Public Eyes on the Bench: Judicial Transparency, Independence, and Firm Outcomes

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Abstract

We examine whether increasing judicial transparency through live-streaming of courtroom proceedings affects judicial behavior and economic outcomes using the staggered introduction of live-streaming in Indian Courts. Live-streaming increases government victories in business–state disputes by 8.1 percentage points. Textual analysis shows a pro-government tilt and greater reliance on government narratives. We interpret this through a mechanism where transparency raises salience, shifting judicial incentives toward public sentiment. Consistent with this channel, live-streamed cases receive greater social media amplification, and effects are stronger in politically popular states and among judges facing career concerns. We document ex-ante and ex-post real effects: exposed firms reduce borrowing and investment and face higher financing costs.

Keywords: Judicial Transparency; Judicial Behavior; Government–Business Disputes; Regulatory Enforcement; Text Analysis; Firm Outcomes
JEL Codes: K22, K42, K40, G30, O43, D73

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1 Introduction

Judicial decisions shape the economic environment in which firms operate. A large body of work exploiting quasi-random assignment of cases to judges shows that judicial identity, and the leanings associated with it, can systematically influence case outcomes (Abrams et al. (2012); Dobbie et al. (2018); Frandsen et al. (2023); Bhuller and Sigstad (2025)). Whether courts tilt toward one side or the other — across racial, gender, ideological, or institutional lines — shapes who gains access to justice and how economic disputes are resolved, including in corporate and financial contexts (Gormley et al. (2025); Chen et al. (2025)). Because judicial decisions affect contracts, property rights, and the enforcement of regulation, understanding the forces that shift these outcomes is a first-order question for institutional credibility and economic performance.

A long-standing view holds that increasing transparency within the courtroom, and thus making judicial behavior more observable, can serve as a check on judicial conduct by increasing accountability (European Network of Councils for the Judiciary (2024); Youm (2012); Lasser (2004); France (2019)). Live-streaming of court proceedings has emerged as a prominent mechanism for enhancing such transparency: it exposes judicial reasoning, demeanor, and interactions to public and professional scrutiny in real time, potentially disciplining how judges weigh competing claims. Several countries have adopted this practice in recent years.[1] Despite such widespread implementation of livestreaming, the economic implication of enhanced judicial transparency remains understudied.

Theory offers competing predictions on this question. The "judicial trilemma" posits that only two of three features–transparency, independence, and accountability to the executive–can coexist (Dunoff and Pollack (2017)). In systems where courts are accountable to political branches, greater transparency may undermine independence by enabling political oversight. India, the setting of our study, differs: judicial appointments and evaluations are controlled by a judicial collegium rather than the executive, suggesting that transparency and independence could coexist. Yet even without direct political control, transparency may influence judicial behavior through reputational channels. Judges may care about reputation, prestige, and influence, and their decisions may depend on how they are perceived by relevant audiences (Posner (1993)). As transparency expands the set of observers—from lawyers and litigants to the broader public—reputational stakes increase. Models of career concerns

[1]The United Kingdom, Canada, and Brazil have adopted live-streaming in their apex courts, while Australia and Singapore have expanded audiovisual access and remote observation, and South Africa allows broadcasting in defined circumstances (UK Supreme Court (2023); Supreme Court of Canada (2023); Supremo Tribunal Federal (Brazil) (2023); High Court of Australia (2023); Singapore Judiciary (2023); South Africa Department of Justice (2023)).

show that when actions are observable, decision-makers may conform to audience expectations rather than rely on private information (Prat (2005)). The judicial politics literature similarly argues that courts seek public legitimacy and respond to public opinion when decisions become salient (Spiller and Gely (2007)). Together, these perspectives suggest that transparency may either discipline judicial conduct or induce alignment with public sentiment, with consequences for judicial objectivity. This paper studies this interaction using the staggered introduction of live-streaming across Indian High Courts. We examine its effects on outcomes in disputes between governments and businesses–a setting where shifts in judicial decision-making have significant economic and policy implications–and on firms' ex-ante and ex-post real outcomes.

The Indian Judiciary setting is well suited to study this question. The Supreme Court of India in 2018 directed all High Courts to introduce live-streaming of court proceedings to enhance transparency. The directive did not impose a binding deadline for implementation, resulting in staggered adoption across courts. By the end of our sample period, 13 of the 25 High Courts had adopted live-streaming, while the remaining courts were in the process of implementing the reform. This staggered rollout provides a natural setting to study the interaction between transparency and judicial independence in an institutional environment where courts are both de jure and de facto insulated from executive control but operate within a democratic system. Our empirical analysis focuses on disputes between governments and businesses, a context in which public perceptions may strongly influence how judicial decisions are interpreted. The key question is whether increased transparency changes judicial behavior by making judges more responsive to public sentiment. In business–government disputes, decisions favoring firms may be perceived as privileging private interests or signaling corruption, potentially encouraging judges to be cautious in business-government cases (Huang et al., 2025; Cohen and Yang, 2019). Whether transparency weakens judicial independence by inducing courts to align decisions with public sentiment - potentially tilting outcomes in favor of the government - is the central question we address in this paper.

Our headline result is an 8.1 pp increase in government victories in business–government disputes following live-streaming. Text analysis shows a rise in pro-regulation language and greater weight on government arguments. We interpret this as transparency increasing public salience and shifting accountability toward public opinion, with judges tilting toward positions aligned with prevailing sentiment. Supporting evidence includes greater social media amplification of broadcast cases, stronger effects in states with higher incumbent support, and concentration among judges nearing tenure confirmation. We also find meaningful real effects for exposed firms, including declines in investment and borrowing and higher borrowing costs, both ex ante and ex post.

Our primary data source is Indian Kanoon, a comprehensive digital repository of Indian legal documents. We compile all High Court judgments related to commercial cases issued between January 2016 and December 2025, yielding 57,457 decisions. Among these, 10,607 involve disputes between the government and private businesses, which form the core of our analysis. Each judgment follows a structured format with seven sections: a case overview, the legal issue, plaintiff's arguments, defendant's arguments, analysis of precedents, judicial interpretation, and the final conclusion. Organizing the data at the judge–judgment level, we employ a staggered difference-in-differences design and find that live streaming increases the likelihood of government victory in government–business disputes by 8.1 pp, a 37% increase relative to the unconditional win rate.

At this stage, it is important to address the concern that adoption of live-streaming may be endogenous. We first rule out pre-trends using event study analysis, and also show that the estimates are robust to the Sun and Abraham (2021) test for staggered difference-in-differences. The results indicate that courts with an emerging pro-government tilt did not differentially adopt live-streaming. Next, we test whether adopting states differ in factors that could affect outcomes, including the share of government–business disputes, case volumes, legal administration spending, case composition, and pre-existing judicial ideology, and find no meaningful differences. We also compare adopting and non-adopting states across a wide set of political, economic, judicial, and social characteristics and again find no systematic differences. A related concern is time-varying endogeneity. To address this, we examine outcomes in lower courts and tax tribunals under High Court administrative jurisdiction that were not required to adopt live-streaming. We find no pro-government shift in these forums in adopting states. Taken together, these results suggest adoption was driven by administrative and logistical factors rather than changes in judicial behavior.

As a complement to tests based on case outcomes, we analyze judgment text. We first examine whether judicial language reflects a tilt toward the government. To avoid mechanical correlation with outcomes, we exclude the operative judgment section and focus on the case summary written by judges. Following the zero-shot learning framework of Larochelle et al. (2008) and Lampert et al. (2009), we classify economic orientation using the BART-large-MNLI natural language inference model. For each judgment, we construct pro-regulation (pro-government), neutral, and pro-market scores. We find a significant increase in pro-government orientation after live-streaming, with no change in neutral or pro-market sentiment and no evidence of pre-trends.

Our second test examines whether live-streaming alters the weight judges place on the competing narratives of the government and the firm. Building on text-based measures of narrative incorporation (Kelly et al., 2021; Chen et al., 2025), we construct a measure cap-

turing the extent to which judicial reasoning aligns more with the government's submissions than with those of the corporate litigant. Using the structured format of judgments, we extract the petitioner's narrative, the respondent's narrative, and the judge's recognized facts, and compute a relative similarity measure based on TF–IDF representations validated with transformer embeddings. We find that live-streaming increases the incorporation of government narratives into the recognized-facts section of judgments. We conduct tests that rule out alternative explanations based on mechanical correlation between the winning party's submissions and the judgment text, as well as shifts in government submissions.

In the second part of the analysis, we examine the mechanism through which live-streaming affects judicial behavior. The first link is that live-streaming increases the public salience of court proceedings. Consistent with this, we find that cases heard under live-streaming receive at least 4.55 times greater amplification on social media than comparable cases in non–live-streaming courts. We also observe substantial viewership and engagement on official High Court YouTube channels, indicating that live-streaming significantly expands the audience for judicial proceedings.

The second link is that greater public attention may tilt sentiment toward the government in disputes between businesses and the state. Consistent with this, we find that the pro-government shift in outcomes is significantly stronger in states where incumbent governments enjoy high public support. This pattern is not driven by other state-level differences: results remain when comparing treated and control courts within politically popular states.

The final link is that the judiciary responds to public opinion to maintain legitimacy. We use the fact that judges recruited as Additional Judges (non-permanent) are typically confirmed after about one to three years of service by the Judicial collegium. We find that the pro-government shift following live-streaming is significantly stronger for judges close to this career milestone. This effect is absent among the permanently appointed judges around the same threshold, ruling out a inexperience-driven explanation. At the same time, we find no evidence that judges nearing the mandatory retirement age of sixty-two display similar behavior, which helps rule out explanations based on post-retirement opportunities such as commissions, governorships, or nominations to the upper house. Taken together, these patterns suggest that the observed effects are unlikely to reflect rewards from the government and instead are consistent with the judiciary responding to public scrutiny. Consequently, transparency shifts judicial accountability toward public opinion. While we cannot directly observe motivations, our findings align with broader evidence that courts respond to public sentiment even when formal institutional design suggests otherwise. For example, Chen et al. (2025) show that courts in China adjust decisions to reflect public preferences despite operating in a non-democratic system. This makes it even more plausible that judges respond

to public sentiment in a democracy like India.

The third part of the paper examines the real effects of the shift in judicial behavior. If live-streaming increases the likelihood that courts tilt toward the government in business–state disputes, firms with greater legal exposure should anticipate less favorable treatment in regulatory or contractual conflicts. Such expectations can generate ex-ante responses: firms may scale back borrowing and investment if they perceive weaker property rights protection or higher regulatory risk, while lenders may demand higher interest rates to compensate for adverse judicial outcomes. These effects should be most pronounced in sectors with greater inherent legal exposure - industries subject to higher regulatory scrutiny, greater reliance on legal enforcement, or higher litigation costs. We identify such sectors as those in the top half of legal expenses relative to revenue. Using this classification in a difference-in-differences framework, we find that firms in these sectors experience a 13.3% decline in borrowing and a 10.3% decline in investment following live-streaming adoption. Borrowing costs increase by 54.5 basis points, consistent with lenders pricing higher legal risk. These changes translate into weaker operating performance, with firm revenue declining by 6.6%. The results remain robust to an alternative measure of legal exposure based on the incidence of business–state disputes in our case-level data.

We next examine ex-post effects for firms involved in litigation with the government. Firms in courts that adopt live-streaming underperform their industry peers in investment and sales. We do not observe similar patterns for firms in tax tribunals, which were not subject to live-streaming. This contrast suggests that the effects reflect market expectations of a pro-government tilt rather than broader economic conditions.

In the final part of the paper, we conduct several robustness and additional tests. First, we verify that our results hold for the subset of treated cases with available video records. Although High Courts livestream commercial proceedings, not all hearings are archived due to exclusions (e.g., sensitive cases) and technical constraints, and recordings are often uploaded as full-day proceedings rather than case-level files. Within these limits, we convert available videos into text and match them to cases, identifying 44.76% of cases decided during the livestreaming period. This reflects only the share that can be reliably matched, not the total extent of livestreaming. Second, we rule out changes in the government's appeal strategy by examining the ratio of cases lost in lower courts to those appealed in High Courts using District Court and tax tribunal data; we find no evidence of a decline after adoption. Third, using machine-learning predictions based on pre-reform data, we show that livestreaming increases both the probability of government victory and outcome uncertainty, providing a channel for the real effects documented earlier.

A caveat to our interpretation is that the empirical design does not, on its own, distinguish

a new pro-government tilt from correction of a pre-existing anti-government distortion. The pre-period government win rate of 22.8% may itself reflect a distorted equilibrium, in which case the post-live-streaming shift represents movement toward efficient adjudication rather than away from it. One possible reason for the low baseline win rate is that our sample consists of appellate cases, where governments may be more likely to appeal adverse decisions because of greater resources and institutional incentives relative to private litigants.

To address the above concern, we examine heterogeneity in our main estimates by pre-period government win rates across case types, where case type refers to the law governing the dispute, ranging from the Income Tax Act to creditor-rights and commercial laws. Pre-period win rates vary from 13% to 37% across these categories. If the documented shift reflects correction of a prior distortion, the effect should be larger in case types with lower initial government win rates. We find no such gradient: estimates remain stable across the distribution of pre-period win rates, inconsistent with proportional correction and more consistent with a level shift induced by live-streaming. Nonetheless, we cannot fully rule out mean reversion. Importantly, our conclusion that increased transparency can weaken judicial independence holds under either interpretation

Our paper contributes to the law and economics literature on judicial decision-making by bringing together three strands of research on judicial incentives, transparency, and institutional design. First, a large literature models judges as strategic actors whose decisions are shaped by reputational and career concerns. Posner (1993) provides a seminal economic interpretation, arguing that judges maximize nonpecuniary objectives such as reputation, prestige, and influence. Subsequent work formalizes how judicial behavior responds to audiences and incentives (Garoupa and Ginsburg, 2015). Second, an agency-theoretic literature shows that transparency alters behavior by changing incentives. Prat (2005) demonstrates that when actions are observable, agents concerned about reputation may ignore private information and conform to what appears appropriate to observers. Related models show how career concerns can induce decision makers to cater to external audiences (Prendergast, 1993; Morris, 2001; Maskin and Tirole, 2004). Third, an institutional literature highlights tensions between transparency, independence, and accountability in court design. Dunoff and Pollack (2017) characterize this as a judicial trilemma, while related work emphasizes trade-offs in structuring judicial authority (Carrubba et al., 2008; Stephenson, 2004; Garoupa and Ginsburg, 2009). Finally, a related judicial politics literature shows that courts respond to public legitimacy when decisions become salient (Spiller and Gely, 2007).

These studies suggest that transparency reforms may have unintended effects on judicial independence depending on institutional context. Our paper brings these ideas to data using the introduction of live-streaming in India. Unlike settings where transparency interacts with

executive oversight, the Indian judiciary is both de jure and de facto insulated from executive control through the collegium system. This provides a setting to test whether transparency affects judicial behavior even without formal political accountability. We show that greater transparency shifts outcomes in government–business disputes toward the state, consistent with judges responding to expanded public audiences rather than direct political pressure.

Our paper also contributes to the debate on whether judicial bias reflects judge-specific fixed effects (Canay et al., 2024; Frandsen et al., 2023) or evolves through exposure and institutional incentives. Recent work shows that judges update behavior in response to training (Ash et al., 2025; Cao, 2023), experience (Bernstein et al., 2019), and appellate reversals (Bhuller and Sigstad, 2025). We complement this literature by showing that public scrutiny induced by live-streaming can shift judicial decision-making.

Finally, we contribute to the literature on how judicial bias affects firm outcomes (Cahuc et al., 2024; Araujo et al., 2023; Coupé et al., 2021). Related work shows that judicial ideology affects outcomes such as employment, investment, recoveries, and valuation (Bernstein et al., 2019; Huang et al., 2025). Our contribution differs in a key respect: we study the ex-ante effects of judicial transparency. When transparency amplifies pro-government bias, markets and firms anticipate a less favorable legal environment and adjust behavior before disputes arise. To our knowledge, this paper provides the first evidence that judicial transparency has ex-ante real effects on firms, affecting borrowing, investment, and related decisions through changes in expected judicial behavior.

2 Institutional Details

2.1 Legal System in India

The judicial system of India is organized hierarchically. At the apex is the Supreme Court of India (SC hereafter), the final court of appeal and authority on constitutional interpretation. Below the SC, each state and union territory has a corresponding High Court. At the base are district and sessions courts operating at the district and sub-divisional levels.

A defining feature of India's judiciary is its independence and insulation from the executive, most evident in judicial appointments. These are controlled by the judiciary through the "Collegium System" (Tripathy and Dhanee (2020)). In the initial decades after independence, Supreme Court and High Court judges were appointed by the President on the advice of the union government. This changed after 1993, following Supreme Court of India (1993), when the Supreme Court held that neither the executive nor the legislature would have a role in appointments, establishing the collegium system that continues today.

Under this system, High Court appointments are initiated by a collegium comprising the Chief Justice of the High Court and its two senior-most judges. Appointees enter either as Permanent Judges or as Additional Judges. Under Article 224, Additional Judges are appointed to address temporary increases in workload or case arrears, after which they may be elevated to Permanent Judges subject to performance review and vacancy availability. Recommendations for both initial appointments and elevations are then reviewed by the Supreme Court collegium, consisting of the Chief Justice of India and the four senior-most Supreme Court judges. The executive's role is limited to seeking clarifications or returning recommendations for reconsideration; if reiterated, the appointment is binding.[2] Promotions from the High Courts are decided by the SC collegium.

At a deeper level, the basic structure doctrine, established in Supreme Court of India (1973), recognizes judicial independence as part of the Constitution's core that cannot be amended. A proposal to create a National Judicial Appointments Commission, which would have given the government a role in appointments, was struck down by the Supreme Court in 2015, reaffirming judicial self-governance. Judges also enjoy constitutionally guaranteed salaries and tenure until retirement, and removal requires impeachment by Parliament with a two-thirds majority, a threshold never met so far. Tripathy and Dhanee (2020) show that the collegium system substantially limits executive control over judicial careers. Importantly, judges do not face reappointment, promotion, or salary incentives linked to the executive, limiting scope for direct political pressure. Even complaints against judges are adjudicated entirely within the judiciary, with no external oversight.[3]

Our analysis focuses on the High Courts. There are 25 High Courts, each with jurisdiction over one or more states and union territories; the full mapping is provided in Table A1 of the online appendix. India's judiciary operates under a common law system (Djankov et al. (2003)). All High Courts operate under the same constitutional framework, follow uniform procedural rules, and are bound by Supreme Court precedent (Sharafi (2016)), making them comparable despite geographic differences.

High Courts hear appeals from subordinate courts and adjudicate writ petitions involving fundamental rights and review of administrative or legislative actions. Appeals are decided on questions of law rather than fact, i.e., High Courts assess whether lower courts correctly interpreted statutes, applied legal standards, or followed due process, rather than re-evaluating evidence or witnesses. Because such determinations require applying legal principles across varied contexts, judges exercise considerable discretion. This creates scope for factors be-

[2]For an example, see: https://www.ndtv.com/india-news/top-5-judges-reiterate-justice-josephs-name-for-elevation-to-top-court-1850841

[3]For example, see the recent: *Delhi High Court judge cash controversy*

yond strict legal reasoning - such as responsiveness to public scrutiny - to influence judicial decision-making (Aney et al. (2021), Chandra et al. (2018), Gico Jr. et al. (2021)).

