Front-line Courts As State Capacity: Evidence From India

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Front-line courts handle the bulk of legal cases and yet they are severely under resourced with potential implications for economic development. Using rich court-level panel data from India, constructed using the universe of legal case records, and an event study research design, I show that an increase in judge staffing levels substantially reduces case backlog in local district courts, and subsequently improves the productivity of local formal sector firms. These effects are largely driven by last mile enforcement of credit contracts by such courts, enabling banks to lend more to the industrial sector. Hiring district judges is highly cost-effective. (*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; Chemin 2009a,b; 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, front-line district courts in India have 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. These courts 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 variation in the timing of judge staffing-level changes within a sample of 195 district courts (with $\approx$ 4600 judge positions) between 2010 and 2018 in an event study design. Each district court has multiple judge positions whose staffing vary over time. This variation results from a combination of recruitments, retirements, rotation of judges between district courts, and their interaction with one another. These are driven by centralized policies on retirement at 60 years of age, sporadic and often failed state-level recruitment drives for district judgeships, and frequent rotation of judges between district courts required as per state policy, all of which are executed at the state-level, a level above the district courts themselves. The resulting implication on the timing of judge staffing levels changes within a specific court could thus provide a source of plausibly exogenous variation

[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 Section A.0.A. for details on national-level summary statistics and cross-country comparison.

for causal inference.[2]

I measure the timing variation using case-level time-stamps from the universe of legal cases from the sample courts during the study period. I estimate the causal effects by employing a modified stacked event study design building on Cengiz et al. 2019. The modification incorporates both types of staffing changes (as changes could be positive or negative) within the same specification, which along with the stacked design accounts for possible SUTVA violation.[3] This design also accounts for dynamic and heterogenous treatment effects (Sant'Anna and Zhao 2020, Sun and Abraham 2021). 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 an additional support. 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.

Civil disputes involving monetary contracts (such as debt) and property within the geographic jurisdiction of district courts form the majority of the types of cases litigated in such courts. Given this context, there are two likely economic mechanisms linking court-level performance to local economic sectors: (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 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). These firms account for a large share of value addition in the economy and pay corporate taxes.[4] I supplement

[2]Informal interviews with several state-level and district-level judges in India indicate that there is an element of "randomness" to staffing decisions within the district judiciary over time because of its unique institutional history: these decisions require coordination between higher ranked officials within the judiciary, state-level executive, and the state legislature, which is unlike staffing decisions for general administration bureaucrats that has been recorded in the literature (Iyer and Mani 2012; Khan et al. 2019). I describe this context in detail in Section 2.

[3]The intraclass correlation (ICC or ρ) in judge staffing levels between district courts within a state is 0.2 and the ICC in change in staffing levels is close to 0. While this is not sufficient to address concerns of SUTVA violation, including event dummies for both positive and negative staffing-level changes within the econometric specification and results from robustness tests suggest that this is unlikely to affect the main findings.

[4]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

firm-level analysis by employing various sources of district-level data to examine district-level economic outcomes. These include credit, crime, investments, and nightlights as outcomes.

Local courts are potentially important for credit markets as they are responsible for *executing all* judgements even if some of them are awarded by a different court or arbitrator. For example, debt disputes could be resolved through mutual arbitration or through specialized courts (such as through debt recovery tribunals as in Visaria 2009 or bankruptcy courts) but these judgements have to be executed by the corresponding district court. Consider the case where a creditor has to take an action determined in the judgement including directions for liquidating collateral or approaching the borrower for repayment in the case of unsecured loans. Such actions can only be taken if the creditor has the judgement executed through their corresponding district court as per India's Code of Civil Procedure, 1908, Order 21. As a result, lending decisions could be tied to how well-functioning a specific district court is, especially if there are supply-side constraints (Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012; Bau and Matray 2023; Bazzi et al. 2023).

Applying the judicial staffing change research design to this context, 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 over 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.[5]

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

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 describe data construction in detail in Section 3. Unlike in the United States, many non-financial sector firms in the formal sector in India - the key population of interest - have the same location of registration and production.

[5]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. I also find positive effects of increased staffing on other court-level outcomes such as overall backlog reduction, backlog reduction among cases involving firms, fewer dismissals and fewer contests of the judgement, suggesting an overall improvement in local judicial capacity.

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 half in magnitude, the effects on productivity measures are symmetric per-judge. Furthermore, these effects appear with a lag, potentially following more immediate changes in intermediate outcomes such as credit 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 district-level, I find an increase in aggregate lending by banks to industrial borrowers. At the firm-level I find increased working capital and lower interest expenditure. These results are consistent with a lending model where creditors incorporate the ability of courts to enforce contracts into their lending decisions along extensive margin - who they lend to - and intensive margin - interest rate on loans. I find 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. Furthermore, I find lower interest expenditures among firms across the size distribution as evidence towards price response by creditors. The effects on access to credit as a mechanism dominate alternative explanations for firm-level productivity gains in a decomposition exercise.

I take a number of precautions and perform different robustness checks to verify the 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 firms in the district that could affect competition. A decrease in competition could bias the estimation upward leading to erroneous inference of a direct positive treatment effect. In contrast, I find the opposite - the number of firms and new incorporations increase within the district following judge staffing-level increase. This could imply better business dynamism and increased competition, instead generating a downward bias in the estimates. As a result, the estimates reported in this paper are likely a lower bound. 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 a treated district court has no jurisdiction. More broadly, I find suggestive positive effects on district night light intensity following positive judge staffing-level change and negative effects following negative changes.[6]

[6] 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

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 I use are more conservative 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 subnational-level evidence on 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 contributes to a growing 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 2023). Specifically, I construct a unique, at-scale court-level panel dataset from the universe of legal records and leverage dynamic variation in judge staffing-levels, both of which, to my knowledge, have not been studied in any context. As a result, the findings speak to the structure and everyday capacity of legal institutions other than the design of the law.

Second, this paper also demonstrates the impact of front-line courts on the financial sector, particularly in the presence of market frictions (Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012; Castellanos et al. 2018; Rigol and Roth 2021; Breza and Kinnan 2021; Bazzi et al. 2023). 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 through their role in *executing all judgements*, which has implications for financial institutions in the actual recovery of unpaid debt. The main findings of this paper support the role of local district courts as the last mile enforcer of credit contracts.

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

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.

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.[7]

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.

2 Context: The Indian Judiciary

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. 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. These courts are similar to county courts within the state judiciary in the United States (for example, New York City Civil courts, or Chester County court in Pennsylvania) and are not like the US federal district or circuit courts. The geographic jurisdiction of a district court is the corresponding administrative district in India (a sub-provincial region like a county or a municipality), with an average population of around 2 million people as of 2011 census. Each court has multiple judgeships (judge positions), where judges hear trials in courtrooms within the district court's premises.

District courts are managed by the respective state High Courts. Due to separation of

[7]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.

powers, funding these courts require approval by the state-level executive and legislature, and the dispute resolution processes has to adhere to specific procedural or substantive code that are created and amended by the state-level legislature. Thus, the functioning of the district judiciary requires inter-institutional coordination and coordination failures underpin many of the constraints in expanding front-line judicial capacity in India. One such 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.[8] In contrast, there are close to 100 judges per million in the United States and close to 200 per million in the European Union. The judge to population ratio is further reduced when we account for the extent of vacancies. The total number of judge posts in a district court 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.[9]

The judiciary also faces persistent judge 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 district courts are handled by the state-level High Courts. District judges are senior legal professionals, who are either inducted from the local bar council or promoted from sub-district courts. A few are directly

[8] As per the response by the national Law Minister, Arjun Ram Meghwal, to Parliamentary questions during winter session in Dec 2024 reported by a national media outlet, NDTV, at this link.

[9] 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. The predicted number of judges following the algorithm is typically much larger than the number of judges that I observe in my data.

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. Unlike the United States or other advanced economies, judges in India are career civil servants who are selected via recruitment processes and none are elected.

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 - no one is assigned a district where they have worked in the past nor are they assigned to districts that are their home district. 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. The stated objective behind these rules is to create an independent judiciary.

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 net effect could either be positive, negative, or no change 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.

2A Courts and Bank Credit Circulation

A large majority of cases listed in district courts are civil disputes (see Panel A Figure 1). Financial sector enterprises such as banks rely on district courts for executing debt contracts by enabling last resort recovery. The total non-performing assets (NPA), which are mainly defaulted loans, formed close to 15% of the lending portfolio in this period (Rao 2020). Banks follow policies set up by India's central bank - the Reserve Bank of India (RBI) - for debt recovery, including filing suits in district civil courts, or in Debt Recovery Tribunal (for very large valued loans), or initiate bankruptcy proceedings.[10] 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.[11] Judicial staffing support could

[10]See response by the Ministry of Finance to parliamentary questions on the recovery of bank NPA at this link.

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

thus facilitate recoveries through 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 including non-response by debt defaulter (evasion or failure to appear in response to bank's repayment notice is considered a crime) as well as property crimes such as thefts. Relatedly, a large bulk of criminal cases in district courts are what are known as "summary trial" cases.[12] This could have 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.

[12] 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 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.

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.[13] 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 (see Figure A.3 for an example).

