

Information, Conciliation, and the Lawyer Market: Experimental Evidence from Mexico's Labor Courts *

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Abstract

Courts in developing countries are congested with low-value cases with low settlement rates. We randomize three interventions among dismissed workers as they arrive at the Mexico City Labor Court — before they hire a lawyer. A personalized statistical prediction of case outcomes recalibrates beliefs and reshapes the market for representation: the share suing with low-quality "coyote" lawyers falls by a third. But information alone does not produce settlement. Pairing it with a court-issued invitation to a pre-lawsuit conciliation meeting raises settlement by 16 percentage points — a third of the control mean — and reduces suits by 11 points, converting suits into settlements among the cases with the lowest predicted value. A free ride to the public attorney raises consultation by 20 points but changes neither coyote retention nor worker welfare proxies. After results from the experiment, Mexico made pre-lawsuit conciliation mandatory. Consistent with our experimental results, disputes reaching courts fell by more than half even as the the number of workers making formal conciliation filings increased by more than a third.

Keywords: Pre-judicial conciliation, settlement, lawyer quality, coyotes, information frictions, congested courts.

JEL codes: K31, K41, K42, J52, J83

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1 Introduction

Courts in developing countries are congested, and the congestion is economically costly: judicial delay has been shown to depress credit, investment, and firm growth (Visaria, 2009; Ponticelli and Alencar, 2016; Boehm and Oberfield, 2020; Chemin and Aberra, 2020). Settlement rates in civil courts are low across the developing world (Chang and Klerman, 2023). A large share of the caseload in these courts consists of low-value disputes that follow the same procedures as the smaller number of high-value cases.¹ Court reforms have concentrated on the supply side: improving judicial incentives and streamlining procedures (Chemin, 2009; Kondylis and Stein, 2018), or creating specialized and small-claims tribunals (Lichand and Soares, 2014; Visaria, 2009; Mattsson and Mobarak, 2023). The supply-side reforms do not address the volume of low-value suits or the low settlement rates. We focus on the demand side, and ask whether encouraging parties to attempt conciliation before a suit is filed is effective in reducing the volume of court cases.

France, South Africa, Australia, and Argentina all mandate a version of required conciliation for labor disputes. Existing evaluations of conciliation regimes use descriptive data (Bhorat et al., 2009; Esposito et al., 2014; Fraisse et al., 2015). In this paper, we provide field experimental evidence on the effectiveness of pre-lawsuit conciliation. Following the results of the research we report here, Mexico adopted required conciliation for worker dismissal disputes in 2019. We use the staggered rollout of Mexico's 2019 reform to assess whether the outcomes we observe in the experiment are present at national scale, providing evidence on the causal evidence on pre-lawsuit conciliation at a national scale.

Our setting is the Mexico City Labor Court (MCLC), which approached us in 2015 to study why its settlement rates were low. When we began working with the court, it received around 35,000 filings a year, over 95% of which were claims for unpaid severance by dismissed workers. The court resolved around 30,000 cases a year, and carried a backlog of more than 100,000 cases. The median case decided by judgment took around four years against a statutory 90-day target. Roughly half of its cases were settled, against 70% of cases in Japan's labor tribunals and 79% of unfair-dismissal claims before Australia's Fair Work Commission, both of which typically close a case within two to four months (Ebisui et al., 2016). Discussions with the court's leadership pointed to three demand-side frictions behind the low settlement rate – plaintiff misinformation, the prevalence of low-quality "coyote" lawyers and the lack of knowledge of the availability of free public lawyers. We designed our experiment to test these three hypotheses.

Plaintiff misinformation is driven by the fact that 94% of workers are first-time users of the court. Workers approaching the MCLC before filing a suit report an average subjective probability of winning at judgment of 89%, while statistical models trained on 5,000 completed casefiles predict 41% for the same cases; about half can state no figure for what they expect to recover, and fewer

¹ Brazil's National Council of Justice reports approximately 85 million pending judicial processes nationwide; state courts, which handle most low-value disputes, account for 78% (Conselho Nacional de Justiça, 2024). India has 55.8 million pending cases as of early 2026, with roughly 66% of pending civil cases involving land and property disputes that are typically low-value (DAKSH, 2017; Data for India, 2026). In Bosnia and Herzegovina, small claims represent more than 70% of all civil cases (Osmanovic-Pasic et al., 2019). The ubiquity of the problem is indicated by the 132 countries that have built institutional workarounds in the form of either a stand-alone small claims court or a simplified procedure (Petkova and Senderayi, 2020). Compounding this problem, settlement rates in civil courts are low across developing countries (Chang and Klerman, 2023).

than one-third correctly identify the 90-day severance entitlement. This naivete leaves workers susceptible to persuasion by lawyers whose incentives are not perfectly aligned with their own. The federal labor authority warns workers in particular against using the low-quality "coyote" lawyers who recruit clients outside the courthouse.² But [Sadka et al. \(2024\)](#), working in the same court, find evidence of misalignment between workers and lawyers more broadly: information about case strength provided to parties in ongoing cases raises settlement only when the worker is present to receive it. In Section 4 we show that uninformed workers and low-quality, high-volume (coyote) lawyers interact in a way that reverses the well-established theoretical prediction that contingency-fee lawyers settle too often ([Helland and Tabarrok, 2004](#); [Calandrillo et al., 2024](#)). The coyotes' high-volume business model reduces their marginal cost of continuing a case to nearly zero. Because their clients are liquidity-constrained and undiversified, it is the client, not the lawyer, who values settlement more. The Court management believed that making workers aware of the free public lawyers from the Procuraduría de la Defensa del Trabajo (PRODET) was a potential way to inform workers and avoid coyotes.

Given the court's concern with workers' choice of lawyers, it was crucial that the intervention took place before workers hired a lawyer. We chose to intervene as the worker arrived at the court seeking information on how to file, before they had hired a lawyer. This makes the choice of lawyer an experimental outcome rather than a fixed feature of the worker's case, allowing us to study how the interventions shape the market for legal services. We randomized the interventions at the day level among more than 3,000 dismissed workers who stopped at an information booth we operated at the court's only entrance between 2017 and 2019. We provided all workers approaching the booth with a brochure describing the filing process. Compared with a control group receiving only this brochure, we test three treatments. The *Information* arm added a personalized statistical prediction of the worker's own case — the range of settlement amounts among similar historical cases, the share of cases still unresolved after four years, and the probability of winning nothing if the worker continued the suit until a court judgment. These predictions were generated by machine-learning models trained on the 5,000 completed casefiles. The Information treatment targets overconfidence and misinformation. Importantly, the printout contains no advice about lawyers and no warning about coyotes. The *Conciliation* arm added to the information a letter inviting the worker's employer to a pre-lawsuit conciliation appointment with a court conciliator. The letter was on court letterhead, and signed by the Secretary General of Individual Affairs. The Conciliation arm contains the same statistical information component as the Information arm, so that the contrast between the two identifies the effect of the conciliation channel holding this information fixed.³ The *Referral* arm offered a pre-paid ride to the public attorney's office (PRODET) one mile away, targeting the search-and-transport friction in reaching free legal counsel. An earthquake in September 2017 damaged the public attorney's building and made the Referral arm temporarily infeasible, so we ran Information and Conciliation in a first phase and Referral in a second, each compared to its contemporaneous

² Mexico has no Bar exam and an order of magnitude more law schools than the US. Mexican authorities warn that the coyote lawyers deceive clients and have made it official policy to try to get rid of them. They have stated that coyotes "deceive you and charge you for a procedure that is free... They advise you not to settle and to go to trial, with the promise of obtaining greater financial resources" (Centro Federal de Conciliación y Registro Laboral, <https://www.facebook.com/centrofcrl/videos/1117632750035898/>.)

³ We consider the letter itself and any signal value in it as part of the conciliation channel. Conciliation supplies a physical space and a court conciliator at the court premises.

control group (Section 3).

Our central results demonstrate a complementarity between beliefs and institutions: calibrating workers' beliefs influences the lawyer they hire, but does not, by itself, produce settlement; settlement rates increase when workers receive both information and access to an institution through which to bargain. The calculator does move beliefs. Roughly 30% of treated workers lower their stated probability of winning between the baseline and a re-elicitation conducted minutes after receiving the information, and the share unable to state an expected recovery amount falls by a third (Section 5.2). But the Information arm raises the settlement rate by a statistically insignificant 3 percentage points. The Conciliation arm raises settlement by 16 percentage points, roughly a third of the control mean, and we reject equality of the Information and Conciliation arms at the 1% level (Section 5.3). The Referral arm facilitates a meeting with the public attorney, but provides no information. This raises settlement by 7 percentage points, significant at the 10 percent level. Filing of suits moves in a corresponding direction. Conciliation reduces the share of workers filing suit by 11 percentage points, about a quarter of the control mean, while the Information and Referral arms produce smaller reductions that are not statistically significant in the full sample. The Conciliation and Referral arms cause low-value cases to disappear. Splitting workers by predicted case value, Conciliation lowers filing of workers with case values below the median by 20 percentage points and raises settlement by 24 percentage points. Referral shows a similar pattern at about half the magnitude, while the point estimates for Information show a muted and insignificant negative suing / positive settlement pattern (Section 5.4). The treatments have no effect on cases with predicted values above the median, and do not increase the share of workers who walk away, neither settling nor suing, in any arm. Conciliation thus decongests the court by converting would-be lawsuits into settlements, particularly for low value cases where, given fixed court costs and congestion, litigation is likely least appropriate.

The Information and Conciliation treatments also restructure the market for lawyers by making it less likely that the worker files a lawsuit with a low-quality coyote lawyer. The share who sue with a coyote falls by 4 percentage points in the Information arm and 5 percentage points in the Conciliation arm, a third or more of the 12% control mean. The estimated drop does not change when we compare workers within narrow bins of predicted case value (Section 5.5), suggesting that it is not purely driven by changing case-value composition. The drop also happens in spite of the fact that workers who receive the calculator are no less likely to *talk* to a coyote — they still walk past them at the entrance. Since the information printout says nothing about lawyers, we read this as a revealed-preference response: more informed clients buy less of the low-quality product. The Information arm also raises the share of retained lawyers who graduated from a top-ranked law school by 10 percentage points, while both the Information and Conciliation arms also raise workers' satisfaction with their lawyer by 0.2–0.3 standard deviations. The same two interventions also reduce reported financial distress at two months, with workers 6 to 9 percentage points less likely to have lacked money for food or stopped paying for basic services (Sections 5.6 and 5.7). The Referral arm, on the other hand, does not improve representation. The free Uber ride raises consultations with the public attorney by 20 percentage points, suggesting that the search-and-transport friction it targets is real. But the treatment leaves coyote retention, lawyer satisfaction, and the welfare proxies unchanged. Referral shifts workers away from formal private lawyers and,

perhaps as a result, lowers some measures of the quality of retained lawyers.

This experiment implemented in 2017, together with coauthor Joyce Sadka’s service on the labor legislation drafting committee, subsequently helped shape national policy.⁴ In 2019 Mexico rewrote its federal labor law to make pre-lawsuit conciliation mandatory, with a summons modeled on the letter we randomize here, though the reform differs from our treatment in that conciliation became compulsory and is administered by dedicated state conciliation centers rather than the labor court. The centers opened in three waves staggered across states between November 2020 and October 2022. We use the rollout to ask how mandatory pre-lawsuit conciliation performs at national scale. Using staggered difference-in-differences event studies, we show that the number reaching the courts fell by 54%. Data from nine states with consistent pre- and post- data from conciliation centers shows a 38% increase in the number of workers accessing the formal labor justice system. The conciliation stage, where 55% of cases end in agreement, accounts for this pattern. (Section 6). One qualification applies: because the conciliation centers opened jointly with the new labor tribunals that replaced the old boards, the rollout identifies the effect of the reform package rather than of the conciliation stage in isolation. The fall in litigation mirrors the experimental Conciliation arm — fewer suits, with the difference resolved by settlement before filing. Beyond the similarity with the experiment, the findings provide evidence of the potential effectiveness of mandatory conciliation stages, such as those in France, South Africa, Australia, and Argentina.

Our results contribute to four literatures. First, existing work on courts and access to justice in developing countries documents the economic costs of judicial delay (Ponticelli and Alencar, 2016; Boehm and Oberfield, 2020; Chemin and Aberra, 2020) and evaluates reforms that create parallel-tribunals (Chemin, 2009; Kondylis and Stein, 2018; Mehmood, 2022; Lichand and Soares, 2014; Visaria, 2009; Mattsson and Mobarak, 2023). Where that literature evaluates reforms to the processing of cases already filed, we intervene before a suit exists, and we find that the interventions reduce low-value filings and raise settlement. To our knowledge, ours is the first field experimental estimate of state-facilitated pre-lawsuit conciliation, and the rollout estimates are the first causal evidence on a mandatory conciliation regime operating at national scale⁵.

Second, on credence goods and expert markets, theory shows that low-quality experts can retain market share when clients cannot observe quality (Akerlof, 1970; Leland, 1979; Dulleck and Kerschbamer, 2006; Pesendorfer and Wolinsky, 2003; Egan et al., 2019). Empirical work documents expert misbehavior in real estate (Levitt and Syverson, 2008), vehicle inspection (Hubbard, 1998), and financial advice (Anagol et al., 2017; Egan et al., 2019). We show that a scalable personalized-information policy reduces the market share of low-quality experts by a third, without any disclosure about the experts themselves.

Third, ours is a light-touch information and assistance intervention similar to Bettinger et al. (2012), Kling et al. (2012), and Belot et al. (2019). Closest to us is Finkelstein and Notowidigdo

⁴ See the J-PAL case study at povertyactionlab.org/case-study/information-and-mediation-improve-labor-courts. See also cega.berkeley.edu/feature/labor-reform-mexico.

⁵ The closest randomized experiment of which we are aware is one conducted by the circuit court in the city of Baltimore, Maryland, USA in 2000-2001 (Mandell and Andrea, 2002). The court assigned 400 ongoing workman’s compensation cases to either control or a required 2-hour mediation session. An analysis shows that the mediation increased settlement rates by 13 percentage points.

(2019), who separately identify the effects of information and of information-plus-assistance on SNAP take-up. In their setting, the marginal enrollees drawn in by assistance are healthier and benefit less from treatment than infra-marginal enrollees, worsening targeting; in ours, the marginal filings deterred by assistance are the low-value cases, arguably an improvement in targeting.

Fourth, on bargaining and agency in pre-trial settlement, canonical models attribute bargaining impasse to asymmetric information (Shavell, 1982; Kennan and Wilson, 1993) or to overoptimism (Yildiz, 2003; Babcock and Loewenstein, 1997). Our information arms operate directly on the overoptimism margin, and their contrast shows that correcting the plaintiff's beliefs is more effective when calibrated beliefs are paired with a venue in which to bargain. Relative to (Sadka et al., 2024), which delivers similar information in ongoing lawsuits in the same court, intervening before a lawyer is hired makes the worker's choice of lawyer an experimental outcome. Furthermore, Sadka et al. (2024) does not study conciliation services or referral to public lawyers. Finally, a small theoretical literature studies whether contingency fees screen weak claims out of court (Helland and Tabarrok, 2004) or generate them (Rosenberg and Shavell, 1985; Calandrillo et al., 2024). We argue that the cost structure of high quality private lawyers provides incentives to screen out low-value cases, while the near-zero marginal cost of the coyote business model sustains low-value filings.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting, severance system, and lawyer market. Section 3 describes the experimental design, sample, and data. Section 4 explains the business models of coyotes and formal private lawyers. Section 5 presents the empirical results, and Section 6 analyses the nationwide rollout of the conciliation centers. Section 7 concludes.

2 Context

Severance pay is the basis for protection against the cost of job loss in Mexico, as in most middle-income countries (Gerard and Naritomi, 2019). On paper, Mexican labor law is generous: dismissed workers are entitled to at least three months of salary plus 12 days per year of service in the firm, payable even when dismissal is due to loss of business by the firm.⁶ Enforcement, however, disadvantages workers for three reasons. First, high informality (Kumler et al., 2020) makes it difficult to prove employment relationships or wage levels, since wages are often paid in cash and written contracts are rare. Second, firms often undermine future claims at the time of hiring, for example by requiring workers to sign undated letters of resignation that the firm later dates and presents as evidence of voluntary departure. Third, payments are made directly by firms rather than through a government-administered system, so collection is difficult: firm assets are typically unregistered, courts cannot place liens at the time of ruling, and firms can avoid asset seizure through bankruptcy, transfer, or bribes (Aldeco Leo et al., 2014). Among the 5,000 historical cases we coded for the calculator, 54% of the plaintiffs who won a judgment collected nothing.

The system is governed by Mexico's federal labor law, with most disputes adjudicated in state-level labor courts. We partnered with the Mexico City Labor Court (MCLC), the state court serving

⁶ A dismissed worker may demand three months' salary (Art. 48), plus a seniority premium of twelve days' salary per year of service (Art. 162), and twenty days' salary per year of service if the employer refuses reinstatement (Art. 50).

Mexico City and one of the largest labor courts in Latin America. Over 95% of cases filed at the MCLC are claims for unpaid severance filed by dismissed workers. At the time of our intervention the court received around 35,000 filings a year and resolved around 30,000, so the backlog (more than 100,000 active cases in 2017) was growing.⁷ The settlement rate of 60% is low by comparison to the 80 to 90% in other OECD countries (Ebisui et al., 2016). Cases that settle usually do so within a year of filing. Cases that do not settle typically involve six to eight hearings; the median case decided by judgment takes roughly four years against the statutory 90-day target.

Low-value cases account for much of the caseload. These cases settle at rates 10-15 percentage points lower (Figure OA-2), and are more likely to be handled by coyotes. Interestingly, and consistent with the anecdotal story in footnote 2, cases obtain, on average, 1/5 of what they sued for (OA-4), and only around half of the minimum severance the law mandates. These outcomes reflect both a high probability of losing at trial and challenges in collecting awards even after winning a judgment.

Half of the cases filed have an expected recovery of less than USD 1,250.⁸ In Appendix OA-A we detail calculations showing that for 52% of the cases, the expected value is lower than the legal and court administration costs, and that workers themselves should expect to lose money in 40% of the cases they file. Because neither party pays the costs the court incurs in managing a case, these costs do not enter the filing decision (Shavell, 1982). And because the court is severely congested and processes cases largely on a first-come, first-served, basis, each case imposes a congestion externality on top of the court's operating costs.

Why would such a large fraction of plaintiffs file lawsuits with negative expected returns? Workers are uninformed about the law and overly optimistic about their prospects. 94% of plaintiffs in our sample are first-time users of the court, with little prior exposure to the legal process or to lawyer reputations. Sadka et al. (2024) show that workers filing suits have limited knowledge of the law (only about one-third of plaintiffs correctly identified the 90-day constitutional severance entitlement) and even of their own cases: less than half knew what they were asking for in their own lawsuits. Workers are also strikingly overconfident. Dismissed workers approaching the MCLC before filing reported an average subjective probability of winning a judgment of 89%, while statistical models trained on the 5,000 historical cases predicted 41% for the same cases and only 65% of workers who file recover anything through settlement or judgment. In a prior project covering ongoing cases in the same court, the sum of plaintiffs' and defendants' subjective probabilities of winning exceeded 140% (Sadka et al., 2024).

3 Intervention and Data

We work with a population of dismissed workers approaching the MCLC in search of information about their rights and how to file a lawsuit. Together with the court, we established an information booth at the court's only entrance, operated by research assistants with a law degree.

⁷ Lengthy trial durations are widely documented across many countries. Boehm and Oberfield (2020) document average pending case ages of one to four and a half years across Indian states; Mehmood (2022) finds a 3.4-year average for Pakistani High Court cases; Ponticelli and Alencar (2016) report 2.6 years for Brazilian bankruptcy cases; and Lorizio et al. (2014) report 1,185 days to enforce a contract in Italy.

⁸ This value is expressed in January 2017 USD in real terms. The exchange rate at that time was 1 USD = 21.3853 MXN.

The information booth was announced on a large banner on the wall of the court building (Appendix Figure OA-6). Approaching the booth was optional, and we estimate that around one-third of those coming to the court stopped. The population represented by this sample could differ from the population of workers who file cases in the court, but the experimental design ensures that we have internal validity within this sample.

3.1 Experimental design

All workers approaching the booth received a schematic map of the court and a one-page summary of severance entitlements (Appendix Figure OA-7). They were then allocated to a control group or one of three treatment arms: *Information* (calculator), *Conciliation* (calculator plus appointment letter), or *Referral* (an Uber ride to the Public Attorney’s Office). Randomization was at the day level, with sequential treatment windows that we describe below.⁹ Figure 1 shows the timeline and the randomized sample size by arm.

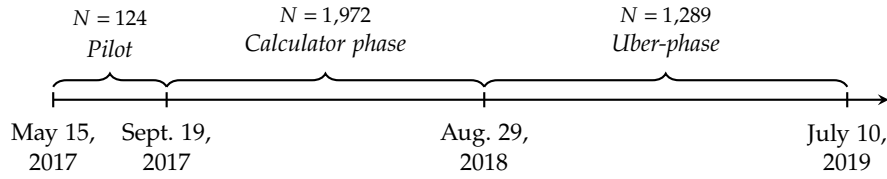
The control plus the three treatment arms were originally designed to run in parallel, and a brief five-arm pilot (Control, Information, Conciliation, Referral, and a combined Information + Referral) ran from May 15 to September 19, 2017 with 124 baseline observations. On September 19, 2017 an earthquake in Mexico City damaged the Public Attorney’s office building. PRODET relocated to the labor court itself for roughly eleven months, during which the Uber treatment was infeasible because there was no separate office to be transported to. We therefore suspended the Uber arms and, ultimately, split the experiment into two sequential treatment windows.¹⁰ In the Calculator phase (September 2017–August 2018, $N = 1,972$ baselines), workers were randomized into Control, Information, or Conciliation. Assignment probabilities were unequal by design: to economize on resources, the Information arm was assigned with lower probability than the control arm, and the Conciliation arm, whose appointments required additional staff, with lower probability still. In the Uber phase (August 2018–July 2019, $N = 1,289$ baselines), after PRODET returned to its own building, workers were randomized into Control or Referral, with Referral + Information not implemented for budgetary considerations. The pre-earthquake pilot sample is excluded from the main analyses so that the treatment arms are consistent.

A key feature of the design is that the Conciliation arm contains the same information component as the Information arm. The contrast between these two arms therefore identifies the effect of the bargaining/conciliation channel holding the information component fixed.

⁹ The experiment was registered in August 2017 (AEARCTR-0002339, <https://www.socialscisceregistry.org/trials/2339>), during the pilot and before the two treatment windows used in the analysis. The registered description combined the public-attorney referral with the calculator in a single arm; from the start of the pilot the two components were randomized as separate arms (Figure 1). The registration also planned a six-month recontact; we instead surveyed workers at two weeks and two months, the latter after the statute of limitations for filing had expired. A natural concern with day-level randomization is that third parties at the courthouse, particularly the *coyote* lawyers described in Section 4, might adapt their effort across treatment and control days, generating a SUTVA violation. Treatment-day assignment was not announced to the court or to lawyers, and the booth operated continuously throughout the experimental window. We show in Section 5.5 that workers in the Information and control arms report statistically identical contact with coyotes, suggesting that coyotes did not reallocate recruitment effort across days in response to treatment.

¹⁰ All treatment effects are estimated within window: the Information and Conciliation arms are compared to their contemporaneous control group, and the Referral arm is compared to its contemporaneous control group. No across-window comparisons are made. Table OA-4 compares the two groups and shows they are very similar.

Figure 1: Experimental timeline.



Notes: The booth operated from May 2017 to July 2019 in three sequential windows. The *Pilot* (May–September 2017) randomized into five arms (18 Control, 28 Information, 26 Conciliation, 25 Referral, and 27 Information+referral) with very few observations per cell and is not used in the main analyses. The *Calculator phase* (September 2017–August 2018) randomized at the day level into Control (817), Information (713), and Conciliation (442). The *Uber-phase* (August 2018–July 2019), randomized at the day level into Control (705) and Referral (584). Sample sizes are baseline counts.

Control. The control group received only the basic court map and the severance-entitlements summary (Figure OA-7), identical in both experimental windows. We provided a version of the basic information to workers in all three treatment arms as well.

The Information treatment (Calculator). In addition to an information brochure containing a more detailed explanation of legal rights and the lawsuit process (Appendix Figure OA-8), we provided workers with a personalized statistical prediction of their case outcome. Predictions were derived from machine-learning models trained on the 5,000 historical casefiles described in Sadka et al. (2024). Because participants had not yet filed a case, we used a simplified set of inputs (daily wage, tenure, gender, and industry) collected on the spot, rather than the richer casefile-based inputs used in Sadka et al. (2024). Appendix Figure OA-9 shows the printout each worker received. The printout displays: (a) the worker’s basic employment data, so the worker can verify the prediction is based on the correct inputs; (b) the 25th and 75th percentiles of settlement amounts (in days of salary) among similar historical cases, with text framing them as the range in which the majority of settlements fall; (c) the share of cases that remain unresolved four years after filing; and (d) the estimated probability that if the worker continued a lawsuit against the employer until a court judgment, she will recover nothing. The calculator printout contains no advice on which lawyer to hire and no warning about coyotes. We administered this treatment on random days during the Calculator phase window (September 2017–August 2018).¹¹

The Conciliation treatment (Calculator + Letter). The second treatment provided the calculator and rights material in the Information arm, plus a court-issued letter on the court’s letterhead, signed by the Secretary General of Individual Affairs, inviting the worker’s employer to a pre-lawsuit conciliation appointment at the court. The letter cited Articles 685 and 987 of the Federal Labor Law, which govern the conciliation procedure before the Labor Court, specified a date and time roughly two weeks ahead, identified the location, and informed the employer that the worker had received statistical information about case outcomes and that the same information would be made available to the employer at the appointment. The worker hand-delivered the letter to the employer, and our team called both parties in the intervening days to confirm receipt and attendance. When both parties appeared, our staff provided the calculator information to the firm’s representative and then left the parties with a court-employed conciliator to negotiate in private.