2.2 Case Allocation

Prior to 1996, case allocation in the Supreme Court was managed by the registry, raising concerns about "bench hunting." To address this, a random allocation system was introduced. Under this system, the Chief Justice of India assigns subject-matter specializations to judges, cases are categorized accordingly, and allocated to benches with relevant expertise through a computerized, roster-based process. Since multiple benches are linked to each specialization, the system enables randomization (Aney et al. (2021)) and is designed to be manipulation-proof (Supreme Court of India (2017)). High Courts largely follow a similar structure.

2.3 Live-streaming of Courtrooms in India

Calls for greater transparency in the Indian judiciary had been growing for several years, driven by concerns about opacity in court functioning and the need to strengthen accountability. In response, in September 2018 the Supreme Court of India approved the live-streaming of judicial proceedings.[4] The Court noted that "live-streaming of court proceedings would enhance the rule of law and judicial accountability and make justice delivery more transparent and accessible." Justice D.Y. Chandrachud emphasized that increased visibility would discipline judicial behavior. Building on this, in February 2020 the Supreme Court issued an administrative directive instructing all High Courts to adopt similar practices,[5] marking a nationwide push toward audiovisual transparency.

Subsequently, the Court released the *Model Rules for Live Streaming and Recording of Court Proceedings*, which provide procedural and technical guidelines. Proceedings are live streamed as a rule, subject to limited exclusions such as matrimonial disputes, sexual offences, and certain criminal matters.[6] These rules were adopted by the Supreme Court and 13 of the 25 High Courts in a staggered manner between October 2020 and December 2025, corresponding to our sample period.[7] Table A2 in the online appendix lists adoption dates.

[4]Swapnil Tripathi v. Supreme Court of India, 2018. Writ Petition (Civil) No. 123 of 2017.

[5]`https://www.livelaw.in/top-stories/cji-on-administrative-side-sc-on-application-for-framing-live-streaming-rules-152328`

[6]For an exhaustive list, see: `https://cdnbbsr.s3waas.gov.in/s388ef51f0bf911e452e8dbb1d807a81ab/uploads/2021/06/2022091599.pdf`.

[7]Bombay High Court adopted livestreaming in July 2025 but subsequently scaled it back; therefore we exclude it from our main sample, though results are robust to its inclusion. For more, please see: https://www.barandbench.com/news/transparency-v-misuse-bombay-high-court-live-streaming-loses-steam

Proceedings are broadcast in real time via YouTube or official websites, with variations between courts in streaming and archival practices. Figure A1 provides examples.

3 Data

Our analysis draws on multiple data sources, including the text of court judgments, firm-level accounting and financial data, and social media activity. We describe these sources below.

3.1 Data on Judicial Cases

Our primary dataset consists of final judgments of all Indian High Court cases decided between January 2016 and December 2025. We obtain these data from India Kanoon, a legal research organization that compiles judgments from the Supreme Court, High Courts, and select tribunals. The data provide rich case-level information, including the judgment date, court, judicial bench, and the identities of the litigating parties.

Panel A of Table 1 describes the main sample. We identify commercial disputes using a comprehensive keyword list drawn from relevant statutes.[8] This yields 57,457 cases across 25 High Courts between January 2016 and December 2025. We then classify litigants as government or firm based on party names and restrict the sample to government–business disputes. In this setting, "government" includes both central and state entities, such as ministries, departments, statutory authorities, public corporations, and local regulatory bodies. The final sample comprises 10,607 disputes adjudicated by 1,029 judges. Most of the litigation between the government and businesses centers on the state's regulation of business conduct. The most prominent categories are Taxation & Revenue (29.5%), Company Law Compliance (13.1%), and Criminal & Economic Offences (10.8%), comprising over half of all cases. Other notable categories include Financial & Banking (20.1%), Electricity and Land (7.0%), Labor-related disputes (6.0%), and Environmental cases (nearly 3.0%).

Each judgment is segmented into distinct components reflecting the structure of judicial reasoning. We classify the text into seven sections: (1) facts, summarizing the background of the dispute; (2) legal issue, defining the question of law; (3) petitioner's arguments; (4) respondent's arguments; (5) precedents cited; (6) judicial reasoning, where legal principles are applied to the facts; and (7) conclusion, which states the outcome and identifies the winning party. Examples are shown in Figure A2 of the online appendix. To measure judicial sentiment, we focus on sections (1) through (6), which capture the informational content, legal framing, and reasoning underlying the final decision.

[8]Keywords are listed in Tables A3 and A4 of the online appendix.

We use the Conclusion section to identify the winning party. A zero-shot large language model classifies each case as petitioner wins, respondent wins, or ambiguous based on the concluding text. Cases are labeled ambiguous when outcomes involve partial relief, split orders, or do not clearly identify a prevailing party; these are excluded from the main analysis. The final sample comprises 7,996 observations.

The unit of observation varies across analyses. For case outcomes, we use judge–judgment level data. Because multiple judges may preside over a case, the 7,996 disputes yield 9,661 judge–judgment observations. To quantify judicial sentiment, we apply a zero-shot learning framework using the BART-large-mnli model, classifying each judgment along three dimensions: pro-government, pro-market, and neutral. The model assigns scores between 0 and 1 reflecting alignment with each label (details in Section B1 of the online appendix). For text-based analysis, we aggregate to the judge–year level, yielding 1,594 observations.

Panel A of Table 2 reports case-level statistics. The mean government win rate is 22.8%, with average sentiment scores of 0.353 (pro-government), 0.314 (pro-market), and 0.332 (neutral). Panel B presents judge–year aggregates used to analyze changes in judicial bias; the mean pro-government and pro-market scores are 0.355 and 0.312, respectively.

3.2 Tribunals and District Courts

Our lower court data mirror the High Court sample. The data cover 27 Income Tax Appellate Tribunals (ITATs) across Indian states over the same period, as well as district courts in Bengaluru and Delhi. The Bengaluru District Court falls under the Karnataka High Court, which adopted live streaming, while the Delhi District Court falls under the Delhi High Court, which did not. Importantly, neither jurisdiction implemented live streaming at the district court level. Panel B of Table 1 presents the lower court sample. We assemble data from 27 ITATs, yielding 109,917 cases, of which 79,420 involve government–firm disputes. At the district-court level, Bengaluru and Delhi contribute 21,602 commercial cases, including 179 government–business disputes. Table A5 in the online appendix reports summary statistics for district courts (Panel A) and tax tribunals (Panel B), along with High Court-level controls used in subsequent tests (Panel C).

3.3 Social Media Data

For media related analysis, we collect data from the social media platform X. We compile all posts published between January 2023 and December 2024 by two prominent verified legal news outlets, LiveLaw India and Bar & Bench. We also collect geographical information on accounts engaging with these posts. The sample period is determined by API access and

budget constraints.

Panel C of Table 1 describes the social media sample. We collect 70,913 posts, of which 17,052 include video attachments. Panel C of Table 2 reports summary statistics. Average bookmarks, views, likes, quotes, comments, and reposts are 1.408, 11,134, 48.825, 2.190, 4.173, and 13.939, respectively. Median values are substantially lower, while 99^{th} percentiles are much higher, indicating strong skewness. Standard deviations are large relative to means, consistent with power-law patterns in social media engagement (Jo et al. (2013)).

3.4 Firm Level and Loan Level Data

For the firm-level analysis, we use the Prowess dataset from the Centre for Monitoring Indian Economy (CMIE), which provides detailed financial information for listed and unlisted Indian firms, including borrowing, investment, and balance sheet variables. Panel D of Table 1 describes the sample. We begin with 40,750 firms over 2016–2025, yielding 238,813 firm-year observations for borrowing and 263,492 for additions to fixed assets. For additions to plant and machinery and sales, we have 176,943 and 236,968 observations, respectively. Panel D of Table 2 reports summary statistics, with average outstanding borrowing and investment of INR 5640 millions and INR 4239 millions.

For borrowing costs, we complement this with loan-level data from the Ministry of Corporate Affairs (MCA), constructed from statutory loan contract filings. Under the Companies Act (2013), secured loans must be registered through the creation of a charge on borrower assets (Chopra et al. (2021)), making the MCA database a comprehensive source for secured corporate borrowing. The data include interest rates, loan amounts, lender identity, and loan dates. Panel E of Table 1 describes the sample. Matching MCA records to Prowess yields 29,041 loans across 4,486 firms and 198 lenders, with an average interest rate of 9.66%. Panel E of Table 2 reports the state-year controls used in the analysis.

3.5 Economic, Political, and Social Variables

For balance tests, we compile state-level economic, political, and social variables. Economic data are sourced from the Ministry of Statistics and Program Implementation and the Reserve Bank of India, political data from the Election Commission of India, and social indicators from the Ministry of Statistics and Program Implementation. Definitions of all variables and the data sources are detailed in Tables A6 and A7, respectively.

4 Main Result

Our central hypothesis is that live streaming increases the salience of judicial proceedings and, to the extent that judiciary values public legitimacy, induces decisions that align more closely with public opinion. Business–government disputes in India provide a natural setting for two reasons. First, these cases often involve a perceived conflict between public and private interests and center on questions of legal interpretation - such as taxation, capital assets, or contractual obligations - rather than facts, making judicial discretion more relevant. In such settings, the government side may be viewed as representing public interest. Second, survey evidence shows that trust in government in India exceeds trust in corporations, and public narratives have often been skeptical of large firms.[9]

These patterns suggest that greater visibility of judicial proceedings may lead judges to place more weight on perceived public preferences. Recent events illustrate the influence of public opinion on institutional responses. For example, the government's farm law reforms in 2020, which aimed to improve market access for farmers, triggered widespread protests amid concerns that they favored large corporations. Although the legal provisions largely expanded farmers' options - such as allowing exit from contract farming agreements - and the government had clear authority to enact such laws, the reforms were widely perceived as pro-business and were eventually withdrawn. Notably, instead of issuing a definitive ruling, the Supreme Court appointed interlocutors to engage with farmers, reflecting caution in a politically salient setting. These considerations motivate our focus on government–business disputes and support the hypothesis that increased transparency, by amplifying public scrutiny, shifts judicial decisions toward the government.

To examine the above thesis, we use the staggered rollout of live-streaming across Indian High Courts. We use a staggered difference-in-differences design, comparing case outcomes in High Courts that adopted live-streaming earlier (treatment group) with those that adopted later (control group). To implement the empirical test, we organise the data at a judge-judgment level. We estimate the following baseline regression specification:

$$Y_{ijkt} = \beta \cdot Post_{it} + \alpha_i + \gamma_t + \delta_k + X_{it-1} + \varepsilon_{ijkt} \tag{1}$$

where Y_{ijkt} is a binary variable that equals 1 if the government wins the case j in court i presided by judge k in year t and 0 otherwise. $Post_{it}$ is the staggered treatment indicator variable. It equals one for all observations in court i after live-streaming is introduced and zero before introduction, with the indicator switching from zero to one at different time points across different high courts due to the staggered rollout. For high courts that never

[9]See: `https://www.worldvaluessurvey.org/WVSDocumentationWV7.jsp`

adopt live-streaming during our sample period, the variable remains zero throughout.

We include court fixed effects (α_i) to absorb time-invariant differences across High Courts, such as administrative practices and case management. Year fixed effects (γ_t) account for nationwide shocks, including macroeconomic conditions and changes in litigation demand. Judge fixed effects (δ_k) allow identification from within-judge variation over time, comparing outcomes before and after live-streaming adoption. We cluster standard errors at the court level to account for serial correlation within High Courts. Our full specifications include lagged time-varying High Court–level controls (X_{it-1}) capturing economic, political, and judicial conditions, including GDP growth, credit growth, investment-to-GDP ratio, electoral margins, pendency per judge, and pre-period government win rates. These variables are summarized in Table A5 of the online appendix.

The key coefficient of interest, β, captures the change in the probability of a government victory following the adoption of live streaming. Panel A of Table 3 reports the results. Both columns (1) and (2) include court, judge, and year fixed effects. Judge fixed effects do not fully subsume court fixed effects due to judge transfers. Across specifications, the coefficient on the *Post* indicator is positive and statistically significant. In the fully saturated specification (column (2)), the estimate is 0.081, implying an 8.1 pp increase in the probability of a government victory in adopting High Courts relative to non-adopting courts. This corresponds to 37% of the unconditional government win rate and is economically meaningful.

Given our difference-in-differences design, the results do not by themselves distinguish between a new pro-government bias and a correction of a pre-existing anti-government bias. The baseline government win rate of 22.8% may lead a reader to infer the presence of a pre-existing anti-government bias that is now correcting. As discussed in Section 2, our sample consists of appellate cases at the High Courts, which hear appeals from the lower judiciary rather than first filings. Governments may be more likely than private litigants to appeal adverse lower-court decisions given their greater resources and institutional incentives. Government law officers face limited downside from pursuing appeals: litigation costs are borne by the public exchequer rather than the officer or department, and declining to appeal an adverse ruling can attract scrutiny from auditors and oversight bodies, while pursuing an appeal rarely does. The composition of cases reaching High Courts, therefore, tilts toward those the government has chosen to pursue against unfavorable rulings, which mechanically depresses the observed government win rate. In Section 7.2, we show that the ratio of cases lost in lower courts to those appealed in High Courts does not change with live-streaming, indicating that selection into appeals does not drive our results.

To address the mean reversion concern directly, we exploit variation in baseline government win rates across court–law pairs. If the estimates reflect mean reversion, the effects

should be concentrated in settings with low pre-period government win rates and attenuate once this variation is accounted for. We augment Equation 1 by interacting Post with the pre-period government win rate at the court–law level. Columns (3)–(4) of Table 3 show that the coefficient on Post remains stable (0.075–0.079) and statistically significant, while the interaction term is small and statistically insignificant. The pro-government shift is uniform across court–law pairs, which is inconsistent with both mean reversion and correction of prior bias.

4.1 Endogeneity in Adoption of Live-Streaming

A concern with the above analysis is that the adoption of live streaming across High Courts may not be random. Early adopters may differ in institutional, administrative, or political characteristics that also influence judicial behavior. The main identification threat therefore arises from court-level factors - both time-invariant and time-varying - that jointly affect adoption timing and case outcomes. To address this concern, we conduct two sets of tests: (i) tests for pre-existing differences across courts, and (ii) tests for time-varying endogeneity.

4.1.1 Pre-Existing Differences

Our first test in the category of pre-existing differences is an event-study analysis that examines pre-trends. Specifically, we estimate the following specification.

$$Y_{ijkt} = \alpha_i + \gamma_t + \delta_k + \sum_{k=-3, k\neq -2}^{k=-1} \beta_k \cdot \text{Pre}_{k,it} + \beta_0 \cdot \text{Post}_{0,it} + \sum_{k=1}^{3} \beta_k \cdot \text{Post}_{k,it} + \varepsilon_{ijkt} \qquad (2)$$

where Y_{ijkt}, α_i, γ_t, and δ_k are the same as defined in equation 1. The variables $\text{Pre}_{k,it}$ are dummy indicators that take the value one if the judgment in court i occurred k years prior to the adoption of live-streaming. $\text{Post}_{0,it}$ is a dummy for the year of live-streaming adoption, and $\text{Post}_{k,it}$ indicates k years after adoption. We cluster standard errors at the court level.

The coefficients β_k trace the evolution of the probability of a government victory in the years leading up to and following the adoption of live-streaming. We present the estimates in panel A of Figure 1. The horizontal axis denotes years relative to the adoption of live-streaming, while the vertical axis reports the estimated change in the likelihood of a government victory relative to the pre-adoption baseline.

We find that the coefficients on the pre-treatment indicators are statistically indistinguishable from zero and small in magnitude, indicating no systematic pre-trends in government win rates across adopting courts. Starting in the year of adoption ($\hat{\beta}_0$), the coefficient becomes positive and statistically significant, and the effect strengthens in the post-adoption

period. This suggests that live streaming leads to a significant increase in the probability that the government prevails in commercial disputes relative to the pre-adoption period.

Because adoption is staggered across courts, conventional two-way fixed effects event studies can be difficult to interpret. With variation in treatment timing, already-treated courts may serve as controls for later-treated courts, contaminating relative-time coefficients in the presence of heterogeneous effects (Sun and Abraham (2021)). As a result, estimated dynamics may not reflect causal effects at each lead or lag. To address this, we estimate the interaction-weighted event-study specification of Sun and Abraham (2021), which compares treated courts only to not-yet-treated or never-treated courts while allowing for cohort- and time-varying effects. Panel B of Figure 1 shows that our results are robust to this approach.

Our second test examines time-invariant differences between treated and control High Courts by comparing courts and their corresponding states across observable characteristics. We consider four categories - court composition, economic, political, and social factors - each plausibly related to both adoption timing and judicial behavior. Table A8 of the online appendix presents these comparisons.

For court composition, we examine the share of government–business disputes, government win rates, measures of judges' prior pro-government leanings (as described in Section 3), average judicial tenure, the share of judges from the same state, and caseload per judge. Economic factors capture standard state-level macro conditions. Political variables include ruling party majority, state–center alignment, and leadership stability. Social factors include demographics, literacy, education, and health indicators. Across all categories, we find no statistically significant differences between adopting and non-adopting states across 31 characteristics. These results suggest that selection into adoption based on observables is unlikely to drive our findings.

4.1.2 Time-Varying Differences

We next address concerns about time-varying endogeneity. A potential issue is that contemporaneous changes at the judicial level, coinciding with live-streaming adoption, may drive the observed pro-government shift. To address this, we exploit the institutional link between High Courts and lower judicial bodies. High Courts exercise appellate and administrative oversight over district courts and ITATs, implying that court-level shocks should also affect these bodies. We therefore conduct a placebo test using case-level data from district courts and ITAT (Section 3), estimating the same specification as in Equation 1.

Panel B of Table 3 shows that the coefficient on *post* is small and statistically insignificant. This suggests that our results are unlikely to be driven by concurrent time-varying shocks. Taken together with earlier tests, this reduces concerns that our estimates reflect systematic

differences between treated and control courts.

4.2 Text-Based Evidence on Ideological Orientation

We complement the evidence on pro-government tilt in case outcomes by examining whether judges also become more pro-government in their written reasoning under live-streaming (Liu and Tang (2025)). As described in Section 3, we measure the economic orientation of judgment text along pro-regulation (pro-government), pro-market, and neutral dimensions, with scores between 0 and 1. We aggregate the data to the judge–court–year level by computing average scores for each category. We estimate the following specification:

$$\mathrm{Y}_{ijt} = \beta \cdot Post_{it} + \alpha_i + \gamma_t + \delta_j + X_{it-1} + \varepsilon_{ijt} \tag{3}$$

where Y_{ijt} denotes the orientation score for judge j in court i during year t. The remaining variables have the same meaning as in equation 1. The coefficient of interest β measures the average change in the linguistic orientation of judicial reasoning following the adoption of live-streaming.

Panel A of Table 4 presents the results of the sentiment analysis. Even-numbered columns include the full set of controls. In columns (1)–(2), where the dependent variable is the pro-government score, the coefficient on $Post_{it}$ is positive and statistically significant, with a magnitude of 0.027, implying a 2.7 pp increase following live-streaming adoption. This corresponds to about 7.6% of the unconditional mean, indicating an economically meaningful shift in judicial language toward government or regulatory positions. Columns (3)–(4) and (5)–(6) of Panel A report results for pro-market and neutral scores, respectively. The coefficients on $Post_{it}$ are statistically indistinguishable from zero in both cases, indicating no change in pro-market or neutral sentiment. Overall, the evidence points to a selective increase in pro-government language.

