

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, constructed using the universe of legal case records, and an event study research design, I show that changes in judge staffing levels affecting vacancy rates in local district 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 support 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 changing the supply of 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-levels substantially reduces pending case backlog and enhances the productivity of local formal sector firms, with indications of broad-based improvements in the local economy. The backlog reduction through improved judicial staffing helps release and recirculate valuable assets, such as bank credit, which were otherwise stuck in litigation for long periods of time. The overall economic returns are large and rapid, occurring within a short timescale of 2-3 years from the time of staffing-level increase.

District courts in India, similar to county courts in the US/UK, are the relevant front-line judicial institutions that are central to improving state 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 under dispute. Resolution of such 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.

¹This 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.

The staffing variation results from a combination of recruitments, retirements, and rotation of judges between district courts. These changes at the district court-level are driven by centralized policies on retirement at 60 years of age, sporadic and often failed recruitment drives, and frequent rotation of judges between district courts, which are executed at the state-level. I show that the timings of these changes are unrelated to existing court backlog and other changing socio-economic and political conditions, including investments/budgetary allocation by government or the private sector in any of the study districts. Specifically, given the constraints in implementing the different policies concerning judge assignment, the net effect on the judge staffing levels at any given point in time and in any given court is plausibly exogenous.² These generate sharp and persistent discontinuities in the number of judges and vacancy rates. Assuringly, I find no significant trends in the prior period across key outcomes and potential confounders as a support for the assumptions for causal identification. I also estimate the impact by employing generalized difference in difference (DiD) research designs to find qualitatively similar results. 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](#)).

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 the study 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 ([Bandiera 2003](#)).³ Given the jurisdiction of district courts and the types of cases that are litigated in such courts, there are two main channels through which frontline judicial capacity affects firm productivity: (a) debt contract enforcement, which is particularly relevant for the recovery of bank capital stuck under litigation, affecting local credit supply, and (b) protection of property from thefts and embezzlement that enable firms to safeguard their stock of raw material, inventory, and capital goods. Resulting debt recovery could plausibly increase liquidity-driven additional lending by banks to firms, which is particularly relevant in the presence of financial market frictions that limit costless movement of capital even within the country. Indeed, local branch managers of large national banks confirm that “the lending decisions and ability to give credit depend largely on how small/ big the non-performing asset (NPA) portfolio at their branch is”. Local credit supply constraints subsequently affect the ability of firms to borrow, as documented by a rich literature (e.g., [de Mel et al. 2008](#); [Banerjee and Duflo 2014](#); [Bazzi et al. 2023](#)). Improved access to capital, including operating capital, through

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

³All 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](#).

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

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 the resolution of bank-related cases in courts, subsequent district-level lending by banks to industrial borrowers, and firm-level impact on working capital and interest expenditures. 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 on the role of lower monitoring costs, I examine district-level reported crime outcomes and firm-level expenditure on raw material. Raw material and inventory are movable properties that require safeguarding and therefore, expenditures on these are important indicators of how firms respond to varying degree of law enforcement.

There are three key 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-level change results in two more judges added to a court that persists over 3-4 years. Correspondingly, I note a sustained 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-level changes have roughly half the effect size in reducing the staffing levels, and thus have commensurately smaller effects on disposal rate.⁴

Second, local firm-level productivity improves substantially following net increases in the number of judges and decreases following net reductions. Specifically, firm-level wage expenditure and profits respond significantly to changes in judicial staffing levels. I find that the average wage bill increases by around 5% 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 (such as through a reduction in interest expenditures). On the other hand, a decrease in the number of judges has a negative effect:

⁴This 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.

wage bill contracts 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 a natural lag 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 firm-level results using a balanced panel of incumbent firms 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 suggestive evidence that improved judicial staffing could result in higher entry of firms through new incorporations without affecting exits. Greater firm entry could imply better business dynamism and increased competition. This would downward bias the estimated impact. Second, I find that the effects of judicial staffing-level changes 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 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-level 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. Specifically, the increase in judge staffing levels improve the resolution of debt recovery cases involving banks and reduce the time period for resolving such cases. This is consistent with the claim that courts facilitate local markets by enforcing contracts and property rights, which is important for routine debt recovery by banks and other

⁵The 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 construction of the balanced panel sample. 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 could likely be downward biased due to 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 the main analysis.

lenders. Debt recovery cases typically precede bankruptcy and given the larger scale of debt recovery disputes, addressing this could also have implications for bankruptcy proceedings.

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 (measured using 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. There are two plausible reasons: First, the intensity of the negative staffing-level changes are relatively weak compared to the positive changes to detect small effects. Second, the time horizon for recognizing defaults and accumulation of debt-recovery cases in courts ([Ashraf et al. 2020](#); [Breza and Kinnan 2021](#)) could introduce a lag beyond the effect window of judge staffing-level changes.⁶ In contrast, the noted increase in lower-order crimes could increase 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 front-line judiciary. A back of the envelope calculation of the benefit-cost ratio shows large returns. I measure benefits accruing to the sample firms (through taxable corporate profit) and their employees (through taxable wages). 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 assumptions are conservative. These calculations suggest that adding one more judge can generate over 6 times net tax revenue, considering even the most conservative estimates. The social return is orders of magnitude higher. These estimates are likely a lower bound considering that an improvement in judicial capacity could generate many other 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 levels 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 rather than a permanent improvement in the rule of law. 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

⁶Based on conversations with bankers in India, the general debt recovery strategy involves litigation in courts as 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](#).

was declared as NPA, according to India's central bank RBI). A 2-3 percentage point improvement in court-level backlog resolution could unfreeze a large amount of capital ($0.1 \times \$170\text{billion} \times 0.02 \approx \300 million) stuck in litigation. The estimated effects on district-level growth in lending and firm-level working capital expansion is consistent with the extent of recoveries from case settlement in courts. 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 expands the literature on courts and development (Chemin 2009a,b, 2012; Ponticelli and Alencar 2016; Amirapu 2017; Kondylis and Stein 2018; Mattsson and Mobarak 2023). A key contribution of this paper is highlighting the significant transactional role that ordinary front-line courts play in the efficient functioning of local markets, ranging from credit markets to safeguarding private property. 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; Ponticelli and Alencar 2016). In addition to specific policies, 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 levels, which can potentially generate liquidity implications for corresponding bank branches engaged in debt recovery litigation. 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 front-line 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 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 im-

plementation for strengthening state capacity (Dal Bó et al. 2013; Muralidharan and Sundararaman 2013; Coviello et al. 2015; Khan et al. 2015; Muralidharan et al. 2016; Lewis-Faupel et al. 2016; Neggers 2018; Banerjee et al. 2020; Dasgupta and Kapur 2020; Ganimian et al. 2021; Fenizia 2022; Narasimhan and Weaver 2023; Mattsson and Mobarak 2023) by studying staffing constraints in courts. An important contribution to this literature is the benefit-cost ratio I estimate using direct measures of economic outcomes including wage bill and firm profitability, which imply large returns to local judicial capacity.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 economic framework. 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 cases begin, report to state-level High Courts, which are overseen by the Supreme Court of India. High Courts and the Supreme Court are appellate courts, with the exception of constitutional disputes or disputes concerning interstate commerce. In this paper, I examine the functioning of district-level general courts of law, which are often the first interface of the judicial system. Specifically, I study the District and Sessions Court, hereinafter called district court, which are similar to county courts in many common law countries. These are courts of first instance for many types of legal disputes, across civil (for e.g., property or debt-related disputes), criminal (ranging from violent crimes to lower-order property and financial crimes), and commercial (for e.g., enforcing regulatory laws, contractual disputes) issues. There is one district court per administrative district, which also correspond to the geographic location of the dispute.

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.

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.

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.

The total number of judge posts in a district is determined jointly by the respective state high court and the state-level executive (through budget allocation). There is no clear rule on how the number of judge posts is determined. Periodic reports by the Law Commission of India, an executive body under the central government Ministry of Law and Justice (particularly, the Law Commission Report No.245), point out that this is relatively ad hoc without any specific calculus. Typically, the numbers are determined at the time of district formation and depend on the district population count from the most recent decadal census. These numbers are rarely updated over a shorter time scale, including the scale of the study time period. [Figure A.1](#) (Panel A) shows a strong, albeit imperfect correlation between district population and the number of judge posts.⁸

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

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

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

⁸The 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](#)).

April-May, so that the allocation orders are sent before the courts open after their annual summer recess.⁹ This process is relatively similar across all states in India.

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

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. A large majority of cases listed in district courts are summons requiring the appearance of the defaulting party in response to complaint filed by the aggrieved (see [Figure 1](#)).

Banks mainly 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. Not only was this confirmed during qualitative interviews with a sample of bank managers and their legal counsels but literature has also documented this to be a standard practice in the banking industry worldwide (for example, [Nguyen 2019](#) describes a similar lending system in the US). This co-location requirement with the borrower is important in the context of this paper irrespective of whether the borrower is a firm or a private individual. For enterprise borrowers, this coincides with their registered office, whereas in the case of individuals, this corresponds to their verifiable residential location. Cross-district borrowing relationships are not common, and plausibly does not occur at all, suggesting deeper frictions in the financial markets. This makes the local contract enforcement environment critical for credit markets to function efficiently.

Each bank 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. 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 following court's execution orders are considered income, and serve as positive liquidity shock to the branch.

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). As a result, the bulk of the lending portfolio are loans towards agriculture as well as consumption of individuals and households (called personal loans).¹⁰ Income shocks to individuals lead to defaults, particularly among unsecured loans such as

⁹Courts are closed for hearings and active business for a few weeks in May-June every year.

¹⁰Calculated 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.

credit card payments or policy-directed lending (Giné and Kanz 2017). 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 at the branch and district-level. Enabling settlements in such default cases is particularly important for the health of local branch balance sheet.

To highlight the magnitude of financial transactions stuck under disputes, data on pre-trial settlement for debt recovery cases indicate a recovery amounting to USD 240,000 per district per mediation session. This translates to USD 1-1.5 million over 4-6 such sessions held over a year. Assuming a similar magnitude of recovery for full trials, these amounts can potentially translate to a large, positive liquidity shock at the local level.¹¹

This context on banking reveals two important facts relevant for this paper: (a) common debt defaults create a negative effect on branch balance-sheet, and (b) resolution of debt recovery cases in district courts generates positive balance sheet effects. While settlement results in immediate balance sheet effect from recovered capital, delays in case resolution may not immediately affect the balance sheet. Any subsequent defaults may not immediately trigger a write-off as banks follow specific procedure for recognizing a loan as a non-performing asset, which induces natural lags in the write-off and 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 criminal disputes as well (Figure 1). Thus, capacity of these courts are also important for containing crime, which in turn could affect economic productivity in the area. While police could play a more direct role in containing violent crimes, the bulk of crimes are typically non-violent, and concern property crimes such as thefts, where the functioning of courts could be more important through fines and recovery of property. Relatedly, a large bulk of criminal cases in district courts are what are known as “summary trial” cases. A few examples of these according to the Code of Criminal Procedure are (a) “Offense of theft, under section 379, section 380 or section 381 of the Indian Penal Code, 1860, where the value of the property that has been stolen does not exceed two thousand rupees.”, (b) “Offenses relating to receiving or retaining stolen property, under section 411 of the Indian Penal Code, 1860, where the value of the property does not exceed two thousand rupees.”, and (c) “Offenses relating to assisting in the concealment or disposal of a stolen property, under section 414 of the Indian Penal Code, 1860, where the value of such property does not exceed two thousand rupees.” The monetary value may be updated from time to time through amendments to procedural law, but the main import is that a large bulk of criminal cases pending in district courts pertain to protection of property from thefts and embezzlement. This likely has implications on monitoring costs for local firms in securing and protecting their property, particularly movable property like raw material and inventory that could be pilfered.

¹¹Data on recovery accessed from <https://nalsa.gov.in/statistics>.

3 Data and Sample Construction

In this section, I detail all the sources of data and discuss the construction of the study sample and variables used in the econometric analysis.

3A Court-level Variables: Explanatory Variables

I assemble 6 million public legal case records from the E-Courts database ([Figure A.3](#)), spanning the universe of all legal cases filed or pending for resolution between 2010 and 2018, from a sample of 195 district courts across 15 states in India ([Figure A.4](#)). These districts were selected to ensure an overlap with the location of registered formal sector firms across industrial districts and are representative of other similar districts. Each record details the case meta-data including detailed timestamp information over the case lifecycle.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 the universe of legal cases between 2010 and 2018, I enumerate judges within a court over the study period based on annual workflow observed for a given courtroom generated from the 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. A 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 remaining judges in the court.¹⁴

Following this process, I generate the number of judges in a district court for each year in the study period. I also calculate vacancy rate as the relative shortfall in the number of judges in a given calendar year relative to the maximum number of observed judges in the court within the study period. This generates a similar aggregate measure of district court vacancy rates at the state-level as reported in the official statistics (see [Figure A.4](#)). However, this method assumes that the maximum number of judges is indeed the total number of judge posts, and is agnostic to long-run vacancies or any changes in the number of sanctioned posts over time. To be conservative,

¹²E-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.

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

¹⁴While I also expand the workflow definition to include case resolution, outcome of a hearing, and passing interim orders as a robustness check, using these aren'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.

I restrict all the analyses using annual changes in the number of judges rather than changes in vacancy rates, which requires these additional assumptions.

In the absence of judge-level demographic and tenure data, this construction contributes an important measure of local judicial staffing level 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 case database with the latest status. This follows the main e-governance objective of the Supreme Court of India to reform data capture workflow.¹⁵

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: I construct court-level annual performance measures also using the timestamps from individual case records. I define and 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).¹⁶

3B Firm-level Outcomes

Data source: I use CMIE-Prowess dataset that includes annual balance sheet data of the universe of listed firms and a representative sample of unlisted but registered formal sector firms to measure annual firm-level outcomes. An important feature of this dataset is that it also contains detailed identifying information of firms, including firm name and registered office location, which is useful to match with the court-level dataset.

Firm-level variables: To measure productivity improvements at annual periodicity, I examine profit, sales revenue, wage bill, the value of capital goods (plants and machinery), and raw material

¹⁵Data generated thus are less likely to have been doctored between the time of an event (i.e. a case hearing) and digitization since such applications minimize 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.

¹⁶Court workload includes both pending as well as new cases, which is around 20000 cases per district court. Resolved cases also include those that are dismissed without full trial or a final judgement order. Disposal rate is a relevant metric of judicial capacity relative to average or other moments of case duration that necessarily have a selection component in what cases are resolved. Focusing on disposal rate is also important from the point of view of the volume of tied-up factors of production. While case duration may matter for individual litigant directly involved with the judicial system, annual performance indicators such as the disposal rate measures the extent of congestion and is more appropriate metric of institutional capacity.

expenditures as key outcomes. To examine one of the main mechanisms on improved credit access, I examine yearly working capital and interest expenditure across all borrowings.

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 $\approx 40\%$ of sales, 60% of VAT, and 87% of exports ([Economic Survey, 2018](#)), and therefore captures a large share of value addition in the economy. Second, these firms report production outcomes every year in the form of balance sheet statements, which is useful given the time-scale of the identifying variation.¹⁷

Of the 49202 firms, spanning multiple sectors, within the CMIE databased as of 2018, I could match 9032 non-financial sector firms with 157 of the 195 sample court districts. Remaining 38 district courts result in no match. These form the main population of interest for this study. These firms were incorporated in different years and many do not report data on all key variables every year within the study timeframe. As a result, I generate a balanced panel sample of incumbent firms. The population-level data provides measures of the total number of registered firms as well as the number of new incorporations per district 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 time.

Firm sample construction for firm-level analysis: To estimate effects on firm productivity, I restrict the sample to incumbent non-financial firms, incorporated before 2010. Since many firms have missing balance sheet information for multiple years in the study period, I create a balanced panel of such firms with no missing data. This offers two important advantages: (a) ensuring internal validity if missing-ness of data is non-random, and (b) accounting 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 larger, unbalanced panel of firms as well.