¹¹A similar informational intervention, delivered at hearings of *ongoing* lawsuits in Sadka et al. (2024), increased settlement by 15 percentage points when the worker was present to receive it. The sample population and the exact display of the information differ.

Appendix Figure OA-10 reproduces a sample letter with an authors' translation in the figure notes.

Pre-filing conciliation was *not* legally mandatory, so the employer faced no statutory penalty for failing to appear. However, we estimate, based on confirmation calls, that 56% of treated workers hand-delivered the letter to the employer, and 48% of employers appeared at the scheduled appointment. We view the letter as effective for two reasons. First, the formality of the letter on court letterhead with the Secretary General's signature gave it weight. Second, a worker who arrives at the firm carrying a court-issued appointment letter signals that she is well informed about her rights and prepared to litigate, which raises the employer's perceived probability of a costly lawsuit and thus the value of an early settlement. The Conciliation arm provides experimental evidence on the effect of pre-trial conciliation, a mandatory feature of labor law in several countries which has previously been evaluated only descriptively.

Uber: the Referral treatment. The third treatment provided workers with a pre-paid Uber ride to the Public Attorney's office and an accompanying flyer with the address and arrival instructions (Appendix Figure OA-11). The mean cost of the ride was MXN 40 (USD \$2). We did not include the calculator printout in this arm because public lawyers provide a similar assessment when meeting with potential clients in person, and our goal was to isolate the search-and-transport friction. Take-up, measured as actually riding to the Public Attorney's office on the same day, was 66%. An additional 16 percent of the sample reported in the two-week survey that they had subsequently talked with a public lawyer, bringing the broader take-up rate to 82%.¹² We administered this treatment on random days during the Uber-phase window (August 2018 through July 2019), after PRODET returned to its own building.

3.2 Data

Survey data. We conducted three surveys. The *baseline* survey was administered in person at the booth by research assistants before randomization. We recorded gender, daily wage, tenure, industry, date of dismissal, contact information, and the worker's subjective probability of winning a judgment and expected amount conditional on winning. The two expectations questions used a two-part format: we first asked workers the point estimate directly, and then asked those who reported they did not know whether the value was above or below a benchmark (75% for the probability of winning and half a year of salary for the expected amount) chosen as the approximate average from pilot surveys. For workers receiving the calculator (Information and Conciliation treatments), we re-elicited expectations a few minutes after providing the treatment. This allows us to measure immediate expectation updating. We did not re-elicite expectations for the control group because we found in piloting that respondents answered with the same number and were irritated by being re-asked identical questions almost immediately.

We conducted a *two-week follow-up* by telephone to record whether the worker had consulted with any lawyer and, if so, with which type (public attorney, private formal, or coyote); whether the worker had settled with the employer, dropped the case, or filed suit; and an updated subjective probability of winning and expected amount conditional on winning. The instrument is

¹²Most of the 34% who declined the ride on the day said either that they had their own transportation or that they did not have all of their employment documents with them.

intentionally short, with the more detailed questions deferred to the two-month survey.

Our *two-month follow-up* survey, also conducted by telephone, is timed to fall after expiration of the 60-day statute of limitations for filing a lawsuit.¹³ We surveyed four groups of outcomes. First, the final status of the case—settled, filed, or dropped—together with the amount and date of any settlement. Second, we asked the type of lawyer retained and how the lawyer was found, following the same categories as in the two-week survey. Third, we asked how satisfied the worker was with their lawyer on a 1–10 scale, and whether the worker would keep her lawyer if she could change, as yes/no/don’t know¹⁴. Fourth, we collected basic welfare proxies: ability to pay basic services, ability to spend on food, life satisfaction on a 1–10 scale, and current employment status. Response rates in the Calculator (Uber) phase were 82% (90%) at two weeks and 79% (84%) at two months; attrition and balance are discussed in Section 3 and reported in Appendix Tables OA-2 and OA-3. The full questionnaires are reproduced in Appendix Sections H.1 and H.2. The main analysis is conducted on the 2-week and 2-month surveys separately, but Appendix OA-F presents results using the balanced panel.

Administrative data. We complement the survey data with three administrative sources obtained through our agreement with the MCLC. First, the court provided us, in electronic form, the universe of casefiles for unjustified-dismissal suits filed during the experimental window (May 2017–July 2019). Each casefile records the plaintiff and defendant, the date of filing, the amount and items claimed, basic worker characteristics (daily wage, tenure, weekly hours, industry), and the names of the (typically three) lawyers granted power of attorney and, for completed cases, the amount recovered. We match these data to the experimental sample by plaintiff and employer name. We use the matched files in defining the *Sued* indicator. A worker is coded as having filed if she reports a lawsuit in either follow-up survey or if a case filed within the statutory window in her name appears in the court’s records. This allows us to code workers who did not respond to the surveys. Because a worker may file and later settle in court, *Sued* and *Settled* are not mutually exclusive. We also extract the lawyer names from the matched casefiles and use these for the lawyer classification we describe in Section 4. Second, we use the 5,000 historical casefiles filed in 2011 and concluded by 2015, originally digitized for Sadka et al. (2024), to train the random-forest model that produces the predicted case value, $\hat{x}_i$, used in Sections 4 and 5.4: the model’s prediction of the worker’s recovery given daily wage, tenure, gender, and industry, and the recovered amount.¹⁵ Third, we match each lawyer name on an experimental casefile to the federal registry of professional licenses (Cedulas Profesionales) to recover the law school of graduation. We use this to construct a QS-school quality proxy described in Section 4 and Table 5.

¹³The Federal Labor Law establishes that the worker has 2 months from the date of dismissal to initiate a labor lawsuit.

¹⁴The registration listed as an additional primary outcome the quality of the lawsuit as rated by a team of labor lawyers blinded to treatment and lawyer type. We made several attempts to rate files, hired different lawyers and using different casefiles, but inter-rater correlations were too low to use. We therefore rely on the three proxies above.

¹⁵The model takes daily wage, tenure, gender, and industry (not lawyer type) as inputs and predicts the amount actually recovered (not the amount awarded by the judge). Sadka et al. (2024) reports the full model and its goodness-of-fit measures. Their predictive value is reasonably high in part because the law established wage and tenure as part of the formula of severance owed (see footnote 6).

3.3 Analysis sample, attrition, and balance.

Most of the outcomes of interest use data from the two-month follow-up survey, though the data on which lawyers participants had *talked to* comes from the two-week survey. In the paper, we use the sample that is the intersection of the baseline and two-week survey for the lawyer contact results and the intersection of the baseline and the two-month sample for the other results. In the appendix, we show results for the sample that is the intersection of the baseline, two-week, and two-month surveys. The results are in all cases nearly identical, though in a few cases the smaller baseline, two-week, and two-month survey sample leads to a decrease in statistical significance. Baseline counts by arm are 817 Control, 713 Information, and 442 Conciliation in the Calculator phase, and 705 Control and 584 Referral in the Uber-phase (Figure 1). Of these, 642 (Control), 579 (Information), and 362 (Conciliation) are in the two-month survey used in the Calculator-phase regressions, and 595 (Control) and 505 (Referral) in the two-month survey use in the Uber-phase regressions.

Appendix Table OA-2 reports attrition (retention) in the two-week, two-month and intersection samples. Table OA-3 reports balance across treatment arms and survey completion in each phase. The control-group dependent-variable means (bottom row of each panel) characterize the typical worker arriving at the booth: roughly half are female, with about three and a half years of tenure at the dismissing firm, a daily wage near 330 MXN (around USD 16 at the time), and a baseline subjective probability of winning a judgment of about 0.88. About half of workers report no point estimate for the expected award conditional on winning (0.58 in the Calculator phase, 0.48 in the Uber-phase). Control-group means are otherwise similar across the two phases. Table OA-2 shows that response rates do not differ by treatment arm, and Table OA-3 shows that baseline covariates are balanced across arms within each phase.

4 Lawyer types and business models

Workers have 60 days from the date of dismissal to file a lawsuit. Mexican law requires that workers be represented: only lawyers can file a lawsuit, and workers have to be represented at all hearings. Lawyers typically require the worker to sign a power of attorney, so that the worker need not attend hearings. This gives the lawyer effective control over the conduct of the lawsuit, including the ability to settle or drop it without the worker present. This is problematic for at least two reasons. First, lawyer quality is widely viewed as low, and there is no source of systematic information about it. According to experts, two things combine to generate this: Mexico has more than 2,100 schools actively offering law degrees — roughly ten times the number of ABA-accredited law schools in the (much larger) United States (CEEAD, 2022) — but has no national bar examination.¹⁶ Second, lawyers operate under a significant informational “expert” advantage over their plaintiff clients; Sadka et al. (2024) document evidence of the resulting moral hazard in this court.

¹⁶One of Mexico’s premier experts, Fix-Fierro (2020), argues that “the unregulated expansion of legal studies makes it impossible to expect uniform levels of quality, so that surely the majority of graduates do not perform good legal work”. The federal government’s 2015 *Diálogos por la Justicia Cotidiana* concluded that the poor quality of legal services is a recurring problem with consequences for citizens’ assets and legal standing” (Gobierno de México, 2015).

4.1 Types of lawyers

We distinguish three types of lawyers.¹⁷ The first is public lawyers employed by the Procuraduría de la Defensa del Trabajo del Distrito Federal (PRODET) in Mexico City. As in many other countries, Mexico offers free legal representation as a way to help low-income populations access justice. Public lawyers do not charge for their services, but they are overburdened, with each of them handling roughly 127 case files concurrently—almost three times the typical private attorney caseload. As a result, public attorneys devote limited time to each case. Moreover, because public lawyers are paid a modest flat wage, PRODET does not attract lawyers from the highest-quality law schools. PRODET is about one mile from the court (6–8 minutes by car), so accessing it requires a separate trip; many workers are unaware that free representation is available nearby.

We are able to cleanly identify public lawyers from court documents, using the presence of PRODET letterheads in the casefile and lawyer names. We use lawyer names because occasionally, coyote lawyers tell their clients they are public attorneys, and they may use the PRODET letterhead in filings. For public attorney cases, the second and / or third lawyers will be one of a small number of PRODET supervisors. The three lawyer names taken together therefore allows us to cleanly identify the cases filed by public attorneys.

Having identified the public attorneys, we then separate private lawyers into two groups based on the business model they pursue. The first is a high-volume, low-touch model pursued by the coyote lawyers. Coyotes recruit clients on the street outside the court. They are widely seen by court authorities as problematic because they take advantage of workers' naivete and convince them to pursue cases that workers would be better served to settle without suing. Appendix OA-C shows several examples of material from labor courts warning workers away from coyote lawyers.¹⁸

We compile an initial list of coyote lawyers from survey responses. We ask each worker in our sample filing a suit how and where they found their lawyer. We tag as a coyote any lawyer reported as having been found “just outside the court.” We then match the respondents to their casefiles using the court's administrative records. Each casefile typically lists three lawyers, and we tag each of these lawyers as a coyote. Lawyers who are reported in the survey to be found “through networks” are initially classified as formal private lawyers. However, a plaintiff will sometimes find a coyote lawyer through a referral from a family member or friend. Indeed, we find some lawyers who are classified as coyote (found at the court) in some cases and formal private (found through networks) in other cases. We classify these lawyers as coyotes in all of their cases.

We validated the list of coyote lawyers compiled from the surveys and casefiles by giving the list, aggregated with a small sample of non-coyote lawyers, to two senior administrative officers of the conciliation center. We asked them to independently classify each name as either a coyote or not. The two reviewers' classifications agreed substantially with our initial list: they added 4

¹⁷The exact procedure we use to process the data is described in Appendix OA-B. The appendix shows that we pull out public lawyers as the third step. We re-order the steps in the discussion here only to simplify the exposition.

¹⁸For example, the federal labor authority characterizes *coyotaje* as “an act of illegal intermediation, in which an outside person interferes between citizens and public servants. . . They deceive you and charge you for a procedure that is free. . . They advise you not to settle and to go to trial, with the promise of obtaining greater financial resources.” (Authors' translation. The full statement and source are reproduced in Appendix OA-C.) Centro Federal de Conciliación y Registro Laboral, <https://www.facebook.com/centrofcrl/videos/1117632750035898/>.

names previously missed and removed 9 names that the network had tagged but that they did not regard as coyotes. These steps results in a final coyote list. Throughout the paper, when we refer to “coyotes” we mean lawyers on this validated list. We use this list in a final step: Some workers tell us in the 2-month survey that they have not sued, but we find a case with their name and matching characteristics in the court records. For these workers, we classify the lawyer as public, coyote or formal private using the lawyer names and the coyote lawyer list. Coyotes are relatively few in number but handle around 15% of the cases in the MCLC.

Formal private lawyers are by far the most numerous group at the MCLC and handle around 75% of filed cases. They work from offices away from the court, often in partnership with other lawyers. Misclassification in the coyote list would attenuate the differences we estimate between the two types. The fact that we nonetheless find large differences in caseloads—a mean of 288 cases per coyote versus 46 per formal private lawyer, with medians of 120 and 20, and in the quality measures we describe in the next subsection, indicates that the classification captures a real distinction.¹⁹

Both coyotes and formal private lawyers typically charge a fixed fee (as much as 2,000 MXN, about \$100 USD) for filing a case and take a contingency fee of 30 - 40%. The initial filings (*escrito inicial de demanda*) for unjustified dismissal are short, stylized documents of three to four pages, detailing the identity of the parties, a numbered list of statutory claims (*indemnización constitucional, salarios caídos, prima de antigüedad*, accrued vacation and bonuses), a brief narrative of facts, and citations to the relevant articles of the Ley Federal del Trabajo.

4.2 Characteristics of *coyote* and formal lawyers

Before formally modeling incentives of coyote and formal lawyers, we provide data on three descriptive measures of lawyer quality for each of the three lawyer types. We use data only from our control groups in constructing the measures we report. The descriptive evidence is consistent with the view that coyotes deliver lower-quality representation than formal private lawyers.

We first compare the law schools attended by each of the three lawyer types. We identify the law school attended by each lawyer in a casefile using Mexico’s college graduate registry (Cedulas Profesionales) and proxy quality by graduation from a school listed in the QS World University Rankings for Law and Legal Studies.²⁰ Among 948 casefile/lawyer pairs, 31% of the 224 coyote pairs attended a QS-listed school, compared with 49% (149 of 303) of formal private lawyers and 33% (139 of 421) of public lawyers. The 18-percentage-point coyote-vs.-formal gap is substantial and highly statistically significant, while there is almost no gap between coyotes and public lawyers.

Next, we measure workers’ satisfaction with the lawyer they have chosen. In the two-month follow-up, we ask workers two questions that provide a measure of satisfaction with their lawyer. The first asked whether the worker wanted to change their lawyer, allowing responses “yes”, “no”, and “don’t know.” 33.6% of control-group workers represented by coyotes report wanting to change their lawyer, compared with 16.3% for formal private lawyers ($p < 0.01$) and 17.5% for public lawyers. One in three coyote clients thus already regrets the choice within two months of dismissal, when

¹⁹Shares and caseloads come from matching lawyer names in the 5,000 historical casefiles to the coyote list compiled for this experiment. These data are descriptive only and we do not use them in any estimation.

²⁰Five Mexican law schools appear in the 2025 QS rankings, with UNAM (the country’s largest) and ITAM in the top 100. Results are similar using a top-100 cutoff or UNAM alone.

the case is at most in its initial stages. We also asked workers, on a 1–10 scale, how satisfied they were with their lawyer. Coyote clients in the control group rated their lawyer 0.62 points below clients of formal-private lawyers ($p < 0.01$) and 0.24 points below clients of public-lawyers.

On all three measures—credentials, retention intent, and satisfaction—coyotes rank below formal private lawyers, with public lawyers between the two. Table 1 summarizes the three measures side by side. We analyze the effect of treatment on these measures in Section 5.6.

Table 1: Descriptive evidence on lawyer quality

	QS share (%)	Wants to change lawyer (%)			Satisfaction (1–10)
		Yes	No	Don't know	
<i>Coyote</i>	30.8	33.6	45.1	21.2	8.12
Formal private	49.2	16.3	62.6	21.1	8.74
Public	33.0	17.5	30.5	51.9	8.36
<i>p</i> -value: <i>Coyote</i> = Formal private	<0.001	<0.001	<0.001	0.053	0.003

Notes: All statistics are computed in the control group. *QS share* is the share of lawyers who graduated from a law school listed in the 2025 QS World University Rankings for Law and Legal Studies, based on court records (948 case-lawyer pairs: 224 coyote, 303 formal private, and 421 public). *Wants to change lawyer* reports responses to “If you could change lawyers at this moment, would you do it?” at the two-month follow-up (494 respondents: 113 coyote, 227 formal private, and 154 public); rows sum to 100%. *Satisfaction* is the mean rating on a 1–10 scale, where 10 indicates perfect satisfaction, among the same respondents. The reported *p*-value tests the null hypothesis that shares are equal for *coyote* and formal-private lawyers; standard errors are clustered by case. Lawyer types are defined in Section 4.

Coyotes disproportionately serve low-value cases. Among control-group workers who sue, those in the top quartile of predicted case value are 24 percentage points less likely to use a coyote than those in the bottom quartile (Table OA-5, column 6). Using our calculator projections and data from the historical casefiles, the mean (median) case value for coyote-filed cases is 1,845 (1,019) USD compared with 2,524 (1,371) USD for formal private lawyers.

4.3 Coyote and formal lawyer business models

The lower quality and lower-value case filings help explain why courts want to reduce the use of coyotes. The descriptive facts alone, however, admit two very different interpretations. Under the first, the pattern reflects efficient specialization: coyotes’ low-cost technology gives them a comparative advantage in low-value cases that formal lawyers cannot profitably serve, and without them small claims would go unrepresented or represented by public lawyers. Under the second, part of the coyote caseload consists of cases that would not be filed by an informed worker: coyotes’ business model profits from filing low-value cases regardless of merit, and misinformed workers retain them to pursue claims they would be better off settling or dropping. The two interpretations carry opposite policy implications, and they differ in a testable way: if workers’ choices already reflect calibrated beliefs, providing information should change neither filing nor the choice of lawyer. We write down a simple framework that nests both interpretations. We focus on private lawyers because they file 90% of the cases. The framework shows how the two business models coexist in equilibrium and which cases each type accepts, and then examines incentives for settlement, where we show that private lawyers in our context have an incentive to settle *less* than their clients would prefer, reversing the standard agency prediction.

Two features distinguish coyotes from formal private lawyers in our data: each coyote manages six times as many active cases as each formal private lawyer, and coyotes acquire their clients at the

courthouse itself rather than through offices or referrals. We interpret these features as reflecting a choice between two business models — a high-volume, low-cost model and a lower-volume, higher-cost one — that differ most importantly in the marginal cost of taking on an additional client.

Lawyers bear two costs in taking on a client. The first is the fixed cost of preparing and filing the case: gathering the facts and drafting the initial filing. The second is attending hearings, which are typically about six months apart over a period of four years or longer for cases that go to judgment. The two business models economize on these costs differently. Coyotes use boilerplate filings — three to four stylized pages — which keeps the fixed cost low, and they hire staff to maintain a permanent presence at the courthouse, which jointly produces client acquisition and court attendance and makes the cost of attending an additional hearing very low. Formal private lawyers draft longer, customized filings and keep no standing presence at the court, so each hearing requires a dedicated trip. We capture these differences in reduced form as lower fixed and marginal costs for coyotes.

Lawyers. Consider a case i with case value V_i , defined as the expected amount collected conditional on a positive recovery, handled by private lawyer type $j \in \{C, F\}$, where C denotes a coyote and F a formal private lawyer. The probability of recovering a positive amount is p_{ij} .²¹ Expected recovery is therefore $x_{ij} = p_{ij}V_i$. Consistent with actual practice, both types of private lawyers are paid under the same contract: an upfront fee b and a contingency share α of the amount recovered. Each case requires an expected number h_i of hearings. Lawyer type j incurs a fixed case cost f_j and a marginal per-hearing cost c_j . Consistent with the two technologies described above, and with formal lawyers' higher opportunity cost of time, we assume $f_C \leq f_F$ and $c_C < c_F$. Formal lawyers are also higher quality in the sense that they generate weakly higher probabilities of recovery: $p_{iF} \geq p_{iC}$. Expected profit from case i for lawyer type j is $\pi_{ij} = b + \alpha x_{ij} - f_j - h_i c_j$. Thus lawyer type j accepts case i if and only if

$$x_{ij} \geq \bar{x}_j(h_i) \equiv \max \left\{ 0, \frac{f_j + h_i c_j - b}{\alpha} \right\}.$$

We assume the upfront fee does not cover the costs of pursuing a case, $b < f_j + h_i c_j$ for both lawyer types, so that both acceptance cutoffs are strictly positive.

Prediction 1: Coyotes have lower acceptance cutoffs. Because coyotes have lower fixed and per-hearing costs, their acceptance cutoff lies strictly below that of formal lawyers: $\bar{x}_C(h_i) < \bar{x}_F(h_i)$ for every hearing burden h_i . Two implications follow. Coyotes will accept cases with expected recovery between below $\bar{x}_C(h_i)$ and below $\bar{x}_F(h_i)$, but formal lawyers will not; and neither coyotes nor formal lawyers will take cases with expected recovery below $\bar{x}_C(h_i)$. Because the same case generates different expected recoveries with different lawyer types ($p_{iF} \geq p_{iC}$), we next translate these cutoffs into case values.

To map expected-recovery cutoffs into case-value cutoffs, we hold the expected hearing burden fixed at h and (to simplify notation) suppress the i subscript on the recovery probabilities. Writing

²¹We interpret p_{ij} as the probability that the worker collects a positive amount — not merely the probability of winning a judgment. Among the historical casefiles, 54% of winning plaintiffs collected nothing (Section 2). V_i is a characteristic of the case rather than of the lawyer: all differences in lawyer quality operate through p_{ij} in this stylized framework.

$\bar{x}_j \equiv \bar{x}_j(h)$ and expected recovery as $x_j(V) = p_j V$, with $p_F \geq p_C$. Let V_C and V_F denote the case-value thresholds at which coyotes and formal lawyers are just willing to accept a case: $V_C = (f_C + hc_C - b)/(\alpha p_C)$ and $V_F = (f_F + hc_F - b)/(\alpha p_F)$. Although formal lawyers are higher quality, the coyote cutoff in case-value space is lower whenever the coyote cost advantage dominates the formal-lawyer quality advantage. That is, $V_C < V_F$ if and only if $(f_C + hc_C - b)/(f_F + hc_F - b) < p_C/p_F$. Assuming both the fixed costs and probabilities of winning are independent of the case value, the situation maps to the informality literature (see, for example, [Ulyssea \(2018\)](#), Figure 6) and yields four distinct ranges: a value below which the case is profitable for neither informal nor formal lawyers; a threshold V_C marking a range of values where only coyotes find the case profitable; a threshold V_F marking a range above which either lawyer finds the case profitable, but where the cost advantage of the coyotes dominates; and a final threshold above which a formal lawyer earns more from the case than a coyote does, because the quality advantage grows with case value while the cost advantage does not.²² The evidence described in this section is consistent with these conditions holding here. The low coyote marginal cost weakens the force that, in standard contingency-fee models, screens the lowest value cases out of court ([Helland and Tabarrok, 2004](#)).

Worker participation and sorting. A lawyer agreeing to take a case is necessary but not sufficient. The worker must also want to pursue the case. Consistent with evidence we present section 2 on worker over-optimism, we allow workers to have different subjective probabilities of winning than lawyers. We use tildes denote the worker's subjective beliefs: $\tilde{p}_{ij}$ is her perceived probability of recovering a positive amount with a lawyer of type j , and $\tilde{V}_i$ is her perceived case value. These may differ from the true p_{ij} and V_i . The worker will only pursue the case if her perceived gain covers the costs she bears,

$$(1 - \alpha)\tilde{p}_{ij}\tilde{V}_i \geq b + \tilde{\kappa}_i,$$

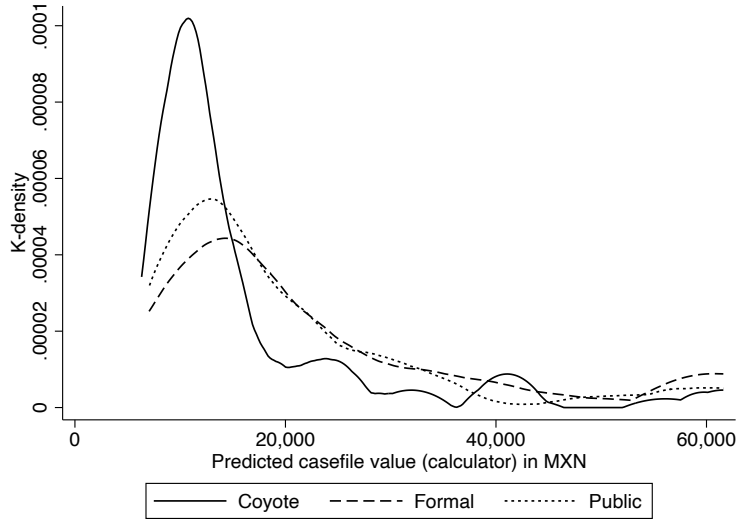
where $\tilde{\kappa}_i$ is her perceived non-fee cost of litigating — the time cost of hearings spread over years, and the liquidity and risk costs of waiting for an uncertain payment. Anything that lowers $\tilde{p}_{ij}$ or $\tilde{V}_i$ removes lower-value cases from the filing pool. Consistent with what we observe in the field, the client pays the same contract to either private lawyer type, so her perceived net payoff from hiring lawyer j is $U_{ij} = (1 - \alpha)\tilde{p}_{ij}\tilde{V}_i - b - \tilde{\kappa}_i$. When both lawyer types are willing to accept her case, a client who perceives that formal lawyers are at least as good ($\tilde{p}_{iF} \geq \tilde{p}_{iC}$) chooses the formal lawyer; a client unable to rank the two types can be thought of as choosing at random. Either behavior delivers the sorting prediction below, because coyotes receive all of the cases that only they accept and at most a share of the cases formal lawyers also accept.

