Front-line Courts As State Capacity: Evidence From India

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

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1 Introduction

Courts play a central role in enforcing contracts and property rights, which supports the development of formal financial sector, investment, and economic growth (La Porta et al. 1998; Djankov et al. 2003). Long lags in dispute resolution due to congested courts can increase uncertainty and transaction costs that impede effective contracting and weaken *de facto* property rights (Johnson et al. 2002; Laeven and Woodruff 2007; Ponticelli and Alencar 2016; Sadka et al. 2018). Despite this, courts are chronically underinvested in developing countries, reflected in the low judge-population ratio and the enormous pending case backlog per judge that are many times higher compared to high income countries. This underinvestment is severe in front-line courts that are citizen-facing, have the largest caseload, and have the highest pending backlog. For example, district courts in India have fewer than 20 judges per million population, and have over 18 million legal disputes pending for more than 3 years, which translate to 5 times fewer judges per capita and 10 times higher backlog per available judge relative to similar 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 impact of adding judges to district courts with vacancies in India, by leveraging a first of its kind court-level panel data, merged with key economic outcomes. I show that improving judicial staffing substantially reduces pending case backlog and enhances the productivity of local formal sector firms, with indications of broad-based improvements in the local economy. Importantly, the economic returns are large and rapid, occurring within a short timescale of 2-3 years from the time of staffing increase.

District courts in India, similar to county courts in the US/UK, are the relevant front-line judicial institutions that are central to improving judicial capacity. These courts have jurisdiction over the smallest administrative unit, face the largest legal caseload (44 million cases), and have the highest pending case backlog (29 million cases). A majority of cases in these courts pertain to debt recovery and property disputes, with millions of dollars worth assets stuck under litigation over long periods of time. Resolution of debt recovery cases is particularly important for banks facing credit supply constraints, where recovery of debt defaults enables credit circulation, with implications for economic development (Castellanos et al. 2018; Breza and Kinnan 2021; Bazzi et al. 2023).

For causal identification, I leverage the timing of judge staffing-level changes between 2010 and 2018, constructed using the universe of case-level time-stamp data for the period, in a stacked event study design (Cengiz et al. 2019). This design accounts for dynamic and heterogenous treatment effects (Sant'Anna and Zhao 2020, Sun and Abraham 2021) and is particularly well-suited to the setting of this study where the changes in staffing-levels occur more than once and are bi-directional, comparing courts experiencing a positive (or a negative) change with those experiencing no change. The staffing variation results from a combination of recruitments, retirements, and rotation of judges between district courts. These changes at the court-level are driven by state-level

[1] This is based on calculations using data from the National Judicial Data Grid for India and respective state and federal court websites for the United States. See Appendix A.1 for details on data and calculation.

policies on retirement at 60 years of age, sporadic and often failed recruitment drives, and frequent rotation of judges between district courts, which I show are unrelated to existing court backlog and other changing socio-economic and political conditions in the district.[2] These generate sharp and persistent discontinuities in the number of judges and vacancy rates. I also estimate the impact by employing generalized difference in difference (DiD) research designs. Specifically, I implement an event study design with continuous-valued judge staffing variable (Schmidheiny and Siegloch 2020; Freyaldenhoven et al. 2021) and local projection DiD design (Dube et al. 2022). Assuringly, I find no significant trends in the prior period across key outcomes using any of the methods as a support for the parallel trends assumption. The causal-effect estimates are also qualitatively similar across the estimation strategies.

On economic outcomes, I focus on locally registered, tax-paying firms and district-level economic outcomes, mapping to the geographic jurisdiction of each court in my sample. Local judicial capacity affects firms, including those in the formal sector, because they borrow from local branches of banks (Nguyen 2019) and seek protection from property and financial crimes such as theft and embezzlement (Bandiera 2003).[3] There are two main channels through which local judicial capacity can affect firm productivity: (a) contract enforcement, which is particularly relevant for the recovery of bank capital stuck under litigation, affecting local credit supply, and (b) protection of property that enable firms to safeguard their stock of raw material, inventory, and capital goods. These channels are important in this context, where a large majority of court cases pertain to debt recovery by banks and minor criminal offenses concerning property. Resolution of such cases could plausibly increase liquidity-driven additional lending by banks to firms. A rich literature has documented how firms in developing economies are credit constrained (e.g., de Mel et al. 2008; Banerjee and Duflo 2014; Bazzi et al. 2023), and thus, access to capital - particularly to finance operating expenditures through bank loans, could improve firm productivity.

Building on this intuition, I develop a conceptual framework centered around profit maximization by firms in the presence of monitoring costs and credit constraints to guide my empirical analysis. I start with a standard lending model (as in Besley and Coate 1995 and Banerjee and Duflo 2010) and introduce a contract enforcement parameter that affects credit availability and price of credit for firm-level production. Additionally, firms incur monitoring costs to protect their property from thefts, which also vary as a function of local judicial capacity. A key implication of this framework is that lenders respond to an improvement in contract enforcement capacity by expanding access to credit to hitherto unbanked firms (by reducing wealth threshold for lending) as well as lower the

[2] No one agent - the judiciary, the executive, or the elected representatives - alone controls these judge staffing policies, which often requires close coordination between two or more agents, unlike in the context of general administration bureaucrats where elected representatives play a central role (Iyer and Mani 2012; Khan et al. 2019). This further adds to the uncertainty in the timing of these changes.

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

price of credit (interest rate), generating productivity implications. Further, firms experience lower monitoring costs with better judicial capacity.

On the empirical side, I start by estimating the reduced form effects on court, firm and district-level productivity outcomes following changes in judge staffing-levels. To understand the economic mechanisms stemming from the conceptual framework behind improved productivity gains, I conduct two empirical exercises. To shed light on the credit channel, I examine district-level lending by banks to industrial borrowers as well as firm-level working capital and interest expenditures on all borrowing. I also examine heterogeneity by firm-size (asset size) to test the implications of credit expansion to firms with lower debt exposure. For the second channel, I examine the role of lower monitoring costs by examining district-level crime reporting outcomes and firm-level expenditure on raw material.

There are three classes of results. First, I find a significant effect on court-level outcomes when there is a net increase in the number of judges relative to when there are no changes. These include a persistent effect on reducing vacancies, where a positive staffing change results in two more judges added to the court on average. Correspondingly, I note an increase in the number of case resolutions by 200 cases per additional 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 changes have roughly half the effect size in reducing the staffing levels, and thus have commensurately smaller effects on disposal rate.[4]

Second, local firm-level productivity improves substantially following net increases in the number of judges and decreases following net reductions. Specifically, firm-level wage expenditure, sales revenue, and profits respond significantly to changes in judicial staffing. I find that the average wage bill and sales revenue increase by around 5% and 2%, respectively, in the long run when more judges are added in net. The effect on profit is substantial at over 40%, reflecting both productivity and accounting improvements. On the other hand, a decrease in the number of judges has a negative effect: wage bill and sales contract by around 2%. Profits drop by 20% in the long run. Since the net change in the number of judges following negative events is half the change following positive events, the effects on productivity measures are symmetric per-judge. These effects are significant economically since the sample of firms are among those contributing a large share of value addition and employment in India. Further, these effects appear with a lag, consistent with the economic framework, where the firms' optimization follows changes in the credit market and monitoring costs.

I take a number of precautions and perform different robustness checks to confirm firm productivity results that I discuss in detail in Section 5. First, I note that the results are not driven by changes in the composition of firms, such as differential firm exits that could lower competition in the districts, overestimating the treatment effects. In contrast, I find that improved judicial staffing results in higher entry of firms through new incorporations without affecting exits. Greater firm

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

entry could imply better business dynamism and increased competition. This would downward bias the estimated impact. Second, I find that these effects are also seen among the subset of firms with no legal cases across the entire study period. This supports the fact that the estimates capture beyond any immediate effects due to case resolution for the litigating firms. Finally, I note that the effects are only observed among local firms and not among firms in the neighboring districts where the district court has no jurisdiction, suggesting that these effects are not due to any spurious correlation. More broadly, I find suggestive positive effects on district night light intensity following net addition and negative effects following net reduction in the number of judges. These broad-based effects suggest that the the firm-level estimates are likely a lower bound of the actual economic gains from judicial capacity improvements.[5]

Third and related to the economic mechanisms discussed earlier, I note an immediate increase in firms' working capital and a reduction in interest expenditure following net judge addition. Firms also increase their expenditure on raw material. At the market (district)-level, I find an increase in aggregate lending by banks to industrial borrowers and a drop in reported crime rates following positive staffing changes. This correlation between improved judicial capacity and credit circulation is consistent with the findings of Ponticelli and Alencar (2016) and Müller (2022) in the context of better bankruptcy enforcement. Importantly, what I show is that courts enable routine debt recovery and general contract enforcement that typically precedes bankruptcy, which is critical given the larger scale of debt recovery disputes. This enables credit circulation in a context with supply constraints. The credit mechanism is also supported by the results on heterogeneity by firm-size. Specifically, I note an increase in working capital, reduction in interest expenditure, and an increase in profit among smaller firms with low ex-ante debt exposure (leverage ratio). This suggests extensive margin increases in banks lending to smaller firms with previously low-levels of borrowing, improving their productivity, and thus spurring local economic development.

On the other hand, a net reduction in the number of judges does not lead to a symmetric decline in firms' access to capital or bank lending behavior but is associated with an increase in lower-order recorded crimes, such as thefts and property crimes. The lack of credit effects within my study timeframe subsequent to negative staffing changes is plausible in the presence of natural lags in recognizing defaults and time lag in the accumulation of debt-recovery cases in courts (Ashraf et al. 2020; Breza and Kinnan 2021).[6] In contrast, the noted increase in lower-order crimes could increase

[5]The main firm-level analyses use a balanced panel of incumbent firms that report balance sheet data for each year in the study period. I do this primarily to ensure internal validity. However, this raises an important concern whether the estimated effects are due to sample construction, particularly if the composition of firms not in the balanced panel sample vary over time that could be correlated with both judge staffing changes and sample firms' outcomes. I address this in two ways. First, I find positive effects on new firm incorporations and total firms in the district following net judge addition but no effect on either following net reduction. This suggests that the incumbent firm-level results are observed even in the presence of new firm entry and is likely lower than what the effect may have been in the absence of increased competition. Second, I find similar effects qualitatively, using the larger, unbalanced panel data. However, I find that data for many variables are missing non-randomly - that is, data reporting is correlated with judge staffing changes but without any pre-period trends. While the latter is assuring, the former suggests that using the unbalanced panel will not produce unbiased estimates of the causal effect and I abstain from using it for my main analysis.

[6]Based on conversations with bankers in India, the general debt recovery strategy involves litigation in courts as

the costs of property protection for firms, consistent with the observed decline in firms' productivity.

These findings highlight substantial economic gains generated by strengthening staffing levels in the frontline 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). On costs, I consider the personnel expenditure per judge using the recommended salary and non-wage compensation from the Second National Judicial Pay Commission. The recommended compensation is typically higher than the current compensation structure across district courts, and therefore, the cost calculations are conservative. My calculation suggests that adding more judges 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 effects not examined in this analysis and imputed costs are higher than actuals.

It is important to note that this paper documents the economic impact of marginal changes to judicial staffing in district courts. Substantive changes in personnel policies such as increasing the steady-state staffing levels or upgrading court infrastructure may have different implications, and require further study. Additionally, it is also plausible that the benefits estimated in this paper are more likely driven by liquidity implications for bank lending from resolved legal cases. While data limitations does not allow me to separately identify the role of liquidity, this is a plausible mechanism given the magnitude of total bank capital under litigation (in a context where over 10% of the \$170 billion commercial bank lending portfolio was declared as non-performing asset/NPA until recently as per India's central bank estimates). So even a 2-3 percentage point improvement in court backlog resolution could unfreeze a large amount of capital (0.1 x \$170billion x 0.02 $\approx$ \$300 million) stuck in litigation. However, additional research is needed to differentiate the liquidity channel from court's role in creating and maintaining trust in economic and financial transactions and remain as open questions for future research.

This paper makes several contributions. First, the findings in this paper underscore the importance of general courts of law for local economic development through an expansion of formal sector economic activity. In this regard, this paper provides evidence on the microfoundations connecting legal institutions and economic growth (La Porta et al. 1998; Djankov et al. 2003; Johnson et al. 2002; Laeven and Woodruff 2007; Nunn 2007) and builds on existing empirical literature on courts and development (Chemin 2009a,b, 2012; Ponticelli and Alencar 2016; Amirapu 2017; Kondylis and Stein 2018; Mattsson and Mobarak 2023). A novel contribution of this paper is highlighting the significant transactional role that ordinary frontline courts (as opposed to specialized tribunals or higher-level courts) play in the efficient functioning of local markets, ranging from credit markets to safeguarding private property. Consistent with existing literature that mainly exploit cross-sectional

the last stage in the recovery process. Bank managers try other methods for recovery first, such as sending notices, collection agents, etc., before filing a case in court, naturally introducing delays between occurring of a loan default and filing a case in the court. However, once cases are filed in a court, the timing of resolution has more immediate implications on recovery and liquidity through changes in the bank branch-level balance sheet entries. I discuss these in greater detail in Section 2.

variation, I leverage court-level panel data and judicial staffing changes over time. In doing so, I also contribute a first of its kind court-level panel dataset that include many key measures of judicial capacity - from staffing to backlog resolution rate - merged with firm and district-level economic outcomes.

Second, this paper connects judicial capacity with the development of financial sector in developing countries. Faster and efficient debt recovery has been a core focus of many economic policies (Visaria 2009; von Lilienfeld-Toal et al. 2012; Lichand and Soares 2014). In spite of specialized courts for debt recovery, general courts of law continue to be the final authority on contract enforcement, particularly when it comes to executing judgement orders, creating a heavy reliance on local courts by the financial sector. Institutions supporting the development of a strong financial sector are fundamental for firm and economic growth through access to credit (Rajan and Zingales 1998; Burgess and Pande 2005; Castellanos et al. 2018; Breza and Kinnan 2021; Bazzi et al. 2023) and inputs (Boehm and Oberfield 2020). This paper shows that local courts help unlock capital tied-up in legal disputes following an increase in judge staffing, potentially generating liquidity implications for corresponding bank branches. In the presence of financing frictions preventing costless movement of capital between banks or their branches (Khwaja and Mian 2008; Paravisini 2008; Schnabl 2012; Castellanos et al. 2018; Rigol and Roth 2021), the resulting liquidity from debt recovery can affect local supply of credit for manufacturing and industrial uses.

Third, this paper demonstrates that investment in frontline courts generates large and rapid returns, strengthening state capacity (Besley and Persson 2009). One plausible reason for underinvestment in courts in developing countries could be a misalignment between political incentives to invest relative to the perceived timescale of economic returns to improved functioning of courts. I show that the returns from adding an additional judge more than pays for itself and generates large welfare gains within the time horizon of electoral cycles. This contributes to the evidence on program implementation for strengthening state capacity (Muralidharan et al. 2016; Lewis-Faupel et al. 2016; Banerjee et al. 2020; Ganimian et al. 2021) by studying staffing constraints in courts. Another related contribution is to the literature on the personnel economics of the state (Dal Bó et al. 2013; Muralidharan and Sundararaman 2013; Coviello et al. 2015; Khan et al. 2015; Neggers 2018; Dasgupta and Kapur 2020; Fenizia 2022; Narasimhan and Weaver 2023; Mattsson and Mobarak 2023) in the context of subnational courts and legal services. Importantly, this paper estimates benefit-cost ratio using direct measures of economic outcomes including wage bill and firm profitability.[7]

The rest of the paper is organized as follows. Section 2 discusses the context, detailing both the judicial organization structure and how this interacts with local credit market and crime environments. Section 3 documents the data sources, and discusses the construction of court and economic outcome variables. Section 4 details the empirical strategy for causal identification, with the main results summarized in Section 5. Section 6 discusses potential mechanisms situated within an eco-

[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.

nomic framework on access to credit. I discuss the broader implication of local judicial capacity using back of the envelope benefit-cost analysis in Section 7. Section 8 concludes.

2 Context

The judiciary in India is a three tier unitary system: district courts, where the bulk of trials 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.

Due to separation of powers, the judiciary has to coordinate with both the executive and the legislature for its effective functioning. While the judiciary alone manages its organization structure and sets internal policies, it relies on the executive for budgetary approvals and funding, and the legislature for laws, including amendments to procedural codes. Coordination failures underpin many of the constraints in expanding judicial capacity. One such key constraint that I examine in this paper is inadequate judge staffing levels that the judiciary alone is unable to address. I describe the judicial staffing constraints in detail in the following sub-section.

2A Judicial Staffing

The number of judges relative to India's population is perhaps one of the most critical constraints. On average, there are 20 authorized judge posts per million. In contrast, there are close to 100 judges per million in the United States and close to 200 per million in the European Union as per official statistics. This ratio is further reduced when we account for the extent of vacancies in these posts.

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

The judiciary also faces persistent vacancies. About a quarter of judge posts in district courts are vacant, which have continued or worsened over the years (Panel A Figure A.1). Though vacancies are natural as judges reach retirement age, they persist or worsen if recruitment does not catch up with the extent of turnover. Addressing vacancies in district courts requires close coordination between the judiciary and the state-level executive, particularly to organize and implement recruitment drives. These are implemented sporadically, with varying success rates.[8]

Personnel policies such as judge tenure and assignment to courts are handled exclusively by the state-level high courts. District judges are state officials appointed by the corresponding high court. They are senior legal professionals, who are either inducted from the local bar council or promoted from sub-district courts after reaching seniority. A few are directly hired through competitive exams. They typically serve 10-15 years before retiring, unless promoted to the state high court, if at all. These judges serve a short tenure in any given court - 2-3 years, and are either rotated (reassigned) to a different district court or retire from the court where they turn 60 years in age during their tenure.[9]

Three personnel policies - recruitments, retirements, and rotation between courts - generate both positive and negative changes in the number of judges in a court during my study period. Figure A.2 presents a schematic to show this dynamic and how this affects judge staffing in a court over time.

2B Courts and Bank Credit Circulation

Financial sector enterprises such as banks rely on district courts for executing debt contracts by enabling last resort recovery. As confirmed during my qualitative interviews with a sample of bank managers and their legal counsels, banks lend to borrowers only 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. 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.

[8]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 (Panel B, Figure A.1).

[9]The specific assignment process for determining which judges are to be rotated where is based on a seniority-first serial dictatorship mechanism, subject to the specific constraints: non-repeat and no home district assignment. A judge coming up for a reassignment is asked to list 3-4 rank-ordered district court locations for their next posting. The high court committee collates these lists and carries out the assignment algorithm each cycle. First, the senior-most judge is assigned their top ranked location. Next, the second senior-most judge is assigned their top-ranked location 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 a court with vacancy, subject to the home district constraint. This process is relatively similar across all states in India.

This model of lending is followed both by the dominant public sector as well as the growing private sector banks. All banks are regulated by the central bank - Reserve Bank of India, and follow national-level monetary and lending policies. Important among these policies are branch-level lending quotas and targets per year, with additional quotas for specific economic sectors (for example, small and medium enterprises or agricultural borrowers). Therefore, even when a specific branch is part of a large public or private-sector bank spanning national or international markets, the amount of credit for circulation is typically determined based on local targets.

