Finding Justice? The Causal Effects of Winning Labor Lawsuits on Workers' Labor Market Outcomes*

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Abstract

This paper studies the causal impact of winning a labor court case on workers' subsequent labor market outcomes in Brazil. We exploit the random assignment of cases to judges with heterogeneous propensities to rule in favor of employees, which generates exogenous variation in the probability of winning. Using this design, we show that winning a case leads to significant increases in employment and labor income. These gains are not driven by reinstatement with the original employer. We then investigate potential mechanisms linked to institutional trust, examining workers' subsequent engagement with civil institutions, participation in social programs, and interactions with the state.

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1 Introduction

Institutions that adjudicate labor disputes are a central component of modern labor markets. By enforcing contractual obligations and providing redress for violations, labor courts not only protect individual workers but also shape broader patterns of employment and economic activity. A well-functioning judiciary can reduce uncertainty, facilitate contracting, and improve the allocation of resources, with implications that extend beyond the immediate parties involved (Jappelli et al., 2005; Ponticelli and Alencar, 2016; Acemoglu et al., 2020). Yet, despite the salience of these institutions in many countries, credible evidence on their causal impact on workers' trajectories remains scarce.

This paper studies the economic consequences of winning a labor lawsuit in Brazil, one of the world's largest and most active labor court systems. Using population-wide administrative data that link individual labor market histories to universe of court records, we exploit random assignment of cases to judges who differ in their propensity to rule for workers. Heterogeneity in judge leniency generates exogenous variation in the probability of winning, allowing us to identify the effect of a favorable ruling on subsequent labor market outcomes. Our design follows a growing literature that leverages random case assignment and judge heterogeneity for causal inference (e.g., Mueller-Smith, 2015; Bhuller et al., 2020; Cahuc et al., 2024; Bamieh et al., 2025).

We document three main findings. First, favorable rulings deliver economically meaningful gains in the formal labor market: over the three years following filing, annual labor income rises by roughly 8 percent and annual months worked increase by about 7 percent. Pre-trends are flat in event-time regressions, and first-stage estimates show that judge leniency is a very strong predictor of case outcomes. Second, these improvements are not driven by reinstatement with the original employer—a relatively rare outcome in the Brazilian system. Decompositions show that the entire effect arises from reemployment in *other* firms, consistent with court victories improving workers' access to formal jobs and bargaining position rather than returning them to the status quo ante. Third, we detect broader spillovers: successful

plaintiffs subsequently appear less often as defendants in civil courts (years +3 and +4), and their households reduce participation in the Bolsa Família conditional cash transfer program in the immediate post-period (years 0 and +1), with sharper declines for men.

We probe several mechanisms and heterogeneous responses to interpret these patterns. To distinguish labor market channels from one-off liquidity effects, we construct an alternative instrument based on judges' typical monetary awards (a "compensation IV", see Cahuc et al. 2024). The results obtained by using this margin are negligible, reinforcing that the main effects reflect improved formal reemployment rather than a transitory income shock. Heterogeneity by employment status at filing shows larger gains for workers who were unemployed at baseline, which rules out concerns that the results reflect stigma or employer reluctance to dismiss litigious employees, as in that case, the employed group would be expected to benefit more. Consistent with institutional context, effects are stronger after dismissal than resignation, and they are broadly similar across firm sizes but somewhat more pronounced for plaintiffs suing smaller firms. We also examine baseline environments: gains are larger where exposure to informality is lower (occupation–municipality measure), suggesting complementarity between judicial enforcement and the formal sector, and we find meaningful but cautious differences across areas with varying recorded inspection intensity.

Our contribution speaks to several literatures. First, we add worker-level evidence to research on courts and economic outcomes. Prior work shows that stronger judicial enforcement improves credit markets and firm investment (Jappelli et al., 2005; Ponticelli and Alencar, 2016) and that confidence in dispute resolution shapes cooperation and state engagement (Acemoglu et al., 2020). Closest to us, Cahuc et al. (2024) use judge bias in labor courts to study firm-side impacts; we complement this perspective by quantifying the causal effects on workers' labor supply and earnings at scale. Second, we contribute to the literature on shocks to income and labor supply. Evidence from lotteries finds labor supply responses that vary with wealth and context (Cesarini et al., 2017; Golosov et al., 2021), raising external validity concerns. Our setting provides a distinct form of random institutional intervention:

legal enforcement that changes worker–firm bargaining and job finding in the formal sector, not merely household resources. Third, we connect to work on enforcement, inspections, and informality, where compliance and formalization respond to monitoring and institutional quality (e.g., Ronconi, 2010; Almeida and Carneiro, 2012). Our heterogeneity suggests that judicial enforcement complements formal labor institutions and may partially substitute where administrative oversight is less salient.

Finally, we relate to the literature on social assistance and adult labor supply in Latin America. Studies of Bolsa Família generally find small or nuanced effects on labor supply—often preserving participation but shifting time toward domestic activities for women (Foguel et al., 2012) and yielding modest adult labor responses (Angelucci et al., 2010; Alzúa et al., 2013). In contrast, we show that judicial victories reduce households' reliance on transfers by steering plaintiffs back into formal employment, highlighting a channel—enforcement of rights—that is complementary to safety-net provision.

The Brazilian context is particularly informative for these questions. Labor lawsuits are common, monetary compensation is the typical remedy (reinstatement is rare), and cases are randomly assigned across judges within courts and years. Our administrative linkage yields a large estimation sample (over two million lawsuits filed by nearly two million workers, spanning 221 courts and 1,164 judges) and high-frequency, worker-level outcomes in the formal sector. Together, these features enable a precise and policy-relevant assessment of how access to justice affects labor market reintegration.

Our findings speak directly to policy. Courts can act as an institutional stabilizer in environments with high turnover and informality, by validating claims and facilitating reentry into formal jobs. This perspective matters for debates on the scope and accessibility of labor justice. For instance, Brazil's 2017 reform increased filing costs and altered incentives; while our study purposefully focuses on pre-reform cohorts to avoid composition changes, the mechanisms we document imply that restricting access could disproportionately hinder the very workers who benefit most from enforcement. More broadly, our results suggest that

strengthening adjudication capacity and maintaining random assignment can yield returns that extend beyond redistribution—supporting formal employment, reducing downstream litigation, and lowering reliance on transfers.

The rest of the paper proceeds as follows. Section 2 describes the institutional background, section 3 describe the datasets, sample selection and outlines the construction of instrument. Section 4 outlines our empirical strategy. Section 5 presents the main findings on labor income and employment, while Section 6 explores mechanisms and spillovers. Section 7 documents heterogeneous effects, and Section 8 presents robustness checks. Section 9 concludes.

2 Institutional Background

Brazil's judicial system is structured into four specialized branches—Federal, Labor, Electoral, and Military—alongside the 27 state-level courts. Labor-related disputes are handled exclusively by the *Justiça do Trabalho* (Labor Courts), which play a central role in enforcing employment rights in the country.

2.1 Court Structure and Procedures

The labor judiciary operates through a three-tiered structure. At the first instance, local *Varas do Trabalho* (labor courts) are staffed by a *juiz titular* (senior judge), often assisted by a *juiz substituto* (substitute judge). At the second instance, appeals are handled by 24 *Tribunais Regionais do Trabalho* (regional labor courts, TRTs), each serving a jurisdictional region (e.g., TRT-2 covers Greater São Paulo). At the top of the hierarchy sits the Superior Labor Court (TST), responsible for harmonizing labor jurisprudence nationwide.

Cases are randomly assigned to judges or sessions, minimizing judicial selection bias. A typical case proceeds in two phases: an initial conciliation hearing, which resolves nearly 43% of disputes at the first instance,[1] and, if unsuccessful, a judicial trial. About 30% of

[1] See International Labour Organization (2012).

first-instance decisions are appealed to second instance, with only a fraction reaching the TST.

2.2 Case Volume and Processing

Brazil leads the world in labor litigation. In 2019, labor filings represented roughly 11% of all new judicial cases across state and federal branches (Conselho Nacional de Justiça 2019). More recently, in 2023, labor courts received 3.5 million new lawsuits—equivalent to 3.2% of the economically active population (Franco et al. 2025). This contrasts sharply with Spain, where labor disputes correspond to only 1.6% of the working population in litigation.

Despite the scale of litigation, cases are processed relatively quickly. The average time to a first-instance ruling is around 191 days, appellate decisions take about 415 days, and TST judgments are typically issued within three years (Machado Meyer, 2024). Compared with civil or criminal courts, labor courts are more efficient: judges process an average of 170 cases per judicial official per year (Conselho Nacional de Justiça 2024).