Panel C of Figure 1 plots event-study coefficients for the pro-government score. Pre-treatment coefficients are statistically indistinguishable from zero, supporting parallel trends, while post-treatment coefficients are positive and significant. We also address concerns with staggered DiD using the Sun and Abraham (2021) estimator (Panel D), which yields similar results. Together, these findings show that judicial language becomes more aligned with state and regulatory perspectives following live-streaming.

4.3 Composition of Judicial Reasoning

Having documented a pro-government shift in judicial language following live-streaming, we next examine whether this shift is reflected in the composition of judicial reasoning. Specifically, we test whether judges place greater weight on government arguments in their written opinions. If the tilt extends beyond aggregate sentiment, it should appear as greater incorporation of government submissions relative to those of firms. We therefore examine whether the textual prominence of government arguments increases following live-streaming.

We use the structured organization of our corpus of judgment text to define an identification strategy for the above. This segmentation allows us to isolate the government's factual narratives and the judge's subsequent synthesis of those facts and arguments. Following Kelly et al. (2021) and Chen et al. (2025), we develop a text similarity–based measure of judicial incorporation. For each judgment j, we extract three textual components: (i) the petitioner's factual narrative, (ii) the respondent's factual narrative, and (iii) the set of facts and reasoning acknowledged by the judge in the judge's interpretation section. Using these components, we compute cosine similarity scores between the vectorized text representations of the judge's recognized facts and those of each party. Text is represented using term frequency–inverse document frequency (TF–IDF) weighted embedding. TF-IDF method is used to determine the importance of a word within a text piece compared to a collection of such text pieces (corpus). It is calculated by multiplying two values: Term Frequency (TF), which measures how often a word appears in a specific document, and Inverse Document Frequency (IDF), which measures how rare a word is across the entire corpus.

Formally, we construct similarity measures that capture the textual alignment between the judge's reasoning and the factual narratives advanced by each side. Let V_{Judge} denote the embedding of the judicial reasoning section, and V_{Gov} and V_{Corp} denote the embeddings of the government's and corporate party's factual submissions, respectively. We compute $\text{Similarity}_{Gj} = \text{sim}(V_{\text{Judge}}, V_{\text{Gov}})$, $\text{Similarity}_{Cj} = \text{sim}(V_{\text{Judge}} and V_{\text{Corp}})$, where $\text{sim}(\cdot)$ represents the cosine similarity between the corresponding embedding vectors. We then define $Similarity\ ratio_j$ as the ratio between $Similarity_{Gj}$ and $Similarity_{Cj}$, which measures the relative degree of incorporation of government versus corporate narratives in judgment j. A higher value of $\text{Similarity ratio}_j$ indicates that the judge's reasoning more closely reflects the government's submissions relative to those of the corporate litigant.

We first validate this measure by formally examining its relationship with case outcomes. Specifically, we test whether higher relative similarity to the government's narrative predicts a greater likelihood of a government victory in the pre-livestreaming period (before 2020).

To do so, we estimate the following regression:

$$Win_{ijkt} = \beta \cdot \text{Similarity ratio}_{ijkt} + \alpha_i + \gamma_t + \delta_k + \varepsilon_{ijkt}, \tag{4}$$

where Win_{ijkt} is an indicator equal to one if the government wins case j in court i handled by judge k in year t, and zero otherwise. Similarity ratio$_{ijkt}$ is the relative textual similarity between the judge's reasoning and the government's submissions compared to those of the corporate litigant, as defined above. Rest of the variables are as defined in equation 1.

We report the results in panel B of Table 4, with all specifications including Court, Year, and Judge fixed effects and column (2) additionally including controls from Equation 1. The coefficient β is positive and statistically significant in both columns. In column (1), a one–standard-deviation increase in Similarity ratio$_{ijkt}$ is associated with a 4 pp higher probability of government success. This robust correlation supports interpreting the similarity ratio as a measure of judicial attention and alignment toward government narratives.

To estimate how live-streaming influences this relative incorporation, we employ the difference-in-differences specification similar to equation 1, except that the dependent variable is Similarity ratio$_{ijkt}$, which measures the relative incorporation of government versus corporate narratives in case j heard by judge k in court i at time t. The rest of the variables carry the same definition as defined in section 1. Standard errors are clustered at the court level. The key coefficient of interest, β, measures the differential change in judicial incorporation of litigant statements following the adoption of live-streaming. We report the results in panel C of Table 4. Column (4) presents the specification with court, judge, and year fixed effects, and the full set of control variables. We find that the introduction of live-streaming increased the similarity between judges' recognized facts and the government's statements relative to those of firms. The magnitude of the effect implies that Similarity ratio increases by approximately 1.72 units, equivalent to about 50% shift relative to the pre-reform mean, indicating an economically significant increase in the textual alignment of judicial reasoning with government narratives in treated courts.

A natural concern is that the increase in similarity between government submissions and judicial reasoning may mechanically reflect case outcomes. Judges may devote greater attention to the arguments of the winning party, so if live-streaming raises the probability of government victories, similarity could increase even without any change in judicial reasoning. We address this by examining whether the increase in similarity persists conditional on case outcomes. In columns (5)–(6) in Panel C of Table 4, we estimate the effect separately for cases where the government wins and where it does not. This allows us to compare judgments holding the outcome constant. The increase in similarity persists in both subsamples,

indicating that the post-live-streaming rise in alignment with government narratives is not solely driven by higher government win rates.

A related concern is that the increase in similarity reflects changes in the language of government submissions rather than judicial incorporation. We address this in two ways. First, we compare post-reform submissions to pre-reform judgment text at the court level. We find no differential change in the relationship between judgment text and submissions for government relative to firms, in either treated or control courts, ruling out shifts in submission language. Second, we examine the length and sentiment of government and business submissions before and after the reform and find no significant changes. Detailed results are provided in Section B2 of the online appendix. Overall, these results provide evidence consistent with the aggregate linguistic shifts documented earlier.

5 Mechanism

In terms of the mechanism, our central thesis is that live-streaming makes judicial proceedings more visible and accessible to the public, prompting judges to become more attentive to public perceptions. In an environment where the optics of siding with private interests can be politically or socially unpopular, judges may avoid rulings that appear to favor business over the state. Consider, for instance, a mining dispute where the case concerns the division of revenue between the government and a large private firm. In such cases, the public may perceive judgments favoring business as privileging private profit over public welfare. This dynamic is particularly pronounced in India, where public faith in government and state institutions remains exceptionally high, as noted in Section 4.

5.1 Social Media Amplification

To examine this mechanism, we first investigate whether video content from live-streamed cases attracts greater public attention than standard text updates on social media. We collect data from two prominent legal journalism accounts on the social media platform *X* (formerly Twitter): *LiveLaw India* and *Bar & Bench*. These accounts routinely share real-time updates, video clips, and summaries of ongoing court proceedings. Our identification idea is straightforward: if posts containing video clips of hearings generate higher engagement than those without videos, this provides direct evidence that live-streamed cases attract greater public attention.

As noted in Section 3, we compile a dataset of social media posts from these two accounts covering judicial proceedings between January 1, 2023, and December 31, 2024. For each

post, we record engagement metrics, including the number of likes, reposts, and comments. We estimate the following regression specification with the above data at a post level:

$$\text{Engagement}_{it} = \beta \cdot \text{Media}_i + \alpha_h + \gamma_t + \varepsilon_{it} \tag{5}$$

where Engagement_{it} is the total number of likes, retweets, and comments associated with post i in month t. The variable Media_i is an indicator equal to 1 if the social media post i has a video attached to it and 0 otherwise. We include hour-of-day fixed effects (α_h) to control for within-day variation in user traffic and visibility. We also include month fixed effects (γ_t) to capture broader temporal variations and news cycles in social media dynamics. Standard errors are clustered at the month level.

We present the results in columns (1)–(3) of Table 5. In column (1), without fixed effects, the coefficient on Media_i is positive and statistically significant, indicating that posts with video receive substantially higher engagement than non-video posts. The magnitude, 16,668.30, implies that live-streamed cases attract at least 1.47 times greater amplification. In column (3), including month and hour-of-day fixed effects, the coefficient remains positive, statistically significant, and qualitatively unchanged. These results provide consistent evidence that live-streaming increases public engagement with judicial proceedings.

A natural concern with the general engagement result is geographic relevance. If the heightened engagement for a specific High Court's proceedings is driven entirely by out-of-state viewers, the local political and social pressure on the presiding judge is arguably weaker. For the institutional legitimacy mechanism to hold, transparency must amplify visibility among the court's immediate local public. To test this, we map the social media posts to specific High Courts using keyword and hashtag matching. Due to platform restrictions on X, we cannot observe the location of users who simply like or repost. Therefore, we restrict our focus to the comments on each post, extracting the self-reported geographical locations of the commenting profiles. Arranging the data at the post level, we calculate the proportion of comments originating from the same state as the focal High Court. We estimate the following regression:

$$\text{LocalShare}_{ijt} = \beta \cdot \text{Media}_i + \delta_j + \gamma_t + \varepsilon_{ijt} \tag{6}$$

where $LocalShare_{ijt}$ is the proportion of geographically identifiable comments on post i, regarding High Court j in month t, that originate from users located in High Court j's jurisdiction. We include High Court fixed effects (δ_j) to control for baseline differences in local engagement across states, and month fixed effects (γ_t) to account for time trends. Standard errors are clustered at the High Court level.

We report the results in columns (4)–(5) of Table 5. Column (5) includes the full set of fixed effects. The coefficient on $Media_i$ is positive and statistically significant ($\beta = 0.310$), indicating that video posts increase the local share of engagement by 31 pp relative to text posts. This suggests that live-streaming not only attracts broader attention but also increases engagement within the court's jurisdiction.

While the social media analysis captures indirect amplification, a stricter test of public scrutiny is whether citizens directly engage with courts' primary broadcasts. To assess this, we collect video-level engagement metrics - views, likes, and comments - from the official YouTube channels of High Courts that live-stream proceedings. Summary statistics, reported in Table A9 of the online appendix, indicate substantial public attention, with average viewership of 384,718 (SD = 901,264) per high court-month and 1,684 interactions per session. These patterns suggest an active audience directly engaging with court proceedings. Taken together, the evidence indicates that live-streaming creates a highly visible environment in which judges operate under greater public scrutiny.

5.2 Strength of Local Government and Case Outcome

A key question is why responsiveness to public opinion would translate into a pro-government tilt in judicial decisions. As noted above in this section, in business–government disputes, public sentiment is often more aligned with the state, a pattern reinforced by popular narratives - including media and films - that have historically portrayed large businesses with scepticism. We exploit this asymmetry to design a cross-sectional test. If the public opinion does align with the state, then our results should be stronger in states where the state government is highly popular. In contrast, narrow electoral margins reflect a more divided electorate and should attenuate the effect found in this paper. Importantly, support for state governments need not track support for the Union government. We therefore expect this moderating effect to arise primarily in cases involving state governments, where local mandates are directly relevant, and not in cases involving the central government.

We examine this hypothesis using the following triple-differences specification.

$$\begin{aligned} Y_{ijkt} =& \beta 1 \cdot Post_{it} \times Margin_{jt} \times State_gov_i + \beta 2 \cdot Post_{it} \times Margin_{jt} + \beta 3 \cdot Post_{it} \\ & \times State_gov_i + \beta 4 \cdot Margin_{jt} \times State_gov_i + \beta 5 \cdot Post_{it} + \beta 6 \cdot Margin_{jt} \\ & + \beta 7 \cdot State_gov_i + \alpha_i + \gamma_t + \delta_k + X_{it-1} + \varepsilon_{ijkt} \end{aligned} \tag{7}$$

Where the variable $State_gov_i$ takes a value of one when case i involves a state government entity and zero otherwise. The variable $Margin_{j,t}$ is the percentage difference between the number of electoral seats won by the ruling party in the state and the number of seats

won by the runner-up in the last election held before year t in state j. The higher the difference, the stronger the political mandate for the ruling party. It is important to note here that we interact the indicator that identifies the state government with $post_{it}$ and $margin_{jt}$ because the high courts hear cases involving both central and state government entities. Hence, incorporating indicator for state government involvement allows us to test whether the incremental effect of government strength is particularly pronounced when the litigating government actor is the state.

The rest of the variables are as defined in equation 1. The coefficient of interest is β_1, which represents the increase in differential likelihood of the government winning after the introduction of livestreaming in cases involving local government as the strength of the ruling party increases. If the likelihood of an increase in wins after the introduction of live-streaming is higher in strong local government, we expect the coefficient $\beta 1$ to be positive and statistically significant.[10]

Table 6 reports the results. All specifications include court, judge, and year fixed effects, with column (2) additionally including controls from Equation 1. The coefficient on $post_{it}$ remains positive and statistically significant, indicating a general increase in government victories after live-streaming. More importantly, the interaction coefficient β_1 is positive and significant at the 5% level, implying a stronger pro-government tilt in states with higher electoral support, particularly in cases involving non-central government entities. A one standard deviation increase in electoral margin is associated with a 12 pp increase in the likelihood of a government win.

A natural concern is that electoral margin may be correlated with state characteristics - such as administrative capacity, economic performance, or judicial composition - that independently affect case outcomes. We address this in three ways. First, we compare high- and low-margin states based on pre-determined observables. High-margin states are those where the average seat margin exceeds the median during the live-streaming period. Using the same characteristics as in Section 4.1, we find that the two groups are similar across 24 of 30 variables (Table A10), indicating no systematic differences.

Second, we augment the baseline specification by including the observables that differ across groups - proportion of government–business cases, pendency per judge, GDP growth, credit growth, an indicator for national-party governments, and unemployment - and their interactions with $Post_{it}$. As shown in column (3) of Table 6, the coefficient of interest remains stable. We also control for Union government electoral strength and its interaction with $Post$, with similar results. Finally, we restrict the sample to high-margin states, excluding

[10]In section B3, we also show the results with specification involving double interaction between *post* and *margin* within a sample of state government cases.

closely contested elections. Re-estimating the triple-difference model yields similar estimates (column (4) of Table 6).

5.3 Judicial Career Incentives and Institutional Legitimacy

We next examine what drives the observed pro-government shift in judicial behavior. In particular, we distinguish between individual incentives tied to government rewards and institutional incentives linked to maintaining judicial legitimacy (Aney et al., 2021; Garoupa and Ginsburg, 2015). Given that the tilt is toward the government rather than private parties, any individual benefits are unlikely to take the form of direct monetary transfers. Instead, they are more plausibly associated with post-retirement opportunities offered by the government, such as positions in tribunals, commissions, statutory bodies, or public sector boards. If judges respond to such incentives, the effect should be concentrated among those nearing retirement. In contrast, if the shift reflects institutional incentives within the judiciary, it should be strongest among judges approaching tenure confirmation.

To test the internal incentive channel, we exploit variation in judicial recruitment. Since Additional Judges in the high courts are subject to confirmation, it creates strong career incentives to perform and signal alignment with institutional preferences. The confirmation process typically takes one to two years, and the evaluation begins well before the formal confirmation date. During this period, judges are assessed based on criteria that are not public. Since we do not observe the exact start date of these deliberations, we use the pre-period median tenure as a cutoff to distinguish judges who are likely to be in the early versus later stages of this process. Judges with tenure below the median are early in their tenure and are unlikely to have entered the final stage of confirmation deliberations. For them, confirmation is still uncertain, and current performance is likely to matter. In contrast, judges with tenure above the median are close to the typical confirmation point, where decisions are largely settled. By this stage, judges are likely to have already received clear signals about their outcome—whether positive or negative—and additional effort is unlikely to significantly affect the final decision. Therefore, the below-median group captures judges who are still in the high-uncertainty, high-incentive phase, while the above-median group captures those in a lower-uncertainty, lower-incentive phase. If internal career concerns are important, the behavioral response should be stronger among judges below the median threshold.

To test this, we identify Additional Judges from High Court records and define *Tenure Threshold* as an indicator equal to one for judges whose tenure falls below the pre-period median time

to confirmation (1.7 years), and zero otherwise. We estimate the following specification

$$\begin{aligned} GovWin_{ijkt} = \beta_1(Post_{it} \times TenureThreshold_k) + \beta_2 Post_{it} + \beta_3 TenureThreshold_k \\ + \theta X_{ct} + \alpha_i + \gamma_t + \delta_k + \varepsilon_{ijkt} \end{aligned} \tag{8}$$

The data are organized at the judge–judgment level, with variables defined as in Equation 1. Columns (1)–(2) of Table 7 report the tenure-track results, with even-numbered columns including controls. Consistent with the institutional legitimacy hypothesis, the coefficient on $Post_{it} \times TenureThreshold_k$ is positive and statistically significant. The coefficient on $Post_{it}$ also remains positive, indicating a broad shift across judges.

A concern is that the results reflect caution among inexperienced judges rather than tenure-related incentives. To test this, we re-estimate Equation 8 on a subsample of permanently appointed judges, for whom confirmation incentives are absent. Under the institutional incentive channel, we expect no effect; under the inexperience channel, the effect should persist. Columns (3)–(4) of Table 7 show coefficients that are statistically indistinguishable from zero. This falsification test rules out inexperience as the driver and supports the institutional incentive mechanism.

We then examine the post-retirement incentive channel. To test this hypothesis, we estimate the specification similar to equation 8, except that $TenureThreshold_k$ is replaced with Ret_k, which takes a value of one if a judge is within one year of the mandatory retirement age of 62, and zero otherwise. Columns (5)–(6) of Table 7 report the results. We find no evidence that judges nearing retirement exhibit a differential pro-government shift following live-streaming: the coefficient on $Post_{it} \times Ret_k$ is statistically indistinguishable from zero. In contrast to the tenure-track results, where the effect is present for judges approaching confirmation, this pattern is inconsistent with a post-retirement incentive channel. Instead, the evidence is more consistent with an institutional response to heightened public scrutiny.

6 Real Effects

If live-streaming increases the likelihood that courts tilt toward the government in business–state disputes, this can generate ex-ante real effects through changes in expected legal outcomes. Firms undertake investment and borrowing decisions under the possibility of future disputes with the state. A higher probability of adverse judicial outcomes lowers the expected payoff from such projects, especially those reliant on regulatory approvals, contract enforcement, or protection against state action. As a result, firms may scale back or forgo marginal investments that are viable only under more balanced enforcement. This channel

also operates through financing conditions. Lenders, anticipating a higher likelihood of adverse outcomes, may demand higher risk premia or tighten credit, leading firms to reduce borrowing and face higher borrowing costs. Taken together, these responses imply that shifts in judicial behavior can affect real economic activity ex-ante.

A key implication is that effects should vary with firms' exposure to courts. Many firms have limited interaction with courts and are unlikely to adjust behavior, whereas firms in industries more reliant on legal and regulatory processes are more sensitive to such shifts. To capture this, we construct an industry-level measure of legal intensity based on pre–live-streaming legal expenses, providing a predetermined proxy for reliance on courts. We then test whether the effects are concentrated among firms in high legal-exposure industries.

To examine whether the effects of live-streaming are concentrated among firms with greater legal exposure, we assemble a firm-year dataset and assign firms to High Courts based on the state of registration. Using Prowess data, we compute for each two-digit NIC industry the average ratio of legal to total expenses in 2020, the year preceding live-streaming adoption. We classify industries with above-median values as high legal exposure and define $High_legal_i$ accordingly. This measure captures the extent to which firms operate in environments with greater legal disputes, compliance burdens, and regulatory interaction.

