# Information and Bargaining through Agents: Experimental Evidence from Mexico's Labor Courts

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**Abstract**

Well-functioning courts are essential for the health of both financial and real economies. Courts function poorly in most lower-income countries, but the root causes of poor performance are not well understood. We use a field experiment with ongoing cases to analyze sources of dysfunction in Mexico's largest labor court. Providing the parties with personalized outcome predictions doubles settlement rates and reduces average case duration, but only when the worker is present to receive the information. An intervention before plaintiffs contact a lawyer increases pre-suit settlement. The experiment illuminates agency issues among plaintiffs with private lawyers. For most workers, the treatment appears to improve welfare, as measured by discounted payouts and ability to pay bills.

Keywords: labor courts, settlement, overconfidence, statistical information

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# 1 Introduction

Well-functioning courts underpin markets and constrain private power in developed economies, but courts function poorly in most developing countries. Outcomes are unpredictable, parties are misinformed, and inefficient processes lead to slow decisions and large case backlogs (Djankov et al. (2003)). In addition to raising concerns for justice, poorly functioning courts undermine both the financial (Ponticelli and Alencar (2016)) and real (Boehm and Oberfield (2020); Chemin (2020)) economies. The effects of poorly functioning courts are increasingly well documented, most convincingly through studies analyzing changes following the creation of new institutions providing legal services for specific activities (Visaria (2009); Lichand and Soares (2014)). However, as Boehm and Oberfield (2020) note, even "Newly created courts tend to...accumulate backlogs over time" (p. 2009). Thus, while the existing literature provides evidence on the importance of well-functioning legal institutions, it is much less informative on how to improve the performance of existing institutions, or how to ensure the continued high performance of the new institutions. This is due in part to a lack of rigorous empirical work aimed at understanding the micro-analytics of court proceedings, and a particular paucity of randomized experiments in courts (Greiner and Matthews (2016)).

To illuminate the causes of dysfunction in courts, we conduct a randomized experiment with active cases in the Mexico City Labor Court (MCLC). The MCLC is responsible for enforcing labor law for all private employers located in Mexico City. Most cases involve workers who claim to have been involuntarily separated from their jobs by employers who then failed to make severance payments to the workers as required under the labor law. Each year the court receives more than 35,000 filings, but resolves less than 30,000 cases. The MCLCs growing case backlog stems in part from its settlement rates, which are low in comparison to those of similar courts in other countries. Taking a cue from the literature on bargaining under asymmetric information, our experimental treatment provides parties with case-specific predictive outcomes, generated from machine-learning models using data from 5,000 concluded cases. We use the experiment to understand how the relationship between plaintiffs and their lawyers affects the settlement and progression of their cases through the court.

Mexico's job-insurance system is based on severance payments made directly by firms to dismissed workers. On paper, the law is straightforward and very favorable for Mexican workers. Dismissed workers are entitled to a minimum severance payments of 90 days' wages and,

depending on circumstances, substantially more. Payments must be made even if the dismissal is due to loss of business by the firm. However, while the law itself is generous, the framework for enforcing the law disadvantages workers for at least three reasons. First, workers and firms are often informal (Kumler, Verhoogen and Frias (2020)). Wages are paid in part or entirely in cash and there is no formal written contract. In these circumstances, dismissed workers may find it difficult to prove wage levels or even the existence of the labor relationship itself. Second, at the time of hiring, firms often take actions that undermine the workers subsequent claim of unfair dismissal. For example, firms may require the worker to sign an undated letter of resignation as a condition of hiring. Third, while the payments from the unemployment insurance systems used in most countries are made by the government, payments in Mexico are made directly from the firm to the worker. Workers who win a judgment often face challenges in recovering payments ordered by the court from their former employers. The firms can avoid making payments through a combination of transferring assets to other entities, firm bankruptcy, and bribes. With the threat of avoiding payment, firms often negotiate much lower payments even after the court rules against them. Indeed, administrative records show that that, more than half of the time, workers winning a court judgment are unable to recover anything.

We demonstrate the relevance of these disadvantages by using the historical case files to document a set of stylized facts about the functioning of the court. First, the historical data show that plaintiffs receive, on average, only around half of the minimum 90-day compensation called for in the law. Second, although the law stipulates that suits should be adjudicated within three months, more than a third of the cases filed in the court between 2009 and 2012 remained unresolved in early 2016. The backlog of cases is driven in part by low settlement rates: around 55 percent of cases are settled in Mexico, compared with 80 to 90 percent in higher-income countries. Third, parties are overconfident: the sum of the two parties' probabilities of winning far exceeds 100 percent, and both the probability of winning and the expected size of the award are optimistic, particularly on the plaintiff's side, relative to predictions based on historical cases. Fourth, plaintiffs have little knowledge of their legal entitlements. Moreover, even those represented by private lawyers are surprisingly uninformed about the contents of their own lawsuits. Finally, we show that although private lawyers file much larger claims, they do not recover more than public lawyers. After accounting for fees, private lawyers actually recover less than public lawyers.

Working with the court, we conduct an experiment in three phases, which differ in the point

at which we intervene in the process. In phase 1, we intervene in ongoing cases at any point in the life of the lawsuit. We find that the information treatment increases settlement on the day of treatment by 75 percent, and that settlements are more frequent in cases that are early in the process. In phase 2, given the potential of earlier interventions to be more effective, we intervene in the first hearing of each case. Although there are more settlements at the first hearing, we find that the effect of the information treatment is of very similar magnitude. In both the first and second phases, we find that the treatment is effective only when the employee is present at the hearing, and only in cases where the employee is represented by a private lawyer.[1] Administrative data from 42 months after treatment indicate that an additional 38 percent of the cases in both the control and treatment groups are settled after the day of our intervention; however, the treatment effect and the relevance of the employee receiving the treatment directly remain constant over that time. The importance of the employee's presence for even long-run outcomes suggests that lawyers do not convey the information to their clients. The persistence in the treatment effect indicates that the intervention led to settlement of cases that would not have been settled otherwise. Perhaps most importantly, data on case outcomes from phase 1 and 2 show that the increase in settlements improves the outcomes for the typical plaintiff. These patterns suggest that case trajectories are affected by lawyer-plaintiff agency issues. Given this, in phase 3, we work with a sample of recently dismissed workers who come to the court seeking information about their rights before they contract with a lawyer and file a case. The treatment providing predicted case outcomes is again effective in increasing settlement, this time before a case is filed.

The results of the three experiments suggest that informational asymmetries between the worker and her lawyer are one underlying cause of malfunctioning courts. A majority of plaintiffs earn below the median wage and have modest levels of schooling; more than 80 percent are using the court for the first time. Survey responses indicate that 38 percent of plaintiffs found their lawyer on the sidewalk outside the court building, where many lower-quality lawyers find clients. The workers are wildly overconfident about their chances of winning their lawsuit. We show analytically that the incentives of the lawyers, who have information advantages, do not always align with those of their clients even though private lawyers in the MCLC cases almost always receive a share of the award collected by their plaintiffs. Differences in the ability to diversify

[1] Both the choice of lawyer and the presence of the employee at the hearing may be endogenous to outcomes. However, the treatment is orthogonal to either. We discuss this issue in more detail below.

risk and discount rates lead to very different preferences over settlement options between the plaintiffs and their lawyers. Lawyers also charge an initial fee that is generous relative to the time required to file a suit, giving them incentives to file cases even when they have little prospect of winning.

By 2020, almost 90 percent of both treatment and control cases in the first two phases were resolved. This allows us to examine how treatment affects the welfare of plaintiffs. Relative to the control group, those in the treatment group are 6.7 percent more likely to have settled, 7.3 percent less likely to have lost a judgment, and 0.4 percent less likely to have won a judgment. The additional settlements come mainly from cases with modest recovery amounts. When we compare the net present value of the amount collected by plaintiffs in the treatment and control groups, we find that the treatments improved the outcomes for the typical plaintiff. Though making a precise statement about plaintiff welfare is difficult for reasons we discuss in Section 7, surveys two months after treatment show that treated workers in Phase 3 are 7 percent less likely to report not having enough food to eat or being unable to pay for basic services; reported happiness is insignificantly higher with treatment.