2.3 Institutional Oversight and Reform Initiatives

The labor judiciary is monitored by the *Conselho Nacional de Justiça* (CNJ), which publishes biannual performance reports and coordinates reforms. To address rising caseloads, CNJ enacted Resolution 586 (2024), which strengthens the role of mediation and pre-litigation procedures through the *Centros Judiciários de Solução de Conflitos e Cidadania* (CEJUSCs). These initiatives aim to reduce formal litigation and judicial backlog by expanding alternatives to adversarial rulings.

2.4 Relevance for Our Study

This institutional setting—characterized by random assignment of judges, high caseloads, relatively fast resolution, and a growing emphasis on mediation—makes the Brazilian labor court system uniquely suited for causal analysis. Random assignment enables identification

through judge leniency, while the volume and speed of rulings enhance the external relevance of our findings. At the same time, ongoing reforms highlight the evolving role of labor courts in shaping workers' labor market outcomes and bargaining power.

3 Data

Our study relies on rich administrative data sources that can be linked at the individual level.

Labor Court Records: We use newly available case-level data covering the full population of labor lawsuits filed in Brazil between 2013 and 2020. These data include a unique judicial identifier, case start and end dates, classifications of case subjects, court location, the assigned judge, whether the case was randomly allocated, the final ruling, and unique identifiers for individuals (CPF) and firms (CNPJ) involved in each case.

RAIS (Relação Anual de Informações Sociais): This is a matched employer–employee dataset covering the universe of formal workers and firms in Brazil from 2002 to 2019. It provides detailed information such as job start and end dates, job location, employer and employee identifiers, contract type, occupation and sector codes, worker education, race, earnings, and the unique CPF and CNPJ identifiers.

Cadastro de Pessoas Físicas (CPF): Provided by Receita Federal (the Brazilian IRS), this registry includes the full population of Brazilian individuals. It contains personal details such as full name, date of birth, postal code of residence, year of death, and the unique CPF identifier.

3.1 Sample selection and construction of the instrument

Our labor court dataset covers more than ten million cases filed between 2013 and 2020. To analyze individuals' labor market trajectories before and after they initiate lawsuits, we restrict the sample to a narrower time window. We define the filing date of the lawsuit as time zero and construct a balanced panel around this reference point. Leveraging comprehensive

administrative records of the Brazilian formal labor market, we are able to track plaintiffs' outcomes up to 2019.

As will be shown in section 4, our analysis focuses on cases filed during judicial years 2014–2016. This restriction ensures that we observe three post-filing years even for cases filed in 2016. A further reason for capping at 2016 is the 2017 labor reform (Law 13,467), effective November 11, 2017, which substantially altered the institutional environment by liberalizing labor rules, making union contributions optional, and increasing contractual flexibility, and more importantly increasing the cost of filing lawsuits.[2] These changes likely affected case composition and filing behavior, potentially biasing our analysis.

From the five million cases filed during 2014–2016, we restrict attention to those that were randomly assigned (identified in the data). We exclude special cases subject to non-random allocation (e.g., collective disputes or cases filed under special procedural rules). We further keep only cases that have a first ruling, that are heard in tribunals with more than one judge, and that are not class actions (cases with a repeated code for multiple plaintiffs). These restrictions ensure that variation in the instrument reflects random judicial tendencies rather than systematic case features. The resulting sample comprises 4,053,096 cases.

To ensure the instrument is based on judges with a sufficiently representative caseload, we exclude judges who handle fewer than 75 percent of the tribunal's average caseload in a given year. This trimming removes only about 22,000 cases (0.05%) but avoids situations in which judges oversee very few cases (often due to specialization or temporary assignments). Some cases, despite being initially assigned randomly, are subsequently reallocated to another judge—for instance, due to specialization or connections with prior cases. We nonetheless treat them as assigned to the initial judge, analogous to an intention-to-treat approach.

After these restrictions, 4,031,096 cases remain for constructing the instrument. For each judge–year, we calculate the leave-one-out average win rate: the share of cases ruled in favor of the plaintiff (either full "upheld" rulings or partial "settlements") excluding the case in

[2]See Library of Congress (2017).

question.[3] The third possible outcome, "dismissal," corresponds to a ruling against the plaintiff. Formally, the judge leniency measure is:

$$JL_{jt(-i)} = \frac{\sum win_{jt} - win_i}{N - 1}, \tag{1}$$

where $\sum win_{jt}$ is the sum of case outcomes handled by judge j in year t and N is the total number of cases. This approach follows recent work on judge-induced variation (Mueller-Smith, 2015; Bhuller et al., 2020; Cahuc et al., 2024; Bamieh et al., 2025).

3.1.1 Linking to labor market outcomes

Although the instrument is based on roughly 4 million cases, we lose observations when merging labor court and RAIS data. While plaintiffs can be located in RAIS using their CPF, firm identifiers and unique firm–individual matches are sometimes unavailable. We merge plaintiffs to RAIS using CPF identifiers and link them to the relevant firm spells underlying each lawsuit. For consistency, we exclude extreme outliers in the income distribution and restrict the sample to cases filed within two years of the job separation that triggered the dispute as it is required by the law.

Our final estimation sample comprises 2,033,155 lawsuits filed by 1,962,782 workers during 2014–2016, spanning 221 labor courts and 1,164 judges. The instrument has a mean of 71.1% and a standard deviation of 0.08, providing ample variation to identify exogenous differences in winning probabilities. The actual win rate is 73.9%. Figure 1 plots the distribution of the judge-leniency instrument against actual winning probability, with a fitted local linear regression. The figure confirms that the instrument strongly predicts case outcomes, both on average and across deciles of the distribution.

Table 1 reports descriptive statistics for the final estimation sample of over two million lawsuits filed between 2014 and 2016. Panel A summarizes case characteristics. About 74% of cases

[3] In section 8, we present results using a time-invariant instrument as an additional robustness check.

result in a ruling favorable to plaintiffs, with settlements (41%) being slightly more common than full wins (33%). Nearly 29% of cases are appealed, and the average case duration is roughly nine months, though there is substantial variation. The average filing occurs about 210 days after dismissal, suggesting that most lawsuits are initiated relatively quickly. The mean final compensation is substantially lower than the initial claim value, consistent with prior evidence that settlement and judicial discretion reduce awarded amounts. Nevertheless, considering the baseline earnings, the amount is still large.

Panel B describes baseline employment conditions. Roughly one-third of plaintiffs file their lawsuits while still employed, whereas the majority sue after separation from their jobs. On average, workers had close to nine months of employment in the year prior to filing, and their mean annual labor income was around 19,000 BRL, which corresponds to the lower segment of the Brazilian wage distribution. Managers account for a small minority (5%) of the sample, indicating that the typical plaintiff is a rank-and-file worker rather than a high-level employee.

Panel C presents demographic characteristics of plaintiffs. The average age is 36, and one-third of plaintiffs are women. Educational attainment is concentrated at lower and middle levels: about 44% of plaintiffs have less than high school education, while half have completed high school; only 6.5% hold a college degree. Regarding racial composition, nearly half of plaintiffs identify as white, with the remainder distributed across mixed race (8%), black (7%), Asian and Indigenous categories, while about 10% of cases have missing race information. Overall, the descriptive evidence highlights that labor lawsuits in Brazil are disproportionately filed by relatively young, less-educated, and lower-income workers, underscoring the relevance of judicial rulings for vulnerable groups in the labor market.

4 Empirical Strategy

We are interested in estimating the causal effect of winning a labor lawsuit on individuals' subsequent labor market outcomes. A natural starting point is the following model:

$$Y_i = \omega Win_i + X_i'\beta + \theta_{ct} + \varepsilon_i, \tag{2}$$

where Y_i denotes an outcome of interest, such as labor income or annual months of work, measured as the average over k years after the case is filed. The indicator Win_i takes the value one if plaintiff i wins her case, and zero otherwise. The vector X_i includes plaintiff and case characteristics, while θ_{ct} are court-by-year fixed effects.

Equation (2) cannot be consistently estimated by OLS, since winning a case is endogenous: plaintiffs with stronger cases or better labor market prospects may be systematically more likely to win. To address this concern, we exploit the random assignment of cases to judges within court-years, as described in Section 3, and use judge leniency as an instrument for case outcomes. The identifying variation comes from heterogeneity in judges' decisions (in terms of being pro-worker or pro-firm), net of the individual case itself, which provides exogenous shifts in the probability of winning.

