Judicial Discretion, Credit, and the Real Economy *

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Abstract

We investigate how court rulings affect banks' views on the protection of creditor rights, their lending practices, and the ultimate effects on business performance. Leveraging the random assignment of judges to cases brought against financial institutions, we demonstrate that banks restrict credit after observing unfavorable decisions issued by pro-debtor judges in disputes involving them. This informational shock is transmitted to firms within banks' relationships through credit rationing for small businesses, adversely affecting their performance. Our research highlights the significant role of judges in shaping economic activity beyond the immediate parties involved in a legal conflict.

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The literature has documented a positive association between the effective protection of creditor rights and the development of financial markets (La Porta et al., 1997, 1998; Djankov et al., 2008). Laws that guarantee the enforcement of contracts and debt recovery reduce risks and create incentives for repayment. However, the application of these rules depends on judges' decisions. If judges are free to exercise discretion and decide based on personal preferences, similar cases may have different outcomes. Therefore, not only the written rule but also the way judges decide impacts how society perceives laws' effectiveness and, for banks, the expected legal risk associated with lending.

Since Basel II, legal risk has been defined as a subset of operational risk, integrating it into banks' overall risk management. Even before that, the Federal Reserve Bank, through the Division of Banking Supervision and Regulation, incorporated legal risk oversight within their risk management guidelines (Board of Governors of the Federal Reserve System, 1995)[1]. In Brazil. It is ubiquitous in banks' financial reports that judicial risks are managed considering the likelihood of losing lawsuits in which they are involved. The reports point out that the assessment depends on various factors, including the specific circumstances of each case, the complexity of legal proceedings, and the bank's previous experience with similar claims.

This paper describes the informational channel through which judicial decisions impact banks' perceptions of the creditor-friendliness behavior of local courts – thus, the local legal risk of lending – and its economic consequences. For that, we test if banks restrict credit supply after observing adverse judicial decisions on lawsuits moved against them and if this informational shock is transmitted to firms in their relationship. Since the probability of losing a lawsuit is associated with unobserved variables, such as lawyers' quality or contract characteristics, we leverage judicial discretion and the random assignment of judges to lawsuits as an exogenous source of variability in trial outcomes. The objective is to examine whether banks reduce credit in response to less favorable decisions when more lenient judges arbitrate their disputes. Additionally, we investigate whether the worsening credit conditions of adversely affected banks are transmitted to firms with credit contracts expiring just after creditors acquire new information about local judicial leniency through court rulings. We focus on Brazil, which is well suited to carry out this type of study, as it has independent justice, well-paid tenured judges that cannot be transferred between

[1]In the document *"Legal risk arises from the potential that unenforceable contracts, lawsuits, or adverse judgements can disrupt or otherwise negatively affect the operations or condition of a banking organization."*

districts against their will[2], and the possibility for the plaintiff to choose the district to file a lawsuit is very restrictive.

Moreover, the pro-debtor bias of the Brazilian judicial system has long been a topic of debate (Arida et al., 2005). Surveys conducted with judiciary members by Pinheiro (2003) and Lamounier (2002) indicate that most respondents believe that social concerns can justify decisions in breach of contracts. Despite that, we show that there is significant variation in judges' inclination to favor debtors. Allied to this rich institutional environment, we have available the universe of judicial sentences for courts of the State of São Paulo, comprehensive data on credit contracts provided by the Brazilian Central Bank, and data on the formal labor market at the firm level from the Ministry of Employment and Labor.

We start the analysis by compiling 339.671 decisions, spanning 2013 to 2018, from the Court of Justice of the State of São Paulo (TJSP), corresponding to civil cases in which financial institutions are defendants in litigation[3]. From these sentences, if the plaintiff had his demand fully or partially granted, we classify it as a pro-debtor decision. The trials involving financial institutions were selected by an approximate match of the defendants' written names in the sentences and the names in the list of financial institutions authorized to operate by the Brazilian Central Bank. In the sample, we precisely identified sentences related to Brazil's four largest financial conglomerates, comprising two-thirds of the observations.

Since judges are randomly assigned to lawsuits within the same judicial district, it is possible to identify banks' responses after the results of their trials using a composition of judges' pro-debtor bias as an instrument to the proportion of pro-debtor decisions they observe. This empirical approach is very similar to multiple papers studying the impact of specific decisions on the parties of the litigation[4], but differently from them, we have a continuous treatment (the proportion of pro-debtor decisions) with a composition of judge fixed effects as instruments.

Next, we investigate the transmission of judicial shocks experienced by banks to the firms they are connected with. To do this, we leverage the timing of short-term credit contract maturities to assign the treatment and the corresponding judicial bias banks face to their clients in the district and quarter where they operate. For

[2]Weak incentives to limit discretion as discussed by Posner (2008).

[3]We show in section 2 that these types of lawsuits are typically claims about contract review and compensation for damages caused by financial institutions misconduct.

[4]Frandsen et al. (2023) provides a complete review of the literature that relies on this identification strategy. He also proposes a test for the identification assumptions for binary treatment, which is not our case.

instance, if judicial rulings involve a bank in a specific quarter and district, we assign the proportion of pro-debtor decisions and the judicial bias related to this event to all firms operating in the same court district with credit contracts expiring with this bank in the following quarter. When examining the impact on firms' performance using labor market outcomes, which are only available annually, we try to capture the most relevant shock using the quarter and bank associated with the highest value of contracts expiring in the year to assign the treatment to the borrower the same way as before. Short-term credit is particularly suitable for this analysis because it offers banks discretion while firms face greater urgency for renewal, minimizing self-selection concerns.

This approach assumes that bank shocks are more likely to impact firms when contracts are due for renewal and allows us to observe how the change in banks' view on creditor protection and local legal risk impacts the issuance of new contracts for these firms. Moreover, as highlighted in Chodorow-Reich (2014), sticky relationships make firms susceptible to changes in their bank's behavior. Combining this transmission effect and the limitation of borrowers in switching banks outlines a pathway through which changes in lenders' perceptions of creditor protection due to judge discretion influencing lawsuits resolutions impacts firms' performance via shifts in credit conditions.

The results obtained in this paper confirm that banks restrict credit when institutions are perceived as unable to protect creditor rights. We find that an increase of 10% in pro-debtor judgments against banks due to having faced more lenient judges implies a 23 percentage points larger proportional decrease (smaller increase) in the amount of new credit between quarters. Furthermore, this result is accompanied by a relative increase of 7.5 percentage points in the average interest rate of new contracts. By comparing judicial trials within the same district, we minimize concerns about omitted variables, usually present in studies that use more aggregated data, as the random assignment of judges allows us to identify an ex-ante credible exogenous variation in treatment. Nevertheless, the most appealing feature of this quasi-experimental design is the possibility of observing changes in banks' legal risk perception, controlling for the same institutional environment, highlighting the role of judges in setting incentives for the function of credit markets.

These findings suggest that differences in the behavior of local judges impact banks, which we could even extrapolate to a broader context involving nations' judicial systems and laws. However, whether this local variation in judicial leniency

impacts borrowers is unclear. If the cost of switching bank relationships is low, we would not expect to see any impact from the variation in the local relative bias banks face. To address this issue, we observe changes in credit terms for firms with contracts that are to expire.

We show that firms whose credit matures in the quarter immediately following adverse court decisions observed by the respective creditor bank are less likely to enter into new contracts with the same institution. This effect is particularly pronounced among small businesses, which make up 80% of the sample and account for 70% of formal employment in the country. Focusing on this group, we demonstrate that a 10% increase in pro-debtor decisions observed by the connected bank, driven by more lenient judges, is followed by a one percentage point lower probability of obtaining a new contract and a 13 percentage point proportional decrease in the amount of new credit obtained in the quarter that a firm has a contract expiring. This effect holds even after considering only firms that obtained credit in the previous quarter. However, we do not find any significant effect on the amount of credit or interest rates for firms that obtain a new contract, conditional on having obtained a contract in the previous quarter. Putting all the results together, we conclude that judicial decisions impact banks and firms connected to them, especially the small ones, with suggestive evidence that this effect is driven by the exclusion of some companies from the market.

Finally, we explore how lenders' perception of a local hostile institutional environment affects borrowers' performance. If firms have difficulty obtaining new credit, it may have consequences on their activities. To test this, we use the variation in total wage bill and employment between years as dependent variables for the same set of firms as before. Since we have yearly data on labor market outcomes and quarterly data on banks, we assign the treatment to the firms based on the bank-quarter-district observation with the most credit contracts expiring. When we exclude firms observed in the fourth quarter of the year, the results show that a 10% increase in pro-debtor decisions observed by the relationship bank just before a firm has its largest quarterly value of credit expiring is negatively associated with a proportional decrease between years of 3.6 and 3.5 percentage points in firms' wage bill and total employment, respectively. We also find that this effect is concentrated in smaller firms, as in the previous results.

Related Literature:

The first contribution of our paper to the literature is to the debate on the impact

of creditor rights protection on credit markets. Since the seminal papers of La Porta et al. (1997, 1998), this discussion has focused mainly on the strength of law and the quality of judicial enforcement. While studies such as Djankov et al. (2008) or Araujo et al. (2012) exploit law reforms in cross-country comparisons to show that better credit recovery regulations result in more loans at lower prices, others use variation in justice quality across regions within countries to obtain similar results (Visaria, 2009; Jappelli et al., 2005)[5]. By examining the introduction of the new bankruptcy law and the variation in congestion levels across districts in Brazil, Ponticelli and Alencar (2016) demonstrates the crucial role of justice quality in implementing legal reforms that enhance the protection of creditor rights. Our paper exploits a different channel in enforcement quality: the variation in judges' pro-debtor bias affecting banks' perception of the effective level of protection provided by law. The within-district variation rules out concerns about the endogeneity in legal reforms across countries or differences in judicial celerity or specialization between regions of a country. The only variation in creditor rights protection faced by financial institutions in our setting is the judges assigned to their trials, which is plausible exogenous. Moreover, we highlight that individual judges' behavior impact credit markets, affecting firms beyond the litigation parts.