Experiments in courts are uncommon. Greiner and Matthews (2016) review the small number of randomized evaluations in the US legal system, finding 50 studies conducted between 1963 and 2015. Most of these examine mediation and alternative dispute resolution, though a handful evaluate programs that affect the use of lawyers. There are likewise very few legal experiments in low-income countries.[2] As a result, there is little credible evidence on the source of delays and the effect of differences in rules and organizational structure. Indeed, given the scarcity of diagnostic information about courts in developing countries, the stylized facts we derive from extensive administrative records contribute to the literature on courts as institutions for development.

The paper also contributes to the literature on experts and moral hazard. The most closely related paper in this literature is Anagol, Cole and Sarkar (2017), who conduct an audit experiment in the Indian life insurance market. They show that agents use their informational

[2]Two notable exceptions are Aberra and Chemin (2019), who provide access to lawyers to randomly selected plaintiffs in cases involving land disputes, showing that legal assistance leads to greater investment in land held by the plaintiffs; and Sandefur and Siddiqi (2015), who provide access to paralegals that lower the cost of accessing the formal legal system in Senegal. They find that access to the formal legal system is particularly valuable for those most likely to be disadvantaged in the customary legal system – for example, women who are suing men. Kondylis and Stein (2018) use an abrupt change in procedural rules rolled out across six civil courts in Senegal to identify the effect of the reforms; the authors find that the new regulation resulted in in faster pre-trial phases of cases.

advantage to induce clients to make decisions that are favorable to the agents' interest. There is much more extensive observational evidence that agents take advantage of superior information in diverse settings: Schneider (2012) in Canadian auto repair shops; Emons (1997) among doctors in Switzerland; and Levitt and Syverson (2008) among real estate agents in the US.[3]

A third related literature is that of bargaining in the field. Courts are a disciplining device for a bargaining game between the parties to the case. The canonical Rubinstein (1982) bargaining framework shows that bargaining outcomes are immediate and efficient where information is complete, delays are costly, and parties bargain by making alternating offers. However, the efficiency result breaks down if parties have asymmetric information (Myerson and Satterthwaite (1983)). Bargaining may also break down or be delayed if parties are overly optimistic.[4] Our data indicate that both misinformation and excessive optimism are characteristics in the MCLC cases. Most of the relevant bargaining literature constructs a game between two parties, but court cases typically involve four parties: the plaintiff, the defendant and the lawyers representing either side. This distinction is relevant if agency is important. Note that, theoretically, the addition of lawyers may result in either more- or less-efficient outcomes. Gilson and Mnookin (1994) model court cases as prisoners dilemmas in which the parties play once and the lawyers play repeatedly. As such, the lawyers may cooperate when the players would not, generating more-efficient outcomes. Ashenfelter and Dahl (2012) examine data from arbitration cases involving emergency services unions and municipalities in New Jersey. In a context in which the parties sometimes represent themselves and sometimes are represented by lawyers, they show that lawyer-agents provide positive benefits to the party they represent.

Finally, we contribute to the literature on the effects of information provision and decision making. Information has been shown to improve decision making in wide range of other contexts: in the functioning of private markets (Andrabi, Das and Khwaja (2017); Belot, Kircher and Muller (2019)), in schooling decisions (Jensen (2010); Dizon-Ross (2019)), and in political institutions (Chong et al. (2015); Reinikka and Svensson (2011)). Our results focus on courts and suggest that it is important that the information be conveyed directly to the party affected by the decisions.

[3]Hubbard (1998) and Hubbard (2000) suggest that reputation is effective in controlling agency in the automobile emissions testing market in California. However, in labor courts, the development of reputations among lawyers is limited both because plaintiffs typically use the court only once, and because the plaintiffs seldom connect with one another.

[4]See Yildiz (2011) for a review of the related theory. Overoptimism may also reflect self-serving bias (Babcock and Loewenstein (1997)).

The rest of the paper proceeds as follows: We begin by describing the context and the Mexican labor law in Section 2. We then detail the data from both administrative records and surveys of litigants and lawyers in Section 3. Section 4 uses those data to describe a set of stylized facts that motivate our experiment. Section 5 describes the experimental protocol and Section 6 the results. We discuss the welfare implications of the results in Section 7 and conclude in Section 8.

## 2 The Context

Gerard and Naritomi (2019) shows that while unemployment insurance is the most common job-displacement insurance system in higher-income countries, severance payment programs are the norm in Africa, Asia and Latin America. Mexico's system of severance payments fits this pattern. Mexican workers dismissed from their job for almost any reason are entitled to 90 days' wages and often substantially more, with payments made directly by firms to workers. Severance payment provisions in the private sector are governed by federal labor law in Mexico, with adjudication of disputes in most industries assigned to state-level labor courts.[5] We work with the Mexico City Labor Court, the state court serving Mexico City. Each year, the MCLC receives more than 35,000 new cases and concludes fewer than 30,000 cases. Its portfolio of 100,000 active cases is therefore not only large but growing.

Lawsuits filed by dismissed workers claiming they have not received severance payments owed to them account for over 95 percent of MCLC filings. These cases are assigned to one of 20 "subcourts" according to the industry in which the plaintiff was employed. In the first phase of the project, we worked with Subcourt 7, which deals mainly with firms in the retail automotive and transport services industries. In phase 2, we expanded to four additional subcourts specializing in industries such as private education, security, restaurants, retail banking, large department stores, and medical services. In phase 3, we worked with dismissed workers approaching the court who might ultimately file a case assigned to any of the 20 subcourts.

We describe the Mexican Labor Law and the court procedures in more detail in Appendix A. Plaintiffs may be represented by either private or public lawyers. Private lawyers typically charge around 2000 MXP (USD 100) to file a case, and then receive 30 percent of any amount recovered by the worker. Lawyers from the Public Attorney's office are paid a fixed wage and

[5] Disputes in a few "strategic" industries named in the Mexican constitution - oil and gas, pharmaceuticals and auto manufacturing, for example are handled by a federal-level labor court.

do not charge their clients for services. Employers most often respond to a suit in one of three ways: denying the existence of a labor relationship, offering reinstatement, or claiming the worker resigned voluntarily and producing a letter of resignation signed by the employee.[6] High levels of informality make it more difficult for plaintiffs to prove employment relationships and easier for employers to hide assets. These strategies decrease the likelihood that workers who win a judgment or collect the compensation the court awards them.

The parties reach a settlement in around 55 percent of the cases. Cases that settle usually do so within a year of filing. In the absence of settlement, cases typically involve at least six to eight hearings spread over several years. The hearings are conducted by administrative assistants to the judge, but the judge of the subcourt makes all rulings, based on the written record prepared by the assistants. Judgments within three years of filing are rare. Even when workers win their suit, a large proportion of firms do not pay the judgment voluntarily. The plaintiffs must then pursue seizure of assets through officers of the court. Firms have many ways of avoiding asset seizure, for example by declaring bankruptcy or transferring assets to another entity. In more than half the historical cases we code, workers winning the suit are not able to collect any payment.

The challenges for workers arise from informality on several levels. Even workers employed formally and registered with the Mexican Social Security Administration are often paid part of their wages off-books and in cash (Kumler, Verhoogen and Frias (2020)). Workers will generally only be able to recover based on the formal part of their wages. Firm assets are also unregistered. Hence, the court cannot place liens on assets at the time of its ruling. Finally, the process of notification is hampered by corruption at all stages (Aldeco Leo, Kaplan and Sadka (2014)). Corruption is likely to be a particular issue at the asset-seizure stage, when the gain from avoiding notification is clearest for the firms.

# 3 Data

We use both administrative records and survey data. We describe these data briefly here, and in more detail in Appendix A. Through an agreement with the court, we had access to the experimental case files and all of the historical case files from the court. The case files register all the legally relevant information in the lawsuit. Given the scarcity of evidence on the

[6] In a large range of low to mid-level jobs, entering employees are obliged to sign a letter of resignation (or a "blank letter") in advance. After firing, the firm adds a date to the letter.

functioning of courts, we view the construction of this data itself as a contribution of the paper.