Each branch maintains annual balance sheet, recording profits and losses generated from their operations. The details of lending, repayments, and write-offs due to unpaid debt, all are accounted in these documents. Branch managers face career incentives based on their performance tied to lending targets as well as the overall health of their branch's balance sheets. These managers are also the authorized representatives of the bank in legal cases, where the specific court's jurisdiction is predetermined by the legal procedure. Write-offs due to non-payments enter as expenditures whereas recovered capital as income. Thus, whenever pending legal cases in courts pertaining to unpaid debts are resolved, recoveries from following court's execution orders are considered income, and serve as positive liquidity shock to the branch.

Bulk of the lending portfolio of banks in India are loans towards agriculture as well as consumption of individuals and households (called personal loans).[10] Unsurprisingly, most of the defaults also arise from defaults of personal and agricultural loans. Income shocks to individuals lead to defaults such as non-payment of credit card dues (unsecured loans), mortgage payments, and non-payment on other such loan products. The quantum of the total write-off from such defaults, when aggregated over multiple individual borrowers in the absence of strong individual-level bankruptcy regulations in India, imply that there are potentially large write-offs for the bank. Enabling settlements in such default cases is particularly important for the health of local branch balance sheet. To illustrate with an example, each pre-trial mediation and arbitration session for debt recovery (facilitated by the district legal services authorities) prior to filing them as legal cases in the district court yields about USD 240,000 in recovery-induced liquidity at the district-level. Summing across 4-6 sessions in a year, this generates about USD 1-1.5 million in recovery on an annual basis per district, which serves as a positive liquidity shock to the local branch balance sheet.[11] Liquidity shocks arising from settlement of legal cases in district courts are also similar in magnitude.

This context on banking reveal two important facts relevant for this paper: (a) defaults are common, especially from the bulk of loans to private individuals/households, and (b) resolution or settlement of debt recovery cases in district courts generates branch-level balance sheet effects. In contrast, delays in debt recovery resolution does not immediately affect the workflow of recognizing and filing such litigation, which first has to account for the write-off arising from the default. The

[10] Calculated using district and sector-level lending data across all banks, made available through data repository at the Reserve Bank of India. Agricultural loans are disbursed to farming households with agricultural land under cultivation. Agriculture is also considered priority sector under the central bank's lending policy.

[11] Calculated based on the official statistics by the National Legal Services Authority for sessions held in district courts across India, which is available at https://nalsa.gov.in/statistics.

procedural steps in recognizing a loan as a non-performing asset and subsequent write-off add natural lags in the filing of debt recovery petitions in district courts.

2C Courts and Law Enforcement

The district courts are general courts of law, with jurisdiction over every kind of legal dispute - whether civil or criminal. Capacity of these courts are also important for containing crime, which in turn could affect economic productivity in the area. However, courts and law enforcement, i.e. police, have to coordinate in containing crime. While police play a more direct role in violent crimes, the bulk of crimes are non-violent, and concern property crimes such as thefts, where the functioning of courts is important through imposing fines and recovery of property.

Importantly, a large bulk of criminal cases in district courts are what are known as "summary trial" cases. A few examples of these according to the Code of Criminal Procedure are (a) "Offense of theft, under section 379, section 380 or section 381 of the Indian 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

3A Court-level Variables: Explanatory Variables

I assemble the universe of 6 million public legal case records from the E-Courts database, spanning all legal cases filed or pending for resolution between 2010 and 2018, from a sample of 195 district courts (Figure A.3). These districts were selected to ensure an overlap with the location of registered formal sector firms across non-metropolitan industrial districts and are representative of other similar districts in India. Each record details the case meta-data as well as lists hearing dates with the corresponding hearing stage.[12]

Judge Headcount and Vacancy: The meta-data includes the courtroom number and the judge designation where a case has been assigned.[13] Leveraging the fact that the data represents

[12]E-courts is a public facing e-governance program covering the Indian judiciary. The setting up of infrastructure for the computerization of case records started in 2007 and the public-facing website - www.ecourts.gov.in and https://njdg.ecourts.gov.in - went live in late 2014. The fields include date of filing, registration, first hearing, decision date if disposed, nature of disposal, time between hearings, time taken for transition between case stages, litigant characteristics, case issue, among other details.

[13]For example, courtrooms numbered 1, 2, 3,... and the judge designations are labeled Principal District Judge (PDJ), Additional District Judge (ADJ) 1, ADJ 2, etc.

the universe of legal cases between 2010 and 2018, I enumerate judges within a court over the study period based on annual workflow observed for a given courtroom, which has an assigned judge, generated from the rich timestamp information.

I define annual workflow as follows: I record a courtroom as active (i.e., with a judge) for a given calendar year if I observe newly filed cases in that year assigned to that courtroom. The court registrar assigns new cases to all incumbent judges, who have assigned courtrooms, immediately after filing and verification of an application by petitioner(s). When an incumbent judge moves (either due to rotation or retirement) with no replacement, that specific courtroom remains vacant and no new cases are assigned to the courtroom. The existing workload at the time of vacancy is transferred to other judges/courtrooms within the court. While I also expand the workflow definition to include case resolution, outcome of a hearing, and passing interim orders as a robustness check, using these isn't my preferred method for constructing the number of judges precisely because existing workload at the time of the vacancy is reassigned to other judges, creating a bias in enumeration.

Following this algorithm, I generate the number of judges in a district court for each year in my study period. These numbers generate a similar aggregate measure at the state-level, as reported in the Law Commission Reports. I also calculate vacancy rate as the relative shortfall in the number of judges in a given calendar year relative to the maximum number of observed judges in the court within the study period. This construction of vacancy rate assumes that the maximum number of judges is indeed the total number of posts, and is agnostic to long-run vacancies or an increase in the number of posts in a court. To be conservative, I restrict all my analyses using annual changes in the number of judges rather than changes in vacancy rates, which requires additional assumptions.

In the absence of data repositories of district judge tenures and their biographies, this construction contributes an important measure of local judicial staffing levels and capacity. Since the launch of the e-courts system, each courtroom's daily business is directly recorded on a digital platform that then periodically updates the e-courts legal case database with the latest status of cases heard on a given day. This follows the central objective of the Supreme Court of India's e-courts committee to reform data capture of courts' proceeding directly on digital platforms rather than digitize physical court records at a later point in time.[14]

Defining Staffing Change Events: As described above, I calculate the number of judges in a district court from the case filing dates. 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.

Constructing annual court-level performance variables: The timestamps from individual trial records also help me in constructing court-level annual performance measures. I define and

[14]Data generated thus are more reliable and less likely to have been doctored between the time of an event (i.e. a case hearing) and digitization since such applications minimizes the time lag between the two. This is critical in a context with substantial quality issues with bureaucrat-reported administrative data (Singh 2020; Muralidharan et al. 2021).Given the granularity of legal case-level data and the requirement for electronically updating case files in real time, this approach likely generates a reliable administrative data on judge staffing.

construct the key performance variable - rate of backlog resolution (henceforth referred to as disposal rate), as the percentage of total workload including pending legal cases that are resolved in a calendar year. The numerator in this ratio is the number of cases resolved in a year whereas the denominator is the sum of cases that are newly filed and those filed in the past years but have not yet been resolved. This measure is strongly correlated with other possible measures of court performance such as case duration or appeal rates (see Table A.1 for pairwise correlations between the different measures).[15]

3B Firm-level Outcomes

Population of Interest: I focus mainly on formal sector firms, with registered office location within the jurisdiction of the sample district courts. I do this two reasons: First, this specific sector accounts for ≈ 40% of sales, 60% of VAT, and 87% of exports (Economic Survey, 2018), and therefore captures a large share of value addition in the economy. Second, these firms report annual production outcomes, which is useful given the time-scale of my identifying variation.[16]

Firms' dataset: I use CMIE-Prowess dataset that includes balance sheets of the universe of listed firms and a sample of unlisted but registered formal sector firms to measure annual firm-level outcomes. Firm-specific outcomes include production (sales revenue, wage bills, value of capital goods, and raw material expenditure), accounting (profit and loss), and borrowing (working capital and interest expenditure) variables. Detailed identifying information in the dataset, including firm name and registered office location, enables me to match them with the court-level dataset.

District-level data on firms: Of the 49202 firms on the CMIE website in 2018, 9032 non-financial sector firms have registered offices in 157 of the 195 sample court districts. Remaining 38 district courts result in no match. I use this data to measure the total number of formal sector firms in the study district as well as the number of new incorporations during the study period. This enables me to examine impacts over this extensive margin as well as to analyze compositional changes in the set of firms over the study time period.

Firm sample construction for balance sheet analysis: In order to measure the effects on annual production outcomes, I pay specific attention to incumbent non-financial firms, incorporated before 2010. Since many firms in the Prowess database have missing balance sheet data for multiple years in the study period, I create a balanced panel of incumbent firms with no missing data. There are two important advantages of using a balanced panel: (a) to ensure internal validity if

[15] Court workload includes both pending as well as new trials, which is around 20000 cases per district court. Resolved trials also include those that are dismissed without 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 trial 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.

[16] A similar periodicity for the informal sector is not available and therefore, I rely on other proximate measures for the extent of overall economic activities within the district that I describe in the next subsection.

missing-ness is non-random, and (b) to help account for firms' time invariant characteristics using firm fixed effects. A total of 393 firms, across multiple 4-digit industrial classification remain in the balanced panel overlapping with 64 districts in the court data. I carry out supplementary analysis and robustness tests using the unbalanced panel of firms as well. Appendix Figure A.4 describes the firm sample construction process in detail.

3C District-Level Outcomes

Banking data: I examine total lending (number of loans) to industrial borrowers at the district-level, aggregated across the local branches of all commercial banks as reported by the central bank, Reserve Bank of India (RBI). This is the lowest-level of disaggregation available publicly for research use.

Reported Crime data: To explore effects on local crime, I use public data on district-level reported crime statistics by National Crime Records Bureau (NCRB). I leverage crime reporting classified into serious crimes such as murders, homicides - those causing injuries to human life - and remaining categories classified as other crimes, which mainly include lesser crimes including small-valued thefts that are typically tried "summarily" by courts (as described in the context section above). Specifically, these crimes also require a court order for the police to investigate before filing a case in the court.

Nightlights data: Finally, to examine more broad-based impact, I use Visible and Infrared Imaging Suite (VIIRS) nighttime light measure Annual VNL V2.1 by the Earth Observation Group and compute the pixel average within the district boundary. This is the updated nightlights data, replacing the product from the Defense Meteorological Satellite Program (DMSP) that ended in 2013.

3D Summary Statistics

Panel A of Table 1 presents summary statistics for the court variables. On average, there are 18 judge posts per district court, with 23 percent vacancy. Over the study period 2010-2018, net judge additions occur 1.62 times with 2 judges added on average and net removals occur about 3.6 times with 3 judges removed on average across the district courts in my sample. This implies that courts on average experience 1-2 positive events and 3-4 negative events over the study period. The corresponding rows describing these events in Table 1 (rows 3-6) suggests that 158 courts experience a positive event whereas 37 courts experience no net addition over the entire duration. On the other hand, every court experiences a negative event during the study period.

Average court-level backlog disposal rate is 14 percent, i.e., 14% of total workload is resolved in a given year. The timestamps on individual cases resolved within the study period indicate an average case duration of 420 days (SD 570 days). A key difference between disposal rate and the average case duration is that the former includes the universe of all legal cases within the study period whereas the latter only includes duration for cases that were resolved. Therefore, disposal rate avoids selection concerns in its construction process and is the main first stage outcome of interest.

Panels B and C describe district and local firm-level outcomes. On average, banks make 9188 loans per year with about USD 4.2 million (INR 310 million) in circulation (outstanding amount) to the industrial sector within the sample districts. The summary on annual firm-level financials indicate that these are 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 adjusted for inflation using Consumer Price Index (base year = 2015).

4 Research Design and Empirical Strategy

As detailed in Section 2, judge staffing levels in a court change frequently due to addition and removal of judges resulting from recruitments, periodic rotations/reassignments and retirements. Central to my identification strategy is that the *timing* of these net staffing changes in district courts that affect judge staffing levels on the margin is plausibly exogenous. A court can experience staffing changes multiple times during the study period, including both net increases as well as net decreases. Therefore, the empirical strategy must take this multiplicity into account. I use positive changes to draw inferences on the causal effect of judicial staffing improvements and negative changes for the effect of staffing declines.

4A Stacked Difference in Differences Event Study

With a one time, albeit staggered, change in district court's 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. The dataset for an event e within a district d is centered around one period prior to the event with relative yearly event-time bins, including binned end points (clubbing all the years in the dataset outside this effect window). I append all such district-by-event datasets to generate a stacked dataset for analysis, with each event indexed by an event number (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).[17]

Finally, I create binary variables - Pos_{de} and Neg_{de} - to distinguish an event as net positive staffing change (vacancy removal) or a net negative change (vacancy creation), and interact these

[17] Event number runs from 1 through 8 for positive events and 10 through 17 for negative events. I generate single event datasets for district courts without any changes. Event ids 0 and 9 are for no positive and no negative change, respectively, in a district court.

with the event time bins in the following dynamic difference in differences, stacked event-study specification:

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

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 treated groups are courts with a net positive or a net negative change occurring in a specific calendar year (for e.g., 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 (i.e., an implementation of staggered net addition or removal). Since there are multiple events, the control group also includes the same district experiencing another positive and/or negative change in the future. As discussed earlier, 37 districts never experience positive staffing 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 term during the pre-period enable testing for any significant pre-trends. As I use a modified stacked-event study specification to account for the multiple and opposing nature of the key policy variation, I simulate the estimation procedure using this modified estimator to confirm that I am able to recover the treatment effects without bias. I report the results of the simulation exercise in Figure A.5.

I restrict the effect window to 4 years prior and post with binned endpoints. The choice of the window incorporates the maximal tenure length of a judge in a court. The coefficients within this window are also estimable without loss of precision given the limitations of my data. Binning of the endpoints accounts for any plausible effects outside the effect window selected, thus capturing any long-run effects of staffing changes. For inference, I use two-way cluster robust standard errors for estimated event-time coefficients, clustering by both district and event (Bertrand et al. 2004, Abadie et al. 2017).[18]

Causal identification requires the following assumptions: (a) exogeneity of timing, and (b) parallel trends, after accounting for heterogeneous as well as dynamic treatment effects in the stacked event study design. As discussed in Section 2, the interplay between three different personnel policies (recruitment, retirements, and reassignments) concerning judges in district courts could have different consequences on the judge staffing levels at any point in time, generating plausible exogene-

[18] For robustness, I also cluster by state and event in order to account for any spatial correlation between districts arising from state-level policies.

ity in the timings of these staffing changes. 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 into add more judges that their turnover from retirement or reassignment away, then the court would experience a positive staffing change. Finally, it is also possible that these movements cancel each other, resulting in no net change to the court staffing levels.

As an empirical support to this claim, I check for the common trends assumption by examining any differential trends in the prior period. Additionally, I carry out multiple empirical tests including: (a) tests to predict the timing and magnitude of the staffing change in the spirit of balance tests using time-varying district-level social, economic, and political outcomes, (b) testing for a lack of effects outside the jurisdiction of my sample courts in the spirit of placebo tests, and (c) dropping large, metropolitan districts and industrial states to check if results are being driven by outliers as well as address potential endogeneity concerns if larger, metropolitan districts are more likely to experience differential staff changing events. I describe the results from (a) in detail later in this section, and discuss (b) and (c) in conjunction with empirical results in Section 5.

4B Complementary Empirical Strategy

There are two important concerns while using a stacked event study design in my context: (a) absence of a never-treated group for negative events, and (b) potential interference between events. To address these, I modify the stacked event study empirical specification to separately include both positive and negative events in a non-parametric form, which would account for plausible interference between events. Second, I bin the end points and normalize the event study coefficients relative to the year period to the event(s) as suggested by Schmidheiny and Siegloch (2020) to address the lack of a never-treated group for negative events as well as relaxing any assumption of no treatment effects outside the effect window.

As a further support to my main empirical strategy, I also execute a more generalized event study strategy 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. This method thus addresses any concerns stemming from the construction or definition of events although trading-off more restrictive assumptions for causal identification.

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

where Δ is the first difference operator and the effect window spans 4 years in the lead and 4 years in the lag. 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}. I chose the maximum possible effect window as estimable using the data and consistent with Equation 1. $x_{i,t-4}$ and $1 - x_{i,t+3}$ serve as the endpoints. For inference, I cluster standard errors by district.

The identifying assumption relies on parallel trends between districts with one more judge in a given year relative to others and homogenous treatment effects. Though using this approach will not produce the same causal effect parameter as the stacked event study approach in Equation 1, I use this approach to verify the results qualitatively.

4C Balance Tests for Staffing Changes

A key advantage of dynamic difference in difference strategy is visual representation of the differential trends in the prior period. However, this empirical test is only a necessary but not a sufficient condition for establishing the validity of the research design and the actual parallel trends assumption can never be tested. While the fixed effects in the main specification Equation 1 - accounting for the smallest geographic and/or economic unit - absorb all time-invariant unobservable potential confounders of the timing of the staffing changes, and state-year dummies account for state-specific flexible time trends, there could be other time-varying confounders of staffing changes. However, a key challenge is availability of data, disaggregated even at the district-level with annual periodicity.

Given these challenges, I leverage multiple rounds of population census, economic census, and electoral data in the decade prior to my study period (these variables are only available at a decadal or quinquennial intervals) to test whether any of these could potentially determine which districts are likely to experience judicial staffing changes. I exploit 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 from this linear prediction exercise. To aid easier interpretation of the coefficients, all dependent and independent variables are transformed into % changes relative to their baseline values (i.e. the earliest period of data availability). None of the individual coefficients are statistically significant nor do they jointly do well in predicting which districts are likely to experience larger staffing changes.

5 Reduced Form Effects of Judicial Staffing Changes

5A Judge Headcount and Vacancy Rate

Panels A and B Figure 1 present the regression coefficients on the interacted terms from Equation 1 using both positive and negative changes dummies with judge headcount (Panel A) and inverse vacancy rates (Panel B) - (100-vacancy in %) - as dependent variables. Three features of these graphs are noteworthy: (a) an immediate increase/decrease in headcount and inverse vacancy rates following the changes, (b) persistence over a 4-year horizon, and (c) lack of any statistically or economically significant point estimates 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 levels and 10 percentage point increase in vacancy. The coefficients indicate economically meaningful persistence, albeit with a gradual decay given the frequency of turnovers, where the staffing levels are higher (or lower) by around 10 (5) percent 3-4 years following the staffing changes. The asymmetry between positive and negative changes is consistent with a context where recruitment drives are sporadic and lumpy. On the other hand, vacancy is typically generated by the retirement of the senior-most judge within a court, and therefore, could explain the lack of lumpiness following negative staffing changes.

Table A.2 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 can be seen across different subsamples of district courts (see Table A.3 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 mechanically from reassignment of judges from one district to another (Figure A.6).

