the expressions for $\frac{\partial R}{\partial \gamma}$ and $\frac{\partial W^*}{\partial \gamma}$ as below. For the distribution functions, I assume $g(\tilde{W}), f(\tilde{W}) \to 0$ since only large firms engage in litigation.

$$\frac{\partial R}{\partial \gamma} = \frac{\overbrace{\frac{\partial C_L(\gamma)}{\partial \gamma}}^{\text{-ve}} \overbrace{(1 - G(\tilde{W}) - C_B g(\tilde{W}))}^{\text{+ve}}}{((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B}$$

$$- \frac{(1 - G(\tilde{W}))C_L(\gamma) - K_B}{(((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B)^2} \left(\overbrace{g(\tilde{W}) \frac{\partial C_B}{\partial \gamma} (K_B - \Gamma)}^{\approx 0} \right)$$

$$\implies \frac{\partial R}{\partial \gamma} < 0$$

$$\frac{\partial W^*}{\partial \gamma} = \frac{\overbrace{(1 - F(\tilde{W})) \frac{\partial C_L}{\partial \gamma} - C_L f(\tilde{W}) \frac{\partial C_B}{\partial \gamma}}^{\text{-ve}}}{((1 - F(\tilde{W}))\Gamma + F(\tilde{W}))\delta} - \frac{(1 - F(\tilde{W}))C_L(\gamma) - K_B}{(((1 - F(\tilde{W}))\Gamma + F(\tilde{W}))\delta)^2} \underbrace{f(\tilde{W}) \frac{\partial C_B}{\partial \gamma} (\delta - \Gamma)}_{\approx 0}$$

$$\implies \frac{\partial W^*}{\partial \gamma} < 0$$

A.3.B. Firm Production

Consider a representative firm with production function $Q = Q(X_1, X_2)$ where $Q(.)$ is twice differentiable, quasi-concave, and cross partials $Q_{X_1X_2} = Q_{X_2X_1} \geq 0$. Further assume that the firm is a price taker in the input market. The firm's problem is to maximize their profits as follows:

$$\text{Max}_{X_1, X_2} \big(\Pi = pQ(X_1, X_2) - w_1 X_1 - w_2 X_2 - m_i(\gamma) \big) \tag{7}$$

$$s.t \; w_1 X_1 + w_2 X_2 + m(\gamma) \leq K_i(\gamma) \; i \in \{S, L\}$$

where w_1 and w_2 are the unit costs of inputs X_1 and X_2, $m_i(\gamma)$ is the monitoring costs arising in the production process, which weakly decreases with improvements in judicial capacity, i.e. $\frac{\partial m_i}{\partial \gamma} \leq 0$. i represents firm size based on their initial wealth endowment, denoted by S for small firms and by L for large ones. Further, I assume that fixed costs form a large share of monitoring costs for small firms such that $\frac{\partial m_S}{\partial \gamma} \approx 0$ whereas for large firms, $\frac{\partial m_L}{\partial \gamma} < 0$ reflecting a lowering of the variable cost. $K = K_M + K_B$, is the total capital available to finance production, including borrowing from bank K_B as in Banerjee and Duflo (2014). From the credit market model above, we know that as judicial capacity, γ, improves, banks begin to lend to smaller firms and the overall interest rate on bank lending, $R(\gamma, .)$ drops.

Proposition 3: Effects of judicial capacity on firm production As judicial capacity,

γ, increases, the firm responds as follows:

1. Optimal input use X_1, X_2 increases on an average.
2. Output increases on an average.
3. Heterogeneity in effects on profits is as follows:
   (a) For large firms, L, optimal inputs and profits increase if decrease in monitoring costs and cheaper credit more than offsets the increase in input expenditure.
   (b) For marginal small firms, S, optimal inputs and profits increase if increase in borrowing is sufficiently large to offset the increase in input expenditure.
   (c) For inframarginal small firms, S, optimal inputs and profits remain unchanged because borrowing and monitoring costs for these firms remain unchanged.

Proof for Proposition 3: From the credit model, borrowing increases with an increase in judicial capacity i.e. $\frac{\partial K_i}{\partial \gamma} > 0$ for the marginal borrowers, i.e. those with $W \approx W^* - \epsilon$, with $\epsilon > 0$, a small positive real number.

Constrained Optimization:

$$\begin{aligned}
\mathcal{L} &= pQ(X_1, X_2) - w_1X_1 - w_2X_2 - m_i(\gamma) + \lambda\big(K_i - w_1X_1 - w_2X_2 - m_i(\gamma)\big) \\
\text{FOC:} & \\
\frac{\partial \mathcal{L}}{\partial X_1} &= pQ_{x_1} - w_1 - w_1\lambda = 0 \\
\frac{\partial \mathcal{L}}{\partial X_2} &= pQ_{x_2} - w_2 - w_2\lambda = 0 \\
\frac{\partial \mathcal{L}}{\partial \lambda} &= K_i - w_1X_1 - w_2X_2 - m_i(\gamma) = 0
\end{aligned}$$

To examine how the optimal production choices vary with exogenous variation in the institutional quality parameter, γ, I use Implicit Function Theorem where X_1, X_2, λ are endogenous variables and γ is exogenous to the firm's problem. A key distinction arises based on whether the firm belongs to the group of small or large firms. For $i = S$ and $W \approx W^* - \epsilon$, $K_i = K_M + K_B$ when γ increases. For $i = L$, $\frac{\partial K_i}{\partial \gamma} = 0$. Applying Cramer's Rule:

$$
\begin{aligned}
Det[J] &= 2pw_1w_2\underbrace{Q_{x_1x_2}}_{+\text{ve}} - p(w_2^2\underbrace{Q_{x_1x_1}}_{\text{-ve}} + w_1^2\underbrace{Q_{x_2x_2}}_{\text{-ve}}) > 0 \\
\frac{\partial X_1}{\partial\gamma} &= -\frac{Det[J_{x_1}]}{Det[J]} = -\frac{\overbrace{p(\frac{\partial K_i}{\partial\gamma} - \frac{\partial m_i}{\partial\gamma})}^{+ve}(w_1\overbrace{Q_{x_2x_2}}^{\text{-ve}} - w_2\overbrace{Q_{x_1x_2}}^{+\text{ve}})}{Det[J]} > 0 \\
\frac{\partial X_2}{\partial\gamma} &= -\frac{Det[J_{x_2}]}{Det[J]} = -\frac{\overbrace{p(\frac{\partial K_i}{\partial\gamma} - \frac{\partial m_i}{\partial\gamma})}^{+ve}(w_2\overbrace{Q_{x_1x_1}}^{\text{-ve}} - w_1\overbrace{Q_{x_2x_1}}^{+\text{ve}})}{Det[J]} > 0 \\
\frac{\partial\lambda}{\partial\gamma} &= -\frac{Det[J_\lambda]}{Det[J]} = -\frac{p^2\overbrace{(\frac{\partial K_i}{\partial\gamma} - \frac{\partial m_i}{\partial\gamma})}^{+ve}(\overbrace{Q_{x_1x_1}Q_{x_2x_2} - Q_{x_2x_1}Q_{x_1x_2}}^{\text{depends on functional form}})}{Det[J]} = ?
\end{aligned}
$$

This implies that the optimal input choices increase for all firms with an improvement in contract enforcement through local courts. On the other hand, how the shadow value responds depends on the functional form of the underlying production function. For example, if the production function is Cobb Douglas, then $\frac{\partial\lambda}{\partial\gamma} = 0$.

Finally, an application of the envelope theorem enables examining how the value function changes with the exogenous court performance, γ:

$$
\frac{dV(\gamma)}{d\gamma} = \frac{\partial\Pi^*}{\partial\gamma} + \lambda\frac{\partial g^*(\gamma)}{\partial\gamma} \text{ where } g(.) \text{ is the constraint}
$$

$$
\begin{aligned}
\frac{\partial\Pi^*}{\partial\gamma} &= \underbrace{(pQ_{x_1} - w_1)}_{\text{This is } w_1\lambda}\frac{\partial X_1^*}{\partial\gamma} + \underbrace{(pQ_{x_2} - w_2)}_{\text{This is } w_2\lambda}\frac{\partial X_2^*}{\partial\gamma} - \underbrace{\frac{\partial m_i}{\partial\gamma}}_{\text{-ve}} > 0 \\
\frac{\partial g^*}{\partial\gamma} &= \underbrace{(\frac{\partial K_i}{\partial\gamma} - \frac{\partial m_i}{\partial\gamma})}_{\text{marginal benefit}} - \underbrace{\Big(w_1\frac{\partial X_1^*}{\partial\gamma} + w_2\frac{\partial X_2^*}{\partial\gamma}\Big)}_{\text{marginal cost}}
\end{aligned}
$$

$\frac{\partial g^*}{\partial\gamma} > 0$ if marginal benefits from an improvement in judicial capacity exceeds marginal cost, in which case, welfare improves. If this is not true, then the welfare effect is potentially ambiguous. Heterogeneity based on firm size distribution imply:

1. For large firms, $i = L$, the marginal benefit $0 - \frac{\partial m_L}{\partial\gamma}$ is mainly due to reduction in monitoring costs since there is no change in their borrowing from banks. If this reduction

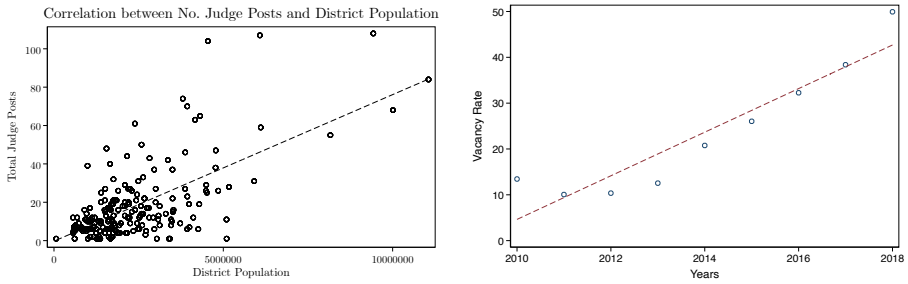
in monitoring costs is greater than the marginal increase in input costs, then profits for such firms will increase.