The second stream of literature related to this paper meets the last point of the previous paragraph. Some authors exploit judges' specific preferences in litigation resolutions to identify the effects of judicial decisions on individuals or firms directly related to the legal dispute. For example, Chang and Schoar (2013), Araujo et al. (2023), and Bernstein et al. (2019) use differences in the propensity of judges to decide for firm reorganization over liquidation in bankruptcy cases to study the impact on firm performance, asset allocation, and labor market outcomes. Differently from them, our paper uses the variation in judicial pro-debtor bias in lawsuits filed against financial institutions to study the impact of the effective application of the law on banks' disposal to lend. When we study the impact on firms' outcomes, the results are based on the transmission of this local bank-level shock to firms of their relationship and not due to the direct effect of a specific lawsuit definition they are involved. This result is also related to the negative impact of judicial uncertainty on credit markets discussed in Costa and De Mello (2008), Gennaioli (2013), and Lee et al. (2022). In our paper, uncertainty acts beyond risk in judicial decisions (known variation in judges

[5]Examples of within-country studies on law reforms are Coelho et al. (2012), Assunçao et al. (2014), Rodano et al. (2016), Aretz et al. (2020)

bias), but through the difference in expectations about creditor rights protection between banks. The varying results of past judicial decisions have diverse effects on their expectations of future outcomes, although all banks are bound by the same legal framework, court standards, and judges. Related to this variation in judicial bias faced by banks within the same judicial district, if firms were able to easily switch banks, we would not expect any impact on their economic performance. However, our research demonstrates this is not the case, connecting this paper to a third branch of the literature.

Barriers preventing borrowers from switching creditors can lead to the transmission of bank idiosyncratic shocks to firms connected to them. For example, although relationship lending minimizes frictions in credit markets, facilitating soft information acquisition (Petersen and Rajan, 1994), it makes switching banks costly and restricts firms from getting credit in other banks (Darmouni, 2020)[6]. The consequence of this friction in credit mobility is that bank-specific shocks impact real economy outcomes (Chodorow-Reich, 2014; Huber, 2018). In this paper, we demonstrate that small businesses with credit contracts maturing when their bank observes negative lawsuit resolutions face greater challenges in obtaining new credit. In addition, these same firms experience a proportional decrease in wages and employment within the same year.

The remaining sections of the paper are structured as follows: section 1 provides a brief overview of the institutional environment relevant to the research; section 2 presents the sources of information used to construct the data sets employed in the empirical analysis; section 3 introduces a conceptual framework that guides the interpretation of the empirical results; section 4 offers a comprehensive discussion on the empirical strategy; section 5 presents and examines the primary findings of the paper; section 6 includes supplementary results and robustness checks; and section 7 concludes the paper.

1 The Brazilian Judicial System

This section describes the institutional environment of legal disputes concerning creditors and debtors in Brazil, focusing on the judicial system of the state of São Paulo (*Tribunal de Jusitça de São Paulo*). São Paulo is the largest Brazilian state, representing more than 20% of the country's population and approximately 30% of the

[6] Schenone (2010) is a good example of how banks take advantage of this information monopoly.

national GDP. The state justice system is one of the largest in the world and is responsible for a quarter of all ongoing lawsuits in Brazil. In addition, lawsuits filed against financial institutions operating in the state are the primary source of demand for local courts[7].

During the period studied in this paper, local courts were spread across 319 judicial districts, covering 615 municipalities. Large municipalities are directly mapped to districts, while smaller municipalities are grouped under the same local jurisdiction. The capital city is the exception, as it has multiple regional courts with different responsibilities depending on the location and the value of the claim. Although states organize the judicial system, any relevant law is determined nationally. States are responsible for lower-complexity residual legislation.

The Brazilian legal system follows the tradition of Civil Law, commonly associated with low levels of judicial discretion due to restrictive rules of procedure[8]. However, as discussed in Reschke (2020), judges in Brazil have uncontrolled discretion in judicial decisions guaranteed by "the principle of the judge's free conviction". In fact, this discussion was central to the new reform of the civil procedure code that came into effect in 2016 (Gomes, 2018). In addition to this strong allowance for judicial discretion, there is a long debate about diffused anti-creditor bias in Brazilian society (Arida et al., 2005). Specifically related to judicial pro-debtor bias, Pinheiro (2003) document in a survey with approximately 700 judges from multiple trial and appellate courts that when confronted with a question asking about social concerns in lawsuits involving credit contracts, only 19.7% agree that contracts must be enforced regardless of social considerations. In the same spirit, Lamounier (2002) conducted a survey with approximately 500 bureaucrats from the judiciary, legislative, and executive branches with similar findings. In this study, among members of the judiciary, 61% agreed that the judge has social functions that justify decisions in breach of contracts.

Finally, two common characteristics of legal systems allow us to explore the heterogeneity in judges' preferences and the possibility of discretion in decisions. First, there is a substantial limitation for forum shopping to limit the plaintiff's strategic behavior in choosing which judicial district to fill the petition. In conflicts involving parties established in a particular jurisdiction, as a general rule, they must sue the local court. Second, and more relevant, cases are assigned randomly between dis-

[7]Banking-related cases are usually heard in local courts, except for those involving the Caixa Economica Federal, which is wholly owned by the government and has its lawsuits judged by federal courts.

[8]Gidi (2003) provides a brief comparison between Brazilian and American civil procedures.

trict courts within the same judicial district. This fact guarantees that banks cannot choose the judge who decides their cases and provides an exogenous variation in the judicial bias they face in their trials.

2 Data

This section outlines the main data sets employed in the analysis throughout the paper. We gather sentences from the State Court of Justice of São Paulo (TJSP) that involve banks as defendants in legal proceedings. Credit data comes from the Credit Information System (SCR), and bank branches' balance sheets from the Banking Statistics Report (ESTBAN), both provided by the Central Bank of Brazil. The Annual Social Information Report (RAIS) provides information on labor market outcomes.

2.1 Judicial Sentences

To construct the data set of judicial sentences, we extracted the text of judicial decisions from civil court trials of the state of São Paulo classified as *"procedimento comum cível"* (standard civil procedure) from 2013 to 2018[9]. From the text, we extracted the process identifier, the date of the sentence, the parties, the responsible judge, the court district, and whether the request was granted. Most of these data are available in a structured format, while for the extraction of the related parties and the litigation winner, it was necessary to identify some patterns in how they are cited in the text. Since there are no identifiers of related parties, we filter actions related to financial institutions by the similarity of the names written in the sentences and the companies' names allowed to operate by the Brazilian Central Bank. For the four largest bank conglomerates and their subsidiaries, we identified their processes and classified them under the conglomerate's name. These banks (and subsidiaries) correspond to 67% of the actions in the final data set and to 60% of the total credit recorded in the balance sheets of the bank branches operating in the state of São Paulo in 2013[10]. The final data set contains 339,671 court decisions for all financial institutions allowed to operate by the Central Bank, which respond to the state

[9]The universe of judicial sentences are available in https://esaj.tjsp.jus.br/cjpg/.

[10]The fifth large bank in Brazi, Caixa Economica Federal, is not part of the constructed dataset because it answers directly to the federal authority.

authority, from 2013 to 2018[11]. Each observation corresponds to the first judicial sentence for each lawsuit, once any posterior judicial review is requested after knowing the case's judge. The extraction of text and the categorization of sentences in detail is outlined in the Appendix A.

The universe of judicial decisions in this paper includes those in which financial institutions were defendants in the litigation. This restriction is motivated by the fact that they typically include situations where the debtor tries to violate the agreed contract. These are circumstances more conducive to the manifestation of the judge's discretion than actions of collection and contract enforcement, in which the nonpayment proof does not leave much doubt and room for judicial discretion in decisions. Additionally, since a substantial portion of credit contracts are subject to out-of-court debt collection, the debtor initiates the judicialization of these cases. Another fact that justifies the exclusion of lawsuits in which the creditor acts as a plaintiff to execute a debt is that the decision is made available only after the debt payment or its extinction. Consequently, some cases are prolonged for a long time or do not even have a conclusion, making it impossible to observe their outcomes. Thus, the results of lawsuits filed against financial institutions are more appropriate to capture the judicial behavior in creditor-debtor conflicts.

Table 1 shows the descriptive statistics of the judicial data set. Panel A divides the sample, first by the most popular subjects of the claims and second by the four banks identified in the sample as defendants. For these groups, the first column shows the number of sentences in the data, the second corresponds to the proportion of pro-debtor decisions by group, and the third column presents the relevance of this group in the whole data set. Panel B presents the descriptive statistics of the number of sentences issued and the proportion of pro-debtor decisions at the judge and judicial district levels.

Observing the most demanded subjects in legal actions makes it possible to verify that the sentences composing the data set reflect lawsuits related to conflicts between financial institutions and borrowers. Contractual issues are found in at least a third of the sample and may reach more, since other claims, such as moral damages, usually correspond to contractual conflicts. From the division by financial institutions, the uneven distribution of pro-debtor decisions across banks is worth noting. This heterogeneity may reflect different types of contracts, clients, strategic legal risk man-

[11] We use all judicial decisions to calculate the instrumental variable presented in the empirical section, while the final sample includes only the four largest Brazilian banks.

Table 1. Sentences and pro-debtor decisions by groups

Panel A: Sentences by subject and bank	n	pro-debtor	proportion
Subject (n = 354)			
contract revision	71,234	0.32	0.21
moral damages	41,171	0.49	0.12
banking contracts	36,372	0.41	0.11
incorrect inclusion in defaulter register	34,547	0.45	0.10
other (n = 350)	156,347	0.44	0.46
Financial institutions			
itaú unibanco	76585	0.34	0.23
bradesco	55575	0.47	0.16
santander	53472	0.42	0.16
banco do brasil	38060	0.57	0.11
other	115979	0.40	0.34
Total sentences	339,671	0.42	1.00
Panel B: Statistics by judge- and district-level	mean	median	sd
Judges (n = 1696)			
sentences	200.28	122.00	235.22
pro-debtor	0.43	0.43	0.19
Judicial district (n = 319)			
sentences	1064.80	245.00	5925.12
pro-debtor	0.42	0.43	0.11

Notes: Panel A split sentences into the four most common subjects and the four most important banks in Brazil. Panel B shows the descriptive statistics for the number of sentences and the proportion of pro-debtor decisions at the judge and judicial district levels.

agement, and other unobservable aspects. Panel B reveals that this heterogeneity in pro-debtor decisions is also present across judges and court districts. Figure B1 in Appendix B shows the distribution of the proportion of pro-debtor decisions by each judge and its correlation with total sentences.

2.2 Credit information

Credit data is obtained from two sources of information from the Central Bank of Brazil. The Credit Information System (SCR) contains private information on all credit transactions greater than 1,000 Brazilian Reals (BRL) up to 2016 and transactions of more than 200 BRL from 2016 to the first quarter of 2019, the end of the sample period. The second source of information is the Banking Statistics Report (ESTBAN), a compilation of bank branches' balance sheets updated monthly.

The SCR data specifies the lender by a unique establishment-level identifier (CNPJ) and also identifies the establishment-related financial conglomerate. For each credit register, we also have the borrower's unique identifier (CPF for individuals and CNPJ

for firms), the date of contracting, the value, and interest rates. The ESTBAN data provide information on the bank branch balance sheet that identifies the bank by municipality, the number of branches, and the balance sheet accounts that include the total credit stock for each bank by municipality and month. The data from ESTBAN make it possible to replicate the main result since it is publicly available. However, the credit stock in bank branches' balance sheets does not precisely map the credit origin, since a bank's loans to borrowers in municipalities where it is not physically present are registered in branches of other regions. Furthermore, credit stocks are less sensible in capturing short-term variations in lending conditions than the new loan information available in SCR. However, ESTBAN data results are reported when possible to confirm the findings in a fully replicable condition.