We then estimate the following regression equation.

$$Y_{ijt} = \beta_0 + \beta_1 \cdot Post_{jt} \times High_Legal_i + \alpha_i + \lambda_{jt} + X_{it} + \epsilon_{ijt} \tag{9}$$

The dependent variable Y_{ijt} represents various firm outcomes for firm i in state j in year t. Specifically, we use the: (i) the natural logarithm of total borrowings, (ii) the natural logarithm of gross fixed asset additions, (iii) the natural logarithm of additions to plant, machinery, land, and buildings, and (iv) the natural logarithm of sales revenue, as dependent variables in different tests. The variable $Post$ is defined as in equation 1. The regression includes firm fixed effects (α_i) to control for time-invariant firm characteristics and state $\times$ year fixed effects (λ_{jt}) to absorb aggregate shocks that vary across regions over time. Standard errors are clustered at the state level. The interaction term $Post \times High_Legal_Expenses_i$ captures the differential effect of live-streaming adoption on firms with greater exposure to judicial proceedings. The coefficient β_1 identifies whether the real effects of judicial bias are concentrated among firms more likely to be affected by court decisions.

Table 8 reports the results. Across all specifications, the coefficient on $Post \times High_Legal_i$ is negative and statistically significant. Firms in high legal-exposure industries reduce both borrowing and investment following the adoption of live-streaming. In column (2), where the dependent variable is log borrowings, the estimates imply a 13.3 pp decline. Columns

(3)–(6) show similar patterns for investment: additions to fixed assets and plant and machinery decline, with the coefficient in column (4) indicating a 10.3 pp reduction in fixed asset investment and column (6) indicating a 6.3 pp decline in plant and machinery investment. Columns (7)–(8) show that sales also decline by about 6.6 pp for high-exposure firms. The results are robust to the inclusion of firm-year controls (even-numbered columns) and fixed effects. Overall, the evidence indicates that firms with greater legal exposure contract borrowing and investment following live-streaming, consistent with a deterioration in expected legal outcomes.

For tests related to borrowing costs, we use loan-level data on borrowing charges filed with the Ministry of Corporate Affairs as described in section 3. Each observation represents a specific loan from a creditor to a firm on a given date. We estimate an analogous specification:

$$InterestRate_{iljt} = \beta_0 + \beta_1 \cdot Post_{jt} \times High_Legal_i + \alpha_i + \gamma_{lt} + X_{it} + \delta_{jt} + \epsilon_{iljt} \tag{10}$$

where Interest Rate$_{ilt}$ is the interest rate on a loan from creditor l to firm i in state j on date t. γ_{lt} and δ_{jt} denote creditor-by-year-month and state-by-year-month fixed effects, respectively to control for time-varying lender and state-specific conditions. Other variables carry the same meaning as explained in equation 9. Standard errors are clustered at the state level.

Columns (9)–(10) of Table 8 report the effects on interest rates. Loan-level data are available only up to 2023. Both columns include fixed effects, with column (10) also including time-varying firm controls. The interaction term Post$_{jt}$ × High Legal Expenses$_i$ is positive and statistically significant in column (10), with a coefficient of 0.545. Given a mean interest rate of 9.66 percent, this implies a 5.64 percent increase relative to the mean for high-exposure firms. The simultaneous decline in borrowing and rise in interest rates is consistent with a contraction in credit supply, as lenders adjust pricing in response to higher legal and regulatory risk.

6.1 Pre-Trends

Further, we also test whether the results documented above are not driven by any pre-existing trends. To test the thesis, we implement an event study framework. The specification is given below.

$$Y_{ijt} = \beta_0 + \sum_{n=-4, i\neq -2}^{n=4} \beta_n \cdot Year_{jn} \times High_Legal_i + \alpha_i + \lambda_{jt} + X_{it} + \epsilon_{ijt} \tag{11}$$

Where $Year_{jn}$ represents the nth year relative to high court j's adoption of live-streaming. The rest of the variables are as in equation 9. We present the plots of β_n in Figure 2. We also estimate an analogous event study specification for the interest rate analysis in equation 10. We do not find any pre-existing trends in any of the measures of firm performance. The plot is presented in panel E of Figure 2. The effects start showing only after the adoption of live-streaming by the respective courts.

6.2 Alternative Measure of Exposure

We further assess robustness using an alternative measure of legal exposure constructed from our case-level data. Using High Court data from the pre–live-streaming period (2016–2020), we count the number of firms in each industry involved in legal disputes and classify industries into above- and below-median groups. We redefine *high_legal* accordingly and re-estimate Equation 9. The results, reported in Table A11 of the online appendix, are qualitatively similar to the main specification, confirming robustness to this alternative proxy.

Taken together, these findings show that increased pro-government judicial bias has meaningful real effects on firm behavior, particularly for firms more exposed to legal and regulatory risk. The declines in borrowing, investment, and sales, along with higher borrowing costs, are consistent with a deterioration in financing conditions and overall performance.

6.3 Ex-Post Impact

Having established that firms with higher ex-ante legal exposure reduce investment and sales following live-streaming, we next examine ex-post effects for firms. Firms involved in ongoing disputes face direct exposure to judicial outcomes and therefore bear the immediate consequences of any pro-government shift. If live-streaming increases the likelihood of adverse rulings, their performance should be adversely affected.

To examine this, we construct measures of firm performance relative to industry peers. This isolates within-industry variation and ensures effects are over and above the industry-level ex-ante impacts documented in Section 6. Specifically, we compute the difference between a firm's year-on-year growth and the corresponding industry average for gross fixed assets, plant and machinery, and sales. We estimate the following specification on a sample restricted to firms involved in litigation during our sample period.

$$Y_{ijt} = \beta_0 + \beta_1 \cdot Post_{jt} \times Post_Case_{ijt} + \beta_2 \cdot Post_Case_{ijt} + \alpha_i + \lambda_{jt} + X_{it} + \epsilon_{ijt} \qquad (12)$$

Where the dependent variable Y_{ijt} represents the performance of firm i relative to its industry

peers, as defined above. The variable $Post_Case_{ijt}$ takes the value one for all years following the first appearance of firm i's case in court j, and zero otherwise. The remaining variables are defined as in equation 9. The coefficient β_1 captures the differential change in post-litigation relative performance for firm i after court j adopts live-streaming. If litigated firms experience a greater deterioration in performance under the live-streaming regime, we expect β_1 to be negative across all measures of relative firm performance.

We report the results in Panel A of Table 9. All specifications include the control variables listed in equation 9, along with firm and state $\times$ year fixed effects. Across all columns, we find that the coefficient on the interaction term is negative and statistically significant. These results indicate that litigated firms experience a larger decline in performance relative to their industry peers following the introduction of live-streaming.

To ensure that the above findings are not driven by other time-invariant or time-varying factors, we conduct a placebo test using data from the ITATs data described in Section 3. Since live-streaming was not introduced in the ITAT during our sample period, we should not expect to observe similar effects in this setting. We therefore estimate a specification similar to equation 12, restricting the sample to firms involved in ITAT cases. We define *post* based on whether the High Court, exercising jurisdiction over the respective ITAT, introduced live-streaming in that year. The dependent variables remain the same as in equation 12. We report the results in Panel B of Table 9. As expected, we do not find evidence of a relatively larger decline in the performance of litigated firms in the ITAT sample. Taken together, this evidence complements our ex-ante results by showing that, following the introduction of live-streaming, litigated firms are more likely to experience a decline in performance relative to their industry peers.

A natural concern is whether the real effects reflect only a predictable decline in firms' probability of success or also an increase in uncertainty of judicial outcomes. This distinction matters, as greater uncertainty can raise risk premia and depress investment beyond the effect of lower expected success rates. To distinguish these channels, we construct a machine-learning model that predicts case outcomes using only the textual content of government and business submissions, excluding judicial reasoning and final judgments. The model is trained on pre-reform cases (2000–2019) and achieves an AUC of 71% on a hold-out sample. When applied to post-reform cases in control High Courts, predictive accuracy remains strong (AUC = 66%). In contrast, for post-reform cases in adopting courts, the AUC falls to about 52%, no better than random. This sharp decline suggests that live-streaming not only reduces business success rates but also increases outcome unpredictability, providing an additional channel for the observed real effects.

7 Robustness

7.1 Livestreams from YouTube

A critical identification challenge remains to our main result is whether the observed pro-government shift represent a direct response to actual, real-time public observation, or does the mere installation of cameras alter judicial behavior across all cases, regardless of whether they are broadcast? Ideally, observing the exact broadcast status of every case would allow us to disentangle the direct effect of live-streaming from broader institutional spillovers. However, the official judgment copies do not record whether a case was actually live-streamed, nor do the High Court websites maintain systematic logs of broadcasted cases.

To address this and identify the direct treatment effect, we construct a novel dataset of verified, live-streamed proceedings. We collect the complete archive of available videos from the official YouTube channels of all treated High Courts (we document the total volume of videos collected and the specific archiving practices of each High Court in Panel A of Table A12 of the Online Appendix). While High Courts mandate the live-streaming of commercial proceedings, archiving practices naturally vary. Uploaded videos typically contain an uninterrupted, day-long stream of a courtroom rather than discrete, case-specific files, and some proceedings may be excluded due to sensitive matters or technical interruptions. We confirm that none of the high courts in the control group have livestreamed videos on their channels.

To map these videos to our case data, we transcribe the audio from the day-long YouTube recordings into text. Using natural language processing techniques, we match the transcribed dialogue and spoken case numbers from the videos against the official text of the judgments in our primary dataset. We describe the algorithm of matching in section B4 of online appendix. Through this procedure, we successfully identify and match 44.76% of the commercial cases decided during the post-treatment period to a verified, archived video recording. The unmatched cases in the post-treatment period were not necessarily un-streamed; rather, they represent cases for which archived recordings were either not uploaded to YouTube or where audio quality prevented a reliable algorithmic text match. In particular, some courts - such as Patna and Kerala - do not systematically store or upload past proceedings. These features imply that our youtube archive based identification is necessarily incomplete and should be interpreted as a lower bound. In the institutional setting, the default presumption is that all commercial cases are live-streamed; however, we only observe the subset that is both streamed and subsequently archived in accessible form. Nonetheless, this matched subsample provides a unique, definitive subset of cases that were broadcast to the public.

Then we re-estimate our primary specification (Equation 1) using only the matched

subsample of confirmed live-streamed cases as the treated group. We report these results in Panel B of Table A12 of the Online Appendix. The coefficient on the Post indicator remains positive and statistically significant. The magnitude of the effect in this restricted sample is 0.078, indicating that in cases definitively known to have been broadcast and archived, the likelihood of a government victory increases by 7.8 pp. This result closely mirrors our main findings and provides compelling evidence that the observed pro-government bias is a direct consequence of judges adjudicating under the explicit scrutiny of a live audience.

7.2 Government Appeal Strategy

A potential concern is that the increase in government win rates reflects a change in the government's appeal strategy rather than a shift in judicial behavior. Following the introduction of live-streaming, the government may have become more selective in filing appeals, pursuing only cases with a high probability of success in order to avoid public embarrassment. Such strategic selection could mechanically raise observed government win rates in government–business cases without any change in judges' response.

We address this concern in two ways. First, we examine cases decided within the first year following the adoption of live-streaming. Appeals resolved during this window were likely filed before it became known that the court would implement live-streaming and are therefore less likely to reflect strategic adjustments by litigating parties. We modify Equation 1 by splitting the post-adoption period into two indicators: 1) $post1$ representing the first year after implementation, and 2) $post2$ representing the remaining post period. All other elements of the specification remain unchanged. Table A14 in the Online Appendix reports the results. The coefficients on both $post1$ and $post2$ are positive and statistically significant. The effect also presents itself in cases that were plausibly appealed before the introduction of live-streaming. Hence, the effect is less likely to be due to a change in the government's appeal strategy after the introduction of live-streaming.

Second, we directly test whether the government became more selective in appealing adverse decisions. To do so, we construct a government appeal ratio using data from the ITATs and the High Courts. Assuming that all tax cases in the High Courts are inherently appeals, we take the total count of these government-filed appeals in the High Courts and divide it by the total number of cases the government lost at the ITAT level. If the government became more cautious and selective post-reform, this appeal ratio should decline following the adoption of live-streaming. Furthermore, to account for the procedural lag in filing appeals, which could mean that cases appearing early in the post-reform period were actually filed beforehand, we conservatively redefine the $Post$ period to begin one year after the adoption

date. We estimate Equation 1 using this appeal ratio as the dependent variable. The results, reported in columns (3) and (4) of Table A14, show that the post-adoption coefficients are statistically insignificant, indicating no measurable change in the government's propensity to appeal. Taken together, these findings indicate that changes in government appeal strategy are unlikely to explain the observed increase in government win rates.

8 Conclusion

This paper examines how expanding public visibility of judicial proceedings reshapes judicial behavior in a setting where courts are formally insulated from political control. We show that increased transparency leads to a systematic shift in case outcomes in favor of the government and is accompanied by greater alignment of judicial reasoning with government narratives. These findings suggest that transparency alters the basis of judicial accountability, shifting it away from institutional structures toward public perception. In doing so, transparency does not merely reveal judicial decision-making; it changes it. The effects are stronger in states where incumbent governments enjoy higher public support, consistent with judges responding to prevailing public sentiment. Moreover, the shift is more pronounced among judges close to tenure confirmation and promotion - both determined by the senior judiciary - indicating that concerns about legitimacy within the judicial system play an important role. In contrast, we find no such effects for judges nearing retirement, who are more likely to receive post-retirement appointments from the government, suggesting that our results are unlikely to be driven by direct political influence.

We also document economically meaningful real effects, with the strongest responses occurring ex ante. Firms exposed to courts that adopt live streaming adjust behavior in anticipation of a less favorable judicial environment, reducing borrowing and investment and facing higher financing costs. These responses are most pronounced in sectors with greater legal exposure, consistent with firms internalizing changes in expected enforcement and regulatory risk. Ex post, firms engaged in disputes in these courts experience weaker performance relative to their peers, including lower investment and borrowing, higher interest rates, and credit downgrades. Together, these findings indicate that shifts in judicial behavior induced by transparency affect not only realized outcomes in litigation but also firms' forward-looking decisions, highlighting the broader economic consequences of judicial transparency.

Our findings should be interpreted in light of two limitations. First, while our evidence is consistent with the judiciary seeking legitimacy through public opinion, we cannot directly establish why they place weight on public sentiment. Second, we cannot fully distinguish whether live streaming introduces a pro-government bias or corrects a pre-existing anti-

government bias. While our textual evidence and the stability of results across court–law combinations point toward a pro-government tilt, our difference-in-differences design does not allow us to generalize this interpretation across all outcomes. Taken together, our results indicate that transparency influences judicial independence, but the precise mechanisms and normative implications remain open for further investigation.

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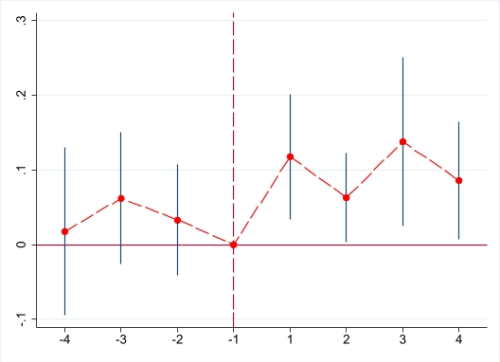
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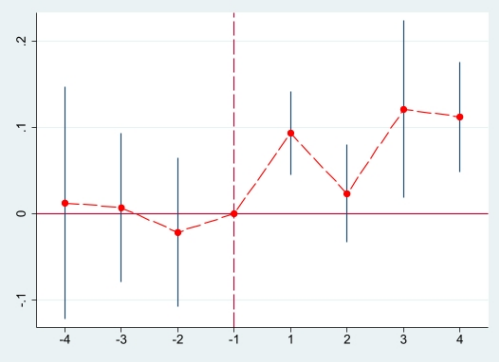
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Figure 1: In this figure, we plot the event study coefficients for the specification testing change in government win probability and the sentiment in the judicial text after the implementation of live-streaming. Panel A plots the coefficients for the specification with an indicator variable identifying government win as the dependent variable, Panel C plots the coefficients for the specification with pro-government sentiment as the dependent variable. In panels B and D, we plot the event study coefficients for the specification testing change in government win probability and the change in pro-government sentiment in the judicial text after the implementation of live-streaming using the Sun and Abraham (2021) methodology, respectively. Panel B plots the coefficients for the specification with an indicator variable identifying government win as the dependent variable, Panel D plots the coefficients for the specification with progovernment sentiment as the dependent variable.

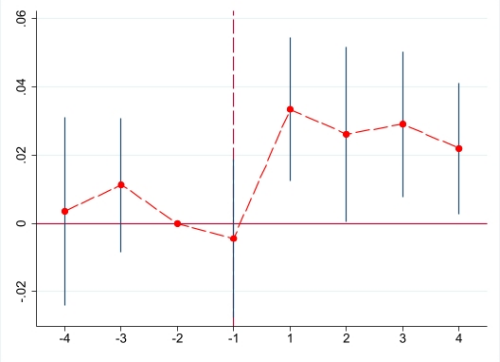
(a) Panel A: Government Win



(b) Panel B: Sun and Abraham (2021) - Government Win



(c) Panel C: Sentiment



(d) Panel D: Sun and Abraham (2021) - Sentiment

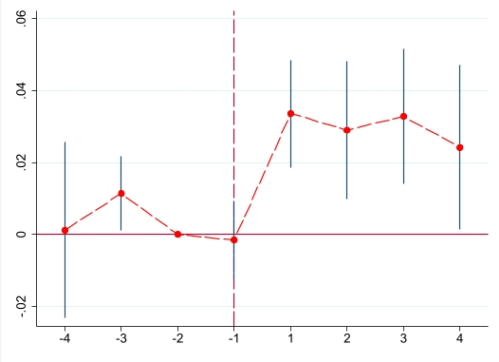
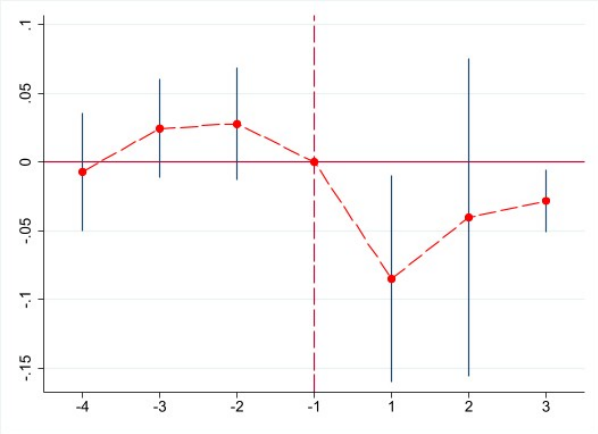
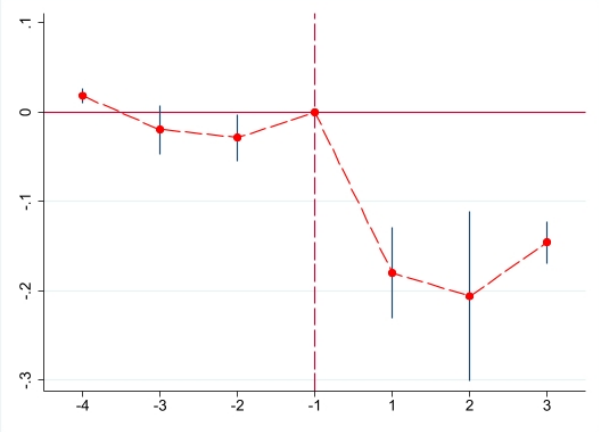


Figure 2: In this figure, we plot the event study coefficients for firm outcomes. Panel A plots the specification with log borrowing as the dependent variable, Panel B plots the specification with log gross fixed assets as the dependent variable, Panel C plots the specification with log gross plant and machinery as the dependent variable, and Panel D plots the specification with log sales as the dependent variable. In panel E, we plot the event study coefficients for interest rates. It plots the coefficient corresponding to the loan level specification with interest as the dependent variable. The sample spans the years 2016 to 2023 for panel E.