2. For marginal small firms, $i = S$ and $W \approx W^* - \epsilon$, the marginal benefit $K_B - \frac{\partial m_S}{\partial \gamma}$ is due to both availability of borrowing from banks K_B as well as a reduction in monitoring costs. I assume that the monitoring costs for small firms do not decrease substantially since a large share is fixed cost for these firms. If the increase in borrowing is large enough to offset the increase in input costs, then profits for such firms will increase.

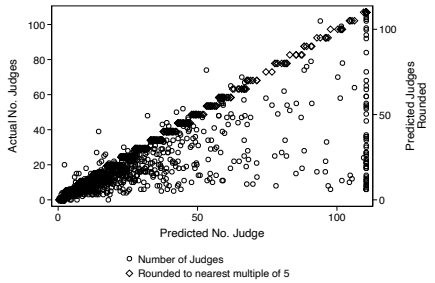
3. For inframarginal small firms, $i = S$ and $W << W^*$, neither their optimal inputs nor their profits change since $(\underbrace{\frac{\partial K_S}{\partial \gamma}}_{=0} - \underbrace{\frac{\partial m_S}{\partial \gamma}}_{\approx 0}) \approx 0$.

A.4 Appendix: Figures

Figure A.1: Judge Posts, Vacancy, and District Population
Panel A: Court-size, vacancy, and district population

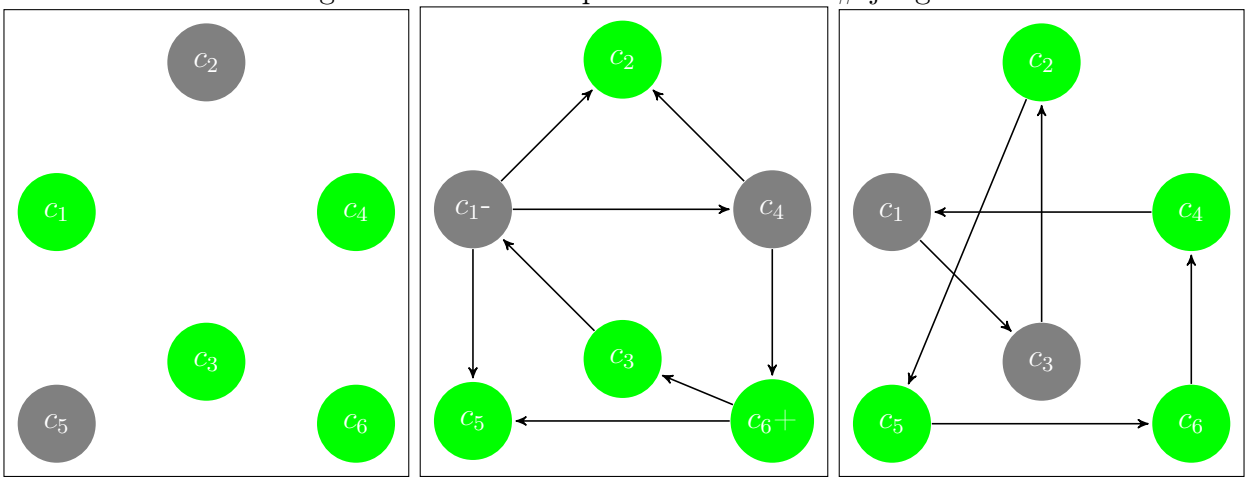


Panel B: Actual Number of Judges vs. Law Commission Recommendation



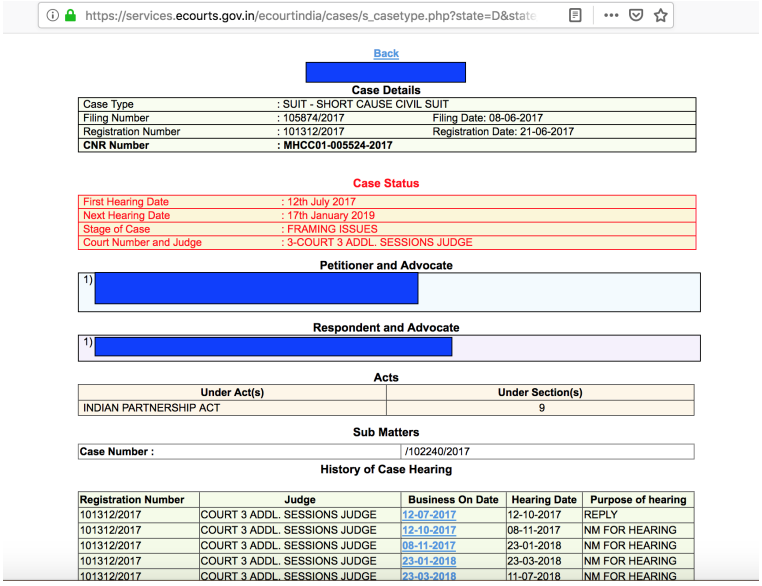
Notes: Y axis presents total number of judge posts across the sample courts. X-axis is the district population as measured in 2011 census. In the bottom panel, I plot the observed number of judges in a district court-year on the left y-axis, predicted number of judges based on the Law Commission Report No. 245 on the x-axis, and the predicted number rounded to the nearest multiple of 5 on the right y-axis. If the high courts followed the algorithm subject to integer rounding, the relationship between observed number of judges and predicted number of judges should follow a step function as shown in diamond markers.

Figure A.2: An example of variation in # judges



Notes: This graphic represents a stylized example of net judge staffing changes over time. Panel 1 presents t=0, Panel 2 - t=1, and Panel 3 - t=2. A node refers to a district court. Green node implies no judge vacancy and gray node implies some judge vacancy. At the end of t=0 and t=1, there are staffing changes arising from recruitments, retirements, and rotations, with rotations represented by directed arrows in Panels 2 and 3. The direction of the arrows in Panels 2 and 3 indicate judge rotation, from origin to destination courts. The + and − inside the nodes indicate addition of a newly recruited judge and retirement, respectively. The node colors in Panels 2 and 3 presents the resulting implications of staffing changes on judge vacancies in the sample courts in t=1 and t=2, respectively. At t=1, C2 and C5 no longer have any vacancy whereas C1 and C4 experience vacancy as a result of these dynamics. C3 and C6 remain at full occupancy at both t=0 and t=1. At t=2, C3 experiences a vacancy whereas C4 is back at full staffing levels. All the other courts experience no net change between t=1 and t=2.

Figure A.3: Individual Case Record Example


https://services.ecourts.gov.in/ecourtindia/cases/s_casetype.php?state=D&state

Back

[redacted]

Case Details

Case Type	: SUIT - SHORT CAUSE CIVIL SUIT	
Filing Number	: 105874/2017	Filing Date: 08-06-2017
Registration Number	: 101312/2017	Registration Date: 21-06-2017
CNR Number	**: MHCC01-005524-2017**	

Case Status

First Hearing Date	: 12th July 2017
Next Hearing Date	: 17th January 2019
Stage of Case	: FRAMING ISSUES
Court Number and Judge	: 3-COURT 3 ADDL. SESSIONS JUDGE

Petitioner and Advocate

1) [redacted]

Respondent and Advocate

1) [redacted]

Acts

Under Act(s)	Under Section(s)
INDIAN PARTNERSHIP ACT	9

Sub Matters

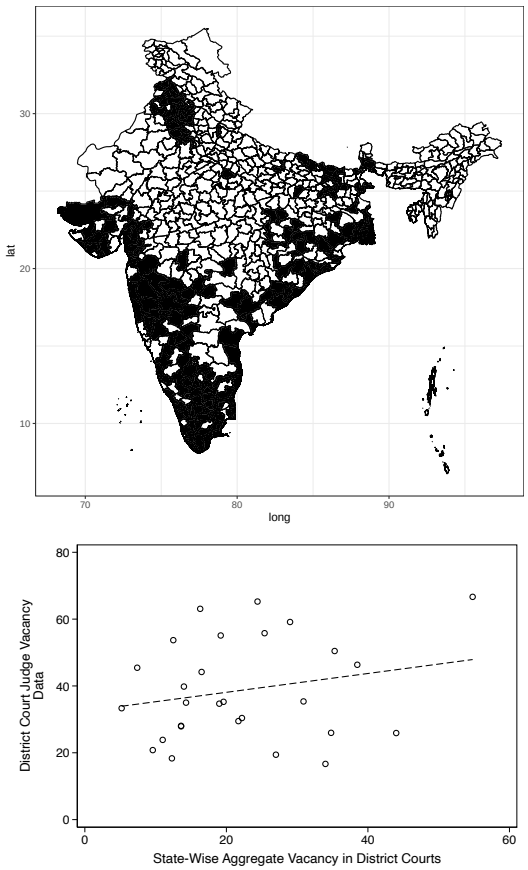
Case Number :	/102240/2017

History of Case Hearing

Registration Number	Judge	Business On Date	Hearing Date	Purpose of hearing
101312/2017	COURT 3 ADDL. SESSIONS JUDGE	12-07-2017	12-10-2017	REPLY
101312/2017	COURT 3 ADDL. SESSIONS JUDGE	12-10-2017	08-11-2017	NM FOR HEARING
101312/2017	COURT 3 ADDL. SESSIONS JUDGE	08-11-2017	23-01-2018	NM FOR HEARING
101312/2017	COURT 3 ADDL. SESSIONS JUDGE	23-01-2018	23-03-2018	NM FOR HEARING
101312/2017	COURT 3 ADDL. SESSIONS JUDGE	23-03-2018	11-07-2018	NM FOR HEARING