We conducted the first phase of the experiment in Subcourt 7 between March and May, 2016 and the second phase in Subcourts 2, 7, 9, 11, and 16 between October 2016 and February 2017. In Phases 1 and 2, we intervened in case hearings where both parties had been notified and were therefore obligated to attend the hearing. We conducted Phase 3 between May 2017 and August 2018 with a sample of dismissed workers who had not yet filed a case. In all three phases, we carried out very brief surveys of the plaintiffs and lawyers when they were present, though in Phase 2 the survey was very limited for logistical reasons.

### 3.1 Administrative data

**Historical cases:** We began by digitizing the historical case file data with the goal of building predictive models of case outcomes, as we describe below. Given the duration of the average lawsuit, we chose to focus on cases filed in 2011, the earliest year for which the court had digital (pdf) records of all initial case filings. For phase 1, we digitized 2,158 lawsuits filed in 2011 or 2012, assigned to Subcourt 7, and concluded by December 2015. Only 55 of those cases were concluded by a decision of the judge. In order to increase the sample of cases concluded by the judge's decision, we reached back to lawsuits filed in Subcourt 7 in 2009 and 2010, identifying 241 additional case files concluded by a judge's decision. Together with the 2011 and 2012 cases, we use these to calibrate the likelihood of winning and amount collected at trial.

For the second phase of the experiment, we used data from 1,000 concluded cases in each of the five participating subcourts. We again selected cases filed in 2011 and concluded by December 2015. We used all such cases from Subcourt 7, and a random sample of approximately 1,000 cases in each of Subcourts 2, 9, 11, and 16. Thus, the calculator for Phase 2 was calibrated with historical data covering 5005 cases, all filed in 2011 and concluded by December 2015.[7]

Because we often intervened in the first hearing, the predictive model could only use information included in the initial filing. From the case filing, we capture the amount claimed by the plaintiff, the date of the lawsuit, whether the lawyer is public or private, the worker's gender, age, daily wage, tenure at the firm, and weekly hours worked. The variables are defined in Table A.1 in Appendix A. The basic formula for severance payment in the law is in large part a function of the wage, tenure and hours worked.

[7]For phase 1, the calculator used the full set 2,158 cases filed in 2011 and 2012 in Subcourt 7. We also include the 2012 cases from Subcourt 7 in the descriptive data we present in the paper.

We also record the outcome of the suit and the termination date. Cases end in one of five ways: being dropped by the plaintiff, expiring due to lack of activity, a judge's ruling with no collection, a judge's ruling with a positive collection, or settlement between the parties. We record the amount recovered by the worker at the end of the proceedings. The majority of the cases with positive recoveries end in settlement. These are essentially always recorded at the court, in order to assure that the plaintiff does not continue to pursue the case. For cases ending in judgments in favor of the plaintiff, the details of the judge's decision are sometimes complex and somewhat opaque and hence difficult to code. Therefore, we did not capture the details of the decision in the dataset. Note that the amount the plaintiff recovers is often different from the amount awarded by the judge for three reasons: first, the law provides that if the judgment is not enforced immediately, additional lost wages may be added to the award; second, the parties may reach a post-judgment settlement, with the worker accepting a lower payment to avoid the high costs of enforcing payment; and third, the worker may be unable to collect the judgment found by the court, for example because the firm may have no assets that can be seized by the time the judgment is enforced. We show below that in the experimental cases where the plaintiff won a judgment *and collected a positive amount*, the plaintiff collected only 52 percent of the estimated reward.

In addition to providing the raw material for the prediction calculator, the historical data allow us to construct a set of stylized facts about the functioning of the court. We discuss what the data show with regard to trial length, frequency of settlement, amount collected, the fraction of plaintiffs collecting awards, and so forth, in the next section.

**Administrative data for ongoing cases:** We code the initial case file data from all of the ongoing lawsuits involved in Phases 1 and 2 of the experiment. We use these data, combined with the predictive model developed with the historical data, to predict the outcome of the lawsuit. We also use the administrative records to determine who attended the hearing on the day of the experiment, whether the lawsuit ended on that day through a settlement, and the amount of money recorded for the settlement. We then repeat the data collection in 2020 around 42 months after the start of each phase of the experiment. Administrative records from late 2019 also allow us to see which of the Phase 3 workers sued or settled. As we noted, settlements are generally registered in court files even for cases that settle out of court, because this is the only way the firm can ensure that the employee does not continue to pursue the case.

### 3.2 Survey Data

We conducted surveys with parties involved in the experiment in all three phases. In Phases 1 and 2, these were short surveys at the hearing on the day of the experiment. We interviewed the defendant's lawyer and either the plaintiff, if she was present at the hearing, or her attorney if she was not.[8] The survey was conducted before parties were aware of their treatment status. We asked about knowledge of the case file and the relevant law, expected case outcomes, and, where the plaintiff was present, demographic characteristics of the plaintiff.

In Phase 3, our sample is dismissed workers approaching the court in search of information. Our survey in this phase collected demographic data similar to Phases 1 and 2, but we also collected enough information about the worker's employment to be able to use the calculator to make predictions on the worker's own case outcomes. In the first two phases, the relevant employment data came from the case file itself, but in Phase 3, the workers had not yet filed a case. In Phase 3, we also conducted telephone follow-up surveys two weeks and two months after the initial contact. The follow-up surveys recorded actions taken after our initial interaction, and elicited updated beliefs about case outcomes. For example, we asked workers if they had talked to a lawyer, and if so, how they had found the lawyer(s). We also asked whether they had filed a suit, settled, or decided not to pursue any claim, and collected a measure of life satisfaction and difficulty in paying bills. We conducted at least one of the two telephone surveys with 89 percent of the sample. Survey questions and response rates from all three phases are discussed in more detail in Appendix A.

Table 2 shows summary statistics of the cases for each of the three phases of the experiment. We use the administrative data for the first two phases and the survey data from Phase 3. Table A.2 in Appendix A summarizes data from the plaintiff surveys. We find that 58 percent of plaintiffs had at most lower secondary schooling. Plaintiffs with public lawyers were more likely to attend the hearing: 29 percent of workers present at Phase 1 and 2 hearings had a public lawyer, while only 10 percent of the case files in the experiment had a public lawyer. Of those workers who showed up and had a private lawyer, most (nearly 82 percent) said their agreement called for paying their lawyer a fraction of any award (30 percent, on average). Only 7.6 percent were currently employed, and for those not currently working who were searching for a job, the

[8] Lawyers for the plaintiff and defendant were almost always present, but the plaintiff was present only 18 percent of the time, and the defendant only 1.4 percent of the time. In the interest of time, we surveyed only one individual from each side of the case. At least one party completed the baseline survey in 71 percent of the cases. Survey compliance rates are detailed in Table A.3 in Appendix A.

average reported likelihood of finding a job in the next three months was 58 percent.

### 3.3 Construction of the calculator

In the experiment, we provide personalized predictions on important case outcomes to a subset of plaintiffs and defendants. We developed simple, parsimonious, predictive models using the historical case records. We considered several machine learning models, including boosting, random forest, and regularization methods (e.g., ridge), along with OLS and logit. The construction of the calculator is described in detail in Appendix A, but we summarize the main points here. As we noted, the calculator in Phase 1 was developed using 2,158 cases from Subcourt 7 and the calculator used in Phases 2 and 3 used 5,005 cases from the five participating subcourts. The information we provided to the parties also changes somewhat in each phase, as we describe below.

Our goal was to provide predictions on the expected amount collected by the plaintiff, the duration of the case, and the probability the case ends by being dropped, expiring, judgment with zero recovery, judgment with positive recovery, or settlement. The main explanatory variables were all taken from the initial case filing: daily salary, hours worked per week, tenure at the firm, gender of the plaintiff, type of lawyer, whether or not the worker was registered with Social Security, if s/he was employed in a position of high trust (an 'at-will' worker), the specific claims in the case (reinstatement, overtime, back pay, vacation pay, Christmas bonus, statutory profit sharing, severance pay) and the industry of the firm. We used 70 percent of the data to fit the models and the remaining 30 percent for testing. For each outcome, we used cross-validation to choose the model and variables with the best fit on the testing sample, measured by the correlation between predicted and actual values. Tables A.5 & A.6 in Appendix A present goodness of fit measures for all the models and highlight those we selected.