3C District-Level Outcomes

Banking data: Ideally, one would need bank-branch level data on defaults, exposure to litigation, and subsequent recoveries to test whether resolution of disputes in district courts generate credit market implications. In the absence of branch-level data, I examine total legal case resolutions and the total lending (number of loans) to industrial borrowers at the district-level. This aggregation clubs information from the local branches of all commercial banks, which is public data maintained by the RBI. This is also the lowest-level of disaggregation available publicly for research use.

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

¹⁷A 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.

small-valued thefts that are typically tried “summarily” by courts (as described earlier).

Nightlights data: To examine 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 2010-2018, courts experience 1.62 positive staffing changes with 2 judges added on average and 3.6 negative staffing changes with 3 judges removed on average. Of 195 courts in the sample, 158 experience at least one positive event whereas 37 courts experience no net judge addition over the study duration. On the other hand, every court experiences at least one negative event during the study period.

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

Panels B and C describe district and local firm-level outcomes. On average, banks issue over 9000 loans per year to the industrial sector, with about USD 4.2 million (INR 310 million) in circulation (outstanding amount) within the sample districts. The sample of firms include large firms, with USD 103 million (INR 8.4 billion) in average sales revenue and USD 4.5 million (INR 371 million) in average profits. All financial variables are reported in million Indian Rupees and are adjusted for inflation using Consumer Price Index (base year = 2015).

4 Research Design and Empirical Strategy

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

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

4A Stacked Difference in Differences Event Study

With a one time, albeit staggered, change in district 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 annual event-time bins and an effect window of 4 years in lead and lag. I bin the end points by clubbing all the years in the dataset outside this effect window. Binning of the endpoints accounts for any plausible effects outside the effect window selected, thus capturing any long-run effects of staffing-level changes. I append all such district-by-event datasets to generate a stacked dataset for analysis, with each event indexed by an event number (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).¹⁸ Further, to distinguish a positive staffing-level change from a negative change, I create respective binary variables - Pos_{de} and Neg_{de} - and interact these with the event time bins in the following specification:

$$y_{it} = \sum_{j=-4-, j \neq -1}^{4+} \beta_j^+ \mathbb{1}\{|t - T_{d,e}| = j\} \times Pos_{d,e} + \sum_{j=-4-, j \neq -1}^{4+} \beta_j^- \mathbb{1}\{|t - T_{d,e}| = j\} \times Neg_{d,e} + \alpha_i + \alpha_e + \alpha_{st} + \epsilon_{it} \quad (1)$$

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

The treated groups are courts with a net positive or a net negative change occurring in a specific calendar year (for e.g., a change occurring in calendar year $T_{d,e} = 2013$) relative to the previous year. The control group is the set of districts that don't experience any positive or negative change in the same year but could in the future. Since there are multiple events, the control group also includes

¹⁸Event 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.

the same district experiencing another positive and/or negative change in the future. 37 districts never experience positive staffing-level change (never-treated for net addition) whereas every district experiences a negative change at least once within the study period. The coefficients of interest are $\beta_{j \geq 0}^+, \beta_{j \geq 0}^-$ - coefficients on the event-time bins interacted with the positive or negative change dummies, normalized relative to $t = -1$ (the year prior to the corresponding event), representing the dynamic treatment effect of judge staffing changes. $\beta_{j < 0}^+, \beta_{j < 0}^-$, i.e. the coefficients on the interacted terms during the pre-period enable testing for any significant pre-trends. For inference, I use two-way cluster robust standard errors for estimated event-time coefficients, clustering by both district and event (Bertrand et al. 2004, Abadie et al. 2017).¹⁹

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

Causal identification with this strategy requires the following assumptions: (a) exogeneity of timing, and (b) parallel trends, as the estimator accounts for heterogeneous as well as dynamic treatment effects. As discussed in [Section 2](#), the interplay between three different personnel policies (recruitment, retirements, and reassessments) concerning judges in district courts could have different consequences on the judge staffing levels at any point in time, generating plausible exogeneity in the timings of these staffing changes. 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 than their turnover from retirement or reassignment away, then the court would experience a positive staffing change. Finally, it is also possible that these forces cancel each other, resulting in no net change to the court staffing levels.

As empirical support to the two causal identification assumptions, I carry out prediction exercises as well as check for differential trends in the prior period. The specific empirical tests include: (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) test if the timings are correlated with government or private sector investments and budgetary allocations in the district, (c) testing for pre-trends in the event study analysis, (d) testing for a lack of effect outside the jurisdiction of the sample courts in the spirit of placebo tests, and (e) dropping large, metropolitan

¹⁹For robustness, I also cluster by state and event in order to account for any spatial correlation between districts arising from state-level policies.

districts and industrial states to check if results are being driven by outliers. I describe the results from (a) and (b) in detail later in this section, and discuss (c) - (e) in conjunction with empirical results in [Section 5](#).

4B Complementary Empirical Strategy

Two important concerns still remain unaddressed with the above strategy: (a) absence of a never-treated group for negative events, and (b) potential interference between events within the study period. To address the first concern, I bin the end points and normalize the event study coefficients relative to the year prior to the event(s) as suggested by [Schmidheiny and Siegloch \(2020\)](#). The binning also relaxes the assumption of no treatment effects outside the effect window. To address the second concern, I supplement the main empirical strategy with 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. Further, this strategy complements the stacked event study specification by providing a per-judge estimate, which complements the extensive margin effects estimated by the event study specification in [Equation 1](#). This also addresses any concerns stemming from the construction of events in [Equation 1](#), 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 as in [Equation 1](#). x_{it} is the number of judges in district i in year t . y_{it} is the unit-level outcome variable, where i refers to district when outcomes are at the district-level, or a firm when the outcomes are at the firm-level. The specification includes unit fixed effect and state-year fixed effect. I normalize using $t = -1$ such that the coefficients δ_j are relative to δ_{-1} . I chose the maximum possible effect window as estimable using the data, 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 with no changes, parallel trends between courts experiencing different level-changes (for example, courts experiencing net addition of 1 judge is assumed to be trending similarly to those experiencing a net addition of 2 judges in the counterfactual scenario), and 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 Threats to Causal Identification

An ideal research design would require random assignment of judges to courts and subsequently, individual cases to judges. In the absence of this ideal variation, I exploit the next best variation,

which leverages the timing of net changes in the judge staffing levels in a court conditional on unit-level and state-year-level fixed effects. The main threat to causal identification using this strategy is that the timing of these staffing changes could be correlated, or even driven by local economic conditions. For example, if a district is identified as a priority region by the federal or state-level government to attract investments or relegated from such policy targeting due to changing political considerations, then the timing could be endogenous. Alternatively, local business owners could lobby the government to assign more judges to their courts in anticipation of growing their businesses. Further, judges themselves could lobby to be assigned to district courts that are more lucrative. However, these threats are unlikely to bias the causal estimates from this event study design because of the multiple constraints on any one agent - political, bureaucratic, electoral, corporate, or judicial - to effectuate such changes.

Recall that hiring, retirement, and judge assignment decisions are within the control of the state-level judiciary whereas the state-level bureaucrats and elected representatives can only approve total budgetary allowance for staffing requirements across all districts, aggregated, within the state. In order to assess the validity of the research design, I conducted informal interviews with state-level high court committees engaged in personnel policies in 5 different states and asked them their reasons for the persistence of judge-staffing levels and whether the timings in their variation could be plausibly exogenous ([Figure A.5](#)). Almost all stated difficulty in recruiting suitable candidates as the top reason for the inadequate number of judges. According to them, this shortage is because very few qualify for the position due to inadequate experience and/or insufficient knowledge of the law among the many candidates that apply to job openings. On the other hand, graduates of top law schools in India prefer corporate, high-paying, private sector jobs. This lack of suitable candidates is further exacerbated by the fact that many of the district court locations are remote and rural, and the few candidates selected refuse their assignments to such courts even after incorporating their preferences. However, these preferences are not dynamic and have been a persistent concern among judicial administrators over many years. For example, if a candidate refuses their assignment to district A because it is remote and rural, then the propensity to refuse among such candidates rarely changes, at least over the time period of this study and at the level of identifying variation central to the research design. Adding to this is insufficient funds and administrative staff at the state-level coupled with staffing shortages that grow over time as more judges retire following seniority. Finally, political or corporate interference in district judge assignment or staffing levels were not stated among important factors at all.

To verify these stated claims by those within the judiciary, I carry out two specific empirical exercises in the spirit of balance tests. First, I leverage multiple rounds of population census, economic census, and electoral data in the decade prior to the study period (these variables are only available at a decadal or quinquennial intervals) to test whether any of these could predict which districts are likely to experience judicial staffing-level changes in the future. For this prediction exercise, I employ a long differences specification where I regress long-run changes in judge staffing levels (i.e. between 2010 and 2018) on decadal changes in population, number of establishments,

employment in manufacturing, demographic composition (caste, literacy, and urbanization), and electoral outcomes as important determinants (i.e. as RHS variables). [Table 2](#) presents the results. To aid easier interpretation of the coefficients, all dependent and independent variables are transformed into % changes relative to their baseline values (i.e., relative to the earliest period of data availability). None of the individual coefficients are statistically significant nor do they jointly do well in predicting which districts are likely to experience staffing changes.

Second, I check whether the timing of staffing-level changes coincide with concurrent or past investments by the government and the private sector (for example, for infrastructure and/or new greenfield projects) in the sample districts. Any significant prior-period or immediate correlation could both violate the exogeneity assumption for identification and also affect inference whether the estimated treatment effect is truly due to staffing shocks or other concurrent changes. I show that this is not an important concern in [Figure A.8](#), where I find that there are no significant pre-trends in either government or large private sector investments relative to the timing of staffing level change. If anything, the results suggest that the investments could increase much later in the long run as a consequence of improved capacity in front-line courts.

Lastly, a key advantage of both the event study specifications in [Section 4](#) is the visual representation of the differential trends in the prior period. While a lack of pre-period trends is only a necessary but not a sufficient condition for establishing the validity of the research design, combined with the above-mentioned empirical tests and a variety of falsification tests that I discuss later in [Section 5](#), it serves to support plausible exogeneity in the timing of the staffing-level changes.

5 Reduced Form Effects of Judicial Staffing-Level Changes

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

5A Judge Headcount and Vacancy Rate

Panels A and B [Figure 2](#) 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 staffing levels. The coefficients indicate economically meaningful persistence where the staffing levels continue to be higher (or lower) by around 10 (5) percent 3-4 years following the events, albeit with a gradual decay given the frequency of turnovers. The asymmetry between positive and negative changes is consistent with a context where recruitment drives are often 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 levels 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.9](#)).

5B Court Performance

Panel C [Figure 2](#) 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 cases in a context where the average annual judge-level workload is ≈ 2000 cases.²⁰ 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.9](#)).

The lack of a significant negative result following negative changes could be driven by the fact that fewer number of judges turnover relative to those added and that existing workload is shared among other judges in the court. 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,

²⁰I also confirm these numbers by estimating the specification using number of resolved cases 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.

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 backlog at the court-level (Column 6 [Table A.4](#)).

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

5C Robustness: First Stage

One would be concerned if any mechanical correlations between coding of the staffing change events and the disposal rate could drive the estimated effects. Note that the staffing-level change is only measured using the number of judges constructed using filing of new litigation, and thus should have little mechanical correlation with case resolutions or backlog from past years. Additionally, I estimate the effects of judicial staffing changes on court performance using [Equation 2](#), which does not rely on event construction and uses leads and lags of continuous valued changes in the number of judges as key explanatory variables. [Figure A.10](#) presents the results from this specification in a graphical format. I find that existing workload and courts performance are not correlated with the current or future judge staffing-level changes, further supporting the parallel trends and exogeneity of timing assumptions. Also assuring is that the demand for litigation (number of new filings) does not change significantly to these staffing-level changes. However, current and past changes in staffing-levels are only consequential for current and future disposal rates, which are qualitatively similar to the results from the event study design. These findings are also supported by local projection DiD estimation based on a sequence of first difference regression specifications following [Dube et al. \(2022\)](#) reported in [Figure A.11](#).

5D Local Firms' Production

To examine the downstream economic implications of local judicial staffing-level changes, I start with the reduced form effects on the productivity of a balanced panel of incumbent, formal sector firms located within the jurisdiction of the sample courts.

[Figure 3](#) and [Figure 4](#) depict the event study graphs for sales, wage bill, profit, capital, and raw material expenditures following a net increase and a net decrease in the number of judges, respectively. Three key features of these graphs are: (a) a gradual increase (or decrease) in the outcome following staffing change, (b) effects visible in the long-term, and (c) statistically and economically insignificant prior period estimates. The gradual and long-run nature are consistent with the fact that these firms represent an average, formal sector firm in the district, and not just those with legal cases in the court. These effects take time to appear as they are channeled through market mechanisms. This also suggests that the effects are unlikely due to specific legal cases being resolved in these courts and more indicative of improvements in institutional quality.

[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% ($p = 0.037$ in the long run but $p = 0.93$ immediately, and $p = 0.095$ 3 years from the staffing change) and 2% ($p < 0.001$ in the long run but $p = 0.001$ immediately, and $p = 0.016$ 3 years from the staffing change), respectively, over the long run following net staffing-level increases. The effect on profit is 40% over the period ($p < 0.001$ in the long run but $p = 0.26$ immediately and $p = 0.002$ 3 years from the staffing change). The effects on capital goods, i.e., the value of plant and machinery, are not statistically significant even though the point estimates are large and in the same direction as wage bill or sales. Lastly, expenditures on raw material used for production also increases, with a persistence over the long run ($p < 0.001$).

Since the sample firms are large in terms of 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 accounting expenses.

The effects of negative staffing changes 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 as well as expenditure on raw material also decrease but the point estimates are imprecise. Normalizing effects per judge suggest that the changes in productivity outcomes are symmetric with respect to staffing variations.

5E Robustness: Firm-level Outcomes

A key concern is whether the above results reflect biased estimates due to firm sample construction to create a balanced panel. That is, if the outcomes of the analysis sample are correlated with the changing composition of all firms in the district (particularly those that are not in the balanced panel due to missing data) - for example, if there are greater firm exits that reduces competition environment for the sample firms, the productivity effect could be overestimated. So, even if the composition of the sample firms 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-level changes on new firm incorporations (firm entry) and total number of firms in the district. This itself could indicate a more broad-based impact of judicial capacity, answering (b) above, and the direction of

effects would help shed light on (a) and (c). I find increased firm entry and fewer net exits (as the total number of firms in a district also marginally increase) around positive staffing events. With increased competition, the results are more likely downward biased, providing a lower bound of the true effect. 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-level changes. I find a decrease in the extent the missing data consequent to improved judicial staffing and greater missing entries following net decreases (but importantly, with no pre-trends; see [Table A.10](#) 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 further supports plausible increased competition and downward bias in the estimated productivity 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.

A second concern is whether the effects are mechanical due to litigating nature of firms. If this is the case, then the effects could be due to gains from resolution of ongoing litigation in courts. There are two reasons that suggest that the effects are much broader, reflecting the role of courts in facilitating local economic transactions: (a) the main sample of incumbent, non-financial firms are not litigation intensive but financial sector firms such as banks are, which I study separately, and (b) the effects persist even among firms with no legal case data in the sample courts over the entire study period (see [Table A.12](#) and [Table A.13](#)).

A third concern is about more general spurious correlation, such as those arising from concurrent local macro-economic shocks not captured in state-year fixed effects. To address this, I check whether the effects are restricted to firms within a court's jurisdiction and not experienced among similar firms in the neighboring districts, which may also experience these unobserved shocks. [Table A.14](#) and [Table A.15](#) document the results, showing that the point estimates are statistically and economically insignificant, suggesting that the estimates reflect the impact of improved judicial capacity within the local courts.

Lastly, the results are also qualitatively similar when using complementary econometric specifications (such as [Equation 2](#) and and local projection DID as in [Dube et al. 2022](#)), suggesting plausible real effects of local courts on economic activities and development outcomes ([Figure A.12](#) and [Figure A.13](#)).²¹

²¹The results are robust to a battery of standard sensitivity tests, particularly: (a) dropping top industrial states, and (b) dropping metropolitan districts. Dropping these specific clusters address two additional concerns: (i) any outliers driving the effects, and (ii) potential sources of endogeneity if judge allocation rules are ignored in these more economically attractive locations. I find that the point estimates become larger and more precise with sales revenue and raw material expenditure (see [Table A.16](#), [Table A.17](#), [Table A.18](#), and [Table A.19](#), respectively). Further, 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 effects on profits are still significant at 5% in the year(s) following the events (see [Table A.20](#) and [Table A.21](#)).