5B Court Performance

Panel C Figure 1 plots the regression coefficients on the event-time bins interacted with positive or negative change dummies as per Equation 1 using annual court-level case disposal rate as the dependent variable. This outcome increases by ≈ 2 percentage points over a baseline disposal rate of 12.62% of existing workload following positive staffing changes ($p = 0.004$ immediately, $p = 0.047$ 3 years from the staffing change, and $p = 0.019$ in the long run). Each additional judge resolves 200 additional trials in a context where the average annual judge-level workload is ≈ 2000 cases.[19] A clear break in trend following positive changes suggests a causal relationship between increase in staffing and the capacity of district courts in reducing litigation backlogs.

On the other hand, disposal rate does not respond significantly following a negative change with the estimated decline ≈ 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). Columns 3 and 6 of Table A.2 present the event study estimates on disposal rate in a tabular format for net increase and net decrease in judge staffing, respectively. Importantly, the point estimates in the periods prior to the staffing changes are both statistically and economically insignificant, supporting the parallel trends assumption. The estimates are also robust to clustering by state and event to account for spatial correlation between districts (Figure A.6).

The lack of a significant negative result following negative staffing changes is likely driven by

[19] I also confirm these numbers by estimating the specification using number of resolved trials as the dependent variable in Table A.4. I focus on disposal rate as the key measure as it measures backlog resolution in terms of percentage reduction in the number of existing workload of legal cases.

the fact that fewer number of judges turnover relative to those added and that existing workload at the time of vacancy is transferred to other judges in the court. Despite this muted effect on disposal rate, increased vacancy could plausibly affect the quality of legal services. One example of such a measure is the handling of certain types of cases, such as "frivolous" appeals against judgements from lower courts (for example, if such appeals do not stand any merit, such cases should be dismissed immediately rather than after 3 years). In the absence of adequate number of judges, it is likely that easy to resolve disputes continue as unresolved workload in the court. I note an increase in the share of appeal cases from lower courts (Column 6 Table A.4), which could indicate how judge vacancy affects courts' functioning even though there is no significant reduction in the court-level disposal rate.

Finally, I note treatment effect heterogeneity by underlying district size (which also corresponds to the size of the court). Mid-sized and smaller courts experience larger improvements in disposal rate following net judge additions whereas the negative effects of net removal are mainly driven by large courts (see Table A.5).

5C Robustness: First Stage

A concern with the estimated effects on court performance is if there are any mechanical correlations between coding of the staffing change events with the disposal rate or other measures of court performance. Note that the staffing change is only constructed using filing of new litigation, and thus should have little mechanical correlation with case resolutions or backlog from past years. In order to address concerns arising from the construction of the events, I estimate the effects of judicial staffing changes on court performance using Equation 2, which includes leads and lags of continuous valued changes in the number of judges. Figure A.7 presents the results from this specification in a graphical format. Positive integer labels on the x-axis report regression coefficients on the lagged explanatory variables whereas the negative integer labels correspond to the lead variables. Important to note is that existing workload and performance of courts is neither significantly or economically meaningfully correlated with the current or future judge staffing changes. Additionally, current and past changes in staffing impact disposal rates through increased resolution in the current as well as future years while the demand for litigation (number of new litigations) does not change significantly.

I also note significant effects on disposal rate using local projection DID estimation based on a sequence of first difference regression specifications following Dube et al. (2022) reported in Figure A.8. This strategy is particularly useful in my setting, which is similar to those typical in macro-finance where shocks occur as impulses over a short time period.

5D Local Firms' Production

To examine the downstream economic implications of local judicial staffing and capacity, I start with the reduced form effects on incumbent, formal sector firms located in the same geography as the jurisdiction of the district court. Specifically, I estimate the effects on profits, sales revenue, wage bills, and value of plant and machinery.

Figure 2 and Figure 3 depict the event study graphs 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 mechanical from specific legal cases being resolved in these courts.

Table A.6 and Table A.7 present the results in a tabular format corresponding to each of the figures, respectively. Wage bill and sales revenue increase by around 5% ($p = 0.93$ immediately, $p = 0.095$ 3 years from the staffing change, and $p = 0.037$ in the long run) and 2% ($p = 0.001$ immediately, $p = 0.016$ 3 years from the staffing change, and $p < 0.001$ in the long run), respectively, over the long run following net judicial staffing increases. The effect on profit is 40% over the period ($p = 0.26$ immediately, $p = 0.002$ 3 years from the staffing change, and $p = 0.001$ in the long run). Lastly, the effects on capital goods, including the value of plant and machinery, are not statistically significant even though the point estimates are large and in the same direction as other measures of productivity.

Since the sample firms are large in terms of revenue, profitability, and employment 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 the increase in profits are also likely to be driven by a reduction in other expenditures such as interest payments and other accounting expenses.

The effects of negative staffing changes generating vacancies are negative, commensurately with the treatment intensity of the negative changes. In the long run, wage bill and sales revenue contract by about 2% each ($p = 0.82$ immediately, $p = 0.085$ 3 years from the staffing change, and $p = 0.003$ in the long run for wages and $p = 0.46$ immediately, $p = 0.06$ 3 years from the staffing change, and $p = 0.006$ in the long run for sales), respectively. Profits contract by 20% ($p = 0.36$ immediately, $p = 0.05$ 3 years from the staffing change, and $p = 0.003$ in the long run). The value of plants and machinery also decreases but the point estimates are imprecise. Normalizing effects per judge to compare with the estimates from addition events suggest that the changes in productivity outcomes are symmetric.

5E Robustness: Firm-level Outcomes

One important concern is whether the above results reflect biased estimates due to firm sample construction to create a balanced panel. That is, the estimates could be biased if the outcomes of the analysis sample are correlated with the changing composition of excluded firms in the district (due to missing data) in a way that reduces competition environment for the firms in the sample.

So, even if the composition of firms in the sample remain fixed, which helps with internal validity, the sample construction could be introducing bias due to 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 the welfare effects in the presence of such a bias.

I address this concern in three different ways: First, I examine the effect of staffing changes on new firm incorporations (firm entry) and total number of firms in the district. This itself could indicate a more broad-based impact of judicial capacity, answering (b) above, and the direction of effects would help shed light on (a) and (c). I find increased firm entry and fewer net exits (as the total number of firms in a district also marginally increase) around positive staffing events. Since this could imply an increase in competitive forces in the local production economy, the results on the balanced sample of firms are likely to be downward biased, i.e. presenting a lower bound. Second, I estimate the effects using the full sample of unbalanced firms, which are qualitatively similar (see Table A.8 and Table A.9). Third, I check if missingness of data is correlated with staffing changes and if so, how that would affect the interpretation of the results. I find a decrease in the extent the missing data consequent to improved judicial staffing and greater missing entries following net decreases (but importantly, with no pre-trends; see Table A.10 and Table A.11). This suggests that firms are more likely to report data (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 supports plausible downward bias in the estimated effects of improved judicial capacity. This also implies that using unbalanced panel of firms is not a feasible strategy to estimate the causal effects, since missing data is not random.

Second, the effects could be plausibly be due to the fact that some of the sample firms may directly gain from resolution of their legal cases in the courts under more judges. I find that the effects persist even among firms with no legal case data in the sample courts in the entire study period and thus are suggestive of broader, local equilibrium effects (see Table A.12 and Table A.13).

Third, one would be concerned about more general spurious correlation, such as those arising from time varying macro-economic unobservables not captured in state-year fixed effects. Given the local nature of dispute resolution and market transactions, I check whether the effects of improved district judicial capacity are restricted to firms within the district and not experienced among incumbent firms in the bordering districts as a placebo test. Table A.14 and Table A.15 document the results, showing that the point estimates are statistically and economically insignificant, addressing concerns of potential spurious correlation.

Lastly, I estimate the effects using the generalized event study specification (Equation 2) and local projection DID (Dube et al. 2022) approaches, both of which show similar patterns of effects on firm productivity (Figure A.9 and Figure A.10).[20]

[20]The results are robust to a battery of standard sensitivity tests, particularly: (a) dropping top industrial states, and (b) dropping metropolitan districts. If anything, the point estimates become larger and I gain more precision with sales revenue and raw material expenditure (see Table A.16, Table A.17, Table A.18, and Table A.19, respectively).

5F Plausible Broad-Based Impact

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

As discussed above, I note a significant increase in new firm incorporations and total number of formal sector firms with little evidence of increased exits (Cols 1-2 Table 3) following net judge increase. On the other hand, a net decrease in the number of judges has minimal effect on these extensive margin changes. In status quo where about 2 firms incorporate in a district in a year, better functioning judiciary with more judges increase the extent of incorporations by about 30% ($p = 0.007$ in the long run).

In the absence of accurate district-level annual GDP data for the period of my analysis, I rely on the recent nighttime light data source from VIIRS satellite imagery. Using this as a proxy for overall district GDP growth, I find suggestive evidence of increase in nightlights intensity following positive staffing (intensity increases by about 6%) and a decrease (by about 3%) following negative staffing changes (Cols 3 and 6 Table 3). The nightlight analysis, albeit noisy ($p = 0.315$ in the long run), 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

An examination of the legal case data presents the following facts about the bulk of cases in frontline courts: (a) banks are litigation intensive - there are many more cases per bank relative to per capita caseload of any other litigant, (b) about 50% of all commercial banks in India have at least one ongoing legal case during the study period in the study districts, and (c) in 80% of cases involving banks, banks are the initiator of the complaint (appear as a petitioner). Further, the value of assets under litigation involving debt recovery disputes are many orders of magnitude larger than other dispute types. Typically, such disputes are settled in favor of the lenders, where judges facilitate a settlement to enable partial or complete recovery (see Figure A.11 for descriptive statistics).[21] These in conjunction with the fact that banks are required to file debt non-payment cases in the corresponding courts to culminate the recovery process (for e.g., before liquidating the assets of the borrower), suggests that well-functioning judiciary is important for banks' business and lending workflow.

Inference is robust to clustering standard errors by state and event, in order to account for any spatial correlation between district courts arising out of judge rotation and state-level recruitment and retirement policies that drive the variation. The effect on wage bills and profits are still significant at 5% in the year(s) following the events (see Table A.20 and Table A.21).

[21] 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.

6A *Local Credit Supply*

I begin by examining district-level credit data to examine the effect of judicial staffing changes on total bank lending to all industrial borrowers in a district. 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 trials involving banks at the start of the study period (i.e., in the period prior to any staffing change recorded within the study period). Panel A Figure 4 presents the event study graph using total number of loans to industrial borrowers across all banks in a district as the outcome variable. The figure also shows lending by private sector banks, which face market incentives in contrast to public sector banks. The key findings are: (a) total lending to industrial borrowers increases between 6-8% over the long run, following an increase in judicial staffing ($p = 0.07$ in year 3 and $p = 0.11$ year 4 and beyond), with private sector banks playing an important role (private lending increases by over 12% in the long run, $p = 0.016$), and (b) effect following net decrease in the number of judges is relatively muted and noisy.

The positive effects following net judge addition are consistent with the time horizon for short and medium-term loans such as those towards operating expenses. An increase in the resolutions of debt recovery cases potentially enables banks to recover capital stuck in litigation, which could increase liquidity in the corresponding bank branch by lowering provisions they need to make in their profit and loss statements for write-offs. The average capital stuck in bank debt recovery cases is large, upwards of $ 15000. As each additional judge resolves 200 legal cases, resolving even 10 debt recovery cases could unfreeze capital worth $150,000. At the district-level, with 2 judges added on average, this translates to $ 0.3 million in recovered capital at the district-level. This increased liquidity is likely recirculated as additional credit to industrial borrowers. A 6% growth in bank lending to the industrial sector, which average at INR 310 million or $ 3.73 million (Table 1), this growth translates to $ 0.2 million additional lending to industrial borrowers.

The lack of negative effects following net reduction in judicial staffing is consistent with the fact there is no significant decline in case resolutions. This however relies on the assumption that borrower default behavior does not respond to changes in the number of judges, but the increase in credit circulation is plausibly driven by an increase in recovered capital from case backlog resolution.[22] However, it is also likely that banks and lenders update their expectation on debt recovery and lend more as a result. Due to data limitation, I am unable to distinguish between these two specific channels and this remains an open question for future research.

To examine the role of credit market and overall improvement in court backlog resolution for firm-level productivity, I develop an economic framework that I discuss below, which provides additional hypotheses to test with data.

[22]As noted in Figure A.7, there is no significant and meaningful correlation between changes in judicial staffing and new case filing rates in district courts.

6B 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 local 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 credit to finance operations, which has some stochastic probability of success. A lender (e.g., bank) bases their lending decisions on whether repayment can be enforced through courts. The lender takes into account borrower firm's wealth towards collateral requirement and/or past borrowing behavior in order to lend. 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, which is costly. Evasion leads to default, which initiates debt recovery process and subsequently, litigation. This recovery process incurs a cost to both lender and borrower, as a decreasing function in court's effectiveness. That is, lower backlog in courts implies lower litigation related costs, ceteris paribus. Availability of judges has a direct implication on the extent of backlog resolution as discussed in Section 5B.

Some borrowers may choose to litigate if their payoff is better under litigation. This is plausible, 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 and avoid continuing the litigation process. Sub-game perfect Nash equilibrium (SPNE) through backward induction implies that the lender uses a wealth cut-off (or any other proxy for repayment capability) in their decision to lend. Improvement in contract enforcement environment results in lower interest rates for all borrowers and leads to increased lending to smaller borrowers. The framework is discussed in detail in Appendix A.3.

An important implication of this framework is that there are extensive margin changes determining who a bank lends to and the overall price of credit (interest rate) on loans, following variations in the local judicial capacity. These changes can be driven both by: (a) an improvement in contract enforcement environment, and (b) short-run liquidity effects through increased recovery of defaulted loans.

Subsequently, firms would re-optimize their production decisions following credit market-level changes. In addition to the credit channel, improved courts could also directly benefit firms' production processes through lower transaction costs, for example, from lower monitoring and security costs in protecting assets, inventory, and raw material stock from thefts and embezzlement. This implies that, on average, firms expand production and incur lower production and non-production expenses that would impact their production outcomes and balance sheet.

Empirically, I note an increase in firms' working capital and a decrease in interest expenditure immediately following staffing changes (Panel B Figure 4). Working capital reflects the extent of cash available to meet operating expenses.[23] These immediate effects on working capital and

[23] 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.

interest expenditure is consistent with the plausible role of liquidity in local credit markets. Working capital increases immediately by 39% ($p < 0.001$) that persists in the long run ($p = 0.021$). Interest payments decline by 8% immediately and also persist in the long run ($p < 0.001$). The immediate effects on firm-level working capital and interest expenditure, and the corresponding effects on district-level private sector bank lending to industrial borrowers suggest that liquidity likely plays an important role. Lastly, I note an expansion in raw material expenditures (increase by 3% in the long run, $p = 0.009$), which could reflect an expansion in operational expenditure (see Panel C Figure 4).

6C Additional Empirical Tests: Heterogeneity by Firm-Size

The conceptual framework generates additional hypotheses that can be tested in the data, namely: (a) borrowers' (including firms) propensity to litigate as defendants is a function of their asset size, and (b) increase in lending to smaller borrowers (or those with limited past relationship with banks) following an increase in judicial enforcement capacity. I use firm-level data on total asset value as well as the extent of debt-exposure (to measure leverage) at the start of the study period to classify firms into size bins (above and below median) to carry out subsample analysis to examine these additional hypotheses.

Figure A.13 presents descriptive statistics of legal cases involving firms as defending litigants, which highlights the following key facts: (a) defendant firms have higher asset value compared to similarly leveraged non-litigant firms, and (b) asset value of high-leveraged (frequent borrower) defending firms is higher than other (limited borrowing) defending firms. Not only are the average values different between the groups, but the entire distribution of asset value among the defendant firms is shifted to the right.

To examine how judicial staffing variations affect these outcomes by firm-size heterogeneity, I follow the event study analysis in Equation 1 among subsamples of firms by their ex-ante asset size. Figure 5 shows that smaller firms are more likely to appear as defendants in legal cases when there are more judges. Additionally, I also note that smaller firms with low ex-ante leverage experience greater working capital infusion, face lower interest expenditure, and record higher profits.

6D Additional Mechanisms and Decomposition of Firms' Profit

Another important function that courts provide is law enforcement and improving safety of property and individuals in the local area. One way to think of improved safety is in terms of its implications on monitoring and security costs in firms' production function. With better local enforcement capacity, such expenses are plausibly lower. I elaborate this in the conceptual framework (see Appendix A.3).

Empirically, I examine the effects of judicial staffing changes on two types of reported crime - serious or violent crimes that include homicide, riots, crime causing significant bodily injuries, and less serious crimes (recorded as "other IPC crime"), which include small-scale theft and property crimes. Table A.23 shows reductions in both types of crimes following a net increase in the number of judges. A net decrease, on the other hand, generates a corresponding increase in less serious crimes but no significant effect on serious crime rates. With the caveat that I unable to distinguish whether these changes are due to reporting or true occurrence of crimes, these results suggest that local courts plausibly play an important role in protecting physical and financial property of local firms.

Finally, I decompose firms' profits into that arising from production (sales), credit access (working capital), and local crime channels (monitoring costs) using a distributed lags model by incorporating lagged values of firm profits and firm fixed effects. I also add district-year and industry-year dummies to account for time varying unobserved drivers of firm profits. Table A.24 presents a suggestive but important insight that interest expenditure has a large negative elasticity with respect to profit. Lowering of such expenditure may affect profit substantially through both productivity and accounting channels.

7 Benefits and Costs of Reducing Judge Vacancy

The analysis in this paper suggests that investing in frontline judicial staffing is important for improving local firm productivity and subsequently, overall economic development. Leveraging the fact that the firms in my sample are tax-paying firms, employing labor force 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 4, I present data, computations, and assumptions to generate a back-of-the-envelope benefit-cost ratio from investing in filling judge vacancies in district courts.

On the benefits side, I use the median values of profits and wage bills among the sample firms per district to compute the increase in firm-level surplus and salaried income. Since both formal sector firms and their salaried workers pay corporate and income tax on their net income respectively, I apply the average tax rates on net increases in firm profit and wage bill. Corporate tax rates for registered domestic firms are specified in the Taxation Laws Amendment Ordinance (2019). I calculate the effective income tax rate on salaried workers as 7.3 percent, as a lower bound, after applying all possible exemptions and tax-slabs specified in the Union Budget, 2018-19. [24]

On the expenditure side, I calculate the increase in total district-level judge salaries from net increase in the number of judges using the median proposed salary of a district judge in the Second National Judicial Pay Commission. I further inflate the salary to account for fringe costs incurred by the state to cover judges' benefits and allowances, including transport, housing, etc., and account for annual increments. 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 increase in profits, wage bills, the

[24] These assumptions are motivated by articles in the news media, with sources mentioned in Table 4. I calculate the average individual income tax using media reports on average filed annual income of a salaried tax-payer in India for the year 2018-19, which is INR 690,000 or roughly USD 10,000. Applying exemptions, an individual with this income incurs an effective tax rate of 7.3 percent.

associated tax revenue, the corresponding expenditure on additional judges for 5 years following the increase in the number of judges. I assume the discount rate to be 5% in the base calculation and perform sensitivity analyses using lower and higher discount rates. I also compute the confidence intervals by bootstrapping the NPV calculating using the regression coefficients and their standard errors of the estimated parameters, namely, the marginal increase in the number of judges, profit, and wage-bill growth following positive staffing changes. 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 (with highest discount rate and the left-end of the confidence interval) suggest that the returns to investing in district judicial staffing is high and more than pays for itself.