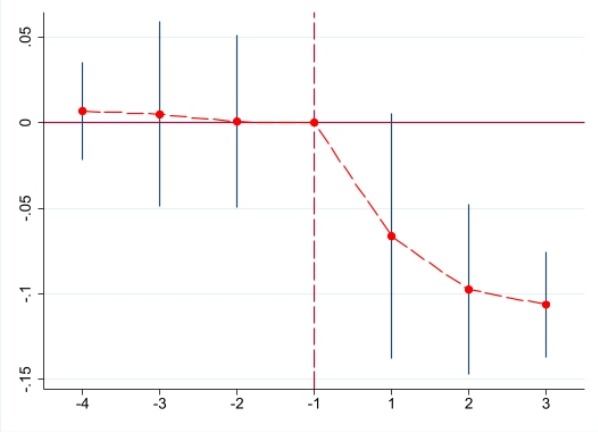
(a) Panel A: Borrowing



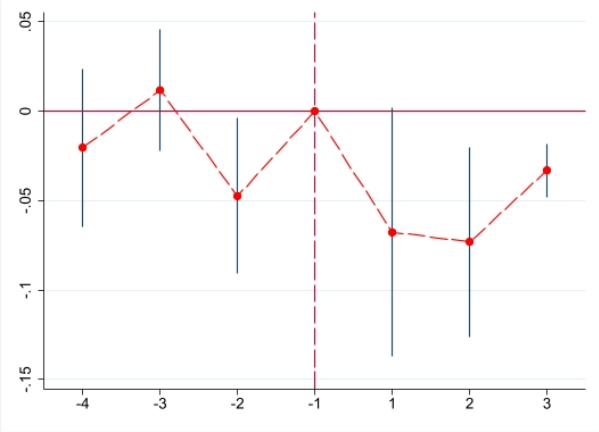
(b) Panel B: Gross Fixed Assets



(c) Panel C: Gross Plant and Machinery



(d) Panel D: Sales



(e) Panel E: Interest rate

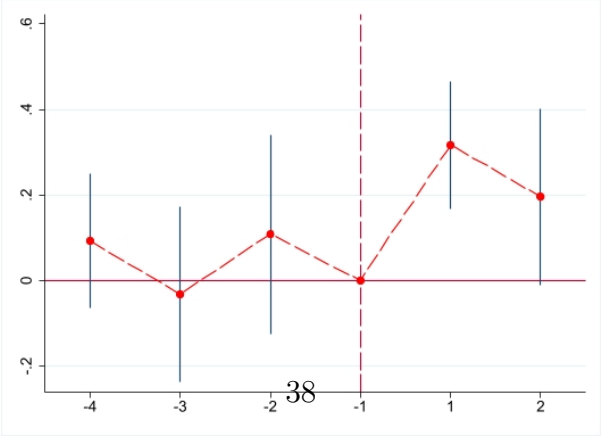


Table 1: Sample Construction

This table presents the construction of the samples used across the empirical analyses in the paper. The overall sample period spans 2016 to 2025. In panels A and B, we summarize the composition of the judicial datasets. Panel A documents the full set of High Court judgments, including the subset of cases in which a government body appears as a litigant against a firm, along with counts of courts and unique judges. Panel B reports the corresponding sample construction for the judgments by District Courts of Delhi and Bangalore, and Income Tax Appellate Tribunals (ITAT). Panel C presents the construction of the social media dataset used to measure online engagement with judicial content. It reports the total number of posts and the number of posts containing media content over the period January 2023 to December 2024. Panel D summarizes the Prowess firm-level dataset, including the number of unique firms, firm-year observations on borrowings, and fixed-asset additions. Panel E reports the construction of the loan-level dataset, including the number of unique loans, firms, and lenders.

Sample period	Jan 2016 to Dec 2025
Panel A: Judgement Data - All High Courts	
Total number of cases	57,457
Number of government - firm cases	10,607
Number of government - firm cases with judgment classification	7,996
Number of judge-judgment level observations	9,661
Number of high courts	25
Number of unique judges	1029
Panel B: Judgment Data - Bangalore and Delhi District Courts & Income Tax Tribunals	
Total number of cases from District Courts	21,602
Number of government–firm cases	179
Total number of cases from Income Tax Tribunals	109,917
Number of government-firm cases and the other party a firm	79,420
Number of Tax Tribunals	27
Panel C: Social Media Sample (Period: Jan 2023 to Dec 2024	
Total number of posts	70,913
Total number of posts with media (video)	17,052 (331)
Panel D: Firm level Prowess Data (Period: Financial Year 2016 to 2025)	
Number of unique firms	40,750
Number of firm-years with data about borrowings	238,813
Number of firm-years with data about gross fixed assets	263,492
Number of firm-years observations about plant and machinery	176,943
Number of firm-years observation on sales	236,968
Panel E: Loan Level Data (Period: Financial Year 2016 to 2023)	
Number of unique loans	29,041
Number of unique firms	4,486
Number of unique lenders	198

Table 2: Descriptive Statistics

In this table, we report the descriptive statistics for the key variables used in the empirical analysis of judicial outcomes, credit allocation, state-level characteristics, and social media engagement. Panel A presents summary measures for the High Court case-level dataset. Panel B reports judge-year–level language scores. Panel C provides loan-level and firm-level summary statistics. Panel D presents state-level economic and political indicators, while Panel E reports descriptive statistics for the social media dataset. Variable definitions are provided in Table A6 of the online appendix.

Variable	N	Mean	Standard Deviation	Median	1st Percentile	99th Percentile
Panel A: Case Level						
Gov win	7,996	0.228	0.420	0	0	1
Pro-govt score	7,741	0.353	0.082	0.348	0.168	0.565
Pro-market score	7,741	0.314	0.077	0.310	0.149	0.514
Neutral score	7,741	0.332	0.114	0.335	0.112	0.609
Post	8,005	0.227	0.419	0	0	1
Similarity ratio	6,243	2.609	19.550	0.951	0	28.606
State gov	7,001	0.529	0.499	1	0	1
Tenure Track	7,996	0.301	0.458	0	0	1
Panel B: Judge-Year Level						
Average Pro-government score	1,594	0.355	0.073	0.349	0.192	0.548
Average Pro-business score	1,594	0.312	0.067	0.310	0.170	0.494
Average Neutral score	1,594	0.332	0.103	0.329	0.124	0.582
Panel C: Social Media Sample						
Bookmarks	70,913	1.408	8.882	0	0	20
Views	70,913	11,134	39,932	4,483	244	109,221
Likes	70,913	49	215	11	0	684
Quotes	70,913	2.190	14.221	0	0	38
Comments	70,913	4.173	18.367	1	0	62
Re-posts	70,913	13.939	67.938	3	0	222
Total Engagements	70,913	11,203	40,140	4,501	246	109,493
Local Share	7,366	0.133	0.246	0.090	0	0.509
Panel D: Loan Level and Firm Level						
Interest rate in %	29,041	9.667	1.902	9.300	7.030	13.900
Borrowings (INR million)	2,38,836	5640	82270	162	0	75262
Gross Fixed Assets (INR million)	2,63,494	4239	56793	152	0	61929
Gross Plant (INR million)	1,76,947	3140	38110	110	0	48841
Sales (INR million)	2,37,050	6548	72819	518	0	90040
High legal	3,14,149	0.503	0.499	1	0	1
Delta GFA change	4,560	-0.085	0.952	-0.14	-0.851	2.718
Delta Plant change	3,422	-0.046	0.648	-0.113	-0.632	2.336
Delta Sales change	4,623	-0.054	1.66	-0.199	-1.447	7.914
Panel E: State and High-Court Level						
Credit growth	219	0.123	0.056	0.122	-0.013	0.239
GDP growth	219	6.022	23.815	7.035	-8.493	16.417
Election win margin	219	0.352	0.234	0.296	0.022	0.914

Table 3: Effect of Live-streaming on Government Win Rates

This table presents difference-in-differences estimates of the impact of live-streaming on the likelihood of a government victory in commercial disputes between government entities and private firms in Indian High Courts, along with placebo estimates examining whether the increase in government win rates observed in High Courts extends to district courts and tribunals that did not adopt live-streaming. The data are at case level for the corresponding courts and span 2016 to 2025. In panel A, we present the main specification, while in panel B, we present the placebo tests. The data are at a judge-judgment level for High Courts (Columns 1-4) and at a judgment level for district courts and tribunals (Columns 5-8). The dependent variable, *Gov Win*, is a binary indicator equal to one if the government wins and zero otherwise. The key explanatory variable, *Post*, is the staggered treatment indicator variable. It takes a value of one for all judgments in the corresponding court after live-streaming introduction and zero before that. It takes a value of zero if the corresponding high court is never treated during our sample period. For district courts, *Post* equals one for all judgments in Bengaluru District Court after Karnataka High Court adopted live-streaming and zero before that; it is zero for all Delhi District Court judgments since Delhi High Court did not adopt live-streaming during our sample period. For Income Tax Appellate Tribunals (ITATs), the sample includes all ITAT judgments involving tax-related disputes between government entities and private firms. In all columns, we include court & year fixed effects. We include judge fixed effects in columns (1)-(4). Control variables include one-year lagged state-level credit growth, GDP growth, margin of win by the states' ruling political party, industrial investment to GDP ratio, inflation, per-judge caseloads and proportion of government wins in the previous year. Columns (3)-(4) additionally include the interaction of *Post* with the pre-period government win rate at the court × case-type level to control for mean reversion. Standard errors are clustered at the court levels, robust and adjusted for heteroskedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Gov Win							
	Panel A: Main Result				Panel B: Placebo			
	Baseline		Mean Reversion Control		District Court		ITAT	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	0.077**	0.081**	0.075**	0.079**	0.065	0.000	0.004	-0.023
	(0.031)	(0.038)	(0.033)	(0.038)	(0.010)	(0.015)	(0.013)	(0.016)
Post × Pre-period Win Rate			0.004	0.051				
			(0.044)	(0.024)				
Observations	9,365	8,789	8,986	8,599	178	173	79,364	78,158
R-squared	0.275	0.283	0.276	0.283	0.067	0.133	0.033	0.017
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Court F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge F.E.	Yes	Yes	Yes	Yes	No	No	No	No
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: Effects on Judicial Sentiment and Incorporation of Governments' Narrative

This table presents difference-in-differences estimates of the change in judicial sentiment (Panel A) and the incorporation of government narrative into judicial reasoning (Panels B and C) in commercial disputes between government entities and private firms in Indian High Courts. The sample spans 2016 to 2025, except in Panel B, which is limited to 2016–2019 (before any High Court adopted live-streaming) to validate the similarity measure. In Panel A, data are aggregated at the judge-year level. In Panel A, Columns (1)–(2) report estimates for the Pro-Government sentiment score, Columns (3)–(4) for the Pro-Market sentiment score, and Columns (5)–(6) for the Neutral sentiment score. *Post* is the staggered treatment indicator, equal to one for all judgments in court i after live-streaming adoption and zero before; it equals zero for courts never treated during our sample period. *Gov Win* is a binary indicator equal to one if the government prevails. *Similarity ratio* measures the relative degree to which the judge's written reasoning aligns with the government's factual narrative compared to the private firm's narrative. A higher value of *Similarity ratio* indicates that the judge's synthesis of the case facts more closely reflects the government's submissions. All columns include Court, Judge, and Year fixed effects. Even-numbered columns except column (6) include the full set of control variables listed in Table 3. Standard errors, reported in parentheses, are clustered at the High Court level, robust, and adjusted for heteroskedasticity.. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Judgment sentiment						
	Pro Govt		Pro Market		Neutral	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.027**	0.026*	-0.002	-0.009	-0.026	-0.017
	(0.013)	(0.013)	(0.015)	(0.013)	(0.024)	(0.023)
Observations	1,594	1,441	1,594	1,441	1,594	1,441
R-squared	0.404	0.432	0.427	0.474	0.466	0.515
Control variables	No	Yes	No	Yes	No	Yes
Court, Judge, and Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
	Panel B: Validation		Panel C: Change in Similarity			
	Gov Win		Similarity ratio			
	(1)	(2)	(3)	(4)	(5)	(6)
Similarity ratio	0.001***	0.001***				
	(0.000)	(0.000)				
Post			1.398**	1.727*	2.720***	1.715*
			(0.596)	(1.005)	(0.271)	(0.925)
Observations	1,086	788	5,974	4,847	1,378	4,353
R-squared	0.318	0.358	0.081	0.080	0.080	0.304
Control variables	No	Yes	No	Yes	No	No
Court, Judge, and Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: Effect of Media Presence on Total Engagement and Local Share

This table presents estimates examining whether posts containing video clips of judicial proceedings generate higher public engagement on social media and whether such engagement is locally concentrated. The sample in columns (1)–(3) includes all posts published by the two major legal journalism accounts on the platform *X*, *LiveLaw India* and *Bar & Bench*, between January 1, 2023, and December 31, 2024. The data are organized at the post level. In columns (1)–(3), the dependent variable, *Total Engagement*, measures the total number of interactions a post receives, including likes, reposts, and comments. In columns (4)–(5), the dependent variable, *Local Share*, is the proportion of geographically identifiable comments on a post that originate from users located in the same state as the focal High Court. The key explanatory variable, *Media*, is a binary indicator equal to one if the post contains an embedded video or a link to a video, and zero otherwise. Column (1) reports the specification without fixed effects. Column (2) incorporates month fixed effects, which absorb common variation in social media activity, news intensity, and posting patterns across months. Column (3) includes month and hour-of-day fixed effects, thereby additionally controlling for within-day variation in user activity and posting times. Column (5) include High Court fixed effects and month fixed effects, respectively, to control for baseline differences in local engagement across states and aggregate time trends. The standard errors in columns (1)-(3) are clustered at the month level; standard errors in columns (4)-(5) are clustered at the High Court level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Total Engagement			Local Share	
	(1)	(2)	(3)	(4)	(5)
Media	16668.30***	16668.30***	16386.14***	0.284**	0.310**
	(347.02)	(2028.79)	(2010.44)	(0.119)	(0.134)
Observations	70,911	70,911	70,910	7,366	7,366
R-squared	0.0462	0.0462	0.0470	0.041	0.113
Month F.E.	No	Yes	Yes	No	Yes
Hour F.E.	No	No	Yes	No	No
High Court F.E.	No	No	No	No	Yes

Table 6: Effects of Live-streaming on Government Win: Strong vs. Weak Governments

This table presents difference-in-differences estimates of how High Court live-streaming affects case outcomes, differentiated by government strength and case type. The data are at a judgment level. In column (4), we limit the sample to high courts belonging to states with above median value of election win margin among all the states. The variable *gov win* is as defined in Table 3. State gov takes a value of one when case i is against the non-central government and zero otherwise. We identify such cases where either of the party names does not contain "Union of India" or "Government of India." The variable *Post* equals one for cases after the corresponding High Court adopted live-streaming. The variable *Margin* is the percentage difference between the number of electoral seats won by the ruling party in the state and the number of seats won by the runner-up. Columns (2) and (4) include the full set of control variables listed in Table 3. Column (3) includes the control variables - proportion of business-government cases, pendency per judge, GDP growth, credit growth, an indicator identifying states led by a national party, and unemployment rate - and their respective interactions with post. All columns include Court, Year, and Judge fixed effects in their respective specifications. Standard errors are clustered at the High Court level, robust, and adjusted for heteroskedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Gov win			
	(1)	(2)	(3)	(4)
Post X Margin X State gov	0.462***	0.515***	0.503***	0.546***
	(0.139)	(0.134)	(0.117)	(0.171)
Post X Margin	-0.293*	-0.272*	-0.302**	-0.100
	(0.143)	(0.134)	(0.140)	(0.218)
Post X State gov	-0.251***	-0.283***	-0.281***	-0.349***
	(0.080)	(0.079)	(0.080)	(0.109)
State gov X Margin	-0.027	-0.096	-0.093	-0.259*
	(0.083)	(0.080)	(0.079)	(0.140)
Post	0.221***	0.243***	0.110	0.172
	(0.064)	(0.062)	(0.308)	(0.144)
Margin	0.141	0.140	0.166	0.126
	(0.103)	(0.118)	(0.128)	(0.091)
State gov	0.076	0.113**	0.112*	0.236**
	(0.054)	(0.054)	(0.055)	(0.099)
Observations	9,368	8,792	8,404	3,128
R-squared	0.279	0.288	0.297	0.265
Control variables	No	Yes	Yes	Yes
Court F.E.	Yes	Yes	Yes	Yes
Judge F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes

Table 7: Mechanism: Effect of Career Stages and Retirement Proximity on Government Win Rates

This table presents the estimates of the impact of internal tenure-track incentives (Columns 1-2), its falsification test in non-tenure-track (permanent) sample (Columns 3-4), and external post-retirement rewards (Columns 5-6) on the likelihood of a government victory in commercial disputes in Indian High Courts following the introduction of live-streaming. The dependent variable, *Gov Win*, is a binary indicator equal to one if the government wins. *Post* is the staggered treatment indicator for live-streaming as defined in Table 3. *Tenure Threshold* is an indicator taking the value of 1 if the judge's tenure is below the pre-period median time to confirmation for additional (tenure-track) judges. *Retirement* is an indicator taking the value of 1 if a judge is within 1 year of the mandatory retirement age. Columns (1), (3), and (5) report baseline estimates, while Columns (2), (4), and (6) incorporate one-year lagged state-level control variables listed in Table 3. All specifications include court, year, and judge fixed effects. Time-invariant main effects are absorbed by the judge fixed effects. Standard errors are clustered at the High Court level, robust, and adjusted for heteroskedasticity. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Gov Win					
	Tenure Track		Permanent		Retirement	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Tenure Threshold	0.071**	0.079**	0.005	0.005		
	(0.021)	(0.036)	(0.031)	(0.061)		
Post × Retirement					0.023	0.003
					(0.090)	(0.073)
Post	0.088**	0.053**	0.101**	0.099**	0.073**	0.080**
	(0.042)	(0.028)	(0.011)	(0.013)	(0.029)	(0.037)
Observations	1,112	1,077	7,061	6,659	9,366	8,790
R-squared	0.271	0.288	0.200	0.205	0.275	0.283
Control variables	No	Yes	No	Yes	No	Yes
Court F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Judge F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: Effects of Livestreaming on Firm Outcomes