5F Towards Broad-Based Impact

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

On firm entry, I note a significant increase in new incorporations of formal sector firms with little evidence of increased exits (Cols 1-2 [Table 3](#)) following net judge increase as discussed earlier. On the other hand, a net decrease has minimal effect on these extensive margin changes.

On GDP growth, I find suggestive evidence of increase in nightlight intensity following positive staffing-level changes (intensity increases by about 6%) and a decrease (by about 3%) following negative changes (Cols 3 and 6 [Table 3](#)). In the absence of accurate district-level annual GDP data for the period of analysis, I rely on the recent nighttime light data source from VIIRS satellite imagery as a proxy for overall district GDP growth. Albeit noisy ($p = 0.315$ in the long run), this analysis complements the results from the formal sector analysis under the assumption that the nightlight data would capture informal and household sector outcomes and investments in infrastructure.

6 Mechanisms

How could local courts have such large effects on the local economy and within the timespan of the relatively short-run nature of the identifying variation? The answer to this lies in the fact that these courts facilitate economic transactions in the short run, which could also have implications for long run evolution of trust in markets. While this paper is not poised to disentangle between short run and long run mechanisms behind legal institutions and economic development, it brings attention to the role of courts in day to day economic transactions such as those engaging much of the financial sector.

Using the individual legal case records, I document that: (a) banks are litigation intensive - there are many more cases per bank relative to per capita caseload of any other type of litigants, (b) about 50% of all commercial banks in India have at least one ongoing legal case during the study period in the sample courts, and (c) in 80% of cases involving banks, banks are the initiator of the complaint (appear as a petitioner) (see [Figure 1](#)). 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.²² These suggest that well-functioning judiciary could be important for banks' business and lending workflow.

²²Based 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 the resolution of debt recovery cases in the sample courts (see Panel A [Figure 5](#)). The average disposal rate of bank-related legal cases in the sample court is similar in magnitude to the overall disposal rate, which increases by 2% per year following positive staffing-level changes. The median duration of such cases also decreases when there is no vacancy in a court. Given that the average value of capital stuck in bank-related cases is about \$15000 per case (Panel A [Figure A.15](#)), resolving 22-23 cases of debt recovery could unfreeze capital worth \$330,000, which could increase liquidity at the local bank branches by lowering provisions for write-offs. The banks could recirculate the increased liquidity as additional credit to industrial borrowers.

In the absence of bank branch-level lending data, I examine district-level aggregate lending to industrial borrowers to estimate the effect of judge staffing-level changes on local credit supply. Since bank's lending response to improved judicial capacity depends on the extent of pending cases, I weight the regression specification in [Equation 1](#) by the number of pending cases involving banks at the start of the study period. Panel B [Figure 5](#) presents the event study graphs using total number of bank loans to industrial borrowers in a district as the outcome variable.²³ The figure shows average lending across all banks and lending by private sector banks, which face market incentives in contrast to public sector banks. The main takeaway is that the total lending to industrial borrowers increases between 6-8% over the long run following an increase in the number of judges ($p = 0.07$ in year 3 and $p = 0.11$ year 4 and beyond), with private sector banks playing an important role (private lending increases by over 12% in the long run, $p = 0.016$). This 6% growth in bank lending to the industrial sector, which average at INR 310 million or \$ 3.73 million, translates to \$ 200,000 in additional lending, in-line with the ball-park estimate on capital recovery from litigations.

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

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 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's

²³The credit data also includes total outstanding loan amount at the district level but this includes new loans as well as defaulted loans including NPAs. Thus, it is unclear a-priori how this measure would map to improved lending as an outcome. Unfortunately, there is no NPA or collections data at the district-level.

wealth (to liquidate in the event of default) 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. Evasion leads to default, which initiates debt recovery process including litigation. This recovery process is costly for both lender and borrower, and is a decreasing function of court's effectiveness in contract enforcement. Some borrowers may choose to litigate if their payoff is better with litigation - for example, if litigation enables the borrowers to renegotiate a reduced interest rate or alter other repayment terms. Other borrowers may choose to settle with the lender to avoid litigation. I model this interaction as a lender-borrower game with a sub-game perfect Nash equilibrium that requires a wealth cut-off (or any other proxy for repayment capability) for lending. Comparative statics with respect to exogenous variation in judicial capacity suggests that the lender would lend to smaller borrowers and lower interest rates for all levels of borrowing with better courts.

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

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

6C Firm-level Working Capital and Interest Expenditures

Empirically, I note an increase in firms' working capital and a decrease in interest expenditure following staffing-level changes that persist over the long run (Panel C [Figure 5](#)). Working capital reflects the extent of cash available to meet operating expenses, which I use as a proxy for borrowing in the absence of reliable firm-level borrowing data.²⁴ These immediate effects on working capital and interest expenditure is consistent with the plausible role of liquidity in local credit markets. Working capital increases by 39% ($p < 0.001$) that persists in the long run ($p = 0.021$). Interest expenditures decline by 8% immediately and also persist ($p < 0.001$ over the long run).

The conceptual framework generates additional hypotheses relating to firm-size that can be tested in the data, namely smaller firms benefit from improved judicial capacity, and witness an increase in credit access and productivity effects. I use firm-level data on total asset value as well

²⁴Borrowing 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.

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 examine these additional hypotheses. Specifically, I estimate the event study specification in [Equation 1](#) among subsamples of firms that are small and have below-median leverage. [Figure 6](#) shows that smaller firms are more likely to appear as defendants in legal cases when there are more judges. These firms also experience greater working capital infusion, face lower interest expenditure, and record higher profits.

Lastly, these effects on credit behavior are not symmetric with respect to changes in the judge staffing levels. This lack of a symmetric negative effect is plausible in the presence of natural lags in recognizing defaults and filing of debt recovery litigation in courts as discussed earlier in [Section 2](#). So, even if borrower defaults go up following negative staffing-level changes, I do not note its effect on court-level outcomes (see the lack of an effect on filing rates in [Figure A.10](#)) or district-level lending outcomes (Col 3-4 in [Table A.22](#)) within the time-period of this study.

6D Decomposition of Firms' Profit and Other Plausible Mechanisms

How important is access to credit as a mechanism to explain the observed reduced form effects of improved judicial capacity on local firms' profitability? Since courts enforce not just contracts but also property rights and law and order by adjudicating criminal complaints, an improvement in judicial capacity could also translate into local development outcomes through these channels. Indeed, these channels could certainly be at play. For example, I find that judicial staffing-level changes also have positive implications for containing both serious/violent crimes (homicides, and those causing bodily injuries) as well as minor property crimes (thefts). [Figure 7](#) shows reductions in both types of crimes following a net increase in the number of judges. With the caveat that I am unable to distinguish whether these changes are due to reporting or true occurrence of crimes, these results suggest that local courts plausibly also improve security of persons and property. The effects on firm-level production outcomes, particularly the results on raw material expenditures discussed in [Section 5D](#), are also consistent an expansion in the scale of operations from lower monitoring costs.

However, would these translate into the observed growth of local formal sector firms, which are large and main contributors to value addition in the economy? For example, if such large firms already invested in private security to safeguard their property, then the crime or property rights channel may not translate to huge effects on their profits. To assess this, 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 to comment on the relative importance of each of the channels. I include lagged values of firm profits, firm fixed effects, and flexible controls for district-year and industry-year interactions to account for time varying unobserved drivers of firm profits. [Table 4](#) presents suggestive evidence on the importance of the credit channel: interest expenditure has a large negative correlation with respect to profit, whereas both working capital and sales have significant large positive correlations. The interest expenditure elasticity is equal in magnitude to the sales elasticity, both indicating a 1:1 correspondence with profit. On the

other hand, crime variables do not have substantial explanatory power in predicting the profits of these firms. This suggests that credit channel has a plausibly more immediate effect on economic enterprises and is first order compared to the many other ways through which well-functioning courts can affect local economic development.

7 Benefits and Costs of Reducing Judge Vacancy

This paper suggests that investing in improving judicial staffing in front-line, district courts is important for local firm productivity and subsequently, overall economic development. Leveraging the fact that the firms in the study sample are tax-paying firms and employ labor with taxable income, this investment could generate large returns, both from the perspective of public budget surplus as well as increases in social returns. In [Table 5](#), I present data, computations, and assumptions to generate a back-of-the-envelope benefit-cost ratio from adding one more judge in district courts.

On the benefits side, I use the median values of profits and wage bills among the sample firms to compute increases in firm-level surplus and salaried income. Next, I apply the average corporate and income tax rates on these increases in firm profit and wage bill respectively. 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 accounting for exemptions and tax-slabs specified in the Union Budget, 2018-19.²⁵

On the expenditure side, I calculate the cost of an additional judge using the median proposed salary in the Second National Judicial Pay Commission. I further inflate the salary to account for fringe costs including annual increments, benefits and allowances towards retirement, transport, and housing costs. The actual salaries and benefits would be lower than this figure depending on the extent of adoption of these recommendations by each state.

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

²⁵These assumptions are motivated by articles in the news media, with sources mentioned in [Table 5](#). 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.

8 Conclusion

To conclude, I show that well-functioning front-line judiciary is a core component of state capacity and important for local economic development. The current status-quo underscores a 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. This paper demonstrates that reducing vacancy by adding more judges is a highly cost-effective intervention, which spurs the productivity of local formal sector firms. Adding one more judge increases court-level disposal rate of case backlog by 10 percent. When large amounts of capital are stuck in litigation due to loan defaults, even a marginal increase in judicial capacity frees up a meaningful magnitude of frozen capital. Subsequently, local firms become more productive from increased credit supply, with indications for broad-based economic development outcomes. Importantly, the benefits accrue relatively quickly and potentially within an electoral cycle, making it an attractive investment proposition to improve judicial capacity.

This paper provides one of the first causal evidence on the relationship between judicial staffing levels 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. I leverage new sources of data such as legal case records and contribute a novel, court-level panel data merged with key economic outcomes.

Additionally, this paper highlights the importance of the transactional role that front-line courts play in local credit markets through debt contract enforcement. Recovery of unpaid debt stuck in litigations enables credit circulation in the local economy. This facilitates firm productivity through access to credit from a better functioning local credit market, and lowers 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). Debt recovery through courts is a fundamental instance of contract enforcement, which is more routine and larger in magnitude relative to bankruptcy cases that are often the last step.

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 front-line courts across the world.²⁶ Reducing the extent of such 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 agents interacting with the judicial system on other factors behind case backlogs as more disaggregated data become available for research use.

While this paper does not delve into the subsequent actions of financial institutions in response to changes in judicial capacity on credit misallocation specifically, one could think of capital recovered from the backlog of litigation as reducing misallocation. Further research is needed to examine whether lenders extend credit to firms with higher marginal product of capital or higher TFP and how this interacts with the local judicial capacity. Examining how functioning of district courts

²⁶For 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>.

interact with banks' lending decisions across different borrower types can potentially shed light on important mechanisms behind capital misallocation. Availability of data such as judge biographies, loan-level data, and high frequency data on the productivity of the household informal sector would greatly help answer these follow up questions on the role of courts in finance and development.

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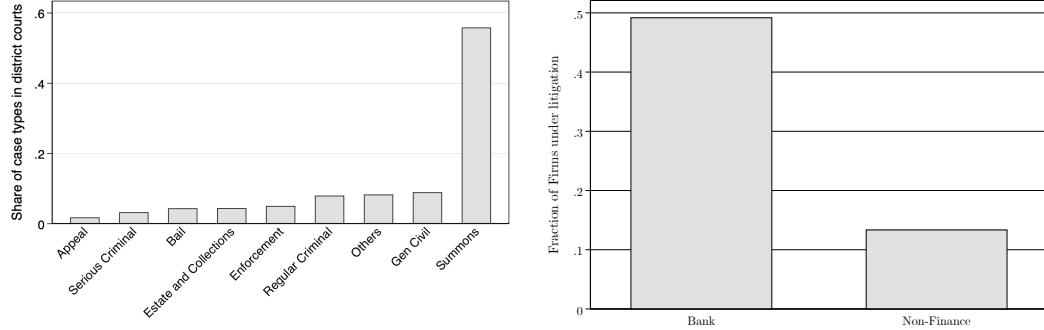
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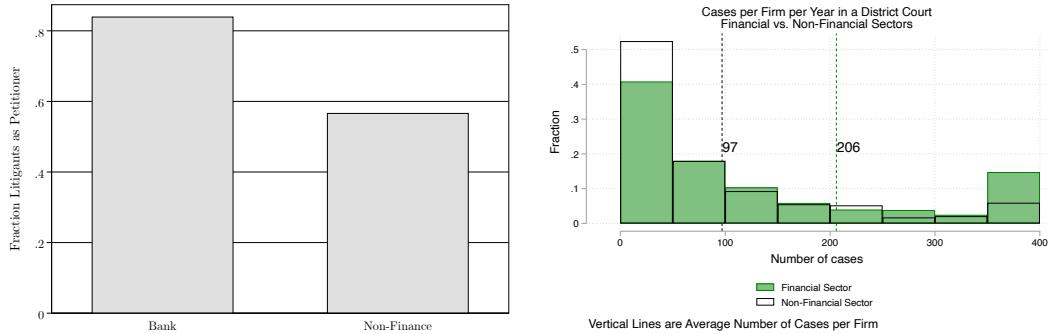
9 Figures