The models allow us to produce individualized predictions that we shared with parties present at the hearing in cases randomized into the calculator treatment. Figure 4 displays the template we used in Phase 1. The template shows the minimum legal entitlement based on the law if the plaintiff were to win on the issue of unfair dismissal and the probability the case ends in each of the five possible endings. For each of these five endings, we showed the expected amount recovered by the plaintiff. We also show the expected payout across all endings and the percentage of cases that were still unresolved after three years. In Phase 1, we provided the same information sheet to both sides of the case. For Phase 2 we adjusted the format, first

to simplify the information so that it could be explained to parties more quickly, and second to address concerns raised by court officials. In particular, conciliators working for the court suggested that we provide the expected settlement amount, conditional on characteristics, and then provide each side with data indicating the contingency they faced if they did not settle. We developed separate templates for the plaintiffs and defendants which are shown in Figure C.2. For the worker, the no-settlement contingency was the percentage of cases where workers collected nothing, and for firms it was the average amount collected by plaintiffs that won judgments. For the firms, we also showed the recovery amount implied by the law. In addition to using the calculator as a treatment in the experiment, we use it to build a proxy of average overconfidence, as we describe below.

There are several potential sources of bias in the predictions based on the historical data. One is that our sample is composed of cases that were concluded when the models were estimated in early 2016, and 29 percent of cases filed in 2011 and 2012 were still ongoing at that time. If concluded and ongoing cases have different potential outcomes, then although our predictions are unbiased for the concluded cases, they may be biased for a random sample of ongoing cases. Note that if cases end in settlement, they almost always do so within the first 24 months after filing. Since the historical data used in the calculator models cover more than 24 months after filing, very few of the 29 percent of historical cases that were unresolved are likely to end in settlement. Therefore, the projected average payment for cases ending in settlement - the most important variable in the calculator information - is not affected by this censoring issue.

For cases ending in other outcomes - being dropped, expiring, or ending in judgment, the censoring is a larger concern. This potential bias was communicated to the parties when the calculator information was provided. We perform two exercises to estimate how large any bias might be. First, we compare characteristics of ongoing cases with those of the historical cases used in the models. In Figure A.1 we show that the two sets of cases are similar on observables.[9] Second, we compare the characteristics of completed and continuing lawsuits within the historical data. To do this we drew a random sample of 956 cases filed in 2011 that were not finished by 2015. Figure A.2 shows that these 956 unresolved cases are very similar to the completed cases used to develop the models.[10]

[9] An exception is that the experimental cases have a higher rate of claiming reinstatement. This is likely because cases demanding reinstatement typically have longer duration, so that they are less likely to be found in a database of concluded lawsuits.

[10] Recall that the sample used to estimate the model also included cases filed in 2008 and 2009 in Subcourt 7.

A second issue is that even if our predictions are unbiased on average, they are not unbiased for any specific case. Parties may have information about the strength of their case that is unobservable to us. Again, we made clear to the parties that the predictions were based on average outcomes, and outcomes of individual cases will vary depending on the circumstances of the case.

Finally, the calculator predictions will build in any biases contained in previous court decisions. For example, if workers collect less because they are unable to prove wage payments made in cash or because firms avoid making payments ordered by the court, then the calculator will implicitly assume these conditions will continue to apply in the future. Although reforming the institutions so that payments more faithfully reflect the law is a goal of current judicial reforms, we believe the calculator faithfully reflected conditions that the parties in our experiment faced. We view the situation as analogous to providing parents and children with accurate information on returns to public schooling. This allows them to make better schooling decisions given the actual quality of education, but may not directly lead to improvements in the quality of schooling. In our case, the goal of the experiment is to uncover the sources of inefficiencies in the courts to allow reformers to focus on the most critical issues while also providing information with which plaintiffs can make more informed decisions given the way the court actually functions.

# 4 Outcomes and Expectations: Stylized facts

We use the administrative and survey data from phase 1 to document a set of stylized facts about the court. These serve as a motivation for the experiment we implement, but also provide some insight on the functioning of the court. We note whether the source of data for each stylized fact is the historical administrative data or survey data.

**Fact 1. Plaintiffs receive little (Historical Data):** *The amount collected is only 20 percent of the amount claimed on average, and 50 percent of what the law mandates.*

Figure 1 uses the sample of concluded cases to show the amounts claimed and recovered for the 4 main outcomes: settlement, drop, judgment, and expiry. Both the historical data and the Phase 2 case files suggest that around 55 percent of cases end by settlement, 20 percent are dropped or expire and 25 percent end with a judge's decision.[11] For each outcome, the first

[11] The percentages show on Figure 1 reflect the outcomes of the cases that were settled by 2015. As we note

bar shows the average amount of money claimed by the plaintiff. The second bar shows the estimated minimum compensation by law based on the details of the cases. We include items stipulated by current law: severance pay of 90 days at the stated wage, one year of end-of-year bonus and vacation pay, and a tenure bonus mandated for unfair dismissal of up to twice the minimum wage for 12 days per year worked. The third bar shows the amount of money *collected*, on average, including zeros where the plaintiff did not collect anything. The final bar shows the average amount collected conditional on collecting a positive amount. The amount collected is zero in the cases where the lawsuit is dropped, the time expires, the lawsuit is lost, or the lawsuit is won but the plaintiff is unable to collect anything from the defendant. In cases ending with a judgment the worker recovers a positive amount only 24 percent of the time. For either settlements or judgments, the amount received is a small percentage of the amount claimed.

In the 24 percent of court judgments where the worker recovers a positive payment, she receives on average 170 percent of the minimum legal compensation for her case and 37.5 percent of her claim. Figure 1 shows that in expected value, plaintiffs recover less than the minimum compensation according to the law and only 8 percent of their claim in a court judgment. As a result of low recoveries, in a significant percentage of cases with private lawyers, the plaintiff's receives negative (discounted) payoffs. Private lawyers typically charge a fee of around MXN$2000 (USD 100) to file a case and receive 30 percent of any amount collected by the plaintiff. Figure C.4 shows realized recoveries from our 5,005 historical casefiles. After subtracting filing and contingency fees, around 40 percent of cases filed by private lawyers have a negative realized return. The majority of the filings with negative net recovery are cases that are either judgments without collection or cases dropped or expired. However, around 7 percent of the settlements are also for amounts that imply a negative net present value for the plaintiff.

**Fact 2. Long suit duration (Historical Data):** *30 percent of trials started in 2011 had not finished by December 2015. Even conditional on reaching a settlement, the average duration is almost 1 year.*

Figure 2 shows the distribution of case length for concluded cases by type of case ending. Even conditional on being concluded in December 2015, cases ending in judgment take 2.4 years on average. Given that many of the 30 percent of cases filed in 2011 and still open in 2016 are

later, 30 percent of the cases filed in 2011 or 2012 were unresolved at the end of 2015. These unresolved cases are very likely to end either in judgment or by being dropped / expired. The percentages in the text account for this censoring.

likely to end in a judgment, the unconditional average is much higher. Settlements occur, on average, 10 months after filing. Moreover, settlement rates are low by international comparison. Only around 55 percent of MCLC cases are settled. By way of comparison firing disputes are settled after filing in 79 percent of the cases in Australia, in 80 percent of the cases in the United States, and in 90 percent of the cases in Sweden (Ebisui, Cooney and Fenwick (2016)).

The long delays and low settlement rates help to explain the large backlog of cases in the court. Delay has direct costs in the form of court staff time, lawyer fees and the opportunity cost of litigants' time. But delay also harms the parties if, as is likely the case, the plaintiffs discount the future at a higher rate than the defendants. Because awards result in payments from the party with a lower discount rate (the firm) to the party with the higher discount rate (the plaintiff), delay results in a collective welfare loss to the two parties. These delays represent pure efficiency losses.