8 Conclusion

To conclude, I show that well-functioning frontline judiciary is a core component of state capacity and crucial for local economic development. The current status-quo underscores the problem of large backlogs of legal disputes in such courts in a context where, on average, about a quarter of the judge posts are vacant. Thus this paper demonstrates that reducing vacancy by adding more judges is a highly cost-effective intervention by spurring local formal sector expansion: Adding one more judge increases court-level disposal rate of case backlog by 10 percent. In a context with large amounts of capital stuck in litigation from defaults - $\approx$ \$ 170 billion in value in 2018 - even a marginal increase in judicial capacity frees up a meaningful magnitude of this frozen capital. Subsequently, local firms become more productive, with indications for broad-based local economic development in the areas served by these courts. Importantly, the large benefits accrue relatively quickly and potentially within an electoral cycle, making it an attractive investment proposition for the state executive to improve judicial capacity.

This paper provides causal evidence on the relationship between judicial staffing in courts and local economic development by leveraging variations in staffing levels over time. I argue that the timing of these variations are plausibly exogenous due to the interplay between recruitment, retirement, and rotation policies. In this study, I leverage new datasources such as legal case records and contribute a novel, court-level panel data merged with key economic outcomes. Availability of additional data such as judge biographies and high frequency data on the productivity of the household informal sector would greatly help answer follow up questions on the effect on the informal sector as well as examine the efficiency of personnel policies in the judiciary.

Central to the observed effect of improved judicial staffing on local firms is the role courts play in contract enforcement, particular debt contract enforcement. Recovery of unpaid debt stuck in litigations enables credit circulation in the local economy. Additional courts also help protect property from thefts through law enforcement, which together with debt recovery, form the bulk of cases in district courts. This facilitates firm productivity through access to credit from a better functioning local credit market, and lowering other transaction costs. These conclusions are consistent with the literature documenting the role of courts in enforcing bankruptcy laws (Ponticelli and Alencar 2016; Müller 2022), adding an important insight that this role goes beyond enforcing any specific law. Debt recovery through courts is a fundamental instance of contract enforcement, which is more routine and larger in magnitude relative to bankruptcy cases. Bankruptcy proceedings are the last step, which initiates restructuring or liquidation when a borrower is unable to fulfill their outstanding debt obligations.

Perhaps one reason for the large number of debt cases in courts could be due to incentives facing lawyers and over-optimism by litigating parties, rather than settling out of court, as documented in Sadka et al. (2018). Nonetheless, such cases are prevalent among frontline courts across the world.[25] Reducing the extent of their backlogs could have important ramifications for credit circulation, particularly in contexts where credit supply is constrained. More research is needed to examine the role of courts and other agents interacting with the judicial system across the world as countries make available rich, case-level data online. This will help generate a common, externally valid, theory of judicial capacity for financial market and economic development.

The main insight of this paper is that the smallest unit of general courts of law are important for day-to-day market transactions and firms' production decisions in the context of India, and therefore judicial capacity is state capacity. While this paper does not delve into the subsequent actions of the agents 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. For example, 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.

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[25]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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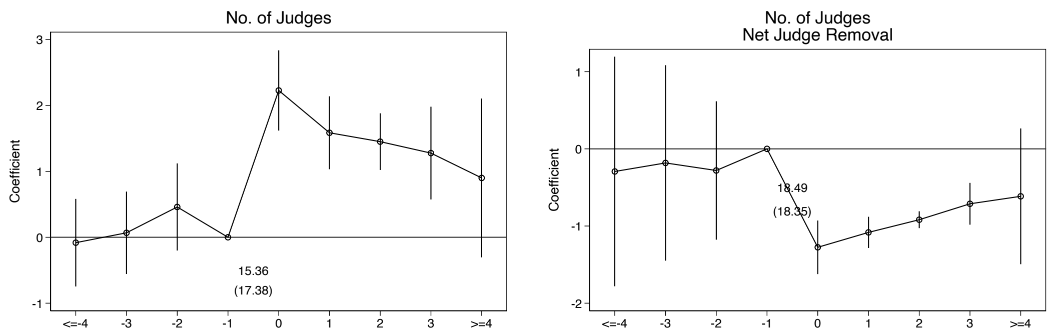
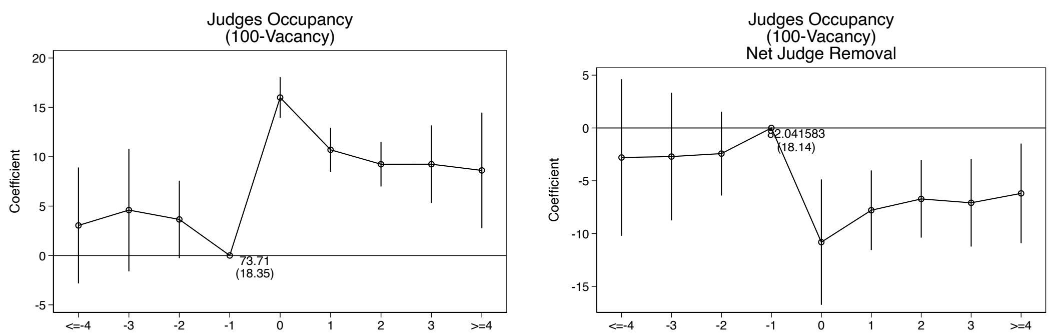
9 Figures

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

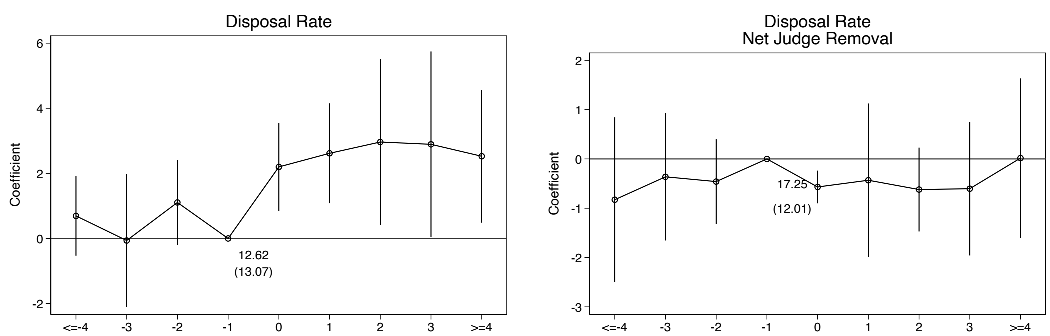
Panel A: Judge Headcount



Panel B: Inverse Vacancy Rate

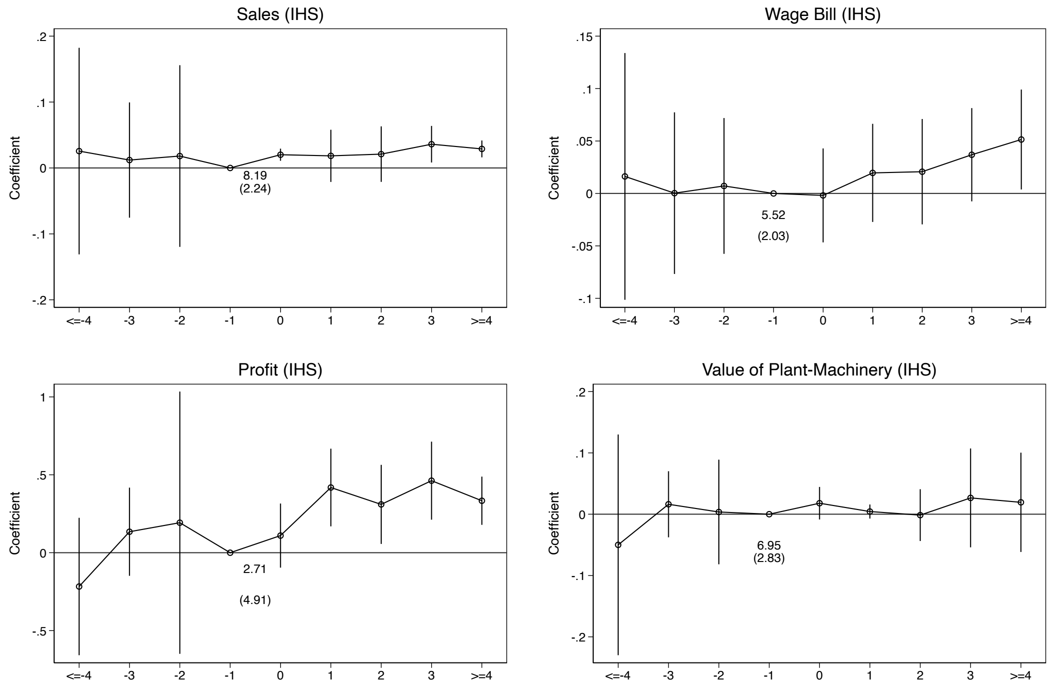


Panel C: Court-Level Disposal Rate



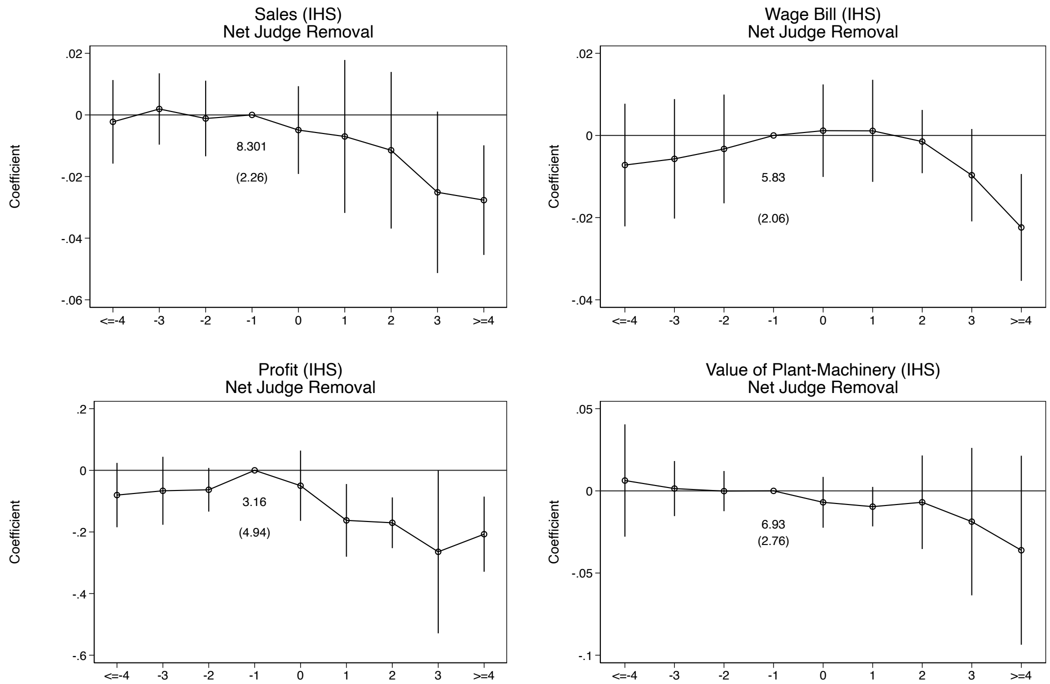
Notes: The figures plot the event study interaction coefficients for positive and negative staffing changes from estimating Equation 1 using total number of judges (Panel A), inverse vacancy rates (Panel B) and disposal rate (expressed in percentage terms in Panel C) as dependent variables, respectively. In all the figures, the end-points take into account relative event-bins outside the effect window in the data. The coefficients are all normalized to the period prior to the event. Standard errors are clustered by district and event. Error bars present 95% confidence interval. The table equivalent of these graphs is Table A.2.

Figure 2: Local Firms' Production: Net Judge Addition



Notes: The figures above plot the event studies coefficients on positive staffing change event-time interaction dummies from estimating Equation 1 for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed in the balance-sheet data. Using log transformation also yields similar results. The sample comprises of a balanced panel of incumbent firms in the district that report their annual balance sheet information over the study period, enabling the use of firm fixed effect in the specification. The first row presents the coefficients with sales revenue and wage bills as the dependent variables. The dependent variables in second row are profit and the value of capital goods (plant/machinery), respectively. In all the figures, the end-points take into account relative event-bins outside the effect window in the data. The coefficients are all normalized to the period prior to an event and standard errors are clustered by district and event. Error bars present 95% confidence interval. The table equivalent of these graphs is Table A.6.

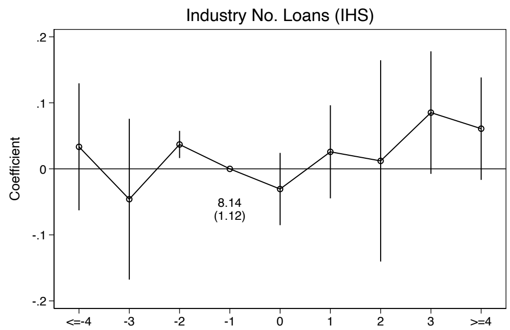
Figure 3: Local Firms' Production: Net Judge Removal

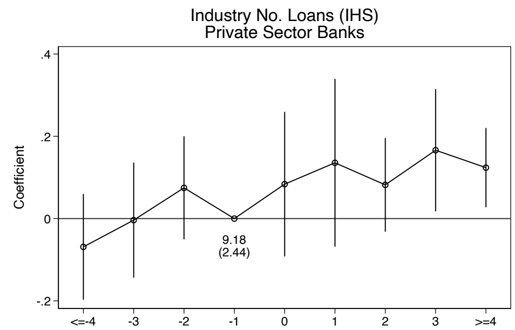


Notes: The figures above plot the event studies coefficients on negative staffing change event-time interaction dummies from estimating Equation 1 for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed in the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

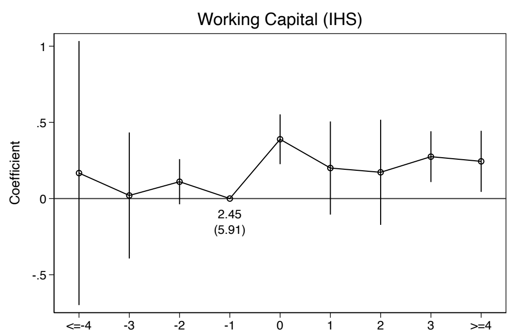
Figure 4: Credit Outcomes

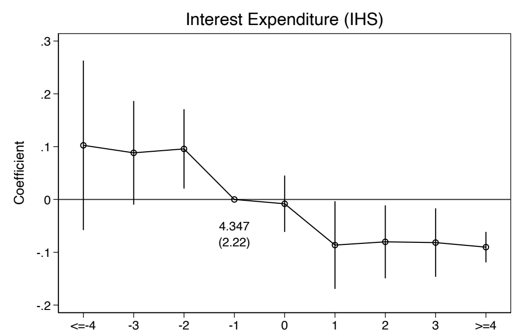
Panel A: District-Level Lending



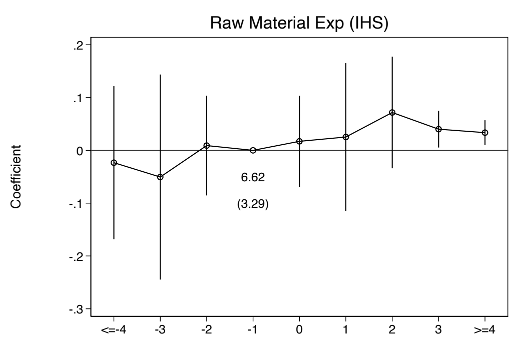


Panel B: Firm-level Working Capital and Interest Expenditure - All Sample Firms



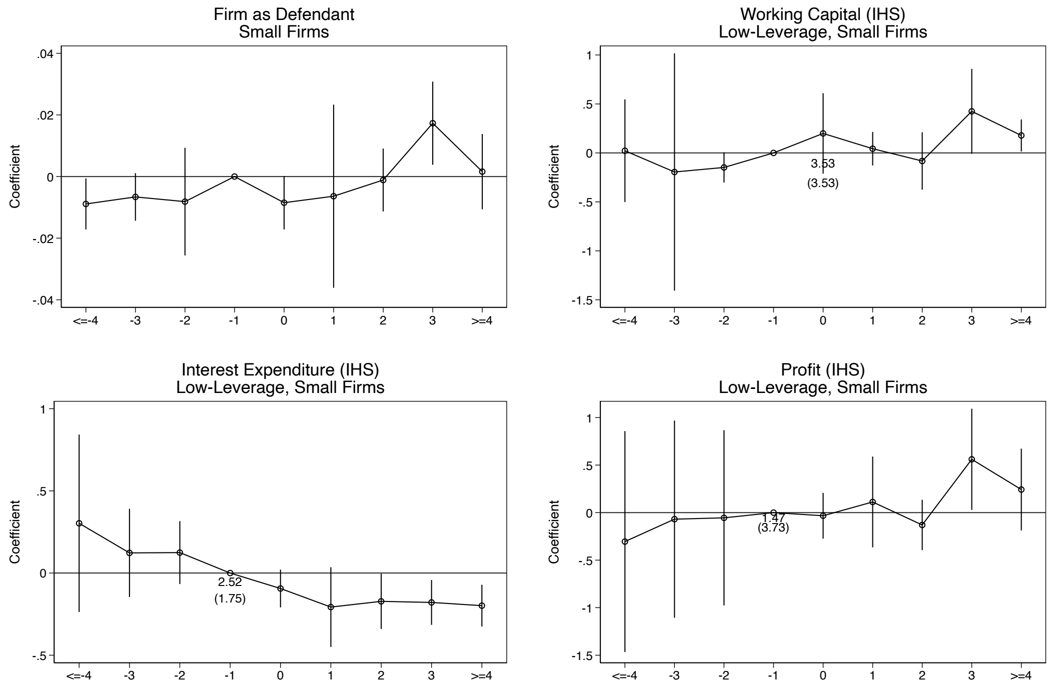


Panel C: Firm-level Raw Material Expenditure - All Sample Firms



Notes: Panel A presents effects on overall lending by all banks branches as well as branches of private sector banks, respectively, within a district to industrial borrowers. Panel B presents effects of judge vacancy removal on working capital and interest expenditure for all firms. Panel C documents the effect of judge vacancy removal on raw material expenditure. Error bars present 95% confidence interval. The table equivalent of the firm-level graphs is Table A.6 and Table A.7, respectively (Col 3, 6, 7). The table equivalent of the district-level bank lending outcome is in Table A.22 (Col 1 and 5, respectively).

Figure 5: Credit Mechanism



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

10 Tables

Table 1: Summary Statistics

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

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

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

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

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

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

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

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

Table 4: Cost-benefit Calculation

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

Notes: I focus on the event of positive staffing change to compute benefit-cost ratios. I calculate effective income tax incidence on salaried individual tax payer using average reported annual income of INR 690,000 and the applicable progressive tax slab on this reported income: income upto INR 500,000 is exempt and the remaining INR 190,000 is taxed at 20%. This gives an effective average tax incidence of 7.3%. Corporate tax rate of 22% is the rate applicable on reported corporate income for domestic companies. Bootstrapped standard error in square brackets from 1000,000 random draws. Figure A.15 shows the distribution of the benefit-cost ratio following the bootstrapping procedure.