Figure 1: Case-Types in District Courts

Panel A: All cases

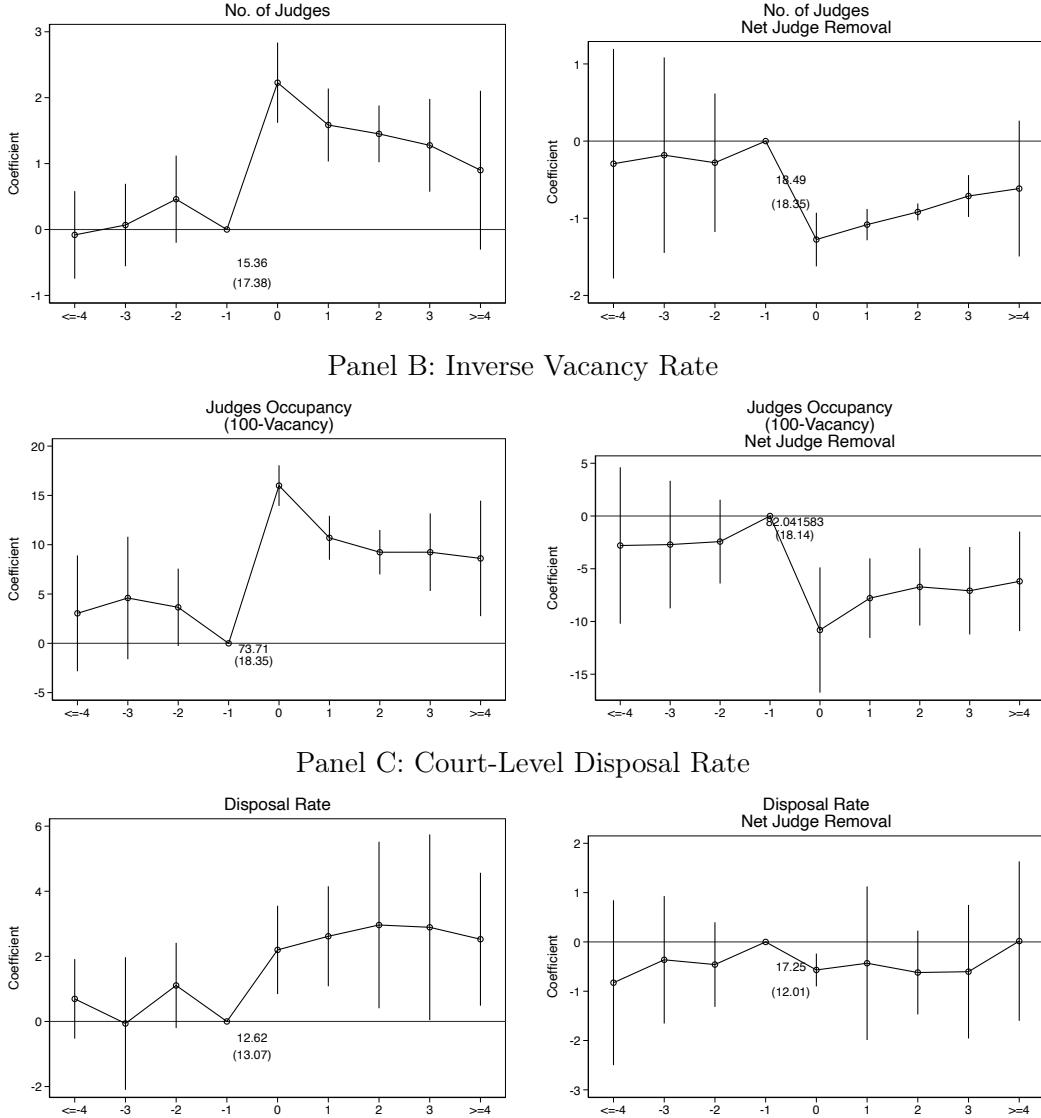


Panel B: Cases with firms as litigants



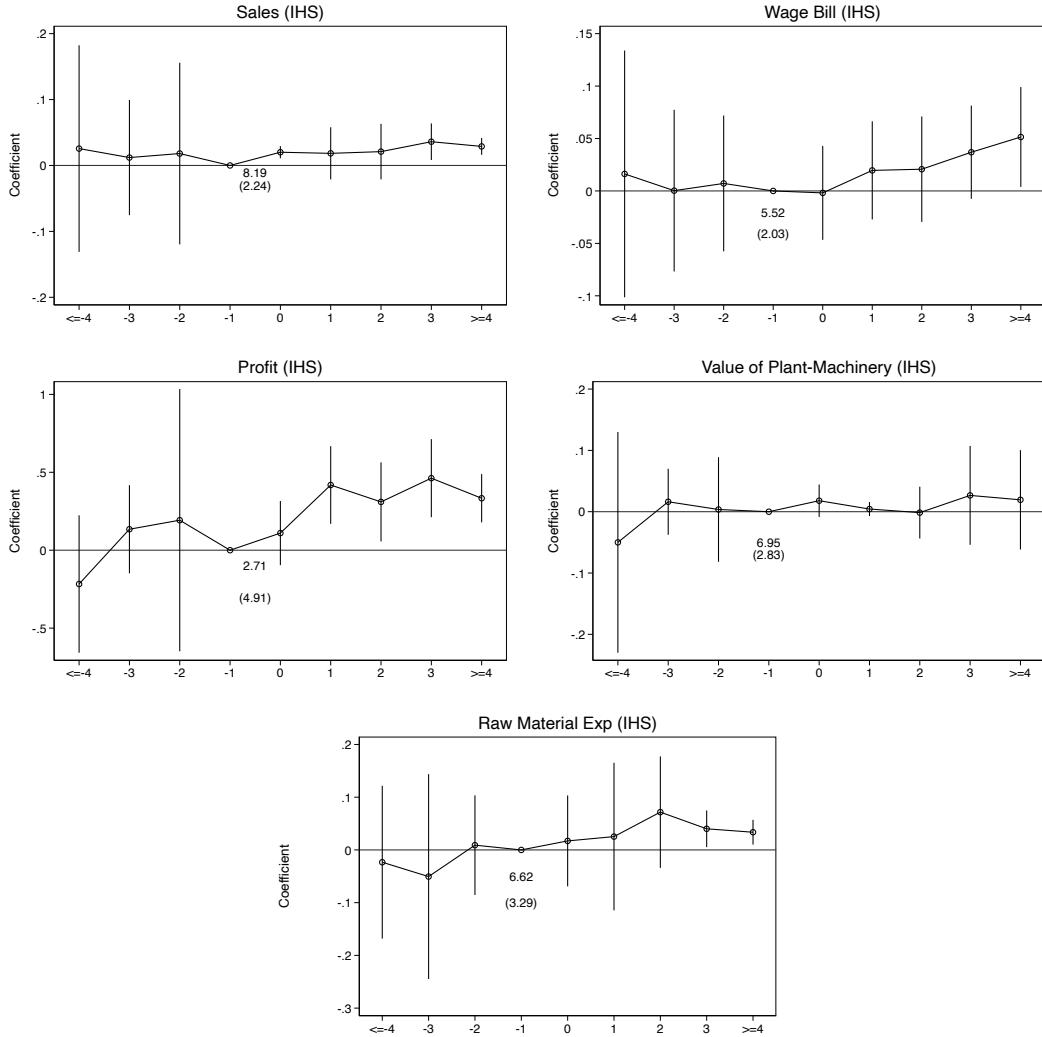
Notes: Panel A illustrates the distribution of legal case types in district courts in a typical industrial state. Left panel categorizes cases by the underlying case-type. A large majority of cases (> 50%) are summon cases, which mean that these are one-sided complaints that require the defendant/respondent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

Figure 2: Net Addition and Removal of Judges and Court Performance
 Panel A: Judge Headcount



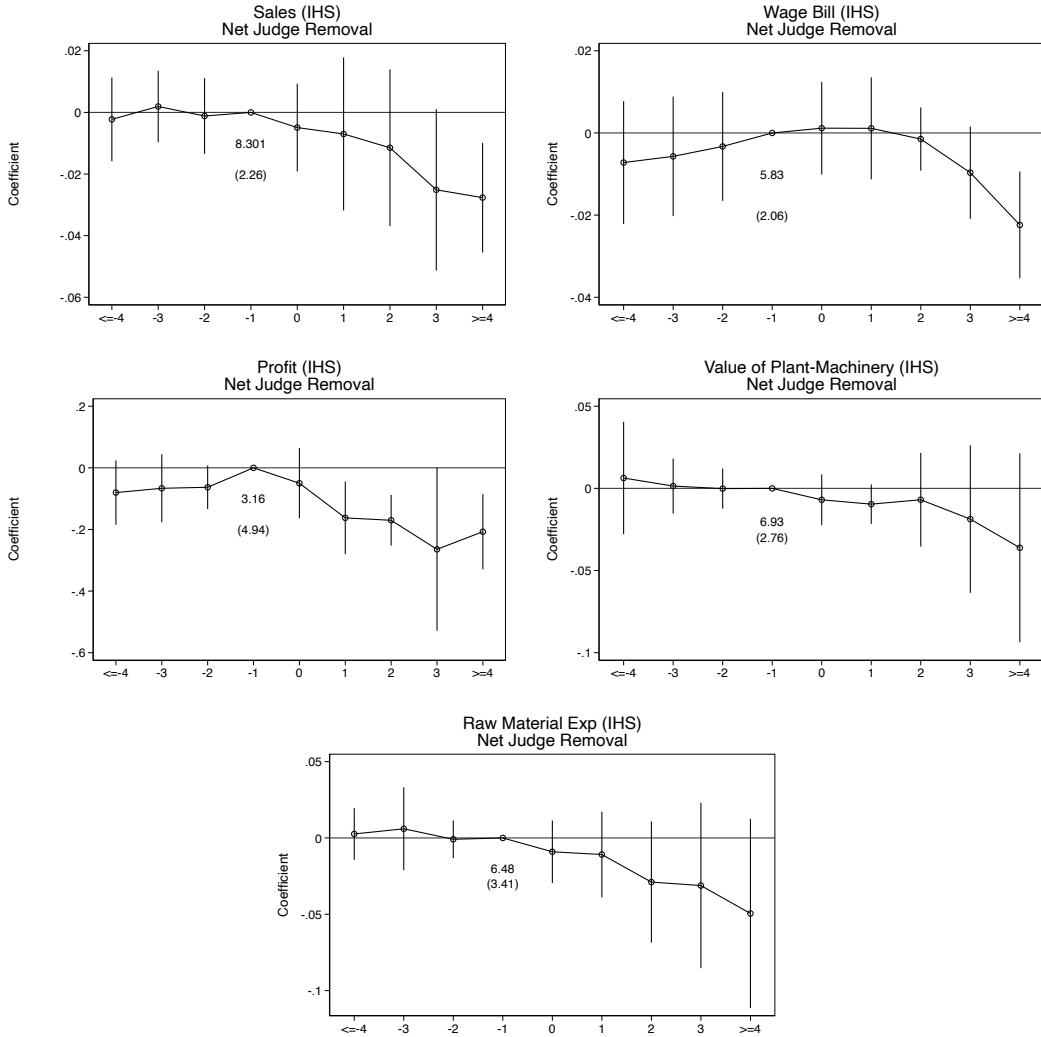
Notes: The figures plot the event study interaction coefficients for positive and negative staffing changes from estimating [Equation 1](#) using total number of judges (Panel A), inverse vacancy rates (Panel B) and disposal rate (expressed in percentage terms in Panel C) as dependent variables, respectively. 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 3: Local Firms' Production: Net Judge Addition



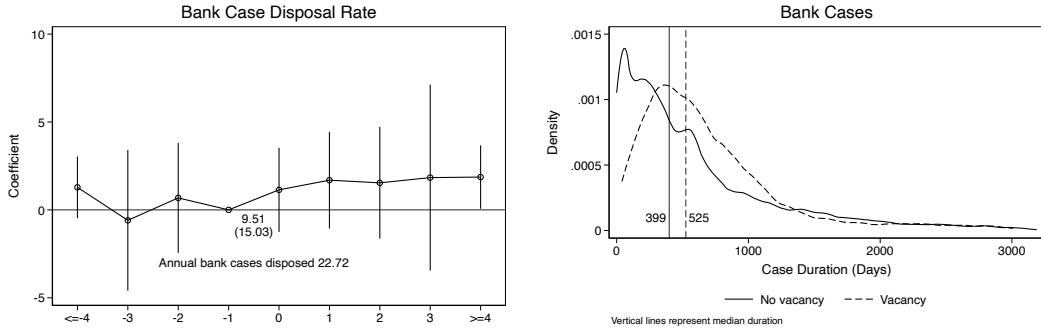
Notes: The figures above plot the event studies coefficients on positive staffing change event-time interaction dummies from estimating [Equation 1](#) for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed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

Figure 4: Local Firms' Production: Net Judge Removal

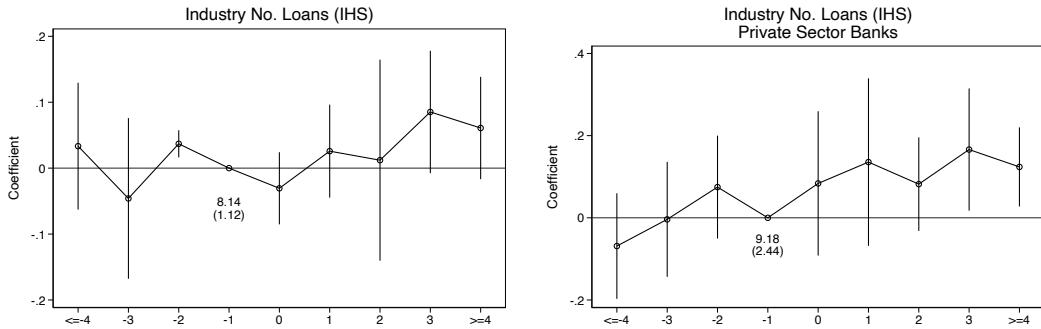


Notes: The figures above plot the event studies coefficients on negative staffing change event-time interaction dummies from estimating [Equation 1](#) for firm-level variables. The outcome variables are transformed using inverse hyperbolic sine function to account for 0s and negative values observed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

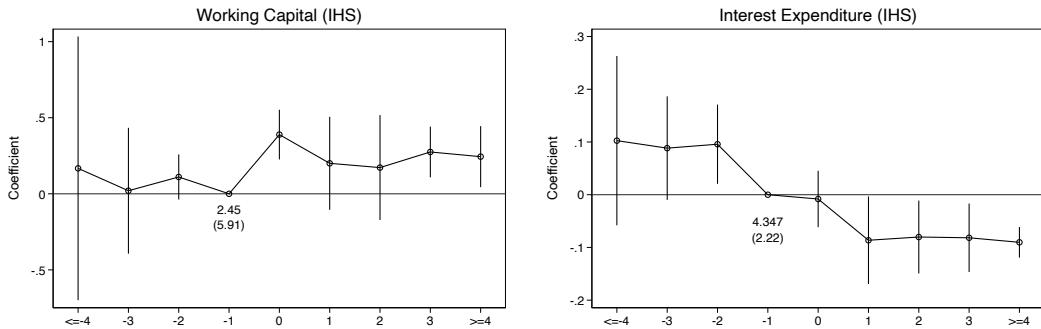
Figure 5: Credit Mechanism
Panel A: Resolution of Banks' Cases in Courts



Panel B: District-Level Lending

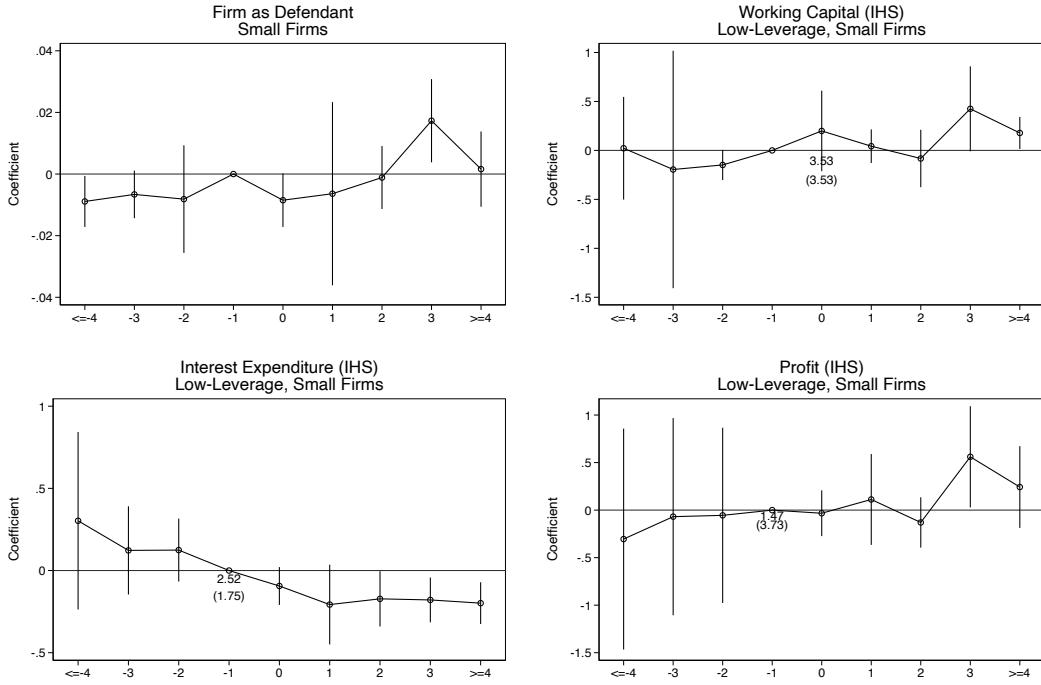


Panel C: Firm-level Working Capital and Interest Expenditure - All Sample Firms



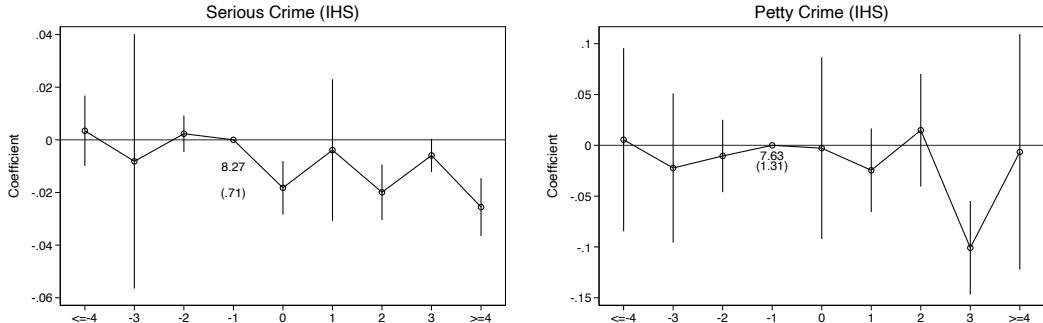
Notes: Panel A presents what happens to banks' legal cases in courts following net judge addition. Left panel shows that the case backlog reduces by 2 percentage points. Right panel shows reduction in median case duration when judge vacancy is resolved. Panel B presents effects of positive staffing-level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

Figure 6: Improved Judicial Capacity and Access to Credit



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

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



Notes: Panel A presents positive staffing effects on overall reported crime outcomes in the district, separated by serious and petty crimes. Panel B documents the effect on raw material expenditure. Error bars present 95% confidence interval. The table equivalent of the firm-level graphs is [Table A.6](#) and [Table A.7](#), respectively (Col 3). The table equivalent of the district-level crime outcome is in [Table A.24](#).