**Fact 3. Inflated expectations (Survey data):** *The subjective probabilities of winning for plaintiffs and defendants (in the same case) sum to 1.47*[12]*, indicating aggregate overconfidence. There is average overconfidence relative to the calculator's prediction as well.*

Excessive optimism of the parties may result in there being no settlement that is acceptable to both parties, even in cases where settlement would be possible with more realistic expectations.[13] We asked parties present at the hearing the likelihood they would win the case. We also asked, conditional on the plaintiff winning, what amount would be paid. In Phases 1 and 2, the average expected probability of winning reported by plaintiffs is 0.79 and 0.80, respectively, while for firm lawyers it is 0.68 in phase 1 and 0.40 in Phase 2. These probabilities sum to 1.47 and 1.20 in the two phases, respectively. Data from the Phase 3 surveys of workers approaching the court suggests that the overoptimism at least initially comes from workers themselves: prior to beginning the process, workers' stated probability of winning was 89 percent.

By comparison, the probability of the worker winning predicted by our calculator in the same cases is 41 percent in phase 1 and 33 percent in Phase 2. In Phases 1 and 2, there are also large differences in the expected amount of the award conditional on winning. Both the worker and her lawyer estimate average amounts more than twice those of defendants. We can build a

[12]This is the measure of overconfidence used by Yildiz (2003) to explain delay or conciliation in a theoretical bargaining model.

[13]Yildiz (2011) shows that optimism alone is not enough to explain bargaining delays in a static model. However, excessive optimism can lead to an empty contracting zone so that, in the absence of learning, settlement does not occur even when it be efficient in the absence of optimism.

proxy of overconfidence as the difference between the subjective expectation and the calculator prediction. Figure C.5 in Appendix C plots the distribution of overconfidence for different parties for peso amounts conditional on winning and probabilities of winning, and Table C.5 in Appendix C shows that, in Phase 1, plaintiffs and plaintiff lawyers are equally overconfident both with regard to the probability of winning and the expected amount recovered.

**Fact 4. Misinformation (Survey data):** *Only one-third of plaintiffs understand their main legal entitlement. Only half know what they are asking for in their own suit.*

The main legal entitlement for unfair dismissal is 90 days severance pay, a right so fundamental that it is enshrined in the Mexican Constitution and taught in elementary schools. However, Panel (b) of Figure 3 indicates that only 27 percent of plaintiffs responding to the survey know the number of days covered by this entitlement. Even more strikingly, the plaintiffs often do not know what they are asking for in their own suit. In the survey, we asked plaintiffs to: "... mark the items you are asking for in your suit among the following...", listing: Constitutional payment, reinstatement, overtime, holiday bonus, Sunday bonus, and insurance. We assess accuracy by comparing the responses to the case file. Panels (c) to (f) of Figure 3 show the proportion of time the plaintiffs responded correctly to questions regarding elements of their claim. We see that between 20 and 50 percent of respondents answered each element incorrectly. Knowledge of both the law and the case increases in the level of education. Figure 3 also shows that plaintiffs represented by private lawyers are significantly less knowledgeable about the content of their cases than those represented by public lawyers.

**Fact 5. Private lawyers file higher claims, but do not recover more (HD):** *Controlling for observables, private lawyers ask for 86 percent more than public lawyers, but win no more. After paying lawyer fees, the average plaintiff therefore recovers much less with a private lawyer compared with a public lawyer.*

The 100 USD fee private lawyers charge upfront far exceeds the marginal cost of filing a standard case. This gives private lawyers an incentive to inflate claims in order to convince workers to file a suit. With regard to case outcomes we find that, conditioning on five basic variables coded from the initial filing[14], private lawyers ask for 86 percent more, on average. But the ratio of the amount their clients recover to the amount demanded is 5.7 percent lower for

[14]The variables are: gender, at-will worker, tenure, daily wage, weekly hours

private lawyers. The result is that the average recovery is insignificantly lower (by 0.5 percent) for private lawyers. We verify that this is the case in Table C.1.

While the amount recovered is the same for public and private lawyers, plaintiffs receive all of the recovered amount with a public lawyer, and only about 70 percent of the recovered amount with a private lawyer. Hence, plaintiffs with public lawyers receive much larger payouts than plaintiffs with private lawyers, conditioning on characteristics. Of course, these data are only descriptive, and we make no attempt to adjust for the endogenous selection of lawyers beyond the five control variables described above.

# 5 Experimental Intervention

The stylized facts presented above show an environment in which workers are uninformed about their legal entitlements and their own lawsuit, and parties to the case are overconfident on average. Our experiment is designed to address a fundamental question: Given these conditions, does the provision of personalized statistical predictions increase settlement rates?

## 5.1 The treatment

Each phase of the experiment compares the effects the provision of statistical predictions of case outcomes against a control group. During the experimental window in Phases 1 and 2, hearings for which both parties were formally notified were assigned to either the treatment arm or the control group. In the third phase, individuals were assigned to treatment or control based on the day they approached the court. We describe the treatments here, and also a describe a placebo treatment that was implemented in Subcourt 7 during a later period and that was designed to show that experimenter effects are not driving outcomes.

**The Calculator:** Subjects in the treatment arm received a personalized prediction of their case's expected outcomes based on the statistical model described above and the covariates of their own case. The predictions were presented in a single sheet of paper like the one shown in Figure 4, which was used for Phase 1.[15]. We extracted the data needed to customize the calculator predictions from the initial filing (Phases 1 and 2) or from a survey (Phase 3). The data were typed into a user interface. This was done in the presence of the parties in Phase

[15]The format and content change somewhat in Phases 2 and 3. Examples of those information sheets are shown in Appendix C, Figures C.2 and C.3

1, but for logistical reasons, away from the parties in Phases 2 and 3. The predictions were then printed and given to all of the parties present at the hearings in phases 1 and 2, and to the worker in Phase 3. A highly trained enumerator working for the research team spent about 5 minutes explaining to the parties the meaning of the numbers. The enumerators explained that these were only statistical approximations and that they were based on concluded cases from historical records. Enumerators gave no additional legal advice. Where the treatment was administered at a hearing, after explaining the calculator information, the enumerators asked the parties if they wanted to delay the start of their hearing for a few minutes to negotiate with the assistance of a court conciliator.

**Placebo:** 13 months after the end of the treatments in Phase 1 of the project, we implemented an additional treatment arm in Subcourt 7. We were concerned that simply making parties aware of the court's conciliation services, or that the presence of research assistants and the carrying out of surveys, might change the behavior of the parties. With this in mind, we implemented a "placebo" treatment in which we provided a leaflet (see Appendix C Figure C.9) describing the role of conciliators in the court process. The leaflet was provided in format similar to the calculator information, but rather than quantitative predictions it simply said: "Do you know that you could resolve this conflict today? Conciliation is fast, free, confidential and impartial. Subcourt 7 has conciliators. Ask for help!". If a party receiving the placebo treatment asked to talk with the conciliators, our enumerators showed them where they were situated.

## 5.2 Implementation

The first phase of the experiment started in Subcourt 7 on March 2, 2016 and continued daily for 12 weeks. The "subcourt" is not a single courtroom, but rather a room with a waiting area and eight counters conducting simultaneous hearings. Subcourt 7 manages about 55 hearings per day. Each night the court gave us a list of hearings scheduled for the following day, along with their notification status. We worked with the subset of hearings for which both parties were duly notified and therefore required to be present. Among the 20 case files meeting this criterion on a typical day, we excluded hearings scheduled to start at the court's opening hour of 9 AM because the court did not want to delay the start time of the first hearings of the day

for fear of causing cascading delays through the day.[16] Note that cases are assigned to hearing times randomly, so our agreement not to consider 9 AM hearings does not compromise the validity of the experiment. On a typical day, this reduced our sample by around 1.5 cases. In what follows we focus on the remaining sample of roughly 18.5 cases per day. In the first phase, the sample cases were at different stages of the process - that is, not all were new suits.[17]

After receiving the list of cases for the following day, we randomized the eligible cases in equal proportions to the treatment and control group.[18] Control cases followed business as usual, except for the surveys we administered. Each morning we set up a survey table and a calculator module in the waiting area just outside the hearings counters. The hearings were displayed on a screen and parties were called up by the subcourt judge's assistants. Except for the 9 AM hearing slot, the start time of hearings is typically delayed, and we carried out surveys and treatments during parties' waiting time.