Formally, the first stage is given by:

$$Win_i = \gamma JL_{jt(-i)} + X_i'\beta_2 + \theta_{ct} + \nu_i, \tag{3}$$

where $JL_{jt(-i)}$ is the leave-one-out measure of judge j's leniency in year t. The second stage corresponds to Equation (2), with Win_i instrumented by $JL_{jt(-i)}$.

This strategy follows a growing literature that leverages random assignment and judge heterogeneity to obtain causal estimates (e.g., Mueller-Smith, 2015; Bhuller et al., 2020; Cahuc et al., 2024; Bamieh et al., 2025). The validity of the approach relies on three assumptions. First, relevance: judges must differ sufficiently in their propensity to rule in favor of plaintiffs and the instrument must be a good predictor of the actual case outcome, which we show in Figure 1. Second, exclusion: judge leniency affects plaintiffs' outcomes only through the

probability of winning their case, not via other channels. Third, monotonicity: assignment to a more lenient judge increases the chance of a plaintiff's win. We provide supporting evidence for these assumptions. While the exclusion restriction is inherently untestable, we can assess the exogeneity of the instrument, which we report later in this section.

In addition to the 2SLS specification, we complement our analysis with reduced-form event-study regressions of the form:

$$Y_{id} = \sum_{d=-3}^{2} \Gamma_d \big(Time_d \times JL_{jt(-i)} \big) + \lambda_{cdt} + \varepsilon_{id}, \tag{4}$$

where d indexes years relative to the lawsuit filing date ($d = 0$ in the filing year). The coefficients Γ_d trace how labor outcomes evolve before and after the case, conditional on judge leniency. Lack of pre-trends ($\Gamma_d \approx 0$ for $d < 0$) provides a placebo test for instrument validity, while significant post-filing estimates indicate causal effects.

Together, the 2SLS and event-study approaches provide a comprehensive framework for identifying the causal effect of winning a labor lawsuit on subsequent labor market trajectories.

4.1 The exogeneity and first stage

Table 2 presents the results of the exogeneity test (columns 1 and 2) and the first-stage regressions (columns 3 and 4). Column 1 shows that workers' time-invariant characteristics and prior labor market performance (past income, months of work, and employment) do not correlate with the instrument. The only exception is gender, for which the coefficient is statistically significant. Theoretically, with many covariates, one would expect at least one significant coefficient by chance. Moreover, the magnitude of this coefficient is extremely small, and its sign is opposite to the correlation observed with the actual winning probability. Nevertheless, the joint F-test fails to reject the null that the covariates jointly explain the leave-one-out mean judge leniency instrument (p-value = 0.382, F-statistic = 1.069). In

addition, neither the initial case value requested by the plaintiff nor the number of days the plaintiff waits after dismissal is correlated with judge leniency.[4] By contrast, column 2 shows that almost every variable is statistically significantly correlated with the actual winning probability. This pattern reinforces our econometric strategy: rather than relying on realized winners and losers, we exploit exogenous variation in the probability of winning that arises from the random assignment of judges.

As for columns 3 and 4, which report the results of the first-stage regressions with and without control variables (but always including court-year fixed effects), we find that our instrument based on judge leniency is a very strong—and nearly perfect—predictor of the actual case outcome.[5] This result is highly reassuring, as the relevance of the instrument is one of the key assumptions underlying our identification strategy.

5 Results

We present the main results on labor income and annual months of work estimated using Equation (4) in Figure 2. Importantly, we do not rescale the outcomes relative to year -1, but instead treat each event-time as an independent experiment. This choice reflects the core of our identification strategy: our claim to causality rests on the random assignment of judges, which effectively randomizes treatment status. Consistent with this approach, we find no statistically significant effects in the pre-treatment period, which provides strong support for the validity of our design and rules out concerns of differential pre-trends.

Turning to the post-treatment years, we observe statistically significant positive effects on both labor income and months of work. The estimates for event-time 0 are relatively smaller, which is consistent with institutional features of the process: lawsuits take on average about 9 months from filing to the first ruling, implying that treatment effects may only begin to materialize gradually within the filing year.

[4] By law, workers have up to two years to file a lawsuit against their employers.

[5] These results are obtained from Equation (3).

Table 3 summarizes the average effects, calculated as three-year averages from event-time 0 to 2. Columns 1 and 2 report reduced-form OLS estimates, while Columns 3 and 4 present 2SLS estimates using judge leniency as an instrument. Given that the first-stage coefficient is close to 0.90, the reduced-form and 2SLS estimates are very similar in magnitude and statistical significance. For this reason, and for ease of exposition, we present reduced-form estimates in the remainder of the paper, while noting that the 2SLS results remain robust and closely aligned.

The findings indicate that winning a labor lawsuit leads to economically meaningful improvements in labor market outcomes. Specifically, average annual labor income increases by approximately 1,527 BRL[6]—equivalent to about 8 percent of the baseline level—while annual months of work increase by 0.57 months on average, or about 7 percent relative to baseline. These are sizable effects, highlighting that access to justice through the labor courts has persistent impacts on both earnings and employment attachment. Taken together, these results underscore the role of judicial decisions as a key institutional determinant of individual labor market trajectories.

Beyond direct labor market outcomes, our findings contribute to the broader literature on the role of judicial institutions in shaping economic behavior. Prior work has shown that judicial efficiency and enforcement affect credit markets and firm investment (Jappelli et al., 2005; Ponticelli and Alencar, 2016), and that trust in judicial institutions fosters greater compliance and participation in state activities (Acemoglu et al., 2020). Our results complement these insights by showing that the protective role of courts directly improves workers' trajectories after dismissal, suggesting that access to effective dispute resolution mechanisms can mitigate the long-run scarring effects of job loss.

Taken together, these results underscore the importance of labor courts as institutions not only for redistributive justice but also for economic resilience. By providing timely compensation and legal recognition, favorable rulings appear to strengthen workers' reintegration

[6]Labor income is adjusted to 2019 prices.

into the formal labor market, offsetting some of the risks associated with unemployment in developing economies.

6 Mechanisms

The positive effects documented in the previous section raise the question of why winning a labor lawsuit improves workers' subsequent income and employment. Understanding the mechanisms is crucial for interpreting the results, as labor courts can influence outcomes through multiple channels. The wins may facilitate reinstatement with the former employer, ease reemployment in other firms, or provide liquidity through monetary awards. Moreover, effects may vary depending on firm size, baseline labor market conditions (e.g., informality, labor inspections), or the nature of the job separation. Beyond direct labor market impacts, judicial rulings may also affect households' reliance on social assistance programs and exposure to civil litigation. In what follows, we systematically examine these mechanisms to shed light on the pathways through which judicial enforcement shapes workers' economic trajectories.

6.1 Reinstatement versus reemployment

A first question is whether the observed effects are driven by reinstatement with the original employer or by reemployment elsewhere. To address this, we decompose the post-lawsuit employment outcomes into jobs at the sued firm and jobs at other firms. As shown in Table 4, the entire increase in income and months of work comes from employment in firms not involved in the lawsuit. This indicates that the effects are not driven by reinstatement but rather by improved reemployment prospects. Judicial victories appear to enhance workers' ability to reintegrate into the formal labor market, rather than returning them to the job they previously lost.

6.2 Liquidity versus labor market channel

A second potential mechanism is that winning provides workers with lump-sum compensation, which could act as a liquidity shock that influences subsequent employment and income. To test this, we construct an alternative instrument based on judges' average monetary awards ("compensation IV"). As shown in Table 5, results based on this instrument are negligible and statistically insignificant. This rules out a pure liquidity channel and reinforces the interpretation that our baseline effects are driven by labor market mechanisms, not by one-off income transfers.

6.3 Employment status at the time of filing

Next, we examine heterogeneity by employment status at the time of filing. Table 6 shows that the positive effects of winning are substantially larger for workers who were unemployed at the time of filing relative to those who were already employed. This pattern is consistent with the reemployment channel: individuals without jobs at baseline benefit the most from judicial victories, which ease reentry into the labor market.

These results also indicate that our findings are not driven by a "stigma effect." If stigma or employer hesitation were driving the results, we would expect effects to be concentrated among workers who were already employed when they won their case, as firms might avoid dismissing them to reduce the risk of further legal disputes. Similarly, if winning a lawsuit made workers "red-flagged" in the broader labor market, we would expect to see weaker or negative impacts for those who were unemployed at filing, since new employers could discriminate against them. Instead, we find the opposite: unemployed plaintiffs experience substantial improvements, both in income and in employment, following a successful lawsuit. These findings reinforce the interpretation that the observed effects arise from enhanced bargaining power and improved access to formal employment opportunities, rather than stigma or selective employer behavior.