Each district court has multiple courtrooms and judges. A courtroom is a physical location within a district court premises where a judge hears trials. To illustrate, consider Coimbatore District and Sessions Court in Tamil Nadu, India, which has 13 judges and a backlog of over 17000 cases on average. A court similar to Coimbatore court is the Chester County court of Pennsylvania, USA, that has 15 judges (2 vacant as of January 2025) and a backlog of 4000 cases.[14]

Leveraging the fact that the data represents the universe of legal cases between 2010 and 2018, I enumerate judges within a district court over the study period based on annual workflow observed for a given courtroom-judge position combination generated from the timestamp information. I define annual workflow as follows: I record a courtroom-judge position 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-judge position. A court registrar assigns new cases to all incumbent judges immediately after filing and verification of a legal complaint. When an incumbent judge moves (either due to rotation or retirement) with no replacement, that specific courtroom-judge position remains vacant and no new cases are assigned to that pair. The existing workload at the time of vacancy is transferred to other remaining judges in the district court.[15]

Using this algorithm, I generate the number of judges in a district court for each year

[13] The states in the sample include all industrial states in India and the districts within them were selected to ensure an overlap with the location of registered formal sector firms. I drop large metro city districts - Mumbai, Delhi, Bangalore, Hyderabad, and Chennai - to minimize potential interference from agglomeration economies. The population and economic characteristics are largely similar between districts in and not in sample with differences in some characteristics significant only at 10%. The sample districts are slightly more urban, literate, and engaged in manufacturing compared to other districts conditional on the state (see Table A.1).

[14] Information on Chester county court in PA accessed at this link `https://www.pacourts.us/courts/courts-of-common-pleas/common-pleas-judges` and `https://www.pacourts.us/Storage/media/pdfs/20241209/183618-chestercounty23.pdf`.

[15] This information on assigning cases to judges is based on extensive qualitative time and motion studies that I carried out in Bangalore and Mumbai district courts.

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 within the study period. Using annual judge count, I define a positive staffing change event as the year 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.[16]

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, which incorporates other metrics of "better capacity" beyond speed (see Table A.2 for pairwise correlations between the different measures). I also focus on case disposal rate and case-specific outcomes for legal cases involving banks given the key mechanisms I discuss later in the paper.[17]

Verification of Constructed Judge Staffing Numbers Since there is no data on the judicial staffing numbers for each district court over time, I rely on the rich meta-data in the case-level database to construct a district court-level panel dataset that includes the number of judges and other court-level metrics across the study sample courts. To ascertain that the construction of staffing numbers is indeed a valid method and reflects the actual judge staffing on the ground, I obtained one-time data on the number of judges in my sample courts in January 2025 by scraping the corresponding, newly created, district court websites. Panel B Figure 1 (left) shows the correlation between the number of judges constructed from data and number of judges in 2025 as reported on official district court websites.[18] The constructed

[16] An ideal dataset would be the personnel records of all judges serving in all district courts over time in order to track their entry and exit from different courts in the data. However, such a dataset does not exist. I verify that constructing staffing levels from the case-records generate similar aggregate estimates as reported in newly available one-time official judge-level data on district court websites that I describe later in this section. Since the e-courts system directly records daily court proceedings on a digital platform, this method of constructing judge staffing levels potentially minimizes concerns about administrative data integrity that could arise due to bureaucrats entering data (Singh 2020; Muralidharan et al. 2021).

[17] 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.

[18] For example, the district court website such as for Visakhapatnam in Andhra Pradesh lists its current judges at `https://visakhapatnam.dcourts.gov.in/list-of-judges/`. I count the number of judges with designation containing "District and Sessions Judge", "Additional District Judge or ADJ"', "Family Court", "Special Judge", or judges of other specialized courts. These judges belong to the district judge cadre, which

number of judges is strongly correlated with the actual number of judges in 2025.

Furthermore, I compare the constructed number of judges with the predicted number of judges if one were to follow the Law Commission Report No. 245 recommended algorithm (Panel B Figure 1, right). The predicted number of judges as per the algorithm is significantly correlated with the constructed number of judges although in many instances, the algorithm predicts a higher staffing levels than those observed. The constructed number of judges is thus within the ranges of actual number of judges and is unlikely to count or attribute other changes within the district court to judge staffing numbers.

Firm-level Variables

There are two important populations of firms relevant for analysis here. First is the set of non-financial sector firms, including manufacturing, services, and trade, engaged in value added production. I use CMIE-Prowess dataset that includes annual balance sheet data of the universe of publicly 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 (balance sheet) data annually, which is useful given the time-scale of the identifying variation (no other firm-level datasets are annual panel of firms that are identified at the district-level).[19] Third, formal-sector firms in Prowess account for $\approx$ 40% of sales, 60% of VAT, and 87% of exports (Economic Survey, 2018), and therefore the data captures a large share of value addition in the economy. Using this data source, I carry out both intensive and extensive margin analyses. For intensive margin effects, I focus on firms that are incorporated within the sample administrative districts corresponding to district court jurisdiction before the start of the study period, i.e., 2010, whom I call the incumbent firms. I estimate productivity improvements on this set of firms. I restrict the analysis sample to a balanced panel of 393 non-financial sector firms to preserve internal validity for causal inference. This can also be viewed as the intensive margin effects of local court capacity among the set of large, formal sector firms.[20] Since Prowess also records the year of incorporation, I can compute firm entry within the sampling frame of listed and

is the main population of interest for this study.

[19] Annual Survey of Industries is only identified at the state or industry-level and cannot be matched with the source of the identifying variation in this study.

[20] 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 district jurisdiction. Remaining 38 district court jurisdictions result in no match with firms in Prowess. This sample further reduces to $\approx$ 7000 firms when restricted to those incorporated before 2010 (the start of the study period) but a lot of the key outcome variables including profit and sales are missing for multiple time periods, hindering an unbiased analysis.

registered formal sector firms over the period to complement the intensive margin analysis.[21] Using year of incorporation, I examine extensive margin changes (at least within the specific sampling frame) as well as analyze compositional changes in the set of firms over time to inform the analysis from the incumbent firm sample.

The second set of firms that are relevant is the financial sector firms, particularly the banking sector. For the latter, 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 variable of interest. 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.

Outcomes: I examine profit and sales revenue - to measure production outcomes, wage bill, the value of capital goods (plants and machinery), and raw material expenditures - to measure inputs to production, as key outcomes for the first set of non-financial sector firms. To examine mechanisms on improved credit access, I examine working capital to measure the effects on the financing of firms' operating costs and total interest expenditure across borrowings.

Other District-Level Variables

Since the firm-level sample does not represent the population of all economic agents that could benefit from better judicial capacity, I complement this with other district-level panel data sources. First, I examine effects on local crime using district-level reported crime statistics by National Crime Records Bureau (NCRB). I classify crimes into serious crimes such as murders and homicides (violent crime), and other crimes, which mainly include small-valued thefts and property crimes.

Second, I use CMIE CapEx dataset that lists the dates of all capital intensive investments including infrastructure outlays by location to examine whether: (a) infrastructure outlays and other district-level investments correspond to the timing of judicial staffing change as a test for exogeneity of the identifying variation, and (b) to examine the longer-run implication of judicial capacity improvements on public and private sector investments.

Third, I examine local economic development effects as measured by nightlights data (pixel average within a district boundary) using the Visible and Infrared Imaging Suite (VIIRS) Annual VNL V2.1 by the Earth Observation Group.

In addition, I use multiple other coarse (decadal or quinquennial) frequency datasets,

[21] Annual data on the number of formal sector firms and new incorporations are not currently available. These are only reported at the state-level by the Ministry of Corporate Affairs.

including population and economic censuses, to test for assumptions involved in causal inference.

3A Summary Statistics

Panel A of Table 1 presents summary statistics for the court variables covering 195 district courts over 9 years. On average, there are 18 judge positions per district court (around 4600 total judge positions in the data), with 23 percent vacancy. Over 2010-2018 study period, 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, which averages to over 20,000 cases in any given year. Around 3200 cases are resolved and similar number are filed every year on average within the sample courts. The average case duration is 420 days (right-tailed distribution with a standard deviation of 570 days).[22] 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. I also examine other metrics reflecting the "quality" of judicial capacity measured as percent uncontested - which is the percent of total cases resolved whose judgement is not contested by either of the litigating parties, and percent dismissed - which is the percent of total cases resolved due to dismissal. Contesting an order is similar in meaning to appeal, although this appeal is made to the same court and not to a higher court. Dismissal of case indicates whether a judge determines that the case does not have merit to be heard by the court. In the study sample, an average of 26.2% of resolved cases remain uncontested and an average of 22% cases are dismissed.

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 includes 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).

[22] To give a comparison with similar county courts in the US, for example Chester County Court in PA, 57% of civil cases are resolved within 6 months of filing, and 75% within a year. The distribution of case duration within the sample courts in India is even more skewed - 25% of cases are resolved within a year and 10% of cases are pending for more than 3 years.

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 over time. I rely on aggregate variation in the number of judges at the level of a district court over time for causal identification. 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.

I show through a number of falsification tests that the variation in the timing of net staffing changes is plausibly exogenous to the functioning of a district court or local economic factors that I discuss in detail.

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 binary variables - Pos_{de} and Neg_{de} - as indicators for positive or negative staffing-level changes, respectively, and interact these with the event time bins as shown below.

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

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).[23]

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. I address this in two ways. First, I find very low intraclass correlation or ICC ($\rho \approx 0.001$) in change in judge staffing levels between district courts within the same state.[24] While this is not a sufficient proof, a lack of substantial correlation in staffing-level changes between the sample district courts in a given state suggests a limited plausibility of a mechanical SUTVA violation. Second, the estimator in Equation 1 overcomes plausible SUTVA violation by including both positive and negative event dummies interacted with the event bins in addition to stacking all events per court. I simulate the estimator by closely following the actual data generating process of court-level variables with multiple positive and negative judge staffing-level changes, drawing from a uniform distribution with parameters matching with data, within a district court over a similar period (9 years). I assign a known treatment effect for backlog disposal rate of 0.3 standard deviations (SD) in the simulated data to verify that the estimator recovers the effects without bias (see Figure A.4). As seen from the simulation results, the estimator recovers the treatment effect in the immediate succeeding period with a gradual decay as also seen in the empirical patterns discussed in Section 5.