Prediction 2: Coyotes handle the smaller cases in equilibrium. Let $G(V)$ be the distribution of case values that clear the worker's participation constraint. The sorting above splits cases across lawyers by value. When workers are fully knowledgeable about lawyer types, the mass of cases handled by coyotes is $M_C = G(V_F) - G(V_C)$, and the mass handled by formal private lawyers is $M_F = 1 - G(V_F)$.²³ The model therefore predicts that the distribution of case value among coyote-filed cases lies to the left of the distribution among formal-private cases. Figure 2 shows the distribution of predicted

²²Comparing profits on the same case, $\pi_{iC} - \pi_{iF} = \alpha(p_C - p_F)V_i + (f_F - f_C) + h(c_F - c_C)$, which falls with V_i and changes sign at $V^* = [(f_F - f_C) + h(c_F - c_C)]/[\alpha(p_F - p_C)]$. Nothing below depends on V^* .

²³We do not observe V_i , so we use the calculator's prediction $\hat{x}_i$ (Section 3) as the expected recovery $x_{ij} = p_{ij}V_i$. Throughout, "case value" refers to V_i , "predicted case value" to $\hat{x}_i$, and $\tilde{V}_i$ denotes the worker's belief about her case value.

Figure 2: Distribution of predicted case value, by type of lawyer hired (control group).



Notes: Kernel density of the predicted case value $\hat{x}_i$ among control-group workers who file a lawsuit, separately by the type of lawyer they hire: coyote, public, or formal private lawyer. Predicted case values are obtained from a random forest model trained on 5,000 completed casefiles using the worker's daily wage, tenure, gender, and industry as predictors; see [Sadka et al. \(2024\)](#) for details. Predictions are adjusted for inflation so that all values are expressed in January 2017 MXN. To limit the influence of extreme upper-tail predictions, predicted case values are winsorized at the 95th percentile of the upper tail before plotting the densities. Lawyer-type classifications follow the procedure described in Section 4 and Appendix OA-B.

case value by lawyer type among cases filed by workers in the control groups of our experiment. We see that the distribution of predicted case value is shifted sharply to the left for cases filed with coyotes, with the distribution of cases filed by formal private lawyers lying to the right. Public lawyers lie in between, but appear to filter out most of the low-value cases.

The high volume model used by coyotes is self-reinforcing. The primitive cost differences make coyotes willing to accept more low-value cases, and the volume then feeds back into costs. The probability $q(n_j)$ that a lawyer must make a dedicated court trip for an additional hearing falls with her active caseload n_j , because a high-volume lawyer is likely to be at court already. Formal lawyers collectively handle the majority of cases, but there are far fewer coyotes, so per-lawyer caseloads satisfy $n_C > n_F$, and at the caseloads of the largest coyotes (several hundred cases, see Figure OA-5) $q(n_j)$ is likely near zero, so an additional hearing requires no dedicated trip. Moreover, as discussed above, coyotes find clients at the court and hence have a presence there every day.

Beliefs, filing, and coyote market share. The efficient-sorting interpretation discussed above corresponds to the model with full information and correct beliefs: coyotes' low-cost technology leads them to take the low-value cases that formal lawyers cannot profitably take and no worker is mistaken about what her case is worth or whom to hire. In that world, providing information changes nothing.²⁴ Section 2 documents instead that workers arrive overconfident about winning and often unable to state a value for their case at all, and that they have no easy way to assess lawyer quality. The authorities' warnings reproduced in Appendix OA-C describe lawyers inflating both the probability of winning and the value of the case to persuade workers to litigate and to hire them. Under these conditions the framework yields two comparative statics. The first operates through the participation constraint: information that lowers $\tilde{p}_{ij}$ or $\tilde{V}_i$ removes cases from the left

²⁴Filing may still be socially excessive under correct beliefs, because neither party pays the court's costs.

tail of the distribution — the composition prediction we test in Section 5.4. Its premise, that the calculator moves workers’ beliefs about their own case, is documented directly in Section 5.2. The second operates through lawyer choice. Uninformed workers may hold distorted beliefs about the relative ability of coyote and formal private lawyers, and the coyotes’ sales pitch described above is a natural source of distortion. To the extent that an independent benchmark makes inflated promises harder to sustain and lowers $\tilde{p}_{iC}$ for these workers, they shift toward formal lawyers conditional on filing. (We do not measure workers’ beliefs about lawyer types, so the premise of this second channel is assumed rather than documented.) The reduction in coyote retention within narrow bins of predicted case value (Section 5.5) is consistent with this channel.

4.4 Incentives for settlement

The framework above describes which cases each private lawyer type files. It does not, by itself, explain whether cases are settled too often or too rarely from the worker’s perspective. We now add a reduced-form settlement decision to capture a simple agency wedge: the worker and lawyer may value delayed and risky trial awards differently and so disagree over whether to accept a settlement.

Two benchmark results frame the settlement decision. First, under contingency contracts the lawyer bears the cost of continuing the case but receives only a share of the award, so the standard prediction is that lawyers settle earlier and more often than their clients would prefer (Helland and Tabarrok, 2004; Calandrillo et al., 2024). Second, because neither litigant pays the costs the court incurs, private settlement decisions ignore the resources that settlement would save, and settlement rates can be lower than is socially optimal even when they are privately optimal (Shavell, 1982). Our context reverses the first result and reinforces the second. Coyotes’ marginal cost of continuation is very low due to their high-volume business model, removing the force that pushes contingency lawyers toward early settlement. Moreover, because lawyers are plausibly more diversified (have more cases) and less liquidity constrained than workers (the latter have just lost their jobs), lawyers are more willing to wait for a trial.

Suppose the defendant offers total settlement S today. (We do not model how S is determined.) If the case continues to trial, the worker recovers a random amount whose expected value is $x_{ij} = p_{ij}V_i$, as above. Consistent with what we observe in the field, the worker receives share $1 - \alpha$ of either settlement or judgment, while the lawyer receives share α . Let δ_W and δ_L denote the worker’s and the lawyer’s discount factors, with $\delta_L > \delta_W$,²⁵ and let $\lambda_W > 0$ denote the worker’s liquidity and risk cost from waiting for trial, normalizing the lawyer’s risk cost to zero. Finally, let K_j denote lawyer type j ’s private cost of continuing the case. By the court-presence channel above, $K_C < K_F$: a coyote’s future hearings rarely require dedicated trips, while a formal lawyer’s do.

Under these assumptions, lawyers may prefer continuation when workers prefer settlement. The worker prefers settlement whenever $(1 - \alpha)S \geq \delta_W(1 - \alpha)x_{ij} - \lambda_W$: she would accept any offer satisfying $S \geq S_W^* \equiv \delta_W x_{ij} - \lambda_W / (1 - \alpha)$. The lawyer prefers continuation whenever $\alpha S \leq \delta_L \alpha x_{ij} - K_j$, or equivalently whenever $S \leq S_{Lj}^* \equiv \delta_L x_{ij} - K_j / \alpha$. There is therefore a range of offers that the worker would accept but the lawyer would reject whenever $S_W^* < S_{Lj}^*$, or equivalently whenever

²⁵Falk et al. (2018) show that discount rates decrease with both education and household income in a sample of 68 countries globally (see Appendix F); Bartoš et al. (2021) show discount rates increase with poverty. Carvalho (2010) estimates an annual discount factor of 78% for low-income households in Mexico, well above credit card interest rates of 30-40%.

$(\delta_L - \delta_W)x_{ij} + \lambda_W/(1 - \alpha) > K_j/\alpha$. In practice the lawyer’s view typically prevails in this disagreement: he holds a broad power of attorney, and his informational advantage means the worker generally follows his advice.²⁶ The condition is more likely to hold when workers are impatient or liquidity constrained, when lawyers are patient and diversified, and when the lawyer’s continuation cost is low. The wedge applies to both private lawyer types, but it is especially relevant for coyotes, whose continuation costs are lower and whose portfolios are larger: coyotes can more easily play the lottery of waiting for a possible judgment rather than accepting a certain settlement today, even when the worker would prefer immediate payment.²⁷

The framework maps directly into the design of our experiment. The Information treatment operates on beliefs. Through the participation constraint, lowering $\tilde{p}_{ij}$ or $\tilde{V}_i$ should remove low-value filings, and by making workers better able to discern lawyer quality, Information should shift retention toward formal private lawyers. The Conciliation treatment operates on the settlement wedge: by convening the worker and the employer before any lawyer controls the case, it allows offers above S_W^* to be accepted regardless of S_{Lj}^* .²⁸

5 Results

5.1 Empirical Strategy

We report intent-to-treat effects from ordinary-least-squares regressions of the form

$$Y_{id} = \alpha + \beta' T_d + X_i' \gamma + \varepsilon_{id}, \quad (1)$$

where Y_{id} is the outcome for worker i treated on day d , T_d is a vector of indicators for treatment assignment, X_i is a vector of baseline controls—gender, daily wage, and tenure with the firm, collectively “basic variable controls,” or “BVC”—and ε_{id} is an idiosyncratic error. Because randomization was conducted at the day level, we cluster standard errors at the day level. Tables report both conventional p -values from day-clustered standard errors and randomization-inference p -values from 1,000 reassignments at the day level; in the text we use the conventional p -values and note the randomization-inference value where the two straddle the 10 percent level. We estimate equation (1) separately for each treatment phase: the Calculator-phase regression (September 2017–August 2018) includes the Information and Conciliation arms relative to the Calculator phase control; the Uber-phase regression (August 2018–July 2019) includes the Referral arm relative to the Uber-phase control. We do not pool across phases because the September 2017 earthquake forced PRODET to relocate inside the courthouse for the eleven months following it, which mechanically affected the Referral arm’s relevance since there is no transport needed. Comparing each treatment to its

²⁶If the worker shares the overoptimism documented in Section 2, her perceived expected recovery $\tilde{p}_{ij}\tilde{V}_i$ exceeds x_{ij} and her reservation value rises above S_W^* , adding a second, worker-side reason settlement fails; the calculator operates directly on this margin.

²⁷Adding the social cost of continuation strengthens this conclusion. If continuing case i imposes an external cost $\Omega_i \geq 0$ — court operating costs and congestion imposed on other litigants — that neither party internalizes, the planner’s settlement threshold $S_{\text{soc}}^* \equiv \delta_W x_{ij} - (\lambda_W + \Omega_i)/(1 - \alpha)$ lies weakly below the worker’s S_W^* , so the range of offers that the lawyer rejects but that settlement would make socially preferable is wider still, and it is widest for the lawyers with the lowest continuation costs K_j .

²⁸The Referral treatment lowers the cost of reaching the public attorney, a margin the framework does not model and on which it makes no prediction.

contemporaneous control isolates the treatment effect from this and other secular changes.

Compliance with assignment was very high in the two information arms: all workers randomized into the Information or Conciliation arm received the relevant materials at the booth before leaving. In the Conciliation arm 48% of employers attended the appointment.²⁹ The Referral arm had a take-up rate of 82%. Given the high compliance in the information arms and the fact that the policy-relevant comparison in the Referral arm is to a worker's general access to public legal advice rather than to the ride itself, we report ITT estimates throughout and treat them as the parameters of interest.

5.2 Expectation updating

For the calculator to affect downstream behavior it must, at a minimum, move beliefs. Two features of baseline beliefs leave room for it to do so. First, a significant share of workers respond that they "don't know" the probability of winning (16%) or the amount they will win (60%). Second, those who do provide responses are wildly optimistic about the likelihood of winning their case. Workers approaching the booth report an average subjective probability of winning a judgment of 89%, while the calculator's average prediction for the same workers is 41%.³⁰ Consistent with the over-optimism, [Sadka et al. \(2024\)](#) shows that the sum of same-case plaintiff and defendant subjective win probabilities in a set of ongoing MCLC cases sums to 1.4 rather than 1.

The calculator moves workers' expectations toward their predictions and decreases the share of workers responding "don't know" in the re-elicitation conducted just after treatment. Among workers who give an expected probability prior to treatment, about 30% in the Information and Conciliation arms lower their stated probability of winning between the baseline and the immediate follow-up. The average drop is small—about 4 percentage points, from 89% to 85%—but the move is in the expected direction and the re-elicitation is too tightly timed to be plausibly contaminated by other events. The treatment also decreases those saying they "don't know" the probability of winning by a third, from 14% to 9%. Table 2 reports the coefficients. The effect on expected recovery amounts are starker. Around a quarter (26%) of respondents decreased the amount they expected to recover conditional on winning (col 4); the measured effect on the amount they expect to win is positive but highly insignificant (col 5). The more important effect on expected awards is that the share of respondents saying they "don't know" how much they would win falls by a third, from 60% to around 42%. Thus, the calculator has an effect of reducing overly optimistic expectations of winning, and gives workers a first estimate of the amount they should expect to receive.

²⁹As noted above, 56% of workers reported delivering the letters; 27 employers showed up even though workers told us they had not delivered the letter.

³⁰The question asked specifically about winning conditional on going to judgment. Around 65% of workers filing suits receive something through settlement or judgment, a percentage still far below the 89% expected by respondents.

Table 2: Post-Treatment Expectation Updating

	Probability			Amount		
	Dummy (lowered) (1)	Continuous (2)	Don't Know (3)	Dummy (lowered) (4)	Continuous (5)	Don't Know (6)
Information	0.31*** (0.02)	-0.05** (0.02)	-0.07*** (0.02)	0.26*** (0.03)	2732.52 (3593.19)	-0.18*** (0.03)
Conciliation	0.28*** (0.03)	-0.02 (0.02)	-0.08*** (0.02)	0.24*** (0.04)	12963.55 (10915.53)	-0.22*** (0.04)
Observations	1,626	1,626	1,945	742	742	1,945
R-squared	0.154	0.006	0.014	0.132	0.017	0.057
Avg. Baseline Mean	.	0.89	0.16	.	57,175.36	0.60
p-value RI (Information)	0.00	0.00	0.00	0.00	0.77	0.00
p-value RI (Conciliation)	0.00	0.24	0.00	0.00	0.05	0.00

The table estimates the effect of the Information and Conciliation treatments on immediate expectation updating, defined as the change in subjective expectations between the baseline survey and a re-elicitation conducted a few minutes after the calculator is provided. Column (1) is an indicator for whether the worker's reported probability of winning at re-elicitation is lower than at baseline. Column (2) is the continuous change in the reported probability of winning across the two surveys. Column (3) is an indicator equal to one if the worker reported not knowing their probability of winning immediately after receiving the treatment, a variable defined regardless of whether the worker answered the question at baseline. Column (4) is the analogue of column (1) for the amount the worker expects to recover conditional on winning. Column (5) is the analogue of column (2) for amounts. Column (6) is the analogue of column (3) for the amount the worker expects to recover conditional on winning. The Referral arm is omitted because the Uber treatment did not deliver statistical information. Standard errors clustered at the day level in parentheses. We assume there was no change in the control group in the 3 or so minutes that it took to give the information treatment, and input the same belief for them in the "after" period.

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

We did not re-elicite expectations after the Uber treatment because the Referral arm sent workers to PRODET and we were not able to interview respondents after their interactions with the public attorney. Moreover, this arm received no statistical information from us.

5.3 Effects on Settlement and Suing

We first examine the extensive-margin outcomes: whether treatment changes the rate at which the worker settles with the employer, files a lawsuit, or does neither ("drops").³¹ Table 3 reports ITT coefficients from regressions of each outcome on assignment to each of the three treatment arms.

The Conciliation treatment produces the largest effect on settlement, raising the probability of settlement by 16 percentage points ($p < 0.01$) relative to a control mean of 50%. This closes a significant part of the settlement gap between Mexico and other developed countries. The treatment effect is five times larger than the Information alone ($\beta = .03, p = 0.34$); the gap between the two treatments is highly significant ($p < 0.01$). The Referral arm raises settlement by 7 percentage points, significant at the 10 percent level.

The fact that the Information arm, which delivers the same statistical information without the court-issued appointment, does not significantly move settlement (+3pp, p -value=0.34) indicates that the bargaining failure in this setting is not purely informational: workers and firms need a structured (and possibly government sanctioned) channel to bargain, in addition to more accurate beliefs about case value.

³¹We code *Settled* as one if the worker reports having reached an agreement with the former employer in either the two-week or the two-month survey; *Sued* as one if the worker reports having sued in the two-month survey or if we locate a case filed in her name in the court's administrative records; and *Dropped* as the residual. *Settled* and *Sued* are not mutually exclusive because *Sued* also incorporates lawsuits found by matching respondents to the court's administrative records. As a result, the three outcomes need not sum to 100%.

Table 3: Treatment effect on case ending

	Settle (1)	Sued (2)	Dropped (3)
<i>Panel A: Calculator Phase</i>			
Information	0.03 (0.03) [0.341]	-0.05 (0.03) [0.121]	0.01 (0.02) [0.795]
Conciliation	0.16 (0.04) [0.000]	-0.11 (0.04) [0.005]	-0.03 (0.02) [0.127]
Observations	1,562	1,562	1,562
R-squared	0.019	0.007	0.005
Dep. Var. Mean	0.50	0.46	0.11
BVC	✓	✓	✓
p-value RI (Information)	0.388	0.148	0.768
p-value RI (Conciliation)	0.000	0.002	0.126
p-value (Information = Conciliation)	0.001	0.101	0.098
<i>Panel B: Uber Phase</i>			
Referral	0.07 (0.04) [0.088]	-0.05 (0.04) [0.175]	0.00 (0.02) [0.878]
Observations	1,097	1,097	1,097
R-squared	0.007	0.003	0.003
Dep. Var. Mean	0.48	0.48	0.09
BVC	✓	✓	✓
p-value RI	0.075	0.178	0.869

Notes: The table reports, in each column, coefficients from an regression of case outcomes on treatment assignment and baseline controls (gender, tenure, daily wage), with standard errors clustered by treatment date in parentheses and conventional p-values in brackets. The first panel estimates the effect of the Information and Conciliation arms relative to the Calculator-phase control; the second panel estimates the effect of the Referral arm relative to the Uber-phase control. The sample conditions on having answered the two-month survey. *Settle* equals one if the worker answered *Yes* to “Have you reached an agreement with your company?” at either the two-week or two-month survey. *Sued* equals one if the worker answered *Yes* to “Did you sue your former employer?” at the two-month survey or if we located a case filed in the worker’s name in the court’s administrative records. *Dropped* equals one if the worker neither solved nor sued. p-values RI are computed via randomization inference with 1,000 permutations.

All three arms reduce the probability of filing a lawsuit, though again the measured effect of the Conciliation treatment is much larger. Conciliation reduces suing by 11 percentage points (significant at the one-percent level), while the Information and Referral arms reduce suing by 5 percentage points each, with neither effect statistically significant ($p = 0.12$ for Information, $p = 0.18$ for Referral). Around 10 percent of the workers in both the Calculator and Uber control groups let their case drop rather than either settling or suing. None of the treatments has a significant effect on the share of cases that are dropped, but Conciliation leads to fewer dropped cases and Information to slightly more, and the difference between the Information and Conciliation treatments is 4 percentage points, significant at the 0.10 level. The Settled-and-Sued patterns imply that cases that would have been filed under control instead settle. The Information and Referral arms, by contrast, produce smaller increases in settlement (3 and 7 percentage points) with offsetting reductions in suing of similar size.

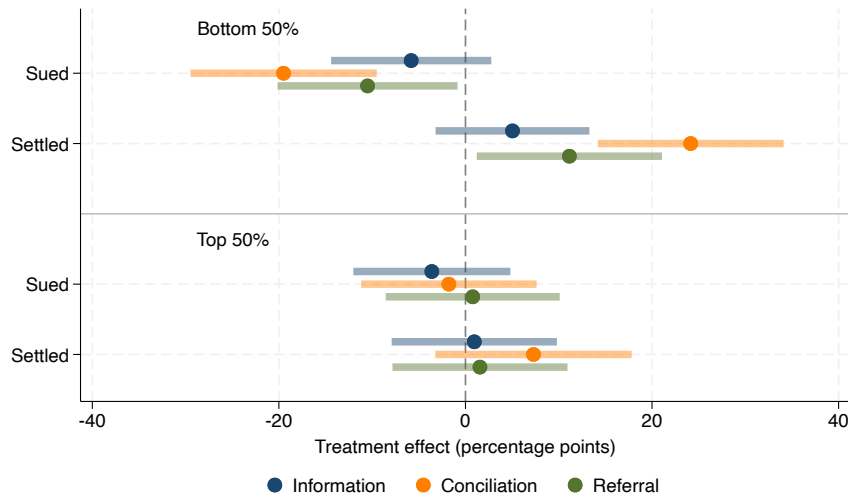
5.4 Effects by casevalue size

The framework in Section 4 predicts *which* lawsuits a pre-lawsuit intervention should deter: through the worker’s participation constraint, filing exits should be concentrated among the cases with the lowest predicted value — the segment that is profitable to serve only under the coyote

business model. This section tests that prediction. Because the predicted case value $\hat{x}_i$ is a function of baseline characteristics alone, we can classify every randomized worker by the ex-ante value of her potential case, and estimate treatment effects by predicted case value.

We split the sample at the median of $\hat{x}_i$ by phase (roughly \$17,000 MXN in January 2017 pesos, or \$795 USD for both phases). We then estimate a fully interactive model, regressing the outcomes of settlement and suing on treatment indicators and the baseline controls. This measures the treatment effect on the rate at which workers with above / below median case values settle or sue. Table OA-7 reports the full regression results and Figure 3 plots the joint-outcome estimates for suing and settlement below and above the median of predicted case value. The regression shows that control-group workers with below median predicted case values are 13.5 percent *less* likely to settle and 9.4 percent *more* likely to sue. This runs against the usual prediction that low-stakes disputes settle more readily because litigation costs loom larger relative to the stakes. However, it is what the coyote business model of Section 4 implies, and re-enforces the court's concern with the role low-value cases play in overcrowding. The Conciliation treatment more than offsets the higher filing rate among low-value cases. Below the median, Conciliation group suits fall by 19.5 percentage points, and settlement increases by 24.1 percentage points (almost 50% of the control group mean of 49.7 percent), with both effects significant at the $p < 0.001$ level. The Referral treatment is also effective in reducing low-value suits: the share of workers suing falls by 10.5 percentage points ($p = 0.047$) while the share of workers settling increases by 11.1 percentage points ($p = 0.031$). The information treatment has effects that are much smaller and statistically insignificant. No treatment deters high-value claims. There are no significant effects among workers with above-median predicted case values, though the (highly insignificant) coefficients show higher settlement for all three treatments and lower rates of suing for Information and Conciliation.

Figure 3: Heterogeneous Treatment Effects on Suing and Settling by Predicted Case Value



Notes: The figure reports linear combinations from separate worker-level OLS regressions of $O_i = \alpha + \sum_k \beta_k T_{i,k} + \delta_S 1\{\hat{x}_i \in S\} + \sum_k \theta_{S,k} (T_{i,k} \times 1\{\hat{x}_i \in S\}) + X_i' \gamma + \varepsilon_i$, where O_i indicates that worker i filed a lawsuit (*Sued*) or resolved the conflict through a settlement (*Settled*), and S denotes the bottom 50% of the phase-specific predicted case-value distribution, pooling all treatment arms within each phase. The top 50% is the omitted category. *Settled* and *Sued* are not mutually exclusive: a worker may report having settled in either follow-up survey while also being matched to a case filed under her name in the court's administrative records. The figure reports $\beta_k + \theta_{S,k}$ for workers in the bottom 50% and β_k for workers in the top 50%. Information and Conciliation estimates use the Calculator-phase sample and its contemporaneous control group; Referral estimates use the Uber-phase sample and its own control group. X_i contains the baseline controls used throughout (Section 3). Points denote the linear combinations expressed in percentage points; bars show 95% confidence intervals based on standard errors clustered at the randomization-day level. Table OA-7 reports the corresponding regression estimates, linear combinations, and randomization-inference p-values.

The joint-outcome estimates have a simple descriptive counterpart. Conditional on filing, the distribution of predicted case value shifts to the right in every arm relative to control, most visibly for Conciliation (Appendix Figure OA-3, Panel A). Because coyotes serve the left tail of the case-value distribution (Figure 2), this composition shift mechanically removes part of their client base; Section 5.5 makes some progress separating that channel from changes in lawyer choice within case value.

5.5 Choice of Lawyer

Section 5.3 showed that the Conciliation arm induces settlement rather than suing, and Section 5.4 showed that the cases not filed come disproportionately from its low-value tail. We now ask which kinds of lawyers workers *consult* and *hire* under each treatment. Table 4 shows the results from regressions of two sets of outcomes related to lawyer choice. The first four columns show the effect of treatment on whether the worker reported talking to a lawyer of a given type at the two-week survey,³² and columns 5 through 8 show the effects of treatment on whether the worker sued with a lawyer of a given type, measured using responses from the two-month survey and matched casefiles, as described in Section 4. Workers who did not file a suit are coded as zero for each lawyer type in the “sued with” regressions. We discuss the results by treatment arm, reading across each row on Table 4.

Information. The Information treatment does not change the overall probability of consulting a lawyer ($\beta = -0.01$, $p = 0.68$), but it changes the *composition* of consultations. Workers in the Information arm are 11 percentage points more likely to consult a formal private lawyer ($p < 0.01$) and 13 percentage points less likely to consult a public lawyer ($p < 0.01$). There is no effect on talking with a coyote. We posit that workers go to the Public Attorney’s office in part to obtain information about case value—a function that public lawyers themselves perform in the initial consultation. Workers who have already seen the calculator’s prediction reallocate that consultation to private lawyers.