Table 1 shows details of the treatments. We began by administering the baseline survey. The survey was conducted blind to the experimental assignment for both the parties and our enumerators. We were able to isolate the survey area from the calculator treatment area, and so avoid contamination, because our sample was only about 18 cases per day. All the parties present were asked to complete the survey, but compliance was optional and in about 70 percent of the hearings, at least one party completed the survey.[19] Treatment status was revealed after the baseline survey, and parties were channeled to their assigned experimental condition and given the appropriate treatment protocol described above.

The implementation of the experiment differed slightly in Phase 2. First, randomization was at the case level in Phase 1 and at the day level in Phase 2. This change was made for logistical reasons, given that during the Phase 2 we were working with a larger number of the subcourts. The second is we intervened in cases at all stages of the process during Phase 1, but

[16] On occasion, there were in excess of eight cases arriving for 9 AM hearings. In these instances, we were able to include some hearings scheduled at 9 AM in our sample.

[17] The experiment in phase 1 also included a second treatment arm, in which parties were referred to the court conciliator. We focus here exclusively on the calculator treatments, leaving the conciliator treatment to future work.

[18] 34 of the 705 phase 1 cases had more than one hearing during the experimental window. We were not able to determine that a case was coming into the experimental sample for a second time, and hence the case was again randomized into treatment or control. For the analysis, we delete the data from the second occurrence of any case, and define the treatment status as the assignment the first time we interacted with the case.

[19] In Phase 1, those completing the survey were told that they would be asked to complete a followup survey after their hearing and were informed they would receive a prize if they did. However, compliance with the post-hearing survey was much lower, as parties did not want to stay after their hearing ended. We do not use these post-hearing survey data for any of the main analysis, though it is included in some of the additional analysis shown in the appendix.

focused on cases holding their first hearing in Phase 2. All first hearings are held on Fridays. Otherwise, the protocol was changed only slightly from Phase 1.

We randomize across Fridays in each of the 5 subcourts during the experimental window. To save time, we shortened the survey and we pre-filled and pre-printed the calculator. The subcourts did not agree to allow us to delay the hearings, so if after receiving the calculator the parties wanted to negotiate with one another, they themselves had to request a delay in the hearing to sit with the court conciliator.

The placebo treatment was implemented in ongoing cases, with hearings Monday through Thursday, in Subcourt 7 during the phase 2 experimental window. For convenience, we randomized the placebo at the bi-weekly level with cases during two weeks given the placebo treatment daily and cases in two adjacent weeks serving as a control group without any intervention. For both groups we coded the variables in the case file and recorded whether there was a settlement on the day of the hearing.

### 5.3 Integrity of the experiment

Table A.3 in Appendix A shows treatment and survey compliance rates for the first two phases of the experiment. We define compliance as the parties being present and willing to receive the treatment. The table shows compliance for each party and at the case level. At least one party received the treatment in 80 percent of cases in phase 1 and 87 percent in Phase 2. We estimate the intention to treat (ITT) in all reported results. Table A.4 in the Appendix C shows that the variables are well balanced across the experimental groups in both phases: only 6 out of 23 tests are significant, 2 at the 5 percent level and 4 at the 10 percent level.

## 6 Results

The historical data and survey responses show that plaintiffs are overconfident and uninformed about the law and even their own case. Settlement rates are low and case durations are long. Our intervention aims to understand if there is a connection between these two sets of facts: If we increase information and reduce overconfidence, do we observe an increase in settlement rates?

### 6.1 Effects on settlement

Given that treatment is randomized, we estimate the causal effect of treatment by estimating the following equation by OLS:

$$y_{it} = \alpha_t + \beta_{tj} T_{ij} + \gamma X_i + \epsilon_{it} \tag{1}$$

The constant $\alpha_t$ estimates the mean for the control group, while $T_j$ indicates assignment to the calculator treatment arm. Thus, $\beta_t$ estimates the ITT effect at a given point in time $t$. We estimate separate regressions for each $t$, with $t$ indicating the day of the hearing or 42 months after treatment (the latter measured in January 2020 for Phase 1 and July 2020 for Phase 2), or two months after treatment in Phase 3. $X_i$ is a vector of controls for case characteristics, including subcourt dummies. Finally, since the effect may differ according to which parties received the treatment, we also interact the two treatment arms with an indicator for whether the employee was present (EP) when we delivered the treatment, while controlling for EP itself. In Phase 2, we add subcourt fixed effects.

The first six columns of Table 3 focus the short-term outcome of same-day settlement. The dependent variable is a dummy for whether there was a settlement on the day of the intervention.[20] The first two columns of Table 3 use data from Phase 1 of the experiment; columns 3 and 4 use data from the second phase of the experiment; and column 5 combines data from the first two phases. Column 1 shows that 6 percent of the control cases in Phase 1 settle on the day of the hearing, while the settlement rate of the treatment group is approximately 5 percentage points higher. The treatment effect is significant at the five percent level.

Column 3 shows that in the second phase of the project, 16 percent of the cases settled on the day of the hearing. Recall that the second phase was conducted with cases holding their first hearing, and the higher settlement rate likely reflects this fact.[21] However, the effect of the calculator treatment is similar in magnitude to that in the first phase: settlement rates on the day increase by 3.9 percentage points in the treatment group compared with control, an effect that falls just below the .10 significance level.

Columns 2 and 4 show our second main result: the treatment effect occurs *only* when the

[20] We use a linear probability model throughout, but the results are robust to other specifications. Column 1 of Table C.6 reports the results of column 5 using a probit specification to show robustness.

[21] Indeed, regressing a dummy variable indicating settlement on treatment using the combined Phase 1 and Phase 2 data shows that there is no difference in settlement rates in the two phases once we control for the age of the case at the time of treatment.

employee is present. In these regressions, we interact treatment with a variable indicating the plaintiff herself was present, while also including a variable indicating that the plaintiff was present. First, note that in both Phase 1 (column 2) and Phase 2 (column 4), settlement on the day is much more likely when the employee is present. In the control group, 19 percent of the Phase 1 cases and 27 percent of the Phase 2 cases are settled on the day of the intervention when the employee is present. But treatment increases settlement rates by 15.5 percentage points in Phase 1 (0.026 + 0.129) and 14.1 percentage points in Phase 2 when the employee is present. The joint effect of the treatment and the treatment / employee present variables are significant at just below the 5 percent level in Phase 1, but not quite at the .10 level in Phase 2. Moreover, the calculator treatment in either phase when the employee is *not* present is close to zero and highly insignificant particularly in the second phase (row 1 in columns 2 and 4).[22] The effect of the treatment when the employee is present increases settlement rates by enough to significantly close the gap with those of developed countries referenced above.

The Phase 2 results provide a replication within the experiment, and the similarity of results in Phases 1 and 2 is reassuring. Combining the samples increases statistical power. We do that in Column 5 using the specification from columns 2 and 4. Not surprisingly, we find very similar treatment effects, with the treatment - employee present interaction effect now itself significant at the 5 percent level, and the effect of the calculator when the employee is not present remaining very close to zero.

The regressions in the first five columns measure the effect of treatment on immediate settlement. The results suggest that lawyers do not act on the calculator information in the absence of their client. But might they share the information with their client after the hearing, producing a delayed effect on settlement? The court's administrative records allow us to track cases over time. Column 7 shows the effect of treatment around 42 months after treatment in December 2019/January 2020 for Phase 1 and July 2020 for Phase 2. The 42-month window allows for several additional hearings in the case, and, indeed, is after almost 90 percent of cases are resolved.