4B Threats to Causal Identification

An ideal research design would require random assignment of judges to courts and random assignment of individual cases to judges, so that the court-level outcomes are orthogonal to judge staffing levels by design. 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, (b) parallel trends, as the stacked estimator accounts for heterogeneous as well as dynamic treatment effects, and (c) no other policy changes in the post period that could confound the treatment effects.

First I address if the timing of staffing level changes are correlated, or even driven by local

[23] Clustering this way follows the research design where the "treatment" is assigned at a district and the event 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.

[24] ICC in the constructed and actual number of judges between district courts in a state is 0.3 and 0.2, respectively.

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 of this study is particularly important 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 substantial variation that could be exploited for causal identification. For example, 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. To test that the assignment does not depend on past vacancy and backlog, I regress judge staffing-level changes on past vacancy rates, conditional on backlog. I find no clear support to suggest that existing court vacancies or backlog determine judge staffing-level changes (see Table A.3).

The falsification tests I run are also consistent with what many senior judicial officers I spoke to said about the constraints facing the district judiciary in India. They highlighted a lack of adequate candidates during recruitment and assignment drives, and routine retirements that occur as existing judges reach seniority as among the main challenges that cannot be timed closely. They said that these challenges have been persistent and are rarely influenced by elected representatives or changing local conditions. This is distinct from general administration bureaucrats (like the Indian Administrative Service), where elected representatives can influence the assignment of bureaucrats (Iyer and Mani 2012; Khan et al. 2019). Moreover, district judges in India are civil servants with no stated political partisanship.

Second, to test for the parallel trends assumptions, 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, to test for any confounding simultaneous or post-period policy shocks, I check whether the timing of staffing-level changes coincide with concurrent or past investments by the government and the private sector (for example, public infrastructure and/or new greenfield projects) in the sample districts. Any significant prior-period or immediate correlation could affect inference whether the estimated treatment effects are truly due to staffing shocks or other concurrent changes. I find no significant pre-trends or coincident effects 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.5).

4C 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 concerns, 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.

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

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. Furthermore, 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 specification also addresses any concerns regarding interference from different events, which are now included in the lead and lagged explanatory variables.

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 First Stage: Judge Headcount and Vacancy Rate

I estimate Equation 1 using judge staffing-levels as well as vacancy rates to provide a "first stage" in order to assess any pre-trends in the staffing levels/vacancy rates as well as examine the persistence of these events over time. Panels A and B in Figure 2 present the regression coefficients on the interacted event dummies when the outcome variable is number of judges (Panel A) or inverse vacancy rate (Panel B) - (100-vacancy in %). I discuss three observations: (a) an immediate increase/decrease in the staffing levels and inverse vacancy rate following either positive or negative staffing-level changes in order to show the extent of these changes of staffing levels and vacancy rates, (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.4 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.5 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 2 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 over 200 cases in a context where the average annual judge-level workload is ≈ 2000 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 could be distributed among other judges in the court.

Columns 3 and 6 of Table A.4 present the event study estimates using disposal rate as outcome in a tabular format for net increase and net decrease in judge staffing, respectively. The point estimates in the periods prior to the staffing changes are both statistically and economically insignificant, supporting the parallel trends assumption. Table A.6 shows heterogeneity by court size - mid-sized and smaller districts experience larger improvements in disposal rate following net judge additions, the negative effects of net removal are mainly observed in large districts.

The effects on court performance measured in terms of backlog disposal rate is also consistent with effects on other performance measures that also indicate an improvement along quality dimension. Specifically, I note that total and firms' case-specific pending backlog are negatively associated with judge staffing-level changes whereas percent cases uncontested and percent cases dismissed are positively and negatively correlated with judge staffing-level changes, respectively (see Table A.7).

Robustness: First Stage 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. Panel A Figure A.6 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, suggesting that court performance metrics are unlikely to determine judge staffing changes. Furthermore, the number of new litigation filed also does not change significantly following staffing-level changes. Current and past changes in judge 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.6.

5C Local Firms' Productivity

Prioritizing internal validity of the event study design, Figure 3 and Figure 4 present the estimates using sales, profit, wage bill, capital (plants and machinery), and raw material expenditures from the balanced panel sample of incumbent firms 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.8 and Table A.9 present the results in a tabular format corresponding to each of the figures, respectively. Among inputs, 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 ($p = 0.095$). 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. Lastly, expenditures on raw material used for production also increases, with a persistence over the long run ($p < 0.001$). Among outputs, 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).

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-level changes on firm-level outcomes are commensurate with the magnitude of the staffing change (which is one judge removed relative to two that are added). Wage bill contracts 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). The value of plants and machinery as well as expenditure on raw material also decrease but the point estimates are imprecise. Sales revenue decreases by 2% ($p = 0.46$ immediately, $p = 0.06$ 3 years from the staffing change, and $p = 0.006$ in the long run) and 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). Normalizing effects per judge suggest that the changes in firm productivity outcomes are symmetric with respect to staffing variations.

Robustness: Firm-level Outcomes A big 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 and total number of firms in the district within the Prowess database. 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).[25] I find increased firm incorporations and an increase in total number of firms in the districts around positive staffing events. If this increases competition among the incumbent firms, the estimates from the balanced panel 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.10). Third, I check if firm-level data in the unbalanced panel is missing endogenously with respect to judge staffing-level changes. I find that missing data decreases with improved judicial staffing and increases following net judge removal (see Table A.11). 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 suggests that the estimated firm-level effects are plausibly downward biased. This also implies that using unbalanced panel of firms is not a feasible strategy to estimate the causal effects, even with imputation for missing values, 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.12).

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.13 documents the results for positive staffing-level changes, showing that the point estimates are statistically and economically insignificant.

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.7).

[25] Although this may not truly reflect the compositional changes at the district-level across the population of all firms, I can examine whether firms enter or exit the Prowess database endogenously.

5D Other District-level Outcomes

Two pieces of evidence suggest that the effect of judicial staffing-level changes are broad-based: (a) changes in new firm incorporations, at least as reported in Prowess, highlighting potential extensive margin firm entry decisions among 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 incorporations, 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 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). While banks could also approach specialized debt recovery courts at the state-level - Debt Recovery Tribunals (DRTs) - for larger value loans (INR 1 million /US$ 11,400 or above),[26] local district courts are the main institutions responsible for the execution or enforcement of any judgement. 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 legal case-level data, 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 petitioners (or plaintiff, those filing a case) in an overwhelming majority of the cases (see Figure A.8). 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

[26] See response by the Ministry of Finance to parliamentary questions on the recovery of bank NPA at this link.

the lenders, where judges facilitate a settlement to enable partial or complete recovery.[27]

Local Credit Supply I begin by examining the resolution of legal cases in the sample courts pertaining to recovery of unpaid debt where a bank is one of the litigant. 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. Furthermore, using individual case-level data involving banks, the median duration of these cases decreases when judge vacancy is resolved (see Panel A Figure 5). Consequently, banks could experience a positive balance sheet effect from recoveries, which provides local liquidity shocks that they can recirculate as additional credit to industrial borrowers.

In the absence of bank branch-level 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.[28] 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. 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

[27] 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.

[28] 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.

decisions on whether repayment can be enforced through courts, in addition to considering borrower's wealth (to liquidate in the event of default if collateralized). 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 transaction costs, both of which would positively impact their productivity and balance sheet outcomes.

I discuss the framework in detail in Section A.0.B.. The main implications 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 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.[29] 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 ex-

[29] 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.

penditure 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: smaller, credit-constrained firms benefit from improved judicial capacity through an increase in credit access. 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.14) 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. 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 explain the productivity effects 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 between profits and 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 apply the estimated effects of positive judge staffing-level changes on profits and wage bill, which are measured in terms of percentage changes, to the median values of the outcomes 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 scaled by the estimated effects on judge staffing levels, 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 backlog of legal disputes and staffing constraints. Reducing vacancy by adding more judges is a highly cost-effective intervention as seen in the Indian context, which supports the growth 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.[30]

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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[30]For example, claims under $25000 form close to a third of pending civil cases across the Superior Courts in California as per CA courts annual statistics: https://www.courts.ca.gov.