In spite of not changing the rate of contact with coyotes, the Information cuts the unconditional probability of suing with a coyote by 4 percentage points ($p = 0.03$). The Calculator phase control mean is roughly 12 percent, so the share of workers who hire a coyote falls by about one-third. As noted above, the Information treatment reduces the share of workers filing a suit by 5 percentage points, an effect that is marginally insignificant ($p = 0.12$). The ITT coefficients on the lawyer types combine the extensive margin (whether to sue) with the intensive margin (which lawyer). A simple decomposition of the extensive and intensive margin effects suggests that about 1.3 percentage points of this effect can be attributed to the reduction in the rate at which Information treatment workers sue.³³ The remaining 2.7 percentage points of the coyote decline represents a lower propensity for a worker to hire a coyote. Table OA-8 shows that this result is unchanged when we control for deciles of case value, suggesting that the calculator induces workers with

³²The survey asked workers if they had talked with PRODET and separately whether they had talked with a private lawyer. For those reporting having talked with a private lawyer, We recorded a single response to where the worker found that. The responses will understate frequencies of talking with either coyote or formal private lawyers for workers who talked with both.

³³In the control group, 46% of workers sue, so the 5 percentage point drop in suits is 11% of the control group mean. Multiplying the 11% drop by the 12% coyote control mean yields a 1.3 percentage points extensive margin drop.

Table 4: Treatment effect on talking and suing with each lawyer type.

	Talked to (2w)				Sued with (2m)			
	Any (1)	Coyote (2)	Formal (3)	Public (4)	Any (5)	Coyote (6)	Formal (7)	Public (8)
<i>Panel A: Calculator Phase</i>								
Information	-0.01 (0.03) [0.679]	-0.00 (0.01) [0.775]	0.11 (0.03) [0.000]	-0.13 (0.03) [0.000]	-0.05 (0.03) [0.121]	-0.04 (0.02) [0.029]	0.04 (0.02) [0.094]	-0.05 (0.02) [0.016]
Conciliation	-0.20 (0.03) [0.000]	-0.03 (0.01) [0.010]	0.02 (0.03) [0.594]	-0.26 (0.03) [0.000]	-0.11 (0.04) [0.005]	-0.05 (0.02) [0.010]	0.01 (0.03) [0.702]	-0.07 (0.02) [0.002]
Observations	1,649	1,649	1,649	1,649	1,562	1,562	1,562	1,562
R-squared	0.029	0.007	0.017	0.056	0.007	0.007	0.003	0.010
Dep. Var. Mean	0.62	0.05	0.29	0.37	0.46	0.12	0.18	0.15
BVC	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI (Information)	0.741	0.765	0.000	0.001	0.164	0.026	0.133	0.026
p-value RI (Conciliation)	0.000	0.010	0.619	0.000	0.005	0.015	0.689	0.002
<i>Panel B: Uber Phase</i>								
Referral	0.07 (0.03) [0.011]	-0.04 (0.01) [0.009]	-0.09 (0.03) [0.002]	0.20 (0.03) [0.000]	-0.05 (0.04) [0.175]	-0.02 (0.02) [0.213]	-0.05 (0.03) [0.064]	0.03 (0.02) [0.213]
Observations	1,156	1,156	1,156	1,156	1,097	1,097	1,097	1,097
R-squared	0.010	0.012	0.020	0.048	0.003	0.004	0.007	0.004
Dep. Var. Mean	0.75	0.07	0.33	0.49	0.48	0.10	0.25	0.13
BVC	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI	0.007	0.016	0.001	0.000	0.182	0.225	0.065	0.206

Notes: The table reports treatment effects on talking to and suing with different types of lawyers. All dependent variables are indicators. *Talked to any* equals one if the worker answered *Yes* at the two-week survey to either “Have you talked to a public lawyer or any lawyer from the Public Attorney’s office?” or “Have you talked to a private lawyer?”; *Talked to public* equals one if she answered *Yes* to the first of these questions. *Talked to coyote* equals one if she reported talking to a private lawyer whom she found near the entrance or inside the court; *Talked to formal* equals one if she reported talking to a private lawyer found elsewhere. *Sued with any* equals one if the worker answered *Yes* to “Did you sue your former employer?” at the two-month survey or if we found a case filed in her name in the court’s records. *Sued with public* equals one if she reported suing with a public lawyer at the two-month survey. *Sued with coyote* equals one if (a) she reported suing with a private lawyer whom she found near the entrance or inside the court, or (b) the court files show that her lawyer is not public and has at least one case with a known coyote lawyer. *Sued with formal* equals one if (a) she reported suing with a private lawyer found elsewhere, or (b) the court files show that her lawyer is not public and has no cases with a known coyote lawyer. Section 4 and Appendix OA-B give the full classification procedure. The sample conditions on having answered the two-week survey in columns (1)–(4) and the two-month survey in columns (5)–(8). Panel A compares the Information and Conciliation arms with the Calculator-phase control group; Panel B compares the Referral arm with the Uber-phase control group. All regressions include an intercept, not reported, and baseline controls (BVC: gender, tenure, and daily wage). Dependent-variable means are for the control group only. Standard errors clustered by randomization day are in parentheses; conventional p -values are in brackets, and the final rows report randomization-inference p -values based on 1,000 treatment reassignments at the day level.

similarly valued cases to shift away from coyotes. The other results in the first row of Table 4 and in Table OA-8 show that the shift away from coyotes is matched by an almost equal shift to formal private lawyers (4 percentage points, $p = 0.094$), and that suing with public lawyers also falls by 5 percentage points ($p = 0.016$). As the Calculator says nothing about lawyer types and contains no warning about coyotes, we read the change in coyote hiring as a revealed-preference response: more-informed consumers buy less coyote services, presumably because evaluate them less favorably once informed. Coyotes tend to overclaim (see appendix OA-C), and that may be harder to do with informed workers.

Conciliation The Conciliation treatment leads to substantial changes in both consulting lawyers and settling disputes. Talking to any lawyer drops by 20 percentage points ($p < 0.01$), driven by a 26 percentage-point fall in talking to a public lawyer ($p < 0.01$); talking to a coyote falls 3 percentage points ($p = 0.01$) and talking to a formal private lawyer is statistically unaffected. The drop in the

number of workers consulting the public attorney is twice what it is with the Information alone. We read this as suggesting that the court-issued conciliation appointment does even more of what public attorneys would otherwise have helped the worker do: structure a pre-lawsuit interaction with the employer.

The Conciliation treatment also dramatically reduces the share of workers filing a case, as we have seen. This share drops by 11 percentage points, almost one-quarter of the control group mean. The drop comes entirely from cases not filed using the public attorney (a 7 percentage point drop, $p < 0.01$) and coyotes (a 5 percentage point drop, $p = 0.01$). Decomposing the 5 percentage point drop in the share of workers suing with coyotes, just over half of the effect (2.9 percentage points) comes from the extensive margin (an 11% drop in suits on a 46% control group mean), with the remaining 2.1 percentage points coming from a shift away from coyotes among those who sue.

Referral. The Referral treatment, as intended, raises consultation with a public lawyer by 20 percentage points ($p < 0.01$). The added consultations with public lawyers come partly at the expense of other lawyers: talking to a formal private lawyer falls by 9 points ($p < 0.01$) and talking to a coyote by 4 points ($p < 0.01$), so that overall consultation increases by 7 percentage points. The effects on suing are smaller. The share of workers filing a case drops by 5 points ($p = 0.18$); suing with a formal private lawyer falls by 5 points ($p = 0.06$), while suing with a coyote (−2 points, $p = 0.21$) and with a public lawyer (+3 points, $p = 0.21$) do not move significantly. The fall in coyote contact at two weeks is thus not matched by a fall in coyote retention at two months. We had expected Referral to displace coyote retention, since public lawyers and coyotes both concentrate in lower-value cases (Figure 2); instead it displaces formal private lawyers. Moreover, contrary to what we and the court expected, inducing workers to consult a public lawyer did not significantly increase the use of free representation. Table 1 offers one clue: on credentials, public lawyers sit closer to coyotes than to formal private lawyers.

5.6 Lawyer quality and satisfaction with the lawyer

Section 5.5 showed that the information treatments shift workers across lawyer types: away from coyotes and public lawyers in the Information and Conciliation treatments, towards formal lawyers in the Information treatment and away from formal lawyers in the Referral treatment. Because treatment was given before workers had retained a lawyer, a natural question is whether the treatments also improve the quality of the lawyers workers hire. We assess this using both an objective measure of lawyer quality — whether the contracted lawyer graduated from a top law school — and subjective measures of workers' satisfaction with their lawyer. Because these outcomes exist only for workers who hire a lawyer the estimates, reported on Table 5, describe the quality of representation among filed cases.

The first column of Table 5 reports ITT estimates for the effect of treatment on the share of the three lawyers listed in the casefile who graduated from a law school listed in the QS World University Rankings. Panel A reports results from the Calculator phase and Panel B from the Uber phase. The Information treatment raises this share by 10 percentage points relative to a control mean of 32% ($p < 0.01$). The Conciliation treatment also has a positive measured effect of 4 percentage points, but the effect is not statistically different from zero ($p = 0.34$). The Referral

Table 5: Lawyer quality: school, desire to change lawyers, and satisfaction

	Share high-quality school	Wants to change lawyer			Satisfaction w. lawyer
	QS	No	Yes	Don't know	Rating
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Calculator Phase</i>					
Information	0.10 (0.04) [0.009]	0.17 (0.05) [0.001]	-0.08 (0.04) [0.037]	-0.09 (0.05) [0.069]	0.43 (0.23) [0.062]
Conciliation	0.04 (0.04) [0.337]	0.19 (0.06) [0.001]	-0.12 (0.04) [0.001]	-0.07 (0.06) [0.224]	0.58 (0.22) [0.007]
Observations	420	552	552	552	371
R-squared	0.021	0.030	0.023	0.009	0.042
Dep. Var. Mean	0.32	0.39	0.21	0.40	8.16
BVC	✓	✓	✓	✓	✓
p-value RI (Information)	0.007	0.004	0.046	0.070	0.066
p-value RI (Conciliation)	0.371	0.001	0.005	0.237	0.030
p-value: Info. = Conc.	0.195	0.748	0.309	0.718	0.510
<i>Panel B: Uber Phase</i>					
Referral	-0.07 (0.04) [0.089]	-0.02 (0.04) [0.628]	0.00 (0.04) [0.960]	0.02 (0.04) [0.638]	-0.21 (0.17) [0.202]
Observations	301	447	447	447	354
R-squared	0.017	0.020	0.011	0.011	0.019
Dep. Var. Mean	0.42	0.58	0.21	0.21	8.76
BVC	✓	✓	✓	✓	✓
p-value RI	0.098	0.616	0.955	0.620	0.223

This table uses the subsample of workers filing a suit, hence the smaller sample sizes. Column (1) shows the effect of the main treatment arms on the share of hired lawyers that graduated from a top-rated law school according to the QS ranking. Columns (2) to (4) show the effect of treatment on the answer to the question “If you could change lawyers at this moment, would you do it?” (*no*, *yes*, *don't know*). Column (5) shows treatment effects on the answer to the question “From 1 to 10, where 10 is perfectly satisfied and 1 is terrible service, how satisfied are you with your lawyer?”. Controls (BVC) include gender, tenure, and wage. The regressions include an intercept term, which is not shown in the table. The dependent variable mean reported in each column corresponds to the control group only. Standard errors clustered at the day level in parentheses.

treatment (Panel B) has a negative effect on lawyer quality of 7 percentage points, significant at the 10% level ($p = 0.089$). This is consistent with the shift away from formal private lawyers documented in Section 5.5.³⁴

We next examine effect of treatment on two subjective measures of worker satisfaction with their lawyer. Columns 2–4 report effects on the question, asked at the two-month survey, “If you could change lawyers at this moment, would you do it?” In the control group, 39% say *no* (indicating satisfaction), 21% say *yes*, and 40% say that they *don't know*. The Information treatment increases the share saying *no* by 17 percentage points ($p < 0.01$) and reduces the share saying *yes* by 8 percentage points ($p = 0.037$), indicating that treatment increases satisfaction with the choice of lawyer. The share saying *don't know* falls by 9 percentage points ($p = 0.07$). The Conciliation arm produces an even larger increase in reported satisfaction: the share of workers saying they do not want to change lawyers increases by 19 percentage points while the share saying they want to change lawyers falls by 12 percentage points ($p < 0.01$ in both cases). Column 5 measures stated satisfaction with the choice of lawyer on a scale of 1–10, with a Calculator phase control mean of 8.16. The Information

³⁴Because the QS ranking may miss lesser-known but high-quality Mexican law schools, we also asked three ITAM law professors—two of whom are practicing labor lawyers—to rate the top Mexican law schools on a 1–10 scale. Results using this alternative ranking are very similar.

treatment raises satisfaction by 0.43 points (0.22 standard deviations, $p = 0.06$), and the Conciliation treatment raises it by 0.58 points (0.31 standard deviations, $p < 0.01$). Panel B shows that the Referral treatment has no significant effect on these satisfaction measures, with point estimates that indicate slight movement to dissatisfaction. Satisfaction is not a direct prediction of the framework in Section 4; we read these responses as consistent with the maintained assumption that formal private lawyers provide higher-quality representation.

The treatments change which lawyer type workers hire, so part of any effect on quality and satisfaction could reflect composition alone: coyote clients report lower satisfaction (Table 1), so moving workers out of the coyote category mechanically raises the average. Controlling for lawyer type in the regression does not answer this question, because lawyer type is chosen after treatment and is therefore a “bad control” (Wooldridge, 2024). Instead, we restrict ourselves to a simple back-of-the-envelope decomposition that separates out the pure lawyer choice channel. channels by separating the lawyer choice channel. Let s_t denote the share of filers hiring lawyer type $t \in \{\text{coyote, formal, public}\}$ and $\bar{y}_t$ the control-group mean of the quality / satisfaction outcome among filers with type t . The pure lawyer composition effect is then $\sum_t \Delta s_t \bar{y}_t$, where Δs_t is the treatment-induced change in the type shares. This measures the effect we would observe if marginal lawyer-type outcomes were equal to average lawyer-type outcomes – that is, if each worker who changed lawyer type had the average outcome for that chosen lawyer type. This is a strong assumption, so we view this only as a benchmark. Under this assumption, lawyer composition accounts for roughly a third of the Information effect on the QS share (a predicted 3.2 against an estimated 10 percentage points) and about one-quarter of its effects on not wanting to change lawyers (4.6 of 17 percentage points) and on the satisfaction rating (0.10 of 0.43 points). For Conciliation, lawyer composition accounts for only about one-quarter of the effect on the desire to change lawyers and one-sixth of the rating effect. Under the same assumption, the remaining effect reflects differences within lawyer type. These come either from treated workers choosing better lawyers within lawyer types, or from treatment affecting which workers files cases such that the “marginal equals average” assumption does not hold.

5.7 Well-being proxies

The cases of almost half of the workers in our sample remained pending at the two-month follow-up survey, so we cannot conduct an analysis of final case outcomes. We did, however, ask each worker a short battery of well-being questions at two months: a 1–10 happiness scale, two indicators of liquidity stress in the previous three months (had to stop paying for a basic service such as electricity, water, or rent for lack of money; lacked money to spend on food on one or more days), and an indicator for whether the worker is currently working.³⁵ The two liquidity-stress measures can be seen as counterparts of the worker-lawyer discount-rate wedge discussed in Section 4: dismissed workers face binding short-run liquidity constraints, and treatments that reduce the expected duration of these constraints should improve welfare on these dimensions. The control group means in Table 6 confirm that baseline liquidity stress is severe. Among Calculator phase controls, 59% had to stop paying a basic service in the previous three months and 55% lacked

³⁵The pre-registration also listed employment, durable purchases, wages, and expenses which we did not collect since they made the phone survey too long.

money for food on at least one day. Among Uber phase controls the analogous figures are 63% and 51%. Only 46% (Calculator phase) and 40% (Uber-phase) of control-group workers report being currently employed two months after dismissal.

Table 6: Well-being proxies

	Happiness (1)	Stopped paying services (2)	Lack of money (3)	Currently working (4)
<i>Panel A: Calculator Phase</i>				
Information	0.058 (0.136) [0.671]	-0.062 (0.027) [0.021]	-0.069 (0.027) [0.013]	0.008 (0.029) [0.777]
Conciliation	0.201 (0.142) [0.158]	-0.056 (0.038) [0.135]	-0.085 (0.035) [0.017]	0.003 (0.035) [0.932]
Observations	1,552	1,565	1,567	1,570
Dep. Var. Mean	7.884	0.595	0.557	0.453
BVC	✓	✓	✓	✓
p-value RI (Information)	0.662	0.033	0.022	0.777
p-value RI (Conciliation)	0.189	0.099	0.008	0.921
<i>Panel B: Uber Phase</i>				
Referral	0.055 (0.142) [0.698]	-0.020 (0.029) [0.489]	0.013 (0.031) [0.670]	0.031 (0.033) [0.357]
Observations	1,094	1,098	1,100	1,100
Dep. Var. Mean	7.742	0.620	0.499	0.402
BVC	✓	✓	✓	✓
p-value RI	0.718	0.485	0.669	0.372

The two-month survey for the experiment included questions measuring proxies for welfare. We asked (Happiness) *On a scale of 1 to 10, where 1 means “not happy at all” and 10 means “totally happy”, in general how happy do you feel about your life lately?*; (Stopped paying services) *In the past 3 months have you had to stop paying for a basic service such as electricity, water or rent due to lack of money?*; (Lack of money) *In the past 3 months, have you lacked money to spend on food one or more days?*; (Currently working) *Are you currently working?*. The regressions include an intercept term, which is not shown in the table. The dependent variable mean reported in each column corresponds to the control group only. Standard errors clustered at the day level in parentheses; randomization-inference p-values in brackets.

Table 6 reports estimated effects. The Information significantly reduces both liquidity-stress measures: workers in the Information arm are 6 percentage points less likely to have stopped paying a basic service ($p = 0.02$) and 7 percentage points less likely to have lacked money for food ($p = 0.01$). Relative to the Calculator phase control means, these are reductions of roughly 10 and 13 percent, respectively. The Conciliation arm produces a directionally similar pattern with an even larger effect on food insecurity: a 9 percentage point reduction ($p = 0.01$, 16 percent of the control mean), together with a 6 percentage point reduction in service-payment stress that is not statistically significant at conventional levels ($p = 0.13$). Effects on happiness and current employment are small and statistically indistinguishable from zero in both arms. Panel B reports estimates for the Uber-phase. The Referral arm does not move any of the four well-being measures.

The fact that the information and conciliation treatments have similar effects on these measures is perhaps surprising, given the differences in the effect of the two treatments on settlement rates discussed above. In Table OA-10, we run regressions that allow the case outcome and well-being treatment effects to vary by case value. We find that the effect of information is concentrated almost exclusively among workers in the lowest quartile of salaries. In this quartile, we find that the

Information treatment significantly decreases suing by 11 percentage points (p -value=0.08), with an imprecise increase in settlement of 5.9 percentage points (p -value=0.37). The effects for conciliation are also larger in the lowest quartile, though the effects on settling, suing and lacking money are also there for higher-income workers. Thus, these information treatment effects are somewhat more consistent in the lowest quartile of case values, though we cannot rule out that the treatments have effects on welfare through mechanisms other than settlement or suing.³⁶

As noted, we do not have final case outcomes for a large share of the cases in the experiment. [Sadka et al. \(2024\)](#) showed that the amount recovered among workers in ongoing cases who settled as a result of a similar information treatment was at not less, and perhaps more than, untreated workers. On Table [OA-6](#) we report results of regressions using data on settlement amounts that we collected in the two-week and two-month surveys. These data should be interpreted with a lot of caution, because treatment affected the probability of settlement and because there were substantial non-response rates on this question even among those who did settle. The 540 observations in the sample represent around 71% of workers who settled in the Calculator arm and 87% in the Uber arm. We control for case characteristics, which at least partly addresses these concerns. With these caveats in mind, we find that settlement amounts are not significantly changed by treatment. Moreover, Column 2 shows that in the information arm, settlements are 95% ($=1.21-0.26$), and in the conciliation arm 118%, of the amount predicted to be obtained at trial. Column 3 shows that treated workers in both arms are not receiving lower settlement amounts as a fraction of the overall predicted value (whether at trial or not) compared to the control group. Referrals have no effect on recovery amounts in either of these columns. The biggest gain to workers is receiving the payment quickly. The typical worker has a discount rate of between 50 and 100% per year, implying a very large discount to recovery from a trial lasting four years on average.

5.8 Summary and Discussion

The results in Sections [5.2](#) through [5.7](#) trace the effect of the Information, Conciliation and Referral treatments on worker expectations, decisions to settle or sue, the choice of lawyers and welfare effects. [Table 7](#) summarizes the results for each of the three treatments.

The clearest and sharpest story comes from the Conciliation treatment. The information component of that treatment moves workers' beliefs both by reducing their expectations on winning and by providing them with an estimate of how much they will win. The Conciliation treatment increases the share of workers settling their dispute by 16 percentage points (32%) and decreases the share filing suits by 11 percentage points (24%). These effects are almost exclusively among workers lower predicted case values. Those workers who do sue are less likely to hire a coyote lawyer and report being more satisfied with the lawyer they choose two months into the process. Finally, workers in the arm are less likely to report having missed meals or not being able to pay bills.

The Information treatment shows effects of significantly smaller magnitude that follow a generally similar pattern. Worker beliefs are re-calibrated as in the Conciliation treatment, and while

³⁶A worker who no longer expects a large and quick award—about 30% of treated workers revise their probability of winning downward (Section [5.2](#))—has reason to spend more cautiously, borrow and search for work more intensively.

Table 7: Summary of Results

Outcome	Information	Conciliation	Referral
Recalibration of case value	• 31% of workers reduce probability of winning • An additional 18% of workers can estimate what they will win	• 28% of workers reduce probability of winning • An additional 22% of workers can estimate what they will win	• NA
Decision to settle or sue	• Settlements increase insignificantly • Insig. drop in suits of 5 percentage points ($p = 0.12$) • Insignificant effect below / above median expected value	• $\uparrow$ 16 percentage point (32%) in settlements • $\downarrow$ 11 percentage point (24%) in filing suits • Effects concentrated in cases with below median expected values	• $\uparrow$ 7 percentage point in settlements (14%) • Insig. drop in suits of 5 percentage points ($p = 0.18$) • Effects concentrated in cases with below median expected values
Lawyer choice	• Coyotes: $\downarrow$ 4 pp (33%) • Private formal: $\uparrow$ 4 pp (22%) • Public: $\downarrow$ 5 pp (33%)	• Coyotes: $\downarrow$ 5 pp (40%) • Private formal: no change • Public: $\downarrow$ 7 pp (45%)	• Coyotes: no change • Private formal: $\downarrow$ 5 pp (20%) • Public: no change
Lawyer satisfaction	• Increase in lawyer quality • $\uparrow$ 17 pp (44%) increase in wanting to keep lawyer • $\uparrow$ 0.22 s.d. in lawyer satisfaction	• No change in lawyer quality • $\uparrow$ 19 pp (49%) in wanting to keep lawyer • $\uparrow$ 0.31 s.d. in lawyer satisfaction	• $\downarrow$ in lawyer quality • No change in wanting to keep lawyer • No change in lawyer satisfaction
Welfare	• $\downarrow$ 7 pp in "lacking money for food"	• $\downarrow$ 9 pp in "lacking money for food"	• No change in wellbeing measures

there are positive measured effects on settlement and negative measured effects on suing, these are much smaller in magnitude compared with Conciliation and not statistically significant. Moreover, information alone does not appear to convince workers with low-value cases to settle rather than sue. However, we do find significant effects of Information on lawyer choice: workers are less likely to contract a coyote lawyer, choose lawyers with degrees from higher-quality law schools, and report higher satisfaction with their lawyers two months into the process. They are also significantly less likely to report missing meals or not being able to pay bills.

Both the Conciliation and Information treatments alter the market for legal services, lowering the coyote market share by more than one-third. The majority of this effect is explained by changes in lawyer choice conditional on filing a suit. We interpret this as showing that the case value information allows workers to interpret the sales pitches that lawyers give them. Since coyotes are, both anecdotally and based on the quality of their training and client satisfaction, lower quality lawyers, workers who are more informed tend not to hire them. The theoretical papers cited above have posited that the informational advantages of experts can lead to equilibria where low-quality experts maintain a positive market share. This paper provides some validation to those papers by showing that reducing that informational advantage lowers their market share.

The Referral treatment was effective in encouraging workers to consult with public attorneys. Workers are more likely to settle and those with below median case values are less likely to sue. This suggests that small frictions like transport costs or indications of where to go for initial advice are highly consequential. However, the Referral treatment does not substantially affect the market for legal services. Workers are no less likely to contract with a coyote and no more likely to use a public attorney to file a case. Nor are workers more satisfied with their choice of lawyer. Finally, we find no welfare effects following the Referral treatment.

The overarching goal of the experiment was to help the Mexico City Labor Court understand the flow of cases into the court. The MCLC had three objectives: 1) reduce the number of low-value cases that account for a large share of the court congestion; 2) improve the quality of legal services by reducing the market share of coyotes; and 3) encourage workers, especially those with lower value cases, toward the public attorney by making them more aware of the services. The Conciliation treatment, and to a much lesser extent, the Referral treatment, met the first objective. The Information and Conciliation treatments met the second objective. The referral treatment was effective in leading workers to consult with the public attorney's office, but none of the treatments led to more representation by public attorneys. Overall, the Conciliation treatment was most effective in meeting the court's objectives.

6 The Reform at National Scale

In 2019 Mexico reformed its labor law, making conciliation a mandatory precondition for individual lawsuits, including the dismissal claims we study ([Congreso de la Unión, 2019](#)). A dismissed worker must first file a request at a newly created state conciliation center; the center summons the employer to a hearing before a conciliator, and only if no agreement is reached does it issue the *constancia de no conciliación* (certificate of non-conciliation) which required before a worker can file a lawsuit. The law caps this stage at 45 days, against a pre-reform court in which the median

case decided by judgment took roughly four years (Section 2). The same law replaced *Juntas de Conciliación y Arbitraje*, that were in the executive branch, by labor tribunals within the judicial branch. These opened in each state on the same date as its conciliation center.