2.3 Labor market information

The Ministry of Labor provides data on the labor market through the Annual Social Information Report (RAIS). The report comprises a dataset that connects employer and employee information. It is updated annually with information provided by firms and includes details on hirings, terminations, and the last salary of each employer. Therefore, the data used in this paper pertains to the reported wages and the number of employers in December of each year at the firm level.

2.4 Final samples

Using information from judicial sentences, credit, and labor, we constructed two datasets. For the first, we aggregated justice and credit data at the bank, municipality, and quarter levels to construct a final sample linking banks' municipal credit information and local courts' decisions observed in each judicial district. We excluded municipalities in which any of the four banks did not operate in any year between 2013 and 2018. We define a bank as not operating in a municipality if it has no new credit contracts with any firm for an entire year. The motivation is to control for variation in bank competition and possible problems of attenuation bias, since even after the bank leaves a city, we still observe judicial decisions against the bank, while the associated outcome is always zero[12]. Figure 1 shows the included and excluded municipalities and the division of the judicial district for the State of São Paulo.

[12]When using ESTBAN, we limited observations to judicial districts (138) with branches from the four banks throughout the sample.

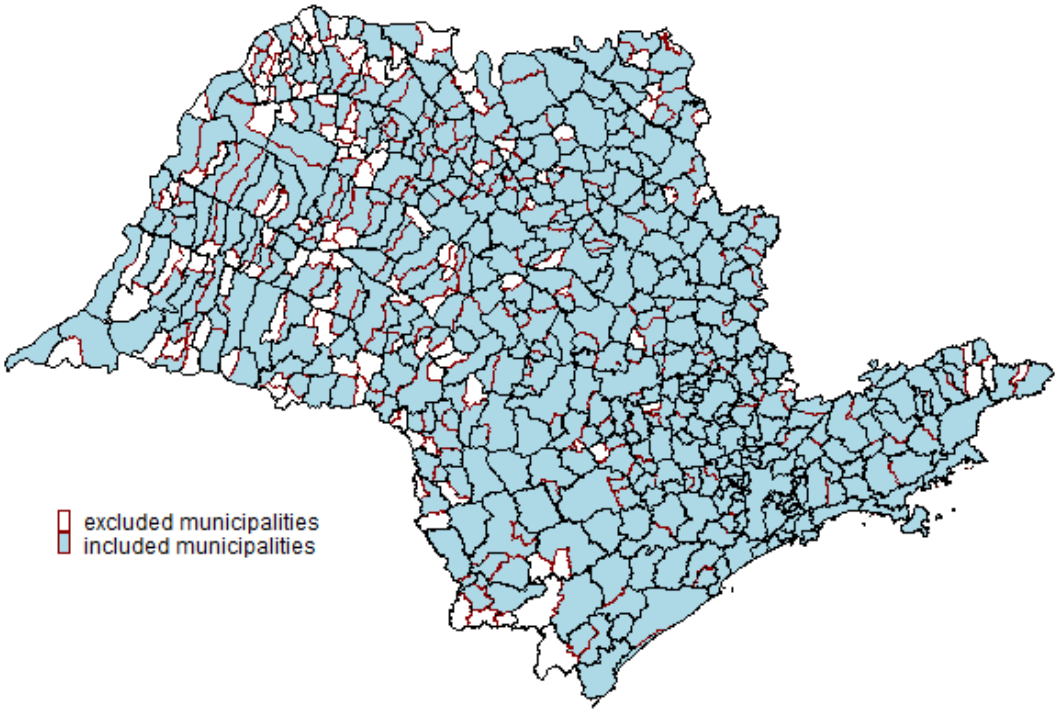

Figure 1. Included municipalities and districts.

Notes: The figure plots the division of the judicial districts (black) and municipalities (red) for the state of São Paulo. Municipalities included in the sample are in blue and excluded in white.

For the second data set, each observation corresponds to a firm-bank relationship, recorded quarterly. These quarters align with the maturity dates of firms' credit agreements with banks. In these instances, we connect the court decisions observed by banks in the district corresponding to the municipality where the firm operates, the amount of new credit agreements the firm entered into with the bank, and information regarding the number of employees and the firm's wage expenses for the year corresponding to the observed quarter. The final dataset comprises quarterly observations of firms identifiable in the RAIS system in the year of observation and the preceding year. As in the bank-level sample, the capital city, São Paulo, is omitted due to its inclusion of multiple court districts within its boundaries. The statistical summary for both datasets is presented in Table 2.

The lower number of observations for interest rates is due to certain quarters in which banks do not initiate new contracts. The table also shows that the quarterly

Table 2. Descriptive statistics of the final samples

Statistic	Mean	St. Dev.	Median	observations
Panel A: Bank level data				
log(pro-debtor+1)	0.341	0.247	0.376	32,892
log(bias+1)	0.351	0.078	0.356	32,892
log (new loans)	14.759	2.909	14.994	32,892
$\Delta \log$ (new loans)$_{t+1}$	0.016	1.537	0.004	32,892
interest rates	0.710	0.672	0.495	32,598
Δinterest rates$_{t+1}$	-0.010	0.552	-0.008	32,598
n sentences	10.7	46.1	3.0	32,598
n municipalities	507			
Panel B: Firm level data				
log(pro-debtor+1)	0.347	0.176	0.357	1,099.610
log(bias+1)	0.349	0.058	0.352	1,099.610
log (new loans)	9.711	3.099	10.074	1,099.610
$\Delta \log$ (new loans)$_{t+1}$	-1.265	4.064	-0.106	1,099.610
interest rates	0.759	0.965	0.437	1,031,036
Δinterest rates$_{t+1}$	0.075	0.782	0.003	873,000
log(employment)$_{y-1}$	2.102	1.389	1.946	1,099.610
log(employment)	2.076	1.384	1.946	1,099.610
log(wage bill)	9.337	1,553	9.199	1,099.610
log(wage bill)$_{y-1}$	9.382	1.545	9.245	1,099.610
n firms	168,485			
n small firms with a single bank relationship	132,502			

Notes: Panel The table shows the descriptive statistics for the main variables used in the empirical analysis. Panel A corresponds to the data set at bank and quarter levels. Panel B corresponds to values at firm and quarter levels. For *log(employment)* and *(wage bill)*, the variables are available for December of each year. Subscriptions t corresponds to the quarter of observation and y to the year of the quarter of observation.

fluctuations in new credit values are negative at the firm level and slightly higher than zero at the bank level. This disparity is because when looking at banks, we are interested in all new loans between quarters, while for firms, we restrict the sample to borrowers with credit maturing in the quarter. The latter case includes zero values for firms not acquiring new contracts. At the bank level, this variable also encompasses new loans for all firms, regardless they have contracts expiring during the period. Lastly, the variable *bias* serves as the instrumental variable described in the methodology section, capturing the leave-out measure of pro-debtor decisions made by the judges assigned to the cases observed by the bank in the quarter.

3 Conceptual Framework

This section discusses the theoretical framework that guides the empirical analysis developed in the rest of the paper. The main idea comes from the fact that banks react to the behavior of courts. It is ubiquitous in financial statements reports of large banks in Brazil that the provision for contingency losses depends on legal risk. In some of them, we can find explanations that the calculation of provisions applies statistical methodology to infer losses on contracts depending on the nature of the contract, characteristics of court lawsuits, and the specific jurisdiction that handles the cases. From this, the following framework captures how the banks' perception of the local court impacts lending.

The model presented here has a key feature: firms cannot commit themselves to not initiating a lawsuit in the event of a project's failure in an effort to reduce the amount of their debt. The expected loss of banks in debt contracts, for a given interest rate and loan size, is contingent upon the probability of project failure, firms' and bank's subjective expected probability of winning a lawsuit, and the cost of initiating a lawsuit. While the entrepreneur has an exogenous perception of the pro-debtor inclination of courts, the bank learns it from past experience. The entrepreneur litigates if the expected cost is less than the full repayment of the contract. We assume that it is never beneficial to initiate a lawsuit in the event of a successful project, as this would lead to exclusion from credit markets and a reduction in the continuation value of the project. If the project fails, the entrepreneur may initiate a lawsuit if the expected reduction in the value of his debt and the cost of initiating a lawsuit is greater than the current debt.

Suppose there is a continuum of equal firms except for the level of assets A and the perception of local court pro-debtor bias. The latter is defined by the probability of winning a trial in the case of starting a lawsuit against a bank. The perception of firms (π^F) is derived from a cumulative distribution $G(.)$, and although π^F is private information, the bank knows $G(.)$. Firms share the same technology and need to run a project that returns RI with probability p and zero with $(1-p)$. Therefore, firms must raise $(I-A)$, but cannot commit themselves to not filing a lawsuit to claim to pay a fraction ϕ of the repayment r agreed with the bank. Starting a lawsuit in case of success is never valuable because it limits access to credit in the future.

Since firms cannot commit not to litigate, given the loan price r, the level of assets A, and the firm's perception of local court pro-debtor bias π^F, litigation occurs if the expected cost of going to trial is lower than to comply with the contract. Assuming a

fixed cost of taking legal action equal to c, the entrepreneur starts a lawsuit against the bank if:

$$\pi^F(\phi r(I-A)) + (1-\pi^F)(r(I-A)) + c \leq r(I-A)$$

In this case, given A and r, we can set $\bar{\pi}(r,A) = \frac{c}{r(1-\phi)(I-A)}$ in which all firms with the expectation of winning a lawsuit against the bank $\pi^F > \bar{\pi}(r,A)$ have an incentive to litigate if the project fails. We can observe that higher r and lower A increase the incentive to litigate for low values of π^F.