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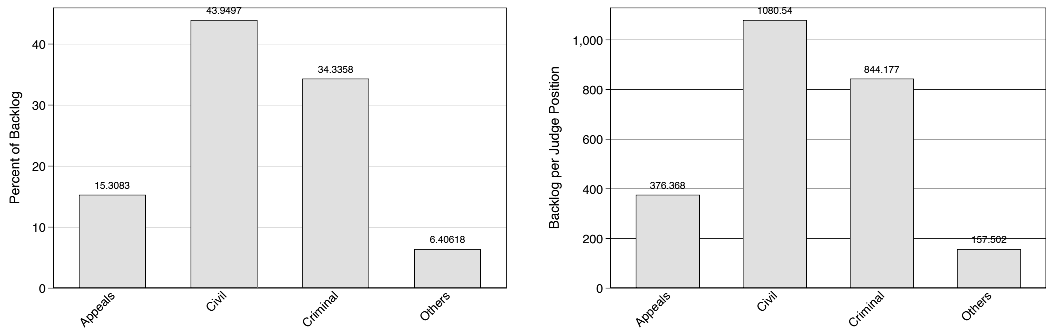
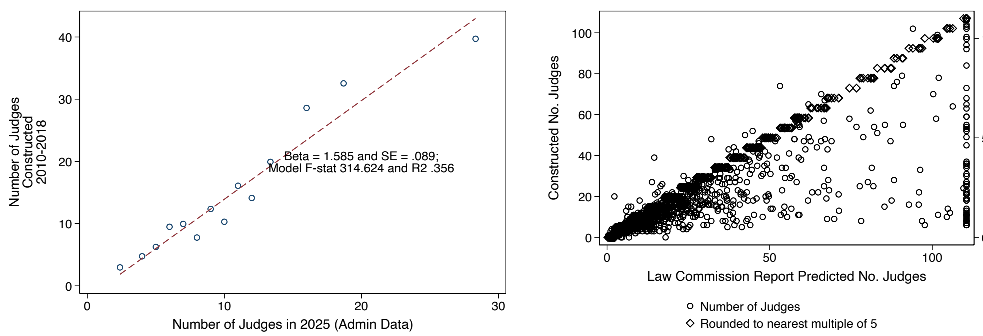
9 Tables and Figures

Figure 1: Number of Judges and Legal Cases in District Courts

Panel B: Case-type Distribution of Backlog



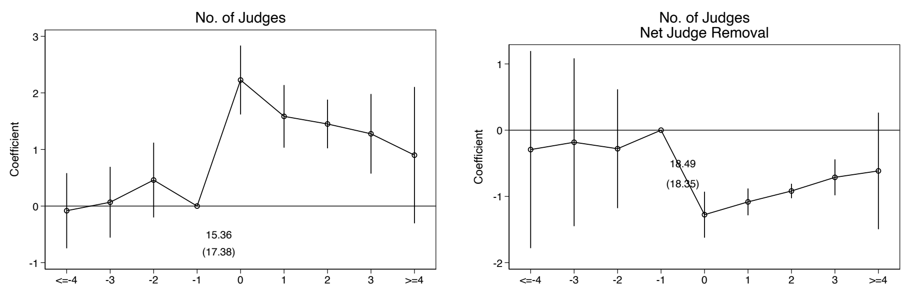
Panel B: Verifying Constructed Judge Staffing Measures



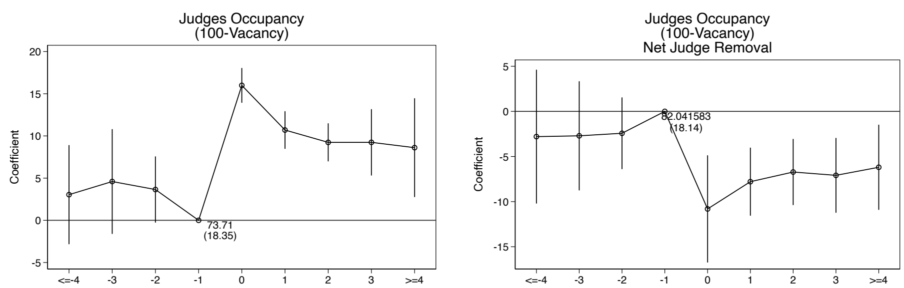
Notes: Panel A presents the distribution of legal case types in the sample courts. Civil cases include monetary contract and property ownership-related disputes. Criminal cases include cases filed under the Indian Penal Code and/or Criminal Procedure Code. Appeal cases are those appealing judgement from lower courts within the district, including magistrate's court. Figure A.8 provides additional descriptive statistics on the types of cases in these courts. Panel B documents the correlation between judge staffing level (number of judges) variable constructed using legal case records (procedure in this paper) and the current number of judges in Jan 2025 as reported on the district court websites with judge titles as district judge or additional district judge (left). I also compare the constructed number of judges with those calculated following the algorithm mentioned in the Law Commission Report No. 245 (right) to assess the bounds of the constructed measure. "Predicted Judges Rounded" is the predicted number of judges as per the Law Commission report rounded to the nearest multiple of 5. For example, if the predicted number of judges is 18.8, it is rounded to 20. Similarly, if the predicted number is 22.9, it is rounded to 25.