The addition of the conciliation stage in the law was motivated by the Conciliation arm in our experiment, as coauthor Joyce Sadka sat on the committee that helped to draft the new law. Each state had to pass enabling legislation and open its center, and this was rolled out in three staggered waves: the law came into force in eight states on 18 November 2020, thirteen on 3 November 2021, and the remaining eleven—including Mexico City—on 3 October 2022.³⁷

We use the staggered rollout of the conciliation centers to compare the results of our experiment with an at-scale implementation of procedure very similar to our Conciliation treatment. We are interested in two outcomes. The first is how many cases reach the labor courts – the *Juntas de Conciliación y Arbitraje* and the labor tribunals after. For this, we have a complete and consistent data series spanning the pre- and post-reform periods from INEGI’s state-level labor-relations statistics, which records cases filed with the labor Juntas before the reform and cases filed with the new Labor Tribunals (not Conciliation Centers) after the reform. This ensures that any change in the number of cases filed is *not* mechanically produced by the transition between the two court systems.³⁸

Based on the results of our experiment, we expect a smaller share of cases to reach the labor court after the reform. However, if more workers enter the conciliation process, it is uncertain whether the total number of cases reaching the court should rise or fall after the reform. Thus, the effect of the reform on the number of court cases is uncertain. We examine this with an event study analysis. We use the INEGI data to assemble a monthly state-level panel of cases reaching the court and estimate staggered difference-in-differences, using Arkhangelsky et al. (2021) as our main estimator and Borusyak et al. (2024) as a robustness check. Identification comes from comparing newly treated states to not-yet-treated states. Every state is treated by October 2022. We normalize event-study coefficients to $t = -3$ months because the 60-day statute of limitations on dismissal claims (Article 516 of the Federal Labor Law) creates a two-month window before a center opens during which a dismissed worker can choose between immediate litigation or waiting for the opening of the conciliation center. Therefore, $t = -1$ and $t = -2$ may be contaminated by anticipation.³⁹

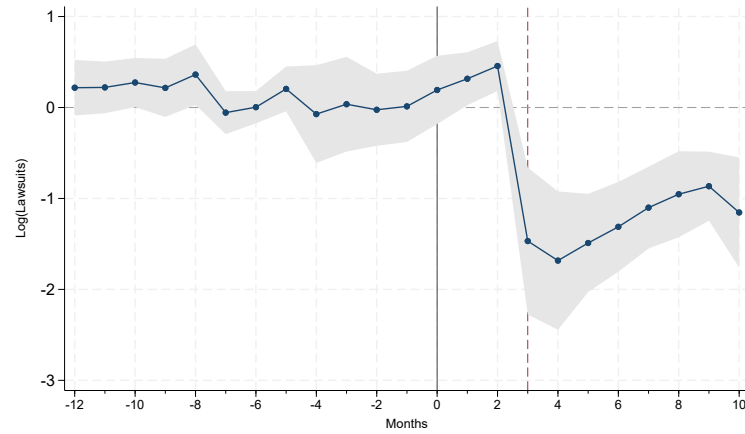
We are also interested in how many workers enter the formal legal system. The conciliation centers significantly decrease the cost of using the system. Workers do not need to hire a lawyer to use the conciliation process, and the procedure is capped at 45 days. As a result, we expect to see an increase in the number of workers entering the formal justice system. Prior to the reform,

³⁷First wave (18 Nov 2020): Campeche, Chiapas, Durango, Estado de México, Hidalgo, San Luis Potosí, Tabasco, Zacatecas. Second wave (3 Nov 2021): Aguascalientes, Baja California, Baja California Sur, Colima, Guanajuato, Guerrero, Morelos, Oaxaca, Puebla, Querétaro, Quintana Roo, Tlaxcala, Veracruz. Third wave (3 Oct 2022): Chihuahua, Ciudad de México, Coahuila, Jalisco, Michoacán, Nayarit, Nuevo León, Sinaloa, Sonora, Tamaulipas, Yucatán. (Consejo de Coordinación para la Implementación de la Reforma al Sistema de Justicia Laboral, 2022) However, three states were ultimately assigned to a later cohort than originally listed above: Hidalgo began operations on 3 Nov 2021 (second wave) instead of 18 Nov 2020, and Baja California Sur and Guerrero began operations on 3 Oct 2022 (third wave) instead of 3 Nov 2021.

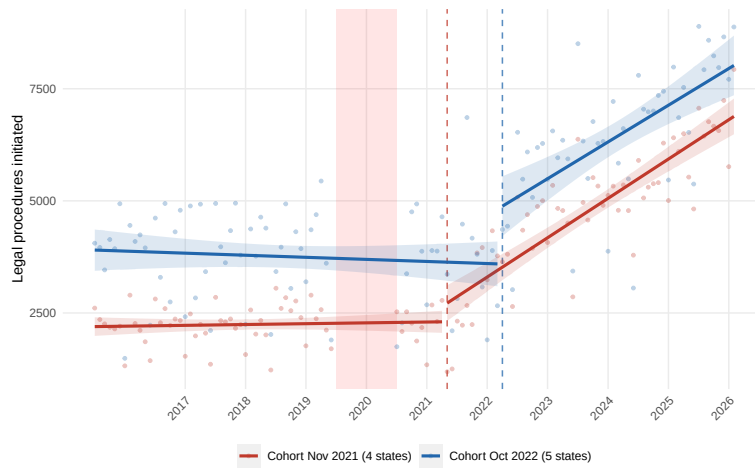
³⁸INEGI, *Estadísticas sobre Relaciones Laborales de Jurisdicción Local* (ERLAJUL). The individual-disputes variable is defined over the Juntas through 2019, over the Juntas and the local Labor Tribunals during the 2020–2023 transition, and over the Labor Tribunals from 2024 (Instituto Nacional de Estadística y Geografía, n.d.).

³⁹This is visible in the data: the event studies show a run-up in the two months immediately before opening, after which the post-opening estimates stabilize.

Figure 4: Lawsuits decrease while access to formal justice increases



(a) SDID event study: effect on individual labor lawsuits (log)



(b) Monthly filings by treatment cohort (spliced series)

Notes. Panel (a) reports the Synthetic Difference-in-Differences (SDID) event study for individual labor lawsuits (log scale). Each point is the estimated treatment effect at event-time horizon h relative to the normalization period $T^* = T - 3$; the gray band is the 95% bootstrap confidence interval ($B = 200$). Horizons $h \in \{-12, \dots, -1\}$ serve as pre-treatment placebo checks; the post-treatment window covers $h \in \{0, \dots, 10\}$; the dashed vertical line marks $h = 0$. Panel (b) plots the monthly sum of legal procedures initiated, by treatment cohort, using a spliced series that combines INEGI individual-conflict microdata before the local conciliation center opened with administrative conciliation-request records from the center afterward, excluding ratification-of-settlement requests. The sample covers nine states for which administrative data are available. Two cohorts are shown: centers opened in November 2021 (four states: Baja California, Puebla, Querétaro, and Quintana Roo, in red) and October 2022 (five states: Mexico City, Baja California Sur, Guerrero, Nayarit, and Sinaloa, in blue); the November 2020 cohort is excluded because we have conciliation center data only from Durango among states opening centers in 2020. Dashed vertical lines mark each cohort's opening date. Separate OLS trend lines are fitted over the pre-opening and post-opening periods for each cohort; shaded bands are 95% confidence intervals. The year 2020 (red shading) is excluded from the estimation sample because COVID-19 disrupted normal filing patterns.

the INEGI data measured both access and lawsuits, because filing a suit was the only path to a formal legal review. However, after the reform, access is measured by filing with the Conciliation Centers rather than the court. There is not yet reliable data at the national level on the number of cases filed with conciliation centers. Instead, we use data from the conciliation centers in nine states (including Mexico City) where we have direct access to administrative records. Given the limited data availability, we do not attempt to implement full difference-in-differences models, but instead display the data graphically. Nevertheless, as we discuss below, the patterns are quite stark.

Hypothesis 1: Fewer disputes reach the courts. Our first hypothesis is that the mandatory conciliation stage resolves disputes before they become lawsuits, as the Conciliation appointment did

in the experiment (Section 5.3), so that fewer disputes reach the courts. Panel (a) of Figure 4 shows that the number of cases filed in labor courts fell across Mexico’s 32 states after conciliation centers opened. Figure 4 panel (a) uses the timing of the opening of the conciliation centers to estimate a staggered difference-in-differences event study estimate using the method of Arkhangelsky et al. (2021). We estimate that court filings fall by 54% ($= \exp(-0.79) - 1$) on average over the 10 months following the opening of a state’s conciliation centers. The estimate is robust to using levels rather than logs and to using Borusyak et al. (2024) as the estimator.

We expect that the simpler conciliation phase will *increase* the number of workers accessing the formal justice system. But we should be concerned that the fall in filings might instead be the result of a discouragement from filing. Adding the requirement that the worker obtain a certificate of non-conciliation adds a step before a lawsuit can be filed. Plausibly, that step – in a new and unfamiliar institution – might deter workers from pursuing their claims at all. In that event, the then fall in cases reaching the courts might reflect fewer workers accessing the formal system. Our second hypothesis distinguishes the two possibilities.

Hypothesis 2: Access to the formal justice system increases. The additional conciliation stage might make accessing justice more costly by adding an additional step, or less costly by providing an effective low-cost entry point. If the share of cases reaching settlement in the conciliation stage is high enough, we might see more access despite the lower number of court cases. Indeed, that is what the available data show. Panel (b) of Figure 4 uses data from nine states for which we have access to administrative data from the conciliation centers to plot the monthly number of procedures initiated (lawsuits before the reform and conciliation requests after), along with linear fits before and after. Four of the nine states opened conciliation centers in November 2021 and the other five in October 2022. For the four states in the November 2021 cohort, the data show that lawsuits were flat at around 2,500 per month from 2016 to November 2021, but jumped almost immediately after the conciliation centers opened. A similar pattern holds for the 2023 cohort: the trend was flat at around 4000 cases per month and jumped immediately after the reform. Panel (b) is descriptive; the pre-treatment trends are broadly parallel across cohorts and the post-opening divergence is consistent with a causal interpretation, though the limited number of states with administrative coverage precludes a rigorous causal identification strategy.

We measure the effects over the 10 months following the reform to be consistent with the analysis of cases reaching the court. We find that the number of workers entering the system *increased* by 38.4% over the 10 months following the reform.⁴⁰ As is clear from the figure, there is a strong upward trend in the number of workers entering the conciliation process. Within three to four years, there are more than twice as many conciliation requests as there were lawsuits before the reform, indicating a near doubling of workers accessing the formal justice system.

⁴⁰We calculate

$$G = 100 \sum_{c=1}^2 w_c \left[\frac{1}{11} \sum_{h=0}^{10} \frac{Y_{ch} - \hat{Y}_{ch}^{pre}}{\hat{Y}_{ch}^{pre}} \right],$$

where Y_{ch} denotes total monthly filings in cohort c , and $h = 0$ is the opening month. The projection $\hat{Y}_{ch}^{pre}$ comes from a linear OLS trend in levels fitted to all available pre-opening observations in the estimation sample (the nine states with administrative coverage), excluding 2020. We use $w_1 = 4/9$ for November 2021 and $w_2 = 5/9$ for October 2022, reflecting the number of states in each cohort, with $\sum c = 1^2 w_c = 1$. This statistic is a descriptive weighted average of cohort-level percentage gaps and does not identify a causal ATT.

Taken together, the two results describe a reform that increased access to justice while decreasing the burden on labor courts. For every hundred disputes that entered the pre-reform system, all of which entered as lawsuits, roughly 138 enter as conciliation requests during the 10 months after the reform. On average, 76 of these are solved by conciliation (a conciliation rate of 55%), 45 proceed to court and 31 are dropped. A large survey discussed in [Sadka et al. \(2024\)](#) indicated that, prior to the reform, only 13% of dismissed workers not receiving severance used the justice system. Even with the 38% increase in engagement with the conciliation centers, only a minority of workers pursue their claims. The upward slope shown in Figure 7(b) suggests that the share pursuing their claim is increasing in the post-reform period. By the participation logic of Section 4, the increase workers entering conciliation process indicates that the centers reduce the cost of legal access, reflecting the benefits of no filing fee, no lawyer, and a statutory limit of 45 days rather than years. There is a range of disputes for which the request is worthwhile but litigation is not. We read the ramp in usage as reflecting learning costs related to the new institution. We do not have records linking conciliation certificates to subsequent court filings, so we are not able to measure characteristics of cases that ultimately file suits relative to those that either settle or are dropped.

The fall in litigation mirrors the experimental Conciliation arm: fewer suits, with the difference resolved by settlement before filing. The rise in entry is a margin the experiment could not observe, since our sample consists of workers who had already come to the court. Two caveats apply to the comparison. First, the rollout identifies the effect of the reform package — the conciliation centers opened jointly with the new labor tribunals that replaced the old labor courts — rather than of the conciliation stage in isolation. Second, the reform’s conciliation is mandatory and administered by dedicated centers, while the treatment we randomized was voluntary. With these caveats, the rollout provides evidence that a regime with institutional features like those operating in France, South Africa, Australia, and Argentina, can simultaneously widen access to justice and reduce the number of disputes reaching the courts.

7 Conclusion

Dismissed workers in Mexico City arrive at the labor court uninformed and overconfident about their cases, are recruited at the door by low-quality lawyers, and, until 2019, had no institution in which to bargain with their employer before filing suit. Settlement was low, especially for small claims, cases took years, and a large share of the suits filed had negative expected value for the worker who filed them. We study three low-cost interventions delivered before a lawyer is hired. The Conciliation arm, a court-issued appointment paired with personalized statistical information, raises settlement by 16 percentage points and cuts lawsuits by a quarter, removing mainly low-value cases. The Information arm, the same information without the appointment, does not measurably raise settlement but restructures the market for representation: the share of workers hiring *coyote* lawyers falls by a third, the quality of retained lawyers rises, and workers report higher satisfaction and less financial distress. The Referral arm, a ride to free public counsel, raises consultation with the public attorney but shifts legal representation away from formal private lawyers without reducing the coyote share or improving the welfare proxies.

We interpret these patterns through a simple theoretical framework. Coyotes maintain a court-house presence at a very low marginal cost per case, and their recruitment appears to benefit from

the overconfidence that the calculator corrects. Informed workers encounter coyotes at the same rate but hire them less often, even within narrow bins of case value. Formal private lawyers have higher marginal costs and select into higher-value cases. Information delivered to the plaintiff changes who she hires, but on its own it does not raise settlement much. Settlement increased only when the parties were encouraged to meet with the court conciliator before the worker, at least, had hired a lawyer.

Court proceedings often take four or more years, and our outcome data stop short of final dispositions. This limits the welfare statements we can make. Partial settlement data, together with the long legal process, suggest that workers very likely benefit from the immediate settlements. Discount rates for low-income households in Mexico are in the 50 - 100% range. Using the [Carvalho \(2010\)](#) estimate of a 78% annual discount rate, a worker prefers any settlement above one-tenth of the expected judgment; collection risk lowers that threshold further. The settlements we observe are far above this level. The average settlement is 95-118% the expected gross recovery from filing, which exceeds the net-of-fee return to litigating even at a zero discount rate.

Mexico's 2019 reform made pre-lawsuit conciliation mandatory nationwide. Its staggered rollout shows entry into the labor-justice system rising by 38% while disputes reaching the courts fell by 54%, with conciliation-stage settlements absorbing much of the difference. The rollout identifies the reform package rather than the conciliation stage alone, but the fall in litigation is the result the Conciliation arm predicts, measured at scale.

The frictions we document — courthouse intermediaries with negligible marginal costs, badly informed plaintiffs, unpriced court access, and a discount-rate wedge between clients and lawyers — are plausibly common in labor and small-claims courts across the developing world. In our setting, the cases that reached the court changed when the parties were given means through which to bargain, and the quality of lawyer they hired changed when the plaintiff was made more knowledgeable about expected case outcomes. Court reforms in developing countries have concentrated on processing the cases already filed. This paper shows that a pre-judicial conciliation stage would be a cheaper way to decrease legal conflict and relieve overburdened courts. This research leaves open questions related to long run or general equilibrium effects. First, whether coyotes exit, cut fees, or relocate to the centers' doors will shape how much of the gain survives at scale. Second, our welfare measures stop at two months; whether the gains persist, and whether they run through job search or borrowing requires longer follow-up.

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Supplementary Appendix for “Information, Conciliation, and the Lawyer Market: Experimental Evidence from Mexico’s Labor Courts”

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OA-A Private and Administrative Costs of Litigation

Section 2 reports two back-of-the-envelope calculations illustrating the prevalence of low-value litigation at the Mexico City Labor Court (MCLC). Both calculations use the sample of 5,000 completed historical casefiles used to train the statistical calculator. The calculator provides an *ex ante* predicted value for each case, and monetary amounts are expressed in January 2017 real terms.

We first construct the worker's expected net present value from filing a lawsuit. The calculation accounts for two components. First, the worker receives only a share of the amount ultimately recovered because private lawyers are typically compensated through a contingency fee. Second, when using private lawyers, filing a lawsuit typically entails a fixed upfront cost. Because recoveries may occur several years after filing, we discount the expected recovery over the duration of the case. The resulting net present value provides a common framework for assessing whether litigation is privately worthwhile for the worker.

Let $\hat{x}_i$ denote the calculator's prediction of the expected recovery in case i , the same variable used throughout the paper (Section 3); it already incorporates the probability of recovering a positive amount and is the counterpart of $x_{ij} = p_{ij}V_i$ in Section 4, not of the case value V_i , which is conditional on a positive recovery. Let α denote the lawyer's contingency-fee share, d_i the duration of the case in years, r the annual discount rate, and F the fixed upfront cost. The distribution of $\hat{x}_i$ is also used to characterize the prevalence of low-value cases in Section 2. In particular, the median predicted expected recovery among historical filed cases is approximately USD 1,250; the corresponding median among workers arriving at the booth is lower, about MXN 17,000 (Section 5.4). Predicted values are first expressed in January 2017 MXN and then converted to USD using the corresponding exchange rate of 21.3853 MXN per USD when necessary.

We define the worker's expected net present value as

$$NPV_i(r) = \frac{(1 - \alpha)\hat{x}_i}{(1 + r)^{d_i}} - F. \quad (2)$$

The first term is the present value of the worker's expected recovery net of the contingency fee, while the second is the fixed upfront cost.

The calculations reported in the paper use this framework in two different ways. Because the appropriate discount rate is not directly observed, Table OA-1 reports calculations under alternative annual discount rates. The figures reported in the main text correspond to our baseline discount rate of 50%.

Worker returns from litigation. To assess whether filing is privately worthwhile, we calculate the net present value of litigation using the predicted value $\hat{x}_i$ of each case i . We assume that the worker retains $(1 - \alpha) = 70\%$ of the amount recovered and that private lawyers charge an upfront fee of approximately MXN 2,000. For a given discount rate r , we classify a lawsuit as generating a negative expected return whenever $NPV_i(r) < 0$.

Under our baseline discount rate of 50%, 38% of filed cases generate a negative expected return for the worker. As shown in Table OA-1, this share increases with the discount rate because the value of recoveries received farther in the future declines.

Cases relative to court administrative costs. The calculation of administrative costs asks a different question. Rather than incorporating the court's costs into the worker's private net present value, we compare the worker's expected net present value from litigation with the

present value of the administrative costs incurred by the court while processing the case. Let C denote the annual administrative cost of keeping a case active. For each case, we calculate the present value of these costs over the observed duration of the case, denoted by $NPV_i^C(r)$, using the same discount rate applied to the worker's expected recovery. A case is classified as low-value relative to court costs whenever $NPV_i(r) < NPV_i^C(r)$. The corresponding share of cases is therefore $\Pr(NPV_i(r) < NPV_i^C(r))$.

Using an annual administrative cost of $C = \text{MXN } 2,100$ and our baseline discount rate of 50%, 45% of filed cases have an expected net present value below the present value of the administrative costs required to process the case. Table OA-1 shows how this share varies under alternative discount-rate assumptions.

Table OA-1: Low-Value Litigation under Alternative Discount Rates

Discount rate	Negative worker NPV (1)	Recovery below court-cost NPV (2)
15%	36%	38%
25%	37%	40%
50%	38%	45%
75%	40%	52%
100%	42%	58%

Notes: The table reports the share of historical casefiles classified as low-value under alternative annual discount rates. Column (1) reports the share for which the worker's expected net present value from litigation is negative, assuming that workers retain 70% of the recovery and pay an upfront legal cost of MXN 2,000. Column (2) reports the share for which the worker's expected net present value from litigation is below the present value of the administrative costs incurred by the court over the duration of the case.

The distinction between the two calculations is important. The first measures the worker's private return to litigation and therefore incorporates the contingency fee, the worker's upfront legal costs, and the delay in receiving any recovery. The second compares the worker's private net present value with the present value of the administrative costs incurred by the court over the duration of the case. Because litigants do not bear these administrative costs, they do not enter the worker's private filing decision. This creates a wedge between cases that are privately worth filing and cases whose private value is low relative to the administrative resources they require from the court.

OA-B Classification of Legal Representatives

We classify the legal representatives observed in the data into three mutually exclusive categories: informal lawyers (“*coyotes*”), public lawyers, and formal private lawyers. The classification combines information from the two-month follow-up survey, administrative case files, and lists of lawyers independently identified as informal or public. Because no single source provides a complete measure of lawyer type, we construct an initial classification and then refine it using information on lawyers’ identities across cases.

B.1 Initial Classification

We begin with workers who reported filing a lawsuit in the two-month follow-up survey. Let S_i indicate that worker i reported filing a lawsuit.

To identify potential coyote representation, the survey asked workers how they found their private lawyer. We define an indicator, F_i , equal to one if the worker reported finding the lawyer at the entrance to, or inside the labor court. We capture the lawyers names from the matched case files. Importantly, workers were not asked to characterize these lawyers as informal. Instead, we use the location and manner of recruitment as a proxy for contact with a coyote.

We supplement this survey-based measure with a predetermined list of known coyotes. A lawyer is included in this seed list if the lawyer was identified as a coyote by court authorities or was commonly known to be a coyote. Let C_i^{seed} indicate that at least one representative in case i appears on this list.

The initial coyote indicator is therefore

$$C_i = S_i \times 1 \{F_i = 1 \text{ or } C_i^{\text{seed}} = 1\}.$$

We next identify cases involving a public lawyer. A case is initially classified as publicly represented only when three conditions are met: the worker reported filing a lawsuit, the worker reported using a public lawyer, and the administrative review of the case file independently identified the representative as a public lawyer. We also require that the case was not already classified as involving a coyote. Thus,

$$P_i = S_i \times 1 \{C_i = 0\} \times 1 \{P_i^{\text{survey}} = 1 \text{ and } P_i^{\text{admin}} = 1\},$$

Finally, we define formal private representation residually. Among workers who reported filing a lawsuit, a case is initially classified as formally represented if it was classified as neither informal nor public:

$$L_i = S_i \times 1 \{C_i = 0 \text{ and } P_i = 0\}.$$

At this stage, the classification follows the hierarchy: coyote > public > formal private.

B.2 Lawsuits Identified Only in Administrative Records

Some workers did not report filing a lawsuit in the two-month survey, but we subsequently located a case with matching name and characteristics in the administrative records. We identify these cases using

$$S_i^{\text{court}} = 1 \{S_i = 0 \text{ and } S_i^{\text{any}} = 1\},$$

where S_i^{any} indicates that a lawsuit was identified in either the follow-up survey or the administrative records. Conditional on the worker not reporting a lawsuit in the survey, $S_i^{\text{any}} = 1$ therefore identifies a lawsuit found through administrative data.

Because survey information on the type of representation is unavailable for these cases, we rely on administrative evidence. A case is classified as represented by coyotes if a representative appears on the coyote seed list. Among the remaining cases, it is classified as public if the administrative case-file review identifies a public lawyer. All remaining administrative lawsuits are classified as formal private. This procedure extends the initial classification to lawsuits found only in the administrative records while maintaining mutually exclusive categories.

B.3 Cross-Case Correction for Coyotes

The initial classification can misclassify a coyote as formal if the lawyer's identity or recruitment method is not sufficiently informative in a particular case. We therefore exploit repeated appearances of the same lawyer across cases.

Let E_i^C indicate that at least one lawyer appearing in case i was identified as a coyote in another survey observation. Starting from the initial classification, we reclassify a formally represented case as coyote when $E_i^C = 1$:

$$Coyote_i = \max \{C_i, E_i^C\}.$$

Whenever a case is reclassified as coyote, its formal-representation indicator is set to zero:

$$Formal_i^{(C)} = L_i \times 1 \{Coyote_i = 0\}.$$

This correction treats credible identification as a coyote as an attribute of the lawyer rather than of an individual case. Consequently, evidence that a lawyer operated as a coyote in one case can be used to correct the classification of other cases involving the same lawyer.

B.4 Cross-Case Identification of Public Lawyers

Public lawyers are not always explicitly recorded as such in every case in which they appear. To reduce the resulting measurement error, we construct a lawyer-level measure based on repeated appearances across case files.

We first define a case as providing direct evidence of public representation when both the worker's survey response and the administrative file indicate that the lawyer was public:

$$D_i^P = 1 \{P_i^{\text{survey}} = 1 \text{ and } P_i^{\text{admin}} = 1\}.$$

For each lawyer j , we then calculate

$$q_j = \frac{\sum_{i: j \in J_i} D_i^P}{\sum_{i: j \in J_i} 1},$$

where J_i denotes the set of named lawyers in case i . Thus, q_j is the share of the lawyer's observed case appearances for which the survey and administrative records jointly identify

public representation. We classify lawyer j as consistently public if

$$A_j^P = 1 \{q_j \geq 0.75\}.$$

The dataset may list as many as three lawyers for each case file. We classify case i as being predominantly associated with public lawyers when at least two-thirds of the named lawyers in the file are consistently public:

$$G_i^P = 1 \left\{ \frac{\sum_{j \in I_i} A_j^P}{|J_i|} \geq \frac{2}{3} \right\}.$$

In practice, this rule requires that the consistently public classification apply to one of one, two of two, or at least two of three named lawyers.