On the bank side, it lends $(I-A)$ at a price r and elastically raises funds at a cost of γ. Each period, the bank learns the pro-debtor bias from local courts by previous decisions. The bank's pro-debtor bias perception π^B captures the expected probability of losing a trial if a client starts a lawsuit. For a certain level of r, banks define the minimum level of capital requirement to lend $(I-A)$. This is defined by the minimum value of A that the bank expects to make zero profit with the operation, which depends on the distribution G(π) in society and the bank's perception of the probability of winning a lawsuit.

$$r(I-A) - (1-p)[1 - G(\bar{\pi}^F(r,A))]\pi^B(1-\phi)r(I-A) = \gamma(I-A)$$

The first term is bank revenue $r(I-A)$, and the second term is the expected revenue loss defined by the interaction of the probability of project failure $(1-p)$; the chance that the firm will initiate a lawsuit $[1 - G(\bar{\pi}^F(r,A))]$; and the subjective probability of the bank of losing a lawsuit π^B. If all these events occur, the bank receives the fraction ϕ of the agreed-upon value, representing a discount of $(1-\phi)$ in bank revenue. The bank breaks even if the revenue and expected loss equal the cost of raising funds $\gamma(I-A)$. From the equation, we can implicitly find A associated with $G(\bar{\pi}(r,A))$, which is the minimum level of assets that banks lend $(I-A)$ to break even.

$$G(\bar{\pi}^F(r,A)) = 1 - \frac{(r-\gamma)}{(1-p)\pi^B(1-\phi)r}$$

An increase in π^B implies a higher G(.), resulting in larger $\bar{\pi}$ and A (or, equivalently, lower I-A). Defining π^B as the expected value of a random variable following a beta distribution $Beta(\alpha,\beta)$, we have $\pi^B = \frac{\alpha}{\alpha+\beta}$. In each period, the bank updates π^B with the previous results of its trials. From that, for values of α and β – which define the bank's expected value of its prior about the pro-debtor bias of local courts–, after

the realization of N new trials, we calculate the bank's expected value of the posterior distribution of beliefs as $\pi^{B'} = \frac{\alpha+\sum_N d}{\alpha+\beta+N}$, where d is an indicator variable equal to one if the debtor has his demands granted by court.

The bank's learning process about the judicial pro-debtor inclination connects judicial decisions and credit rationing. If banks observe different signals about the court's inclination, it is expected that they react by restricting more lending if they observe more judicial decisions against them. The greater the banks' perception of the judiciary's willingness to protect their interests, the more banks will lend to less capitalized firms.

One relevant assumption here is that banks raise money elastically to meet the demand for credit. Since we focus on the four largest banks in the country and study the variation within judicial districts, the loss of specific trials is difficult to affect results by liquidity constraints. We also show that the effects are likely driven by small districts and firms, which reduces the possibility that the results are driven by a few specific cases impacting specific firms. The evidence is stronger for small districts because that is where we find more variation in judicial bias between banks. Small businesses are more affected because they do not have assets to commit to repayment in the next period.

4 Empirics

This section describes the identification strategy employed to estimate the causal effect of pro-debtor decisions on banking credit supply and its effects on firms. The conceptual framework discussed in Section 3 suggests that banks reduce lending when they expect less creditor-friendly justice, and this reaction is more likely to affect smaller firms. Here, we show how we explore the random assignment of judges to banks' lawsuits as an exogenous variation affecting the proportion of pro-debtor results the banks observe. We also provide evidence in support of the random assignment of cases across judges within a judicial district. In sequence, we explain how we use the maturity time of corporate credit contracts to assign banks' treatment to connected firms. This allows us to assess the transmission of adverse decisions to borrowers at the time of credit renewal and its impacts on employment and wages.

4.1 Identification strategy

We depart from the idea that the proportion of pro-debtor decisions is informative for the banks about local judicial bias as discussed by the theoretical framework in Section 3. The problem is that we cannot isolate all factors involving conflict resolution (e.g., interest rates, contract parameters, type of clients, lawyers' skills) that can influence the outcome of a lawsuit, which may vary between banks, judicial districts, and the pool of contracts under analysis. To address this, we use judge-fixed effects as an instrumental variable for trial results. Our approach differs from the literature that relies on this type of strategy, as banks observe multiple decisions from multiple judges in a short period. Thus, we aggregate the leave-one-out measure of leniency of judges by the proportion of decisions they issued in cases involving the respective bank, in each quarter, and judicial district. This process is as follows.

$$B_{jbdt} = \frac{\sum_{n=1}^{n_j} PD_n - PD_{jbdt}}{n_j - n_{jbdt}} \tag{1}$$

$$bias_{bdt} = \frac{\sum_{k=1}^{J} B_{kbdt} n_{kbdt}}{\sum_{k=1}^{J} n_{kbdt}} \tag{2}$$

The first equation is the leave-out measure for the judge j in the cases of bank b, district d, and quarter t. The numerator corresponds to the sum of all pro-debtor decisions across all the sentences of the judge under concern (n_j) in the entire sample, except those corresponding to the bank, district, and quarter related to the observation indexed by correspondent subscripts. The denominator includes the total of the judge's decisions, excluding those related to the observation again[13]. The final measure is the proportion of pro-debtor decisions of each judge, excluding the decisions corresponding to the specific judge-bank-district-quarter observations. As we aim to create an instrument for district-bank-quarter pro-debtor decisions, the final measure of bias faced by banks aggregates the leniency B_{jbdt} weighted by the participation of each judge in the total of decisions involving the bank in a district and quarter. Using this measure of bias faced by banks, we run the following two-stage regression at the

[13]We use all court rulings to calculate judges' proportion of debtor decisions, regardless of which bank. The leave-one-out final instrument is appropriately calculated for the observations of the four largest Brazilian banks that we precisely identified in the judicial dataset and are part of the final sample.

bank-municipality-quarter level:

$$\log(\text{pro-debtor}+1)_{b,d(m),t} = \delta_t + \rho_{d(m),y(t)} + \pi \log(\text{bias}+1)_{b,d(m),t} + \mu_{b,d(m),t} \quad (3)$$

$$y_{b,m,t+1} - y_{b,m,t} = \phi_t + \zeta_{d(m),y(t)} + \theta \log(\text{pro-debtor}+1)_{b,d(m),t} + \eta_{b,m,t} \quad (4)$$

The second stage is given by 4. The explanatory variable in this equation is the logarithm of the proportion of pro-debtor decisions that a bank observes in the district of its municipality ($d(m)$) in a quarter t. The dependent variable is the change in the outcomes of interest (e.g., interest rates) between the quarter in which the judicial decisions are observed and the following one. The first stage, represented by Equation 4, is the regression of the logarithm of the proportion of pro-debtor decisions against the logarithm of *bias* defined in 2. Here, $\log(\text{bias}+1)_{b,d(m),t}$ serves as the instrumental variable used for the two-stage least squares (2SLS) estimate of the effect of treatment $\log(\text{pro-debtor}+1)_{b,d(m),t}$ on changes between quarters of the dependent variable.

In both equations, the first term of the right side denotes quarter fixed effects that absorb common shocks for all observations in a specific period, such as court recesses, law changes, economic shocks, or seasonality in credit demand. Since judges are randomly assigned to lawsuits at the district level, we include the interaction of district $d(m)$ and year $y(t)$ fixed effects in the specification. The second term on the right side of each equation represents that, and the interaction with year fixed effects allows for changes in local court structures, such as the number of judges by district, the creation of local specialized courts, changes in the composition of judges, or other local time-varying shocks. Therefore, the relevant variation that identifies θ is the variation of the dependent variable for the four banks operating in each municipality, between quarters, within the same judicial district and year, that observed different judicial decisions due to having been randomly assigned to judges with different levels of pro-debtor biases. We cluster errors at the district level to account for the correlation in the error term across banks and, over time, operating in municipalities within the same judicial district.

As a first evaluation of the first-stage behavior, Figure 2 shows the unconditional distribution of the instrumental variable and the average value of the associated treatment for each histogram bin. The distribution reveals a wide variation of observed bias across banks, districts, and periods. Moreover, it confirms a positive correlation between the instrumental variable and trial outcomes, indicating that the share of pro-debtor rulings reflects the judges' bias toward the banks. The first-stage esti-

mates are presented in the paper together with the other results.

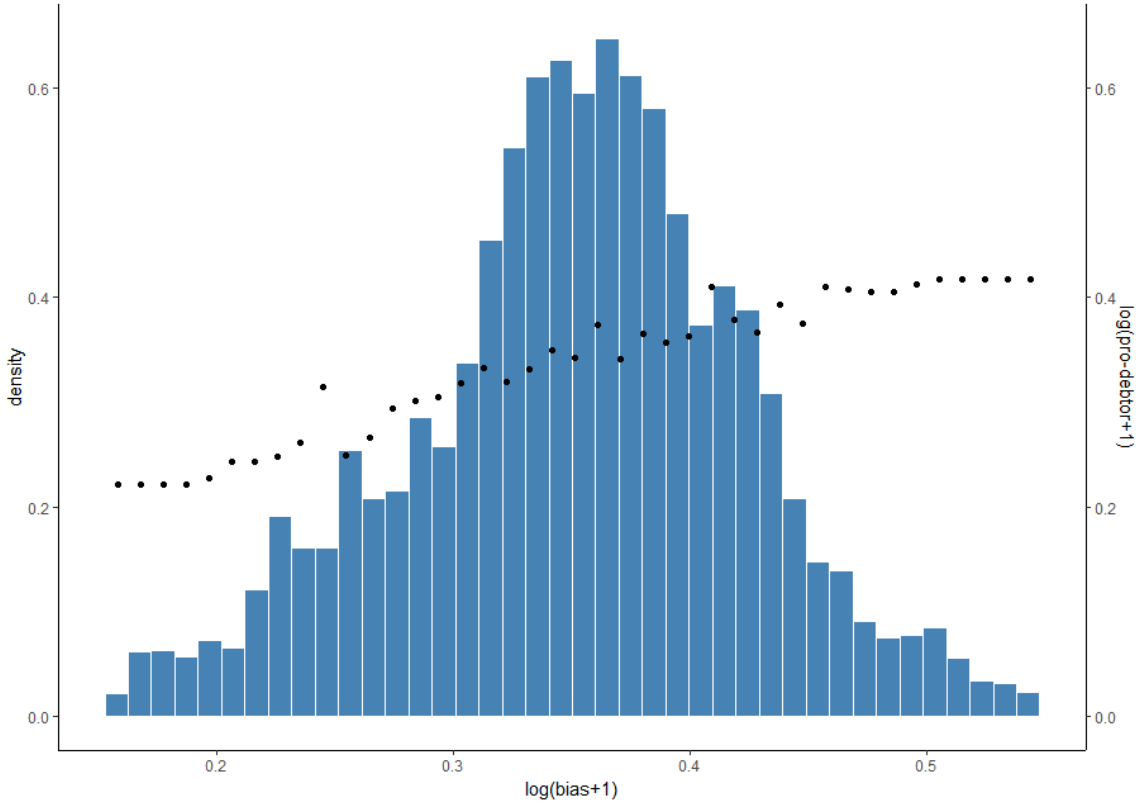

Figure 2. Bias distribution and correlation with pro-debtor decisions.

Notes: The figure plots the histogram of log(bias+1) and the unconditional correlation between log(bias+1) and log(pro-debtor+1) variables represented by the dots. Extreme values for log(bias+1) corresponding to 1% of the observations are excluded.

4.2 Randomization test

The identification strategy relies on judges being randomly assigned to cases independently of other factors such as banks' characteristics or other idiosyncratic shocks. Table 3 reports the results when regressing the treatment and the instrument values against banks' fixed effects and time-varying variables. In both cases, we include the interaction of district-year fixed effects since the random assignment of judges occurs at the district level, and the year interaction is flexible to allow for changes in judges, type of contracts, and lawsuit composition. While column (1) shows that bank-specific variables are predictive of pro-debtor decisions, it is not the case for the judicial bias that each bank faces.