Figure 2: Net Addition and Removal of Judges and Court Performance

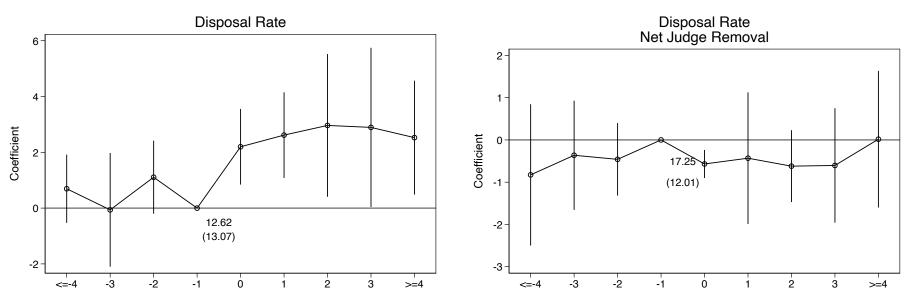
Panel A: Judge Headcount



Panel B: Inverse Vacancy Rate

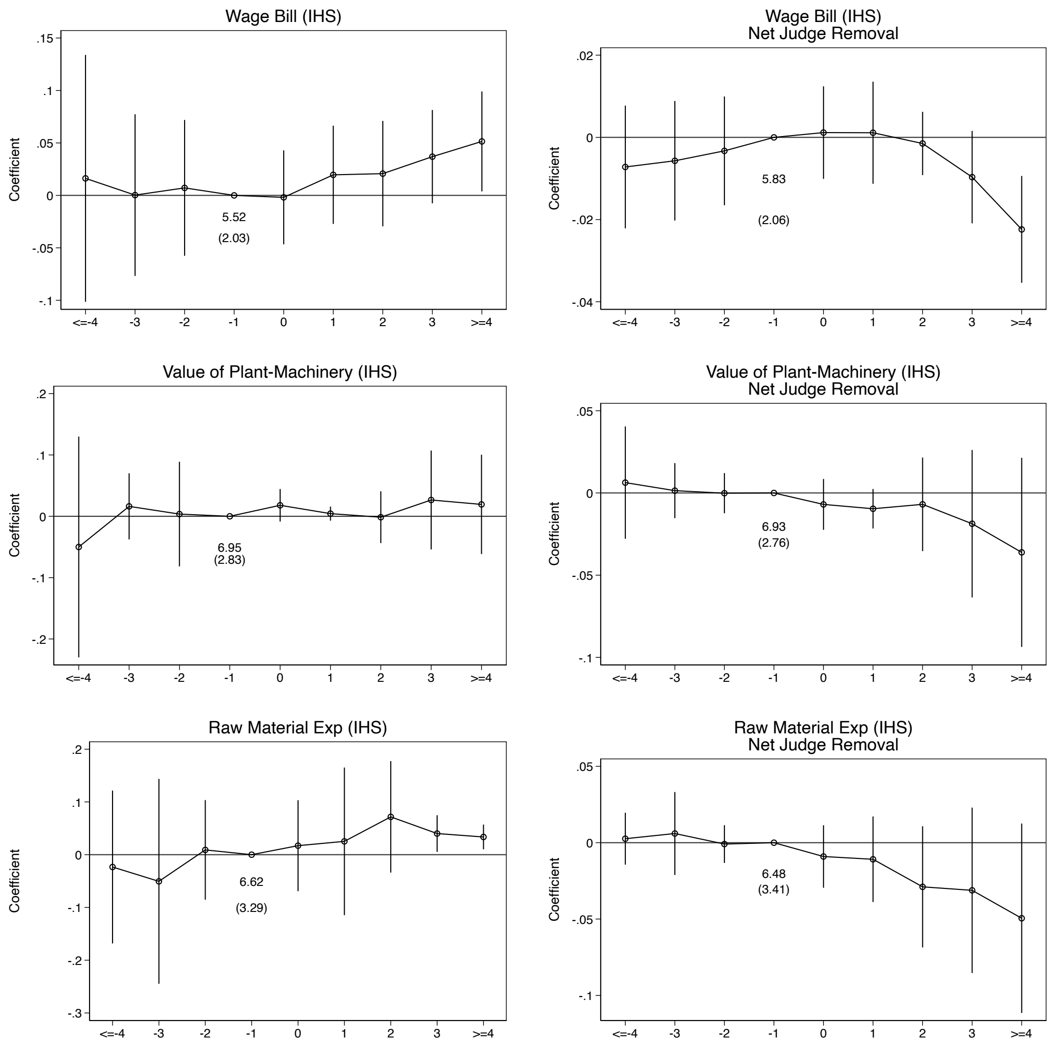


Panel C: Court-Level Disposal Rate



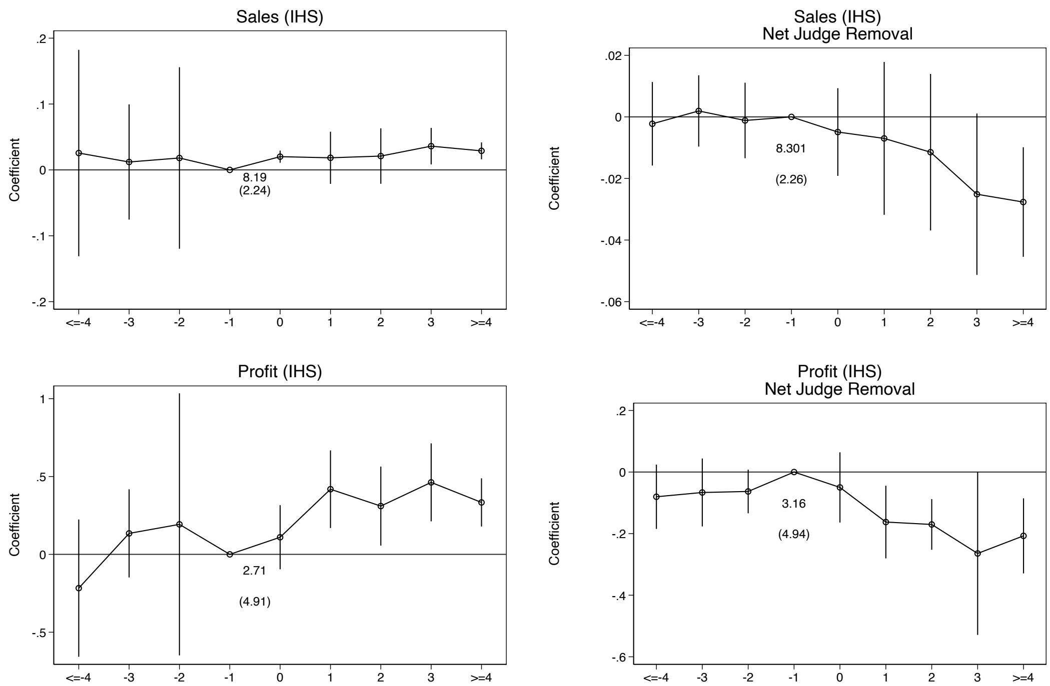
Notes: The figures plot the event study interaction coefficients for positive and negative staffing changes from estimating Equation 1 using total number of judges (Panel A), inverse vacancy rates (Panel B) and disposal rate (expressed in percentage terms in Panel C) as dependent variables, respectively. 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.4.

Figure 3: Local Firms' Production: Input Use



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 equivalents of these graphs are Table A.8 and Table A.9.

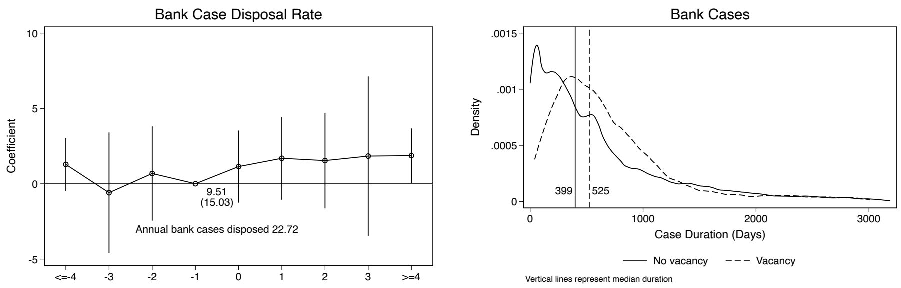
Figure 4: Local Firms' Production: Output



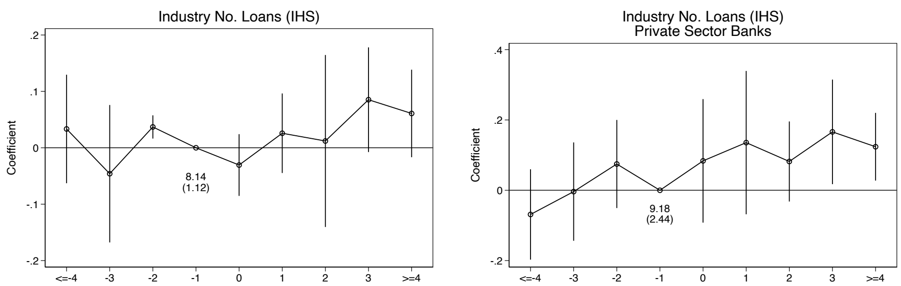
Notes: The figures above plot the event studies coefficients on negative staffing change event-time interaction dummies from estimating Equation 1 for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed in the 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 equivalents of these graphs are Table A.8 and Table A.9.

Figure 5: Credit Mechanism

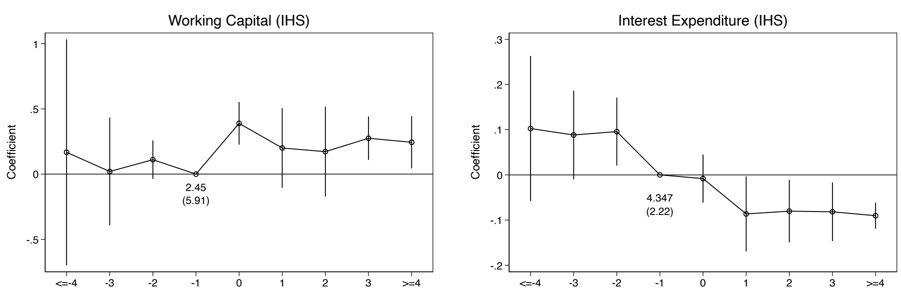
Panel A: Resolution of Banks' Cases in Courts



Panel B: District-Level Lending



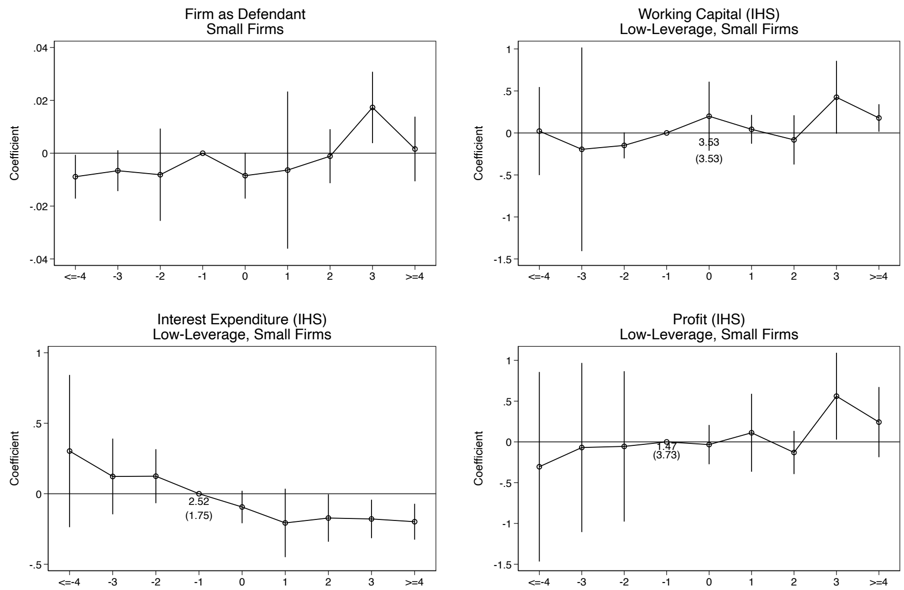
Panel C: Firm-level Working Capital and Interest Expenditure - All Sample Firms



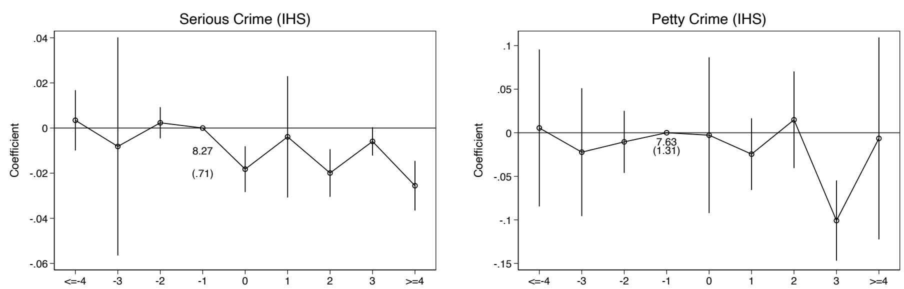
Notes: Panel A presents what happens to banks' legal cases in courts following net judge addition. Left panel shows that the case backlog reduces by 2 percentage points. Right panel shows reduction in median case duration when judge vacancy is resolved. Panel B presents effects of positive staffing-level 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.8 and Table A.9, respectively (Col 3, 6, 7). The table equivalent of the district-level bank lending outcome is in Table A.14 (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: Additional Mechanisms

Panel A: Access to and Cost of Credit Among Smaller Firms



Panel B: District-Level Crime Outcomes



Notes: Panel A 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. Panel B 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. The table equivalent of the firm-level graphs is Table A.8 and Table A.9, respectively (Col 3). Error bars present 95% confidence interval.

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 Backlog	195	1755	20345	26268	0	247953
Cases Resolved	195	1755	3221	3455	1	37994
Cases Filed	195	1755	3298	3684	1	34427
Percent Uncontested	195	1755	26.2	19.4	0	100
Percent Dismissed	195	1755	22	16.3	0	100
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 current period long-differenced judicial staffing measures - the number of judges as well as judge vacancy rates - on long-differenced district-level measures from population and economic census including population, demographic composition, urbanization, employment including manufacturing employment, and electoral outcomes in the decade prior to the judicial staffing variation studied. The regression specification is $\Delta_{2018-2010} Judge_d = \alpha_0 + \Delta_{2009-2001} \mathbf{X}_d B + \epsilon_d$, where $Judge_d$ refers to either number of judges or vacancy rate and $\mathbf{X}_d$ is a vector of district-level explanatory variables measured prior to treatment variation. All variables are measured in terms of percentage changes. I use long difference specification because many of the district-level variables are not available at annual frequency. Annual periodicity is only available for the outcomes considered in this paper and the pre-trends in event studies provide additional support for causal identification. I run multiple other falsification tests such as Table A.3 and others reported in the appendix.