This table presents staggered difference-in-differences estimates of the effect of the High Court live-streaming on firm-level outcomes and borrowing cost. Firms are mapped to High Courts based on the state of their registered office. The data are organized at the firm–year level in first 8 columns, and at the loan level in columns 9 and 10. Columns (1)–(2) use log borrowing as the dependent variable, Columns (3)–(4) use log additions to fixed assets, Columns (5)–(6) use log additions to plant and machinery, and Columns (7)–(8) use log sales. In columns 9 and 10, the dependent variable is the interest rate charged for the respective loan. The variable *Post* equals one for firm-years after the corresponding High Court adopted live-streaming. *High Legal* is an indicator for industries with an above median value of the ratio between legal expenses and total expenses among all industries. The even-numbered columns include full set of one-year lagged firm-level control variables: total assets, leverage, current ratio, profit margin (PAT/Sales), and Interest Coverage Ratio. All specifications in the first 8 columns include firm fixed effects and state-by-year fixed effects. In columns 9 and 10, we include firm, bank-by-year-month, and State-by-year-month fixed effects. Standard errors are robust, clustered at the corresponding state level, and adjusted for heteroskedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Log borrowing		Log gross fixed assets		Log gross plant		Log sales		Interest Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post X High Legal	-0.092*	-0.143***	-0.182***	-0.109***	-0.078***	-0.063***	-0.067*	-0.066**	0.309***	0.545***
	(0.051)	(0.026)	(0.030)	(0.022)	(0.015)	(0.022)	(0.034)	(0.025)	(0.041)	(0.087)
Observations	237,006	169,884	262,474	165,383	175,747	133,877	235,153	169,854	25,154	15,411
R-squared	0.883	0.881	0.945	0.959	0.954	0.961	0.913	0.921	0.846	0.817
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank X Year-Month F.E.	No	No	No	No	No	No	No	No	Yes	Yes
State X Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Performance of Litigated Firms

This table presents difference-in-differences estimates of the effect of firm-level outcomes for litigated firms after the introduction of live-streaming. The sample spans 2016 to 2025. In panel A, the sample is limited to firms under litigation in high courts during the sample period. In panel B, the sample is limited to firms under litigation in ITATs during the sample period. The variable ΔGFA change is the difference between the firms' year-on-year percentage growth in gross fixed assets and the average year-on-year percentage growth in gross fixed assets of the respective industry. The variable ΔPlant change is the difference between the firms' year-on-year percentage growth in plant and machinery and the average year-on-year percentage growth in plant and machinery of the respective industry. The variable ΔSales change is the difference between the firms' year-on-year percentage growth in sales and the average year-on-year percentage growth in sales of the respective industry. The variable *Post* equals one for firm-years after the corresponding High Court adopted live-streaming. The variable $Post_Case_{ijt}$ takes a value of one for all the years after the respective firms' case appears in the respective court for the first time. All columns include the full set of one-year lagged firm-level control variables: total assets, leverage, current ratio, profit margin (PAT/Sales), and Interest Coverage Ratio. All specifications include firm, state-by-year, and industry-by-year fixed effects, controlling for time-invariant firm characteristics, time-varying state-level shocks, and industry-level trends. Standard errors are robust, clustered at the corresponding state level, and adjusted for heteroskedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: High Court			Panel B: ITAT		
	ΔGFA change	ΔPlant change	ΔSales change	ΔGFA change	ΔPlant change	ΔSales change
	(1)	(2)	(3)	(4)	(5)	(6)
Post Case X Post	-0.162**	-0.103**	-0.179*	-0.023	0.033	-0.047
	(0.069)	(0.045)	(0.108)	(0.037)	(0.052)	(0.054)
Post Case	0.066**	0.110***	0.005	-0.013	-0.020	0.061**
	(0.026)	(0.034)	(0.073)	(0.017)	(0.018)	(0.026)
Observations	3,445	2,768	3,846	23,105	18,013	27,191
R-squared	0.324	0.423	0.290	0.241	0.282	0.218
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
State X Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Internet Appendix

Figure A1: Live-Stream Example

This is an example of live-streaming cases in one of the high courts.

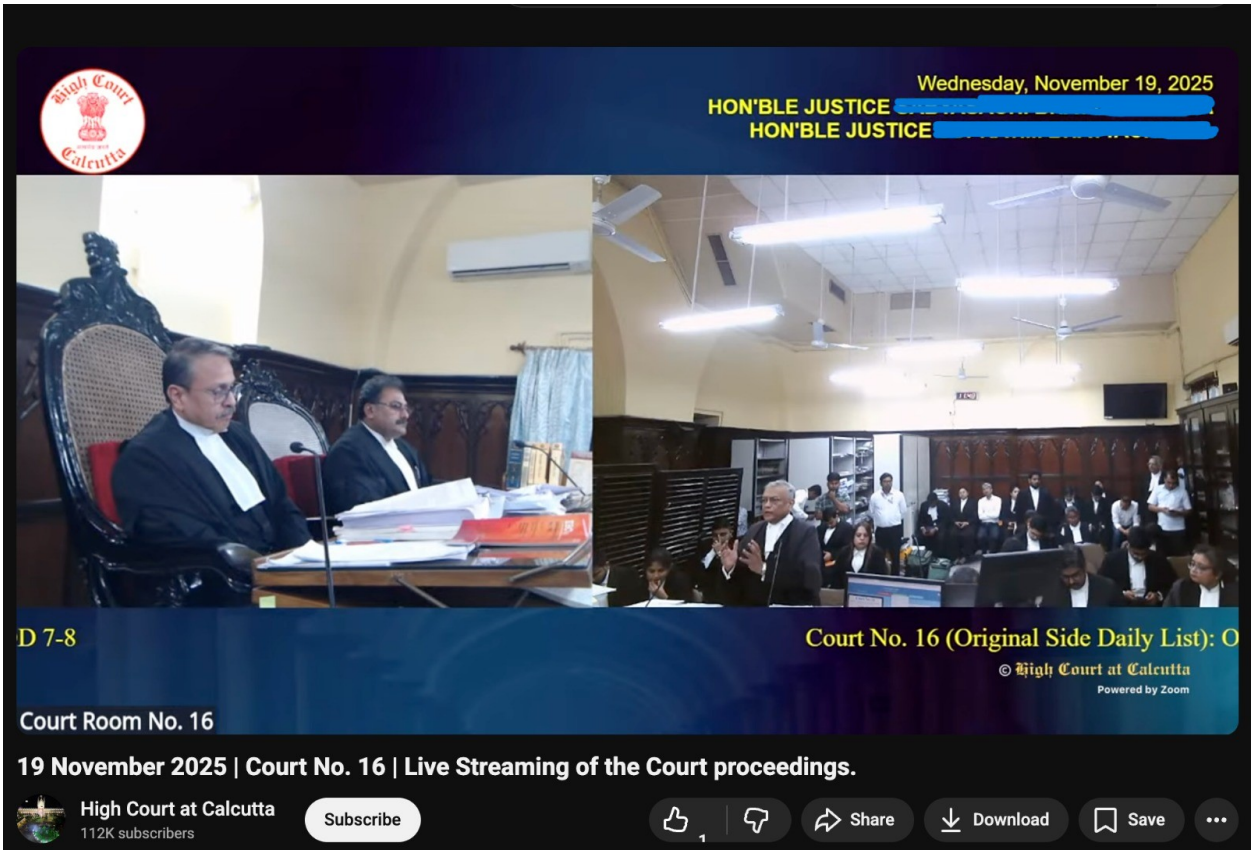
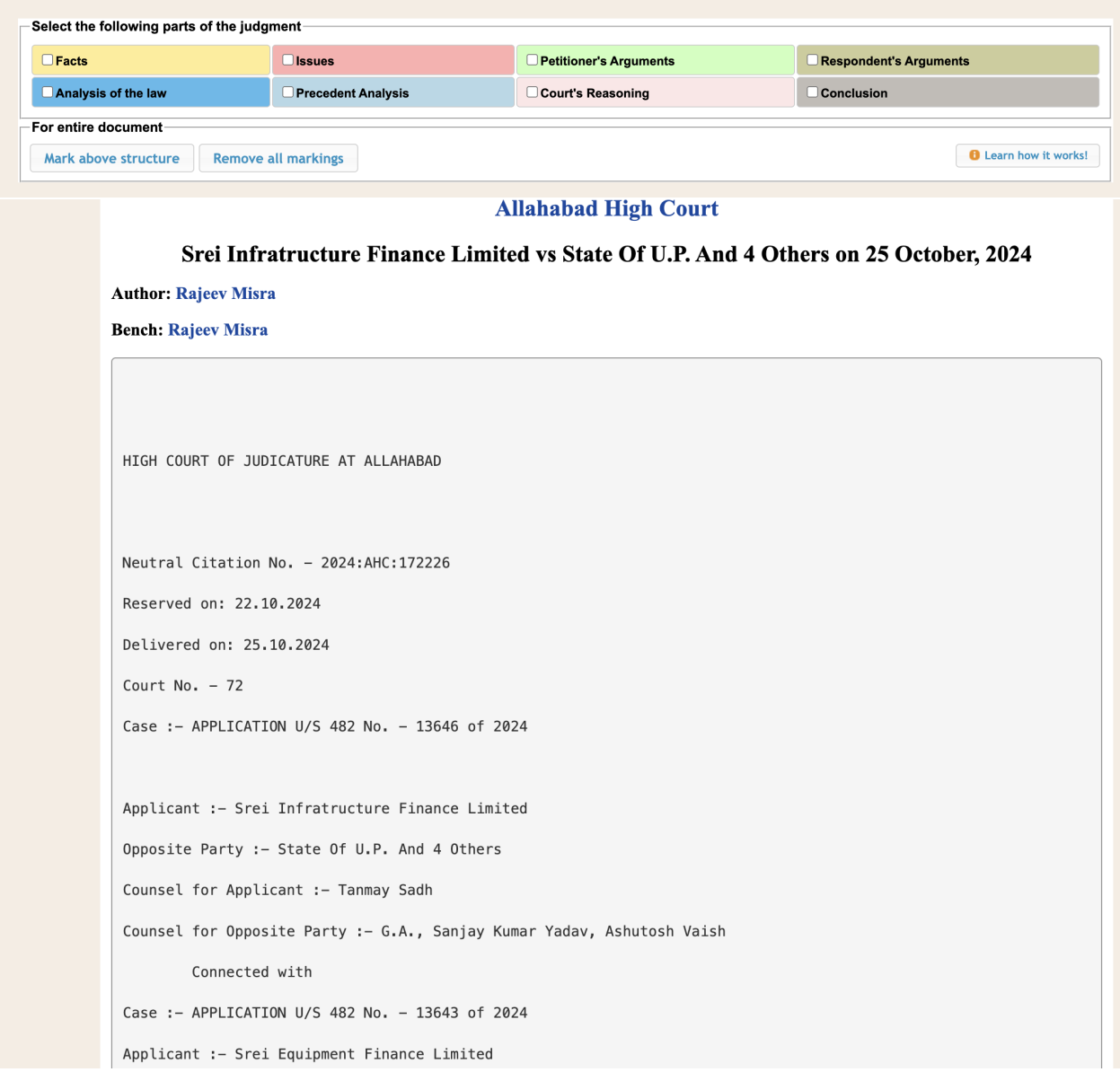


Figure A2: Screenshot of Labeled Judgments

This is an example of a labeled judgment from IndiaKanoon's website.



Case :- APPLICATION U/S 482 No. - 13643 of 2024

Applicant :- Srei Equipment Finance Limited

Opposite Party :- State Of U.P. And 4 Others

Counsel for Applicant :- Tanmay Sadh

Counsel for Opposite Party :- G.A., Sanjay Kumar Yadav, Ashutosh Vaish

Hon'ble Rajeev Misra,J.

1. This application under Section 482 Cr.P.C. has been filed by accused-applicant-Srei Infrastructure Finance Limited challenging the police report/charge sheet no. 13 of 2023 dated 30.12.2023 submitted by the CBI in Case Crime No./FIR No. RCBD12021E0001, under Sections 120-B r/w Section 409, 420 IPC and Sections 13(2) r/w Section 13(1)(d) of Prevention of Corruption Act, 1988, Police Station-CBI, BSFB, New Delhi, the Cognizance Taking Order/Summoning Order dated 14.03.2024 passed by Special Judicial Magistrate (CBI), Ghaziabad in consequential Criminal Case No. 12 of 2024/Registration No. 14248 of 2024 (CBI Vs. M/s SVOGL Oil and Gas Enegry Limited and Others), under Section 120-B IPC read with Sections 420, 471 IPC read with 468 IPC and substantive offence under Section 420 IPC, Police Station-CBI, BSFB, New Delhi as well as the entire proceedings of aforementioned criminal case.

2. Affidavits have been exchanged between the parties. Counsel for the parties agreed that the matter be decided finally at the admission stage on the basis of pleadings of the parties without formally admitting the present application. In view of above and as provided under the Rules of the Court, the present application is being disposed of finally at the admission stage itself. Criminal Misc. Application 13646 of 2024 (Srei Infratructure Finance Limited Vs. State of U.P. and 4 Others) shall be treated as the leading petition. The facts pleaded in the aforesaid petition shall alone be referred to for the sake of convenience.

3. I have heard Mr. Anirban Bhattacharya, Advocate assisted by Mr. Tanmay Sadh, the learned counsel for applicant, the learned A.G.A. representing State-opposite party-1, Mr. Gyan Prakash Srivastava, the learned Deputy Solicitor General of India assisted by Mr. Sanjay Kumar Yadav, the learned Standing Counsel for CBI representing opposite parties 2, 3 and 4 and Mr. Ashutosh Vaish, the learned counsel representing opposite party-5.

5. Record shows that an FIR dated 10.03.2021 was lodged by first informant-opposite party-5 Ms. Mausumi Majundar, DGM, Punjab National Bank, LCB, NOIDA, Branch, U.P. and was registered as FIR No. RCBD12021E0001, under Sections 120-B r/w Section 409, 420 IPC and Sections 13(2) r/w Section 13(1)(d) of Prevention of Corruption Act, 1988, Police Station-CBI, BSFB, New Delhi. In the aforesaid FIR, the following have been nominated as named accused;-

Analysis of the law

(I). M/s SVOGL Oil Gas & Energey Limited, Registered Office at Tower-1, Fifth Floor, NBCC Plaza, Sector V, Pushp Vihar, New Delhi-110017 (Accused No. 1).

(II). Mr. Padam Singhee, S/o Mr. Chimanlal Singhee, Managing Director of SVOGL, resident of House No. 4A, Street C-1, Sainik Farms, New Delhi-110062 also at House No. J-238/3, Lane W-15J, Western Avenue, Sainik Farm, New Delhi-110062 (Accused No.-2).

(III). Mr. Prem Chimanlal Singhee S/o Mr. Chimanlal Singhee, Director of SVOGL, resident of House No. 4A, Street C-1, Sainik Farms, New Delhi-110062 also at House No. J-238/3, Lane W-15J, Western Avenue, Sainik Farm, New Delhi-110062.

Apart from above, the following have also been arraigned as accused (IV) Unknown public servants and others.

It is thus apparent that applicant- SREI Infratructure Finance Limited has not been nominated as a named accused in the aforesaid FIR.

6. After aforementioned FIR was lodged, the CBI proceeded with statutory investigation of aforementioned FIR in terms of Chapter-XII Cr.P.C. On the basis of material collected by CBI, during course of investigation, it came to the conclusion that complicity of various persons is prima-facie apparent in the crime in question. It, accordingly, submitted the police report/charge sheet no. 13 of 2023 dated 30.12.2023 in terms of Section 173(2) Cr.P.C., whereby the following have been charge sheeted;-

I. M/s SVOGL Gas & Oil Exploration Services Ltd., U/s 120-B r/w 420, 471 IPC r/w 468 IPC & Substantive Offence u/s 420 IPC.

II. Shri Prem Singhee, U/s 120-B r/w 420, 471 IPC r/w 468 IPC & substantive offence u/s 420 IPC.

9.3. Subsequent to above, the Reserve Bank of India filed an application before the National Company Law Tribunal, Calcutta Bench, under Section 227 read with Clause (zk) of Sub-Section (2) of Section 239 of the Insolvency and Bankruptcy Code, 2016 read with Rules 5 and 6 of the Insolvency and Bankruptcy (Insolvency and Liquidation Proceedings of Financial Service Providers and Application to Adjudication Authority), Rules, 2019 seeking initiation of Corporate Insolvency Resolution Process of the applicant. The same was registered as CP(IB) No. 295/KB/2021. In the matter of Reserve Bank of India Vs. SREI Infrastructure Finance Limited.

9.4. The National Company Law Tribunal Calcutta Bench, vide order dated 08.10.2021 admitted the aforesaid application and further appointed one Mr. Rajneesh Sharma as the Administrator of applicant. For ready reference, the order dated 08.10.2021 is reproduced herein below;-

"ORDER Per: Rajasekhar V.K., Member (Judicial)

1. The Court convened via video conferencing.

2. This is an application filed by the Reserve Bank of India (Appropriate Regulator) under section 227 read with section 239(2(zk) of the Insolvency and Bankruptcy Code, 2016 (in short "IBC") for initiations of Corporate Insolvency Resolution Process (in short "CIRP") against Srei Infrastructure Finance Limited (in short "SIFL"), the Financial Service Provider.

Fact

3. The respondent/Financial Service Providers (in short "FSP") is a company registered under the Companies Act, 1956, having CIN: L29219WB1985PLC- 055352, and registered with the Registrar of Companies, West Bengal, Kolkata. Therefore, this Bench has jurisdiction to deal with the present petition.

4. Mr. Sudipto Sarkar, Learned Senior Counsel appearing on behalf of the Petitioner/Appropriate Regulator submits as follows :-

These documents demonstrate that the account is in default with the status of "SMA-2", which means that the interest of principal has been overdue and remains unpaid for a period in excess of 60 days calculated in accordance with the RBI Master Circular on Prudential Norms on Income Recognition, Asset Classification and Provisioning pertaining to advances. E-mail communication evidencing classification of SIFL as a Red Flag Account by UCO Bank. The Certificate under the Bankers' Book Evidence Act, 1891 is also attached.

6. The RBI vide its notification dated 04/10/2021, in exercise of its powers under section 45-IE of the Reserve Bank of India Act, 1934 has superseded the Board of Directors of SIFL and appointed Mr. Rajneesh Sharma as the Administrator. It has also constituted a three-member Advisory Committee to assist the Administrator of SIFL in the discharge of its duties. The RBI has proposed the same person to be appointed as the Administrator of the Corporate Debtor.

7. Mr. Sudipto Sarkar, Learned Senior Counsel appearing for the Petitioner/ Appropriate Regulator submits that in view of the huge default committed by the respondent/FSP, there is a need to initiate CIRP against the respondents with speed. He, therefore, urged this Adjudicating Authority to pass appropriate orders expeditiously keeping in view the public interest in the matter.

8. Mr. Sudipto Sarkar, Learned Counsel, further submitted that the challenge to the notification dated 04/10/2021 issued the RBI, in Writ Petition (Lodging) No. 22872/2021, was dismissed by the Hon'ble Bombay High Court vide order dated 07/10/2021.

9. Rule 5(a)(i) of the Insolvency and Bankruptcy (Insolvency and Liquidation Proceedings of Financial Service Providers and Application to Adjudication Authority) Rules, 2019 mandates that no CIRP shall be initiated against an FSP which has committed a default under section 4, except upon an application made by the appropriate regulator in accordance with rule 6. Sub-clause (ii) thereof which specifies that an application under sub-clause (i) shall be dealt with in the same manner as an application by a financial creditor under section 7.