Table 3. Randomization test

	log(*pro debtor*) (1)	log(*bias*) (2)
log(new loans)	0.002	0.000
	(0.001)	(0.000)
log(outstanding credit)	-1.003***	0.000
	(0.001)	(0.000)
interest rates	-0.009***	0.000
	(0.003)	(0.001)
bank2	0.110***	0.000
	(0.007)	(0.001)
bank3	0.042***	0.000
	(0.006)	(0.001)
bank4	0.070***	0.000
	(0.005)	(0.001)
F-statistics	[376,620]	[0.216]
R^2	0.99267	0.71555
District-year FE	✓	✓
N	32,892	32,892

Notes: Columns (1) and (2) show the result of regressions using the treatment and the instrument in the two-stage model defined by equations 3 and 4 as dependent variables, respectively. The regressors are, in order, at the bank-municipality-quarter level: the total value of new credit contracts; the total of outstanding credit; the mean of interest rates of new credit contracts; and bank fixed effects. Both regressions include the interaction of district-year. The F statistics correspond to the joint hypothesis test, excluding the district-year fixed effects. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Overall, the estimates are consistent with the random assignment of judges to lawsuits, which relieves concerns about manipulation. However, the instrument's independence does not guarantee the non-violation of the exclusion restriction, which is an unstable hypothesis. In the paper, we assume that judges indirectly reveal their types by the results of trials in which they were allocated. If the bias faced by banks impacts credit in other ways than just by observing pro-debtor decisions, the estimates might be biased. If, independently of the results, banks can infer their types through the decision content, personal contacts, intermediate procedures, or any other interaction, this would violate the exclusion restriction. Some features of the research design may reduce concern about that. First, if we think that pro-debtor judges affect credit, then banks' reaction would be anticipated when they learn about the judge, which would reduce the effect captured by the reduced form and, consequently, the magnitude of the 2SLS coefficient. However, if banks learn about judges to the point that they can change the sentence, it would overestimate the 2SLS coefficient. We mitigate this concern using only the first sentence issued by the judge in each

case. It is before the moment banks can appeal the sentence for the same judge in an effort to change the decision. Still connected to this last point, since we measure the judges' bias by the proportion of pro-debtor decisions issued in other lawsuits, if banks systematically can change the final sentence by the prior knowledge of judge types, the instrument will capture it, and the first stage will be correctly capturing the expected effect.

4.3 Firm level

After exploring how judicial bias affects the amount of credit banks provide, we examine how this impact spreads to firms within their relationships. The challenge is that the selection of firms that accept new contracts is not exogenous. A firm that faces bad conditions on a contract offered by a bank due to a negative judicial shock may prefer to wait or search for other institutions. To address this issue, we focus on short-term contracts (less than 12 months) to determine if firms with debt maturing in a particular quarter renew contracts with the same bank under different conditions depending on the judicial decisions that each financial institution observes.

In this phase of the analysis, we narrow our focus to firms with short-term contracts maturing in the quarter immediately following the bank's observation of judicial decisions. The rationale behind this is that short-term contracts are typically used for emergency financing to address liquidity issues. Any delays or challenges in renewing these contracts could significantly impact the companies' performance, potentially leading to job losses or wage cuts. This urgency limits the firms' capability to delay the decision to accept bank conditions or seek alternative lenders. Hence, it becomes more plausible to identify the causal effect of the transmission of banks' reactions after observing their trial results to firms with established relationships with them in this context.

Similarly to the previous specification, we run a two-stage regression at the firm-bank-quarter level. In this case, the treatment is assigned as the proportion of pro-debtor decisions observed by the connected bank in the quarter before the firm's contract maturity. For a bank that observes a set of judicial decisions in t, it is included in the sample firms with credit contracts maturing in $t+1$ with this bank. The analysis then compares the changes in the firm's new contracts with the same bank between these two quarters. The regression is defined as follows:

$$\log(\text{pro-debtor}+1)_{b(f),d(f),t} = \gamma_{b(f)} + \alpha_t + \beta_{d(f),y(t)} + \lambda \log(\text{bias}+1)_{b(f),d(f),t} + \nu_{b,d,t} \quad (5)$$

$$y_{f,b,t+1} - y_{f,b,t} = \omega_{b(f)} + \kappa_t + \sigma_{d(f),y(t)} + \chi \log(\text{pro-debtor}+1)_{b(f),d(f),t} + \varepsilon_{f,b,t} \quad (6)$$

The system of equations above resembles the bank-municipality level equations, but centers on firms f that are associated with banks b, operating in the same judicial district d as them. We use the subscripts $b(f)$,$d(f)$ to denote the bank and district related to the firms. Thus, $\log(\text{pro-debtor}+1)_{b(f),d(f),t}$ in equation 5 is the logarithm of the proportion of pro-debtor decisions that a bank in the same district as the firm f, with contracts that mature in the quarter $t+1$, observes. The rest of the notation is analogous to previous explanations with the addition of bank fixed effects represented by the first term on the right-hand side of both equations.

Unlike the previous case, in which banks cannot control when the decisions are released, in this setting, contracts maturing in a specific quarter reflect the banks' reaction to previous shocks. We include fixed effects of banks in our econometric model to account for possible differences in their lending behavior and exposure to judicial decisions. This controls for any bank-specific characteristic that may influence the treatment assignment at the firm level. For example, a bank may respond more strongly to a negative shock from judicial decisions by cutting their short-term credit supply to firms. This would mean that fewer firms linked to these banks would have credit maturing in the future, generating a correlation between the bank and more lenient judges. By including bank fixed effects, we can separate the causal effect of judicial bias on the firms' outcomes from the confounding effect of banks in selection. In the robustness test section, we run a regression using the interaction of bank with district-year fixed effects, allowing the variation in pro-debtor decisions between firms in the same district, connected to the same bank within a year. The results do not change.

4.4 Real effects

Lastly, we assess whether a negative shock to a bank affects the wages and employment of firms linked to them. Here, another difficulty arises because firms have multiple contracts maturing throughout the year, and banks constantly observe court rulings on their lawsuits. However, we have available data on labor market outcomes reported for December each year. To capture which is the relevant judicial shock transmitted to a specific firm through its creditor bank, we propose to select the bank and quarter in which firms have their highest amount of credit maturing within a year and assign the respective bank's (quarterly) treatment value to them. For that, we select the

bank and quarter in which each firm has the largest amount of credit expiring within a year and then assign the pro-debtor and bias values this bank observed in its lawsuits in the immediately preceding quarter. Thus, we have one yearly observation of judicial shock for firms that represents the bank-quarter shock when the most representative contracts are just expiring for a firm. The idea is that changes in creditor behavior when firms are most exposed to it can capture the transmission of the judicial behaviour to firms through adverse credit conditions, which might impact borrowers' performance.

In this case, it is not difficult to imagine that a shock measured in the first or fourth quarter has different effects when we evaluate changes in wages and employment between years. To account for this, we include district-quarter fixed effects instead of the district-year of the previous specifications. Hence, we limit the comparison of firms selected in the same quarter but connected to different banks. The regressions are similar to the previous ones, just varying the discussed aspects, such as the inclusion of fixed effects interacting district and quarter, and we now have yearly observations.

$$\log(\text{pro-debtor}+1)_{b(f),d(f),y(t)} = \gamma'_{b(f)} + \beta'_{d(f),t} + \lambda' \log(\text{bias}+1)_{b(f),d(f),y(t)} + \varsigma_{b,d,y} \quad (7)$$

$$y_{f,y(t)} - y_{f,y(t)-1} = \omega'_{b(f)} + \sigma'_{d(f),t} + \chi' \log(\text{pro-debtor}+1)_{b(f),d(f),y(t)} + \epsilon_{f,y} \quad (8)$$

The prime symbol distinguishes the parameters from those of the earlier equations. The setup is akin to the previous section, but in this instance, we have a single annual observation, indexed by $y(t)$, per firm. This observation corresponds to the one associated with its most substantial credit due in the upcoming quarter. For example, in the equations at the firm level 5 and 6, firms can appear multiple times if they have credit maturing in a quarter with any bank. From these observations, we select the one tied to the highest value of credit that a firm has maturing within the year. The dependent variable is the difference in the log of the firms' wage bill and employment from December of the preceding year of the observation to December of the same year. The remaining variables are consistent with the previous descriptions.

The caveat we must raise here is that this strategy may suffer from selection bias. As we define the treatment time as the quarter in which firms have the most significant amount of credit expiring, this might be correlated with adverse shocks to their banks and firms' quality. For example, some firms may not be able to get new credit in the subsequent semester after their bank's adverse shock. Another group connected to another bank may experience a positive shock, and some of them renew

contracts, while some without the need to take new credit in the semester do not. So, the pro-debtor bias values will be correlated with firms' types if we select in the same semester bad firms that are not able to renew contracts due to adverse shocks, as in the first case, and good companies that do not independently of their banks' shocks, as in the second case.

Unfortunately, our setting does not provide a straightforward solution for that. However, we present some evidence to confirm the validity of the results. Table B1 in Appendix B shows the randomization test, similar to the one presented at the bank level in table 3, to our preferred subsample and specification. This includes district-quarter and bank fixed effects, excludes observations in the 4th quarter of the year, and excludes large businesses. We excluded firms with the most relevant shock in the 4th quarter because the labor market outcomes are measured in the same period (December), so we do not expect to find such a fast adjustment in employment. The restriction to SMEs is supported by the theoretical framework, which predicts more significant effects in this subgroup of companies.

The test supports the idea that the instrument is not correlated with firm-specific variables such as the number of bank relationships, the initial number of employees, and total wages, nor the amount of credit expiring in the quarter and bank selected to assign firms' treatment. Further, in the robustness section, we discuss alternative treatment assignments, such as using the first maturity date of contracts in the year or selecting the most relevant shock using only contracts expiring in the first semester. Both the test and the alternative approaches mitigate the concern about the influence of selection bias in the estimates.

5 The effect of pro-debtor decisions on lending and the transmission through banking relationships

5.1 Bank-level evidence of credit rationing

We begin the analysis by examining the impact of pro-debtor decisions on banks' credit supply. We employ two distinct datasets, SCR and ESTBAN, to assess the effects on credit. The first dataset is detailed and provides extensive information, while the second is publicly available and allows for replication with similar qualitative results. When using the ESTBAN data, the dependent variable is the difference between the logarithm of the credit stock recorded on the bank branches' balance sheets at the bank-district level from one quarter to the next. With the SCR data,

we can observe the new loans issued by banks in each municipality. To measure changes in credit, we calculate the difference between quarters in the logarithm of the total of new loans issued at the bank-municipality level. Finally, we use the difference in the interest rates of new loans, weighted by each contract's participation in the total value of new contracts in the period, to understand the impact on prices.