6.4 Baseline labor inspections

We also explore heterogeneity by the intensity of labor inspections faced by firms prior to filing. Table 7 indicates somewhat larger effects in areas with fewer inspections recorded (about 1,976 BRL and 0.84 months) compared to those with more frequent inspections (about 1,769 BRL and 0.51 months). In the Brazilian context, however, low inspection intensity does not necessarily indicate weaker enforcement capacity, as inspections are often complaint-driven and reactive to prior violations. Accordingly, we interpret these findings cautiously: judicial impacts are heterogeneous across regulatory environments, but we refrain from labeling this strictly as substitution for weak enforcement. Instead, the results suggest that courts may play a compensatory role where inspection activity is relatively limited.

6.5 Baseline exposure to informality

Another dimension is baseline exposure to informality, measured at the occupation-municipality level. As shown in Table 8, the effects of winning a case are roughly twice as large for individuals in the bottom quartile of informality exposure (i.e., those in more formal labor market contexts). This suggests that the judiciary's impact is magnified where the formal sector is more prevalent, consistent with a complementarity between judicial enforcement and existing formal labor institutions.

6.6 Nature of separation

We further differentiate results by the cause of termination of the last contract—dismissal versus resignation. Table 9 shows that the effects are concentrated among workers who were dismissed, with strong positive impacts on both income and months of work. By contrast, results for resignation cases are small and statistically weak. This reinforces the interpretation that the effects operate primarily through reemployment prospects following involuntary job loss.

6.7 Firm size

We then investigate whether effects differ by firm size. As reported in Table 10, effects are broadly similar across the firm-size distribution but somewhat more pronounced among workers suing smaller firms. This indicates that judicial victories benefit workers across the board, though there may be stronger marginal impacts in more resource-constrained environments.

6.8 Civil litigations

An important complementary analysis concerns plaintiffs' subsequent involvement in civil litigation. Specifically, we examine whether winning a labor lawsuit affects the likelihood that plaintiffs later appear in civil courts as defendants. To do this, we merge our final estimation sample with the universe of civil court cases filed between judicial years 2013 and 2020. In this dataset, unique personal identifiers such as CPF are not available; instead, individuals are identified by their full names. Following the methodology used in previous research on Brazilian administrative data (e.g., Britto et al., 2022), we perform the merge based on common first and last names. This strategy is particularly effective in the Brazilian context, where the longstanding tradition of using both paternal and maternal surnames significantly enhances the precision of record linkage.

After implementing this procedure, we inevitably lose some observations—around half of the cases—but we still retain a sample of over one million labor lawsuits, which ensures ample statistical power for the analysis.

We then estimate the effect of winning a labor lawsuit on civil court involvement using our event-study framework described in Equation (4), extending the window to include four years after filing the labor suit. The results, presented in Figure 3, indicate a meaningful decline in civil court appearances as defendants in event years +3 and +4.

This finding suggests that improvements in economic stability brought about by successful labor litigation reduce subsequent exposure to civil disputes. One possible interpretation is

that workers who experience better labor market reintegration and higher incomes are less likely to default on contracts, loans, or other obligations that commonly trigger civil litigation. In other words, access to justice in the labor market appears to generate positive spillovers in other domains of the judicial system, reinforcing the broader role of courts in fostering both economic security and social stability.

6.9 Social assistance

Second, we construct household identifiers to study engagement with the Bolsa Família conditional cash transfer program. Results in Figure 4 show a significant decline in household participation in event year 2 for male plaintiffs. This indicates that labor court victories reduce reliance on social assistance, consistent with the observed rise in labor income and formal employment.

A related strand of research has examined how conditional cash transfer (CCT) programs affect labor supply and household behavior. In the context of Bolsa Família, Foguel et al. (2012) show that while the program does not significantly reduce female labor force participation, it induces a reallocation of time away from formal work and toward domestic activities and caregiving. This highlights a limitation of CCTs: although they provide crucial income support and reduce poverty, they do not necessarily foster stronger integration into the formal labor market. More broadly, the literature on CCTs points to modest or null impacts on adult labor supply in Latin America, with effects depending on program design and local labor market conditions (e.g., Angelucci et al., 2010; Alzúa et al., 2013). Our findings complement this evidence by showing that winning a labor lawsuit causally reduces Bolsa Família participation at the household level, particularly for men in the years immediately following the lawsuit. Unlike CCTs, which operate as a safety net, judicial victories appear to facilitate re-employment and income recovery in the formal sector, thereby reducing households' reliance on transfers. Taken together, these results underscore that institutional enforcement through the courts plays a distinct and complementary role to social assistance

programs in shaping labor market trajectories.

6.10 Summary

Taken together, the evidence indicates that judicial victories improve workers' labor market outcomes primarily through reemployment in new firms, not reinstatement or liquidity effects. The magnitude of impacts varies with baseline informality, local inspection activity, and the nature of job separation, and they are broadly similar across firm sizes. Importantly, the consequences extend beyond employment and income: judicial rulings reduce subsequent civil litigation and reliance on social transfers, underscoring the broader economic and social returns of judicial effectiveness in labor disputes.

7 Heterogeneous analysis

Table 11 reports the effects of winning a labor lawsuit on income and months of work, separately by gender, education, and occupation. The results demonstrate consistent positive impacts across groups, yet also highlight important differences in the magnitude of these effects. Overall, these findings reinforce our main conclusion that judicial rulings have economically meaningful consequences for workers' labor market trajectories, while also revealing variation that sheds light on underlying mechanisms.

Panel A shows that both male and female workers experience significant gains in labor income and employment months following a favorable judgment. The estimated effect on income is slightly larger for men (1,342 BRL) compared to women (1,275 BRL), although the differences are not statistically significant. A similar pattern emerges for months worked: men gain 0.53 months on average, compared to 0.44 months for women. These results suggest that winning a case benefits both groups.

In Panel B, we turn to heterogeneity by education. Workers with more education experience larger income gains (1,403 BRL) than those with lower education (923 BRL). Interestingly,

however, less-educated workers see a somewhat larger increase in months worked (0.55 vs. 0.42 months). One interpretation is that better-educated workers may convert a favorable ruling primarily into higher-paying job opportunities, while less-educated workers benefit more on the extensive margin of re-employment. These findings resonate with the literature on income effects and labor supply, where responses are often mediated by outside opportunities and baseline labor market attachment (e.g., Cesarini et al., 2017; Golosov et al., 2021). In the context of Brazilian labor courts, the results suggest that judicial rulings serve as an important buffer for more vulnerable workers, though the channels of adjustment differ by skill level.

Panel C explores variation by occupation. For managers, point estimates are large but imprecisely estimated, reflecting the relatively small number of such cases. In contrast, non-managers—who constitute the vast majority of plaintiffs—display strong and precisely estimated gains in both income and months worked. The difference in precision is consistent with the underlying composition of the sample, but it also suggests that the broader labor force, rather than a narrow set of highly skilled occupations, drives the overall effects of winning lawsuits. This reinforces the external validity of our results for understanding the role of judicial decisions in shaping labor market outcomes for typical workers.

Taken together, these results highlight two central insights. First, the positive effects of winning a lawsuit are broad-based, spanning gender, education, and occupational groups. This strengthens our interpretation that the findings are not driven by a particular subgroup but reflect a general mechanism. Second, the heterogeneity in magnitudes suggests that court decisions interact with existing labor market inequalities. Men, higher-educated workers, and non-managers tend to benefit somewhat more, pointing to complementarities between judicial outcomes and pre-existing labor market advantages. These findings echo the broader literature on how institutional quality affects economic outcomes (Jappelli et al., 2005; Ponticelli and Alencar, 2016; Acemoglu et al., 2020).

8 Robustness tests

8.1 Fishing expedition

A potential concern with our design is that some workers may exploit the judicial system once they succeed in a lawsuit. In particular, one might worry that favorable rulings encourage opportunistic behavior, such as repeatedly filing new lawsuits with potentially weak or even bogus claims. If this were the case, our main results could partly reflect strategic litigation rather than genuine improvements in labor market outcomes.

To address this issue, we examine whether winning a case affects the probability of filing a new lawsuit in the years immediately following the initial ruling. We estimate event-time regressions similar to Equation (4), using as the dependent variable an indicator for whether the plaintiff files another suit in years 0 through 2 after the initial case. The interaction coefficients between judge leniency and the event-time dummies are negative in the first two years: –0.27 in event year 0 and –0.08 in event year 1. By year 2, the coefficient is essentially zero (0.006) and statistically insignificant (see Figure 5).