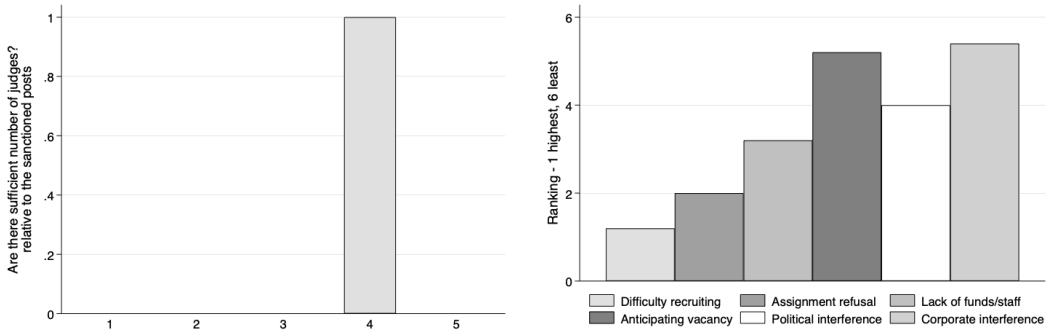
Notes: Names have been redacted for privacy.

Figure A.4: Sample district courts



Notes: 7 of 14 states in the sample include over two-thirds of their districts. Gujarat, Punjab, and Tamil Nadu are among the top industrialized states and have over 80% of their districts in the study sample. The bottom panel compares vacancy rates as generated using data with aggregate vacancy rates reported at the state-level. Key difference between the two measures is that the data-driven approach only uses sample districts whereas the official statistics uses reported data from all district and sub-district courts within the state.

Figure A.5: Stated Reasons for Judge Vacancy



Notes: The figures document key reasons for the persistence of district judge vacancy through qualitative interviews with stakeholders engaged in recruitment and assignment of district judges in 5 states. The figure on the left shows that in 4 of 5 states, there aren't enough number of judges as the number of sanctioned judgeships (x axis records the state id and the y axis records the binary response - Yes (1) and No (0) - to the question "are there sufficient number of judges relative to the sanctioned posts"). The figure on the right presents the ranking of different plausible reasons for judge vacancy. A lower score indicates higher ranking. For example, difficulty in recruiting suitable candidates or failed search is among the highest ranked reason across the 5 states I interviewed.

Figure A.6: Construction of sample of firms

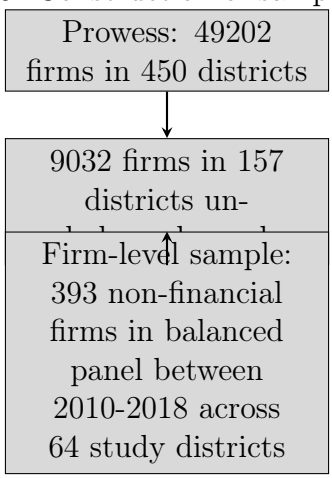
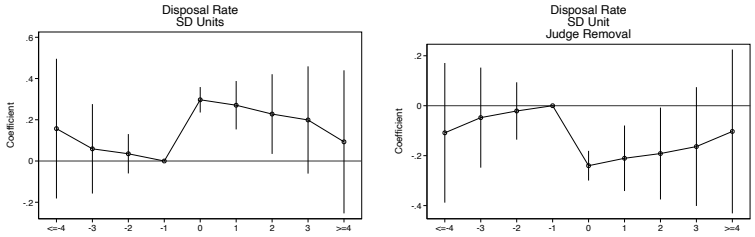
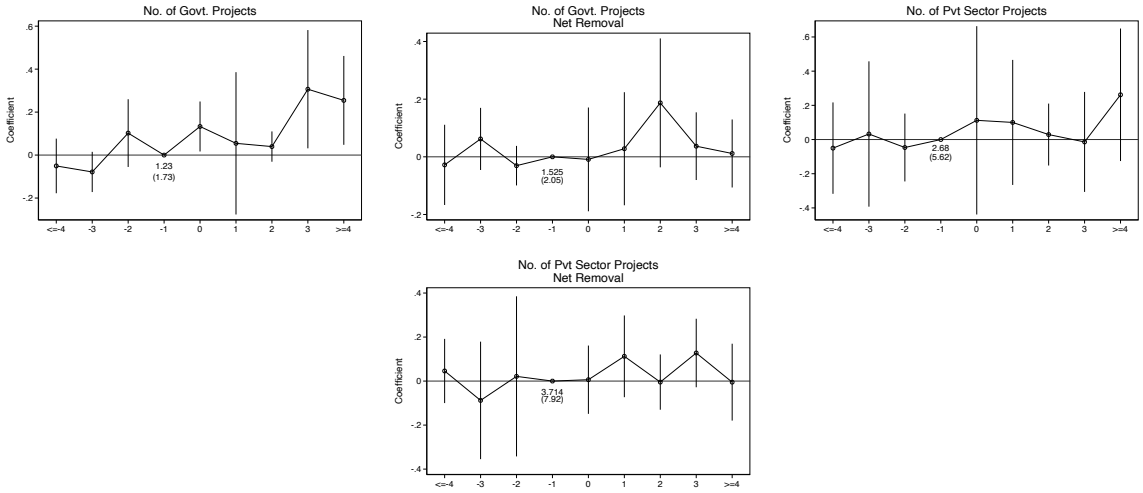


Figure A.7: Multiple Event Study Estimator: Simulation



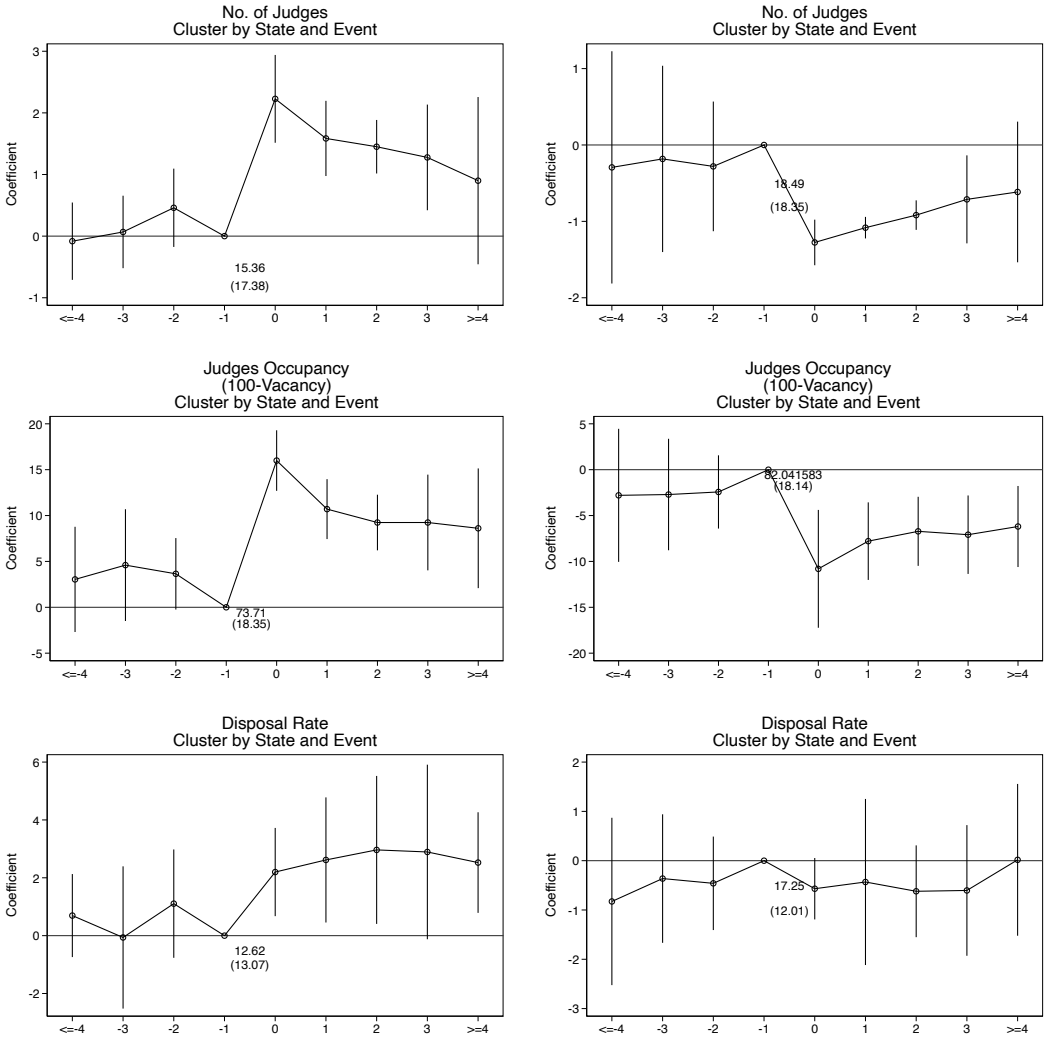
Notes: The above graphs present estimation of the treatment effects using the stacked event study estimator for multiple events using simulated data. The DGP of the number of judges and disposal rate follows an AR(1) process with random shocks introduced by either a positive or negative staffing-level change event of equal magnitudes (0.3 standard deviation units). Each district court is randomized to have 2 positive and 3 negative shocks to the number of judges over a span of 9 years.The starting values for both number of judges and disposal rate is random, drawn from a uniform and gamma distribution, respectively, with parameters matching data. The idiosyncratic error term for the number of judges is drawn from a uniform distribution whereas for disposal rate, it is distributed as a gamma function, mimicking data.