Our third main result is that the effect of treatment does not change materially at any point up to 42 months after the intervention, even though the number of cases settled overall

[22] Table C.3 in Appendix C examines balance in key variables in the subsample of cases where the employee is present. Table C.4 in the Appendix C tries to predict EP using case characteristics with mild success. Employees are more likely to attend in cases with public lawyers and when they had a long tenure at the firm, and less likely to attend when they worked longer hours at the firm.

increases substantially. Focusing first on cases where the employee was not present at the hearing, comparing column 5 with column 7, we see that in the control group, the settlement rate increases from 11 percent to 44 percent. Meanwhile, the effect of the calculator when the employee was not present (row 2) remains a fairly precisely estimated zero (and, indeed, if not slightly negative). Where the employee was present to receive the treatment, the treatment effect also remains almost unchanged over time. The 13 percentage point effect on the day of the hearing drops (insignificantly) to 11 percentage points after 42 months.

Treatment is random conditional on the presence of the employee at the hearing, but we might be concerned with the endogeneity of employee presence itself. While this is fundamentally an external validity issue (since treatment is random conditional on the plaintiff's presence), it is relevant for how we interpret the null treatment effect when the employee is not present. Linking this finding to plaintiff-lawyer agency issues implies that settlement rates would have been higher had the employee been present in the subset of cases where she was not present. We might be concerned that plaintiffs are present when there is potential for the case to be settled, and not present when there is little potential for settlement. However, the long-run follow-up data suggest that the plaintiff's presence on the day is not determinant of settlement in the control group. First, among cases in the control group where the employee *was* present on the day, the effect of the employee's presence dissipates over time; 42 months after treatment, the effect in the control group is no longer significant (row 2, column 7 of Table 3). Second, among the control group cases where the employee was not present on the day, an additional 33 percent of the cases settled over the following 42 months. Taken together, the results imply that neither the presence nor absence of the plaintiff on the day of the intervention determined settlement in the longer run among the control group cases. On the other hand, settlement in the treatment group was affected by the presence of the employee, both on the day of treatment and in the longer run.

To address any residual concerns with the endoegeneity of the plaintiff's presence, in column 6 we use a control function approach (Wooldridge (2015)), using settlements on the day of treatment as the outcome.[23] Employees are more likely to be present when their hearings are scheduled for one of the first two hearing times (9:00 or 9:30) or the last hearing times (12:00 or 12:30). Hearing times are assigned to cases randomly, and we find that a dummy variable

[23]Wooldridge (2015) shows that the control function approach is equivalent to instrumental variables when all specifications are linear, but has advantages when the first stage is non-linear and the second stage includes interaction terms. Both of these hold in our case.

indicating the two early / two late hearing times is highly significant in predicting employee presence.[24] The results indicate that the control function variable itself is significant at the 5 percent level, but the control function has little effect on the magnitude and significance of the interaction between employee presence and treatment.

In Phase 3 of the experiment, we provide the calculator to *all* of the dismissed workers in the treatment group. Recall that our sample for Phase 3 is dismissed workers approaching the court seeking information. Column 8 shows the treatment on settlement in this sample prior to filing a case. Note that almost two in five (39 percent) of the control group in Phase 3 settle their case by two months after they come to the court. The calculator nevertheless significantly increases settlement: an additional 6.4 percent of workers assigned to receive the calculator treatment settle before filing. This represents 10.4 percent of the 61 percent of workers who would not have settled without treatment, an effect size only slightly smaller than the effect when the plaintiff was present in Phases 1 and 2.

Collectively, the results from Phases 1 and 2 suggest that the lawyers do not share the calculator information with clients. The historical and survey data give us reason to suspect that agency issues might be particularly relevant in cases with private lawyers. In Table 4, we separate plaintiffs according to whether they are represented by a private or public lawyer. We repeat the regressions in columns 5 and 6 of Table 3 for each type of lawyer. The results show that the calculator has an effect only when the plaintiff is represented by a private lawyer.[25] In cases where the plaintiff is represented by a private lawyer, the calculator treatment increases settlement by 19 percentage points when the plaintiff is present, and not at all when the plaintiff is not present. Both of these outcomes change only slightly 42 months later. Moreover, for the control-group cases represented by private lawyers, the effect of employee presence drops from 14 percent to 5 percent over the 42 months, suggesting again that the presence of the employee on the day of the experiment is not in itself determinant of longer-run outcomes in the case. Meanwhile, the treatment has no significant effect on settlement in the much smaller sample of plaintiffs represented by public lawyers. That agency underlies this pattern is also suggested by the data on Figure 3, which shows that plaintiffs using private lawyers are significantly less

[24] The first stage regression is shown on Table C.6. We might worry that the time of the hearing affects settlement for reasons other than the plaintiff's presence. We can not rule this out, though we note that in the control group, the time-of-hearing dummy does not significantly predict settlement when the employee is not present (p=0.65).

[25] As with the employee being present, the choice of lawyer is endogeneous, but the treatment is orthogonal to the type of lawyer.

informed about the contents of their case than are plaintiffs using public lawyers.

One concern is that the parties may believe the calculator information is provided by experts, and so simply agree to settle for the amount presented to them. Figure C.6 in Appendix C shows the ratio of the agreed settlement to the calculator predicted settlement for all cases ending in settlement. We find that only 29 percent of settlements in the treatment group are within 25 percent of the calculator prediction. This is indeed higher than the 24 percent of control-group settlements that fall within this band, but nevertheless suggests that the calculator served only as a guide for initial bargaining. Figure 5 shows the distribution of calculator predictions among cases that settle in the treatment and control groups. The figure shows that the increased settlements come disproportionately from cases with modest predicted settlement amounts.

We read the collective results as indicating that plaintiff-lawyer agency issues are important in this context. Private lawyers appear not to transmit evidence to plaintiffs who are not present to receive the information directly. Of course, it is possible that lawyers do not explain the calculator to their clients because they are unable to recall the meaning of the data provided on the sheet. Given the simplicity with which the data are presented, we find this unlikely. However, even if the failure to pass on the information to their clients simply reflects the difficulty lawyers have in explaining the calculator, the lack of a treatment effect when the plaintiff is not present indicates that the plaintiff does not fully trust her lawyer to make decisions on her behalf.[26]

A final result, shown in Appendix C Table C.8 is that the placebo has no effect on settlement. The placebo makes parties aware of the availability of the court conciliation process, but provides no information on their own case. We interpret the lack of any effect of the placebo treatment as evidence that the content of the calculator information matters.

### 6.2 Case outcomes

What is the counterfactual outcome for the cases induced to settle by treatment? We examine this first in Table 6 by looking at the pattern of case outcomes in the control and treatment groups for the first two phases of the experiment. We accessed administrative records for the Phase 1 cases in December 2019 and January 2020 and the Phase 2 cases in July 2020. By those dates, only 12 percent of the cases remained unresolved. The outcomes on Table 6 suggest that,

[26]The possibility that, when the calculator is explained to both the plaintiff and her lawyer in person, the lawyer does not understand the calculator while the plaintiff does seems highly implausible given that the lawyers have both more education and more experience in labor cases.

in aggregate, the treatment shifts cases from court rulings without collection to settlements. Compared with the control group, settlement rates are 6.5 percentage points higher and court judgments without collection 7.3 percent lower in the treatment group. For the purpose of Table 6, we have classified plaintiffs as winning if the judge rules in their favor, regardless of whether or not they are able to collect the award from the defendants. As we have noted, collection of the award is far from automatic. Indeed, as of the last date we accessed the records, there were 50 cases where plaintiffs had won but not yet collected anything from the defendants. Moreover, in 17 cases where the plaintiffs had won judgment and collected a positive amount, they recovered only 52 percent of the judgment, on average.