Table 4 consolidates the primary results of the paper. Column (1) presents the estimates for the ESTBAN data at the bank-district level, while columns (2) and (3) utilize SCR data at the bank-municipality level. Panel A indicates that it is not possible to identify any statistically significant estimate for the simple correlation between pro-debtor decisions and credit or interest rates. According to the conceptual framework, if the outcomes of previous lawsuits are informative to banks, they should respond by reducing the amount of lending. The problem is that many of the pro-debtor variations are possibly driven by multiple unobservable variables, such as the characteristics of the credit contracts that led to the legal dispute, the quality of the lawyers, or even the socioeconomic status of the plaintiff. Consequently, the proportion of trials lost by a bank in a district is not informative about the creditor-friendliness of the local courts if we cannot control for all characteristics involving the lawsuit.

To address this issue, Panel B displays the results for the 2SLS estimator using the judicial bias faced by banks, defined by equation 2, as instrument. The second line of column (2) reveals that a 10% increase in pro-debtor decisions faced by a bank due to less creditor-friendly judges (more pro-debtor biased) is associated with a 23 percentage point larger proportional decrease (or smaller increase) in the total values of banks' new loans in the next quarter relative to the quarter of observation. Column (1) shows the same qualitative result, indicating credit rationing when banks observe worse pro-creditor conditions, reflected by the outcome of their trials. Based on the ESTBAN dataset, when a bank observed a 10% increase in pro-debtor decisions due to having their lawsuits decided by more pro-debtor-biased judges, the bank's stock of credit growth rate between quarters decreased by 1.23 percentage points in relative terms. Continuing with Table 4, in Panel B, Column (3), it is demonstrated that an increase of 10% in pro-debtor decisions corresponds to a rise of 7.5 percentage points in interest rates, on average, for contracts in the subsequent quarter[14].

[14]While it is not included in the results, there is no statistically significant effect on credit or interest rates for personal loans. A crucial institutional aspect is that payroll lending is the primary credit instrument for individuals in Brazil (Coelho et al., 2012). This instrument has the lowest interest rates charged by institutions, with debt payments automatically deducted from individual

Table 4. Bank level effect of pro-debtor decisions on credit

	$\Delta \log(credit)$	$\Delta \log(new\ loans)$	$\Delta interest\ rate$
	(1) ESTBAN	(2) SCR	(3) SCR
Panel A: OLS			
log(*pro debtor*)	-0.001	-0.010	0.011
	(0.004)	(0.041)	(0.015)
Panel B: 2SLS			
log(*pro debtor*)	-0.123**	-2.300***	0.755**
	(0.062)	(0.850)	(0.374)
Panel C: Reduced Form			
log(*bias*)	-0.040**	-0.569***	0.186**
	(0.019)	(0.179)	(0.081)
Panel D: Firts Stage			
		log(*pro debtor*)	
log(*bias*)	0.332***	0.247***	0.246***
	(0.062)	(0.059)	(0.059)
F-statistics	[28.2]	[17.5]	[17.3]
District-year FE	✓	✓	✓
Quarter FE	✓	✓	✓
N	13,793	32,892	32,400

Notes: Column (1) presents results using ESTBAN credit data at the bank-judicial district level restricted to districts where the four banks have branches operating throughout the analysis period. Columns (2) and (3) use SCR credit data at the bank-municipality level restricted to municipalities with new originations from the four banks in all years of the sample. For panels A, B, and C: column (1) uses as dependent variable the mean value per quarter of the credit stock reported on the balance sheets of bank branches ; columns (2) and (3) use the sum of new loans per quarter as the dependent variable. Panel D presents the result of the first stage for ESTBAN data in column (1) and for SCR data in columns (2) and (3). Standard errors are clustered at the judicial district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Finally, panels C and D round out the table, demonstrating that the proposed instrument not only impacts the treatment but also directly impacts the outcomes. The first stage F statistics provides evidence against weak instrument, but the validity of the exclusion restriction hypothesis is untestable. The experimental design and the results from Table 3 in Section 4 suggest that the instrument is not correlated with unobservable banks' characteristics or other time-varying shocks that locally affect banks. However, some concerns might invalidate the idea that the instrument only affects lending through observed decisions. For example, if we imagine banks learn the judicial bias before trial outcomes –maybe by intermediate procedures– it potentially

paychecks. Furthermore, a minor positive point estimate found might imply that firms with restricted access to credit could use personal credit as a workaround to corporate credit restrictions (Herkenhoff et al., 2021; Corradin and Popov, 2015).

affects banks' behavior regardless of the observed pro-debtor decisions. In this case, the anticipated reaction would decrease the reduced form and the 2SLS estimates. On the other hand, if banks learn to the point that they can change the lawsuit outcomes, the 2SLS estimates will be biased upwards because of a smaller first stage. This concern is mitigated by using only the first sentence before the moment banks can appeal the decision to the same judge.

Furthermore, we can also imagine that if banks learn about judicial bias through their trial outcomes, they can also observe other banks' sentences and capture the local judicial bias from them. In spite of it being not easy to imagine that banks might have the necessary information on competitors' lawsuits specifics to infer the judge's leniency, in this case we should not expect the variation in judges to generate any result through the learning process. Considering this to be correct, the observed effect could be just a shock on banks' results, limiting their capacity to originate new loans. Regarding that hypothesis, the next sections show that, at least for short-term debt to firms with previously established bank relationship, the credit rationing is concentraded in small firms, in line with the theoretical framework and against the idea that liquidity issues might be driving the results.

Therefore, the credit results presented suggest that banks reduce lending when faced with more lenient judges in their lawsuits. Additionally, the average interest rate of new contracts increases. However, we cannot definitively determine whether this affects firms that could have obtained loans under better circumstances if different judicial decisions had been made. When faced with worse credit conditions, firms may decide not to take credit, switch to another bank, wait, or even accept the new proposal. Compare firms that take credit after a judicial shock are subject to selection, and the following section addresses this concern to understand how judicial decisions can impact firms that are not part of the litigation.

5.2 Transmission to connected firms

Table 5 presents the 2SLS estimates for the transmission of bank shocks to connected firms described by equations 5 and 6 for different subsamples of firms[15]. According to the conceptual framework in Section 3, the most affected firms by credit rationing are those with low assets. Here, we approximate for empirical purposes as small businesses that represent 80% of the sample. We also observe results when restricted to firms with one-bank relationship that limits comparison between more similar firms.

[15]Table B2 in Appendix B presents the first stage, reduced form, and OLS estimates.

In the table, column (1) shows the result for the entire sample; column (2) restricts observations for firms with one bank relationship; column (3) includes only small businesses; and column (4) restricts observation of small firms with one bank relationship. We assign $pro-debtor$ and $bias$ values to firms based on banks' observations whose firms have credit expiring in the next quarter. To illustrate, if a firm's credit with a bank is set to mature in quarter $t+1$, we use $pro-debtor$ and $bias$ values observed by the related bank in quarter t. A firm is included in the observations always that it has a contract maturing in the subsequent quarter that a bank observes judicial decisions. Our dependent variable is the difference in logarithms of loan values negotiated by the firm and the bank, comparing the quarter preceding the maturity of the contracts (t) with the quarter of maturity $(t+1)$. The aim is to capture the effect of banks' informational shocks just before a firm renews short-term credit.

Table 5. Firm level effect of pro-debtor decisions on credit

	Δ log(new loans)			
	(1) All	(2) SME	(3) 1 bank	(4) SME, 1 bank
log(*pro debtor*)	-0.295	-0.550	-1.174**	-1.387***
	(0.392)	(0.452)	(0.505)	(0.496)
District-year FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
N	1,099,610	866,015	670,218	598,439

Notes: Coefficients correspond to the 2SLS estimates for the model defined by eq. 5 and 6. Column (1) includes all observations; column (2) only small firms ; column (3) includes only firms with one bank relationship; column (4) includes small firms with one bank relationship. Small firms are defined as those with less than 4,8 million BRL (1 million USD) in revenue per year. A firm is considered to have a bank relationship if it holds a contract that matures on any date during the year with a bank. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

The number 1.39 in the fourth column indicates that companies with debt maturing in the following quarter experience a proportional reduction of 13.9 percentage points in the value of new credit contracts if their banks observed a 10% increase in pro-debtor outcomes from lawsuits adjudicated by pro-debtor biased judges. For this same subsample of small firms, Table 6 below demonstrates that this outcome is primarily due to a decreased probability of firms obtaining new credit if their banks observe more negative decisions induced by pro-debtor judges.

Table 6. Credit rationing channel

	$y = 1_{(new\ loan)}$		$\Delta \log(new\ loan)$	$\Delta interest\ rates$
	(1)	(2)	(3)	(4)
log(*pro debor*)	-0.097**	-0.104**	-0.202	-0.018
	(0.045)	(0.047)	(0.170)	(0.100)
District-Year FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
N	598,439	574,861	503,682	503,682

Notes: Results for a subsample of small firms with one bank relationship. Small firms are defined as those with less than 4,8 million BRL in revenue per year. A firm is considered to have a bank relationship if it holds a contract with a bank that matures on any date during the year. Coefficients correspond to the 2SLS estimates for the model defined by eq. 5 and 6. Column (1): Probability of taking a new loan in t+1; column (2): Probability of taking a new loan in t+1, conditional on observing a new contract in t; column (3): Proportional increase in new contracts values between t+1 and t conditional on observing new contracts in t+1 and t; (4): interest rates variation conditional on observing new contracts in t+1 and t. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6 shows the 2SLS estimates related to the system of equations 5 and 6 for different dependent variables for the subsample of small firms with one bank relationship. Column (1) uses an indicator variable that takes the value of 1 when a new contract is observed between the firm and the same bank in $t + 1$. Column (2) is the same as column (1), but it conditions on firms that contracted credit with the same bank in t. Column (3) shows the result when the dependent variable is the change in the logarithm of the value of new contracts, conditional on observing a contract in t and $t + 1$. Column (4) substitutes the dependent variable from column (3) with changes in interest rates of contracts agreed upon in each quarter. When restricting attention to the coefficients in the first two columns, both values indicate that firms are approximately 1% less likely to take a new contract when they have a credit contract that matures in a quarter after their banks observed 10% more pro-debtor decisions induced by pro-debtor judges. On the other hand, the third and fourth columns show that, for firms that take a new contract, there is no impact on value or interest rate.

The findings on credit renewal indicate that certain small businesses may be excluded from the market following a negative informational shock faced by their banks. Ideally, we should observe whether companies that did not contract new credit experienced worse conditions or whether the bank denied them. Unfortunately, this information is not observable, but the absence of any indication that contract terms

changed for companies obtaining new loans suggests the presence of credit rationing.