These results provide little evidence of strategic or opportunistic litigation following a successful ruling. On the contrary, the short-run decline in refiling suggests that favorable court outcomes may even reduce the incentive to pursue additional cases in the immediate aftermath. This pattern reinforces our interpretation that the positive effects of winning a lawsuit reflect genuine improvements in workers' labor market reintegration, rather than gaming of the judicial system.

8.2 Time-invariant instrument

Another consideration regarding our approach is the use of a judicial-year-specific measure of judge leniency. Although our instrument passes several validity tests, we perform an additional robustness check by constructing a time-invariant version of the instrument. Instead of relying on cases handled by judges within a given judicial year, we compute the leave-one-out mean

leniency using all cases a judge handled between 2013 and 2020 (excluding the case in question). Specifically, we use 8,868,606 cases to construct this alternative instrument.[7]

The distribution of this time-invariant instrument is very similar to that of our baseline measure, with an average of 72% and a standard deviation of 7.7. In Table 12, we report the exogeneity and relevance tests, which are fully consistent with the results from our main instrument. Likewise, when we replicate the main analyses using the time-invariant instrument, the findings closely mirror those reported earlier (Table 13). This exercise provides further reassurance that our results are not driven by year-specific fluctuations. Instead, they reflect genuine heterogeneity in judges' leniency, which underpins our entire identification strategy.

9 Conclusion

This paper has examined the causal effects of winning labor lawsuits on workers' subsequent labor market outcomes in Brazil. Leveraging the random assignment of cases to judges with heterogeneous leniency, we show that favorable rulings significantly increase plaintiffs' income and employment. The magnitude of these effects is economically meaningful, suggesting that labor courts play an important role in mitigating the long-run scarring effects of job loss.

Our analysis reveals that the gains are not driven by reinstatement or by one-off liquidity injections, but instead reflect improved reemployment prospects. The fact that effects are strongest among workers who were unemployed at the time of filing provides further evidence against explanations based on stigma or employer caution. Heterogeneity analyses highlight that court victories benefit a broad range of workers, with somewhat larger gains among those with lower baseline attachment to the formal sector. Importantly, we also document spillovers to other domains: successful plaintiffs are less likely to become defendants in civil cases and their households reduce reliance on social assistance programs.

These findings underscore the broader economic and social value of accessible judicial

[7]We impose exactly the same restrictions on cases and judges as in our baseline specification before calculating the leave-one-out averages.

institutions. By providing effective enforcement of labor rights, courts not only redistribute resources ex post but also strengthen workers' ability to reenter the labor market and reduce reliance on the welfare state. In this sense, labor courts act as a stabilizing force in contexts characterized by high turnover and informality.

From a policy perspective, our results suggest that restrictions on access to justice—such as those introduced by the 2017 labor reform in Brazil, which raised the costs of filing lawsuits—may weaken an important safety net for vulnerable workers. Ensuring that labor courts remain accessible and efficient is therefore critical for both equity and efficiency. More broadly, our findings contribute to a growing literature emphasizing that the quality of judicial institutions is a key determinant of economic development.

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Tables

Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.
Panel A: Case Characteristics			
win (all)	2,033,155	0.7389	0.4393
win (upheld)	2,033,155	0.3263	0.4689
win (settlement)	2,033,155	0.4126	0.4923
appeal	2,033,155	0.2893	0.4539
duration (months)	2,033,155	9.2524	9.0836
judicial years	2,033,155	2015.194	0.7966
initial case value	2,033,155	87498.19	91772.55
final compensation	2,033,155	35698.9	31407
days-to-sue	2,033,155	209.1348	203.2103
Panel B: Baseline Employment			
suing while employed	2,033,155	0.3287	0.4698
managers	2,033,155	0.0537	0.2254
months of work	2,033,155	8.6952	3.8354
labor income	2,033,155	19024.16	16358.61
Panel C: Plaintiff Demographics			
age	2,033,155	36.2622	10.8793
women	2,033,155	0.3245	0.4682
lower-education	2,033,155	0.4366	0.4960
high-school	2,033,155	0.4984	0.5000
college	2,033,155	0.0650	0.2465
asian	2,033,155	0.0016	0.0400
white	2,033,155	0.4784	0.4995
black	2,033,155	0.0729	0.2599
native	2,033,155	0.0834	0.2761
mixed	2,033,155	0.4339	0.4956
race-missing	2,033,155	0.0998	0.0984

Notes: The table reports summary statistics for case and plaintiff characteristics in the final estimation sample.

Table 2: Testing for exogeneity and relevance of the instrument

	(1) IV	(2) Pr (Win)	(3) Pr (Win)	(4) Pr (Win)
IV			0.8961***	0.8954***
			(0.0176)	(0.0174)
suing while employed	0.0002	0.0061***	0.0060***	
	(0.0001)	(0.0013)	(0.0013)	
managers	0.0002	0.0232***	0.0231***	
	(0.0002)	(0.0021)	(0.0022)	
days-to-sue	0.0000	-0.0001***	-0.0001***	
	(0.0000)	(0.0000)	(0.0000)	
age	-0.0000	0.0039***	0.0039***	
	(0.0000)	(0.0003)	(0.0003)	
age^2	-0.0000	-0.0000***	-0.0000***	
	(0.0000)	(0.0000)	(0.0000)	
women	-0.0001**	0.0158***	0.0159***	
	(0.0001)	(0.0016)	(0.0016)	
Omitted: less than high-school				
high-school	0.0001	0.0090***	0.0089***	
	(0.0001)	(0.0011)	(0.0011)	
college	0.0003	0.0026	0.0023	
	(0.0002)	(0.0025)	(0.0025)	
Omitted: white				
native	-0.0008	-0.0207*	-0.0200*	
	(0.0006)	(0.0107)	(0.0107)	
black	0.0001	-0.0253***	-0.0254***	
	(0.0001)	(0.0018)	(0.0018)	
mixed	-0.0002	-0.0070	-0.0068	
	(0.0005)	(0.0058)	(0.0057)	
asian	-0.0000	-0.0158***	-0.0158***	
	(0.0001)	(0.0010)	(0.0010)	
missing race	-0.0003	-0.0210***	-0.0207***	
	(0.0003)	(0.0036)	(0.0036)	
std. initial case-value	-0.0000	0.0000	0.0000	
	(0.0000)	(0.0003)	(0.0003)	
std. pre months	0.0000	0.0238***	0.0238***	
	(0.0000)	(0.0007)	(0.0007)	
std. pre income	-0.0000	0.0065***	0.0065***	
	(0.0000)	(0.0011)	(0.0011)	
Court Year FE	yes	yes	yes	yes
Mean dependent	0.711	0.739	0.739	0.739
Pvalue	0.382	2.41e-238	1.06e-234	
F-statistics	1.069	216.5	209.5	
R^2	0.818	0.0385	0.0439	0.0326
Observations	2033155	2033155	2033155	2033155

The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at court and judicial year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The effects of winning labor lawsuits on income and months of work: OLS vs. 2SLS

	OLS Reduced Form		2SLS IV	
	(1)	(2)	(3)	(4)
	Income	Months	Income	Months
leave_out_IV	1367.510**	0.512***		
	(542.385)	(0.193)		
win			1527.209**	0.571***
			(598.061)	(0.212)
Court-Year FE	yes	yes	yes	yes
baseline	19024.16	8.70	19024.16	8.70
R^2	0.0224	0.0163	0.00192	0.00157
Observations	2033155	2033155	2033155	2033155

The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The effects of winning labor lawsuit: sued vs. not-sued firms

	(1)	(2)	(3)	(4)
	Income (not-sued)	Income (sued)	Months (not-sued)	Months (sued)
leave_out_IV	1258.127**	109.268	0.463**	0.049
	(565.959)	(119.499)	(0.207)	(0.043)
Court Year FE	yes	yes	yes	yes
baseline	4547.90	14476,25	2.36	6.33
R^2	0.0184	0.0133	0.0134	0.0191
Observations	2033155	2033155	2033155	2033155

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 5: The effects of judge-induced compensation on income and months of work: OLS vs. 2SLS

	(1) Income	(2) Months	(3) Income	(4) Months
ln_compensation_IV	15.065	0.005		
	(13.912)	(0.004)		
ln_compensation			14.738	0.005
			(13.599)	(0.004)
Court Year FE	yes	yes	yes	yes
baseline	19024.16	8.70	19024.16	8.70
R^2	0.0224	0.0163	0.000835	0.000451
Observations	2033155	2033155	2033155	2033155

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 6: The effects of winning labor lawsuit: employed vs. not-employed at filing the suit