10 Tables

Table 1: Summary Statistics

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

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

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

	(1) Δ Judges	(2) Δ Judges	(3) Δ Vacancy	(4) Δ Vacancy
Δ Pop	-0.597 (0.742)	-0.564 (0.688)	0.387 (0.604)	0.353 (0.578)
Δ # HH	0.349 (0.422)	0.377 (0.523)	-0.282 (0.313)	-0.309 (0.365)
Δ SC Pop	-0.0138 (0.0647)	-0.00937 (0.0759)	-0.00447 (0.0467)	-0.0108 (0.0546)
Δ Lit Pop	0.140 (0.225)	0.0706 (0.140)	-0.0647 (0.190)	0.00732 (0.156)
Δ Urban Pop	-0.0482 (0.0543)	-0.0550 (0.0545)	0.0494 (0.0469)	0.0569 (0.0471)
Δ All Emp	-0.0184 (0.0377)	-0.0203 (0.0363)	0.00872 (0.0299)	0.0108 (0.0285)
Δ Manuf Emp	0.0126 (0.0299)	0.0142 (0.0285)	-0.00562 (0.0240)	-0.00726 (0.0226)
Δ Candidates		0.0176 (0.0182)		-0.0206 (0.0170)
Δ Elec Turnout		0.157 (0.416)		-0.157 (0.324)
Δ Winner Vote Share		0.130 (0.386)		-0.162 (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 (1.009)	-8.789 (7.129)	-0.105 (0.0751)	0.0650 (0.168)	-0.167 (2.483)	0.0315 (0.0322)
Event x -3	-0.212 (0.366)	-4.672 (2.838)	-0.0570 (0.0491)	0.0671 (0.139)	-0.231 (0.599)	0.0201 (0.0213)
Event x -2	-0.168 (0.201)	-1.555 (1.827)	0.00240 (0.00753)	0.144 (0.201)	0.0383 (0.650)	-0.0136 (0.0288)
Event x 0	0.286*** (0.0709)	1.549 (1.659)	0.00893 (0.0165)	-0.0289 (0.0695)	-0.702 (1.145)	-0.00139 (0.0166)
Event x 1	0.286** (0.117)	3.387* (1.875)	0.0234 (0.0275)	-0.0184 (0.0309)	-0.857 (1.438)	-0.0203 (0.0207)
Event x 2	0.520*** (0.0856)	6.808 (4.003)	0.0353 (0.0392)	-0.0840 (0.116)	-2.370 (1.961)	-0.0127 (0.0178)
Event x 3	0.466*** (0.142)	7.635 (4.751)	0.0369 (0.0386)	0.0482 (0.0551)	-1.705 (1.704)	-0.00840 (0.0169)
Event x >=4	0.644*** (0.196)	9.972 (6.544)	0.0584 (0.0559)	-0.0711 (0.0996)	-2.483 (2.944)	-0.0382 (0.0399)
Observations	4806	7497	6993	4806	7497	6993
No. Districts	95	155	192	95	155	192
Control Mean	1.8	22.2	0.96	1.9	48.3	1.55

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

Table 4: Decomposition - Firm Profits

	(1) Profit (IHS)	(2) Profit (IHS)	(3) Profit (IHS)	(4) Profit (IHS)
Sales	1.549*** (0.310)	1.550*** (0.311)	1.289*** (0.255)	1.370*** (0.366)
Working Cap	0.125*** (0.0304)	0.124*** (0.0305)	0.122*** (0.0331)	0.122*** (0.0311)
Interest Exp	-0.860*** (0.207)	-0.861*** (0.208)	-0.779*** (0.203)	-1.029*** (0.209)
Less-Serious Crime	-0.230 (0.149)			
Profit t-1	-0.0139 (0.0224)	-0.0144 (0.0222)	-0.0414* (0.0210)	-0.0753*** (0.0183)
Profit t-2	-0.0313** (0.0125)	-0.0311** (0.0126)	-0.0359*** (0.0118)	-0.0171 (0.0156)
All Crime		-0.514 (0.606)		
Observations	2695	2695	2483	2088
No. Firms	369	369	341	299
Firm FE	X	X	X	X
Unit-Year FE	State-Year	State-Year	District-Year	District-Year, Industry-Year

Standard errors in parentheses

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

Notes: This table presents a firm fixed effect regression of inverse hyperbolic sine-transformed variables - profit (dep var) on sales revenue, working capital, interest expenditure, local crime and lagged profit variables. Following firm-level profit maximizing problem in [Appendix A.3](#), profit should be positively correlated with sales revenue and working capital, 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 other factors, and thus represent an important mechanism through which local courts affect firm productivity.

Table 5: Cost-benefit Calculation

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

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

Appendix

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A.1 Additional details on the context

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

¹As 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.

²Data 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.

³I 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

⁴Note 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.14](#). Litigation is costly and lender incurs a cost, $C_L(\gamma) > 0$, $\frac{\partial C_L}{\partial \gamma} < 0$, as a function of judicial capacity, γ . The borrower can also choose to litigate with costs, $C_B(\gamma) > 0$, $\frac{\partial C_B}{\partial \gamma} < 0$, or settle out of court. Once the lender chooses to litigate and borrower accepts, lender wins with a very high probability. The intuition behind the relationship between 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

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

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

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

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

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

K_B mainly depends on the project and has an ex-ante distribution given by CDF, $G(\cdot)$. 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

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

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

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

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

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

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

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

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

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

1. Effect on W^* is negative. That is, an increase in judicial capacity lowers the threshold of wealth required for lending.
2. Effect on R is negative for each level of borrowing. That is, the interest curve shifts inward.
3. Borrowing becomes cheaper, which expands total borrowing, particularly at lower levels of wealth W .

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

$$\begin{aligned}
\frac{\partial R}{\partial \gamma} &= \frac{\overbrace{\frac{\partial C_L(\gamma)}{\partial \gamma}}^{\text{-ve}} \overbrace{(1 - G(\tilde{W}) - C_B g(\tilde{W}))}^{\text{+ve}}}{((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B} \\
&\quad - \frac{(1 - G(\tilde{W}))C_L(\gamma) - K_B}{(((1 - G(\tilde{W}))\Gamma + G(\tilde{W}))K_B)^2} \left(\overbrace{g(\tilde{W}) \frac{\partial C_B}{\partial \gamma} (K_B - \Gamma)}^{\approx 0} \right) \\
\implies \frac{\partial R}{\partial \gamma} &< 0 \\
\frac{\partial W^*}{\partial \gamma} &= \frac{\overbrace{(1 - F(\tilde{W})) \frac{\partial C_L}{\partial \gamma} - C_L f(\tilde{W}) \frac{\partial C_B}{\partial \gamma}}^{\text{-ve}}}{((1 - F(\tilde{W}))\Gamma + F(\tilde{W}))\delta} - \frac{(1 - F(\tilde{W}))C_L(\gamma) - K_B}{(((1 - F(\tilde{W}))\Gamma + F(\tilde{W}))\delta)^2} \underbrace{f(\tilde{W}) \frac{\partial C_B}{\partial \gamma} (\delta - \Gamma)}_{\approx 0} \\
\implies \frac{\partial W^*}{\partial \gamma} &< 0
\end{aligned}$$

A.3.B. Firm Production

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

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

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

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

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

FOC:

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

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

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

This implies that the optimal input choices increase for all firms with an improvement in contract enforcement through local courts. On the other hand, how the shadow value responds depends on

the functional form of the underlying production function. For example, if the production function is Cobb Douglas, then $\frac{\partial \lambda}{\partial \gamma} = 0$.

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

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

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

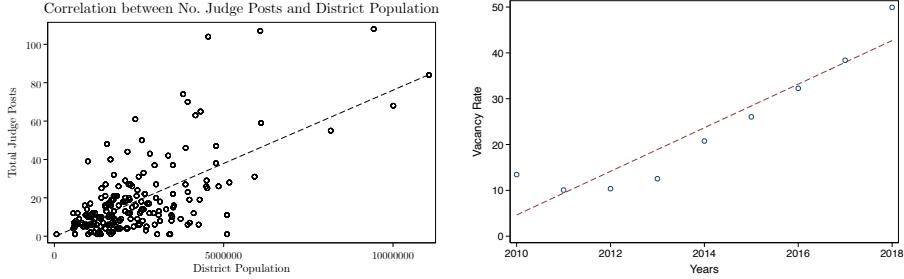
$\frac{\partial g^*}{\partial \gamma} > 0$ if marginal benefits from an improvement in judicial capacity exceeds marginal cost, in which case, welfare improves. If this is not true, then the welfare effect is potentially ambiguous. Heterogeneity based on firm size distribution imply:

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 $W \ll W^*$, neither their optimal inputs nor their profits change since $(\underbrace{\frac{\partial K_S}{\partial \gamma}}_{=0} - \underbrace{\frac{\partial m_S}{\partial \gamma}}_{\approx 0}) \approx 0$.

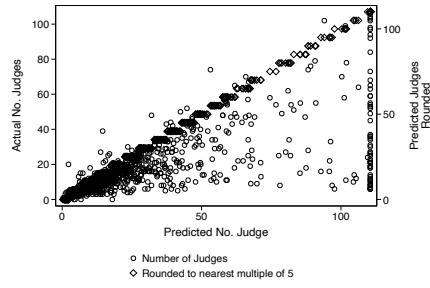
A.4 Appendix: Figures

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

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

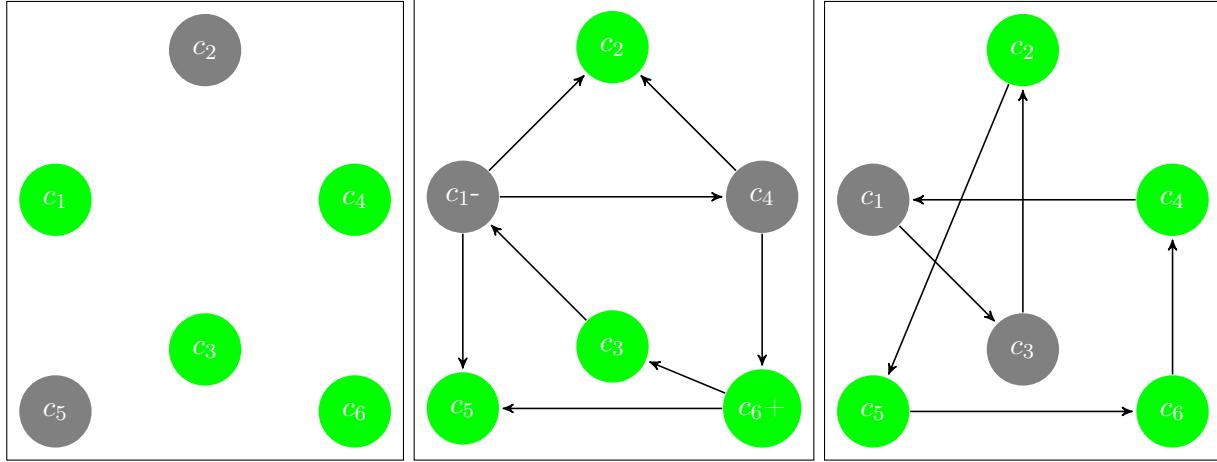


Panel B: Actual Number of Judges vs. Law Commission Recommendation



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

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



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

Figure A.3: Individual Case Record Example

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

[Back](#)

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

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

Petitioner and Advocate	
1)	[Redacted]

Respondent and Advocate	
1)	[Redacted]

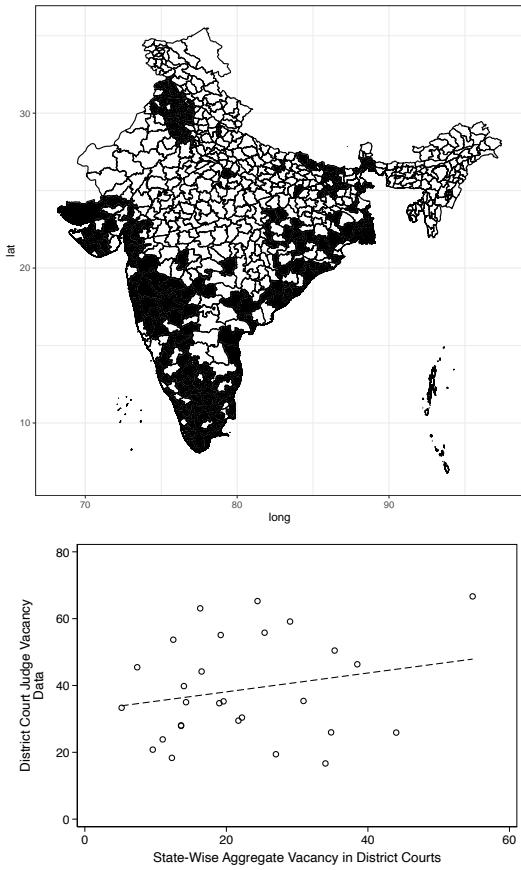
Acts	
Under Act(s)	Under Section(s)
INDIAN PARTNERSHIP ACT	9

Sub Matters	
Case Number :	/102240/2017

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

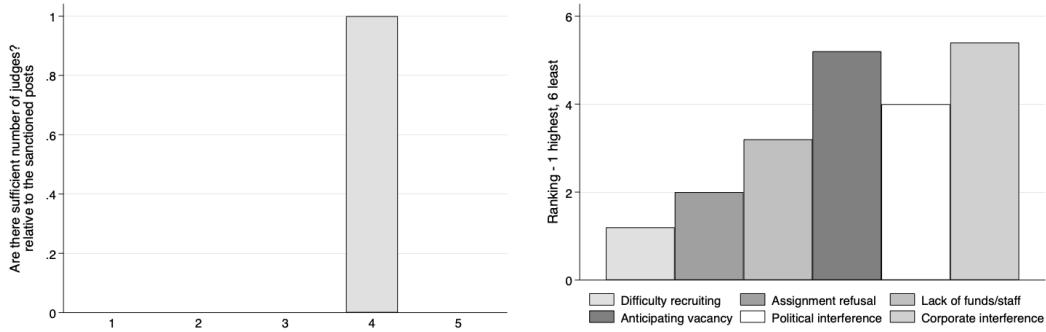
Notes: Names have been redacted for privacy.