Figure A.8: Correlation of Government or Private Sector Investments with Staffing Changes



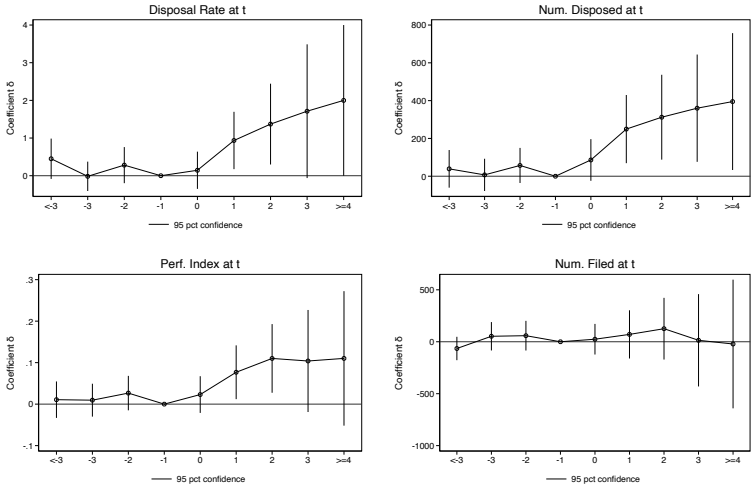
Notes: The figures present the stacked event study estimates using the number of government and private investment projects in a particular district as an outcome variable as in Equation 1. The outcome variable is obtained from CMIE CapEx database of investments, including infrastructure and those generating capacity for setting up plants. Each estimate includes 95% confidence interval. Standard errors are clustered by district and event. These test whether staffing changes correlate with pre-existing stakes (in terms of investment and budgetary allocation) of the government or private sector in the location. For example, one may be concerned that more judges are added to locations where government or private sector players have committed to investments. Presence of significant correlation could suggestion a violation of the exogeneity in the timing assumption. Another concern is that the observed treatment effects could be due to other interventions if staffing changes are bundled with changes to budgetary allocations to a district. The above figures show that these concerns do not threaten the interpretation of the study results.

Figure A.9: Court Outcomes: Inference Robustness



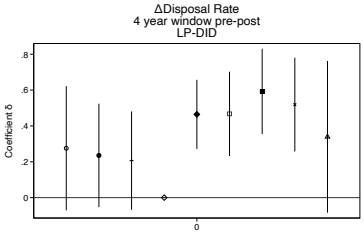
Notes: The figures plot the event study interaction coefficients from estimating Equation 1. Standard errors are clustered by state (instead of district) and event. Error bars present 95% confidence interval.

Figure A.10: Court Outcomes: Continuous Explanatory Variable



Notes: The figures present the generalized event study estimates relative to number of judges from $t+1$ when the court-level outcomes are measured at t as in Equation 2. The value labels on the x-axis needs to be interpreted differently from those in standard event study figures - positive integers refer to the regression coefficient on lagged explanatory variable by period indicated by the integer and negative integers refer to the coefficients on lead variables. For example, regression coefficient corresponding to 1 in the figures is the coefficient on $\Delta x_{i,t-1}$ and -1 corresponds to $\Delta x_{i,t+1}$ in Equation 2. The coefficients on the lead variables indicate whether the number of judges is itself determined by the existing workload in the courts. As noted in these figures, none of the different court performance indicators either significantly or economically meaningfully correlate with the next period staffing levels. In addition to disposal rate, the analysis includes cases resolved, new cases filed, and an index incorporating other possible court-level performance outcomes including appeals, dismissals, and percent uncontested. Each estimate includes 95% confidence interval. Standard errors are clustered by district.

Figure A.11: Local Projection DID: Court Performance

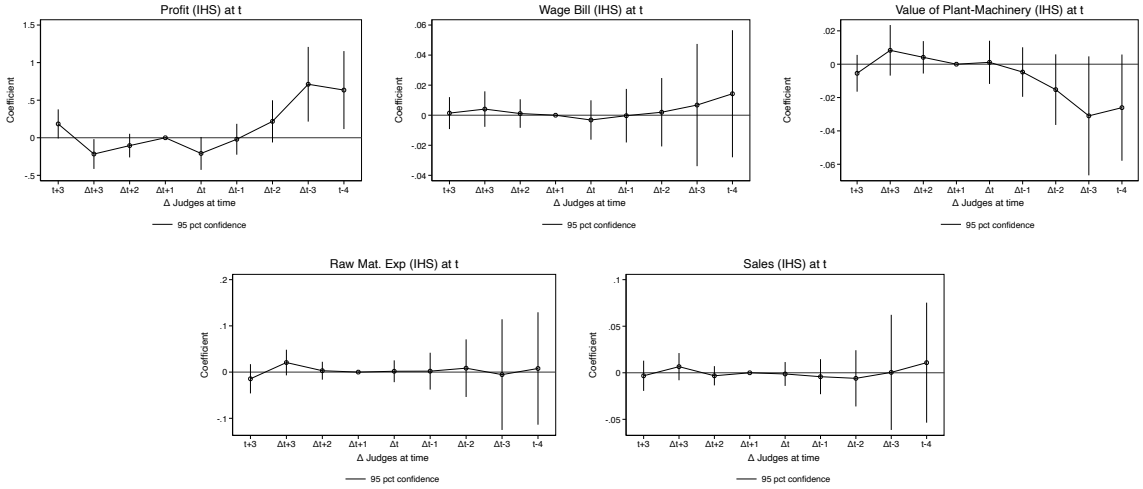


Notes: Following Dube et al. (2022), the local projection DID specification accounts for empirical challenges arising from impulse response functions generated by judicial staffing changes that occur many times and in opposing directions within the study period, similar to events in finance. Each coefficient in the graphs above represent a separate specification as follows with $k = -4, -3, ..., 3, 4$, i representing the unit of observation - firm or a district, and d referring to the corresponding district-court:

$$y_{i,t+k} - y_{i,t-1} = \beta_k \Delta NumJudges_{d,t} + \alpha_d + \delta_t + \epsilon_{i,t}$$

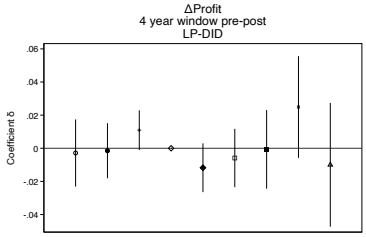
where Δ is the first difference operator.

Figure A.12: Firm-Level Outcomes: Continuous Explanatory Variable



Notes: The figures present the generalized event study estimates relative to number of judges from $t+1$ when the firm-level outcome is measured at t as in Equation 2. Each estimate includes 95% confidence interval. Standard errors are clustered by district.