Appendix
For Online Publication Only

A.1 Additional details on the context

District courts across India have over 18 million legal cases pending for 3 or more years as on 1st July 2023. This translates to 1059 pending cases per judge (the total sanctioned judgeships for district courts is 22677 of which only about 17000 are non-vacant positions). While the US has a slightly different structure of the judiciary, I examine the extent of backlog in both federal as well as state-level frontline courts. US federal district courts have 0.128 million cases pending over 3 years. With 677 federal district judges, this translates to 189 pending cases per judge. Among states, I consider top five most populous states: California, Texas, Florida, New York, and Pennsylvania. California has 39 million population (12% of US population) and about 0.8 million pending over 3 years, which implies 400 pending cases per judge across 2000 judges in California superior courts).[1] Statutory county courts of Texas have about 0.6 million legal cases pending in total. With about 9% US population, 765 active judges, and 4947 assigned judges (including retired judges), this translates to 121 pending case per judge. Florida and New York states have close to 100% clearance rates, with no pending cases over 3 years. Lastly, Pennsylvania with 13 million population (4% of US population) has 44046 cases pending over 3 years across 458 judges, translating to 96 pending cases per judge. This exercise reveals substantial heterogeneity within the US, but even with these differences, most states strive to keep their backlogs low with a specific attention to resolving pending backlog within 3 years. Comparing the backlog of cases per judge between district courts in India with that of relevant frontline courts in the US, the magnitude in India is about 10 times more severe. See Figure A.16 for a cross-country comparison using data by the World Bank on duration of contractual trials in frontline courts and GDP per capita. Unfortunately, no dataset exists that provides information on pending case backlog per judge across cuntries.[2]

Availability of adequate number of judges is among the key constraints in resolving case backlog in courts. There are fewer than 20 judge posts per million population in India in contrast to 70 per million recommended by the United Nations. This ratio worsens after taking into account the extent of vacancies. Staff vacancies and its sporadic redressal is a fundamental problem in bureaucratic organizations worldwide, and is particularly acute in India. For example, vacancy rates are close to 10% across superior courts in California, USA, and over 25% across district courts in India.[3]

[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.

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

A.2 Data Construction

A.2.A. Outcome variables

Intermediate outcomes: Borrowing/Lending These variables depict the intermediate outcomes linking court capacity to credit markets.

1. Bank Lending: Bank lending variables are from RBI data warehouse on Indian Economy (https://dbie.rbi.org) on district-wise number of loans and total outstanding amount (in INR Crore) aggregated annually across 27 scheduled commercial banks (national-level banks).

2. Working Capital: As all firms do not consistently report total borrowing, I use working capital as an indicator of credit use. Sufficient working capital is an indication that a firm will be able to fund its day-to-day operating expenditure.

3. Interest Expenditure: This includes firms' interest payment on all borrowing - long-term and short-term borrowing, trade credit, debentures, interest on taxes, etc.

Impact variables: Following variables represent inputs, production, and value addition mapping, onto firm's production decisions.

1. Annual revenue from sales: This variable captures income earned from the sales of goods and non-financial services, inclusive of taxes, but does not include income from financial instruments/services rendered. This reflects the main income for non-financial companies.

2. Accounting profits (income net of expenditure): I generate this variable by subtracting total expenditure reported by the firm from total reported income.

3. Wage bill: This captures total payments made by the firm to all its employees, either in cash or kind. This includes salaries/wages, social security contributions, bonuses, pension, etc.

4. Net value of plants and machinery: This incorporates reported value of plants and machinery used in production, net of depreciation and wear and tear.

5. Raw material expenditure: This captures total expenditure on raw materials by adding purchases reported in a given year to the value of net stock (opening - closing).

A.2.B. Matching firms with trial data

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

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

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

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

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

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

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

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

A.3 A model of credit market with enforcement costs

A.3.A. Credit Market

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

At the start, borrower needs to invest, K, in a project which returns $f(K)$. Their exogenous wealth endowment is W. They need an additional $K_B = K - K_M$ from the lender to start the project, where K_M is the amount they raise from the market, with market return ϕ. Borrower repays RK_B at the end of the contract period, where $R = 1 + r > 1$ incorporates the interest rate r. The project succeeds with probability s, upon which the borrower decides to repay or evade.

Evasion is costly as the borrower incurs an evasion cost ηK_B leading to a payoff $f(K) - \eta K_B$. The lender loses the entire principal, $-K_B$. Repayment results in $f(K) - RK_B$ as payoff to the borrower and the lender payoff is RK_B. On the other hand, the borrower automatically defaults if her project fails, in which case the lender can choose to litigate to monetize borrower's assets to recover their loan. This game is depicted in Figure A.12. Litigation is costly and lender incurs a cost, $C_L(\gamma) > 0, \frac{\partial C_L}{\partial \gamma} < 0$, as a function of judicial capacity, γ. The borrower can also choose to litigate with costs, $C_B(\gamma) > 0, \frac{\partial C_B}{\partial \gamma} < 0$, or settle out of court. Once the lender chooses to litigate and borrower accepts, lender wins with a very high probability. The intuition behind the relationship behind enforcement costs and judicial capacity can be explained by the fact that the litigants need to spend on travel, logistics, and lawyer fees over the duration of the trial, which is longer when the judicial capacity is lower.[5]

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

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

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

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

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

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

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

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

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

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, W^* and setting the interest rate schedule. The returns from lending to ensure adequate recovery of loan under default gives the following schedule:

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

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

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

The timing of the game where the lender and borrower decide on their strategies are depicted as an extensive form game in Figure A.12.

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

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

Constraints (2) and (5) define the credit contract. Additionally $R \geq \phi$ else the lender would rather invest in external markets than engaging in lending. This gives the relationship between returns - R, borrowing - K_B, and the wealth threshold for lending - W^*, as depicted in Figure A.12.

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

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

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

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

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

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

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

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

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

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

A.3.B. Firm Production

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

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

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

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:

1. Optimal input use X_1, X_2 increases on an average.

2. Output increases on an average.

3. Heterogeneity in effects on profits is as follows:

(a) For large firms, L, optimal inputs and profits increase if decrease in monitoring costs and cheaper credit more than offsets the increase in input expenditure.

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

(c) For inframarginal small firms, S, optimal inputs and profits remain unchanged because borrowing and monitoring costs for these firms remain unchanged.

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

Constrained Optimization:

$$\mathcal{L} = pQ(X_1, X_2) - w_1X_1 - w_2X_2 - m_i(\gamma) + \lambda\big(K_i - w_1X_1 - w_2X_2 - m_i(\gamma)\big)$$

FOC:

$$\frac{\partial \mathcal{L}}{\partial X_1} = pQ_{x_1} - w_1 - w_1\lambda = 0$$

$$\frac{\partial \mathcal{L}}{\partial X_2} = pQ_{x_2} - w_2 - w_2\lambda = 0$$

$$\frac{\partial \mathcal{L}}{\partial \lambda} = K_i - w_1X_1 - w_2X_2 - m_i(\gamma) = 0$$

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:

$$Det[J] = 2pw_1w_2 \underbrace{Q_{x_1x_2}}_{\text{+ve}} - p(w_2^2 \underbrace{Q_{x_1x_1}}_{\text{-ve}} + w_1^2 \underbrace{Q_{x_2x_2}}_{\text{-ve}}) > 0$$

$$\frac{\partial X_1}{\partial \gamma} = -\frac{Det[J_{x_1}]}{Det[J]} = -\frac{p\,\overbrace{(\frac{\partial K_i}{\partial \gamma} - \frac{\partial m_i}{\partial \gamma})}^{+ve}(w_1 \overbrace{Q_{x_2x_2}}^{\text{-ve}} - w_2 \overbrace{Q_{x_1x_2}}^{\text{+ve}})}{Det[J]} > 0$$

$$\frac{\partial X_2}{\partial \gamma} = -\frac{Det[J_{x_2}]}{Det[J]} = -\frac{p\,\overbrace{(\frac{\partial K_i}{\partial \gamma} - \frac{\partial m_i}{\partial \gamma})}^{+ve}(w_2 \overbrace{Q_{x_1x_1}}^{\text{-ve}} - w_1 \overbrace{Q_{x_2x_1}}^{\text{+ve}})}{Det[J]} > 0$$

$$\frac{\partial \lambda}{\partial \gamma} = -\frac{Det[J_{\lambda}]}{Det[J]} = -\frac{p^2\,\overbrace{(\frac{\partial K_i}{\partial \gamma} - \frac{\partial m_i}{\partial \gamma})}^{+ve}\overbrace{(Q_{x_1x_1}Q_{x_2x_2} - Q_{x_2x_1}Q_{x_1x_2})}^{\text{depends on functional form}}}{Det[J]} =?$$

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

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

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

$$\frac{\partial \Pi^*}{\partial \gamma} = \underbrace{(pQ_{x_1} - w_1)}_{\text{This is } w_1\lambda} \frac{\partial X_1^*}{\partial \gamma} + \underbrace{(pQ_{x_2} - w_2)}_{\text{This is } w_2\lambda} \frac{\partial X_2^*}{\partial \gamma} - \underbrace{\frac{\partial m_i}{\partial \gamma}}_{\text{-ve}} > 0$$

$$\frac{\partial g^*}{\partial \gamma} = \underbrace{(\frac{\partial K_i}{\partial \gamma} - \frac{\partial m_i}{\partial \gamma})}_{\text{marginal benefit}} - \underbrace{\left(w_1 \frac{\partial X_1^*}{\partial \gamma} + w_2 \frac{\partial X_2^*}{\partial \gamma}\right)}_{\text{marginal cost}}$$

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

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

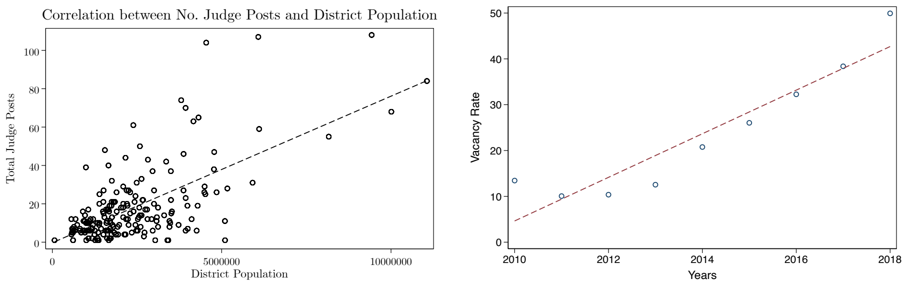
2. For marginal small firms, $i = S$ and $W \approx W^* - \epsilon$, the marginal benefit $K_B - \frac{\partial m_S}{\partial \gamma}$ is due to both availability of borrowing from banks K_B as well as a reduction in monitoring costs. I assume that the monitoring costs for small firms do not decrease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

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

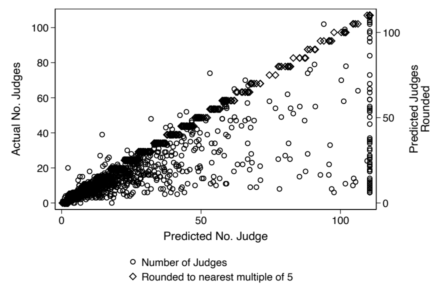
A.4 Appendix: Figures

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

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

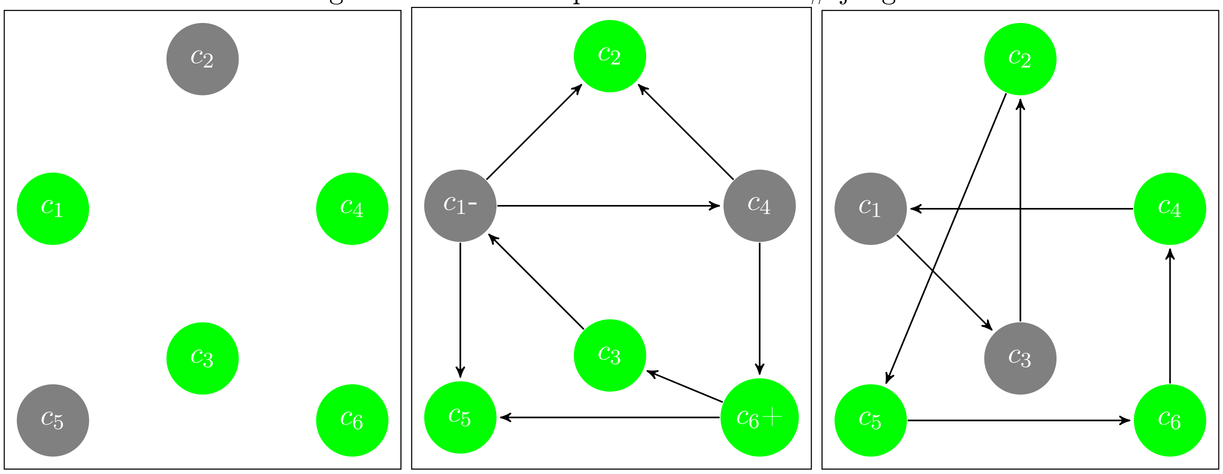


Panel B: Actual Number of Judges vs. Law Commission Recommendation



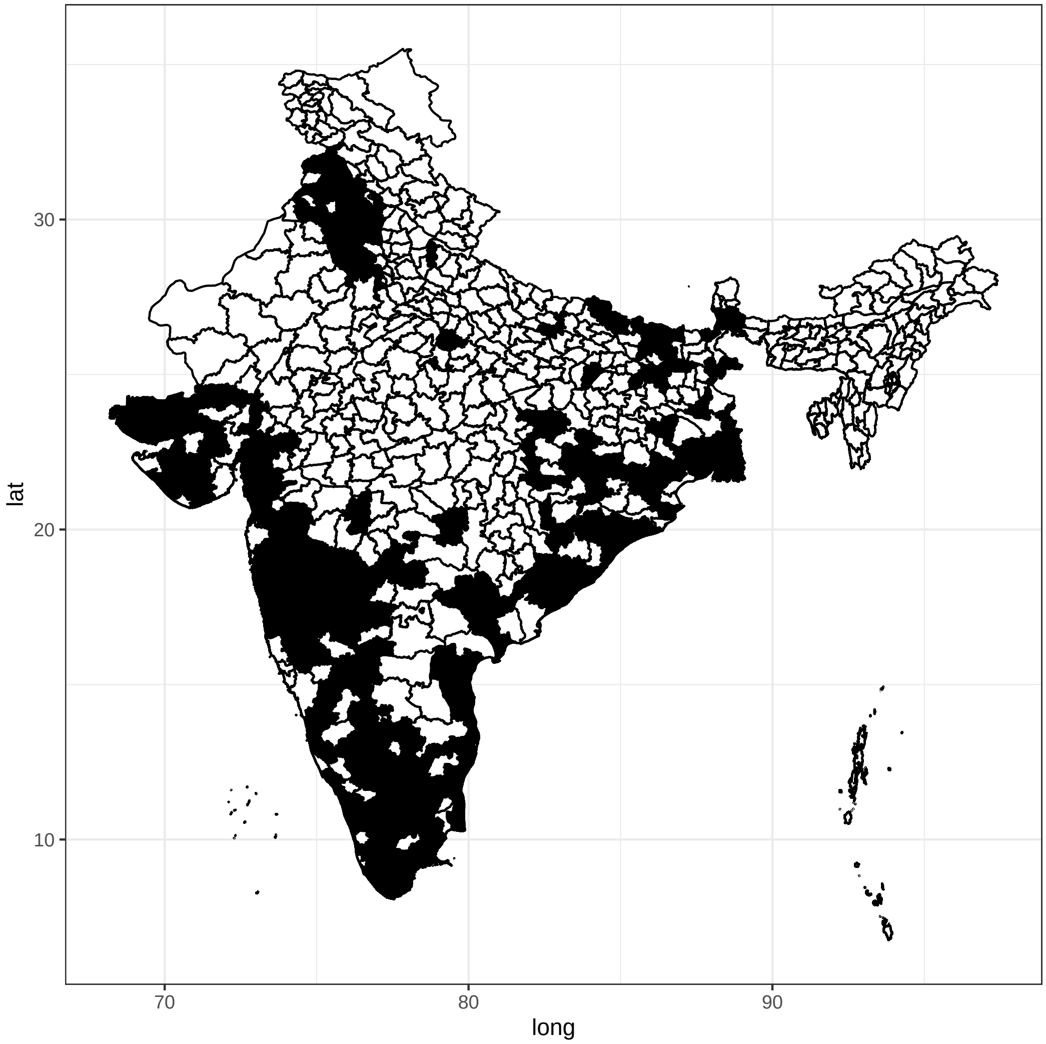
Notes: Y axis presents total number of judge posts across the sample courts. X-axis is the district population as measured in 2011 census. In the bottom panel, I plot the observed number of judges in a district court-year on the left y-axis, predicted number of judges based on the Law Commission Report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

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



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

Figure A.3: Sample district courts



Notes: 7 of 14 states in the sample include over two-thirds of their districts. Gujarat, Punjab, and Tamil Nadu are among the top industrialized states and have over 80% of their districts in the study sample.