If adverse bank shocks are indeed transmitted to companies during credit renewal attempts, we can speculate that these same companies will exhibit poorer economic performance. The following section presents results related to the real effects of pro-debtor judges' decisions.

5.3 Real Effects

This section analyzes how the transmission of judicial shocks to banks affects the labor market outcomes of the firms connected to them. Our analysis focuses on the impact of pro-debtor decisions on both wages and total employment. We use quarterly data on judicial decisions and annual data on labor market outcomes, with the latter measured in December. To assign the treatment, we first identify the firm-bank-quarter combination of firms' highest value of debt maturing within a year. Then, we assign the pro-debtor decisions and the judicial bias faced by the corresponding bank in the prior quarter to this firm. The dependent variables are the differences in employment and wage bill logarithms between the year of observation and the previous. This captures the percentage change in outcomes from the beginning to the end of the year since they are observed in December. Table 7 presents the results for the real effects of pro-debtor biased decisions.

Table 7 shows the coefficients of the 2SLS estimates of equations 7 and 8 of the effect of pro-debtor decisions on firms' wages and employment. The pro-debtor variable corresponds to the value observed by the bank in the immediate previous quarter in which the firm has its highest values of credit contracts to expire within the year. Panel A shows the 2SLS estimates for logarithmic changes in firms' total wage bills between December of the previous year and December of the year of observation as the dependent variable. Panel B shows the same estimates when using log changes of firms' total employment. For both panels, column (1) includes all firms that were able to observe labor market outcomes in the year of observation and the previous. Column (2) excludes observations that the treatments were assigned in the year's fourth quarter. In this case, since labor market outcomes are measured in December, we do not expect such a fast adjustment to observe the effects of the treatment on employment at the end of the year. Column (3) excludes large firms, and column (4) includes control variables. The coefficient value of -0.465 in the last column of panel A indicates that a firm facing a 10% higher proportion of pro-debtor decisions – driven by more pro-debtor judges – in the quarter just before their largest credit

Table 7. Real Effects

	All	1st to 3rd quarter	1st to 3rd quarter SME	1st to 3rd quarter SME
	(1)	(2)	(3)	(4)
Panel A: 2SLS $\Delta \log(wage\ bill)$				
log(pro-debtor)	-0.160	-0.360**	-0.426**	-0.465***
	(0.113)	(0.153)	(0.169)	(0.177)
Panel A: 2SLS $\Delta \log(employment)$				
log(pro-debtor)	-0.152	-0.345**	-0.394**	-0.434**
	(0.108)	(0.147)	(0.164)	(0.174)
District-quarter FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Aditional controls				✓
N	404,345	286,581	238,486	238,486

Notes: Coefficients correspond to the 2SLS estimates for the regression DENIFINIR. The treatment and instrument assigned to the firms correspond to the values observed in the previous quarter of the bank in which the firm had the highest value of maturing credits in the first semester. Columns (1) and (3) include the whole sample of firms, and columns (2) and (4) limit the sample for firms with a single bank relationship. A firm is considered to have a bank relationship if it holds a contract with a bank that matures on any date during the year. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

contracts mature with a bank experiences a proportional decrease of 4.65 percentage points in its wage bill. The same pattern of results is observed for total employment.

Looking at all columns, we observe statistically significant results when we exclude companies with treatment values assigned relative to credit contracts expiring in the fourth quarter. This makes sense if we consider that employment adjustments do not occur immediately after adverse credit shocks. Companies may try other external financing options before cutting wages and jobs, and the institutional context here is also crucial to understand this pattern. Employee dismissals in Brazil are expensive and usually a last resort in short-term financial decisions. Also, a company must give at least one month's notice before effectively terminating an employee. In this context, the results are by what we should expect.

Regarding columns (3) and (4), larger effects are expected for a subsample of SMEs, as discussed in the previous section and the theoretical framework. The additional controls in the last columns include potential drivers of selection such as two digits of CNAE (National Register of Economic Activities) interacting to quarter fixed effects, the value of credit contracts maturing with the bank and quarter selected to assign the treatment, and the number of firms' bank relationships. Despite

larger coefficient estimates, the difference is insignificant to threaten the identification hypothesis. Analyzing these results together with the findings in credit rationing at bank and firm levels, we can observe at least one path from the deterioration of the perception of creditor rights protection, achieving firms' capacity to obtain external financing, and finally resulting in worse economic performance.

6 Robustness Tests

This section details the robustness tests conducted. Initially, in Table C1, panel A in Appendix C, we replicate the primary findings at the bank level using one and two periods of lag in the dependent variable. In both instances, we do not observe statistically significant values for new loans as dependent variable. Next, panel B reproduces the results after excluding potential non-conforming observations. The first column excludes all observations from years where only one judge was active in the district, removing cases with no variation in the judicial bias observed by banks due to the inclusion of district-year fixed effects. In column (2), in addition to the previous restriction, we eliminate 1% of the observations with the most extreme values of bias faced by banks. The aim here is to reduce the noise caused by outliers resulting from judges with few observations in the sample that eventually ruled on all cases observed by a bank in a specific quarter and district. Despite the reduction in magnitude, the result remains qualitatively equal and statistically significant in both subsamples. Finally, panel C of the same table excludes from the sample in the first and second columns one and two quintiles of districts with the highest number of judicial decisions issued by quarter. The increase in estimated values confirms that the results are driven by the smallest districts in which the variation in the bias observed by the banks is greater. For large districts, since all banks observe many decisions in a short period, the bias faced by banks tends to be the average bias of the district, resulting in less variation in the instrumental variable between banks and quarters.

At the Okfirm level, Table C2 displays the results for various specifications and subsamples based on the findings in Table 5. Column (1) includes the interaction of quarter fixed effects with the first two digits of firms' CNAE (national classification of economic activities). Column (2) retains the CNAE-quarter fixed effects and introduces the interaction of bank in the district-year fixed effects, limiting the estimates to capture the variation in firms' credit within the same bank, district, and year.

The coefficient values remain consistent with the initial specification in both cases. Columns (3), (4), and (5) maintain the original specification and conduct tests for subsamples similar to those at the bank level. Initially, observations of district-year with the same judge issuing all decisions are excluded, followed by the exclusion of the fifth quintile of districts with the highest number of decisions per quarter. Finally, column (5) removes district-year observations with only one judge and 1% of extreme values of pro-debtor bias observations. The only notable change in coefficient estimates is observed in the fourth column when the sample excludes the largest districts, aligning with the bank-level results and justified by the fact that large districts present lower variation in bias observed by banks.

To validate the conclusions drawn from Table 6, we run the same regression at the firm level, but we use the difference between the firms' new loans and the value of the loans maturing in the same quarter. This addresses concerns about the imputation of zeros or by exclusion of firms that did not borrow in the period before the maturity of the observed credit contracts. The results for the estimates using the change in the volume of credit of new loans and expiring ones are given in Table C3. Column (1) uses the whole sample of small firms with a single bank relationship, and the coefficient magnitude is basically the same as those in Section 5. Column (2) includes only firms that took new credit in $t+1$, and we do not find evidence that the estimate is statistically different from zero. Again, this reaffirms the previous results, as they are indifferent if we compare the proportional raise in credit using the value of new credit in $t+1$ with loans taken in t or with the value of expiring credits in $t+1$. Using the entire sample, we observe a decrease in the value of new loans in both cases and no effect if we restrict the sample only to firms that actually borrowed in $t+1$. The conclusions of Table 6 remain robust to this change in the dependent variable, demonstrating that the results at the firm level are primarily driven by the reduced likelihood that small firms renew credit contracts after their lender encounters adverse judicial decisions in the quarter just before their credit expires.

Finally, Tables C4 and C5 display the results of different treatment assignments for the real effects analysis. The main estimates are based on the bank and quarter in which companies have their most substantial credit due within the year, as outlined in the preceding section. Alternatively, Table C4 presents the estimates for the same specification when we assign the treatment to companies based solely on the first credit contract expiring in the year. In this scenario, we limit the sample to contracts in the first semester, providing a sufficient time gap between treatment and dependent

variable measurement (December) to observe adjustments in labor conditions. If more than one bank-firm relationship is selected, we use the bank with the most credit to mature in the quarter. Then, as previously, we index the selected quarter-bank observation and define the treatment as the log of pro-debtor decisions observed by the bank in the immediately preceding quarter. Table C5 shows results when we limit the selection of the largest maturing credit in the first semester of the year. In this case, the selection is limited to two periods with a reasonable time gap to observe adjustment in labor outcomes. In the latter case, we also allow for comparisons within the semester instead of quarters. In all instances, despite sample restrictions and specification changes, the outcomes are qualitatively identical and very similar in terms of coefficient magnitude. These results alleviate worries about selection bias influencing the findings since different rules for treatment assignment led to the same conclusion.

7 Conclusion

When judges have room to exert their own preferences to influence litigation outcomes, similar legal claims might have different conclusions. Leveraging the random assignment of lawsuits to judges, this study shows that an increase in trial defeats observed by banks motivated by pro-debtor judges negatively impacts lending activity. Small firms with contracts maturing when their banks are adversely impacted face greater difficulty renewing short-term credit. Given the sticky nature of banking relationships, these businesses subsequently experience a decline in economic performance.

This transmission path from judicial discretion to firms' economic activity underscores the importance of understanding the extent of freedom judges have in influencing conflict resolutions and its consequences. Furthermore, the findings in this paper provide a compelling argument for how the protection of creditor rights shapes credit markets. Our research design addresses common issues such as omitted variables in cross-country studies and concerns of selection or reverse causality when examining changes in local legislation or judiciary organization within the same country. The degree of variation within a single judicial district eliminates regional differences, and the institutional framework is ex ante credible about the exogeneity of judge allocation.

In societies with independent judiciaries, discussions on the extent of judicial dis-

cretion are prevalent. The insights provided in this paper are essential to shed light on the relevance of understanding judicial decision-making. Good laws are ultimately translated into society through court rulings, and even individual decisions of local courts can have far-reaching impacts beyond the parties involved in the lawsuit. Policies that increase judges' awareness of the broader implications of their decisions, coupled with improved laws that minimize the room for overinterpretation, could enhance this situation.

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A Construction of the Judicial Dataset

We constructed the judicial database using case summaries from local court judgments in the state of São Paulo, available on the State Court of Justice's website (https://esaj.tjsp.jus.br/cjpg). The processes followed the steps described below.

Text Extraction First, we use the website search tool to identify 'standard civil procedure' cases, which typically involve creditor-debtor disputes in banking litigation. The search results provided a comprehensive list of results organized by the lawsuit identifier, with the judges' names, district, sentence dates, and sentence text summaries. We collected this information through web scraping, collecting a total of 2,633,136 sentences from 2011 to 2018. However, for consistency, we only used sentences from 2013 onward, as complete data for earlier years was not available.