Starting from the initial public classification, we define the corrected public indicator as

$$Public_i = \max \{P_i, G_i^P\}.$$

The cross-case correction allows us to identify public representation even when the survey response or administrative notation is missing or inconsistent in a particular file, provided that the identities of the lawyers appearing in the case strongly indicate public representation.

B.5 Manual Validation and Resolution of Conflicting Classifications

We supplement the algorithmic procedure with a limited number of manual corrections for lawyers whose status as public representatives could be established with high confidence from a large number of casefiles. The code applies the same correction to five additional lawyers whose public status was similarly verified.

The cross-case corrections can occasionally classify the same case as both public and informal. We resolve these conflicts by prioritizing the public classification when the lawyer-level evidence provided a more definitive identification of the representative's institutional status. Accordingly, if $Public_i = 1$, we set both the coyote and formal indicators to zero. The final classification therefore follows the hierarchy public > coyote > formal private.

By construction, these categories are mutually exclusive. Among identified lawsuits with sufficient information for classification, formal private representation remains the residual category after cases involving public lawyers and coyotes have been identified.

Overall, the procedure combines case-specific reports with cross-case information about lawyers' identities. The initial classification favors direct agreement between survey and administrative sources, whereas the subsequent corrections use repeated lawyer appearances to address incomplete or inconsistent classification in individual files. This approach reduces the likelihood that the same lawyer is classified differently across cases solely because one file contains less complete information than another.

OA-C Coyote sources

Full CFCRL statement on *coyotaje*. The complete federal authority statement quoted in Section 2 reads (authors' translation): "*Coyotaje* is an act of illegal intermediation, in which an outside person interferes between citizens and public servants, seeking an economic benefit. The so-called 'coyotes' are also known as *gestores* or intermediaries. Where to find them? Outside government offices or courts. Watch out, worker! They deceive you and charge you for a procedure that is free. They carry out procedures in your name, putting your personal data at risk. They offer to speed up your procedure. . . in exchange for a payment. They advise you not to settle and to go to trial, with the promise of obtaining greater financial resources. Don't let yourself be deceived! The 'coyotes' will tell you: 'It's not in your interest to settle. Just request your *Constancia de No Conciliación*.' Don't listen to them! At the Federal Labor Center, all of these services are free. You don't have to pay anything for your application, hearing, or legal advice. Don't be fooled by outside promises. At the Federal Labor Center, we support you free of charge." Source: Centro Federal de Conciliación y Registro Laboral, <https://www.facebook.com/centrofcrl/videos/1117632750035898/>.

Coyote presence at the public attorney's office. PROFEDET has officially acknowledged the presence of intermediaries on the corners surrounding its offices who offer to take labor matters in exchange for payment.⁴¹ In its FAQ, PRODET emphasizes that services are personalized and must be obtained "sin la intervención de terceros que pudieran aprovecharse" ("without the intervention of third parties who might take advantage") of the worker's situation.⁴²

⁴¹PRODET Facebook post warning about coyotes (accessed November 2025); video on the same topic (accessed November 2025); statement by PRODET director Carolina Ortiz Porras emphasizing the free nature of services (gob.mx press release).

⁴²PRODET FAQ, accessed November 2025.

Junta Local de Conciliación y Arbitraje de la Ciudad de México
May 6 at 10:40 AM · 🌐

⚠️ Los "coyotes" buscan captar a los trabajadores que acuden a las instalaciones de los tribunales laborales y con engaños los vinculan a despachos ajenos a la dependencia en donde se cobran por una actividad que es gratuita. ⚠️



(a) JLCA Mexico City: official warning



(b) *Mural* (Guadalajara): press coverage



(c) PROFEDET: federal warning campaign



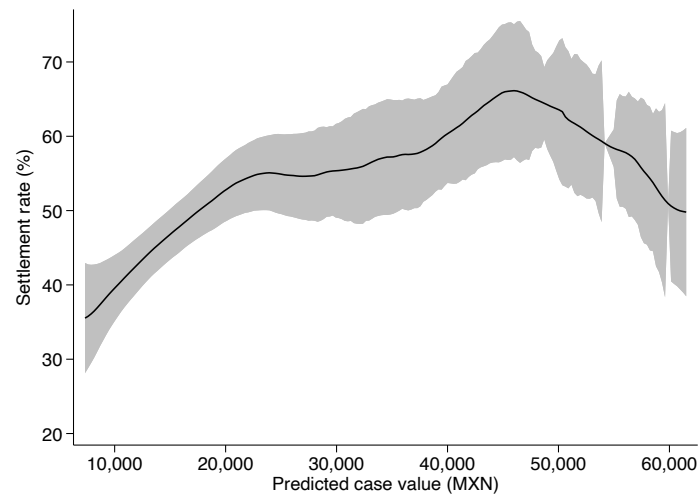
(d) CFCRL: how to identify a *coyote*

Figure OA-1: “Coyote” activity outside Mexican labor justice institutions

Notes: “Coyotes” are unauthorized intermediaries who solicit workers outside Mexican labor justice institutions and charge for services that federal agencies provide free of charge. PROFEDET and CFCRL characterize coyote conduct as (i) charging fees for filings, representation, and advice that the state provides at no cost; (ii) steering workers away from conciliation and toward litigation; and (iii) misusing workers’ personal information for identity theft or extortion. Panel (a): notice posted by the Junta Local de Conciliación y Arbitraje de la Ciudad de México (JLCA-CDMX) warning that “coyotes seek to recruit workers who come to the labor tribunals and, through deceit, link them to law firms outside the agency where they charge for an activity that is free” (Facebook, JuntaLocalCDMX). Panel (b): press photograph from *Mural* documenting individuals offering unauthorized labor advisory at the entrance to Guadalajara’s JLCA (*Mural*, December 6, 2021; photo by Jorge Rangel). Panel (c): PROFEDET public-information post (July 20, 2024) warning that “these coyotes do not belong to the canid family, but they are out looking for a worker to deceive” (Facebook, PROFEDET). Panel (d): CFCRL informational poster on how to identify *coyotes*: they “present themselves as *gestores* or intermediaries,” are located “in the vicinity of government offices or tribunals,” and “offer to intervene to ‘speed up’ your procedure in exchange for a payment” (Centro Federal de Conciliación y Registro Laboral, Secretaría del Trabajo y Previsión Social). Translations are the authors’.

OA-D Figures

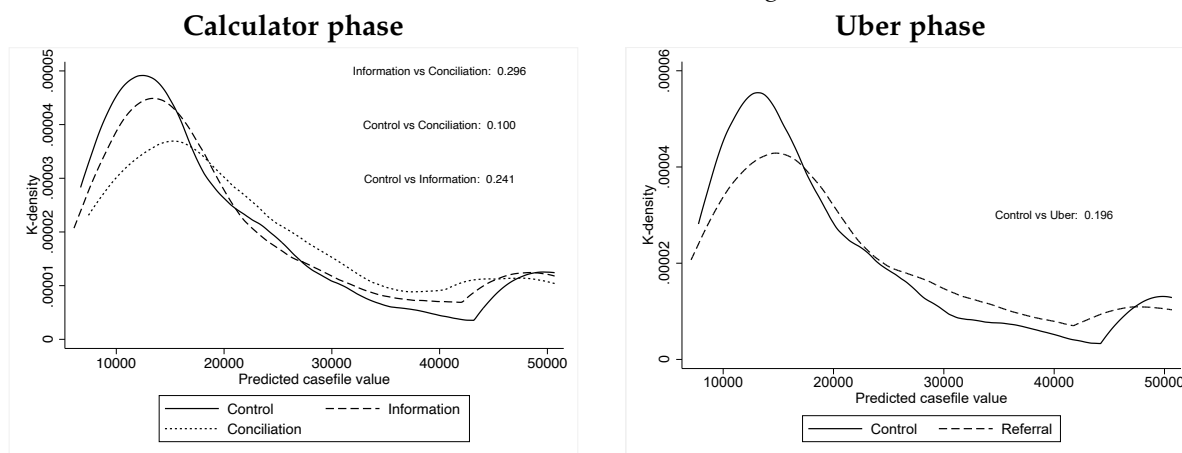
Figure OA-2: Settlement Rates by Predicted Case Value



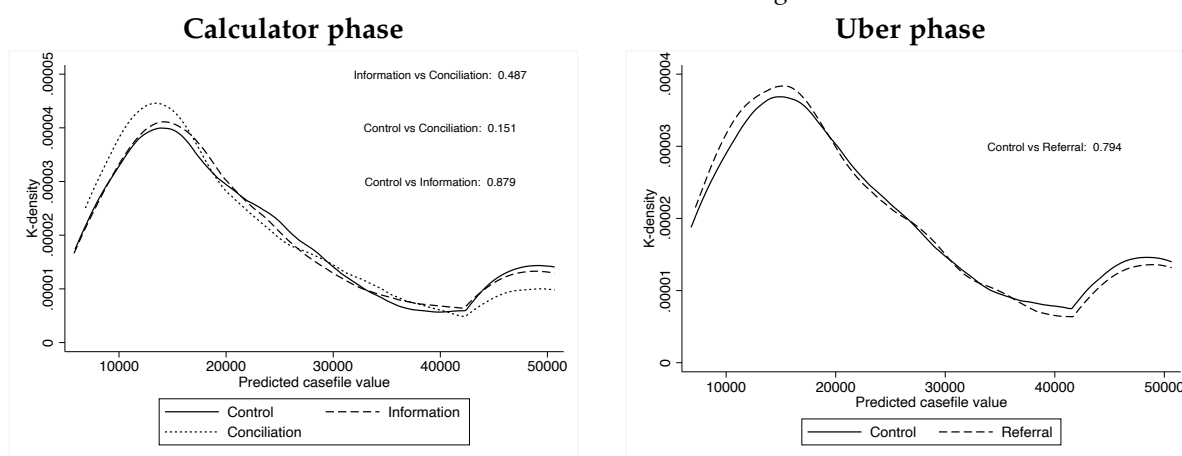
Notes: The figure plots settlement rates against predicted case value among workers assigned to the control groups in the Calculator and Uber phases. Predicted case values are obtained from a random forest with 1,000 trees, expressed in January 2017 MXN, and winsorized at the upper 5%. The figure is estimated in Stata using `lpoly`: the solid line is a local-linear polynomial estimate using an Epanechnikov kernel and a bandwidth of MXN 5,000, evaluated at 200 equally spaced points. The shaded area reports pointwise 95% confidence intervals. Observations below the 1st percentile or above the 99th percentile of predicted case value in the pooled estimation sample are excluded.

Figure OA-3: Distributions of predicted case value by treatment arm, conditional on litigation outcome

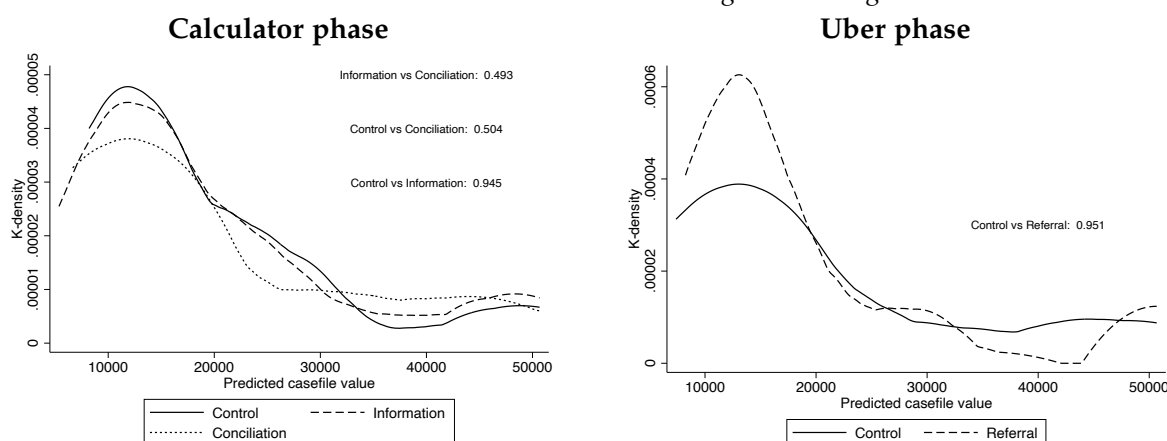
Panel A. Conditional on suing



Panel B. Conditional on settling

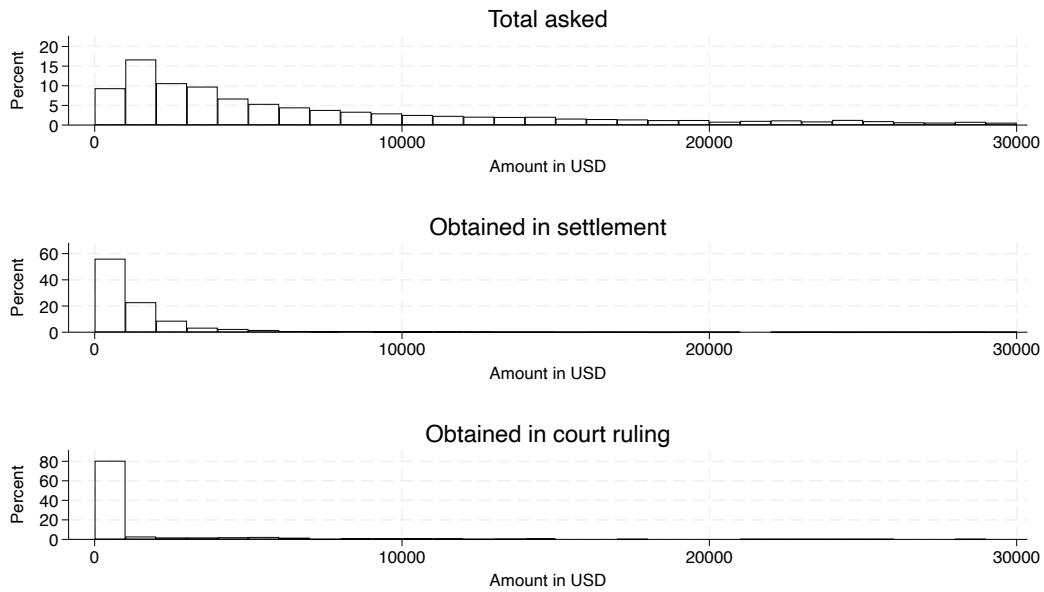


Panel C. Conditional on neither suing nor settling



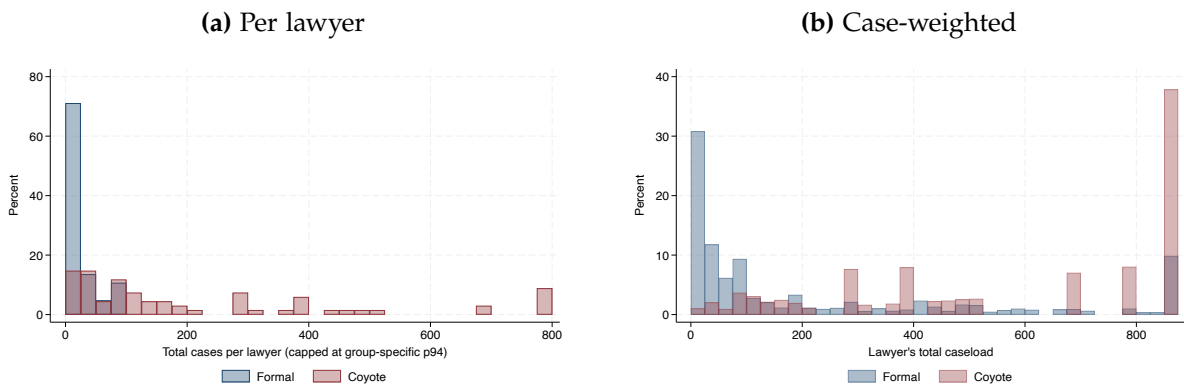
Kernel densities of the predicted case value, by treatment arm, within each treatment phase; left columns show the Calculator phase (Control, Information, Conciliation) and right columns the Uber phase (Control, Referral). Panel A conditions on workers who filed a lawsuit by the two-month survey; Panel B on workers who reported settling in the two-week or two-month follow-up surveys; Panel C on workers who neither filed a lawsuit nor reached an agreement with their employer by the two-month survey. Predicted case values are obtained from a random forest trained on 5,000 completed casefiles using the worker's daily wage, tenure, gender, and industry; wage and tenure are highly predictive because they enter the statutory severance formula. See [Sadka et al. \(2024\)](#) for details. Kolmogorov-Smirnov p -values for pairwise distributional equality are reported within each panel.

Figure OA-4: Distribution of amounts asked and obtained when suing



The figure compares the distributions of amounts claimed by workers with the distributions of amounts ultimately obtained through settlement and through a court ruling using an administrative dataset of 5,000 completed casefiles. Amounts are expressed in January 2017 USD (MXN 13 = USD 21.3853). Histograms are plotted using USD 1,000 bins and are restricted to observations below USD 30,000 to improve readability. The vertical axis reports the percentage of cases in each bin.

Figure OA-5: Distribution of caseloads



Notes: Both panels are based on a sample of 5,005 historical case files. Lawyers are classified as formal or coyote and public ones are excluded throughout. Panel A: for each lawyer appearing in the sample, we count the total number of cases they handle and scale that count to reflect the court's full caseload of approximately 100,000 (stock of) cases. Each lawyer appears once. To improve readability, caseloads are capped at the 95th percentile separately for coyotes and formal lawyers. Panel B: We disaggregate the sample of 5,005 case files to the case-lawyer level, yielding one observation per case-lawyer pair, as there can be up to three lawyers per case. For each observation, we count the total number of cases that the corresponding lawyer handles across the full sample, that is, the number of rows sharing the same lawyer name, and assign that count as the lawyer's caseload. The histogram is then built over all case-lawyer pairs, meaning lawyers with larger caseloads contribute more observations. The width of the bars in both histograms is 35.

OA-E Tables

Table OA-2: Attrition

	2 weeks (1)	2 months (2)	2w + 2m (3)
<i>Panel A: Calculator Phase</i>			
Information	0.05* (0.02)	0.03 (0.02)	0.03 (0.03)
Conciliation	0.06** (0.02)	0.03 (0.03)	0.04 (0.03)
Constant	0.82*** (0.01)	0.79*** (0.01)	0.72*** (0.02)
Observations	1,972	1,972	1,972
R-squared	0.005	0.001	0.002
Dep. Var. Mean	0.82	0.79	0.72
<i>Panel B: Uber Phase</i>			
Referral	0.01 (0.02)	0.02 (0.02)	0.02 (0.02)
Constant	0.90*** (0.01)	0.84*** (0.02)	0.81*** (0.02)
Observations	1,289	1,289	1,289
R-squared	0.000	0.001	0.000
Dep. Var. Mean	0.90	0.84	0.81

The table shows in each column a regression of indicator variables equal to 1 if a worker answered the two-week survey (1), the two-month survey (2) and both surveys (3) on treatment for the Calculator and Uber phases. The dependent variable mean reported in each column corresponds to the control group only. Standard errors clustered at the day level in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table OA-3: Balance table (Baseline data)

	Balance				
	Gender	Tenure	Daily Wage	Pr(Win) baseline	Don't know Amt Win
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Calculator Phase</i>					
Information	-0.01 (0.03)	0.17 (0.25)	-35.59 (24.46)	-0.01 (0.01)	-0.03 (0.03)
Conciliation	0.01 (0.03)	0.02 (0.36)	-15.85 (30.28)	-0.01 (0.01)	-0.07 (0.03)
Observations	1,972	1,964	1,968	1,711	1,972
R-squared	0.000	0.000	0.001	0.002	0.003
Dep. Var. Mean	0.46	3.51	333.56	0.89	0.60
<i>Panel B: Uber Phase</i>					
Referral	-0.05 (0.03)	-0.18 (0.36)	10.29 (20.57)	0.02* (0.01)	-0.00 (0.04)
Observations	1,289	1,287	1,288	1,133	1,289
R-squared	0.002	0.000	0.000	0.006	0.000
Dep. Var. Mean	0.49	3.81	323.95	0.88	0.50
P-val equality of means	0.22	0.26	0.70	0.24	0.00

The table reports, in each column, coefficients from regressions of BVC (gender, tenure, and wage) in columns (1)–(3), the subjective probability of winning reported in the baseline survey in column (4), and an indicator equal to one if the worker does not know the amount she expects to receive if she wins the case in column (5), on treatment assignment during the Calculator and Uber phases. All regressions include an intercept term, which is omitted from the table. The dependent variable mean reported in each column corresponds to the control group only. The bottom row reports the p-value from a two-sample t-test of equality of means between the control group of the Calculator-phase regressions (Panel A) and the control group of the Uber-phase regressions (Panel B), restricted to the exact observations used in each corresponding regression. Standard errors clustered at the day level in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table OA-4: Comparing phase 1 vs phase 2 Control groups balance

	Calculator Control	Uber Control	p-value
Woman	0.46	0.49	0.22
Probability Winning	0.89	0.88	0.24
Daily Wage	333.56	323.95	0.70
Tenure	3.51	3.81	0.26
Production & Infrastructure	0.08	0.05	0.02
Commerce & Business	0.69	0.71	0.44
Social & Personal Services	0.23	0.24	0.62
Observations	817	705	

This table compares control groups for the Calculator and Uber stages. It shows the means and the p-values of a t-test for the null hypothesis of equality of means. Industries—Production & Infrastructure, Commerce & Business, Social & Personal Services—are built using NAICS code, according to the industry respondents reported.

Table OA-5: Who uses Coyotes?

	Talked to Coyote			Sued with Coyote		
	(1)	(2)	(3)	(4)	(5)	(6)
25th–50th percentile	-0.05 (0.02) [0.035]	-0.05 (0.02) [0.036]	-0.08 (0.03) [0.019]	-0.05 (0.03) [0.086]	-0.05 (0.03) [0.092]	-0.12 (0.05) [0.019]
50th–75th percentile	-0.07 (0.02) [0.001]	-0.07 (0.02) [0.002]	-0.11 (0.03) [0.001]	-0.11 (0.03) [0.000]	-0.12 (0.03) [0.000]	-0.21 (0.05) [0.000]
Top 25%	-0.10 (0.02) [0.000]	-0.10 (0.02) [0.000]	-0.14 (0.03) [0.000]	-0.14 (0.03) [0.000]	-0.14 (0.03) [0.000]	-0.24 (0.05) [0.000]
High School		-0.03 (0.02) [0.045]	-0.06 (0.02) [0.013]		-0.04 (0.02) [0.075]	-0.05 (0.04) [0.230]
Observations	1,289	1,275	875	1,228	1,139	546
R-squared	0.023	0.028	0.046	0.029	0.034	0.054
Dep. Var. Mean	0.06	0.06	0.09	0.11	0.11	0.24

Notes: The table reports correlates of talking to or suing with an informal lawyer among workers assigned to the control group in either phase (Calculator- and Uber-phase control groups pooled). Columns (1)–(3) use the two-week follow-up sample and report whether the worker talked to a coyote. Columns (1)–(2) use the full two-week follow-up sample, while column (3) restricts the sample to workers who reported talking to a lawyer. Columns (4)–(6) use the two-month follow-up sample and report whether the worker sued with a coyote. Columns (4)–(5) use the full two-month follow-up sample, while column (6) restricts the sample to workers who filed a lawsuit. Predicted case-value quartiles are defined using the pooled distribution of predicted case values among control-group workers, before restricting the sample by survey response. The omitted category is the bottom quartile (Bottom 25%). *High School* is an indicator equal to one if the respondent attained at least a high school level of education. All regressions include an intercept, which is not reported. Standard errors, clustered at the day-of-entry level, are reported in parentheses; asymptotic *p*-values are reported in brackets.

Table OA-6: Treatment Effects on Settlement Amounts Relative to Predicted Case Value

	Ratio Settlement/ Pred. Court Value (2)	Ratio Settlement/ Pred. Value (3)
Information	-0.216 (0.150) [0.239]	-0.042 (0.069) [0.557]
Conciliation	-0.023 (0.229) [0.923]	-0.036 (0.081) [0.639]
RI <i>p</i> -value: Information = Conciliation	0.330	0.931
Control mean	1.211	1.007
BVC	✓	✓
Observations	540	540
R-squared	0.025	0.053
Referral	0.042 (0.112) [0.715]	0.007 (0.069) [0.922]
Control mean	0.982	0.998
BVC	✓	✓
Observations	458	458
R-squared	0.093	0.383

Notes: This table reports treatment effects on settlement outcomes. Column (1) reports the settlement amount in January 2017 USD assigning a value of zero to workers who did not settle. This outcome is therefore not conditional on settlement and captures both the extensive margin of whether a worker settles and the intensive margin of how much is obtained through settlement. Columns (2) and (3) report the settlement amount relative to predicted court-award value and predicted case value, respectively. Because these ratios are only defined when a settlement amount is observed, these specifications should be interpreted as conditional on observing a settlement amount. The Calculator sample includes workers assigned to the Control, Information, and Conciliation groups, while the Uber sample includes workers assigned to the Control and Referral groups. All specifications include BVC and cluster standard errors at the date-of-entry level. Randomization-inference *p*-values are reported in brackets below the corresponding standard errors. For the Calculator sample, the table additionally reports the randomization-inference *p*-value for the test of equality between the Information and Conciliation treatment effects. Settlement amounts and predicted case values are expressed in January 2017 prices.

For Table OA-6, we restrict the analysis to workers who responded to the two-month follow-up survey. We also exclude cases resolved through reinstatement, since these cases do not involve monetary settlement payments. In the Calculator phase, the sample includes 1,519 observations after applying these filters. The corresponding sample for the Uber phase contains 1,072 workers after applying the restrictions.