	(1) Income (employed)	(2) Income (not-employed)	(3) Months (employed)	(4) Months (not-employed)
leave_out_IV	1071.447	986.170***	0.281*	0.417***
	(715.991)	(345.060)	(0.155)	(0.143)
Court Year FE	yes	yes	yes	yes
baseline	21343.60	17888.24	9.81	8.15
R^2	0.0216	0.0223	0.0150	0.0148
Observations	668384	1364770	668384	1364770

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Employed means the worker is employed at the time of filing and not-employed means that she is not. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 7: The effects of winning labor lawsuit by firm inspections

	(1) Income (bottom 25%)	(2) Income (top 25%)	(3) Months (bottom 25%)	(4) Months (top 25%)
leave_out_IV	1975.534**	1769.017***	0.837**	0.514***
	(847.115)	(579.387)	(0.375)	(0.177)
Court Year FE	yes	yes	yes	yes
baseline	18079.50	18122.47	9.33	7.96
R^2	0.0231	0.0360	0.0186	0.0191
Observations	450940	456554	489860	507265

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. 'The 'Bottom 25%' category includes firms in the lowest quartile of the firm-level labor inspection distribution, whereas the 'Top 25%' category includes firms in the highest quartile. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 8: The effects of winning labor lawsuit by informality

	(1) Income (bottom 25%)	(2) Income (top 25%)	(3) Months (bottom 25%)	(4) Months (top 25%)
leave_out_IV	3088.614**	867.154*	0.837**	0.514***
	(1252.325)	(466.600)	(0.375)	(0.177)
Court Year FE	yes	yes	yes	yes
baseline	23882.77	14749.59	9.33	7.96
R^2	0.0254	0.0227	0.0186	0.0191
Observations	489860	507265	489860	507265

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. 'The 'Bottom 25%' category includes firms in the lowest quartile of occupation-region informality distribution, whereas the 'Top 25%' category includes firms in the highest quartile. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 9: The effects of winning labor lawsuit: dismissal vs resignation

	(1) Income (dismissal)	(2) Income (resignation)	(3) Months (dismissal)	(4) Months (resignation)
leave_out_IV	1569.404***	2047.557*	0.628***	0.375
	(574.364)	(1144.699)	(0.219)	(0.308)
Court Year FE	yes	yes	yes	yes
baseline	20322.56	17050.74	8.98	8.36
R^2	0.0254	0.0244	0.0166	0.0221
Observations	1444625	257650	1444625	257650

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 10: The effects of winning labor lawsuit by firm size

	(1) Income (bottom 25%)	(2) Income (top 25%)	(3) Months (bottom 25%)	(4) Months (top 25%)
leave_out_IV	1367.846***	1247.718	0.375*	0.586*
	(517.716)	(798.434)	(0.193)	(0.308)
Court Year FE	yes	yes	yes	yes
baseline	15071.26	21397.07	7.62	9.35
R^2	0.0216	0.0400	0.0164	0.0337
Observations	515708	508770	515708	508770

Notes: The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. 'The 'Bottom 25%' category includes firms in the lowest quartile of the firm-size distribution, whereas the 'Top 25%' category includes firms in the highest quartile. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 11: The effects of winning labor lawsuits on income and months of work: Heterogeneity by groups

Panel A: By Gender				
	Income (Men)	Income (Women)	Months (Men)	Months (Women)
leave_out_IV	1341.960**	1274.717**	0.534***	0.442*
	(590.449)	(596.514)	(0.202)	(0.230)
Court Year FE	yes	yes	yes	yes
Baseline	20830.38	15264.70	8.88	8.31
R^2	0.0328	0.0143	0.0225	0.0136
Observations	1373340	659815	1373340	659815
Panel B: By Education				
	Income (More edu)	Income (Low edu)	Months (More edu)	Months (Low edu)
leave_out_IV	1403.489**	922.580*	0.423**	0.553**
	(556.619)	(558.224)	(0.172)	(0.239)
Court Year FE	yes	yes	yes	yes
Baseline	21674.89	15602.93	9.09	8.18
R^2	0.0195	0.0369	0.0141	0.0243
Observations	1145576	887578	1145576	887578
Panel C: By Occupation				
	Income (Managers)	Income (Others)	Months (Managers)	Months (Others)
leave_out_IV	3058.133	1148.859**	0.678	0.491**
	(2221.523)	(512.559)	(0.474)	(0.203)
Court Year FE	yes	yes	yes	yes
Baseline	38583.04	17914.54	10.41	8.60
R^2	0.0131	0.0276	0.0155	0.0182
Observations	109150	1924002	109150	1924002

Notes: The dependent variables are averages over the three years following lawsuit filing. The sample comprises judicial years 2014–2016. "Low edu" stands for less than high school diploma, "High edu" stand for high school and more. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Testing for exogeneity and relevance of the time-invariant instrument

	(1) IV	(2) Pr (Win)	(3) Pr (Win)	(4) Pr (Win)
IV			0.8954***	0.8936***
			(0.0507)	(0.0510)
suing while employed	0.0002*	0.0061***	0.0060***	
	(0.0001)	(0.0013)	(0.0013)	
managers	0.0002	0.0232***	0.0231***	
	(0.0001)	(0.0021)	(0.0022)	
days-to-sue	0.0000	-0.0001***	-0.0001***	
	(0.0000)	(0.0000)	(0.0000)	
age	-0.0000	0.0039***	0.0039***	
	(0.0000)	(0.0003)	(0.0003)	
age^2	0.0000	-0.0000***	-0.0000***	
	(0.0000)	(0.0000)	(0.0000)	
women	-0.0001**	0.0158***	0.0159***	
	(0.0001)	(0.0016)	(0.0016)	
high-school	0.0001	0.0090***	0.0090***	
	(0.0001)	(0.0011)	(0.0011)	
college	0.0002	0.0026	0.0024	
	(0.0002)	(0.0025)	(0.0025)	
native	-0.0001	-0.0207*	-0.0206*	
	(0.0004)	(0.0107)	(0.0107)	
black	0.0002	-0.0253***	-0.0255***	
	(0.0001)	(0.0018)	(0.0018)	
mixed	-0.0004	-0.0070	-0.0066	
	(0.0003)	(0.0058)	(0.0058)	
asian	0.0000	-0.0158***	-0.0158***	
	(0.0000)	(0.0010)	(0.0010)	
missing race	-0.0003	-0.0210***	-0.0207***	
	(0.0002)	(0.0036)	(0.0036)	
std. initial case-value	-0.0000	0.0000	0.0000	
	(0.0000)	(0.0003)	(0.0003)	
std. pre months	0.0000	0.0238***	0.0238***	
	(0.0000)	(0.0007)	(0.0007)	
std. pre income	-0.0000	0.0065***	0.0066***	
	(0.0000)	(0.0011)	(0.0011)	
Court Year FE	yes	yes	yes	yes
Mean dependent	0.734	0.739	0.739	0.739
Pvalue	0.472	2.41e-238	1.70e-234	
F-statistics	0.984	216.5	209.1	
R^2	0.826	0.0385	0.0425	0.0312
Number of observations	2033155	2033155	2033155	2033155

The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at court and judicial year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