Figure A.4: Sample district courts



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

Figure A.5: Stated Reasons for Judge Vacancy



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

Figure A.6: Construction of sample of firms

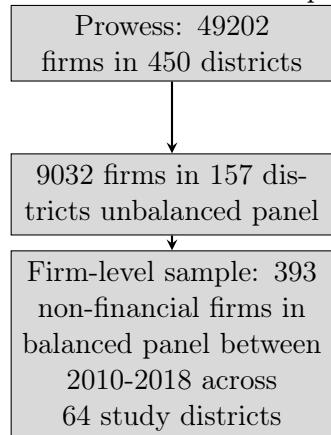
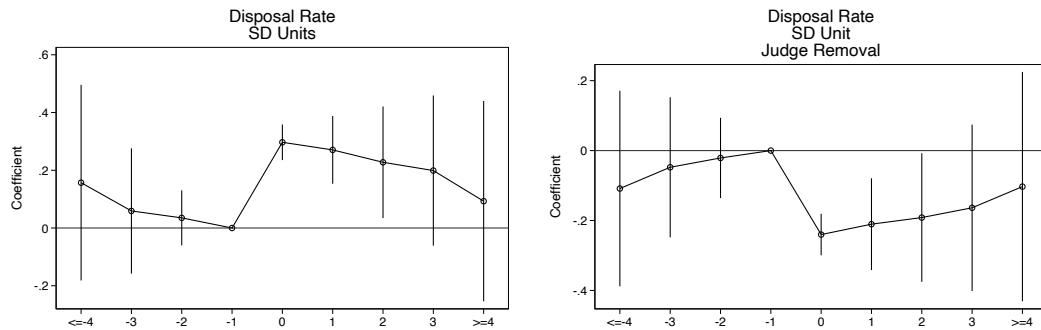
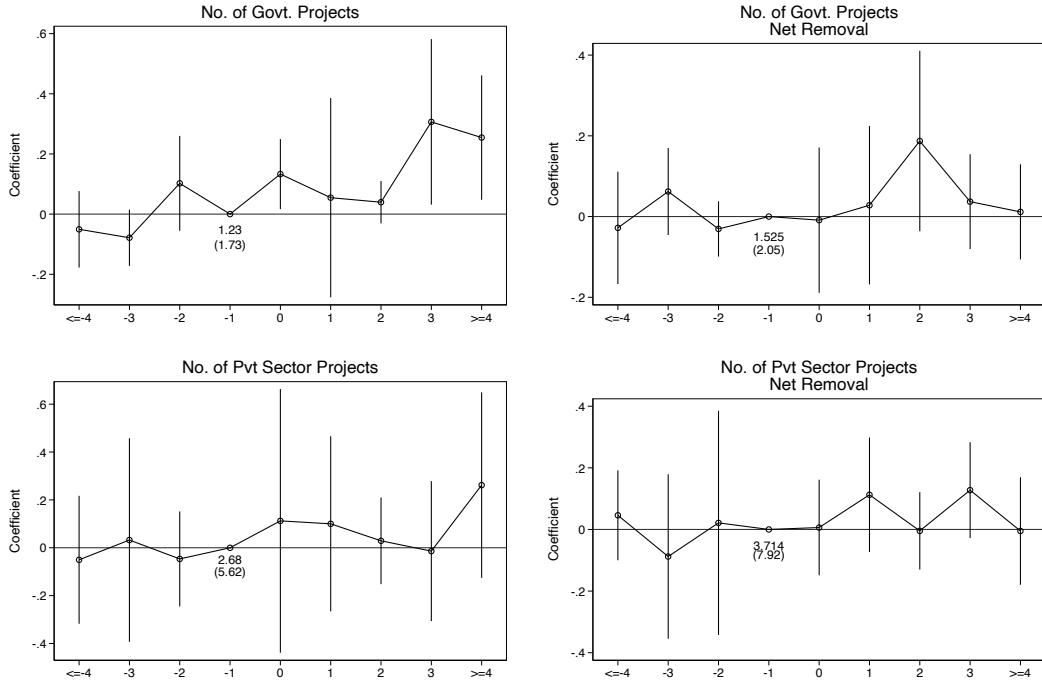


Figure A.7: Multiple Event Study Estimator: Simulation



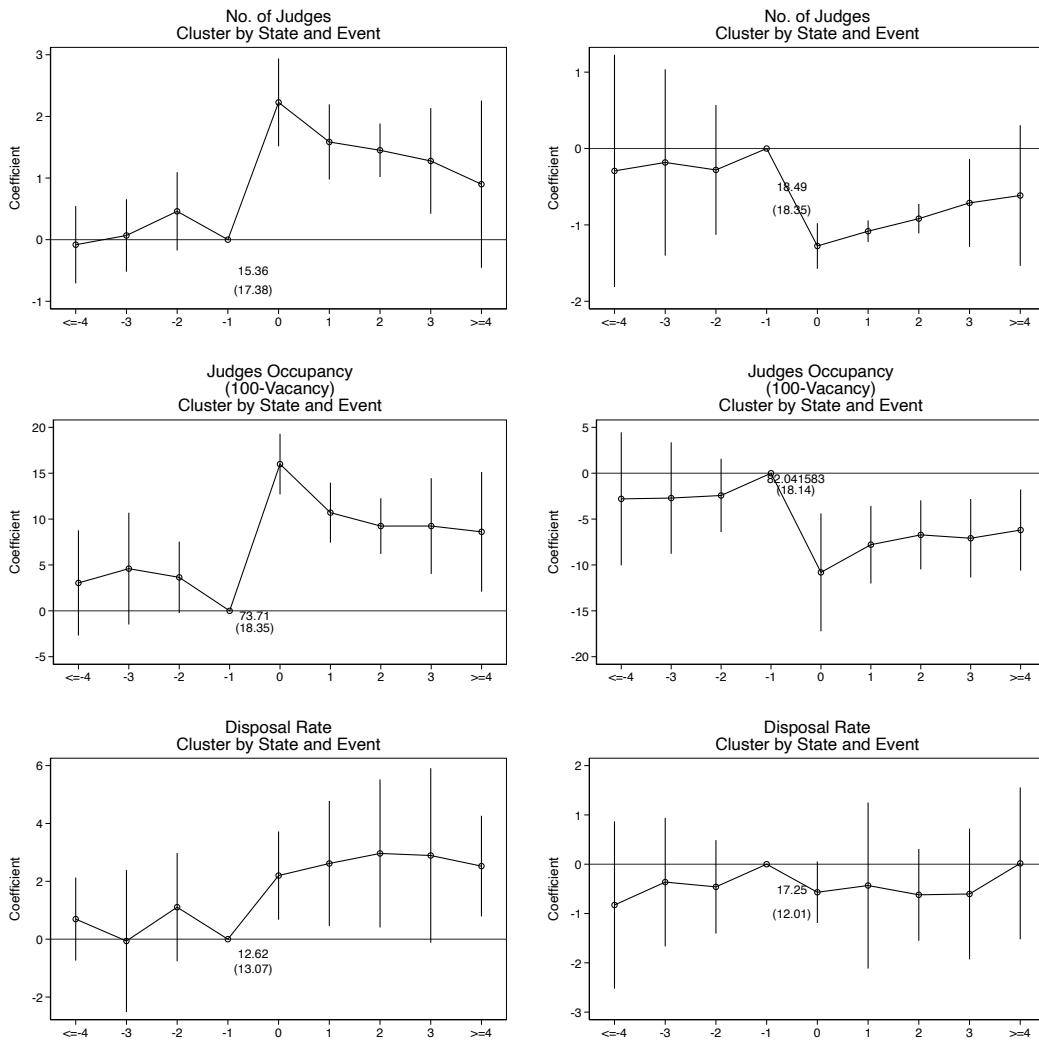
Notes: The above graphs present estimation of the treatment effects using the stacked event study estimator for multiple events using simulated data. The DGP of the number of judges and disposal rate follows an AR(1) process with random shocks introduced by either a positive or negative staffing-level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

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



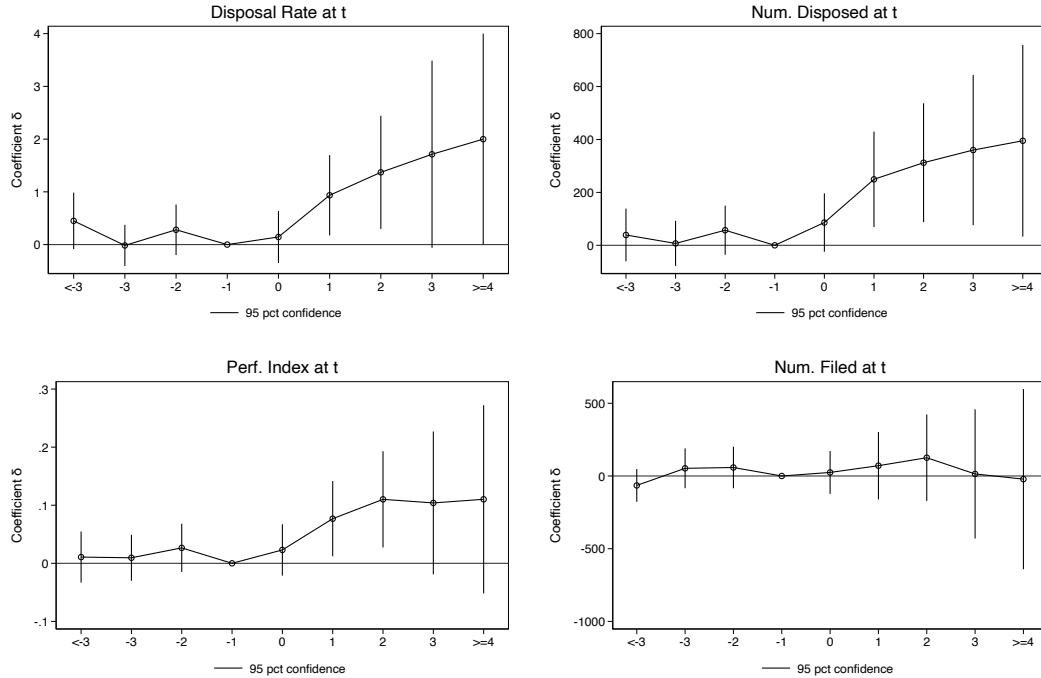
Notes: The figures present the stacked event study estimates using the number of government and private investment projects in a particular district as an outcome variable as in [Equation 1](#). The outcome variable is obtained from CMIE CapEx database of investments, including infrastructure and those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

Figure A.9: Court Outcomes: Inference Robustness



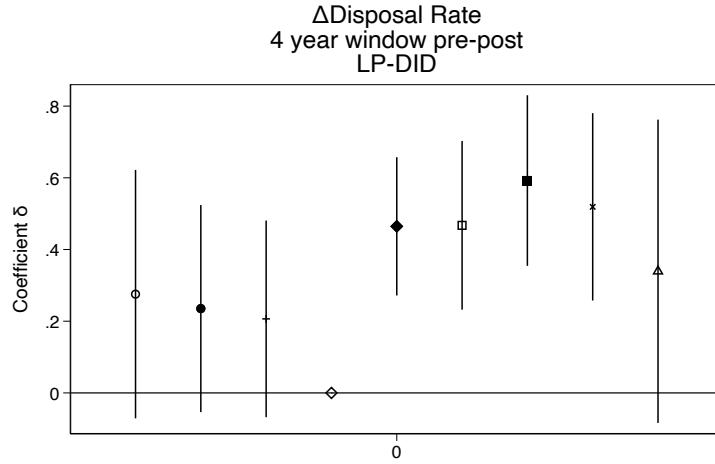
Notes: The figures plot the event study interaction coefficients from estimating [Equation 1](#). Standard errors are clustered by state (instead of district) and event. Error bars present 95% confidence interval.

Figure A.10: Court Outcomes: Continuous Explanatory Variable



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

Figure A.11: Local Projection DID: Court Performance

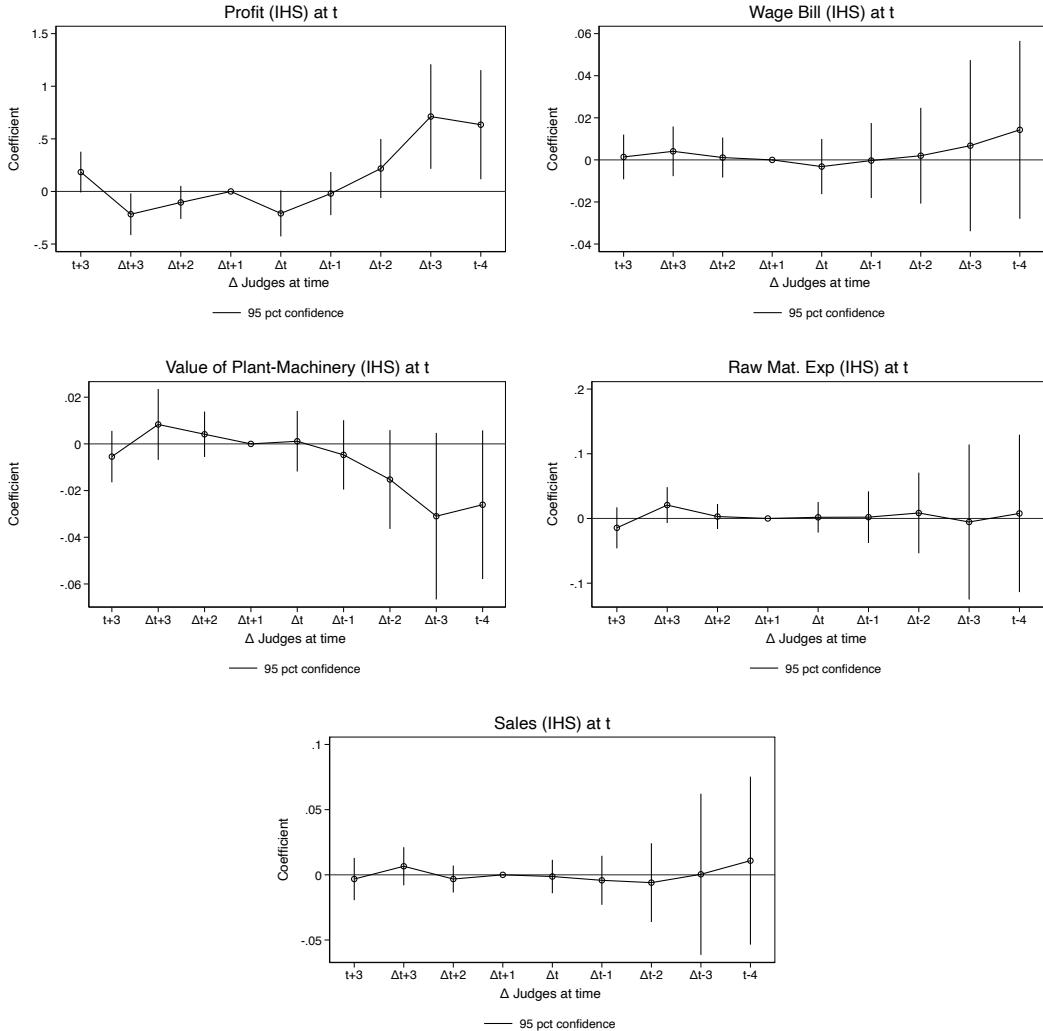


Notes: Following Dube et al. (2022), the local projection DID specification accounts for empirical challenges arising from impulse response functions generated by judicial staffing changes that occur many times and in opposing directions within the study period, similar to events in finance. Each coefficient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

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

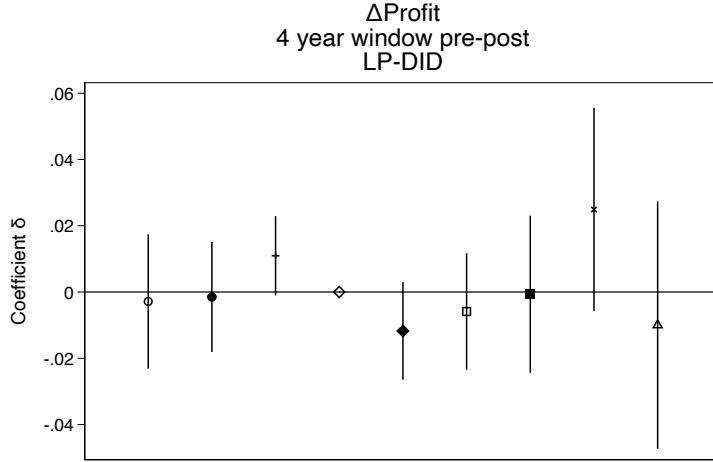
where Δ is the first difference operator.

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



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

Figure A.13: Local Projection DID: Firm Productivity



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

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

where Δ is the first difference operator.