Figure A.4: Construction of sample of firms

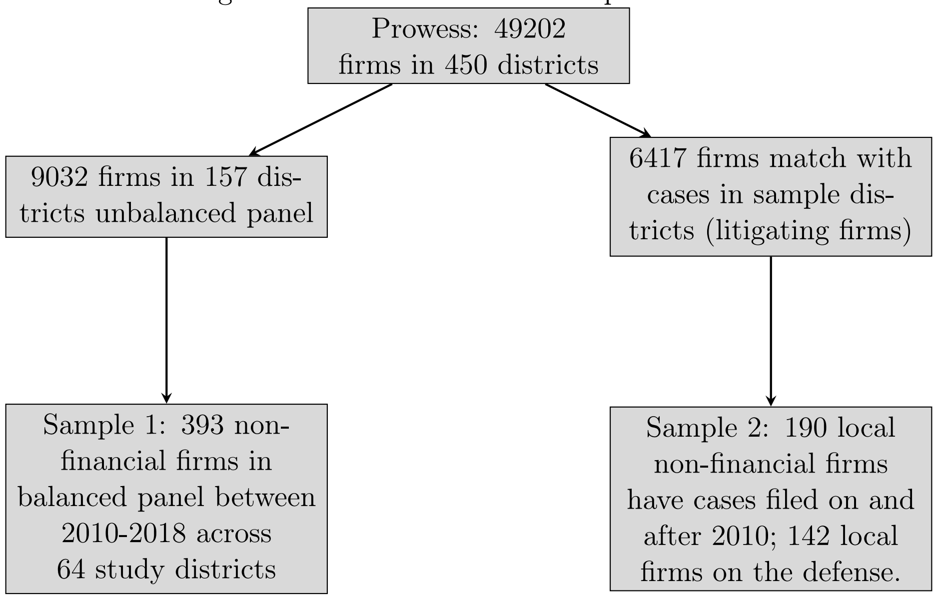
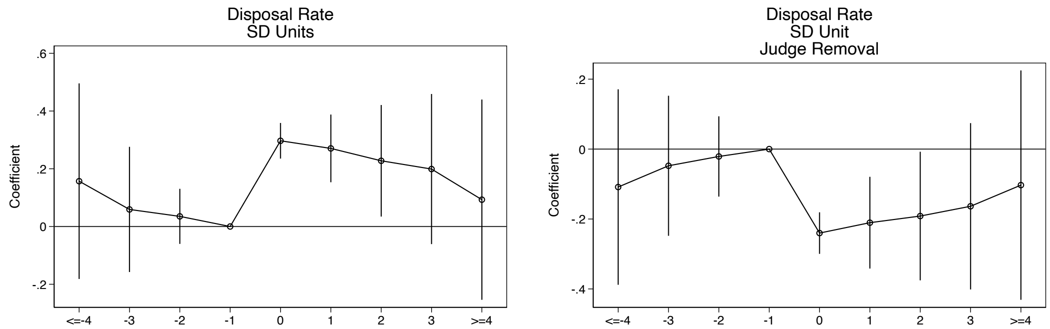
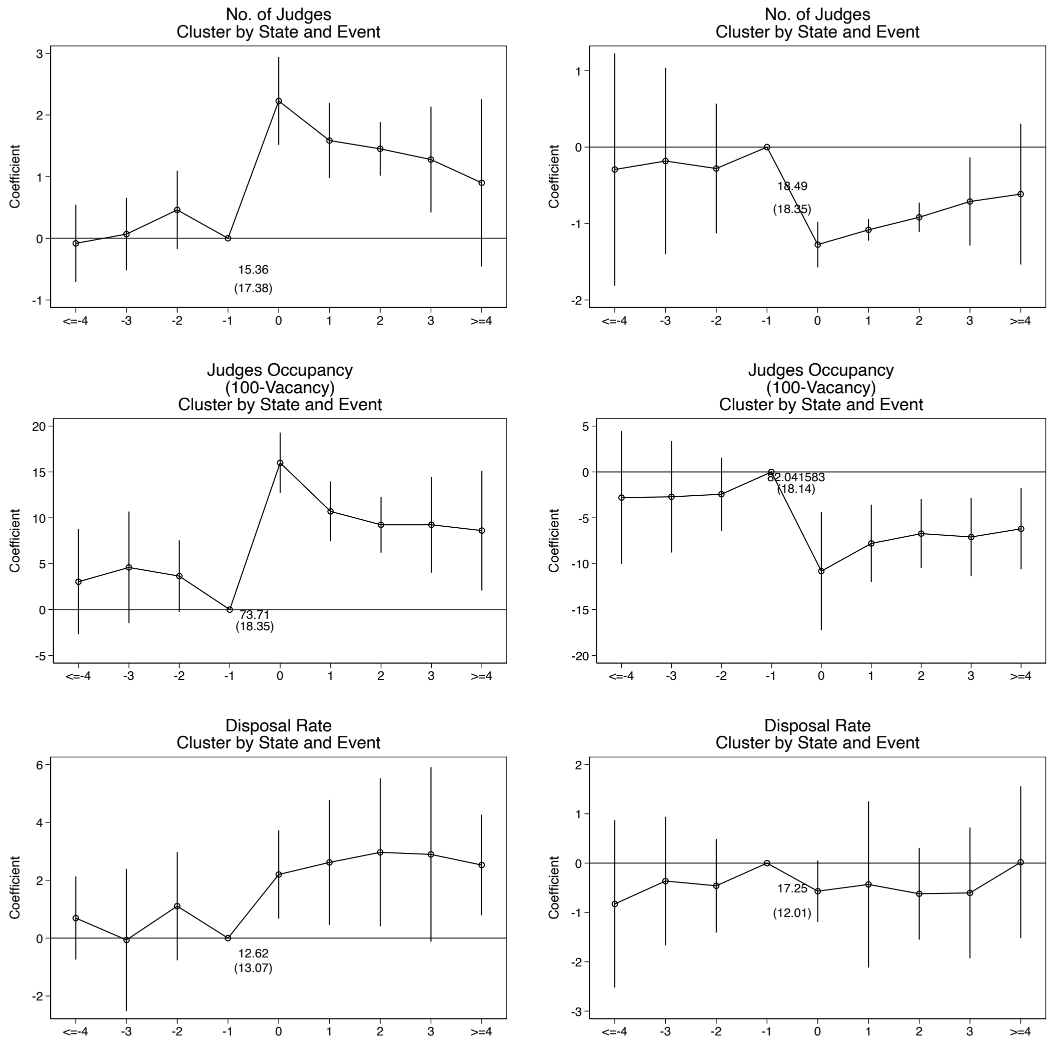


Figure A.5: Multiple Event Study Estimator: Simulation



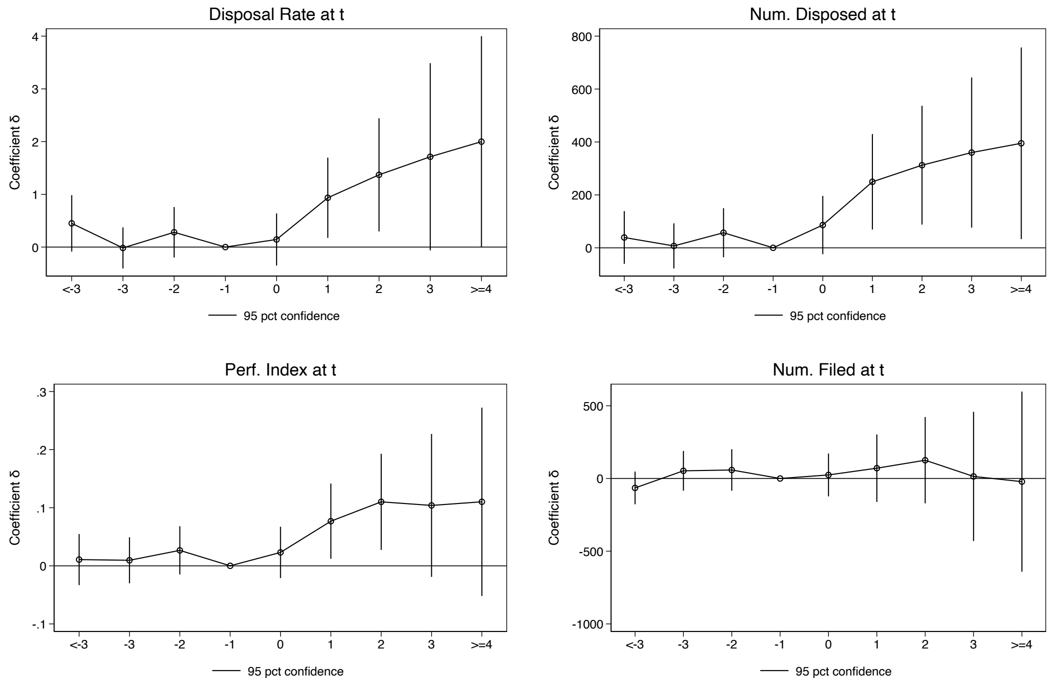
Notes: The above graphs present estimation of the treatment effects using the stacked event study estimator for multiple events using simulated data. The DGP of disposal rate is coded as a function of positive or negative event shocks of equal magnitude - 0.3 standard deviations in effect size - with error term distributed as a gamma function, mimicking data. 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 error term for the number of judges is drawn from a uniform distribution.

Figure A.6: Court Outcomes: Inference Robustness



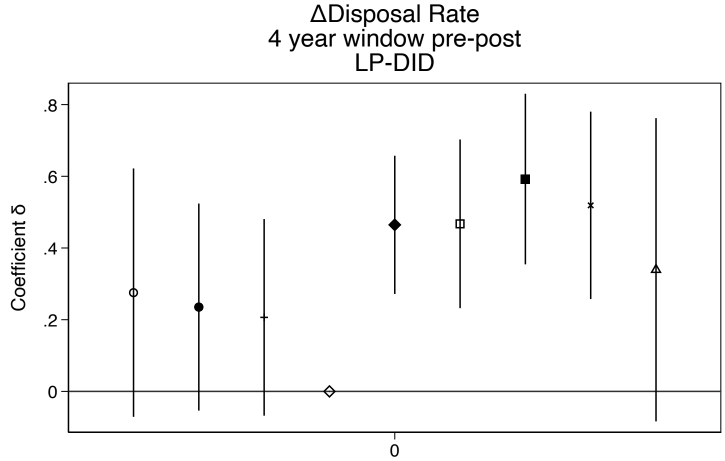
Notes: The figures plot the event study interaction coefficients from estimating Equation 1. Standard errors are clustered by state (instead of district) and event. Error bars present 95% confidence interval.

Figure A.7: Court Outcomes: Continuous Explanatory Variable



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

Figure A.8: Local Projection DID: Court Performance

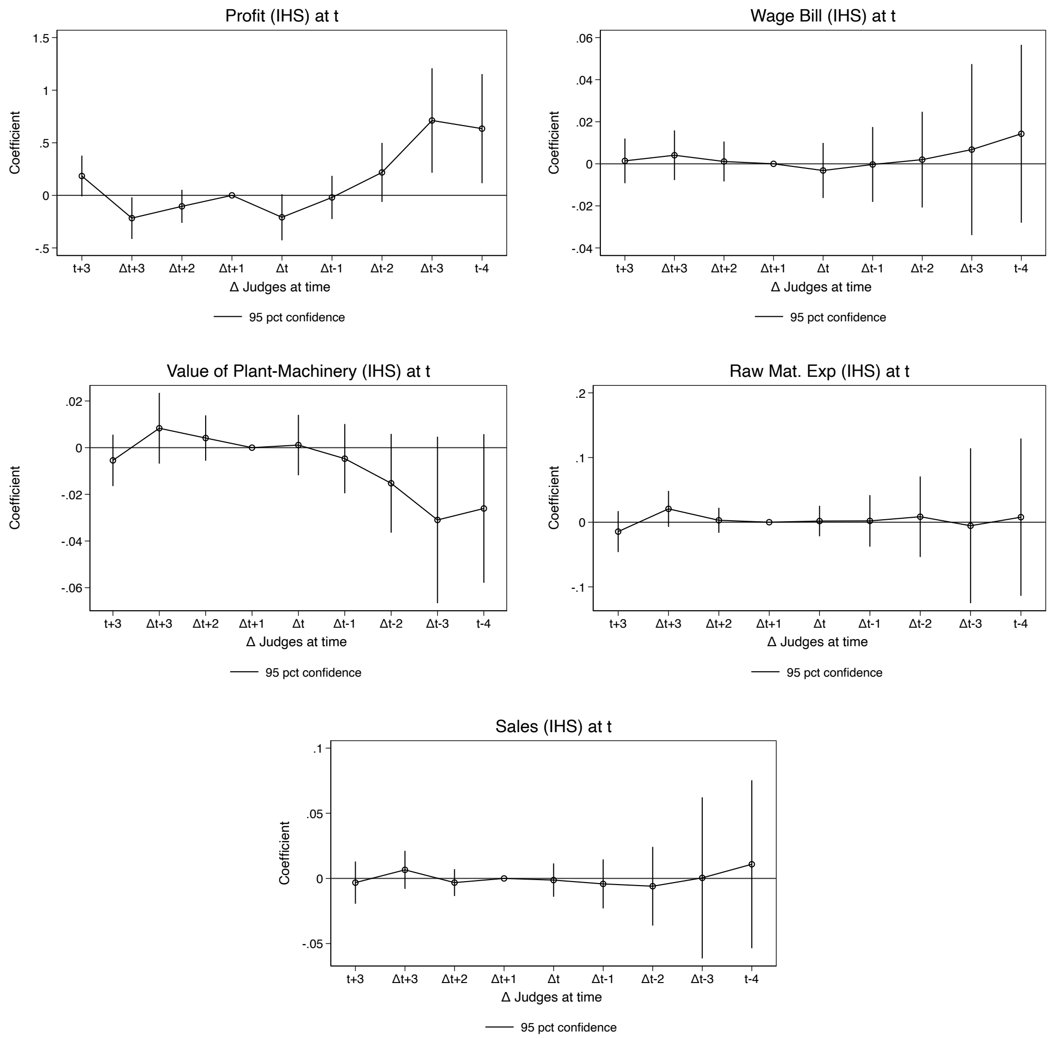


Notes: Following Dube et al. (2022), the local projection DID specification accounts for empirical challenges arising from impulse response functions generated by judicial staffing changes that occur many times and in opposing directions within the study period, similar to events in finance. Each coefficient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

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

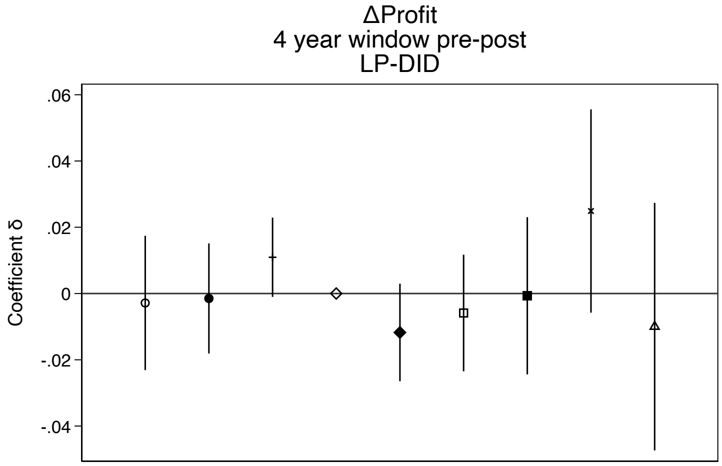
where Δ is the first difference operator.

Figure A.9: Firm-Level Outcomes: Continuous Explanatory Variable



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

Figure A.10: Local Projection DID: Firm Productivity

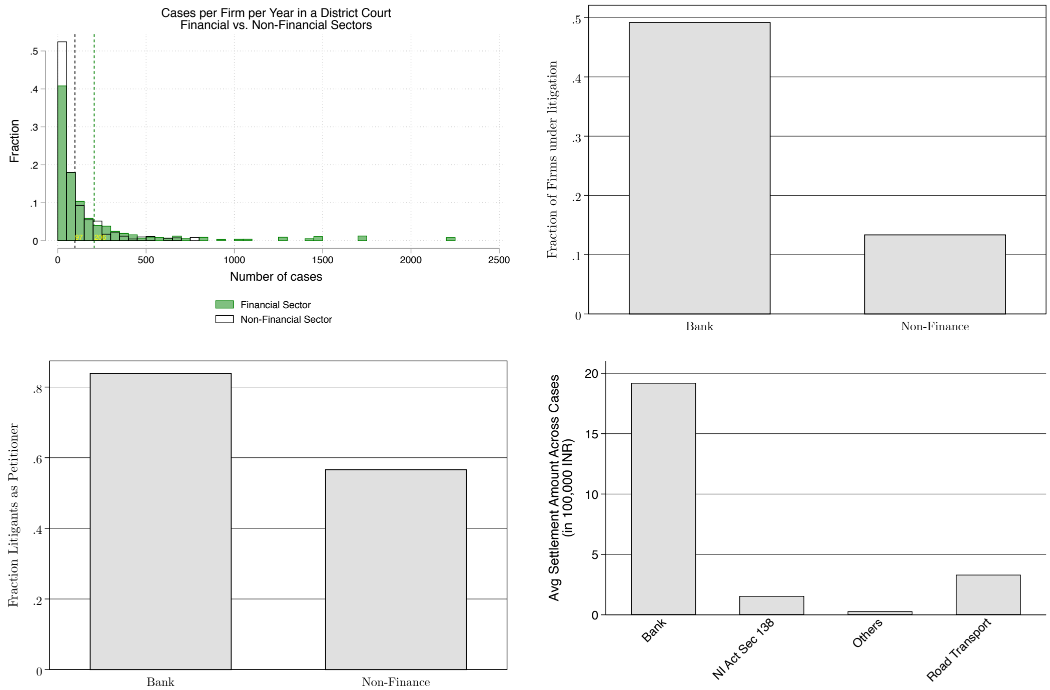


Notes: Following Dube et al. (2022), the local projection DID specification accounts for empirical challenges arising from impulse response functions generated by judicial staffing changes that occur many times and in opposing directions within the study period, similar to events in finance. Each coefficient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

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

where Δ is the first difference operator.

Figure A.11: Case-Types, Debt Litigations, and Settlement Amounts



Notes: Top-left figure presents the intensity of firm-related cases in district courts per firm, categorized as belonging to the financial sector or not. The second figure in the top panel presents the fraction of all firms in Prowess data belonging to either banking sector or non-finance sector (for e.g., manufacturing, services, trade and transportation, etc.) with at least one trial in the trial-level dataset. Bottom-right panel presents the fraction of these litigating firms appearing as the plaintiff (petitioner). Data on settlement amount in the bottom panel are from codified judgement documents from one court only for illustration.