We distinguish between workers classified as having reached a settlement and those who report the corresponding monetary amount. In the Calculator phase, 806 of the 1,519 workers are classified as having settled their disputes. Of these, 578 (71.7%) report a settlement amount. In the Uber phase, 531 of the 1,072 workers are classified as having settled, of whom 462 (87.0%) report an amount. Reported amounts include zero values: 35 in Calculator and one in Uber.

Table OA-6 reports two outcomes. They measure the reported settlement amount relative to the predicted court value and predicted case value, respectively. Both predictions are expressed in January 2017 USD and winsorized at the upper 5% tail. These ratios require an observed, nonzero settlement amount and a valid, nonzero predicted value. Consequently, workers reporting settlement amounts equal to zero are excluded from these specifications.

These specifications contain 540 and 458 observations, respectively. Regressions additionally require non-missing baseline controls. Thus, differences in regression sample sizes should not be interpreted directly as settlement amount non-response rates.

Remarkably, filing a lawsuit and reaching a settlement are not mutually exclusive. Workers who file lawsuits without receiving settlement payments may therefore have a settlement amount of zero. Finally, specifications condition on observed settlement amounts. Their estimates may thus be affected by treatment-induced selection into settlement and amount reporting.

Table OA-8: Treatment effect on talking and suing with controlling for case value deciles

	Talked to (2w)				Sued with (2m)			
	Any (1)	Coyote (2)	Formal (3)	Public (4)	Any (5)	Coyote (6)	Formal (7)	Public (8)
<i>Panel A: Calculator Phase</i>								
Information	-0.01 (0.03) [0.642]	-0.00 (0.01) [0.836]	0.10 (0.03) [0.000]	-0.12 (0.03) [0.000]	-0.05 (0.03) [0.102]	-0.04 (0.02) [0.030]	0.04 (0.02) [0.098]	-0.05 (0.02) [0.012]
Conciliation	-0.21 (0.03) [0.000]	-0.03 (0.01) [0.011]	0.01 (0.03) [0.650]	-0.26 (0.03) [0.000]	-0.11 (0.04) [0.005]	-0.05 (0.02) [0.010]	0.01 (0.03) [0.730]	-0.07 (0.02) [0.002]
Observations	1,649	1,649	1,649	1,649	1,562	1,562	1,562	1,562
R-squared	0.037	0.009	0.032	0.062	0.010	0.013	0.008	0.013
Dep. Var. Mean	0.62	0.05	0.29	0.37	0.46	0.12	0.18	0.15
Case-value deciles	✓	✓	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI (Information)	0.710	0.827	0.000	0.001	0.150	0.028	0.139	0.022
p-value RI (Conciliation)	0.000	0.012	0.664	0.000	0.003	0.015	0.717	0.002
<i>Panel B: Uber Phase</i>								
Referral	0.07 (0.03) [0.011]	-0.03 (0.01) [0.016]	-0.09 (0.03) [0.001]	0.20 (0.03) [0.000]	-0.05 (0.04) [0.189]	-0.02 (0.02) [0.298]	-0.05 (0.03) [0.055]	0.03 (0.02) [0.224]
Observations	1,156	1,156	1,156	1,156	1,097	1,097	1,097	1,097
R-squared	0.012	0.021	0.032	0.046	0.017	0.028	0.020	0.015
Dep. Var. Mean	0.75	0.07	0.33	0.49	0.48	0.10	0.25	0.13
Case-value deciles	✓	✓	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI	0.004	0.032	0.000	0.000	0.204	0.334	0.054	0.204

Notes: The dependent variables and regression specifications are the same as in Table 4, except for the set of controls. These regressions include decile indicators for predicted case value, $\hat{x}_i$, and omit daily wage and tenure, as both are correlated with $\hat{x}_i$. Bottom decile is omitted.

Table OA-7: Heterogeneous Treatment Effects on Settlement and Suing by Predicted Case Value

	Calculator phase		Uber phase	
	Settlement (1)	Suing (2)	Settlement (3)	Suing (4)
Information	0.009 (0.045) [0.834]	-0.036 (0.043) [0.402]		
Conciliation	0.073 (0.054) [0.176]	-0.018 (0.048) [0.711]		
Bottom 50%	-0.135 (0.048) [0.005]	0.094 (0.049) [0.057]	-0.188 (0.048) [0.000]	0.135 (0.051) [0.009]
Information × Bottom 50%	0.041 (0.062) [0.512]	-0.022 (0.063) [0.725]		
Conciliation × Bottom 50%	0.169 (0.072) [0.020]	-0.177 (0.065) [0.007]		
Referral			0.016 (0.048) [0.746]	0.008 (0.048) [0.870]
Referral × Bottom 50%			0.096 (0.062) [0.124]	-0.113 (0.063) [0.074]
<i>Linear combinations: Calculator phase</i>				
Information effect: Bottom 50%	0.050 (0.042) [0.304]	-0.058 (0.044) [0.230]		
Conciliation effect: Bottom 50%	0.241 (0.051) [0.000]	-0.195 (0.051) [0.000]		
<i>Linear combinations: Uber phase</i>				
Referral effect: Bottom 50%			0.111 (0.051) [0.031]	-0.105 (0.049) [0.047]
RI p-value: Bottom 50% = Top 50% (Information)	0.554	0.737		
RI p-value: Bottom 50% = Top 50% (Conciliation)	0.028	0.014		
RI p-value: Bottom 50% = Top 50% (Referral)			0.143	0.081
Observations	1,562	1,562	1,097	1,097
R-squared	0.028	0.014	0.023	0.010
Dep. Var. Mean	0.497	0.456	0.476	0.481

Notes: Columns (1)–(2) use the Calculator-phase sample, and columns (3)–(4) use the Uber-phase sample. Each column reports a worker-level OLS regression of the form $Y_i = \alpha + \sum_k \beta_k T_{i,k} + \delta B_i + \sum_k \theta_k (T_{i,k} \times B_i) + X_i' \gamma + \varepsilon_i$, where Y_i indicates either that the conflict was resolved through a settlement (*Settlement*) or that the worker filed a lawsuit (*Suing*); $T_{i,k}$ denotes the treatment indicators; and B_i is an indicator for cases in the bottom 50% of the phase-specific predicted-value distribution. Settlement includes agreements reached after a lawsuit was filed, so the outcomes (and hence the columns within each phase) are not mutually exclusive. The control group and the top 50% of the predicted-value distribution are the omitted categories. Thus, the reported treatment coefficients correspond to treatment effects in the top 50%, while the interaction coefficients capture the difference in treatment effects between the bottom and top halves of the distribution. The lower panel reports the total treatment effects in the bottom 50%, obtained as the sum of the corresponding treatment and interaction coefficients. All specifications include the baseline controls. Standard errors clustered at the randomization-day level are reported in parentheses. In the main regression panel, the p -values reported in brackets are asymptotic p -values based on the day-clustered variance estimator. In the lower panel, the p -values reported in brackets for the total treatment effects in the bottom 50% are randomization-inference p -values. The rows labeled “RI p -value: Bottom 50% = Top 50%” report randomization-inference tests of equality between the treatment effects in the two halves of the predicted-value distribution. Randomization inference uses 1,000 replications and randomizes treatment assignments at the randomization-day level.

Table OA-9: Lawyer quality: school, desire to change lawyers, and satisfaction controlling for case value deciles

	Share high-quality school	Wants to change lawyer			Satisfaction w. lawyer
	QS	No	Yes	Don't know	Rating
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Calculator Phase</i>					
Information	0.10 (0.04) [0.010]	0.16 (0.05) [0.002]	-0.07 (0.04) [0.045]	-0.08 (0.05) [0.099]	0.39 (0.23) [0.095]
Conciliation	0.03 (0.04) [0.494]	0.18 (0.06) [0.002]	-0.12 (0.04) [0.001]	-0.06 (0.06) [0.246]	0.61 (0.25) [0.014]
Observations	420	552	552	552	371
R-squared	0.029	0.056	0.029	0.029	0.064
Dep. Var. Mean	0.32	0.39	0.21	0.40	8.16
Case-value deciles	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓
p-value RI (Information)	0.009	0.004	0.056	0.104	0.096
p-value RI (Conciliation)	0.521	0.002	0.005	0.251	0.020
p-value: Info. = Conc.	0.142	0.679	0.279	0.762	0.376
<i>Panel B: Uber Phase</i>					
Referral	-0.07 (0.04) [0.095]	-0.01 (0.04) [0.739]	0.01 (0.04) [0.860]	0.01 (0.04) [0.850]	-0.18 (0.17) [0.288]
Observations	301	447	447	447	354
R-squared	0.033	0.024	0.027	0.037	0.027
Dep. Var. Mean	0.42	0.58	0.21	0.21	8.76
Case-value deciles	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓
p-value RI	0.098	0.738	0.864	0.842	0.290

This table is analogue to table 5 controlling for case-value deciles instead of controls (BVC).

Table OA-10: Heterogeneous Treatment Effects on Settlement by Daily Wage

	Sued	Settled	Happiness	Stopped paying services	Lack of money	Currently working
	(1)	(2)	(3)	(4)	(5)	(6)
Information	-0.021 (0.034) [0.532]	0.007 (0.032) [0.822]	-0.028 (0.155) [0.855]	-0.054 (0.033) [0.098]	-0.048 (0.033) [0.144]	0.013 (0.031) [0.683]
Conciliation	-0.086 (0.040) [0.033]	0.104 (0.033) [0.002]	0.116 (0.145) [0.424]	-0.045 (0.044) [0.311]	-0.082 (0.043) [0.056]	-0.003 (0.038) [0.934]
Bottom 25% Daily wage	0.086 (0.043) [0.046]	-0.060 (0.044) [0.169]	-0.232 (0.242) [0.339]	0.068 (0.045) [0.132]	0.100 (0.053) [0.061]	-0.010 (0.042) [0.814]
Information × Bottom 25% Daily wage	-0.097 (0.060) [0.106]	0.052 (0.065) [0.429]	0.350 (0.337) [0.301]	-0.025 (0.066) [0.712]	-0.085 (0.072) [0.240]	-0.023 (0.062) [0.706]
Conciliation × Bottom 25% Daily wage	-0.074 (0.088) [0.404]	0.059 (0.077) [0.448]	0.316 (0.337) [0.350]	-0.040 (0.073) [0.584]	-0.008 (0.083) [0.919]	0.015 (0.071) [0.835]
Information effect: Bottom 25%	-0.118 (0.054) [0.083]	0.059 (0.057) [0.377]	0.321 (0.298) [0.297]	-0.079 (0.055) [0.187]	-0.133 (0.060) [0.048]	-0.011 (0.058) [0.823]
Conciliation effect: Bottom 25%	-0.160 (0.083) [0.030]	0.163 (0.070) [0.017]	0.432 (0.322) [0.194]	-0.085 (0.063) [0.169]	-0.090 (0.069) [0.203]	0.012 (0.065) [0.853]
RI p-value: Bottom 25% = Rest (Information)	0.177	0.488	0.286	0.745	0.251	0.707
RI p-value: Bottom 25% = Rest (Conciliation)	0.337	0.484	0.393	0.594	0.915	0.863
Observations	1,562	1,562	1,544	1,557	1,559	1,562
R-squared	0.010	0.016	0.011	0.009	0.012	0.026
Control mean	0.456	0.647	7.886	0.594	0.559	0.456

Notes: The table reports heterogeneous treatment effects by daily wage in the Calculator experiment. The Bottom 25% group consists of workers whose daily wage is below the 25th percentile of the daily-wage distribution in the analysis sample; the omitted group is the remaining 75% of workers. All specifications include the baseline-value controls and standard errors are clustered by date of entry. For each coefficient, standard errors are reported in parentheses and randomization-inference p-values in brackets, clustered at the date-of-entry level. The rows “Information effect: Bottom 25%” and “Conciliation effect: Bottom 25%” report the corresponding linear combinations of the treatment coefficient and its interaction with the Bottom 25% indicator. The RI p-values for “Bottom 25% = Rest” test the null hypothesis that the treatment effect is equal for workers in the Bottom 25% and for the rest of the sample. The sample is restricted to respondents to the two-month follow-up survey in the Calculator phase.

OA-F Robustness Checks using intersection Samples

This section reports additional versions of the tables in using alternative estimation samples. In particular, we condition on having answered both the two-month survey and the two-week survey. These estimations allow us to assess whether the results are sensitive to the particular sample used in each specification.

Table OA-11: Treatment effect on case ending, conditioning on two-week and two-month survey

	Settle (1)	Sued (2)	Dropped (3)
<i>Panel A: Calculator Phase</i>			
Information	0.03 (0.03) [0.309]	-0.05 (0.03) [0.092]	0.01 (0.02) [0.702]
Conciliation	0.15 (0.04) [0.000]	-0.11 (0.04) [0.005]	-0.02 (0.02) [0.332]
Observations	1,435	1,435	1,435
R-squared	0.016	0.008	0.003
Dep. Var. Mean	0.50	0.47	0.09
BVC	✓	✓	✓
p-value RI (Information)	0.351	0.125	0.699
p-value RI (Conciliation)	0.000	0.003	0.331
p-value (Information = Conciliation)	0.004	0.122	0.229
<i>Panel B: Uber Phase</i>			
Referral	0.07 (0.04) [0.093]	-0.06 (0.04) [0.121]	0.01 (0.02) [0.795]
Observations	1,054	1,054	1,054
R-squared	0.007	0.003	0.003
Dep. Var. Mean	0.48	0.49	0.08
BVC	✓	✓	✓
p-value RI	0.086	0.116	0.809

Notes: This table is analogous to table 3, except that it uses a sample that conditions on having answered both the two-week and two-month survey.

Table OA-12: Treatment effect on talking and suing with different lawyer types, conditioning on having answered both two-week and two-month surveys.

	Talked to				Sued with			
	Any (1)	Coyote (2)	Formal (3)	Public (4)	Any (5)	Coyote (6)	Formal (7)	Public (8)
<i>Panel A: Calculator Phase</i>								
Information	-0.01 (0.03) [0.688]	-0.01 (0.01) [0.415]	0.11 (0.03) [0.000]	-0.13 (0.03) [0.000]	-0.05 (0.03) [0.092]	-0.04 (0.02) [0.024]	0.04 (0.02) [0.080]	-0.05 (0.02) [0.014]
Conciliation	-0.20 (0.03) [0.000]	-0.04 (0.01) [0.004]	0.03 (0.03) [0.380]	-0.27 (0.03) [0.000]	-0.11 (0.04) [0.005]	-0.05 (0.02) [0.018]	0.02 (0.03) [0.582]	-0.08 (0.02) [0.002]
Observations	1,435	1,435	1,435	1,435	1,435	1,435	1,435	1,435
R-squared	0.028	0.009	0.016	0.059	0.008	0.007	0.004	0.011
Dep. Var. Mean	0.62	0.06	0.29	0.38	0.47	0.13	0.18	0.16
BVC	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI (Information)	0.734	0.436	0.001	0.001	0.125	0.027	0.093	0.024
p-value RI (Conciliation)	0.000	0.007	0.391	0.000	0.003	0.027	0.566	0.003
<i>Panel B: Uber Phase</i>								
Referral	0.07 (0.03) [0.015]	-0.03 (0.01) [0.025]	-0.10 (0.03) [0.001]	0.20 (0.03) [0.000]	-0.06 (0.04) [0.121]	-0.02 (0.02) [0.286]	-0.06 (0.03) [0.024]	0.03 (0.02) [0.203]
Observations	1,054	1,054	1,054	1,054	1,054	1,054	1,054	1,054
R-squared	0.012	0.010	0.021	0.045	0.003	0.003	0.011	0.004
Dep. Var. Mean	0.76	0.06	0.33	0.51	0.49	0.10	0.25	0.14
BVC	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI	0.013	0.025	0.001	0.000	0.116	0.300	0.040	0.199

Notes: This table is analogous to table 4, except that it uses a sample that conditions on having answered both the two-week and two-month survey for all columns.

Table OA-13: Treatment effect on talking and suing with controlling for case value deciles, conditioning on having answered both two-week and two-month surveys.

	Talked to				Sued with			
	Any (1)	Coyote (2)	Formal (3)	Public (4)	Any (5)	Coyote (6)	Formal (7)	Public (8)
<i>Panel A: Calculator Phase</i>								
Information	-0.02 (0.03) [0.607]	-0.01 (0.01) [0.491]	0.10 (0.03) [0.001]	-0.13 (0.03) [0.000]	-0.05 (0.03) [0.108]	-0.04 (0.02) [0.034]	0.04 (0.02) [0.124]	-0.05 (0.02) [0.024]
Conciliation	-0.20 (0.03) [0.000]	-0.04 (0.01) [0.006]	0.02 (0.03) [0.532]	-0.27 (0.03) [0.000]	-0.11 (0.04) [0.007]	-0.05 (0.02) [0.029]	0.02 (0.03) [0.611]	-0.08 (0.02) [0.002]
Observations	1,435	1,435	1,435	1,435	1,435	1,435	1,435	1,435
R-squared	0.582	0.058	0.359	0.315	0.428	0.112	0.210	0.149
Dep. Var. Mean	0.62	0.06	0.29	0.38	0.47	0.13	0.18	0.16
Deciles	✓	✓	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI (Information)	0.655	0.501	0.002	0.000	0.133	0.036	0.135	0.034
p-value RI (Conciliation)	0.000	0.008	0.575	0.000	0.005	0.035	0.580	0.002
<i>Panel B: Uber Phase</i>								
Referral	0.07 (0.03) [0.011]	-0.03 (0.01) [0.027]	-0.10 (0.03) [0.001]	0.20 (0.03) [0.000]	-0.06 (0.04) [0.109]	-0.02 (0.02) [0.335]	-0.07 (0.03) [0.017]	0.03 (0.02) [0.207]
Observations	1,051	1,051	1,051	1,051	1,051	1,051	1,051	1,051
R-squared	0.796	0.077	0.313	0.617	0.469	0.102	0.236	0.160
Dep. Var. Mean	0.76	0.06	0.33	0.51	0.49	0.10	0.25	0.14
Deciles	✓	✓	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓	✓	✓
p-value RI	0.011	0.031	0.000	0.000	0.113	0.355	0.032	0.206

Notes: This table is analogous to table OA-8, except that it uses a sample that conditions on having answered both the two-week survey and the two-month survey for all columns.

Table OA-14: Post-Treatment Expectation Updating, conditioning on having answered both surveys

	Probability			Amount		
	Dummy (lowered) (1)	Continuous (2)	Don't Know (3)	Dummy (lowered) (4)	Continuous (5)	Don't Know (6)
Information	0.31*** (0.03)	-0.04* (0.02)	-0.05* (0.02)	0.26*** (0.03)	4640.67 (4420.03)	-0.18*** (0.04)
Conciliation	0.29*** (0.03)	-0.05*** (0.01)	-0.05* (0.02)	0.26*** (0.04)	18317.23 (15449.31)	-0.20*** (0.04)
Observations	1,217	1,217	1,435	556	556	1,435
R-squared	0.151	0.008	0.008	0.140	0.022	0.055
Dep. Var. Mean	0.00	0.00	0.14	0.00	0.00	0.58
p-value RI (Information)	0.00	0.01	0.03	0.00	0.68	0.00
p-value RI (Conciliation)	0.00	0.01	0.04	0.00	0.04	0.00

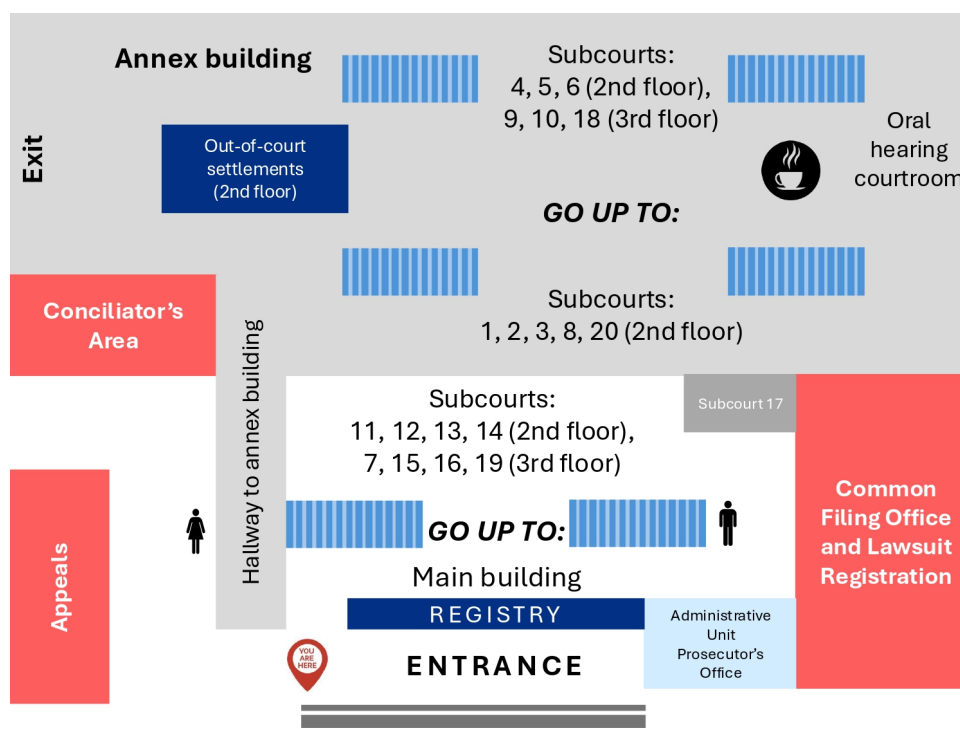
This table is analogous to table 2, but conditioning on having answered both surveys.

OA-G Material

Figure OA-6: Information Booth



Figure OA-7: Flyer about legal rights after unjustified dismissal



What am I entitled to if I am unjustly dismissed?

- **Severance pay = 90 days of salary**
- **Year-end bonus = Pro-rated for this year**
- **Vacation = Pro-rated for the last year worked, plus vacation bonus of 25%**
- **Tenure bonus = 12 days per year worked, up to 205 pesos per day.**

**The total amount is the legal entitlement...
IF YOU WIN THE LAWSUIT**

Figure OA-8: Detailed information about legal rights and the legal process used in the Calculator phase

YOUR LABOR RIGHTS AND THE LAWSUIT PROCESS




FREE INFORMATION BOOTH – JLCA CDMX

Dismissal

In Mexico, the law recognizes two types of dismissal:

- **Unjustified:** You are entitled to **severance pay** and any **benefits** the employer owes you.
- **Justified:** You are only entitled to the **benefits** the employer owes you.

FREE INFORMATION BOOTH – JLCA CDMX

What is justified dismissal?

A dismissal is justified if the employer proves the worker committed one of the following offenses:

a) Deceiving	g) Missing work three times within 30 days
b) Dishonest or violent behavior	h) Disobeying direct orders
c) Damage to materials or the workplace	i) Showing up drunk or under the influence of drugs
d) Endangering workplace safety	j) Being incarcerated
e) Sexual harassment	k) Lacking necessary documents
f) Disclosure of confidential information	*Article 47 of the Federal Labor Law

FREE INFORMATION BOOTH – JLCA CDMX

In practice...

- It is difficult for an employer to legally prove a justified dismissal.
- Therefore, **the outcome of the trial depends on whether unjustified dismissal is proven.**
- However, proving unjustified dismissal is not easy.
- Thus, without a court ruling, you **CANNOT claim the full amount as if you had won the lawsuit.**

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What you are entitled in case of unjustified dismissal

Severance pay (Constitutional)
Proportional year-end bonus
Proportional Vacations
Vacation bonus
Earned wages
Tenure bonus

FREE INFORMATION BOOTH – JLCA CDMX

Benefits explanation

1. **Severance pay:** 90 days of your wage.
2. **Year-end bonus:** 15 days per year.
3. **Vacation:** 6 days per year, increases by 2 days annually.
4. **Vacation bonus:** 25% extra per vacation day.
5. **Earned wages:** unpaid days already worked.
6. **Tenure bonus:** 12 days per year worked, capped at 160 pesos/day.
 - More than 15 years: paid regardless of dismissal type.
 - Less than 15 years: paid only if unjustified dismissal is proven.

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Additional Benefits

7. **Overtime pay:** based on weekly hours worked.
8. **20 days compensation:** if (a) you are an at will worker or (b) you request reinstatement and the employer refuses.
9. **Extra-legal benefits (e.g. vouchers):** you must prove you received them.
10. **Back pay:** daily wage x trial days. During the first year of the trial you get the full wage. After the first year, you get 30% of your wage.

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If you want to sue, you need a lawyer

- **Private lawyers:** They charge a fixed fee or a percentage of what you receive at the end of the trial.
- **Public lawyers:** They offer a free legal service, but are often overburdened.
- We recommend consulting **more than one option** and choosing what works best for you.

FREE INFORMATION BOOTH – JLCA CDMX

Figure OA-9: Calculator



WORKER CALCULATOR

Worker:

Employer:

- The Calculator is intended to show you how unjustified dismissal lawsuits were resolved for people similar to you.
- It is based on 5,000 completed casefiles.
- IMPORTANT! 30% of the lawsuits last more than 4 years, according to the data. (c)

Of the concluded casefiles most similar to your case...		
→ The majority of the settlements are within the range of		
42 days	←————→	59 days
of salary.		(b)
→ Of those who continued until court ruling	<u>41%</u>	did NOT get paid. (d)

The risk of pursuing a labor lawsuit implies that
CONCILIATION is an attractive option for both parties.

Basic data provided by the worker			
Gender:	Man	Daily salary:	\$283.11 MXN per day
Tenure:	1.85 years		(a)

The calculator provides statistical data based on conflicts similar to yours.

It does not affect your rights nor influence your case, and it does not affect any lawsuit you may pursue.

No. Folio: XXXXXXXX

In compliance with Article 9 of the Law on the Protection of Personal Data for Mexico City, the Personal Data collected will be protected and may not be disseminated without express consent, except for what provided in the Law.