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 as reported in Prowess, 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)	(2)	(3)	(4)
	Profit	Profit	Sales	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)	
Dep Var t-1	-0.00816	-0.0683***	0.00268	0.00416
	(0.0179)	(0.0231)	(0.00380)	(0.00484)
Dep Var t-2	-0.0342*	-0.0194	-0.00283	-0.00803
	(0.0179)	(0.0218)	(0.00394)	(0.00522)
Observations	2702	2095	2372	1785
No. Firms	368	298	363	295
Additional		District-Year FE		District-Year FE
Fixed Effects		Industry-Year FE		Industry-Year FE

Standard errors in parentheses

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

Notes: This table presents a log-log regression of profit or sales revenue (dep var) on working capital, interest expenditure, local crime (all crime and lesser crimes) and lagged dependent variables conditional on firm and state-year fixed effects. Following firm-level profit maximizing problem, profit and sales 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, the correlation between sales and interest expenditure is ex-ante ambiguous. A positive correlation could reflect productivity effect from increased working capital and an associated higher total interest payment through quantity mechanism. Alternately, high cost of credit (through higher interest rates) could also increase the interest expenditure but this would negatively correlate with sales (through price mechanism). Columns 2 and 4 introduce district-year and industry-year fixed effects in addition to firm and state-year fixed effects to absorb time-varying unobserved confounders at district and industry levels. Since crime variables vary only at the district-year level, they are dropped due to multicollinearity.

Table 5: Cost-benefit Calculation

Parameter	Value	Units	Source
		Inputs	
Avg. 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 Tax Law
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 Ratio	
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

Notes: I focus on the event of positive staffing-level changes to compute benefit-cost ratios. I apply the estimated treatment effects on district judge staffing-levels and firm-level outcomes to compute the discounted present value of costs and benefits, respectively. Since firm-level outcomes are log transformed, the treatment effects are estimated as percent changes relative to baseline values. I use median profit and wage bill among the sample of firms at baseline and surpluses are calculated relative to these. I calculate the effective income tax rate following the various tax exemptions and the slab corresponding to the average salaried income in the formal sector as reported in the news article listed under Column "Source". This table tabulates inputs other than the treatment effect estimates used in the computation and records the resulting benefit-cost ratio in terms of government net revenue and social surplus net of costs (bottom 2 rows). Bootstrapped standard errors from 1000,000 random draws are reported in square brackets below the cost-benefit estimates. I use a baseline discount rate of 5%. Increasing it to 10% still generates a net tax benefit-cost ratio of 5.52 [1.05] and a social benefit-cost ratio of 29.16 [5.47].

Online Appendix

A.0.A. *Additional details on the context*

District courts across India have over 18 million legal cases pending for 3 or more years and 20 judges per million 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. The judge to population ratio within the US courts is around 100 per million. 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.[2]

A.0.B. *A model of credit market with enforcement costs*

Credit Market I build on the credit contract model in Banerjee and Duflo (2010) to include probability of litigation at a given judicial capacity 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

[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.

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 liquidate borrower's assets to recover their loan. 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 (staffing levels), 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. 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.

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

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

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

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

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, which is also 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 cut-off W^* and project size K_B. Finally, lender's participation constraint is given by

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

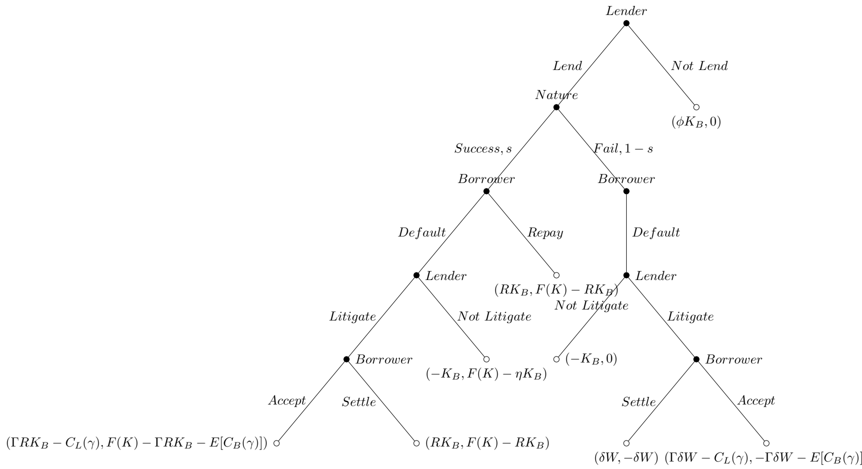
The timing of the game where the lender and borrower decide on their strategies are depicted as an extensive form game below.

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

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

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^*.

Proposition 2: Credit market response to judicial capacity As judicial capacity, γ,



increases, the credit market response varies as follows:

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

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

(c) 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 $g(\tilde{W}), f(\tilde{W}) \to 0$.

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

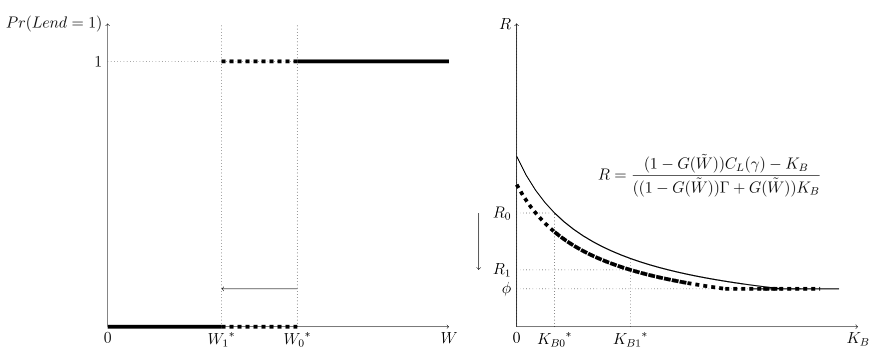
$$- \frac{(1 - G(\tilde{W}))C_L(\gamma) - K_B}{(((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B)^2}\left(\overbrace{g(\tilde{W})\frac{\partial C_B}{\partial \gamma}(K_B - \Gamma)}^{\approx 0}\right)$$

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

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

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

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



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:

$$\text{Max}_{X_1,X_2}\big(\Pi = pQ(X_1, X_2) - w_1X_1 - w_2X_2 - m_i(\gamma)\big) \tag{7}$$

$$s.t\ w_1X_1 + w_2X_2 + m(\gamma) \leq K_i(\gamma)\ i \in \{S, L\}$$

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:

(a) Optimal input use X_1, X_2 increases on average.

(b) Output increases on average.

(c) Profits effects are as follows:

  (i) For large firms, L, optimal input use and profits increase if decreases in monitoring costs and cheaper credit more than offset the increase in input expenditure.

  (ii) For marginal small firms, S, optimal inputs and profits increase if increase in borrowing is sufficiently large to offset the increase in input expenditure.

  (iii) 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:

$$\mathcal{L} = pQ(X_1, X_2) - w_1 X_1 - w_2 X_2 - m_i(\gamma) + \lambda\big(K_i - w_1 X_1 - w_2 X_2 - m_i(\gamma)\big)$$

FOC:

$$\begin{aligned}
\frac{\partial \mathcal{L}}{\partial X_1} &= pQ_{x_1} - w_1 - w_1\lambda = 0 \\
\frac{\partial \mathcal{L}}{\partial X_2} &= pQ_{x_2} - w_2 - w_2\lambda = 0 \\
\frac{\partial \mathcal{L}}{\partial \lambda} &= K_i - w_1 X_1 - w_2 X_2 - m_i(\gamma) = 0
\end{aligned}$$

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:

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

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:

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

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

$\frac{\partial h^*}{\partial \gamma} > 0$ 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. This can be summarized as:

(a) 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.

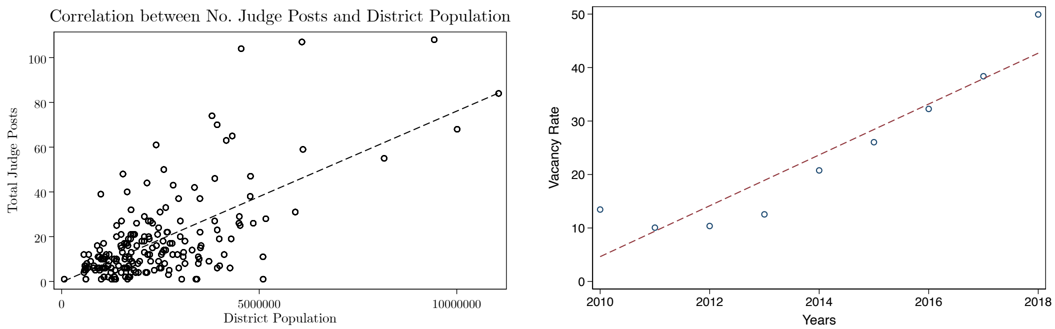
(b) For marginal small firms, $i = S$ and $W \approx W^* - \epsilon$, the marginal benefit $K_B - \frac{\partial m_S}{\partial \gamma}$ is due to both availability of borrowing from banks K_B as well as a reduction in monitoring costs. I assume that the monitoring costs for small firms do not decrease 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.