Table A1: Indian States and Union Territories with Their Corresponding High Courts

State	High Court
Andhra Pradesh	Andhra Pradesh High Court
Arunachal Pradesh	Gauhati High Court
Assam	Gauhati High Court
Mizoram	Gauhati High Court
Nagaland	Gauhati High Court
Bihar	Patna High Court
Chhattisgarh	Chhattisgarh High Court
Goa	Bombay High Court
Gujarat	Gujarat High Court
Himachal Pradesh	Himachal Pradesh High Court
Jharkhand	Jharkhand High Court
Karnataka	Karnataka High Court
Kerala	Kerala High Court
Madhya Pradesh	Madhya Pradesh High Court
Maharashtra	Bombay High Court
Manipur	Manipur High Court
Meghalaya	Meghalaya High Court
Odisha	Orissa High Court
Punjab	Punjab and Haryana High Court
Haryana	Punjab and Haryana High Court
Rajasthan	Rajasthan High Court
Sikkim	Sikkim High Court
Tamil Nadu	Madras High Court
Telangana	Telangana High Court
Tripura	Tripura High Court
Uttar Pradesh	Allahabad High Court
Uttarakhand	Uttarakhand High Court
West Bengal	Calcutta High Court
Union Territory	**High Court**
Andaman & Nicobar Islands	Calcutta High Court
Chandigarh	Punjab and Haryana High Court
Dadra & Nagar Haveli	Bombay High Court
Daman & Diu	Bombay High Court
Delhi (NCT)	Delhi High Court
Jammu & Kashmir	High Court of Jammu & Kashmir and Ladakh
Ladakh	High Court of Jammu & Kashmir and Ladakh
Lakshadweep	Kerala High Court
Puducherry	Madras High Court

Table A2: Live Stream Adoption Dates

This table presents the dates of live-streaming adoption dates for High Courts in our sample period until December 2024.

High Court	**Livestream Start Date**
Gujarat High Court	1 October 2020
Calcutta High Court	8 October 2022
Gauhati High Court	8 October 2022
Jharkhand High Court	8 October 2022
Karnataka High Court	8 October 2022
Madhya Pradesh High Court	8 October 2022
Orissa High Court	8 October 2022
Patna High Court	8 October 2022
Telangana High Court	8 October 2022
Uttarakhand High Court	8 October 2022
High Court of Meghalaya	10 November 2022
Chhattisgarh High Court	16 August 2023
Kerala High Court	1 August 2024
Bombay High Court	5 July 2025

The following High Courts did not adopt live-streaming by Dec 2025: Allahabad, Andhra, Delhi, Himachal Pradesh, Jammu & Kashmir, Madras, Manipur, Punjab & Haryana, Rajasthan, Sikkim, and Tripura.

Table A3: Search Keywords for Commercial Case Identification

Category	**Keywords**
Legal Acts	Companies Act 2013, Companies Act 1956, SARFAESI Act 2002, Insolvency and Bankruptcy Code 2016, IBC, Arbitration and Conciliation Act 1996, Commercial Courts Act 2015, SEBI Act 1992, Competition Act 2002, FEMA
Case Types	Commercial dispute, commercial suit, commercial appeal, commercial arbitration, company petition, winding up petition, oppression and mismanagement, shareholder dispute, director dispute, corporate dispute
Corporate Terms	Company, corporation, private limited, public limited, shareholders, merger, acquisition, amalgamation, liquidation, corporate governance
Financial & Banking	Bank, financial institution, NBFC, loan recovery, debt recovery, secured creditor, unsecured creditor, default, NPA, non-performing asset, mortgage, hypothecation
Contracts	Breach of contract, contractual dispute, supply agreement, distribution agreement, licensing agreement, franchise agreement, tender dispute
Insolvency	CIRP, insolvency resolution, insolvency professional, resolution applicant, committee of creditors, moratorium, fraudulent trading, preferential transaction
Securities	Securities, shares, debentures, investment dispute, insider trading, market manipulation, SEBI order, takeover
Tribunals & Courts	NCLT, NCLAT, National Company Law Tribunal, Debt Recovery Tribunal, DRT, DRAT, Commercial Court, Commercial Division
Specific Provisions	Section 7 IBC, Section 9 IBC, Section 13 SARFAESI, Section 138 NI Act, Section 241 Companies Act, Section 242 Companies Act, cheque dishonor

Table A4: Keywords for Party Identification

Category	**Keywords**
Government — Income Tax Officials	Income Tax Officer, ITO, Assessing Officer, AO, Assistant Commissioner, ACIT, Asstt. CIT, A.C.I.T., Deputy Commissioner, DCIT, Joint Commissioner, Additional Commissioner, Commissioner of Income Tax, CIT, Principal Commissioner, Chief Commissioner
Government — Income Tax Bodies	Income Tax Department, Department of Income Tax, IT Department, Income Tax Appellate Tribunal, ITAT, Central Board of Direct Taxes, CBDT, Tax Recovery Officer, TRO
Government — Regulatory Bodies	SEBI, IRDA, RBI, BIFR, B.I.F.R.
Government — General	Union of India, Government, Govt, State Government, Central Government, Ministry, Authority, Board, Municipal, Corporation, Directorate
Government — Local Bodies	Municipal Corporation, Nagar Nigam, Nagar Palika, Cantonment Board, Development Authority, M.P.R.R.D.A., MPRRD
Government — Ward Classifications	Corporate Ward, Corporate Circle, Non Corporate Ward, Company Circle, Corporate Range, NCW, OSD
Company — Legal Form	Ltd, Limited, Pvt, Private Limited, Inc, Corp, LLP
Company — Business Suffixes	Company, Industries, Services, Technologies, Enterprises, Associates, Group, Holdings, Corporation
Company — Sector Terms	Mills, Foods, Construction, Trading, Shares, Securities, Fabrics, Drugs, Lamps, Rotopack, Cybertek, Foundation
Company — Prefix	M/s
Individual — Honorifics	Smt, Sri, Shri, Mr, Mrs, Ms, Dr

Table A5: Descriptive Statistics: District and Tax Tribunals

In this table, we report the descriptive statistics for the variables used in placebo test and the control variables. Panel A presents summary measures for the District Court case-level dataset. Panel B presents summary measures for the Tax Tribunals case-level dataset. Panel C and D report summary statistics for the High Court-level and firm-level control variables, respectively.

Variable	N	Mean	Standard Deviation	Median	1st Percentile	99th Percentile
Panel A: District Courts						
Gov win	179	0.34	0.475	0	0	1
Panel B: Tax Tribunals						
Gov win	79,420	0.325	0.468	0	0	1
Panel C: Court-level control variables						
GDP growth	219	6.022	4.880	7.035	-8.493	16.417
Credit growth	219	0.123	0.056	0.122	-0.013	0.239
Investmnet to GDP ratio	219	0.290	0.523	0.121	0.000	2.906
Election win margin	219	0.352	0.234	0.296	0.022	0.914
Pendency per judge	219	195	7051	6062	59	25690
Government win rate	200	0.394	0.231	0.400	0	1
Panel D: Firm-level control variables						
Total assets (INR million)	2,36,915	10,887	1,09,044	747	2	1,66,405
Leverage	1,92,627	0.585	14.839	0.303	0.001	3.029
Current ratio	2,29,534	6.612	98.127	1.321	0.018	80.250
Profit margin	2,36,967	3.519	828.559	0.022	-25.000	25.540
ICR	1,85,802	49.514	2,563.772	2.576	-126.000	1,099.000

Table A6: Variable Definitions: Judge-Level and Social Media Variables

Variable Name	Definition
Judge Experience	It is the number of years since the judge's initial appointment to the High Court.
Judge Tenure	It is the number of years the judge has served in the current High Court.
Judge Age	It is the age of the judge in years at the time of the judgment.
Engagement	It is the total number of likes, reposts, and comments received on a social media post reporting a court proceeding.
Media	It is an indicator variable that takes the value 1 if a social-media post contains a video clip of the proceeding and 0 otherwise.
Total Expenses	It is the sum total of all expenses incurred by a firm in a given year.
LegalExp	It is the share of a firm's total expenses that is spent on legal expenditures.
High Legal Exposure	It is an indicator variable that takes the value 1 if the firm's legal expenditure share in 2020 is above the sample median.
Interest Rate	It is the reported lending rate for a specific loan contract from the MCA database.
Loan Amount	It is the value of the new borrowing undertaken by a firm in a given year, obtained from the Prowess database.
GDP Growth	It is the annual GDP growth rate of the state in which the High Court is located.
Credit Growth	It is the annual growth rate of bank credit outstanding in the state.
Political Competitiveness	It is one minus the vote-share margin of the winning party in the most recent state election.
Number of Firms per Capita	It is the number of registered firms in the state divided by the state's population.
Post	It is an indicator variable that takes the value 1 for all cases heard after a High Court introduces live-streaming, and 0 otherwise.
Government Win	It is an indicator variable that takes the value 1 if the judgment concludes in favour of the government and 0 otherwise.
Pro-government Score	It is a text-based semantic score that captures the extent to which the judgment language reflects pro-regulation or pro-government orientation.

Table A6: Variable Definitions: Judge-Level and Social Media Variables

Variable Name	Definition
Pro-market Score	It is a text-based semantic score that captures the extent to which the judgment language reflects pro-market or market-friendly orientation.
Neutral Score	It is a semantic score that reflects neutral orientation in the language of the judgment.
Similarity_Gov	It is a cosine-similarity measure that captures the textual similarity between the judge's reasoning and the government litigant's factual submissions.
Similarity_Firm	It is a cosine-similarity measure that captures the textual similarity between the judge's reasoning and the firm's factual submissions.
ΔSimilarity	It is the difference between Similarity_Gov and Similarity_Firm, and it measures the extent to which the judge's reasoning aligns more closely with the government's narrative relative to the firm's.

Table A7: Data Source for Balance Test Variables

This table presents the sources of the data used in the balance tests in Table A8. Panels A, B, C, and D present the data sources for data on court composition, economic variables, political variables, and social variables, respectively.

Data	Source
Panel A: Court Composition	
Pro Government Tilt	IndiaKanoon
Business-Government Case proportion	IndiaKanoon
Judges from same state proportion	IndiaKanoon
Pendency per judge	Supreme Court
Number of Judges	IndiaKanoon
Average age of judges	IndiaKanoon
Share of commercial cases	IndiaKanoon
Average tenure of judge	IndiaKanoon
Government win proportion	IndiaKanoon
Panel B: Economic	
GDP growth	Ministry of Statistics and Program Implementation
Credit growth	Reserve Bank of India
Inflation	Reserve Bank of India
Agriculture yield	Department of Agriculture and Family Welfare
Investment/GDP	Department for Promotion of Industry and Internal Trade
Ease of doing business ranking	Department for Promotion of Industry and Internal Trade
Budget/GDP	State Government Websites
Fiscal Deficit/GDP	State Government Websites
Number of firms per '000 persons	Reserve Bank of India
Panel C: Political	
Number of political parties	Election Commission of India
Bicameral (Two legislative chambers)	Election Commission of India
Win margin	Election Commission of India
Center ruling party proportion	Election Commission of India
National party proportion	Election Commission of India
Incumbent win proportion	Election Commission of India
Panel D: Social	
Literacy	Ministry of Statistics and Program Implementation
Unemployment rate per 1000	Ministry of Statistics and Program Implementation
Gender Ratio (F/M)	Ministry of Statistics and Program Implementation
Female Literacy	Ministry of Statistics and Program Implementation
Number of Government Hospitals per '000	Ministry of Statistics and Program Implementation
Life Expectancy	Ministry of Statistics and Program Implementation
Infant Mortality per '000 births	Ministry of Statistics and Program Implementation

Table A8: Comparison between Treated and Control High Courts

This table presents the comparison between the treated and control high courts as of the year before the implementation of live-streaming in the first court. We present the comparisons of the variables related to court composition in panel A, which are at the court level. Panels B, C, and D present the comparisons on economic, political, and social dimensions, respectively for the respective states under the jurisdiction of the high courts.

	Treated	Control	Difference	T-stat
Panel A: Court Composition				
Pro Government Tilt	0.344	0.363	0.019	1.036
Business-Government Case proportion	0.199	0.137	-0.062	-1.242
Judges from same state proportion	0.682	0.729	0.047	1.329
Pendency per judge	6000	6919	919	0.487
Number of Judges	22	34	12	1.237
Average age of judges	58.586	58.325	-0.261	-0.740
Share of commercial cases	0.477	0.459	-0.018	-0.2053
Average tenure of judge	6.464	6.402	-0.062	-0.222
Government win proportion	0.329	0.292	-0.037	-0.342
Panel B: Economic				
GDP growth	7.351	7.460	0.109	0.102
Credit growth	14.20%	15.60%	1.40%	0.597
Inflation	4.021	4.065	0.044	0.071
Agri yield	2081	2465	384	1.355
Investment/GDP	1.931	1.677	-0.254	-0.278
Ease of doing business ranking	17.611	17.833	0.222	0.069
Budget/GDP	0.506	0.355	-0.151	-1.698
Fiscal Deficit/GDP	4.70%	4.10%	-0.60%	-0.636
Number of firms per capita	0.908	2.645	1.737	1.509
Panel C: Political				
Number of political parties	13.813	12.267	-1.546	-0.765
Bicameral (Two legislative chambers)	0.167	0.111	-0.056	-0.410
Win margin	0.335	0.330	-0.005	-0.051
Center ruling party proportion	0.500	0.600	0.100	0.544
National party proportion	0.500	0.600	0.100	0.544
Incumbent win proportion	0.250	0.270	0.020	0.103
Panel D: Social				
Literacy	76.74%	79.03%	2.29%	0.784
Unemployment rate per 1000	90.888	70.444	-20.444	-1.488
Gender Ratio (F/M)	0.957	0.906	-0.051	-1.951
Female Literacy	74.12%	73.08%	-1.04%	-0.342
Number of Government Hospitals per '000	0.059	0.049	-0.01	-0.689
Life Expectancy	69.63%	71.61%	1.98%	1.701
Infant Mortality per '000 births	26.222	19.555	-6.667	-1.672

Table A9: High Court YouTube Views

High Court	**Views**
Gujarat High Court	7,355,686
Calcutta High Court	8,107,057
Gauhati High Court	1,564,262
Jharkhand High Court	847,465
Karnataka High Court	8,888,131
Orissa High Court	5,379,477
Patna High Court	1,143,049
High Court of Meghalaya	741,287
Bombay High Court	386,775

Table A10: Comparison between States with Strong and Weak Local Governments

This table presents the comparison between the states with strong and weak governments. We identify strong government states as the states where the percentage margin between the seats won by the winner and runner up is above the median value of the margin. We present the comparisons of the variables related to court composition in panel A, which are at the court level. Panels B, C, and D present the comparisons on economic, political, and social dimensions, respectively for the respective states under the jurisdiction of the high courts. The comparisons are presented as of the year prior to the first adoption of live-streaming.

	Strong	Weak	Difference	T-stat
Panel A: Court Composition				
Pro Government Tilt	0.350	0.356	0.006	0.318
Business-Government Case proportion	0.208	0.134	-0.074	-1.503
Judges from same state proportion	0.661	0.753	0.092	2.684
Pendency per judge	5521	7291	1770	0.952
Number of Judges	27	29	2	0.233
Average age of judges	58.508	58.397	-0.111	-0.313
Share of commercial cases	0.497	0.440	-0.057	-0.645
Average tenure of judge	6.423	6.435	0.012	0.041
Government win proportion	0.375	0.251	-0.124	-0.645
Panel B: Economic				
GDP growth	7.980	6.318	-1.662	-1.878
Credit growth	16.54%	13.70%	-2.84%	-1.108
Inflation	4.381	3.926	-0.455	-0.800
Agri yield	2465	2105	-360	-1.268
Investment/GDP	0.242	0.172	-0.070	-0.762
Ease of doing business ranking	16.8	17.438	0.638	0.176
Budget/GDP	0.44	0.426	-0.014	-0.145
Fiscal Deficit/GDP	4.28%	4.57%	0.29%	0.288
Number of firms per '000 persons	1.979	1.508	-0.471	1.509
Panel C: Political				
Number of political parties	11.933	14.125	2.192	1.096
Bicameral (Two legislative chambers)	0.267	0.188	-0.079	-0.512
Center ruling party proportion	0.533	0.600	0.067	0.3568
National party proportion	0.667	0.400	-0.267	-1.468
Incumbent win proportion	0.333	0.188	-0.146	-0.910
Panel D: Social				
Literacy	76.06%	77.23%	1.17%	0.375
Unemployment rate per 1000	70.233	86.594	16.361	1.353
Gender Ratio (F/M)	0.938	0.966	0.028	1.636
Female Literacy	71.80%	73.70%	1.90%	0.586
Number of Government Hospitals per '000	0.051	0.049	-0.002	-0.121
Life Expectancy	70.32%	70.71%	0.39%	0.313
Infant Mortality per '000 births	25.067	23.938	-1.129	-0.248

Table A11: Effects of Live-streaming on Firm Outcomes: Alternative Measure of Legal Exposure

This table presents staggered difference-in-differences estimates of the effect of the High Court live-streaming on firm-level outcomes. Firms are mapped to High Courts based on the state of their registered office, and the data are organized at the firm–year level. Columns (1)–(2) use log borrowing as the dependent variable, Columns (3)–(4) use log gross additions to fixed assets, Columns (5)–(6) use log gross additions to plant and machinery, and Columns (7)–(8) use log sales. The variable *Post* equals one for firm-years after the corresponding High Court adopted live-streaming. *High Legal* is an indicator for industries with an above median number of firms that are involved in a legal dispute with the government between 2016 and 2020. Columns (2), (4), (6), and (8) include the full set of one-year lagged firm-level control variables: total assets, leverage, current ratio, profit margin (PAT/Sales), and return on assets (ROA). All specifications include firm fixed effects and state-by-year fixed effects to control for time-invariant firm characteristics and time-varying state-level shocks. Standard errors are robust, clustered at the corresponding state level, and adjusted for heteroskedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Log borrowing		Log gross fixed assets		Log gross plant		Log sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X High Legal	-0.148***	-0.107***	-0.205***	-0.134***	-0.135***	-0.100***	-0.075*	-0.019
	(0.030)	(0.031)	(0.018)	(0.022)	(0.022)	(0.020)	(0.037)	(0.040)
Observations	237,006	169,884	262,474	165,383	175,747	133,877	235,153	169,854
R-squared	0.883	0.881	0.945	0.959	0.954	0.961	0.913	0.921
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A12: High Court YouTube Archive and Matched Sample Results

This table presents information on High Court live-streaming archives and corresponding regression estimates. Panel A documents the total number of videos collected from official YouTube channels of treated High Courts. Panel B presents robustness estimates of the effect of live-streaming on the probability of a government win using the matched subsample of cases that are definitively identified as having been live-streamed and archived on YouTube. All variables are defined as in Table 3. Standard errors are clustered at the court level.