Figure A.13: Local Projection DID: Firm Productivity

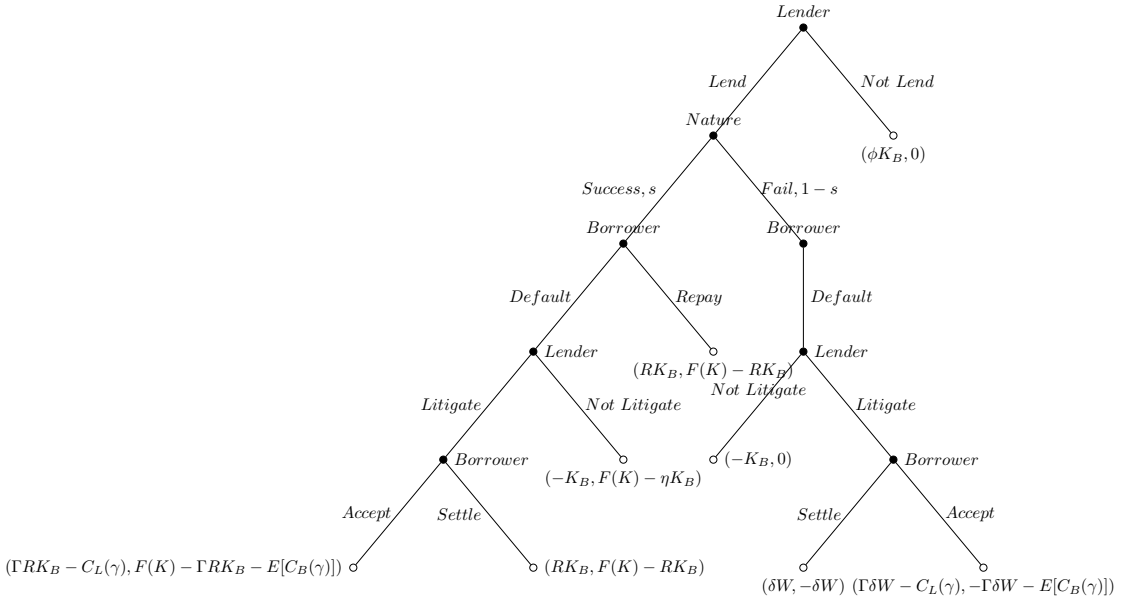


Notes: Following Dube et al. (2022), the local projection DID specification accounts for empirical challenges arising from impulse response functions generated by judicial staffing changes that occur many times and in opposing directions within the study period, similar to events in finance. Each coefficient in the graphs above represent a separate specification as follows with $k = -4, -3, ..., 3, 4$, i representing the unit of observation - firm or a district, and d referring to the corresponding district-court:

$$y_{i,t+k} - y_{i,t-1} = \beta_k \Delta NumJudges_{d,t} + \alpha_d + \delta_t + \epsilon_{i,t}$$

where Δ is the first difference operator.

Figure A.14: Model: Credit and Litigation



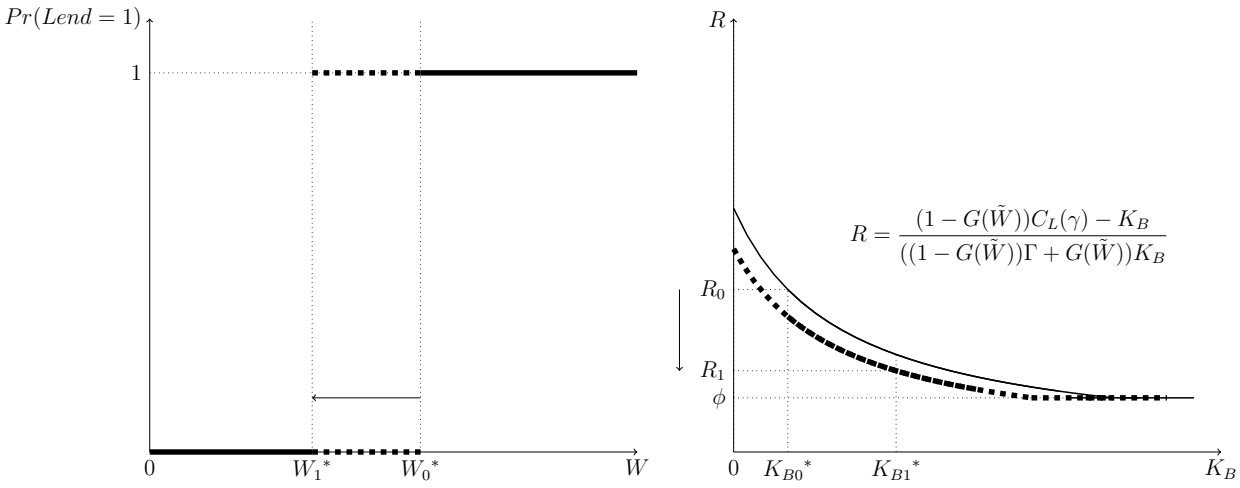
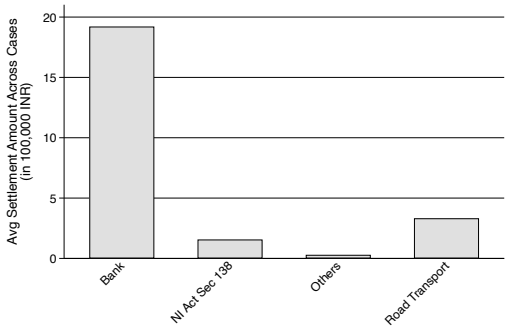
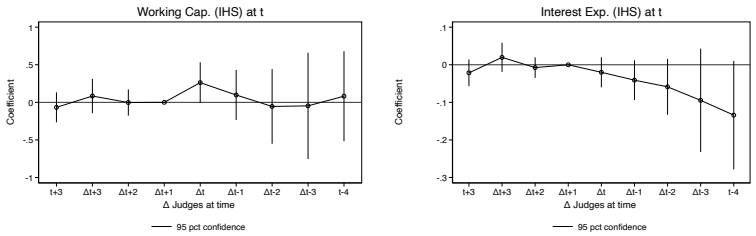


Figure A.15: Value of Dispute in Legal Cases

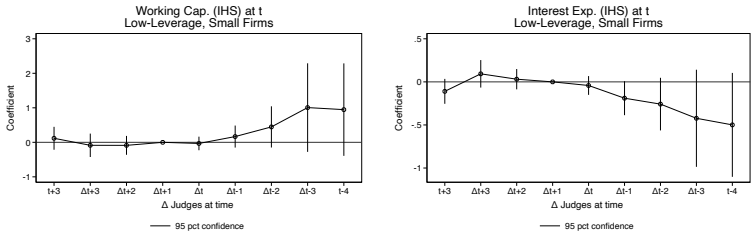


Notes: The figure presents data on settlement amount from codified judgement documents from one court only for illustration.

Figure A.16: Firms' Credit Outcomes: Continuous Explanatory Variable
Panel A: Firm-level Working Capital and Interest Expenditure - All Sample Firms

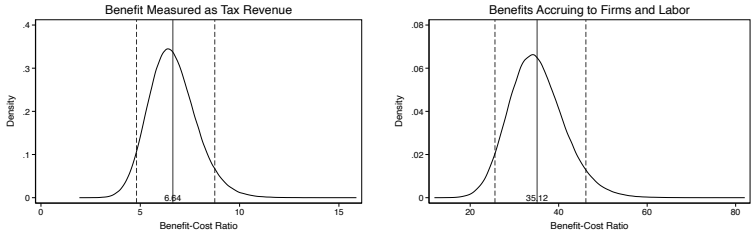


Panel B: Subsample of Low-Leverage, Small Sized Firms



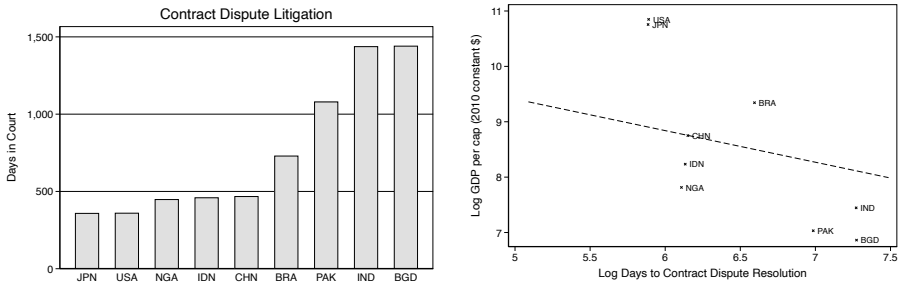
Notes: The figures present the generalized event study estimates relative to number of judges from $t+1$ when the outcome is measured at t as in Equation 2. Panel A presents the coefficients using firm-level working capital and interest expenditure across all firms in the main sample. Panel B presents the coefficients using outcomes on the subsample of low-leverage, small-sized firms. Each estimate includes 95% confidence interval. Standard errors are clustered by district.

Figure A.17: Benefit-Cost Ratio



Notes: Average benefit-cost ratio from tax-revenue perspective is 6.64, with 90% confidence interval [4.81, 8.75]. The ratio computed using benefit accruing to firms and labor is 35.12, with 90% confidence interval [25.6, 46.15]. These are calculated through bootstrapping procedure with 1000,000 draws from random normal distributions using the parameter estimates from net judge additions and their standard errors on total number of judges, profits, and wage bill. Standard errors of the benefit-cost ratios are calculated as bootstrapped standard errors.

Figure A.18: Court Congestion: Top 10 Populous Countries



Data from the World Bank. Cross-country regression of log GDP per capita on log litigation duration yields a coefficient of -0.57 with standard error 0.25. The graph above plots only top 10 populous countries for clarity of illustration.

A.5 Appendix: Tables

Table A.1: Pairwise Correlations Between Different Measures of Court Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Disposal Rate (1)	1.00						
Number Filed (2)	0.2689	1.00					
Number Disposed (3)	0.2497	0.8820	1.00				
Case Duration (4)	-0.1912	-0.1448	-0.0465	1.00			
Share Uncontested (5)	-0.1078	0.1172	0.1225	0.0555	1.00		
Share Dismissed (6)	0.1317	0.0188	-0.0268	-0.1258	0.0932	1.00	
Share Appealed (7)	-0.0811	-0.1593	-0.1787	0.0284	-0.2087	0.2174	1.00
Observations	1755						

Notes: All measures of court performance are constructed using the trial-level data, aggregated by court-year. Case duration is measured in number of days. Share uncontested is the percentage of resolved cases that are not contested by either of the litigants. Share dismissed is the percentage of resolved cases that are dismissed without full trial and judgement order. Share appealed is the percentage of newly filed cases that are appeals against decisions from lower courts within the district court's jurisdiction.