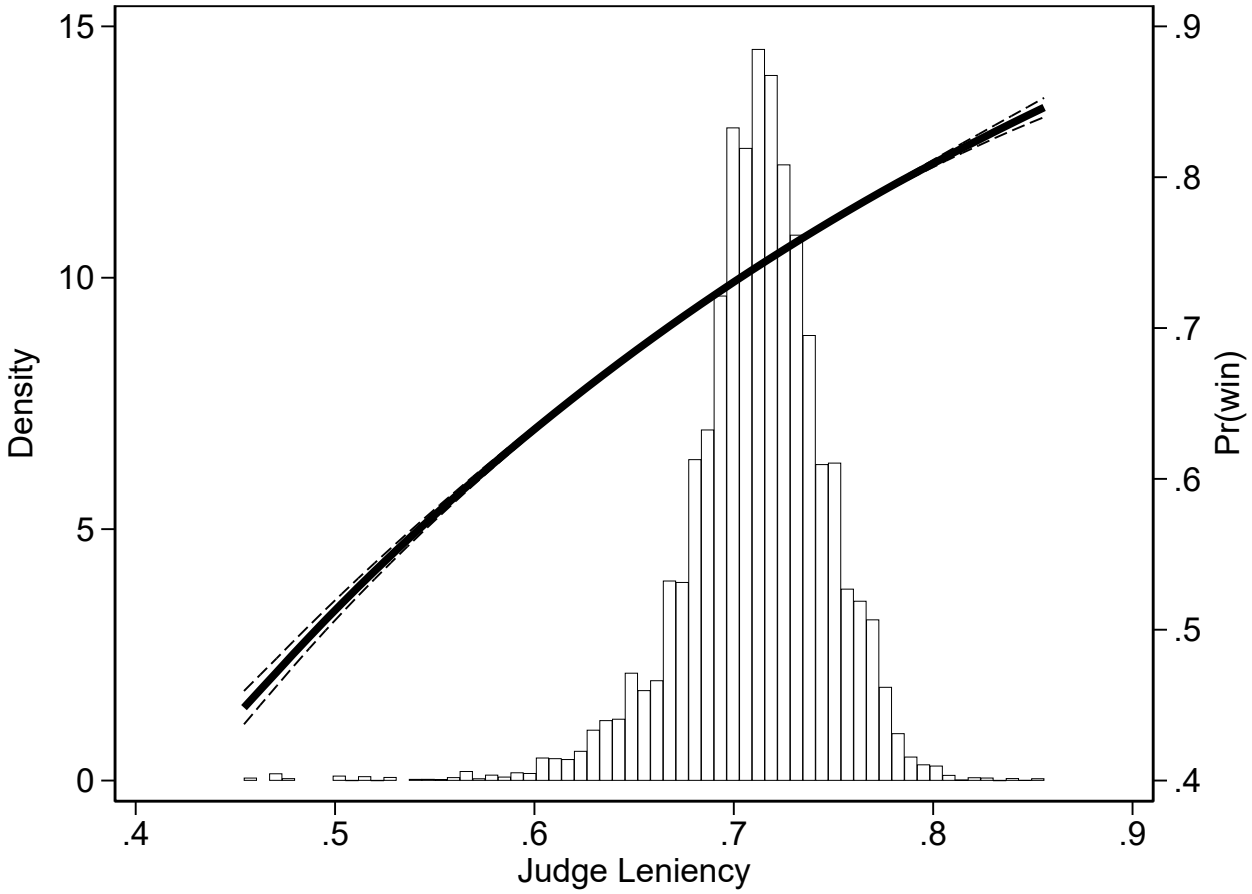
Table 13: The effects of winning labor lawsuits on income and months of work: OLS vs. 2SLS

	(1) Income	(2) Months	(3) Income	(4) Months
time-invariant IV	1443.358**	0.607**		
	(698.502)	(0.246)		
win			1615.131**	0.679**
			(756.048)	(0.267)
Court Year FE	yes	yes	yes	yes
baseline	19024.16	8.70	19024.16	8.70
R^2	0.0224	0.0164	0.00186	0.00115
Observations	2033155	2033155	2033155	2033155

The dependent variables are averages of three years after filing the lawsuit. The sample comprises judicial years from 2014 to 2016. Standard errors are clustered at the court and judicial-year level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

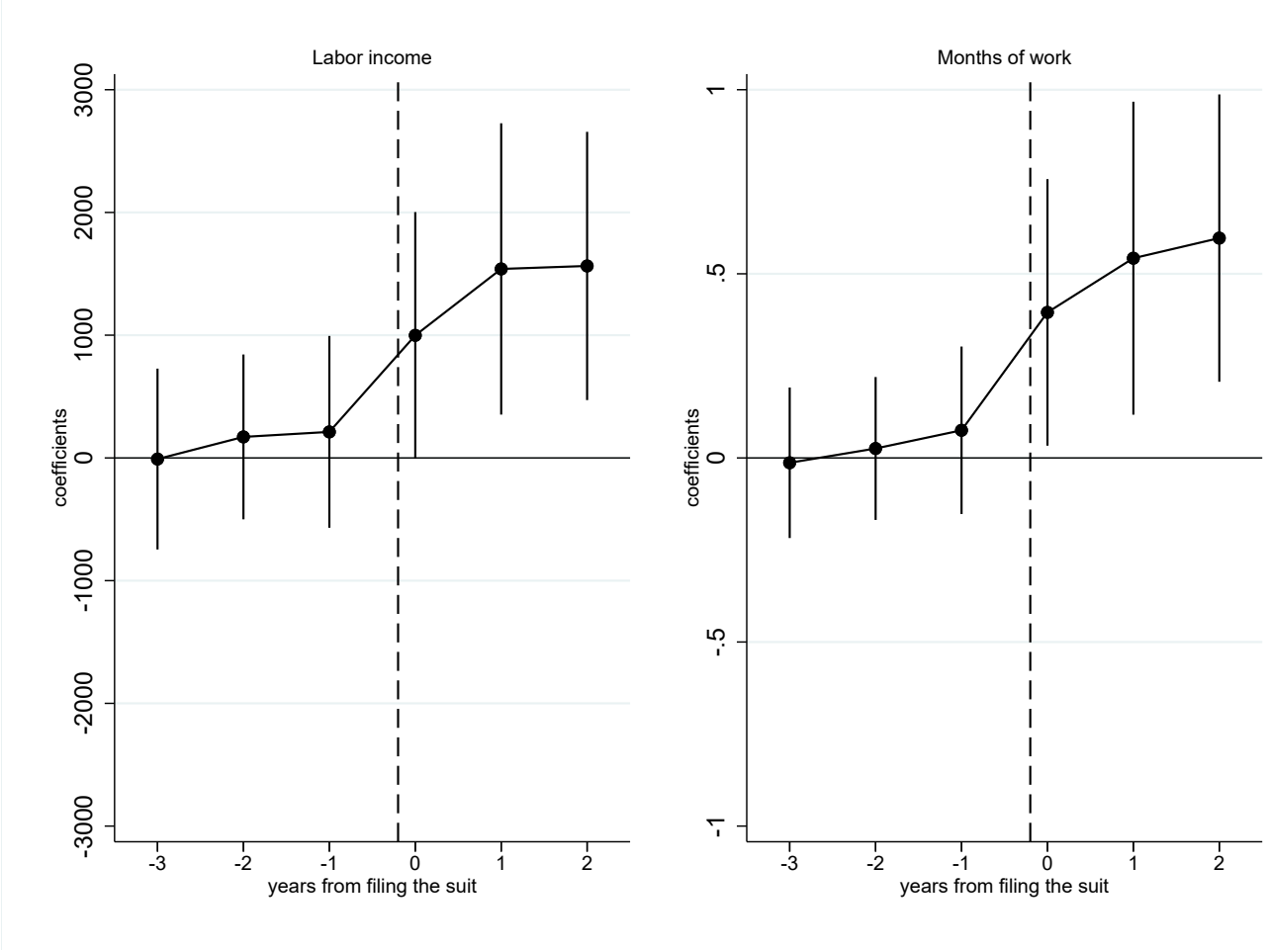
Figures

Figure 1: Relationship Between Judge Leniency Instrument and Actual Winning Probability