Panel A: High Court YouTube Archive

High Court	**Videos**
Gujarat High Court	5,369
High Court of Calcutta	5,047
High Court of Gauhati	3,028
High Court of Orissa	2,592
High Court of Karnataka	2,452
Meghalaya High Court	2,200
Jharkhand High Court	1,033
Patna High Court	705
Chhattisgarh High Court	280
Total	22,874

Panel B: Matched YouTube Sample

	Gov win	
	(1)	(2)
Post	0.078**	0.067**
	(0.036)	(0.087)
Observations	8,110	7,519
R-squared	0.294	0.303
Control variables	No	Yes
Court F.E.	Yes	Yes
Judge F.E.	Yes	Yes
Year F.E.	Yes	Yes

Table A14: Robustness: Government Appeal Strategy

This table presents the robustness related to the change in the government's appeal strategy after the introduction of live-streaming. The overall sample period spans january 2016 to December 2025. The data are at the case level in columns 1 and 2, and at the Income Tax court-year level in columns 3 and 4. The variable *govwin* is as defined in Table 3. The variable *post*1 takes a value of one for one year after the introduction of live-streaming in respective courts, and zero otherwise. It takes a value of zero for the high courts that never introduced live-streaming during our sample period. The variable *post*2 takes a value of one two years or more after the introduction of live-streaming in respective courts, and zero otherwise. It takes a value of zero for the high courts that never introduced live-streaming during our sample period. The variable *post* takes a value of one two years or more after the introduction of live-streaming in respective courts, and zero otherwise. It takes a value of zero for the high courts that never introduced live-streaming during our sample period. The variable appeal ratio is the proportion of cases in the Income Tax tribunal lost by the government that were appealed by the government in the High Court. We include court, judge, and year fixed effects in columns 1 and 2, and court and year fixed effects in columns 3 and 4. Standard errors are clustered at the High Court level, robust, and adjusted for heteroskedasticity in all columns. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Gov win		Appeal Ratio	
	(1)	(2)	(3)	(4)
Post1	0.067**	0.095**		
	(0.026)	(0.041)		
Post2	0.080*	0.087**		
	(0.044)	(0.039)		
Post			-0.019	-0.001
			(0.044)	(0.034)
Observations	9,368	8,792	160	135
R-squared	0.239	0.241	0.182	0.239
Control variables	No	Yes	No	Yes
Court F.E.	Yes	Yes	Yes	Yes
Judge F.E.	Yes	Yes	No	No
Year F.E.	Yes	Yes	Yes	Yes

B1 Zero-Shot Learning Method

This study employs Zero-Shot Learning (ZSL) for automated text classification of the economic orientation of judicial decisions. Zero-shot learning, formalized by Larochelle et al. (2008) and extended by Lampert et al. (2009), is a machine learning methodology that assigns texts to categories without requiring labeled training examples from those categories. In contrast to supervised learning methods, which require a training dataset where each observation is manually labeled with its correct category, ZSL utilizes the semantic representations encoded in large-scale pretrained language models.

A pretrained language model is a neural network that has been trained on large text corpora to learn statistical patterns and semantic relationships in language. These models encode meaning through high-dimensional vector representations that capture contextual relationships between words and phrases. The core mechanism of zero-shot learning involves evaluating the alignment between a text and descriptive statements. We structure the classification task as follows: each judicial text is assessed against three hypotheses: "This judgment favors government regulation of business," "This judgment favors free-market principles," and "This judgment is neutral in its economic orientation." The model determines the degree to which the textual content supports each hypothesis, expressed as a probability distribution over the three categories. This approach reformulates classification as an assessment of entailment, defined as the probability that a given judgment's content logically supports each economic orientation hypothesis.

For example, consider the following excerpt from a High Court judgment concerning environmental regulation:

"The Court holds that the precautionary principle and the polluter pays principle are essential features of sustainable development. The State has a constitutional obligation to protect the environment and prevent activities that cause ecological degradation, even when complete scientific certainty regarding harm is absent."

The zero-shot classifier assigns probabilities to each hypothesis based on textual content. For this sentence, the model assigns a probability of 0.69 to "pro-regulation," 0.16 to "pro-market," and 0.15 to "neutral." The text is classified as pro-regulation given the substantially higher probability for this hypothesis.

We implement BART-large-mnli (Bidirectional and Auto-Regressive Transformer), a transformer-based neural network architecture developed by Lewis et al. (2020). A transformer is a neural network architecture that processes text using attention mechanisms, which are computational operations that weigh the relevance of different parts of the input when generating representations. BART combines bidirectional encoding, which processes text in both forward and backward directions to capture context from the entire sequence, with autoregressive decoding mechanisms that generate output sequentially. The model is pretrained through a de-noising objective: it learns to reconstruct original text from corrupted versions where portions have been deleted, reordered, or masked. This pretraining process, conducted on large text corpora, enables BART to develop representations of semantic relationships and textual meaning without task-specific supervision.

The specific variant we employ, BART-large-mnli, has been fine-tuned on the Multi-Genre Natural Language Inference (MNLI) dataset (Williams et al., 2018). Fine-tuning refers to additional training on a specific task after initial pretraining, adapting the model's

parameters to perform well on that task. The MNLI dataset contains approximately 433,000 sentence pairs labeled across multiple text genres. Each pair consists of a premise (an initial statement) and a hypothesis (a proposed conclusion), with labels indicating whether the hypothesis is entailed by (logically follows from), contradicts, or is neutral with respect to the premise. Natural language inference (NLI) is the task of determining the logical relationship between these text pairs. This fine-tuning process adapts BART's general language understanding capabilities specifically for assessing logical relationships between text segments.

The connection between BART-large-mnli and zero-shot learning operates through the framework of entailment classification. In zero-shot text classification, each category is represented as a natural language hypothesis expressed in plain language. The model treats the input text (the judicial decision) as a premise and evaluates whether this premise entails each candidate hypothesis (the three economic orientation descriptions). The NLI framework provides a principled probabilistic measure of how strongly a text supports different categorical descriptions. Critically, this evaluation does not require training examples of judicial texts labeled with economic orientations. BART-large-mnli's training on diverse textual entailment tasks across multiple domains enables it to generalize this entailment assessment to new domains and hypotheses not present in its training data, including judicial texts and economic orientation categories. This generalization capacity, defined as the ability to apply learned entailment reasoning to novel category descriptions and text domains, is the mechanism through which zero-shot classification is achieved.

The classification procedure involves segmenting each judgment into chunks of approximately 20 sentences. This segmentation addresses computational constraints, as transformer models have limited input length capacities, and maintains processing efficiency. Each chunk is independently evaluated against the three hypotheses, generating entailment probabilities that sum to one across the three categories. These probabilities are aggregated across all chunks within a judgment using arithmetic averaging to determine its overall economic orientation. The final classification assigns each judgment to the category with the highest average probability.

This methodology offers several advantages for textual analysis in economics research. First, it enables analysis of large judicial datasets without the resource requirements of manual coding, where human annotators read and categorize each text. Second, it applies uniform classification criteria across all observations, removing potential inter-coder reliability concerns. Inter-coder reliability refers to the risk that different human coders may interpret and apply classification rules inconsistently. Third, the probabilistic output provides continuous measures of economic orientation rather than discrete categories, facilitating econometric analysis that can incorporate the degree of confidence in each classification. Fourth, the use of explicit hypotheses stated in natural language maintains methodological transparency and interpretability, allowing researchers to understand and validate the classification logic. These classifications serve as the basis for empirical analysis of patterns in judicial decision-making across economic bias dimensions.

B2 Robustness: Change in Litigation Strategy

In this section, we elaborate on the strategy to address the concern that the observed shift in judicial language reflects changes in the government's litigation strategy rather than changes in judicial behavior. Specifically, in response to live-streaming and anticipated public scrutiny, government attorneys may have altered the framing or rhetoric of their submissions. To address this concern, we estimate the textual similarity between the submissions of the parties for each case with the judicial reasoning in the pre-live-streaming period. Note that, in our main specification we estimate the similarity score between the submissions of the two paries and the corresponding cases' judicial reasoning. However, in our robustness test, we do not estimate the similarity between the vectors corresponding to the submissions and the vector corresponding to the respective cases' judicial reasoning. Instead we estimate the similarity between the vectors of the submissions of the two parties, and the average vector corresponding to all the judicial reasonings in the pre-live streaming period (2016 to 2019). with the corresponding cases' judicial reasoning. The key intuition is as follows. If the post-reform shift is driven by changes in government submissions, then the similarity between post-reform government submissions and pre-reform judicial reasoning should decline more compared to the relationship between post-reform business submissions and pre-reform judicial reasoning in a difference in difference sense. In contrast, if judges are independently shifting their reasoning toward the government's position in the post-reform period, then no such decline should be observed.

Specifically, we define cosine similarity scores between the TF-IDF vector of government submissions and the average TF-IDF vector for pre-reform judicial reasoning, and in an analogous way the similarity between the business submissions and average pre-reform judicial reasoning. We then measure their ratio as described above. We estimate Equation 4 using the above ratio as the dependent variable. Column 1 in Table B2.1 reports the result. We find no significant relative change in textual similarity between pre-reform judicial reasoning and post-reform government submissions compared with pre-reform judicial reasoning and post-reform business submissions. This stability indicates that the linguistic structure of party submissions did not undergo a structural break following live-streaming. Instead, the evidence is more consistent with judges shifting the orientation of their reasoning, rather than merely echoing altered government language.

Moreover, we compare the relative lengths and sentiments of government and business submissions in the pre- and post-live-streaming periods. If there is a systematic shift in the content of government and business submissions after the introduction of live-streaming, we expect changes in the relative length and sentiment of these submissions. We measure the relative length of the submissions as the ratio between the number of words in the submissions and the total number of words in the judgment. To measure sentiment, we use the VADER sentiment score. We then estimate Equation 1 using the measure of relative length and different dimensions of the sentiment score as dependent variables.

We present the results in the last six columns of Table B2.1. Columns 2, 3, and 4 (5, 6, and 7) present the results with scores of government (business) submissions as the dependent variable. Specifically, in columns 2 and 5, we present results with the relative length as the dependent variable; in columns 3 and 6, we present results with a score capturing negative sentiment; and in columns 4 and 7, we present results with a score capturing positive senti-

ment. Across measures, we do not find any significant differences in the relative lengths or sentiments of government and business submissions after the introduction of live-streaming.

Taken together, these findings suggest that changes in litigation strategy are unlikely to account for the documented increase in pro-government judicial orientation.

Table B2.1: Robustness: Government Litigation Strategy

This table presents the robustness related to the change in the government's litigation strategy after the introduction of live-streaming. The overall sample period spans January 2016 to December 2025. The data are at the case level. The variable *similarityratio* is the similarity score of the government submission and the pre-live-streaming period judgment text to the similarity score of the business submission and the pre-live-streaming period judgment text ratio. The similarity score is measured as in Table 4. The variable *textratio* is the ratio between the number of words in the submissions and the total number of words in the judgment. The variables *negative* and *positive* are the negative and positive sentiment scores defined based on the VADER sentiment measure, respectively. We include court, judge, and year fixed effects in all columns. Standard errors are clustered at the High Court level, robust, and adjusted for heteroskedasticity in all columns. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Similarity Ratio	Government			Business		
		Text Ratio	Negative	Positive	Text Ratio	Negative	Positive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	0.086	-0.046	0.001	-0.002	-0.017	0.000	0.003
	(0.055)	(0.042)	(0.002)	(0.004)	(0.031)	(0.004)	(0.003)
Observations	7,936	12,117	12,117	12,117	12,117	12,117	12,117
R-squared	0.247	0.596	0.235	0.226	0.555	0.347	0.284
Court F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes

B3 Robustness: Strength of local government

In this section, we show the robustness of our result related to the incremental effect of strength of local government of the change in government victory post the implementation of live streaming. As opposed to running a triple interaction shown in the main specification, we restrict the cases to the ones involving state government and estimate the following regression specification with a double interaction term:

$$\mathrm{Y}_{ijkt} = \beta_1 \cdot Post_{jt} \times \mathrm{Margin}_{jt} + \beta_2 \cdot Post_{jt} + \beta_3 \cdot Margin_{jt} + \alpha_j + \gamma_t + \delta_k + X_{jt} + \varepsilon_{ijt} \quad (13)$$

The variables are as defined in equation 7. The coefficient of interest is β_1 in the interaction term $\mathrm{Post}_{jt} \times \mathrm{Margin}_{jt}$, which captures the differential effect of live streaming on government win rates when the state government has a strong electoral mandate. Standard errors are clustered at the High Court level.

Table B3.1 presents the results. The coefficient on Post × Margin is positive and statistically significant. The coefficient on *Post* alone is statistically indistinguishable from zero, suggesting that the effect of live streaming on government win rates is more pronounced in states with strong governments. This pattern indicates that electoral strength moderates the impact of transparency on judicial outcomes. The results are consistent with Table 6.

Table B3.1: Effects of Live-streaming on Government Win: Strong vs. Weak Governments

This table presents difference-in-differences estimates of how High Court live-streaming affects case outcomes, differentiated by government strength and case type. The data are at a judgment level. We limit the sample to cases involving state government. We identify such cases where either of the party names does not contain "Union of India" or "Government of India." The variable *gov win* is as defined in Table 3. The variable *Post* equals one for cases after the corresponding High Court adopted live-streaming. The variable *Margin* is the percentage difference between the number of electoral seats won by the ruling party in the state and the number of seats won by the runner-up. Columns (1) includes the full set of control variables listed in Table 3. Column (2) includes the control variables - proportion of business-government cases, pendency per judge, GDP growth, credit growth and an indicator identifying states led by a national party - All columns include Court, Year, and Judge fixed effects in their respective specifications. Standard errors are clustered at the High Court level, robust, and adjusted for heteroskedasticity. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Gov win	
	(1)	(2)
Post X Margin	0.316*	0.354**
	(0.176)	(0.171)
Post	-0.019	0.026
	(0.044)	(0.436)
Margin	0.012	0.022
	(0.176)	(0.174)
Observations	3,949	3,747
R-squared	0.372	0.383
Control variables	No	Yes
Court F.E.	Yes	Yes
Judge F.E.	Yes	Yes
Year F.E.	Yes	Yes

B4 Methodology: Linking Judgments to Hearing Transcripts

To examine whether court judgments correspond to publicly live-streamed hearings, we developed a pipeline that automatically matches structured case records against speech transcripts extracted from YouTube videos. The process proceeds in three stages: transcript extraction, candidate retrieval, and LLM-based verification.

B4.1 Transcript Extraction

We collected YouTube videos published on official High Court channels and extracted their spoken content using the YouTube Transcript API, which retrieves auto-generated or manually uploaded captions for each video. For each calendar day on which one or more videos were published, we concatenated all captions into a single merged transcript, yielding a corpus of daily transcript documents. Each document was associated with the video titles, publication date, and source URLs for traceability. Because the captions are produced by automatic speech recognition (ASR), they contain OCR-style noise, words may be mis-transcribed, run together, or inconsistently capitalised, and the pipeline was designed throughout to tolerate such errors.

B4.2 Candidate Retrieval

For each judgment in our dataset, we extracted a set of structured signals from the case record: a canonical case number (e.g., *WP 1234/2022*), tokens derived from the case title and party names, judge name tokens, and contextual keywords drawn from the judgment text and laws cited. We then scored every daily transcript document against these signals using a composite lexical function. The scoring rewarded exact matches on typed case references (+80 points), overlap on number–year pairs such as "1234 of 2022" (+35 points), and TF-IDF-weighted overlap on title and context tokens. A date-proximity prior was added to reflect the expectation that a hearing transcript should appear on or near the judgment's publish date (+40 points for the same day, +28 for an adjacent day). The top three scoring transcript days within a configurable date window were retained as candidates for the next stage.

B4.3 LLM-Based Match Verification

Each candidate was passed, together with the full case profile, to a large language model (Gemini 2.5 Flash Lite) for a final match decision. The prompt supplied the model with the case number, title, judge names, laws cited, and a 2,500-character excerpt from the judgment, alongside the complete merged transcript text for each candidate day. The model was instructed to return a structured JSON response specifying a match decision (*high*, *medium*, *low*, or *no_match*), a confidence score from 0 to 100, and the specific case numbers and party names it observed in the transcript text. Crucially, the prompt specified that an overlap in hearing date or judge name alone was never sufficient evidence of a match; a valid

match required at least one case-specific anchor, such as a case number fragment, a party or entity name, a statute, or a distinctive factual detail, to appear in the transcript itself.

B4.4 Post-hoc Anchor Verification

Because language models can hallucinate plausible-sounding evidence, we applied a deterministic guardrail layer to every model output before accepting a match. This layer independently searched the normalised transcript text for the case number strings, party name phrases, and supporting evidence phrases that the model claimed to have found. A match was accepted only if at least one such anchor was verified to appear in the transcript. Matches resting solely on date or judge overlap were automatically downgraded to *no_match* regardless of the model's stated confidence. Conversely, verified anchors triggered upward adjustments: a confirmed exact case number in the transcript yielded a final confidence of at least 98, and a confirmed party name yielded at least 95. This two-layer design, neural retrieval followed by symbolic verification, was intended to combine the linguistic flexibility of an LLM with the precision of exact-match checks, reducing both false positives driven by superficial overlap and false negatives caused by minor transcription noise.

B5 Uncertainty: BERT Algorithm

To empirically assess whether judicial reforms fundamentally altered decision-making patterns, we employ a machine learning approach designed to replicate judicial outcomes based solely on the legal text of judgments. Our methodology centers on training legal text classifier exclusively on pre period cases and subsequently testing its predictive accuracy across different post-reform samples. This allows us to identify structural breaks in judicial reasoning by examining whether a model that performs well in the pre-reform period loses predictive power once the institutional environment changes.

For each case in our dataset, we combine textual components that capture the essential elements of judicial reasoning as described in section 3. Specifically, we concatenate six labeled sections from the judgment text: Facts (the underlying circumstances of the dispute), Issues (the legal questions to be resolved), Petitioner's Arguments (the plaintiff's legal contentions), Respondent's Arguments (the defendant's legal positions), Precedent Analysis (the court's examination of relevant case law), and Judge's interpretations of the matter. These sections collectively capture the substantive content upon which judicial decisions are based, while excluding the Conclusion section to avoid information leakage from the outcome itself. We then construct the dependent variable, y_gov_win, as a binary indicator equal to one if the government party prevails in the litigation and zero otherwise. We exclude cases with ambiguous or unclear outcomes to ensure the precision of the training labels.

The pre-period sample comprises all cases decided between 2000 to October 1, 2020—the date on which the first High Court adopted live-streaming. We use this sample to train our model. This temporal restriction is essential for our identification strategy. It guarantees that any changes in predictive accuracy we observe in post-reform periods cannot be attributed to the model having learned from post-reform data. The staggered introduction of live-streaming generates two post-reform comparison groups. The control group

consists of judgments delivered after the reform announcement but before live-streaming implementation in that court, while the treatment group includes judgments delivered after live-streaming began in that court. Comparing model performance across these two post-reform groups enables us to distinguish between general time trends and reform-specific effects.

The core of our approach is the use of InLegalBERT, a transformer-based large language model fine-tuned for Indian legal text classification. InLegalBERT is a specialized variant of the Bidirectional Encoder Representations from Transformers (BERT) architecture, pre-trained on a large corpus of Indian court judgments, statutes, and legal documents up to 2016 (Paul et al. (2022)). The model captures the syntactic and semantic regularities of legal argumentation in Indian judicial language, such as formal phraseology, precedent references, and reasoning structure, making it particularly well suited for downstream tasks involving Indian case law (Paul et al. (2022)). We fine-tune InLegalBERT on our labeled pre-reform dataset, using a standard supervised classification setup where the model learns to predict the probability of a government victory from the concatenated text inputs.

The training sample is randomly split into an 80 percent training set and a 20 percent validation set, following standard machine-learning practice (Bertsimas and Paskov (2020)). After training the model on pre-reform cases, we apply it to generate predicted probabilities of government victory across both the control and treatment periods.

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