Table A.2: Court Outcomes and Judge Vacancy Changes

	Net Judge Addition			Net Judge Removal		
	(1) No. of Judges	(2) 100 - Vacancy Rate	(3) Disposal Rate	(4) No. of Judges	(5) 100 - Vacancy Rate	(6) Disposal Rate
Event x <=-4	-0.0821	3.041	0.694	-0.293	-2.796	-0.827
	(0.307)	(2.717)	(0.566)	(0.689)	(3.432)	(0.774)
Event x -3	0.0678	4.598	-0.0628	-0.182	-2.708	-0.363
	(0.289)	(2.874)	(0.943)	(0.586)	(2.799)	(0.598)
Event x -2	0.460	3.650*	1.106*	-0.280	-2.427	-0.459
	(0.306)	(1.816)	(0.606)	(0.415)	(1.838)	(0.397)
Event x 0	2.228***	15.99***	2.199***	-1.276***	-10.81***	-0.569***
	(0.282)	(0.954)	(0.628)	(0.161)	(2.748)	(0.154)
Event x 1	1.585***	10.70***	2.617***	-1.082***	-7.790***	-0.432
	(0.256)	(1.031)	(0.711)	(0.0937)	(1.745)	(0.721)
Event x 2	1.451***	9.240***	2.964**	-0.918***	-6.719***	-0.621
	(0.199)	(1.043)	(1.184)	(0.0505)	(1.696)	(0.394)
Event x 3	1.277***	9.243***	2.893**	-0.712***	-7.086***	-0.604
	(0.326)	(1.820)	(1.320)	(0.125)	(1.917)	(0.627)
Event x >=4	0.900	8.612***	2.526**	-0.615	-6.193**	0.0171
	(0.558)	(2.710)	(0.945)	(0.407)	(2.183)	(0.748)
Observations	9162	9162	9162	9162	9162	9162
No. Districts	195	195	195	195	195	195

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

Notes: This table presents the estimates from Equation 1 using court-level outcomes, equivalent to Figure 1. Columns 1-3 present estimates following net judge increases whereas Columns 4-6 present those following net judge reductions. All court-level specifications include district and state-year fixed effect. Standard errors are clustered by district and event.

Table A.3: Heterogeneity in Judge Staffing Levels

	Net Judge Addition			Net Judge Removal		
	(1) 1st Tercile Population	(2) 2nd Tercile Population	(3) 3rd Tercile Population	(4) 1st Tercile Population	(5) 2nd Tercile Population	(6) 3rd Tercile Population
Event x <=-4	0.658	-0.122	-0.174	0.126	-0.0597	-0.464
	(0.556)	(0.604)	(0.826)	(0.487)	(0.449)	(0.396)
Event x -3	0.251	0.217	-0.160	0.134	-0.157	-0.264
	(0.345)	(0.501)	(0.400)	(0.385)	(0.468)	(0.388)
Event x -2	0.323	0.500	0.680	-0.0462	-0.189	-0.426
	(0.247)	(0.406)	(0.443)	(0.272)	(0.326)	(0.390)
Event x 0	1.491***	1.742***	2.848***	-1.134***	-1.112***	-1.273***
	(0.273)	(0.297)	(0.653)	(0.238)	(0.184)	(0.319)
Event x 1	0.894***	0.928***	2.509***	-1.021**	-0.938***	-1.102***
	(0.264)	(0.117)	(0.695)	(0.372)	(0.200)	(0.241)
Event x 2	0.922***	0.628***	2.501**	-0.834	-0.941***	-0.971***
	(0.242)	(0.117)	(0.920)	(0.510)	(0.215)	(0.131)
Event x 3	0.423	0.569	2.932***	-0.466	-0.937***	-0.984***
	(0.562)	(0.326)	(0.357)	(0.627)	(0.174)	(0.198)
Event x >=4	-0.139	0.833*	2.166***	0.0194	-0.982***	-0.913*
	(0.876)	(0.386)	(0.127)	(0.758)	(0.261)	(0.421)
Observations	2988	3042	2988	2988	3042	2988
No. Districts	71	64	57	71	64	57

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

Notes: This table presents the event study reduced form estimates of judge staffing changes on the number of judge in a year using different subsets of the sample by underlying district population.

Table A.4: Caseload Outcomes

	Net Judge Addition			Net Judge Removal		
	(1) No. Filed	(2) No. Resolved	(3) Perc. Appeal	(4) No. Filed	(5) No. Resolved	(6) Perc. Appeal
Event x <=-4	260.0	436.0*	-0.500	-36.81	-152.6	0.384
	(161.5)	(213.1)	(0.496)	(153.6)	(204.0)	(0.471)
Event x -3	65.23	93.38	0.196	-23.92	-80.41	-0.0926
	(105.7)	(98.07)	(0.533)	(128.4)	(217.5)	(0.272)
Event x -2	177.3**	143.5	0.923**	-68.69	-119.0	0.0816
	(67.19)	(148.9)	(0.385)	(125.0)	(158.5)	(0.192)
Event x 0	243.7	270.6*	0.143	-91.27	-163.7**	0.0248
	(156.7)	(137.8)	(0.334)	(72.22)	(58.00)	(0.555)
Event x 1	215.3	173.2	-0.180	44.04	-0.897	0.00462
	(308.8)	(268.8)	(0.357)	(68.87)	(104.7)	(0.521)
Event x 2	472.0	386.3	-0.982*	-8.926	-50.67	0.343
	(338.3)	(338.6)	(0.491)	(111.2)	(156.3)	(0.502)
Event x 3	436.9	436.6	-0.251	-27.97	-126.4	0.151
	(329.3)	(516.8)	(0.547)	(135.2)	(221.6)	(0.377)
Event x >=4	442.2	398.7	-0.548	16.49	42.24	0.518
	(316.6)	(399.2)	(0.403)	(180.2)	(250.9)	(0.295)
Observations	9162	9162	9162	9162	9162	9162
No. Districts	195	195	195	195	195	195

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

Notes: This table presents the estimates from Equation 1 using other court-level outcomes including a breakdown of caseload by newly filed and resolved as well as the composition of cases that are appeals from lower courts. Columns 1-3 presents estimates for net judge addition and Columns 4-6 for net judge reduction. All court-level specifications include district fixed effect. Standard errors are clustered by district and event.

Table A.5: Heterogeneity in Court Performance: Disposal Rate

	Net Judge Addition			Net Judge Removal		
	(1) 1st Tercile Population	(2) 2nd Tercile Population	(3) 3rd Tercile Population	(4) 1st Tercile Population	(5) 2nd Tercile Population	(6) 3rd Tercile Population
Event x <=-4	0.901	-0.206	0.0257	-1.190	-1.000	0.0712
	(1.840)	(0.818)	(0.991)	(1.620)	(0.569)	(0.853)
Event x -3	-0.519	-2.373*	1.114	-0.290	-0.674	-0.553
	(1.728)	(1.191)	(0.865)	(2.146)	(0.672)	(0.765)
Event x -2	0.667	0.544	1.155	-0.857	-0.465	-0.415
	(1.637)	(0.912)	(0.985)	(1.228)	(0.632)	(0.426)
Event x 0	1.766*	1.605**	1.329*	-0.209	-0.173	-0.988***
	(0.830)	(0.709)	(0.655)	(0.276)	(0.261)	(0.276)
Event x 1	2.062**	1.985	1.560*	-0.739*	-0.180	-0.585
	(0.784)	(2.478)	(0.843)	(0.402)	(1.048)	(0.611)
Event x 2	2.043*	3.425	1.450	-0.208	-0.508	-1.091
	(0.920)	(2.549)	(0.864)	(0.280)	(1.086)	(0.636)
Event x 3	2.257	3.074*	0.941	-0.437	-0.511	-0.989*
	(1.318)	(1.682)	(1.187)	(0.875)	(0.855)	(0.456)
Event x >=4	1.693	3.422**	0.300	-0.0554	0.643	-0.513
	(1.515)	(1.407)	(1.306)	(0.432)	(1.286)	(0.738)
Observations	2988	3042	2988	2988	3042	2988
No. Districts	71	64	57	71	64	57

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

Notes: This table presents the event study reduced form estimates of staffing changes on court-level disposal rate using different subsets of the sample by underlying district population.