Figure A.14: Model: Credit and Litigation

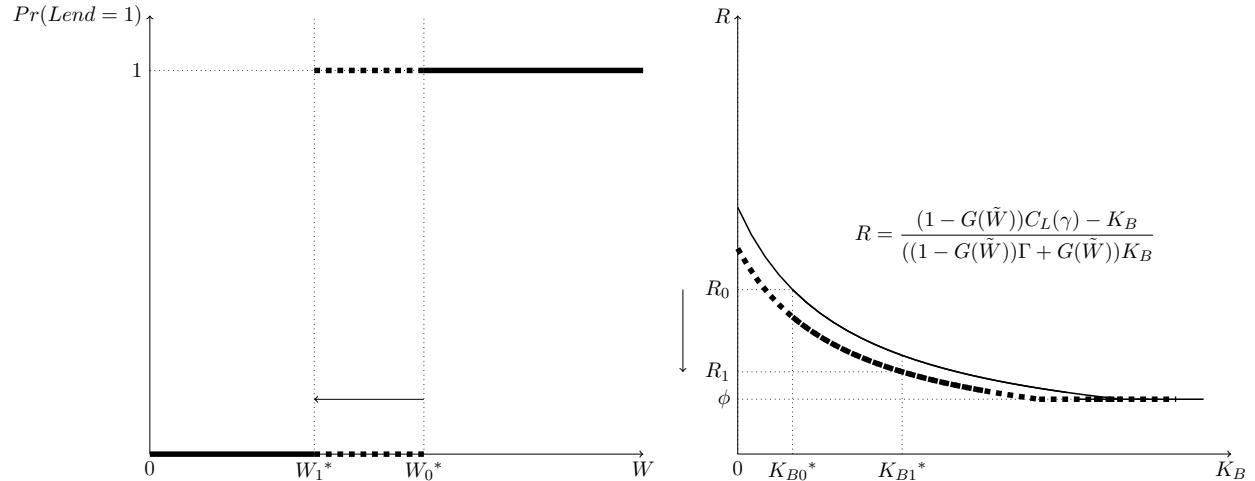
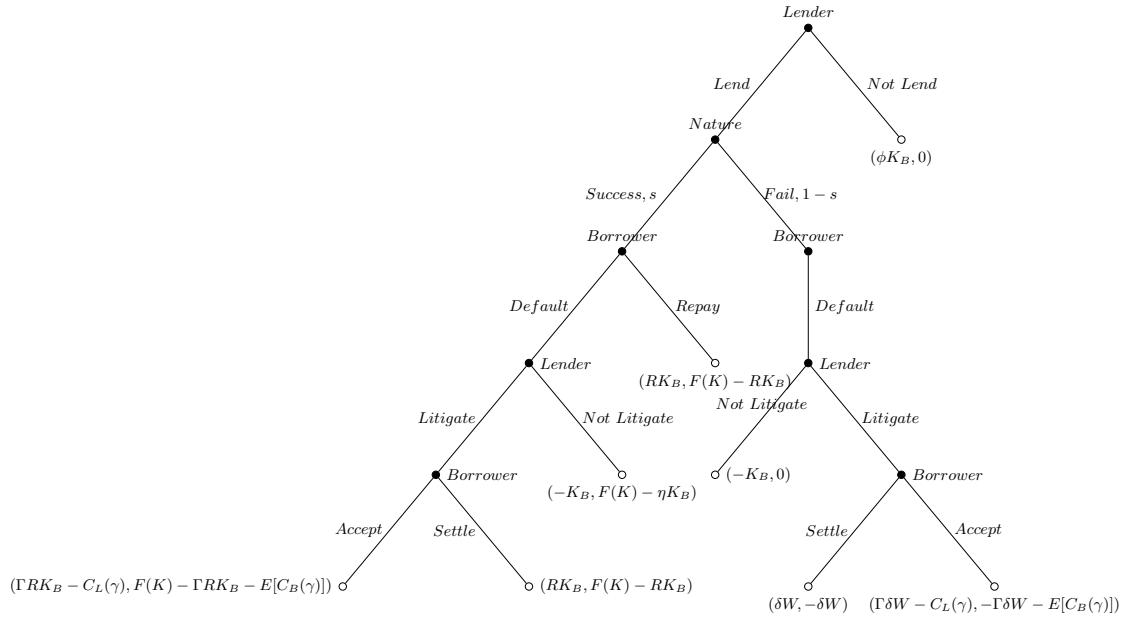
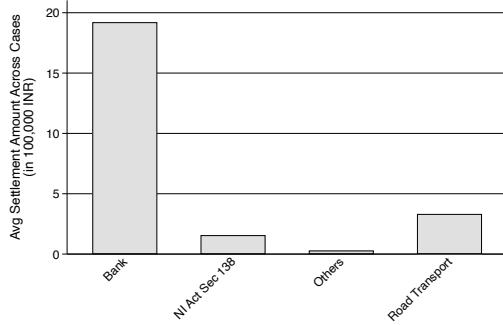
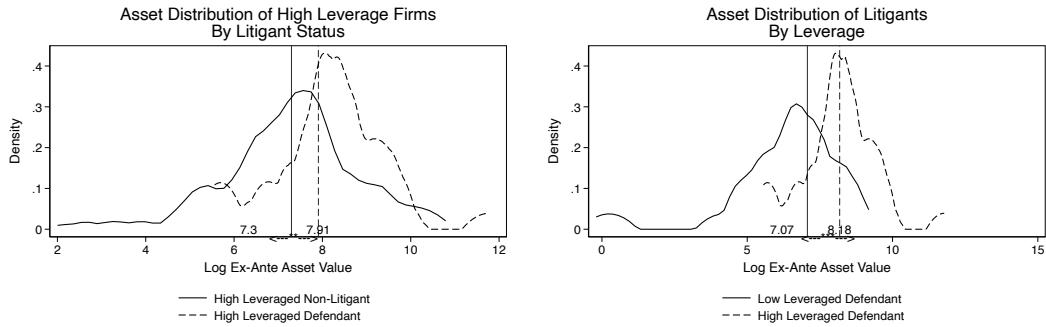


Figure A.15: Firm-related Litigations, and Settlement Amounts
 Panel A: Legal Case Distribution by Firm Type

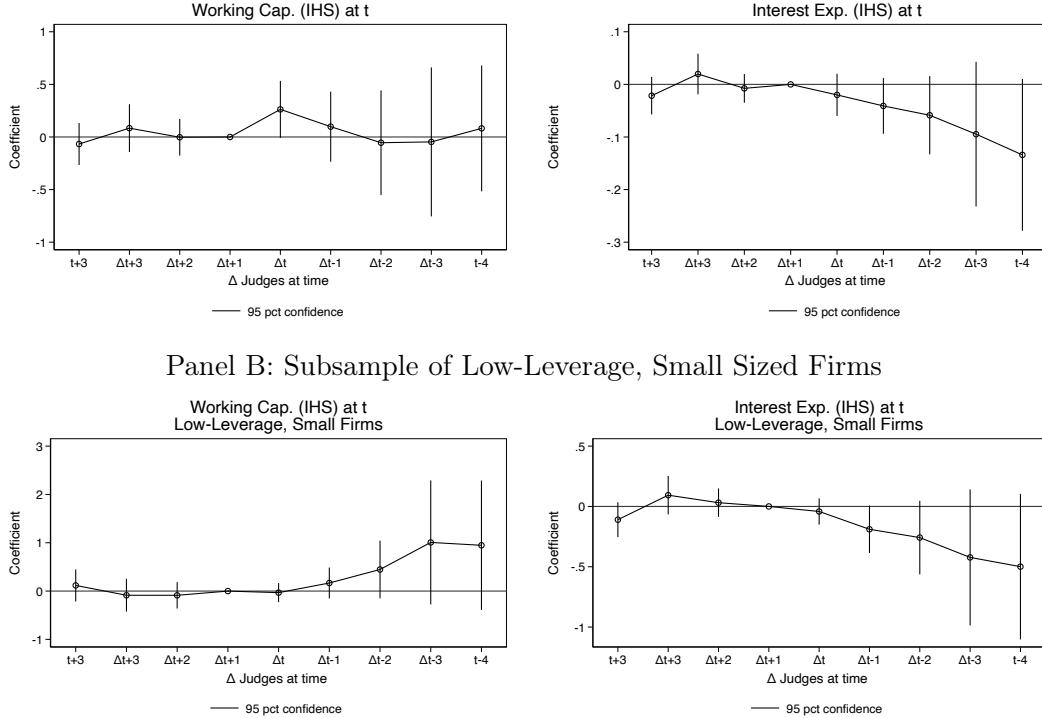


Panel B: Asset Distribution of Litigating Non-Financial Firms



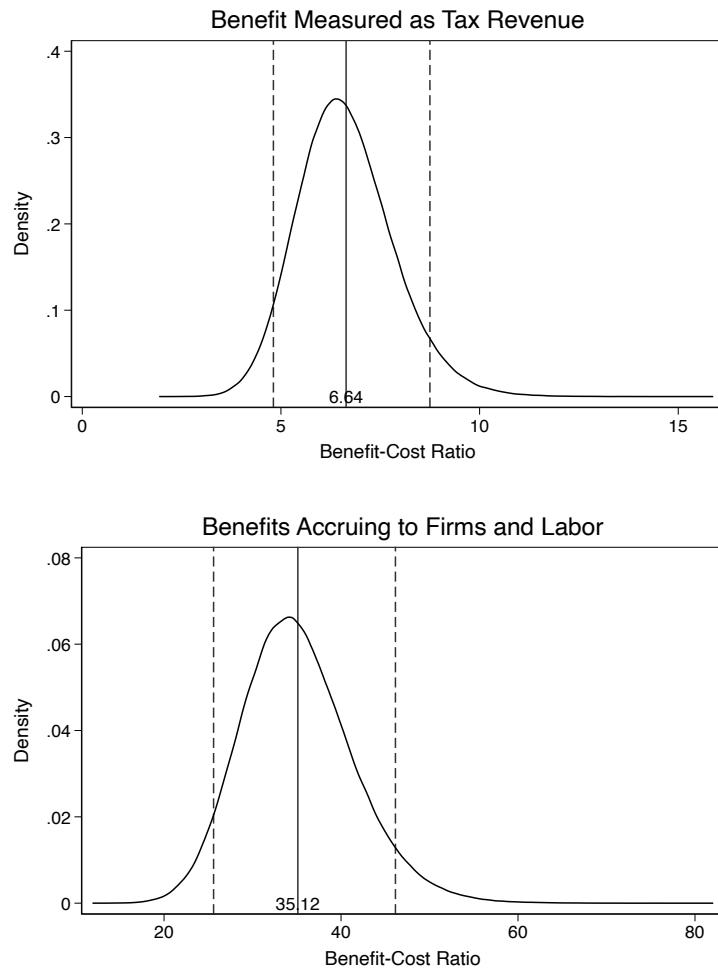
Notes: Panel A presents data on settlement amount from codified judgement documents from one court only for illustration. Panel B presents the kernel densities of local non-financial firms' ex-ante total asset value by: (a) litigation status among high leverage firms (left), and (b) leverage status among the defending firms (right). The lines represent the average asset values with statistical significance of this difference as noted.

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



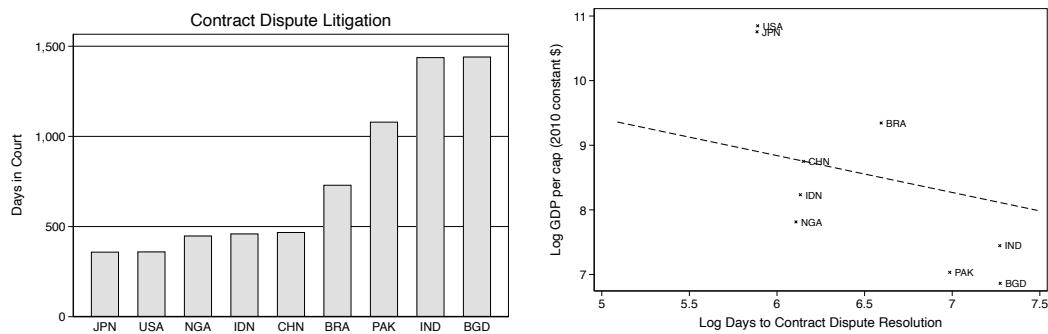
Notes: The figures present the generalized event study estimates relative to number of judges from $t + 1$ when the outcome is measured at t as in [Equation 2](#). Panel A presents the coefficients using firm-level working capital and interest expenditure across all firms in the main sample. Panel B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

Figure A.17: Benefit-Cost Ratio



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

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



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

A.5 Appendix: Tables

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

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

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

Table A.2: Court Outcomes and Judge Vacancy Changes

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

Standard errors in parentheses

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

Notes: This table presents the estimates from [Equation 1](#) using court-level outcomes, equivalent to [Figure 2](#). Columns 1-3 present estimates following net judge increases whereas Columns 4-6 present those following net judge reductions. All court-level specifications include district and state-year fixed effect. Standard errors are clustered by district and event.

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

Standard errors in parentheses

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

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

Table A.4: Caseload Outcomes

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

Standard errors in parentheses

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

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

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

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

Standard errors in parentheses

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

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

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

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

Standard errors in parentheses

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

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

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

Standard errors in parentheses

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

Notes: This table presents the estimates from [Equation 1](#) using firm-level outcomes, equivalent to [Figure 4](#), for net judge removal. IHS refers to inverse hyperbolic sine function. Using logarithmic transformation instead of arcsine yields similar estimates. I restrict the firms sample to a balanced panel in order to ensure no endogenous missing values of firm-level outcomes. All firm-level specifications include firm and state-year fixed effect. Standard errors are clustered by district and event.

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

Standard errors in parentheses

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

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

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

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

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

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

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

Standard errors in parentheses

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

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

Table A.11: Net Judge Removal and Missing Firm-Level Data

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

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

Notes: This table presents the estimates from [Equation 1](#) for net judge removal 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 (0.0480)	0.0299 (0.0418)	-0.0688 (0.0493)	0.0107 (0.0779)	-0.374 (0.325)	-0.480 (0.326)	0.152* (0.0700)
Pos x -3	-0.0122 (0.0244)	0.0239* (0.0128)	-0.0467 (0.0633)	0.0259 (0.0494)	-0.0410 (0.215)	0.160 (0.181)	0.121* (0.0556)
Pos x -2	0.0469 (0.0401)	0.0389 (0.0433)	-0.00119 (0.0538)	0.0475 (0.107)	0.183 (0.410)	0.0577 (0.152)	0.161*** (0.0427)
Pos x 0	0.0198 (0.0246)	-0.00299 (0.0142)	0.0211 (0.0489)	0.0347*** (0.00938)	0.126 (0.141)	0.397** (0.128)	-0.0104 (0.0305)
Pos x 1	0.0398 (0.0238)	0.00294 (0.00926)	0.0478 (0.0795)	0.0448* (0.0243)	0.278 (0.324)	0.0526 (0.112)	-0.0975*** (0.0206)
Pos x 2	0.0416 (0.0270)	0.00400 (0.0116)	0.0835 (0.0627)	0.0363 (0.0363)	0.111 (0.290)	0.134 (0.237)	-0.0568*** (0.0152)
Pos x 3	0.0526*** (0.0165)	0.0338** (0.0127)	0.0374 (0.0281)	0.0423* (0.0226)	0.306 (0.254)	0.0993 (0.161)	-0.0564 (0.0339)
Pos x >=4	0.0695*** (0.0176)	0.0220*** (0.00614)	0.0459*** (0.00907)	0.0575*** (0.00413)	0.463** (0.179)	0.0999 (0.265)	-0.105*** (0.0170)
Observations	11727	11727	11727	11727	11727	11727	11727
No. Firms	203	203	203	203	203	203	203
No. Districts	44	44	44	44	44	44	44

Standard errors in parentheses

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

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

Table A.13: 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 (0.0157)	-0.00366 (0.00309)	0.00909 (0.0116)	-0.00354 (0.00621)	-0.0426** (0.0169)	0.0296 (0.0817)	0.0465*** (0.0143)
Neg x -3	-0.00737 (0.0101)	0.00504 (0.00700)	0.0111 (0.00815)	0.00212 (0.00589)	-0.0138 (0.0614)	-0.0240 (0.158)	0.0250 (0.0140)
Neg x -2	-0.0100 (0.0126)	-0.00350 (0.00527)	0.00556 (0.00746)	-0.00332 (0.00984)	-0.0871* (0.0422)	0.0569 (0.127)	-0.00447 (0.0156)
Neg x 0	-0.00567 (0.00635)	-0.00124 (0.00380)	-0.0138 (0.00988)	-0.0112 (0.00878)	-0.0168 (0.0514)	-0.0348 (0.0794)	-0.0110 (0.0168)
Neg x 1	-0.00563 (0.00762)	-0.00202 (0.00179)	-0.0185 (0.0151)	-0.0184 (0.0139)	-0.119** (0.0398)	0.0705 (0.0769)	-0.0111 (0.0155)
Neg x 2	-0.00942*** (0.00265)	0.00245 (0.00216)	-0.0390* (0.0213)	-0.0221** (0.00787)	-0.0424 (0.0534)	-0.0259 (0.0832)	-0.0239** (0.00853)
Neg x 3	-0.0187*** (0.00442)	-0.0103*** (0.00279)	-0.0466 (0.0316)	-0.0424*** (0.00633)	-0.140 (0.0986)	-0.0694 (0.0603)	-0.0285 (0.0181)
Neg x >=4	-0.0408*** (0.00472)	-0.0196*** (0.00491)	-0.0755* (0.0345)	-0.0606*** (0.00859)	-0.172** (0.0685)	-0.0383 (0.0654)	-0.0398** (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

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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

Table A.14: Outcomes of Firms in Neighboring Districts Following Net Judge Addition (Placebo)

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

Standard errors in parentheses

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

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

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

Standard errors in parentheses

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

Notes: This table presents the estimates from [Equation 1](#) for net judge 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. Notice that the estimates are orders of magnitude smaller than those using the sample of firms within the court's district.