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

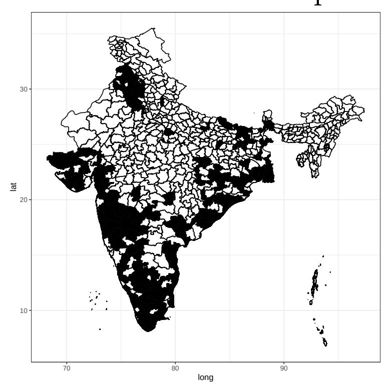
A.0.C. Appendix: Figures

Figure A.1: Judge Posts, Vacancy, and District Population

Panel A: Court-size, vacancy, and district population

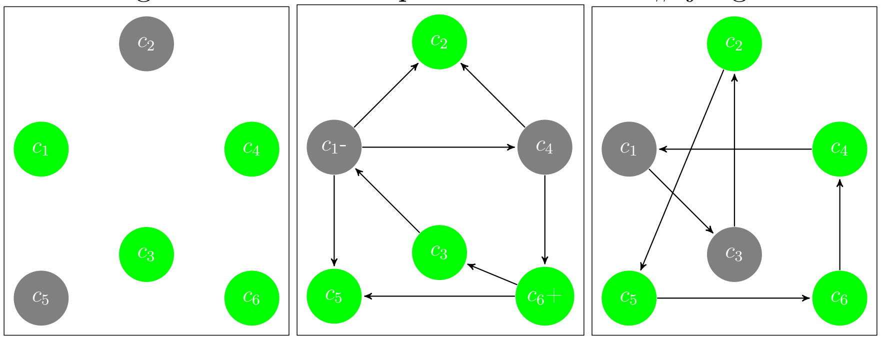


Panel B: Court Sample



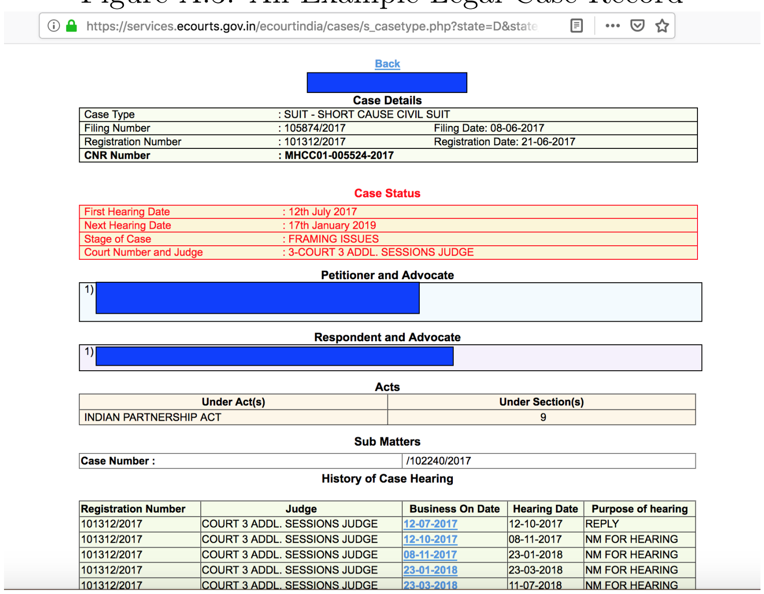
Notes: Panel A shows correlation between district judge posts and population (left) and vacancies over time (right). Panel B shows the geographic distribution of the study districts.

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 and arrows refer to judge reassignments. New recruitment and retirements are shown as + or − within the nodes, respectively. Green node implies no judge vacancy and gray node implies some judge vacancy. Panel 1 is the initial condition. At t=1, C2 and C5 no longer have any vacancy whereas C1 and C4 experience vacancy as a result of staffing dynamics. C6 remains at full occupancy even when C6 has a new recruit assigned as turnover cancels out new additions. 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 even if they experience staffing dynamics.

Figure A.3: An Example Legal Case Record



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

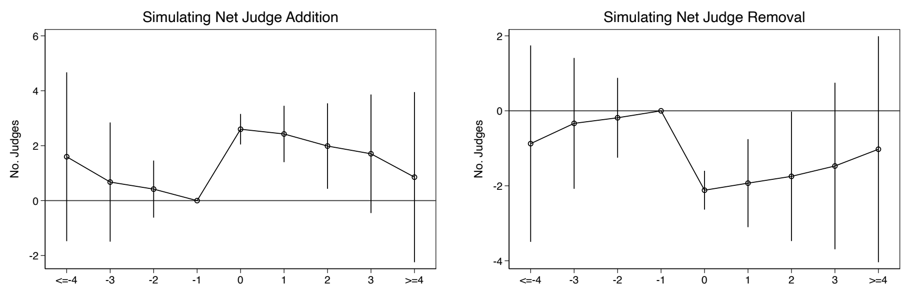
History of Case Hearing

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

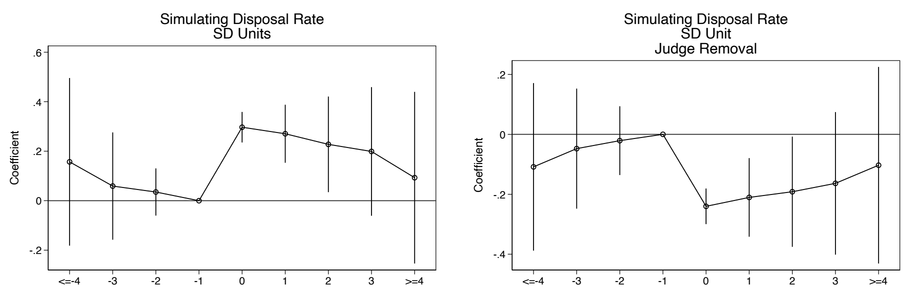
Notes: Above page is an example legal case record. This data can be accessed from the E-Courts database. Names have been redacted for privacy.

Figure A.4: Multiple Event Study Estimator: Simulation

Panel A: Number of Judges

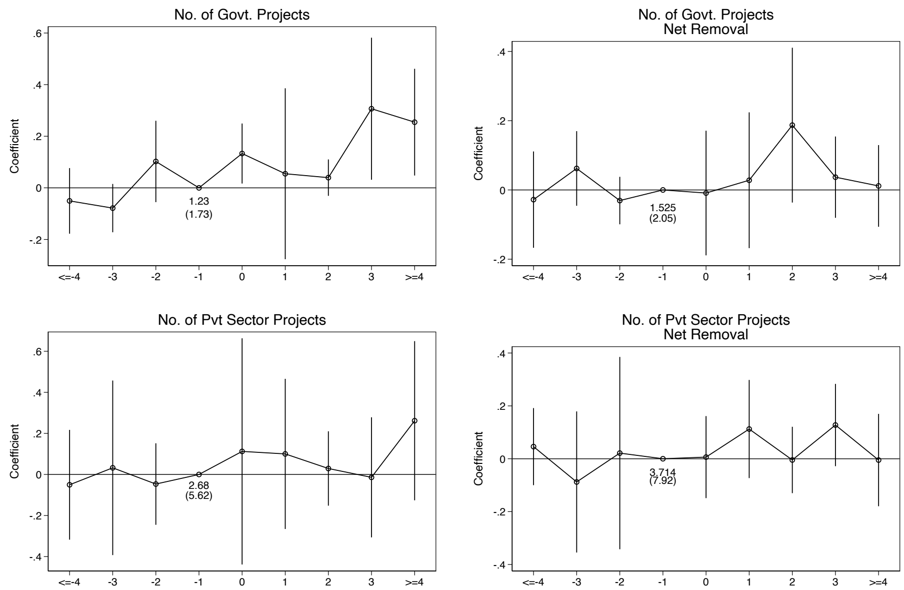


Panel B: Disposal Rate



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 - where the number of judges added or removed is drawn from $\mathcal{U}(1, 4)$, generating a 0.3 standard deviation (SD) effect on disposal rate. 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.

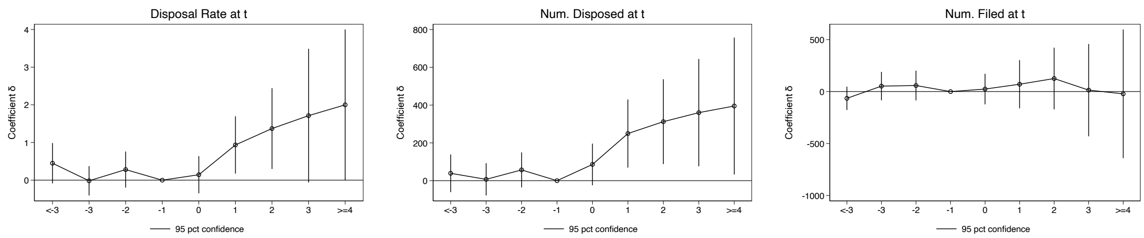
Figure A.5: Correlation of Government or Private Sector Investments with Staffing Changes



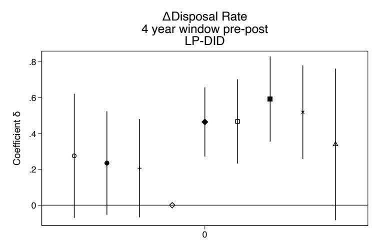
Notes: The figures present the stacked event study estimates using the number of government and private investment projects in a particular district as an outcome variable as in Equation 1. The outcome variable is obtained from CMIE CapEx database of investments, including infrastructure and those generating 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.6: Court Outcomes: Alternate Specifications