Figure OA-10: Letter of Appointment

 **CDMX**
CIUDAD DE MÉXICO

Junta Local de Conciliación y Arbitraje
de la Ciudad de México
Junta Especial Siete

 **JLCA**
JUNTA LOCAL DE CONCILIACIÓN Y
ARBITRAJE DE LA CIUDAD DE MÉXICO

ASUNTO: JUNTA ESPECIAL SIETE
EXPEDIENTE NÚMERO: XXX
[NAME OF WORKER]
VS.
[NAME OF FIRM]
OFICIO-CITATORIO N° XXX

C. APODERADO LEGAL DE:
[NAME OF FIRM]
[ADDRESS OF FIRM]

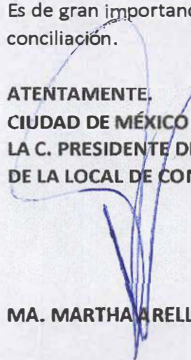
PRESENTE

En cumplimiento y observancia de los Principios Procesales contenidos en el artículo 685 de la Ley Federal del Trabajo, que regulan el procedimiento conciliatorio ante la Junta Local de Conciliación y Arbitraje de la Ciudad de México; con el objeto de obtener mayor economía, concentración, oralidad y sencillez en el proceso de solución de conflictos, se le cita al Demandado **[NAME OF FIRM]**, para que asista a la audiencia de fecha **jueves 14 de junio de 2018**, a las **11:00** horas, con la finalidad de que participe en un programa de la Junta para promover la conciliación.

Se hace del conocimiento del interesado que de conformidad con lo dispuesto en el artículo 47 fracción 1 del Reglamento Interior de éste Tribunal de Trabajo, las pláticas se rigen por el principio de confidencialidad, por lo que el presente no tiene efectos de notificación, emplazamiento o medio probatorio alguno.

Es de gran importancia la asistencia y participación de la Parte Demandada para la realización de la conciliación.

ATENTAMENTE,
CIUDAD DE MÉXICO A 7 DE JUNIO DE 2018
LA C. PRESIDENTE DE LA JUNTA ESPECIAL NÚMERO SIETE
DE LA LOCAL DE CONCILIACIÓN Y ARBITRAJE DE LA CIUDAD DE MÉXICO


MA. MARTHA ARELLANO JASSO


JUNTA ESPECIAL NÚMERO SIETE
DE LA LOCAL DE CONCILIACIÓN Y
ARBITRAJE DE LA CIUDAD DE MÉXICO
PRESIDENCIA

Notes: Authors' translation of the original Spanish letter. "In compliance with and observance of the procedural principles contained in Article 685 of the Federal Labor Law, which regulate the conciliation procedure before the Local Conciliation and Arbitration Board of Mexico City, and for the purpose of achieving greater efficiency, concentration, oral procedure, and simplicity in the conflict-resolution process, the Defendant, [NAME OF FIRM], is hereby summoned to attend the hearing scheduled for Thursday, June 14, 2018, at 11:00 a.m., for the purpose of participating in a Board program to promote conciliation. The interested party is informed that, in accordance with Article 47, Section I, of the Internal Regulations of this Labor Tribunal, the discussions are governed by the principle of confidentiality; therefore, this document has no effect as a notification, summons/service of process, or means of evidence of any kind. The attendance and participation of the Defendant is of great importance for the conciliation to take place. Sincerely, Mexico City, June 7, 2018. President of Special Board Number Seven of the Local Conciliation and Arbitration Board of Mexico City, Ma. Martha Arellano Jasso."

Figure OA-11: Uber treatment flyer

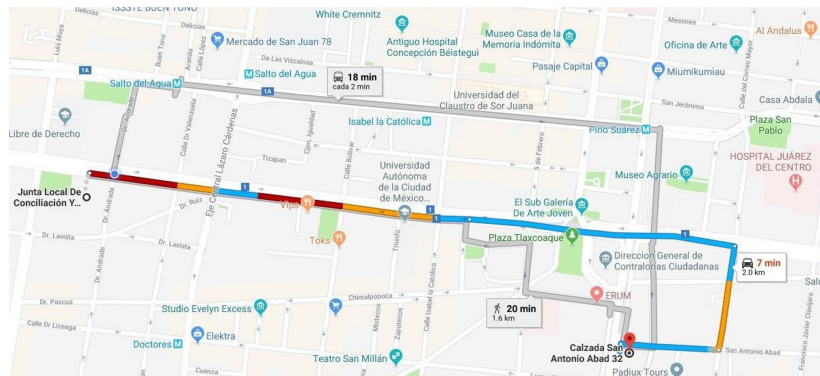
You are traveling in a private and safe transportation service, at no cost to you.

You are heading to:

PROSECUTOR'S OFFICE FOR LABOR DEFENSE

Calzada San Antonio Abad #32, Col. Tránsito, Alcaldía Cuauhtémoc
Look for building #32 and go up to the second floor, where a Public Attorney (public lawyer) will assist you.

IMPORTANT: People who approach you outside the building are not staff from the Prosecutor's Office. Look for staff inside an office on the second floor of the building.



OA-H Follow-up Surveys

H.1 Two-week follow-up survey

Settlement	
1.	Have you reached an agreement with your company?*
	(a) Yes (b) No (Skip to question 3.)
1.1.	Was the agreement for you to return to work at the company?*
	(a) Yes (Skip to question 1.3.) (b) No
1.2.	What amount did you agree to receive from your employer?*
	(a) Tells amount (b) Refuses to tell amount (Skip to question 1.3.)
1.2.1.	Amount (in pesos)* _____
1.3.	On what date did you reach the agreement with your employer?* _____
1.4.	Was your agreement registered at the Court or was it informal?*
	(a) Registered at the Court (b) Informal
Public Lawyer	
2.	Have you talked to a public lawyer or any lawyer from the Prosecutor's Office?*
	(a) Yes (b) No (Skip to question 4.)
2.1.	Where did you contact the public lawyer?*
	(a) At the Court (b) San Antonio Abad (c) Does not know
2.2.	Did the public lawyer schedule a conciliation appointment?*
	(a) Yes (b) No (Skip to question 2.4.)
2.3.	What is the date of conciliation appointment?* _____
2.4.	Is the public lawyer filing your case?*
	(a) Yes (b) No
Private Lawyer	
3.	Have you talked to a private lawyer?*
	(a) Yes (b) No (Skip to question 5.)
3.1.	Where did you find this lawyer?*
	(a) Recommended at the Court's entrance (b) Found at the entrance to the Court (c) Inside the Court (d) Represented me before (e) Recommended by someone I know (f) Is a relative/friend (g) Online (h) Other media (i) Near home (j) Other (k) Doesn't know (l) Refused
3.2.	Did you sign any documents for this lawyer?*
	(a) Yes (b) No (Skip to question 3.3)
3.2.1.	What was the payment scheme you agreed with this lawyer?*
	(a) % at the end (b) Payment to start (Skip to 3.2.1.3. if only this payment) (c) Other
3.2.1.1.	What percentage did you agree?*
	a) Tells (b) Does not tell (Skip to question 3.3)
3.2.1.2.	Amount %* _____
3.2.1.3.	How much did the lawyer ask to file the lawsuit?*
	(a) Tells (b) Does not tell (Skip to question 3.3)
3.2.1.4.	Amount requested to file the lawsuit* _____
3.3.	Did you look for more private lawyer alternatives?*
	(a) Yes (b) No
Conciliators' Area at the Court	
4.	Did you schedule a conciliation appointment at the Court?*
	(a) Yes (b) No (Skip to note *) (c) No and answered No to all above (Skip to question 6.)
4.1.	Date of first conciliation appointment _____
* If the worker settled, skip to question 9. If not, skip to question 6.	

Alternatives and Expectations: case when worker has not solved her conflict	
5.	What will you do in the next month regarding your conflict?*
	(a) Direct settlement (b) Talk to public lawyer (c) Talk to private lawyer (d) Schedule a conciliation appointment (e) Other
6.	What is the likelihood you will win compensation from 0 to 100?*
	(a) Knows (b) Does not know (Skip to question 6.2.)
6.1.	Percentage* _____ (Skip to question 7.)
6.2.	Is your chance above or below 75%?*
	(a) Above 75% (b) Below 75% (c) Does not know
7.	What is your expected compensation (in pesos)?*
	(a) Knows (b) Does not know (Skip to question 7.2.)
7.1.	Amount* _____ (Skip to question 10.)
7.2.	Would you earn more or less than half a year of salary?* (Skip to question 10.)
	(a) More than half (b) Less than half (c) Does not know
Alternatives and Expectations: case when worker has settled	
8.	If you didn't resolve your case, what are the chances you would have won?*
	(a) Knows (b) Does not know (Skip to question 8.2.)
8.1.	Percentage* _____ (Skip to question 9.)
8.2.	Were your chances above or below 75%?*
	(a) Above 75% (b) Below 75% (c) Does not know
9.	How much compensation would you have received?*
	(a) Knows (b) Does not know (Skip to question 9.2.)
9.1.	Amount* _____ (Skip to question 10)
9.2.	Would you have earned more or less than half a year of salary?*
	(a) More than half (b) Less than half (c) Does not know
Worker's characteristics	
10.	Have you filed a lawsuit for dismissal before?*
	(a) First time (Skip to question 11.) (b) Had a conflict before
10.1.	How many years ago was it? _____
10.2.	Did you win or lose that case?*
	(a) Won (b) Lost (c) Ongoing
11.	What is your highest level of education?*
	(a) Elementary (b) Middle school (c) High school (d) Bachelor's or more

H.2 Two-month follow-up survey

Settlement	
1.	Have you reached an agreement with your company?*
	(a) Yes (b) No (Skip to question 3.)
1.1.	Was the agreement for you to return to work at the company?*
	(a) Yes (b) No (Skip to question 1.1.2.)
1.1.1.	Before reaching the agreement, did you sue your employer?*
	(a) Yes (b) No (Skip to question 3.)
1.1.1.1.	Was this lawsuit with a public or private lawyer?*
	<i>Make sure the worker understands that a public lawyer provides free legal representation and is a public worker. Also make sure the worker understands that to file a lawsuit, it is necessary to sign a power of attorney at the PROFEDET.</i>
	(a) Public (b) Private (c) Doesn't know (d) Refused
1.1.2.	What amount did you receive from your employer?* _____
1.1.3.	On what date did you reach the agreement with your employer?* _____
1.1.4.	Was your agreement registered at the Court?*
	(a) Yes (b) No (c) Doesn't know (d) Refused
Lawsuit and lawyer	
2.	Did you sue your former employer?*
	(a) Yes (b) No (Skip to question 3.)
2.1.	When did you sue your former employer?* _____
2.2.	Was this lawsuit with a public or a private lawyer?*
	<i>Make sure the worker understands that a public lawyer provides free legal representation and is a public worker. Also make sure the worker understands that to file a lawsuit, it is necessary to sign a power of attorney at the PROFEDET.</i>
	(a) Public (Skip to 2.2.1.) (b) Private (Skip to 2.2.3.)
Public lawyer	
2.2.1.	Where was your first contact with public lawyers?*
	(a) In the prosecutor's office (b) At the court (c) Doesn't know (d) Refused
2.2.2.	Before filing your case, did the public lawyer schedule a conciliation appointment?*
	(a) Yes (b) No (Skip to 2.3.)
2.2.2.1.	Did your employer attend the appointment?* (Skip to question 2.3.)
	(a) Yes (b) No (c) Doesn't know (d) Refused
Private lawyer	
2.2.3.	How did you get your lawyer?*
	(a) Recommended at the Court's entrance (b) Found at the entrance to the Court (c) Represented me before (d) Recommended by someone I know (e) Is a relative/friend (f) Online (g) Other media (h) Other (i) Doesn't know (j) Refused
2.2.4.	Did you make a fixed payment to your lawyer to start the lawsuit?*
	<i>If the answer is "Yes", then code the amount; if the answer is no, code "No". Also code "Doesn't know" or "Refused".</i>
2.2.5.	Will you pay your lawyer a percentage of what you get?*
	<i>If the answer is "Yes", then code the amount; if the answer is no, code "No". Also code "Doesn't know" or "Refused".</i>
2.2.6.	Is there another way you're paying or will pay your lawyer?*
	(a) Yes (b) No (Skip to question 2.3.)
2.2.6.1.	Specify: _____

Both	
2.3.	Are you asking to be reinstated in your job? (a) Yes (b) No (c) Doesn't know (d) Refused
2.4.	In case of unjustified dismissal, the law grants a constitutional severance pay. Do you know how many days of your salary this compensation represents? _____
2.5.	In a scale from 1 to 10, how satisfied are you with your lawyer? (a) 1 to 10 scale (b) Doesn't know (c) Refused
2.6.	If you could change lawyers, would you? (a) Yes (b) No (c) Doesn't know (d) Refused
2.7.	What is the file number of your lawsuit?* _____
2.8.	On what date will your first hearing be held? _____
2.9.	How angry are you with your employer? (a) Very (b) Moderately (c) Not much (d) Nothing (e) Doesn't know (f) Refused
2.10.	What is the likelihood you will win compensation from 0 to 100? (a) Knows (Write likelihood and skip next question) (b) Doesn't know
2.10.1.	Is it less or more than 75%? (a) Less (b) More (c) Doesn't know
2.11.	How much money do you expect to earn if you win? (a) Knows (Write amount and skip next question) (b) Doesn't know
2.11.1.	Will you earn less or more than half a year of salary? (a) Less (b) More (c) Doesn't know
2.12.	What influenced the amount/percentage you mentioned the most? (a) Lawyer (b) Court booth (c) Info elsewhere in court (d) Family/friends (e) Own beliefs
2.13.	What is the minimum amount you would accept to settle and drop lawsuit _____
Welfare	
3.	In the past 3 months have you had to stop paying for a basic service such as electricity, water or rent due to lack of money? (a) Yes (b) No (c) Doesn't know (d) Refused
4.	In the past 3 months, have you lacked money to spend on food one or more days? (a) Yes (b) No (c) Doesn't know (d) Refused
5.	In the last month, have you or anyone in your household purchased... (a) House/land/vehicle (b) Appliance/electronics (c) No (d) Doesn't know (e) Refused
6.	Are you currently working? (a) Yes (b) No (Skip to 6.2.)
6.1.	Is your current job better, worse, or equal than your previous one? (a) Better (b) Worse (c) Equal
6.2.	Are you looking for a job? (a) Yes (b) No (Skip to 7.) (c) Doesn't know
6.2.1.	On a scale from 0 to 100, how likely is that you will find a new job in the next 3 months? _____
7.	From your home, how long does it take you to travel to the Court, in one direction? _____
8.	Hours spent trying to resolve your conflict* (a) 0-2 (b) 2.01-5 (c) 5.01-10 (d) 10.01-15 (e) 15.01-20 (f) 20.01-30 (g) More than 30
9.	On a scale of 1 to 10, where 1 means "not happy at all" and 10 means "totally happy", in general how happy do you feel about your life lately? (a) 1 to 10 (b) Doesn't know (c) Refused

OA-I The 2019 Labor Reform at National Scale: Data and Methods

This appendix provides background for the national-scale results reported in Section 6. We use the staggered rollout of Mexico's 2019 labor-justice reform to estimate its effect on the number of individual disputes reaching the courts. We also present a broader measure of access to the labor-justice system descriptively. Here we describe the data sources, the construction of these two measures, the estimator and the choice of reference period. We report the full estimates and robustness checks for the court-disputes outcome, which the main text summarizes.

Institutional background and rollout. The 2019 reform made pre-lawsuit conciliation mandatory: a dismissed worker must request a hearing at a local conciliation center and obtain a *constancia de no conciliación* before filing a lawsuit. It also moved labor adjudication from the executive-branch *Juntas de Conciliación y Arbitraje* to judicial-branch Labor Tribunals. Because each state had to build a conciliation center, stand up its tribunals, and pass enabling legislation, the reform entered into force in three staggered waves—eight states on 18 November 2020, thirteen on 3 November 2021, and the remaining eleven on 3 October 2022. We treat the date a state's local conciliation center begins operations as the treatment date and group states into the corresponding three cohorts (Consejo de Coordinación para la Implementación de la Reforma al Sistema de Justicia Laboral, 2022).

Data and measures. We build a balanced monthly panel at the state level. The number of disputes reaching the courts comes from INEGI's *Estadísticas sobre Relaciones Laborales de Jurisdicción Local* (ERLA-JUL). This series follows individual disputes across the institutional break: it records them at the Juntas through 2019, at the Juntas and the new local Labor Tribunals during the 2020–2023 transition, and at the Labor Tribunals from 2024 (Instituto Nacional de Estadística y Geografía, n.d.). A decline in this series is therefore a genuine fall in lawsuits initiated, not a mechanical consequence of the Juntas ceasing to receive cases. This court-disputes measure is the sole outcome in our difference-in-differences analysis, which we estimate in levels and logs.

Conciliation requests come from the administrative records of the local conciliation centers. To describe total access to the system, we construct a separate measure that counts INEGI individual disputes in the months before a state's center opens and conciliation requests from the opening onward. We present this broader access measure descriptively; it is not an outcome in the difference-in-differences analysis.

A note on the descriptive access measure. Combining the two series is meaningful for describing formal entry into the state labor-justice system for individual labor matters, although they refer to different institutional stages: lawsuits before the reform and mandatory conciliation requests afterward. The reform does not mechanically imply that this descriptive measure should increase. Requiring conciliation before filing suit could have discouraged some workers from pursuing a claim and reduced formal entry into the system. At the same time, conciliation may lower the cost of recovering at least part of a severance payment or other labor entitlement, thereby drawing in workers who would not have sued under the old regime. We use the measure to describe access, not to estimate a second causal effect of the reform.

Estimator. For the court-disputes outcome alone, we estimate staggered difference-in-differences event-study specifications using Arkhangelsky et al. (2021) as the main estimator and Borusyak et al. (2024) for robustness checks. Because every state is treated by October 2022, there are no never-treated units; identification compares newly treated states to not-yet-treated states, and standard errors are clustered at the state level.

Reference period. For the court-disputes event study, we normalize coefficients to $t = -3$ rather than the conventional $t = -1$. The 60-day statute of limitations on dismissal claims (Article 516 of the Federal Labor Law) creates a two-month window before a center opens in which an already-dismissed worker can choose between the soon-to-open center and immediate litigation. The periods $t = -1$ and $t = -2$ are therefore contaminated by anticipation, while $t = -3$ predates the window and provides a cleaner baseline; the event study shows a run-up in those two months, after which the post-opening estimates stabilize.

Results. Table OA-15 reports the average post-opening effect on the court-disputes outcome, with estimates in levels and logs under the estimators reported in the table. Figure OA-12 shows the corresponding event studies for the Borusyak et al. (2024) difference-in-differences estimator, in logs and levels, complementing the event studies reported in the main text. Figure OA-13 shows the raw pre- and post-opening means by state for individual disputes reaching court. The broader access measure is presented in a separate descriptive figure and is not included in the table or event-study estimates.

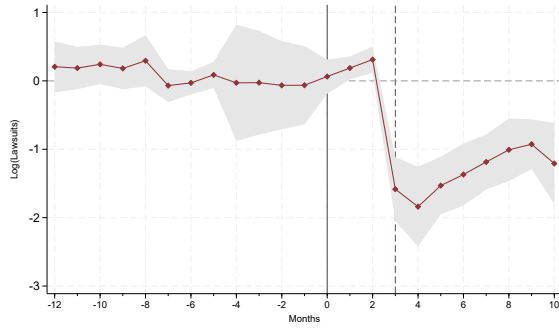
Table OA-15: Average treatment effects — H1: Individual disputes (INEGI). Borusyak, Jaravel, and Spiess and synthetic difference-in-differences estimator of Arkhangelsky et al.

	BJS		SDID	
	Levels	Log	Levels	Log
<i>H1 — Individual disputes (INEGI, 2016–2022)</i>				
ATT ($h = 0, \dots, 10$)	-143.268*** (48.163)	-0.917*** (0.130)	-144.234*** (50.127)	-0.799*** (0.144)
Wald χ^2_{12}	12.94	5.39	—	—
p -value	0.374	0.944	—	—
Dep. var. mean	429.257	5.608	429.257	5.608
N	2,412	2,412	2,496	2,496

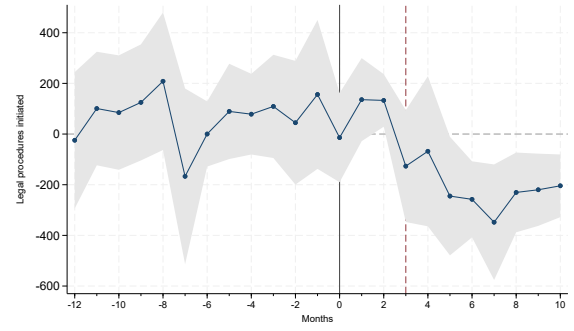
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors in parentheses. Wald χ^2_{12} : joint test H_0 : no pre-trends ($h = -12, \dots, -1$). Treatment date shifted $\bar{T}^* = T - 3$.

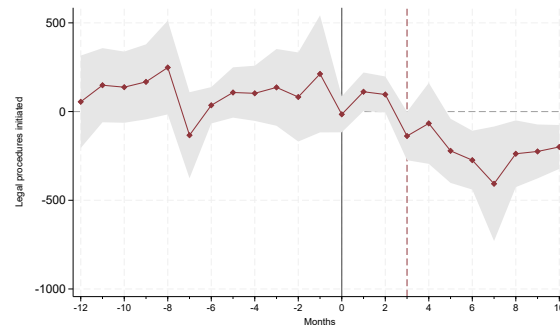
Figure OA-12: Event studies, remaining specifications



(a) BJS — log conflicts.



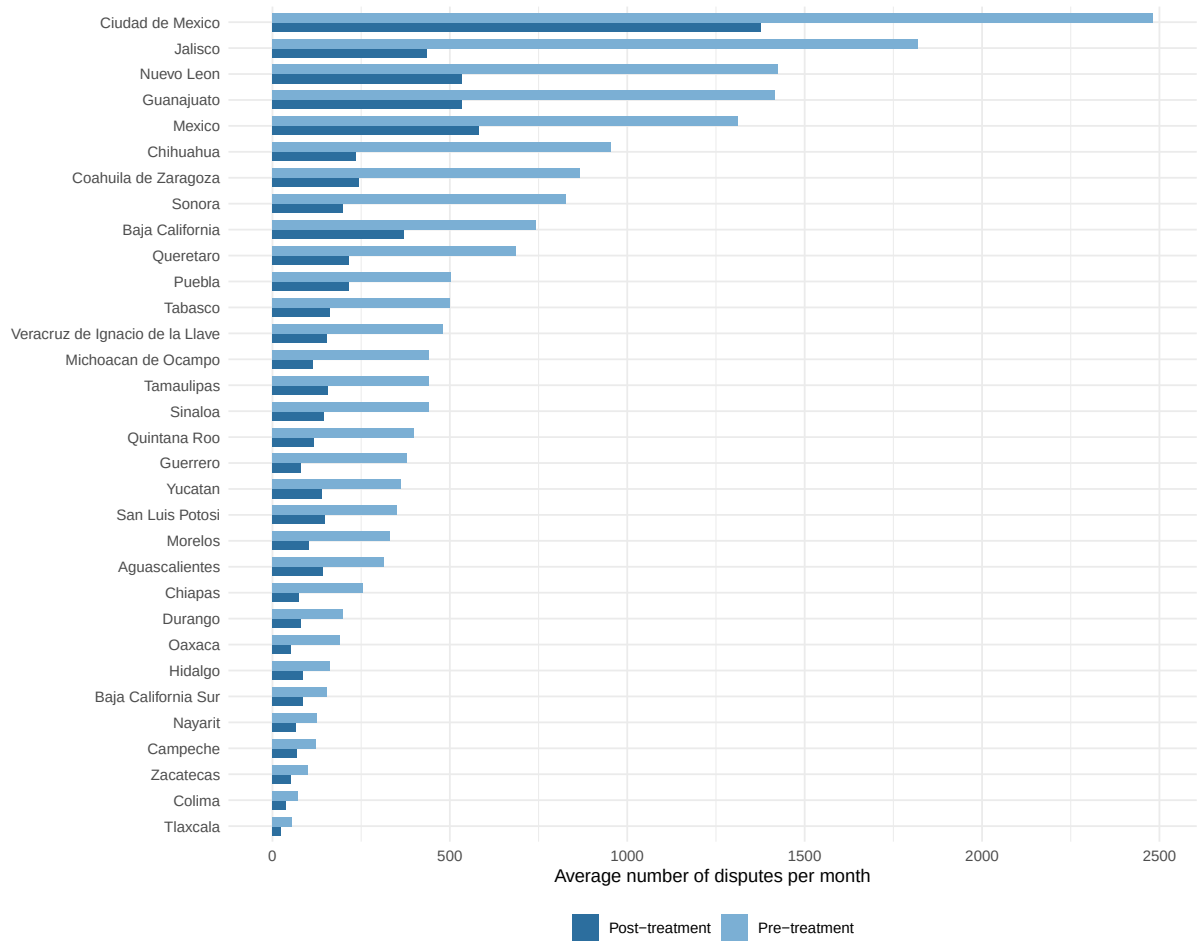
(b) SDID — levels.



(c) BJS — levels.

Estimated treatment effects on individual labor disputes reaching the courts at event-time horizons $h \in \{-12, \dots, 10\}$, relative to the shifted treatment date $T^* = T - 3$. Gray bands show 95 % confidence intervals. For the imputation estimator of Borusyak, Jaravel, and Spiess, the intervals are based on the influence function; for the synthetic difference-in- differences estimator of Arkhangelsky et al., they are based on a bootstrap with 200 replications. The solid vertical line marks $h = 0$, corresponding to the shifted treatment date $T^* = T - 3$; the dashed vertical line marks $h = 3$, corresponding to the actual center opening in October 2022. Pre-treatment horizons $h \in \{-12, \dots, -1\}$ serve as placebo checks..

Figure OA-13: Trials: individual disputes reaching the courts



Note: Average monthly matters per state, before and after the local conciliation center opens. Pre and post opening matters are INEGI individual disputes. States are ordered by pre-treatment average. Sample period: January 2016–November 2024.