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 (0.0719)	-0.0837 (0.139)	0.0104 (0.0364)	-0.00766 (0.105)	0.223 (0.281)	0.232 (0.537)	0.0429 (0.0571)
Pos x -3	0.00852 (0.0331)	0.0219 (0.0470)	-0.00255 (0.0486)	0.00525 (0.0383)	0.716*** (0.168)	0.149 (0.155)	0.0632 (0.0442)
Pos x -2	0.0341 (0.0275)	-0.0256 (0.0281)	0.0177 (0.0112)	0.0126 (0.0762)	0.0742 (0.362)	0.145 (0.220)	0.0770** (0.0336)
Pos x 0	0.0209** (0.00754)	0.0259 (0.0155)	0.0191*** (0.00550)	0.0279** (0.0124)	0.192* (0.101)	0.452*** (0.0695)	0.0220 (0.0375)
Pos x 1	0.0292** (0.0103)	0.00441 (0.0118)	0.0555*** (0.0156)	0.0442** (0.0160)	0.354** (0.116)	0.325*** (0.0599)	-0.0661** (0.0293)
Pos x 2	0.0399*** (0.0107)	-0.00209 (0.0278)	0.0619** (0.0208)	0.0593*** (0.0145)	0.188 (0.225)	0.366*** (0.0254)	-0.0473 (0.0408)
Pos x 3	0.0573*** (0.00799)	0.0186 (0.0328)	0.0493** (0.0185)	0.0649*** (0.0108)	0.446** (0.146)	0.586*** (0.0600)	-0.0347 (0.0201)
Pos x >=4	0.0622*** (0.0186)	0.00895 (0.0404)	0.0345 (0.0408)	0.0398** (0.0147)	0.196** (0.0772)	0.464*** (0.0947)	-0.0587*** (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

Standard errors in parentheses

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

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. This addresses the concern if the judge staffing process is endogenous, particularly in the context of industrial states where consortium of firms could influence policy makers and high court judges in district judge allocation mechanism. The above table shows that the results are similar to the full sample, suggesting that this concern does not threaten the identification strategy or affect the interpretation of the results.

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* (0.0149)	-0.00182 (0.0290)	-0.0193 (0.0176)	-0.0181 (0.0180)	-0.188 (0.129)	-0.216*** (0.0307)	0.0444*** (0.0101)
Neg x -3	-0.0195* (0.00951)	-0.00795 (0.0148)	-0.0110 (0.00976)	0.00604 (0.0162)	-0.262*** (0.0718)	-0.0722 (0.288)	0.0193 (0.0161)
Neg x -2	-0.0191* (0.00873)	-0.00126 (0.0131)	-0.00660 (0.00509)	-0.00724 (0.0167)	-0.160 (0.132)	-0.0199 (0.154)	-0.0174 (0.0227)
Neg x 0	-0.000568 (0.00928)	-0.0164 (0.0157)	-0.00954 (0.0160)	-0.0166 (0.0162)	-0.171 (0.159)	-0.104 (0.188)	-0.0237 (0.0328)
Neg x 1	0.00474 (0.0102)	-0.0183* (0.0100)	-0.0225 (0.0237)	-0.0360** (0.0143)	-0.382*** (0.0965)	-0.00374 (0.173)	0.00109 (0.0340)
Neg x 2	-0.00722 (0.0163)	-0.0149 (0.0249)	-0.0405 (0.0384)	-0.0651*** (0.0137)	-0.500*** (0.102)	-0.152 (0.174)	-0.00367 (0.0166)
Neg x 3	-0.0290 (0.0248)	-0.0359 (0.0490)	-0.0539 (0.0614)	-0.0994*** (0.0304)	-0.777*** (0.215)	-0.362*** (0.0840)	-0.0219 (0.0313)
Neg x >=4	-0.0571** (0.0252)	-0.0922 (0.0680)	-0.0833 (0.0604)	-0.109*** (0.0318)	-0.471*** (0.0763)	-0.189 (0.166)	-0.0146 (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

Standard errors in parentheses

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

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. This addresses the concern if the judge staffing process is endogenous, particularly in the context of industrial states where consortium of firms could influence policy makers and high court judges in district judge allocation mechanism. The above table shows that the results are similar to the full sample, suggesting that this concern does not threaten the identification strategy or affect the interpretation of the results.

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 (0.0665)	-0.0626 (0.108)	-0.0264 (0.0777)	0.00673 (0.0833)	-0.251 (0.258)	0.253 (0.440)	0.0908 (0.0600)
Pos x -3	-0.00157 (0.0415)	0.00833 (0.0353)	-0.0640 (0.102)	0.00726 (0.0441)	0.163 (0.152)	0.132 (0.236)	0.0706* (0.0334)
Pos x -2	0.000595 (0.0290)	-0.00513 (0.0382)	0.0110 (0.0455)	0.0191 (0.0726)	0.105 (0.407)	0.108 (0.109)	0.0810** (0.0274)
Pos x 0	0.00133 (0.0201)	0.0181 (0.0137)	0.0428* (0.0207)	0.0275*** (0.00645)	0.0877 (0.112)	0.319*** (0.0755)	-0.00654 (0.0285)
Pos x 1	0.0212 (0.0167)	-0.00245 (0.00982)	0.0450 (0.0425)	0.0306* (0.0143)	0.424** (0.142)	0.267 (0.175)	-0.103*** (0.0300)
Pos x 2	0.0280* (0.0135)	-0.00481 (0.0266)	0.0927*** (0.0289)	0.0454*** (0.0137)	0.260* (0.132)	0.262 (0.195)	-0.0804* (0.0371)
Pos x 3	0.0460*** (0.00817)	0.0269 (0.0452)	0.0580*** (0.0134)	0.0566*** (0.00797)	0.457*** (0.109)	0.306** (0.0989)	-0.0963*** (0.0261)
Pos x >=4	0.0463** (0.0172)	0.0152 (0.0462)	0.0374 (0.0233)	0.0330*** (0.00922)	0.217*** (0.0633)	0.225** (0.0995)	-0.109*** (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

Standard errors in parentheses

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

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. This addresses the concern if the judge staffing process is endogenous, particularly within metropolitan courts, even if the process is plausibly exogenous in other districts of the state. The above table shows that the results are similar to the full sample, suggesting that this concern does not threaten the identification strategy or affect the interpretation of the results.

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 (0.0114)	0.00185 (0.0210)	-0.00562 (0.0123)	-0.00799 (0.0102)	-0.185** (0.0792)	-0.126 (0.0760)	0.0502*** (0.00924)
Neg x -3	-0.0137 (0.0124)	0.000103 (0.0122)	0.00633 (0.0220)	0.00177 (0.0100)	-0.162* (0.0786)	-0.0273 (0.137)	0.0137 (0.00949)
Neg x -2	-0.00720 (0.0129)	0.000730 (0.0109)	-0.00600 (0.0104)	-0.00410 (0.0113)	-0.121 (0.0969)	0.0794 (0.0954)	-0.0115 (0.0168)
Neg x 0	-0.0000545 (0.00970)	-0.0121 (0.0157)	-0.0239 (0.0208)	-0.0121 (0.0116)	-0.102 (0.128)	-0.0322 (0.199)	-0.0243 (0.0301)
Neg x 1	0.00183 (0.00842)	-0.0127 (0.0138)	-0.0249 (0.0240)	-0.0186 (0.0167)	-0.348*** (0.0744)	0.0280 (0.145)	-0.0106 (0.0294)
Neg x 2	-0.00585 (0.00627)	-0.00723 (0.0243)	-0.0637* (0.0323)	-0.0392** (0.0136)	-0.321*** (0.0966)	-0.0467 (0.0807)	-0.0341 (0.0258)
Neg x 3	-0.0232** (0.00838)	-0.0325 (0.0381)	-0.0647 (0.0436)	-0.0682*** (0.0185)	-0.554*** (0.144)	-0.173** (0.0697)	-0.0310 (0.0398)
Neg x >=4	-0.0428*** (0.00942)	-0.0722 (0.0446)	-0.0988 (0.0591)	-0.0697*** (0.0139)	-0.352*** (0.0620)	0.150 (0.199)	-0.0415* (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

Standard errors in parentheses

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

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. This addresses the concern if the judge staffing process is endogenous, particularly within metropolitan courts, even if the process is plausibly exogenous in other districts of the state. The above table shows that the results are similar to the full sample, suggesting that this concern does not threaten the identification strategy or affect the interpretation of the results.

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 (0.0417)	-0.0500 (0.0667)	-0.0234 (0.0464)	0.0256 (0.0726)	-0.217 (0.204)	0.167 (0.416)	0.103 (0.0600)
Pos x -3	0.000279 (0.0310)	0.0162 (0.0172)	-0.0505 (0.0686)	0.0120 (0.0369)	0.135 (0.340)	0.0202 (0.235)	0.0883* (0.0462)
Pos x -2	0.00715 (0.0339)	0.00361 (0.0368)	0.00903 (0.0267)	0.0181 (0.0558)	0.193 (0.339)	0.111 (0.0783)	0.0957** (0.0348)
Pos x 0	-0.00187 (0.0179)	0.0179 (0.0166)	0.0171 (0.0312)	0.0201*** (0.00576)	0.110 (0.114)	0.389*** (0.0866)	-0.00813 (0.0276)
Pos x 1	0.0196 (0.0184)	0.00435 (0.00621)	0.0253 (0.0482)	0.0184 (0.0210)	0.418*** (0.0892)	0.200 (0.156)	-0.0864* (0.0383)
Pos x 2	0.0207 (0.0234)	-0.00149 (0.0211)	0.0717 (0.0447)	0.0210 (0.0302)	0.310*** (0.0811)	0.172 (0.136)	-0.0802 (0.0472)
Pos x 3	0.0369 (0.0220)	0.0266 (0.0345)	0.0401 (0.0279)	0.0360 (0.0224)	0.462*** (0.0605)	0.275* (0.147)	-0.0817 (0.0588)
Pos x >=4	0.0514* (0.0259)	0.0194 (0.0359)	0.0336 (0.0188)	0.0289* (0.0147)	0.334*** (0.102)	0.244 (0.138)	-0.0903 (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

Standard errors in parentheses

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

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. The sample includes data from 15 states.

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 (0.00675)	0.00629 (0.0108)	0.00261 (0.0100)	-0.00225 (0.00233)	-0.0803 (0.0877)	-0.0779 (0.0506)	0.0251 (0.0328)
Neg x -3	-0.00570 (0.00867)	0.00140 (0.00625)	0.00601 (0.00990)	0.00193 (0.00503)	-0.0664 (0.0741)	-0.0151 (0.0564)	0.00411 (0.0213)
Neg x -2	-0.00328 (0.00590)	-0.000139 (0.00405)	-0.000887 (0.00564)	-0.00116 (0.00341)	-0.0631 (0.0410)	0.0266 (0.0667)	-0.00900 (0.0155)
Neg x 0	0.00116 (0.00542)	-0.00697 (0.00932)	-0.00905 (0.00676)	-0.00492 (0.00718)	-0.0499 (0.0541)	-0.0356 (0.0959)	-0.00827 (0.0168)
Neg x 1	0.00113 (0.00586)	-0.00960 (0.0108)	-0.0109 (0.0108)	-0.00699 (0.0147)	-0.162** (0.0656)	0.0252 (0.0695)	-0.00239 (0.0188)
Neg x 2	-0.00149 (0.00530)	-0.00692 (0.0104)	-0.0289 (0.0165)	-0.0115 (0.0230)	-0.170* (0.0843)	0.00525 (0.0872)	-0.00874 (0.0248)
Neg x 3	-0.00967 (0.00633)	-0.0187 (0.0183)	-0.0312 (0.0221)	-0.0251 (0.0291)	-0.264** (0.116)	-0.0679 (0.0994)	-0.00507 (0.0340)
Neg x >=4	-0.0224* (0.0108)	-0.0361 (0.0396)	-0.0495* (0.0236)	-0.0277 (0.0305)	-0.207 (0.120)	0.0580 (0.186)	-0.0126 (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

Standard errors in parentheses

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

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. The sample includes data from 15 states.

Table A.22: Credit Mechanism: District-level

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

Standard errors in parentheses

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

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

Table A.23: Credit Mechanism: Firm-level Heterogeneity by Size

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

Standard errors in parentheses

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

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

Table A.24: Local Recorded Crime and Judge Staffing Changes

	Net Judge Addition		Net Judge Removal	
	(1) Serious Crime (IHS)	(2) Other Crime (IHS)	(3) Serious Crime (IHS)	(4) Other Crime (IHS)
Event x <=-4	0.00343 (0.00619)	0.00550 (0.0417)	0.0270*** (0.00467)	-0.0160 (0.0348)
Event x -3	-0.00816 (0.0224)	-0.0223 (0.0340)	0.0193*** (0.00416)	-0.0128 (0.0281)
Event x -2	0.00232 (0.00320)	-0.0105 (0.0165)	0.00267 (0.00711)	-0.00100 (0.0194)
Event x 0	-0.0182*** (0.00471)	-0.00279 (0.0414)	0.00498 (0.00841)	0.0280 (0.0356)
Event x 1	-0.00390 (0.0124)	-0.0246 (0.0191)	-0.0112 (0.0104)	0.0578** (0.0206)
Event x 2	-0.0199*** (0.00488)	0.0149 (0.0257)	-0.0124** (0.00546)	-0.0113 (0.0263)
Event x 3	-0.00592* (0.00290)	-0.101*** (0.0213)	-0.0221*** (0.00550)	0.0590 (0.0363)
Event x >=4	-0.0256*** (0.00509)	-0.00650 (0.0537)	-0.0164** (0.00554)	0.0676*** (0.0222)
Observations	9101	9101	9101	9101
No. Districts	195	195	195	195

Standard errors in parentheses

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

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 net judge increase whereas Columns 3-4 present those following net judge reduction as per [Equation 1](#). All specifications include district and state-year fixed effect. Notice that while the estimates in Col 3 are statistically significant but in the opposite direction (counterintuitively), these likely reflect a secular decreasing time trend of serious crimes within a district going by the pre-period time trend. This could happen, for example, if investments in police forces within a district are increasing over time, which would affect serious crime rates. This is because police has a greater discretion in investigation and violent crime containment but not for small-scale property crime, which are more likely to respond to effective law enforcement through fines and recovery of property by courts.