Panel A: Continuous Explanatory Variable



Panel B: Local Projection DiD



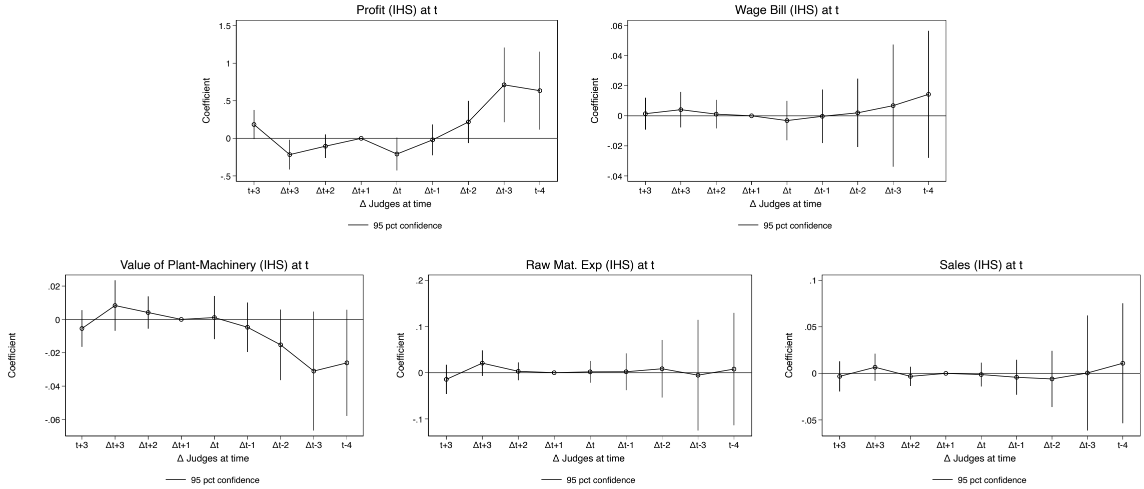
Notes: The figures in Panel A present the generalized event study estimates relative to number of judges from $t+1$ when the court-level outcomes are measured at t as in Equation 2. The value labels on the x-axis needs to be interpreted differently from those in standard event study figures - positive 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. Panel B follows 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:

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

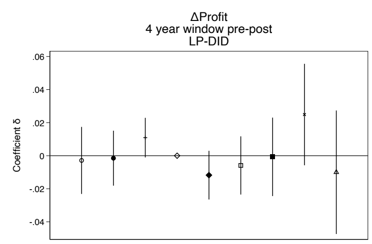
where Δ is the first difference operator.

Figure A.7: Firm-Level Outcomes: Alternate Specifications

Panel A: Continuous Explanatory Variable



Panel B: Local Projection DiD



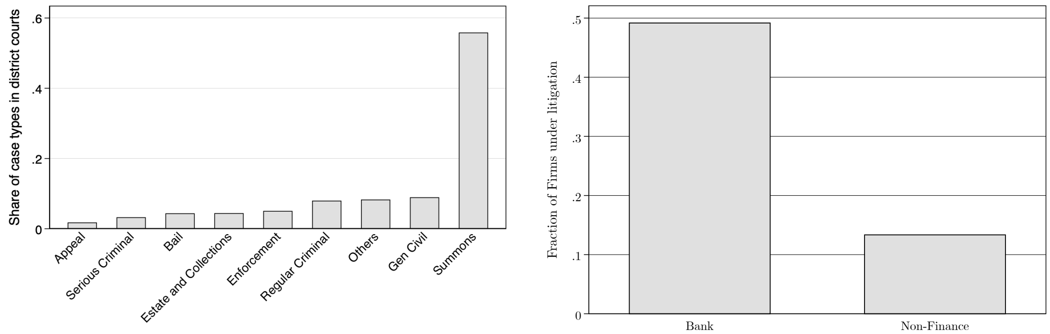
Notes: The figures in Panel A present the generalized event study estimates relative to number of judges from $t+1$ when the firm-level outcome is measured at t as in Equation 2. Each estimate includes 95% confidence interval. Standard errors are clustered by district. Following Dube et al. (2022), the 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 Panel B 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:

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

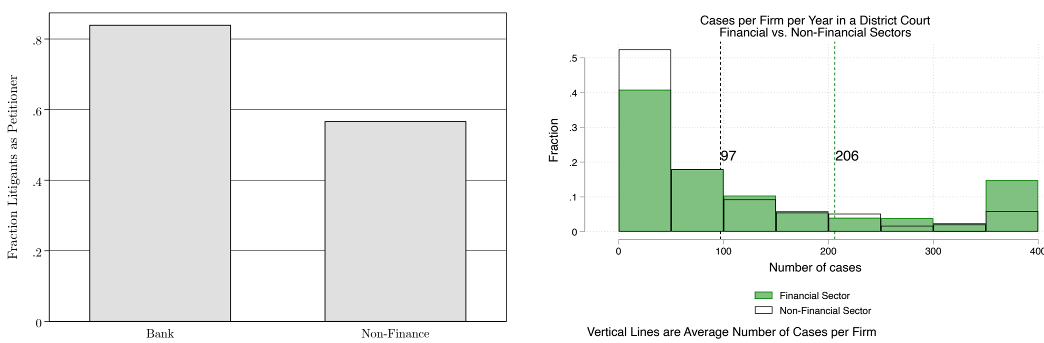
where Δ is the first difference operator.

Figure A.8: 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).

A.0.D. *Appendix: Tables*

Table A.1: Sample Districts

	(1) All Districts India	(2) All Districts 15 States	(3) In Sample	(4) Not In Sample
Total Population	1833157	2385557	2354695*	2417600
	(1464148)	(1520684)	(1608736)	(1427238)
Rural Population	1325590	1702401	1587978	1821201
	(1016274)	(1057026)	(1011937)	(1091998)
Urban Population	507567	683156	766716	596399
	(753773)	(877306)	(1007732)	(709735)
Scheduled Caste Population	311462	424804	397691	452954
	(312795)	(335244)	(329054)	(340165)
Scheduled Tribe Population	163789	143965	170592*	116320
	(250283)	(256964)	(299357)	(200981)
Literate Population	1143112	1507835	1554907*	1458964
	(983559)	(1046325)	(1166911)	(904958)
Non-Agri Employed	109740	150464	174337	125679
	(135820)	(157205)	(174676)	(132722)
Manufacturing Employed	29492	40614	48729*	32188
	(42797)	(50351)	(56244)	(41915)
No. Districts	622	373	190	183

Notes: This table describes and compares population and economic census measures - mean and standard deviation in (.) - between sample and not in sample districts from the period prior to the study period in 15 industrial states of India. Column 1 is the summary statistics from all districts across India as of 2011 census. Column 2 presents the overall mean and standard deviation across districts within the 15 industrial states in the sample. Columns 3 and 4 present the mean and standard deviation of sample and not in sample districts within the 15 industrial states. The sample districts are slightly more urban, literate, has more tribal population, and is more engaged in manufacturing. These differences are only significant at 10% pair-wise as well as jointly.

Table A.2: Pairwise Correlations Between Different Measures of Court Performance

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.3: Falsification Test: Does Vacancy Predict Staffing-Level Change?

	(1) Judge Staffing-Level Change	(2) Judge Staffing-Level Change
Vacancy (t-1)	0.0934	0.0924
	(0.0676)	(0.0681)
Backlog per FTE (t-1)	0.000172	0.000250*
	(0.000110)	(0.000149)
Δ_t-1 Disposal Rate		-0.00717
		(0.00784)
New Cases per FTE (t-1)		0.000241
		(0.000515)
Cases Settled per FTE (t-1)		-0.000772
		(0.000540)
Observations	1746	1746
No. Districts	194	194
Adj R2	0.234	0.234

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

Notes: This table regresses changes in judge staffing level (No. Judges (t) - No. Judges (t-1)) on vacancy (shortfall in judicial staff with respect to FTE), conditional on backlog. Col 2 includes additional district court-level variables to assess whether any other past period performance parameters addresses staffing-level changes. All variables pertaining to number of cases are adjusted per full time judicial staff equivalents (FTE). Regressions include district and state-year fixed effects and standard errors are clustered at the district-level as in all specifications in this study.

Table A.4: Court Outcomes and Judge Vacancy Changes

	Net Judge Addition			Net Judge Removal		
	(1)	(2)	(3)	(4)	(5)	(6)
	No. of Judges	100 - Vacancy Rate	Disposal Rate	No. of Judges	100 - Vacancy Rate	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 2. 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.5: 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.6: 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.7: Case Composition

	(1) Total Backlog	(2) Firms' Case Backlog	(3) Percent Uncontested	(4) Percent Firms' Uncontested	(5) Percent Dismissed	(6) Percent Firms' Dismissed
Judge Staffing-Level Change	-756.6***	-144.6***	0.126	0.422***	-0.197***	-0.369***
(No. Judges (t) - No. Judges (t-1))	(132.2)	(33.76)	(0.133)	(0.140)	(0.0726)	(0.0883)
Observations	1746	1746	1746	1746	1746	1746
No. Districts	194	194	194	194	194	194
Control Mean	14616.12	2804.79	25.54	39.64	23.86	32.93

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

Notes: This table records the association between changes in case composition and judge staffing-level changes. Total backlog is the total number of legal cases that have not been resolved yet. Firms' case backlog is the total number of backlogged legal cases where a firm is one of the litigators. Percent uncontested is the percentage of decisions that are uncontested by either of the litigating parties. This variable is similar to appeal in meaning. Percent dismissed is the percent of decisions that are dismissal of a case without completing the trial. All court-level specifications include district and state-year fixed effects. Standard errors are clustered by district.

Table A.8: 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 3, 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.9: 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 4, 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.10: 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.11: 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.12: 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.13: 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.14: 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.15: Credit Mechanism: Firm-level Heterogeneity by Size

	Net Judge Addition			Net Judge Removal		
	(1) Working Cap. (IHS) Low Lev Small Firms	(2) Interest Exp (IHS) Low Lev Small Firms	(3) Profit (IHS) Low Lev Small Firms	(4) Working Cap. (IHS) Low Lev Small Firms	(5) Interest Exp (IHS) Low Lev Small Firms	(6) Profit (IHS) 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.