Table A.6: Local Firms' Outcomes: Net Judge Addition

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap. (IHS)	(7) Interest Exp (IHS)
Pos x <=-4	0.0162	-0.0500	-0.0234	0.0256	-0.217	0.167	0.103
	(0.0535)	(0.0818)	(0.0658)	(0.0712)	(0.200)	(0.394)	(0.0729)
Pos x -3	0.000279	0.0162	-0.0505	0.0120	0.135	0.0202	0.0883*
	(0.0350)	(0.0245)	(0.0882)	(0.0397)	(0.129)	(0.188)	(0.0446)
Pos x -2	0.00715	0.00361	0.00903	0.0181	0.193	0.111	0.0957**
	(0.0294)	(0.0388)	(0.0429)	(0.0626)	(0.382)	(0.0673)	(0.0341)
Pos x 0	-0.00187	0.0179	0.0171	0.0201***	0.110	0.389***	-0.00813
	(0.0203)	(0.0120)	(0.0392)	(0.00418)	(0.0935)	(0.0742)	(0.0243)
Pos x 1	0.0196	0.00435	0.0253	0.0184	0.418***	0.200	-0.0864**
	(0.0213)	(0.00520)	(0.0636)	(0.0180)	(0.113)	(0.139)	(0.0377)
Pos x 2	0.0207	-0.00149	0.0717	0.0210	0.310**	0.172	-0.0802**
	(0.0228)	(0.0192)	(0.0480)	(0.0191)	(0.115)	(0.157)	(0.0314)
Pos x 3	0.0369*	0.0266	0.0401**	0.0360**	0.462***	0.275***	-0.0817**
	(0.0202)	(0.0366)	(0.0158)	(0.0126)	(0.114)	(0.0757)	(0.0295)
Pos x >=4	0.0514**	0.0194	0.0336***	0.0289***	0.334***	0.244**	-0.0903***
	(0.0216)	(0.0368)	(0.0107)	(0.00581)	(0.0703)	(0.0911)	(0.0131)
Observations	22752	22752	22752	22752	22752	22752	22752
No. Firms	393	393	393	393	393	393	393
No. Districts	64	64	64	64	64	64	64

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

Notes: This table presents the estimates from Equation 1 using firm-level outcomes, equivalent to Figure 2, for net judge addition. IHS refers to inverse hyperbolic sine function. Using logarithmic transformation instead of arcsine yields similar estimates. I restrict the firms sample to a balanced panel in order to ensure no endogenous missing values of firm-level outcomes. All firm-level specifications include firm and state-year fixed effect. Standard errors are clustered by district and event.

Table A.7: Local Firms' Outcomes: Net Judge Removal

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap. (IHS)	(7) Interest Exp (IHS)
Neg x <=-4	-0.00720	0.00629	0.00261	-0.00225	-0.0803	-0.0779*	0.0251**
	(0.00678)	(0.0155)	(0.00772)	(0.00616)	(0.0474)	(0.0398)	(0.0112)
Neg x-3	-0.00570	0.00140	0.00601	0.00193	-0.0664	-0.0151	0.00411
	(0.00661)	(0.00761)	(0.0123)	(0.00526)	(0.0501)	(0.0725)	(0.00978)
Neg x-2	-0.00328	-0.000139	-0.000887	-0.00116	-0.0631*	0.0266	-0.00900*
	(0.00601)	(0.00555)	(0.00561)	(0.00557)	(0.0322)	(0.0877)	(0.00460)
Neg x 0	0.00116	-0.00697	-0.00905	-0.00492	-0.0499	-0.0356	-0.00827
	(0.00511)	(0.00702)	(0.00930)	(0.00647)	(0.0518)	(0.0932)	(0.0174)
Neg x 1	0.00113	-0.00960	-0.0109	-0.00699	-0.162**	0.0252	-0.00239
	(0.00564)	(0.00546)	(0.0127)	(0.0113)	(0.0536)	(0.0856)	(0.0157)
Neg x 2	-0.00149	-0.00692	-0.0289	-0.0115	-0.170***	0.00525	-0.00874
	(0.00350)	(0.0129)	(0.0180)	(0.0115)	(0.0374)	(0.0600)	(0.0110)
Neg x 3	-0.00967*	-0.0187	-0.0312	-0.0251*	-0.264**	-0.0679**	-0.00507
	(0.00511)	(0.0204)	(0.0246)	(0.0119)	(0.120)	(0.0230)	(0.0187)
Neg x >=4	-0.0224***	-0.0361	-0.0495	-0.0277***	-0.207***	0.0580	-0.0126
	(0.00591)	(0.0261)	(0.0282)	(0.00808)	(0.0554)	(0.118)	(0.0204)
Observations	22752	22752	22752	22752	22752	22752	22752
No. Firms	393	393	393	393	393	393	393
No. Districts	64	64	64	64	64	64	64

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

Notes: This table presents the estimates from Equation 1 using firm-level outcomes, equivalent to Figure 3, for net judge removal. IHS refers to inverse hyperbolic sine function. Using logarithmic transformation instead of arcsine yields similar estimates. I restrict the firms sample to a balanced panel in order to ensure no endogenous missing values of firm-level outcomes. All firm-level specifications include firm and state-year fixed effect. Standard errors are clustered by district and event.

Table A.8: Net Judge Addition and Unbalanced Firm-Level Data

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap. (IHS)	(7) Interest Exp (IHS)
Pos x <=-4	-0.0359**	-0.0457***	-0.0406	-0.0185***	-0.195***	0.0594	0.00212
	(0.0122)	(0.00681)	(0.0339)	(0.00299)	(0.0166)	(0.0602)	(0.0267)
Pos x -3	-0.00492	-0.0237***	-0.0164	0.0297***	-0.0860**	0.0570	-0.0207
	(0.00847)	(0.00575)	(0.0267)	(0.00422)	(0.0292)	(0.0728)	(0.0118)
Pos x -2	-0.0143	-0.00698	-0.0270	0.000962	-0.00186	0.0000186	0.00546
	(0.00842)	(0.0105)	(0.0177)	(0.0124)	(0.0875)	(0.0358)	(0.00662)
Pos x 0	0.0128	0.000795	-0.0120	0.00255	-0.0453	0.0166	-0.00727
	(0.00912)	(0.00375)	(0.00719)	(0.0125)	(0.0351)	(0.0368)	(0.0130)
Pos x 1	0.0141***	-0.0102	-0.000444	0.0126	-0.0450**	0.00876	-0.0157
	(0.00438)	(0.0106)	(0.00782)	(0.00877)	(0.0200)	(0.0331)	(0.0108)
Pos x 2	0.0175***	-0.00371	-0.000445	0.0120**	0.0153	0.0335*	-0.0101*
	(0.00269)	(0.00536)	(0.00758)	(0.00413)	(0.0161)	(0.0156)	(0.00471)
Pos x 3	0.0127***	-0.00824	-0.0167*	0.00950	-0.0449*	0.0357	-0.0169***
	(0.00253)	(0.0114)	(0.00922)	(0.00791)	(0.0204)	(0.0231)	(0.00504)
Pos x >=4	0.0120***	-0.0106	-0.0226	0.000800	-0.0652***	0.0332*	-0.0298***
	(0.00335)	(0.0149)	(0.0265)	(0.00824)	(0.00853)	(0.0168)	(0.00433)
Observations	201696	180969	129551	201093	218988	236671	171867
No. Firms	6689	5746	4341	6726	6981	7489	5909
No. Districts	149	148	140	150	150	152	147

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

Notes: This table presents the estimates from Equation 1 for net judge addition using all registered formal sector firms in the district including those with missing data. Standard errors are clustered by district and event. Note that both the number of firms and number of district clusters vary for each variable, making it hard to draw the right inference. Thus, I do not use this table to draw any implications and rely on the balanced panel sample.

Table A.9: Net Judge Addition and Missing Firm-Level Data

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap. (IHS)	(7) Interest Exp (IHS)
Pos x <=-4	0.00106	0.00171*	0.00916	0.00610	0.000377	-0.00158	0.000776
	(0.00438)	(0.000868)	(0.00667)	(0.00473)	(0.000754)	(0.00224)	(0.00645)
Pos x -3	-0.00588	0.00221	-0.000704	-0.000637	0.00351*	-0.00147	0.00203
	(0.00546)	(0.00461)	(0.00561)	(0.00731)	(0.00177)	(0.00187)	(0.00638)
Pos x -2	0.00225	0.00348*	0.00382	0.00228	-0.000224	-0.000940	0.00591***
	(0.00313)	(0.00168)	(0.00468)	(0.00285)	(0.00266)	(0.00139)	(0.00172)
Pos x 0	-0.00889**	-0.00310	-0.00774*	-0.0110**	-0.00267**	-0.00244*	-0.00471
	(0.00381)	(0.00197)	(0.00367)	(0.00483)	(0.00115)	(0.00126)	(0.00465)
Pos x 1	-0.00916**	-0.00442**	-0.00592	-0.00620	-0.000915	-0.00160**	-0.00165
	(0.00341)	(0.00180)	(0.00428)	(0.00419)	(0.000752)	(0.000589)	(0.00656)
Pos x 2	-0.0111***	-0.00938***	-0.00460	-0.00888***	-0.00188**	-0.00205***	-0.00915
	(0.00343)	(0.00214)	(0.00437)	(0.00282)	(0.000673)	(0.000491)	(0.00919)
Pos x 3	-0.0117***	-0.00221	-0.00321	-0.00960***	-0.00171*	-0.00161	-0.00940
	(0.00366)	(0.00155)	(0.00555)	(0.00230)	(0.000854)	(0.00100)	(0.00631)
Pos x >=4	-0.0114***	-0.00499**	-0.00467	-0.00736***	0.000623	-0.00195***	-0.0146*
	(0.00352)	(0.00164)	(0.00639)	(0.00200)	(0.000732)	(0.000392)	(0.00665)
Observations	238401	238401	238401	238401	238401	238401	238401
No. Firms	7534	7534	7534	7534	7534	7534	7534
No. Districts	152	152	152	152	152	152	152

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

Notes: This table presents the estimates from Equation 1 for net judge addition using all registered formal sector firms in the district, with missing data variable encoded as 1 if a firm does not report the corresponding variable for a given year. Standard errors are clustered by district and event.