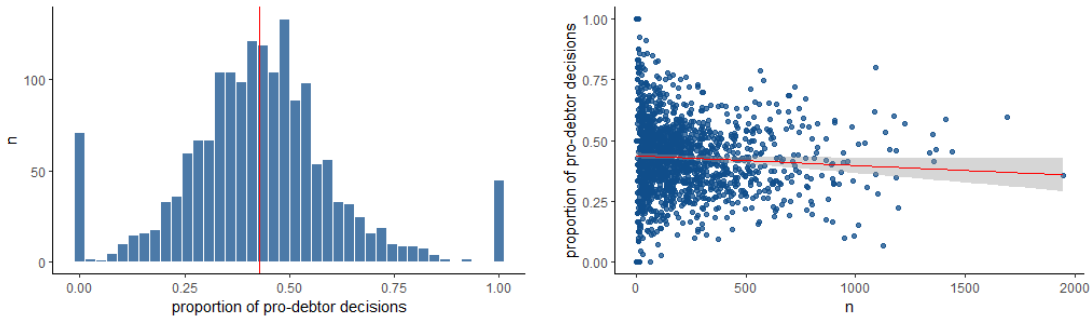
Information extraction from sentence summaries: The next step involved extracting plaintiff and defendant names from the decision summaries using regular expressions, which identified specific word patterns preceding the parties' names. Additionally, we determined the outcome of each case — favorable or not to the claimant — based on standard legal terms found in the summaries. For half of the cases, terms like "julgo procedente" (I rule in favor of), "judge improcedente" (I rule against of) clearly indicated the lawsuit winner. For the remaining, we manually identified variations in language and common writing patterns to classify the decisions. This method enabled us to gather the required information for 91% of the initial sample.

Filtering and classifying cases by financial Institutions: The end of this process involved isolating decisions pertinent to disputes with financial entities. The absence of a standardized identifier for litigants and the considerable variation in the writing of financial institutions — often with typing mistakes — necessitated approximate string-matching techniques. These were applied to the defendants' names against a registry of banks authorized by the central bank to operate. This procedure effectively delineated the cases associated with financial institutions, albeit without identifying the exact bank in each case. In this way, we focus on identifying lawsuits involving the four predominant Brazilian banks — Itaú-Unibanco, Bradesco, Banco do Brasil, and Santander— and their subsidiaries, collectively representing 67% of the sentences in our final dataset.

Selection of sentences: Our analysis was restricted to judicial decisions in which financial institutions were defendants and selected only the initial ruling of each judge involved. This selection reflects cases where clients contest banks against debts, executions, alleged contractual violations, unwarranted fees, etc. Such cases

grant judges a broader autonomy than is typical in default execution orders filled by banks. Furthermore, the intrinsic nature of debt execution in standard credit agreements often leads to debtor-initiated actions. It is also noteworthy that debt execution processes culminate in judicial sentences only upon resolution, a stage not reached in numerous instances, thereby precluding the availability of such data in the TJSP records. Finally, taking into account only the first decision of each judge limits the observation of the outcome before banks' appeals. Also, it captures the right moment when banks learn the judge's bias.

B Other Tables and Figures



(a) Proportion of pro-debtor decisions distribution

(b) Total decisions by judge X proportion of pro-debtor decisions

Appendix Figure B1. Judges' pro-debtor decisions and total sentences

Graphic (a) shows the histogram of the distribution of the proportion of pro-debtor decisions by judge. The red line indicates the mean value. Graphic (b) plots the correlation between total sentences by judge and the proportion of pro-debtor decisions. The red line is the linear tendency line.

Appendix Table B1. Randomization test for the real effects analysis

	log(*pro debtor*) (1)	log(*bias*) (2)
log(wages)	-0.002	0.000
	(0.001)	(0.000)
log(employment)	0.001	0.000
	(0.001)	(0.000)
log(maturing loans)	0.001*	0.000
	(0.000)	(0.000)
log(new loans)	0.000	0.000
	(0.000)	(0.000)
number of bank relationships	0.000	0.000
	(0.000)	
F-statistics	7,851	1.225
p-value	[0.000]	[0.280]
R^2	0.556	0.843
District-quarter FE	✓	✓
Bank FE	✓	✓
N	238,486	238,486

Notes: Columns (1) and (2) show the result of regressions using the treatment and the instrument used for the analysis of real effects discussed in Section 4. The regressors are in order: log of firms' wage bill in December of the previous year; log of firms' total employment in December of the previous year; log of the value of the expiring loans in the quarter of reference to assign banks' treatment to the connected firms; log of new loans in the quarter of the treatment assigned to firms; and the number of bank relationships the firm had in the year. Both regressions include the interaction of district-quarter and bank fixed effects. The F statistics correspond to the joint hypothesis test, excluding the fixed effects. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table B2. Firm level effect of pro-debtor decisions on credit: multiple samples

	(1) All	(2) SME	(3) 1 bank	(4) SME, 1 bank
Panel A: OLS, $\Delta \log(\textit{new loans})$				
$\log(\textit{pro debtor})$	-0.010	0.080*	0.026	0.022
	(0.040)	(0.047)	(0.039)	(0.039)
Panel B: Reduced Form $\Delta \log(\textit{new loans})$				
$\log(\textit{pro debtor})$	-0.125	-0.211	-0.438***	-0.522***
	(0.158)	(0.171)	(0.167)	(0.164)
Panel C: First Stage $\log(\textit{pro debtor})$				
$\log(\textit{pro debtor})$	0.385***	0.384***	0.373***	0.376***
	(0.060)	(0.060)	(0.063)	(0.062)
First-stage F-statistics	[41.2]	[41.0]	[35.1]	[36.8]
District-year FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
N	1,083,697	866,015	670,218	598,439

Notes: Coefficients correspond to the 2SLS estimates for the model defined by eq. 5 and 6. Column (1) includes all observations; column (2) includes only firms with one bank relationship; column (3) includes only small firms ; column (4) includes small firms with one bank relationship. Small firms are defined as those with less than 4,8 million BRL (1 million USD) in revenue per year. A firm is considered to have a bank relationship if it holds a contract that matures on any date during the year with a bank. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

C Robustness tests

Appendix Table C1. Bank level robustness tests

	$\Delta \log(new\ loans)$	
	(1)	(2)
Panel A: Lag in outcomes		
	1 lag	2 lags
log(pro-debor)	1.325	-0.603
	(0.864)	(0.804)
N	32,892	31,547
Panel B: Excluding possible non-conforming observations		
	districts > 1 judges	districts > 1 judges 0.5% - 99.5% *bias* values
log(pro-debtor)	-1.982***	-1.584***
	(0.673)	(0.587)
N	30,708	30,550
Panel C:Excludes larges districts in terms of decisions per quarter		
	1-4 quintiles	1-3 quintiles
log(pro-debtor)	-2.629***	-4.898**
	(0.988)	(2.341)
N	20,230	12,176
District-year FE	✓	✓
Quarter FE	✓	✓

Notes: Coefficients correspond to the 2SLS estimates for the model defined by eq. 3 and 4. Panel A corresponds to the estimates using one and two periods of lag of the dependent variable. Panel B column (1) excludes all observations of the year for districts with only one judge deciding all cases; column (2) excludes 1% of the most extreme values of the instruments, in addition to the exclusions of column (1). Panel B excludes the largest districts in terms of average number of decisions by quarter. Excludes the 5th quintile and the 5th and 4th quintiles as indicated. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table C2. Firm level robustness tests: multiple specifications and samples

	$\Delta \log(new\ loans)$				
	(1)	(2)	(3) +1 judge	(4) +1 judge 1-4 quintiles	(5) +1 judge 0.5%-99.5% *bias* values
log(pro-debor)	-1.390***	-1.422**	-1.347***	-2.028**	-1.409***
	(0.487)	(0.582)	(0.480)	(0.884)	(0.459)
District-year FE	✓		✓	✓	✓
Bank FE	✓		✓	✓	✓
Quarter-CNAE FE	✓	✓			
Year-district-bank FE		✓			
Quarter fixed effects			✓	✓	✓
N	598,439	598,439	591,173	165,161	588,021

Notes: Coefficients correspond to the 2SLS estimates for the model defined by eq. 5 and 6. Column (1) includes the 2-digit CNAE and quarter interaction; column (2) includes year, district, and bank interactions; column (3) excludes district-year observations with one judge issuing all sentences; column (4) additionally to the exclusion in column (2) keeps only observations for the first four quitiles of districts regarding the average number of sentences per quarter; column (5) repeat columns (3) and also excludes 1% of extreme values of *bias*. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table C3. Firm-level robustness tests: alternative dependent variable

	$\log(new\ \ loans) - log(loans\ due)$	
	(1)	(2)
log(pro-debor)	-1.197***	-0.248
	(0.453)	(0.164)
District-year FE	✓	✓
Bank FE	✓	✓
Quarter FE	✓	✓
N	598,439	518,950

Notes: Coefficients correspond to the 2SLS estimates for the model defined by eq. 5 and 6. The dependent variable is the difference in the logarithm of firms' new loans in $t+1$ and the value of the maturing credit contracts in $t+1$. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table C4. Real effects: first contract maturing in first semester

	$\Delta \log(wage\ bill)$	$\Delta \log(employment)$
	(1)	(2)
log(*pro debor*)	-0.329**	-0.295**
	(0.153)	(0.147)
District-quarter FE	✓	✓
Bank FE	✓	✓
Additional controls	✓	✓
N	222,265	222,265

Notes: The coefficients correspond to the 2SLS estimates for the model defined by eq. 7 and 8. The treatment and instrument assigned to the firms are those of the bank in the previous quarter in which the firm had the first maturing contract in the year.We consider only contracts with maturity in the first semester of the year. If a firm has contracts with two banks, we select the observations corresponding to the highest value. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix Table C5. Real effects: maximum value of credit maturing in the first semester

	$\Delta \log(wage\ bill)$		$\Delta \log(employment)$	
	(1) All	(2) 1 bank	(3) All	(4) 1 bank
log(*pro debor*)	-0.210**	-0.378***	-0.166*	-0.323**
	(0.099)	(0.139)	(0.092)	(0.131)
District-year FE	✓	✓	✓	✓
Bank FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
N	303,987	218,798	303,987	218,798

Notes: The coefficients correspond to the 2SLS estimates for the model defined by eq. 7 and 8. The treatment is the pro-debtor decisions faced by the bank in the previous quarter, where a firm has its largest amount of credit expiring in one of the quarters of the first half of the year. Columns (1) and (3) include the whole sample of firms, and columns (2) and (4) limit the sample for firms with a single bank relationship. A firm is considered to have a bank relationship if it holds a contract with a bank that matures on any date during the year. Standard errors are clustered at the court district level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$