Figure A.12: Model: Credit and Litigation

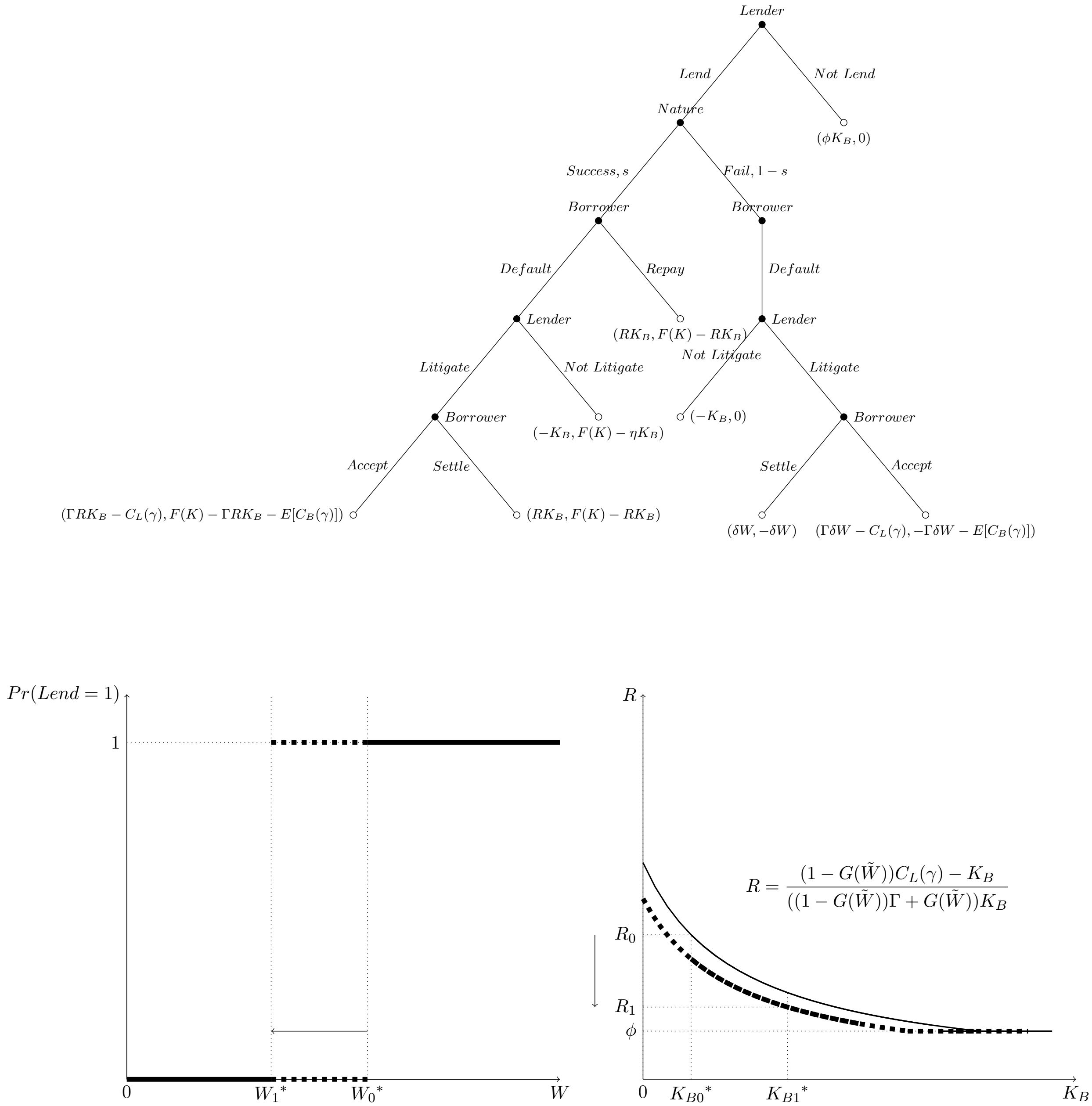
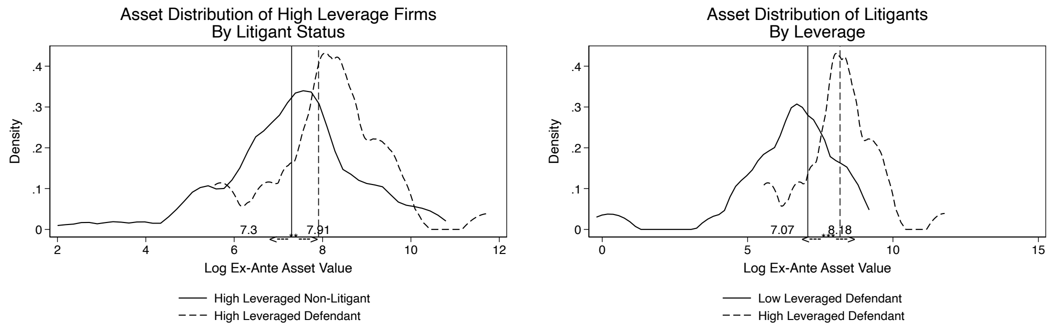
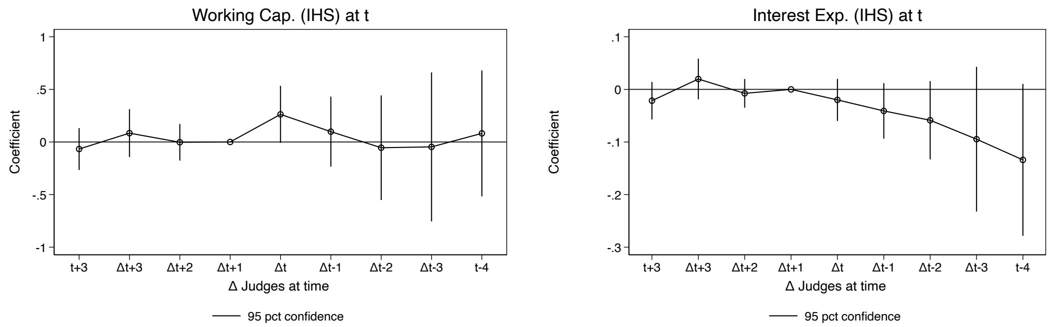


Figure A.13: Litigation Behavior

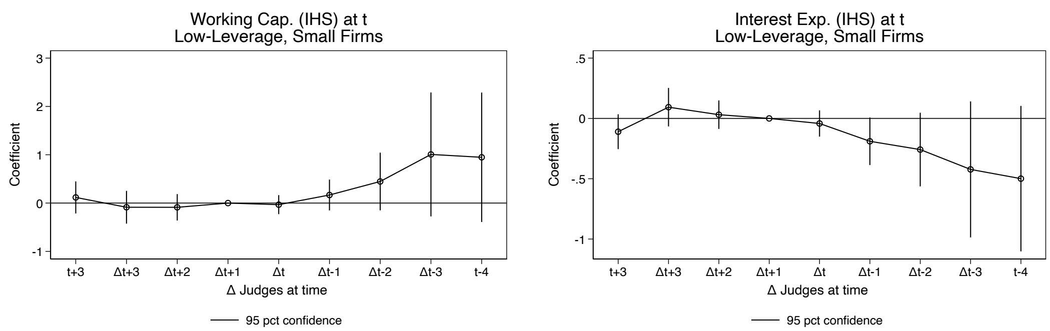


Notes: Panel A presents the kernel densities of local non-financial firms' ex-ante total asset value by: (a) litigation status among high leverage firms (left), and (b) leverage status among the defending firms (right). The lines represent the average asset values with statistical significance of this difference as noted.

Figure A.14: Firms' Credit Outcomes: Continuous Explanatory Variable
Panel A: Firm-level Working Capital and Interest Expenditure - All Sample Firms

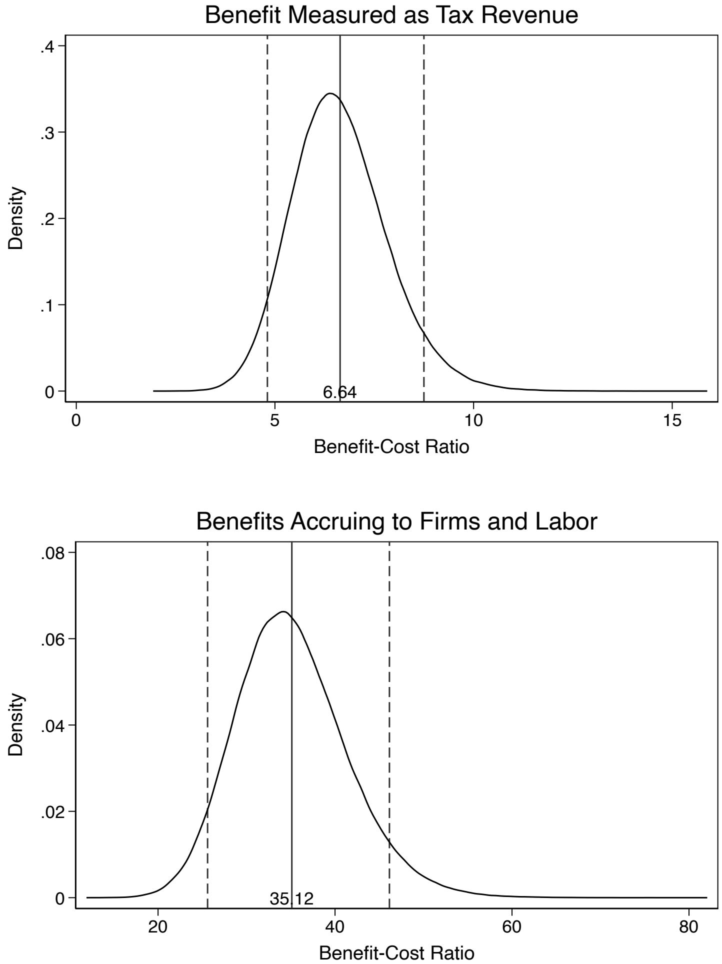


Panel B: Subsample of Low-Leverage, Small Sized Firms



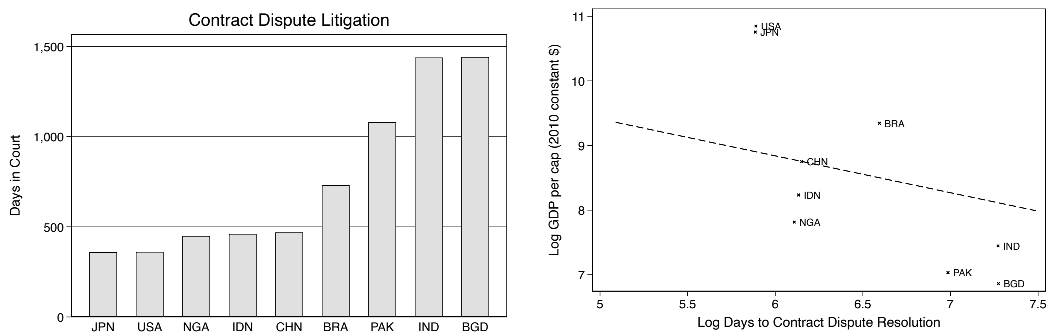
Notes: The figures present the generalized event study estimates relative to number of judges from $t+1$ when the outcome is measured at t as in Equation 2. Panel A presents the coefficients using firm-level working capital and interest expenditure across all firms in the main sample. Panel B presents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

Figure A.15: Benefit-Cost Ratio



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

Figure A.16: Court Congestion: Top 10 Populous Countries



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

A.5 Appendix: Tables

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

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

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

Table A.2: Court Outcomes and Judge Vacancy Changes

	Net Judge Addition			Net Judge Removal		
	(1)	(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

Notes: This table presents the estimates from Equation 1 using court-level outcomes, equivalent to Figure 1. Columns 1-3 present estimates following judge vacancy reduction (net judge increase) whereas Columns 4-6 present those following judge vacancy creation (net judge reduction). All court-level specifications include district and state-year fixed effect. Standard errors are clustered by district and event. I do not report statistical significance stars in line with journal submission guidelines.

Table A.3: Heterogeneity in Judge Staffing Levels

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

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

Table A.4: Caseload Outcomes

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

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

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

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

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

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

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

Notes: This table presents the estimates from Equation 1 using firm-level outcomes, equivalent to Figure 2, for judge vacancy 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. I do not report statistical significance stars in line with journal submission guidelines.

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

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

Notes: This table presents the estimates from Equation 1 using firm-level outcomes, equivalent to Figure 3, for judge vacancy creation. 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. I do not report statistical significance stars in line with journal submission guidelines.

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

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

Standard errors in parentheses

Notes: This table presents the estimates from Equation 1 for judge vacancy reduction using all registered formal sector firms in the district with missing data. Standard errors are clustered by district and event.

Table A.9: Net Judge Removal 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)
Neg x <=-4	0.00474	0.0134	0.0108	0.00469	0.0303	-0.00271	0.0137
	(0.00245)	(0.00662)	(0.00829)	(0.00144)	(0.00831)	(0.00279)	(0.00254)
Neg x -3	0.00157	0.00857	0.00999	-0.000129	0.00846	0.00915	0.0122
	(0.00307)	(0.00503)	(0.00994)	(0.00506)	(0.0110)	(0.0116)	(0.00175)
Neg x -2	0.00399	0.00417	0.0102	0.00196	-0.00443	0.00649	0.00388
	(0.00254)	(0.00392)	(0.00392)	(0.00192)	(0.0155)	(0.0107)	(0.00159)
Neg x 0	-0.00419	-0.00349	0.00258	-0.00188	-0.00632	-0.00667	-0.00110
	(0.00386)	(0.00244)	(0.00158)	(0.00134)	(0.00579)	(0.00468)	(0.00185)
Neg x 1	-0.00555	-0.00820	-0.00282	-0.00572	-0.0298	0.00629	-0.00486
	(0.00282)	(0.00369)	(0.00227)	(0.00189)	(0.0195)	(0.0108)	(0.00187)
Neg x 2	-0.00963	-0.0128	-0.00284	-0.00732	-0.0573	-0.00801	-0.00723
	(0.00130)	(0.00229)	(0.00377)	(0.00197)	(0.0275)	(0.0195)	(0.00402)
Neg x 3	-0.0117	-0.0189	-0.00525	-0.00897	-0.0386	-0.0214	-0.00917
	(0.00202)	(0.00340)	(0.00294)	(0.00435)	(0.0193)	(0.00913)	(0.00272)
Neg x >=4	-0.0147	-0.0245	-0.0106	-0.00878	-0.0480	-0.00563	-0.0144
	(0.00234)	(0.00356)	(0.00852)	(0.00594)	(0.0128)	(0.00678)	(0.00224)
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

Notes: This table presents the estimates from Equation 1 for judge vacancy reduction using all registered formal sector firms in the district with missing data. Standard errors are clustered by district and event.

Table A.10: Net Judge Addition and Missing Firm-Level Data

	(1) Wage Bill (Missing)	(2) Plant Value (Missing)	(3) Raw Mat (Missing)	(4) Sales (Missing)	(5) Profit (Missing)	(6) Working Cap. (Missing)	(7) Interest Exp (Missing)
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

Notes: This table presents the estimates from Equation 1 for judge vacancy reduction 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.11: Net Judge Removal and Missing Firm-Level Data

	(1) Wage Bill (Missing)	(2) Plant Value (Missing)	(3) Raw Mat (Missing)	(4) Sales (Missing)	(5) Profit (Missing)	(6) Working Cap. (Missing)	(7) Interest Exp (Missing)
Neg x <=-4	-0.000405	0.000301	-0.00298	-0.00199	-0.00105	-0.0000105	0.00156
	(0.000412)	(0.000655)	(0.00299)	(0.000538)	(0.000107)	(0.0000468)	(0.000453)
Neg x -3	-0.000449	-0.000137	-0.00227	-0.00215	-0.00101	0.000235	0.000469
	(0.000757)	(0.000650)	(0.00234)	(0.000901)	(0.000503)	(0.000173)	(0.000464)
Neg x -2	-0.000891	-0.000586	-0.00157	-0.00102	-0.000955	-0.0000314	-0.00103
	(0.00133)	(0.000400)	(0.00211)	(0.00137)	(0.000684)	(0.000153)	(0.000888)
Neg x 0	0.00180	0.000392	0.00256	0.00226	0.000503	0.000433	0.000449
	(0.00166)	(0.000492)	(0.00238)	(0.00161)	(0.000649)	(0.000179)	(0.000752)
Neg x 1	0.00339	0.000743	0.00373	0.00284	-0.0000510	0.000205	0.0000261
	(0.000691)	(0.000680)	(0.00179)	(0.000749)	(0.000468)	(0.000112)	(0.00126)
Neg x 2	0.00460	0.00246	0.00499	0.00502	0.000961	0.000417	0.00403
	(0.000609)	(0.00103)	(0.00147)	(0.000673)	(0.000727)	(0.000216)	(0.00131)
Neg x 3	0.00497	0.000480	0.00579	0.00697	0.000773	0.000492	0.00422
	(0.000570)	(0.00108)	(0.00207)	(0.000694)	(0.000398)	(0.000188)	(0.00177)
Neg x >=4	0.00544	0.00194	0.00779	0.00653	0.000558	0.000232	0.00629
	(0.000591)	(0.00164)	(0.00278)	(0.000778)	(0.000298)	(0.000176)	(0.00211)
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

Notes: This table presents the estimates from Equation 1 for judge vacancy creation 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

Notes: This table presents the estimates from Equation 1 for judge vacancy reduction using the subset of non-litigating balanced panel of firms in the district. Non-litigating 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: Net Judge Removal 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)
Neg x <=-4	-0.0149	-0.00366	0.00909	-0.00354	-0.0426	0.0296	0.0465
	(0.0157)	(0.00309)	(0.0116)	(0.00621)	(0.0169)	(0.0817)	(0.0143)
Neg x -3	-0.00737	0.00504	0.0111	0.00212	-0.0138	-0.0240	0.0250
	(0.0101)	(0.00700)	(0.00815)	(0.00589)	(0.0614)	(0.158)	(0.0140)
Neg x -2	-0.0100	-0.00350	0.00556	-0.00332	-0.0871	0.0569	-0.00447
	(0.0126)	(0.00527)	(0.00746)	(0.00984)	(0.0422)	(0.127)	(0.0156)
Neg x 0	-0.00567	-0.00124	-0.0138	-0.0112	-0.0168	-0.0348	-0.0110
	(0.00635)	(0.00380)	(0.00988)	(0.00878)	(0.0514)	(0.0794)	(0.0168)
Neg x 1	-0.00563	-0.00202	-0.0185	-0.0184	-0.119	0.0705	-0.0111
	(0.00762)	(0.00179)	(0.0151)	(0.0139)	(0.0398)	(0.0769)	(0.0155)
Neg x 2	-0.00942	0.00245	-0.0390	-0.0221	-0.0424	-0.0259	-0.0239
	(0.00265)	(0.00216)	(0.0213)	(0.00787)	(0.0534)	(0.0832)	(0.00853)
Neg x 3	-0.0187	-0.0103	-0.0466	-0.0424	-0.140	-0.0694	-0.0285
	(0.00442)	(0.00279)	(0.0316)	(0.00633)	(0.0986)	(0.0603)	(0.0181)
Neg x >=4	-0.0408	-0.0196	-0.0755	-0.0606	-0.172	-0.0383	-0.0398
	(0.00472)	(0.00491)	(0.0345)	(0.00859)	(0.0685)	(0.0654)	(0.0142)
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

Notes: This table presents the estimates from Equation 1 for judge vacancy creation using the subset of non-litigating balanced panel of firms in the district. Non-litigating 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.14: Neighboring Districts Firms' Outcome and 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

Notes: This table presents the estimates from Equation 1 for judge vacancy removal, 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.

Table A.15: Neighboring Districts Firms' Outcome and Net Judge Removal (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)
Neg x <=-4	-0.00872	-0.00271	0.00774	0.00515	0.0267	-0.0555	0.00423
	(0.0103)	(0.00410)	(0.00627)	(0.00964)	(0.0688)	(0.0375)	(0.00470)
Neg x -3	-0.00509	-0.00470	0.00829	0.0000129	-0.0110	-0.0359	0.00431
	(0.00576)	(0.00283)	(0.00547)	(0.00479)	(0.0535)	(0.0223)	(0.00728)
Neg x -2	0.000469	-0.000556	0.00103	-0.00152	-0.0158	-0.00676	0.00424
	(0.00359)	(0.00144)	(0.00349)	(0.00346)	(0.0269)	(0.0321)	(0.00431)
Neg x 0	-0.00166	-0.000180	-0.00104	-0.00167	0.00737	0.0184	0.00134
	(0.00292)	(0.00533)	(0.00492)	(0.00632)	(0.0224)	(0.0368)	(0.00172)
Neg x 1	-0.00471	0.00610	-0.00446	-0.0117	-0.0343	0.00303	0.000545
	(0.00531)	(0.00308)	(0.00562)	(0.0105)	(0.0491)	(0.0498)	(0.00874)
Neg x 2	-0.00603	0.00251	-0.00580	-0.00366	-0.0328	-0.0257	-0.00139
	(0.00543)	(0.00313)	(0.00336)	(0.00769)	(0.0624)	(0.0206)	(0.00497)
Neg x 3	0.00679	0.00248	-0.00394	-0.00768	-0.0578	-0.0387	0.00876
	(0.00685)	(0.000904)	(0.00855)	(0.00661)	(0.0632)	(0.0558)	(0.00646)
Neg x >=4	0.00600	0.00896	-0.00736	0.00822	-0.00678	-0.126	0.00446
	(0.00765)	(0.00245)	(0.00298)	(0.00588)	(0.0434)	(0.104)	(0.0112)
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

Notes: This table presents the estimates from Equation 1 for judge vacancy creation, 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.