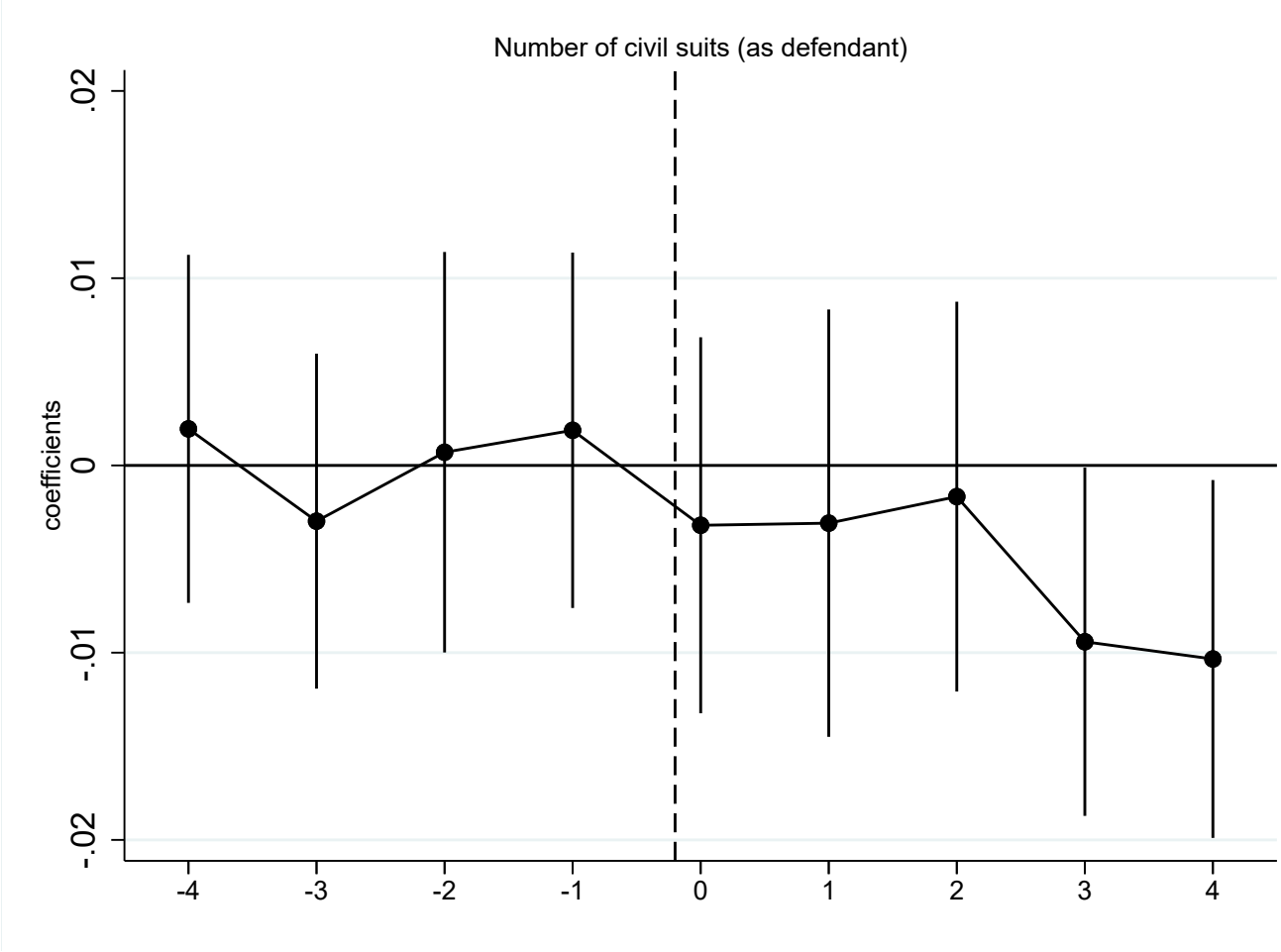
Notes: The figure plots results of a local linear regression of degree 2. The x-axis shows the leave-one-out judge leniency instrument. The right y-axis reports the actual winning probability, while the left y-axis displays the distribution of the instrument. Both the judge leniency measure and the winning probability are expressed as mean-adjusted residuals relative to their court-by-judicial-year averages. The number of observations is 2,033,155.

Figure 2: Results on labor income and months of work: reduced form regressions



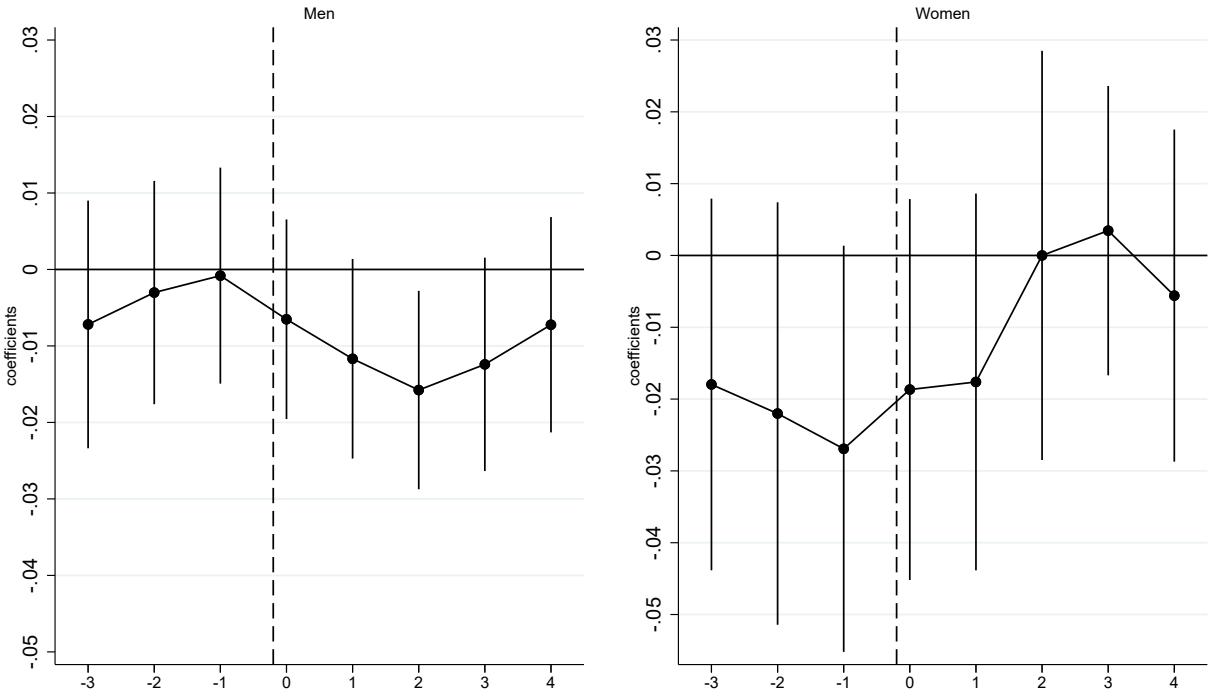
Notes: The figure plots the coefficient estimates of Γ_d obtained from Equation (4) on annual labor income (left-panel) and months of work (right-panel). The number of cases is 2,033,155, the number of observations is 12,198,930.

Figure 3: Results on civil litigations as defendant



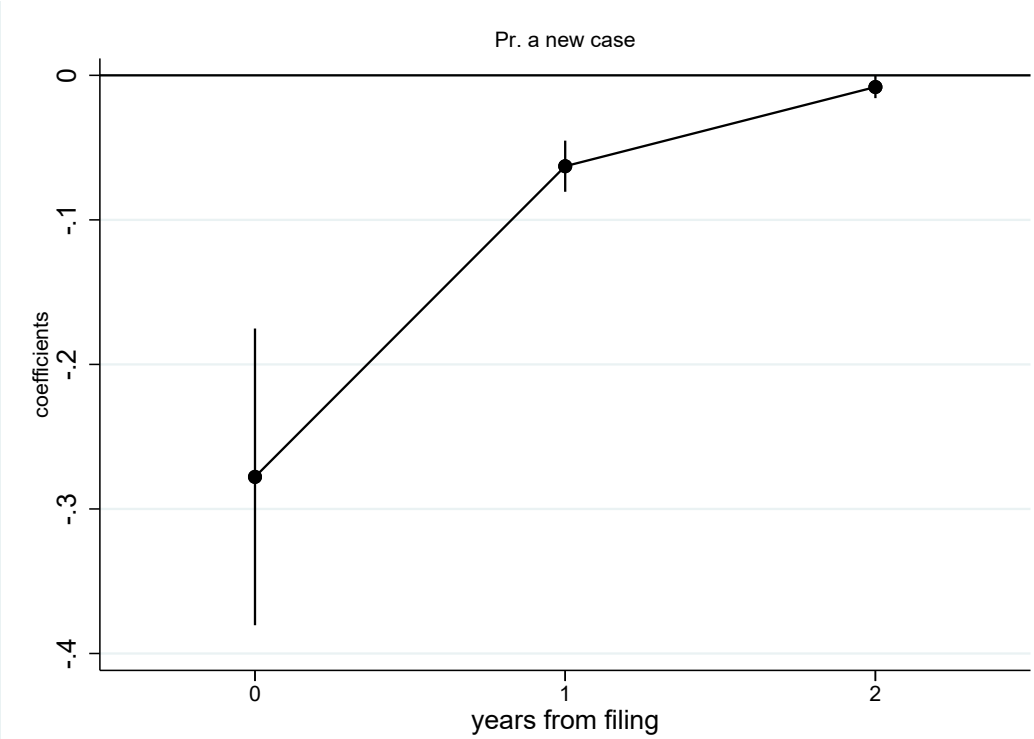
Notes: The figure plots the coefficient estimates of Γ_d obtained from Equation (4). Dependent variable is the number of civil lawsuits workers get involved as defendants.

Figure 4: Results on social transfer by gender



Notes: The figure plots the coefficient estimates of Γ_d obtained from Equation (4). Dependent variable is the household-level probability of receiving social transfer through Bolsa Familia.

Figure 5: Results on follow-up labor suits



Notes: The figure plots the coefficient estimates of Γ_d obtained from Equation (4). Dependent variable is the probability of filing new labor lawsuit.