Table A.10: Net Judge Addition and Non-Litigating Firms

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap. (IHS)	(7) Interest Exp (IHS)
Pos x <=-4	0.0417	0.0299	-0.0688	0.0107	-0.374	-0.480	0.152*
	(0.0480)	(0.0418)	(0.0493)	(0.0779)	(0.325)	(0.326)	(0.0700)
Pos x -3	-0.0122	0.0239*	-0.0467	0.0259	-0.0410	0.160	0.121*
	(0.0244)	(0.0128)	(0.0633)	(0.0494)	(0.215)	(0.181)	(0.0556)
Pos x -2	0.0469	0.0389	-0.00119	0.0475	0.183	0.0577	0.161***
	(0.0401)	(0.0433)	(0.0538)	(0.107)	(0.410)	(0.152)	(0.0427)
Pos x 0	0.0198	-0.00299	0.0211	0.0347***	0.126	0.397**	-0.0104
	(0.0246)	(0.0142)	(0.0489)	(0.00938)	(0.141)	(0.128)	(0.0305)
Pos x 1	0.0398	0.00294	0.0478	0.0448*	0.278	0.0526	-0.0975***
	(0.0238)	(0.00926)	(0.0795)	(0.0243)	(0.324)	(0.112)	(0.0206)
Pos x 2	0.0416	0.00400	0.0835	0.0363	0.111	0.134	-0.0568***
	(0.0270)	(0.0116)	(0.0627)	(0.0363)	(0.290)	(0.237)	(0.0152)
Pos x 3	0.0526***	0.0338**	0.0374	0.0423*	0.306	0.0993	-0.0564
	(0.0165)	(0.0127)	(0.0281)	(0.0226)	(0.254)	(0.161)	(0.0339)
Pos x >=4	0.0695***	0.0220***	0.0459***	0.0575***	0.463**	0.0999	-0.105***
	(0.0176)	(0.00614)	(0.00907)	(0.00413)	(0.179)	(0.265)	(0.0170)
Observations	11727	11727	11727	11727	11727	11727	11727
No. Firms	203	203	203	203	203	203	203
No. Districts	44	44	44	44	44	44	44

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

Notes: This table presents the estimates from Equation 1 for net judge addition using the subset of non-litigating balanced panel of firms in the district. Litigation status is defined as whether a firm in the sample is found to have a legal case in the sample courts during the study period. Standard errors are clustered by district and event.

Table A.11: Outcomes of Firms in Neighboring Districts Following Net Judge Addition (Placebo)

	(1) Wage Bill (IHS)	(2) Plant Value (IHS)	(3) Raw Mat (IHS)	(4) Sales (IHS)	(5) Profit (IHS)	(6) Working Cap (IHS)	(7) Int Exp (IHS)
Pos x <=-4	-0.0117	-0.00305	-0.00259	-0.00801	-0.0391	-0.105*	0.00680**
	(0.0106)	(0.00637)	(0.00564)	(0.00929)	(0.0514)	(0.0527)	(0.00261)
Pos x -3	-0.00614	0.00343	0.000131	0.00175	-0.0278	-0.0488	0.00842*
	(0.00770)	(0.00354)	(0.00452)	(0.00800)	(0.0429)	(0.0330)	(0.00451)
Pos x -2	0.00292	0.00619*	0.00874	0.00220	0.0430	-0.0317	0.00465
	(0.0114)	(0.00293)	(0.0102)	(0.00493)	(0.0354)	(0.0198)	(0.00863)
Pos x 0	-0.000792	0.00160	-0.000159	0.000863	-0.0362	-0.0325	0.00141
	(0.00672)	(0.00266)	(0.00481)	(0.00423)	(0.0218)	(0.0355)	(0.00388)
Pos x 1	-0.000467	-0.00201	-0.000318	-0.00115	-0.0269	-0.0181	0.00336
	(0.00563)	(0.00183)	(0.00443)	(0.00446)	(0.0258)	(0.0199)	(0.00308)
Pos x 2	0.00539	0.00541	-0.00991	-0.0110*	-0.0351	-0.000400	0.00544
	(0.00427)	(0.00369)	(0.00666)	(0.00559)	(0.0368)	(0.0345)	(0.00553)
Pos x 3	-0.00723	0.00714**	-0.0240**	-0.00638	-0.104*	0.0146	-0.00258
	(0.00650)	(0.00320)	(0.00804)	(0.00508)	(0.0475)	(0.0240)	(0.00375)
Pos x >=4	0.00504	0.000668	-0.00877	-0.00554	-0.0344	0.0213	0.00150
	(0.0108)	(0.00325)	(0.00598)	(0.00987)	(0.0680)	(0.0314)	(0.00319)
Observations	35049	35049	35049	35049	35049	35049	35049
No. Firms	597	597	597	597	597	597	597
No. Districts	99	99	99	99	99	99	99

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

Notes: This table presents the estimates from Equation 1 for net judge addition, using firm-level outcomes in districts neighboring the sample court districts. The regressions include firm fixed effects, neighbor district fixed effects and state-time trends. Standard errors are clustered by district and event. Notice that the estimates are orders of magnitude smaller than those using the sample of firms within the court's district.

Table A.12: Credit Mechanism: District-level

	Net Judge Addition		Net Judge Removal	
	(1) All Banks	(2) Private Sector Banks	(3) All Banks	(4) Private Sector Banks
Event x <=-4	0.0334	-0.0688	-0.00525	0.00556
	(0.0437)	(0.0584)	(0.00658)	(0.0592)
Event x -3	-0.0460	-0.00378	0.000752	-0.00277
	(0.0553)	(0.0635)	(0.0104)	(0.0344)
Event x -2	0.0369***	0.0747	-0.00265	-0.00858
	(0.00935)	(0.0569)	(0.0126)	(0.0119)
Event x 0	-0.0306	0.0837	0.00811	-0.00614
	(0.0249)	(0.0798)	(0.0128)	(0.0136)
Event x 1	0.0258	0.136	-0.0121	-0.0268
	(0.0320)	(0.0926)	(0.0101)	(0.0213)
Event x 2	0.0121	0.0819	0.00171	-0.000413
	(0.0693)	(0.0517)	(0.0236)	(0.0193)
Event x 3	0.0852*	0.166**	-0.00314	-0.0259
	(0.0422)	(0.0676)	(0.0244)	(0.0456)
Event x >=4	0.0609	0.124**	-0.0109	-0.0180
	(0.0353)	(0.0437)	(0.0303)	(0.0501)
Observations	5670	5670	5670	5670
No. Districts	110	110	110	110

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

Notes: I use the Reserve Bank of India annual district-level credit data to industrial borrowers aggregated across all banks, and by banking sector. Columns 1-3 present estimates following net judge increase whereas Columns 4-6 present those following net judge reduction as per Equation 1. All district-level specifications for credit circulation are weighted by the number of active cases involving banks in a district and include district and state-year fixed effect. Standard errors are clustered by district and event.

Table A.13: Credit Mechanism: Firm-level Heterogeneity by Size

	Net Judge Addition			Net Judge Removal		
	(1)	(2)	(3)	(4)	(5)	(6)
	Working Cap. (IHS)	Interest Exp (IHS)	Profit (IHS)	Working Cap. (IHS)	Interest Exp (IHS)	Profit (IHS)
	Low Lev Small Firms	Low Lev Small Firms	Low Lev Small Firms	Low Lev Small Firms	Low Lev Small Firms	Low Lev Small Firms
Event x <=-4	0.0222	0.303	-0.305	-0.156	0.0146	-0.0989**
	(0.238)	(0.245)	(0.528)	(0.102)	(0.0261)	(0.0448)
Event x -3	-0.195	0.123	-0.0689	-0.0468	-0.00744	-0.0564
	(0.551)	(0.122)	(0.471)	(0.0739)	(0.0198)	(0.0337)
Event x -2	-0.148*	0.124	-0.0550	-0.0357	-0.0118	-0.0142
	(0.0701)	(0.0870)	(0.419)	(0.0437)	(0.0259)	(0.0513)
Event x 0	0.199	-0.0941*	-0.0330	-0.0343	0.00958	0.0165
	(0.187)	(0.0522)	(0.109)	(0.0843)	(0.0216)	(0.0424)
Event x 1	0.0431	-0.207*	0.112	0.0330	0.0339	-0.0388
	(0.0778)	(0.110)	(0.217)	(0.0683)	(0.0295)	(0.0492)
Event x 2	-0.0826	-0.172**	-0.130	0.0868	0.0290	0.0208
	(0.133)	(0.0764)	(0.120)	(0.0582)	(0.0236)	(0.0708)
Event x 3	0.425*	-0.179**	0.561**	-0.0374	0.0512	-0.189**
	(0.197)	(0.0620)	(0.242)	(0.0373)	(0.0405)	(0.0687)
Event x >=4	0.178**	-0.198***	0.243	0.0591	0.0675	0.00479
	(0.0743)	(0.0578)	(0.195)	(0.0815)	(0.0648)	(0.0152)
Observations	6210	6210	6210	6210	6210	6210
No. Firms	105	105	105	105	105	105
No. Districts	30	30	30	30	30	30

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

Notes: I use firm-level data on working capital and interest expenditure. Columns 1-3 present estimates following net judge increase among small firms with low-leverage whereas Columns 4-6 present those following net judge reduction as per Equation 1 for the specific firm subsample. All specifications include firm fixed effect. Standard errors are clustered by district and event.