Table A.16: Dropping Industrial States: 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	0.0336	-0.0837	0.0104	-0.00766	0.223	0.232	0.0429
	(0.0719)	(0.139)	(0.0364)	(0.105)	(0.281)	(0.537)	(0.0571)
Pos x -3	0.00852	0.0219	-0.00255	0.00525	0.716	0.149	0.0632
	(0.0331)	(0.0470)	(0.0486)	(0.0383)	(0.168)	(0.155)	(0.0442)
Pos x -2	0.0341	-0.0256	0.0177	0.0126	0.0742	0.145	0.0770
	(0.0275)	(0.0281)	(0.0112)	(0.0762)	(0.362)	(0.220)	(0.0336)
Pos x 0	0.0209	0.0259	0.0191	0.0279	0.192	0.452	0.0220
	(0.00754)	(0.0155)	(0.00550)	(0.0124)	(0.101)	(0.0695)	(0.0375)
Pos x 1	0.0292	0.00441	0.0555	0.0442	0.354	0.325	-0.0661
	(0.0103)	(0.0118)	(0.0156)	(0.0160)	(0.116)	(0.0599)	(0.0293)
Pos x 2	0.0399	-0.00209	0.0619	0.0593	0.188	0.366	-0.0473
	(0.0107)	(0.0278)	(0.0208)	(0.0145)	(0.225)	(0.0254)	(0.0408)
Pos x 3	0.0573	0.0186	0.0493	0.0649	0.446	0.586	-0.0347
	(0.00799)	(0.0328)	(0.0185)	(0.0108)	(0.146)	(0.0600)	(0.0201)
Pos x >=4	0.0622	0.00895	0.0345	0.0398	0.196	0.464	-0.0587
	(0.0186)	(0.0404)	(0.0408)	(0.0147)	(0.0772)	(0.0947)	(0.00461)
Observations	8631	8631	8631	8631	8631	8631	8631
No. Firms	149	149	149	149	149	149	149
No. Districts	44	44	44	44	44	44	44

Notes: This table presents the event study reduced form estimates of positive staffing changes on the main firms sample after dropping large, industrial states from the sample.

Table A.17: Dropping Industrial States: 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	-0.0322	-0.00182	-0.0193	-0.0181	-0.188	-0.216	0.0444
	(0.0149)	(0.0290)	(0.0176)	(0.0180)	(0.129)	(0.0307)	(0.0101)
Neg x -3	-0.0195	-0.00795	-0.0110	0.00604	-0.262	-0.0722	0.0193
	(0.00951)	(0.0148)	(0.00976)	(0.0162)	(0.0718)	(0.288)	(0.0161)
Neg x -2	-0.0191	-0.00126	-0.00660	-0.00724	-0.160	-0.0199	-0.0174
	(0.00873)	(0.0131)	(0.00509)	(0.0167)	(0.132)	(0.154)	(0.0227)
Neg x 0	-0.000568	-0.0164	-0.00954	-0.0166	-0.171	-0.104	-0.0237
	(0.00928)	(0.0157)	(0.0160)	(0.0162)	(0.159)	(0.188)	(0.0328)
Neg x 1	0.00474	-0.0183	-0.0225	-0.0360	-0.382	-0.00374	0.00109
	(0.0102)	(0.0100)	(0.0237)	(0.0143)	(0.0965)	(0.173)	(0.0340)
Neg x 2	-0.00722	-0.0149	-0.0405	-0.0651	-0.500	-0.152	-0.00367
	(0.0163)	(0.0249)	(0.0384)	(0.0137)	(0.102)	(0.174)	(0.0166)
Neg x 3	-0.0290	-0.0359	-0.0539	-0.0994	-0.777	-0.362	-0.0219
	(0.0248)	(0.0490)	(0.0614)	(0.0304)	(0.215)	(0.0840)	(0.0313)
Neg x >=4	-0.0571	-0.0922	-0.0833	-0.109	-0.471	-0.189	-0.0146
	(0.0252)	(0.0680)	(0.0604)	(0.0318)	(0.0763)	(0.166)	(0.0177)
Observations	8631	8631	8631	8631	8631	8631	8631
No. Firms	149	149	149	149	149	149	149
No. Districts	44	44	44	44	44	44	44

Notes: This table presents the event study reduced form estimates of negative staffing changes on the main firms sample after dropping large, industrial states from the sample.

Table A.18: Dropping Largest Districts: 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	0.00512	-0.0626	-0.0264	0.00673	-0.251	0.253	0.0908
	(0.0665)	(0.108)	(0.0777)	(0.0833)	(0.258)	(0.440)	(0.0600)
Pos x -3	-0.00157	0.00833	-0.0640	0.00726	0.163	0.132	0.0706
	(0.0415)	(0.0353)	(0.102)	(0.0441)	(0.152)	(0.236)	(0.0334)
Pos x -2	0.000595	-0.00513	0.0110	0.0191	0.105	0.108	0.0810
	(0.0290)	(0.0382)	(0.0455)	(0.0726)	(0.407)	(0.109)	(0.0274)
Pos x 0	0.00133	0.0181	0.0428	0.0275	0.0877	0.319	-0.00654
	(0.0201)	(0.0137)	(0.0207)	(0.00645)	(0.112)	(0.0755)	(0.0285)
Pos x 1	0.0212	-0.00245	0.0450	0.0306	0.424	0.267	-0.103
	(0.0167)	(0.00982)	(0.0425)	(0.0143)	(0.142)	(0.175)	(0.0300)
Pos x 2	0.0280	-0.00481	0.0927	0.0454	0.260	0.262	-0.0804
	(0.0135)	(0.0266)	(0.0289)	(0.0137)	(0.132)	(0.195)	(0.0371)
Pos x 3	0.0460	0.0269	0.0580	0.0566	0.457	0.306	-0.0963
	(0.00817)	(0.0452)	(0.0134)	(0.00797)	(0.109)	(0.0989)	(0.0261)
Pos x >=4	0.0463	0.0152	0.0374	0.0330	0.217	0.225	-0.109
	(0.0172)	(0.0462)	(0.0233)	(0.00922)	(0.0633)	(0.0995)	(0.0114)
Observations	11916	11916	11916	11916	11916	11916	11916
No. Firms	217	217	217	217	217	217	217
No. Districts	61	61	61	61	61	61	61

Notes: This table presents the event study reduced form estimates of positive staffing changes on the main firms sample after dropping large, metropolitan districts from the sample.

Table A.19: Dropping Largest Districts: 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	-0.0141	0.00185	-0.00562	-0.00799	-0.185	-0.126	0.0502
	(0.0114)	(0.0210)	(0.0123)	(0.0102)	(0.0792)	(0.0760)	(0.00924)
Neg x -3	-0.0137	0.000103	0.00633	0.00177	-0.162	-0.0273	0.0137
	(0.0124)	(0.0122)	(0.0220)	(0.0100)	(0.0786)	(0.137)	(0.00949)
Neg x -2	-0.00720	0.000730	-0.00600	-0.00410	-0.121	0.0794	-0.0115
	(0.0129)	(0.0109)	(0.0104)	(0.0113)	(0.0969)	(0.0954)	(0.0168)
Neg x 0	-0.0000545	-0.0121	-0.0239	-0.0121	-0.102	-0.0322	-0.0243
	(0.00970)	(0.0157)	(0.0208)	(0.0116)	(0.128)	(0.199)	(0.0301)
Neg x 1	0.00183	-0.0127	-0.0249	-0.0186	-0.348	0.0280	-0.0106
	(0.00842)	(0.0138)	(0.0240)	(0.0167)	(0.0744)	(0.145)	(0.0294)
Neg x 2	-0.00585	-0.00723	-0.0637	-0.0392	-0.321	-0.0467	-0.0341
	(0.00627)	(0.0243)	(0.0323)	(0.0136)	(0.0966)	(0.0807)	(0.0258)
Neg x 3	-0.0232	-0.0325	-0.0647	-0.0682	-0.554	-0.173	-0.0310
	(0.00838)	(0.0381)	(0.0436)	(0.0185)	(0.144)	(0.0697)	(0.0398)
Neg x >=4	-0.0428	-0.0722	-0.0988	-0.0697	-0.352	0.150	-0.0415
	(0.00942)	(0.0446)	(0.0591)	(0.0139)	(0.0620)	(0.199)	(0.0227)
Observations	11916	11916	11916	11916	11916	11916	11916
No. Firms	217	217	217	217	217	217	217
No. Districts	61	61	61	61	61	61	61

Notes: This table presents the event study reduced form estimates of negative staffing changes on the main firms sample after dropping large, metropolitan districts from the sample.

Table A.20: Net Judge Addition and Firms' Outcomes: Clustering by State and Event

	(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
	(0.0417)	(0.0667)	(0.0464)	(0.0726)	(0.204)	(0.416)	(0.0600)
Pos x -3	0.000279	0.0162	-0.0505	0.0120	0.135	0.0202	0.0883
	(0.0310)	(0.0172)	(0.0686)	(0.0369)	(0.340)	(0.235)	(0.0462)
Pos x -2	0.00715	0.00361	0.00903	0.0181	0.193	0.111	0.0957
	(0.0339)	(0.0368)	(0.0267)	(0.0558)	(0.339)	(0.0783)	(0.0348)
Pos x 0	-0.00187	0.0179	0.0171	0.0201	0.110	0.389	-0.00813
	(0.0179)	(0.0166)	(0.0312)	(0.00576)	(0.114)	(0.0866)	(0.0276)
Pos x 1	0.0196	0.00435	0.0253	0.0184	0.418	0.200	-0.0864
	(0.0184)	(0.00621)	(0.0482)	(0.0210)	(0.0892)	(0.156)	(0.0383)
Pos x 2	0.0207	-0.00149	0.0717	0.0210	0.310	0.172	-0.0802
	(0.0234)	(0.0211)	(0.0447)	(0.0302)	(0.0811)	(0.136)	(0.0472)
Pos x 3	0.0369	0.0266	0.0401	0.0360	0.462	0.275	-0.0817
	(0.0220)	(0.0345)	(0.0279)	(0.0224)	(0.0605)	(0.147)	(0.0588)
Pos x >=4	0.0514	0.0194	0.0336	0.0289	0.334	0.244	-0.0903
	(0.0259)	(0.0359)	(0.0188)	(0.0147)	(0.102)	(0.138)	(0.0566)
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

Notes: This table presents the event study reduced form estimates of positive staffing changes on the main firms sample with standard errors clustered by state and event.

Table A.21: Net Judge Removal and Firms' Outcomes: Clustering by State and Event

	(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
	(0.00675)	(0.0108)	(0.0100)	(0.00233)	(0.0877)	(0.0506)	(0.0328)
Neg x -3	-0.00570	0.00140	0.00601	0.00193	-0.0664	-0.0151	0.00411
	(0.00867)	(0.00625)	(0.00990)	(0.00503)	(0.0741)	(0.0564)	(0.0213)
Neg x -2	-0.00328	-0.000139	-0.000887	-0.00116	-0.0631	0.0266	-0.00900
	(0.00590)	(0.00405)	(0.00564)	(0.00341)	(0.0410)	(0.0667)	(0.0155)
Neg x 0	0.00116	-0.00697	-0.00905	-0.00492	-0.0499	-0.0356	-0.00827
	(0.00542)	(0.00932)	(0.00676)	(0.00718)	(0.0541)	(0.0959)	(0.0168)
Neg x 1	0.00113	-0.00960	-0.0109	-0.00699	-0.162	0.0252	-0.00239
	(0.00586)	(0.0108)	(0.0108)	(0.0147)	(0.0656)	(0.0695)	(0.0188)
Neg x 2	-0.00149	-0.00692	-0.0289	-0.0115	-0.170	0.00525	-0.00874
	(0.00530)	(0.0104)	(0.0165)	(0.0230)	(0.0843)	(0.0872)	(0.0248)
Neg x 3	-0.00967	-0.0187	-0.0312	-0.0251	-0.264	-0.0679	-0.00507
	(0.00633)	(0.0183)	(0.0221)	(0.0291)	(0.116)	(0.0994)	(0.0340)
Neg x >=4	-0.0224	-0.0361	-0.0495	-0.0277	-0.207	0.0580	-0.0126
	(0.0108)	(0.0396)	(0.0236)	(0.0305)	(0.120)	(0.186)	(0.0488)
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

Notes: This table presents the event study reduced form estimates of negative staffing changes on the main firms sample with standard errors clustered by state and event.

Table A.22: Credit Mechanism

	Net Judge Addition			Net Judge Removal		
	(1)	(2) Working Cap. (IHS)	(3) Interest Exp (IHS)	(4)	(5) Working Cap. (IHS)	(6) Interest Exp (IHS)
	All Banks	Low Lev Small Firms	Low Lev Small Firms	All Banks	Low Lev Small Firms	Low Lev Small Firms
Event x <=-4	0.0334	0.0222	0.303	-0.00525	-0.156	0.0146
	(0.0437)	(0.238)	(0.245)	(0.00658)	(0.102)	(0.0261)
Event x -3	-0.0460	-0.195	0.123	0.000752	-0.0468	-0.00744
	(0.0553)	(0.551)	(0.122)	(0.0104)	(0.0739)	(0.0198)
Event x -2	0.0369	-0.148	0.124	-0.00265	-0.0357	-0.0118
	(0.00935)	(0.0701)	(0.0870)	(0.0126)	(0.0437)	(0.0259)
Event x 0	-0.0306	0.199	-0.0941	0.00811	-0.0343	0.00958
	(0.0249)	(0.187)	(0.0522)	(0.0128)	(0.0843)	(0.0216)
Event x 1	0.0258	0.0431	-0.207	-0.0121	0.0330	0.0339
	(0.0320)	(0.0778)	(0.110)	(0.0101)	(0.0683)	(0.0295)
Event x 2	0.0121	-0.0826	-0.172	0.00171	0.0868	0.0290
	(0.0693)	(0.133)	(0.0764)	(0.0236)	(0.0582)	(0.0236)
Event x 3	0.0852	0.425	-0.179	-0.00314	-0.0374	0.0512
	(0.0422)	(0.197)	(0.0620)	(0.0244)	(0.0373)	(0.0405)
Event x >=4	0.0609	0.178	-0.198	-0.0109	0.0591	0.0675
	(0.0353)	(0.0743)	(0.0578)	(0.0303)	(0.0815)	(0.0648)
Observations	5670	6210	6210	5670	6210	6210
No. Firms	NA	105	105	NA	105	105
No. Districts	110	30	30	110	30	30

Notes: I use the Reserve Bank of India annual district-level credit data to industrial borrowers aggregated across all banks, and by banking sector as well as firm-level data on working capital and interest expenditure. Columns 1-3 present estimates following judge vacancy reduction (net judge increase) whereas Columns 4-6 present those following judge vacancy creation (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. Firm-level specifications include firm fixed effect. Standard errors are clustered by district and event. I do not report statistical significance stars in line with journal submission guidelines.

Table A.23: Local Recorded Crime and Judge Staffing Changes

	Net Judge Addition		Net Judge Removal	
	(1) Serious IPC Crime (IHS)	(2) Other IPC Crime (IHS)	(3) Serious IPC Crime (IHS)	(4) Other IPC Crime (IHS)
Event x <=-4	0.00343	0.00550	0.027	-0.0160
	(0.0062)	(0.0417)	(0.00467)	(-0.0128)
Event x -3	-0.00816	-0.0223	0.0193	-0.0128
	(0.022)	(0.0340)	(0.00416)	(0.0281)
Event x -2	0.00232	-0.0105	0.00267	-0.00100
	(0.0032)	(0.0165)	(0.0071)	(0.0194)
Event x 0	-0.0182	-0.00279	0.00498	0.0280
	(0.0047)	(0.0414)	(0.00841)	(0.0356)
Event x 1	-0.0039	-0.0246	-0.011	0.0578
	(0.0124)	(0.0191)	(0.0104)	(0.0206)
Event x 2	-0.0199	0.0149	-0.0124	-0.0113
	(0.00488)	(0.0257)	(0.0055)	(0.0263)
Event x 3	-0.00592	-0.101	-0.022	0.0590
	(0.0029)	(0.0213)	(0.0055)	(0.0363)
Event x >=4	-0.0256	-0.00650	-0.0164	0.0676
	(0.00509)	(0.0537)	(0.0055)	(0.0222)
Observations	9101	9101	9101	9101
No. Districts	195	195	195	195
Control Mean	2518	1930	3677	1744

Notes: I use annual district-level reported crime data by the National Crime Records Bureau (NCRB), under the Ministry of Home, Government of India. All the crime variables are based on reported crimes under the Indian Penal Code (IPC). Serious IPC Crime include the bulk of violent crimes such as murder, riots, and acts causing bodily injuries. Other IPC crimes are small-scale property crimes and financial frauds with low financial value. Columns 1-2 present estimates following judge vacancy reduction (net judge increase) whereas Columns 3-4 present those following judge vacancy creation (net judge reduction) as per Equation 1. All specifications include district and state-year fixed effect. I do not report statistical significance stars in line with journal submission guidelines.

Table A.24: Decomposition - Firm Profits

	(1)	(2)	(3)	(4)
Sales	1.635	1.637	1.405	1.242
	(0.331)	(0.332)	(0.263)	(0.307)
Working Cap	0.115	0.114	0.120	0.107
	(0.0275)	(0.0278)	(0.0305)	(0.0355)
Interest Exp	-0.855	-0.856	-0.756	-0.997
	(0.199)	(0.200)	(0.193)	(0.189)
Lesser Crime	-0.131			
	(0.123)			
All Crime		-0.104		
		(0.463)		
Profit t-1	0.0214	0.0210	-0.00181	-0.00443
	(0.0212)	(0.0212)	(0.0206)	(0.0212)
Profit t-2	0.0161	0.0165	0.00260	-0.00359
	(0.0121)	(0.0122)	(0.0127)	(0.0174)
Observations	2708	2708	2503	2114
No. Firms	369	369	341	299
Firm FE	X	X	X	X
Year Interactions	State-Year	State-Year	District-Year	District-Year, Industry-Year

Notes: This table presents a firm fixed effect regression of asinh-transformed variables - profit (dep var) on sales revenue, working capital, interest expenditure, local crime (depending on other district-time controls) and lagged profit variables. Following firm-level profit maximizing problem, profit should be positively correlated with sales revenue with an elasticity close to 1 as well as the extent of working capital to finance operating expenses, whereas negatively correlated with the cost of borrowing (reflected in interest expenditure) and other costs induced by local crime. Columns 1 and 2 control for state-year dummies to non-parametrically account for macro-economic changes at the state-level in addition to firm fixed effect. Columns 3 and 4 introduce district-year and additionally, industry-year dummies respectively. Since crime variables vary only at the district-year level, these are absorbed by the district-year dummies. The purpose of this table is to suggest that financing-related costs have larger elasticities with respect to firm profits compared to local crime.