### 6.3 Effects on overconfidence

The calculator treatment provides information on likely outcomes of the case. One channel through which the treatment may be effective is by reducing excessive optimism of the parties. Ideally, we would measure beliefs both before and after treatment in both the treatment and control group. We faced operational challenges in constructing this measure in all three phases of the experiment. In Phase 1, the baseline survey data indicate initial overconfidence, but compliance rates with the follow-up survey conducted after the hearing were low, as parties were anxious to leave immediately after the hearing. Nevertheless, the data available from Phase 1, analyzed in Table C.9 in Appendix C, indicate that the treatment lowered the expectations of overconfident plaintiffs. The data from the Phase 3 provide stronger evidence that the treatment tempered optimism, albeit somewhat modestly. As we noted above, in the Phase 3 baseline survey indicates the average worker believed they had an 89 percent chance of winning their case. After presenting them with the calculator information, we elicited expectations a second time. Table 5 shows the difference between the initial and subsequent expectations among the treated sample. After reviewing the calculator information, respondents decreased their probability of winning by 6 percentage points, with 30 percent reporting a lower probability of winning, and 26 percent reducing the amount they expect to recover. However, on average they remain optimistic relative to the historical outcomes, with the probability of winning falling only six percentage points. Note that the regressions on Table 5 use only the treated sample from Phase 3. We initially asked the control group for updated expectations at the end of the baseline survey, but the most common response was agitation that we were asking them the same question again almost immediately, with no reason for their answer to change. As a

result, we stopped asking for expectations a second time from the control group, However, the small sample from the initial control group surveys suggest that no change in expectations is the appropriate counterfactual for the calculator treatment group.

### 6.4 Effects on Case Duration

We should expect that the increase the number of settlements on the day of the treatment will result in shorter average case duration. Table C.10 in Appendix C shows the results of regressing the duration of cases with a private lawyer against the treatment, employee present and the interaction of the two. We define duration as the number of days from the date of the case filing to the final resolution of the case. For cases still unresolved on the date the records were last checked, we use the date we last checked administrative records as the final date. We begin in the first column of C.10 with an OLS regression of case duration. Consistent with the results from Tables 3 and 4, we find that the treatment has no effect on case duration when the plaintiff is not present, but reduces duration by almost four months when the plaintiff is present, and effect significant at the .10 level. The OLS specification ignores the censuring of duration. The percentage of cases unresolved as of the last check is both modest (12 percent) and balanced between treatment and control. In Column 2, we use a Cox proportional hazard model, finding very results very consistent with the OLS results in column 1. Finally, in column 3 we exclude from the sample cases that were settled to test whether the treatment had any effect on the trajectory of cases through a channel other than settlement. The small and highly insignificant effect of the treatment, regardless of the presence of the plaintiff, suggest that treatment affected duration only through the increase in the settlement rate.

Faster resolution of cases is a benefit to the court, helping to reduce congestion. It also plausibly increases the welfare of the parties in a situation where the party paying the judgment likely discounts the future at a lower rate than the party receiving the treatment.

## 7 Is the Increased Settlement Rate Beneficial?

The increased settlement rate generated by treatment helps the court meet its goal of reducing the case backlog. There is evidence that reducing backlogs leads to more efficient outcomes in the economy (Ponticelli and Alencar (2016)). But are the settlements induced by treatment directly beneficial to the parties who receive the treatment? A necessary condition for the

increase in settlements to be beneficial is that the untreated settlement rate is inefficiently low. In this section, we first discuss a simple framework that shows why settlement rates might be inefficiently low. We then analyze the data on the actual outcomes of cases in Phases 1 and 2, and surveys of well-being from Phase 3. Because almost 90 percent of cases in Phases 1 and 2 were resolved by 2020, we are able to measure outcomes for plaintiffs directly, with few cases requiring projected outcomes.

The discussion highlights reasons that the plaintiff's incentives may be misaligned with those of her lawyer, underlining analytical reasons that settlement rates may be inefficiently low. While contingency contracts help to align incentives, they do not eliminate agency issues, particularly with regard to settlement decisions. The empirical analysis shows that the plaintiffs are made better off, on average, by the increase in settlement rates, a finding that is robust to a variety of assumptions on outcomes for the continuing case files.

### 7.1 Risk, Discounting and Settlement Incentives

Treatment increases the probability of settlement, but only when the employee is present. We also find that plaintiffs and the parties collectively are excessively optimistic about their chances of winning their case. While there are four parties involved in the case, for this discussion we focus on cleavages between the plaintiff and her lawyer, which appear to play a central role in the results shown on Tables 3 and 4.

The bargaining literature defines the "settlement range" as the range of outcomes that both parties prefer to continued bargaining. The settlement range will be empty if the parties to the dispute are risk-neutral, have identical discount rates and have identical expectations over outcomes. However, if, for example, future outcomes are uncertain and parties are risk-averse, then both parties may prefer to settle for the (certain) expected outcome to remove the risk associated with the uncertain future outcome. Myerson and Satterthwaite (1983) shows that bargaining can break down even when there is a non-empty settlement range, that is, even when both parties prefer settlement over continued bargaining. This classical explanation for sub-optimal settlement rates may be a factor in the generating inefficiently low settlement rates at the MCLC, but the experimental results suggest this is not the only factor. Given the apparent divergence between the plaintiff and her lawyer, we focus on factors that drive wedges in settlement preferences between plaintiffs and their lawyers. Are there primitive factors that lead to plaintiffs preferring to accept a given settlement offer while their lawyers prefer to pursue

the case? We write down a simple framework in Appendix B that highlights two factors: the ability to diversify risk and differences in discount rates.

The framework formalizes what we view as fairly intuitive points. The plaintiff and her lawyer differ in their ability to diversify risks and, given income differences, likely in their discount rates as well. The framework in Appendix B shows why differences in these two dimensions lead to differences in preferences over taking a certain payoff now (i.e., a settlement) or an uncertain payoff later (i.e., a court judgment).

In deciding between an offer of settlement now and an expected outcome from continuing to pursue the case, each party will account for the difference in timing of the two potential payments, and the uncertainty of the payoff from continuing the case. Taking uncertainty first, lawyers typically handle many cases simultaneously. For example, the median number of ongoing cases among lawyers surveyed in Phase 1 reported was more than 30. Because the outcomes of each of a given lawyer's cases are largely independent of one another, the lawyers are able to diversify the payoff risk inherent in pursuing any single case. Plaintiffs, on the other hand, are a party to only a single case, and therefore cannot diversify that risk. As both intuition and the formal model show, the difference in the ability to diversify risk leads plaintiffs sometimes to prefer the certainty of settlement when their lawyer would prefer the uncertain outcome inherent in pursuing the case: the settlement range between the firm and the plaintiff's lawyer is a subset of the settlement range between the firm and the plaintiff.

The parties may also differ in the rate at which they discount the future. Because plaintiffs will have recently lost a job and will generally have lower incomes even when working, we expect them to be more impatient than their lawyer. For any expected future payout following a judge's decision several years later, the higher discount rate would lead the plaintiff to accept a lower settlement today than her lawyer would be willing to accept. The differences in either the ability to diversify risk or the rate of discount drive a wedge between preferences of the plaintiff and her lawyer, with the plaintiff having a larger settlement range than her lawyer.[27] Again, our objective here is not to be exhaustive in the modeling, but to show that there are plausible reasons that the incentives of the plaintiff and her lawyer are misaligned, and that the misalignment results in too few settlements from the perspective of the plaintiff.

The divergence in preferences of the plaintiff and her lawyer provides an incentive for the

[27] This could be offset by differences in the opportunity cost of time of the lawyer. However, lawyers typically manage many cases in the same court building, reducing the opportunity cost of time spent on a given case.

lawyer not to de-bias the expectations of the plaintiffs - for example, by showing the calculator information to the plaintiff. Hence, it provides a rationale for the calculator treatment being effective only when the plaintiff is present to receive the information directly. Finally, the divergence is also sufficient to produce a settlement rate which is sub-optimally low from the perspective of *both* the plaintiff and the firm.

### 7.2 Empirical Evidence on the Induced Settlements and Plaintiff Welfare

The framework motivates why the plaintiffs *might* be better off with more settlement. But what do the data say about whether they *are* made better off by the additional settlements our treatments induce? We examine those data here to provide evidence on the welfare of the plaintiffs induced to settle through the treatments.

Table 6 showed that, on average, the primary effect of treatment was to shift outcomes for the plaintiff from a losing court judgment to a settlement. However, these average outcomes may mask more nuanced underlying patterns. Here we instead analyze the effect of treatment on the amount awarded to the plaintiffs. Table 7 reports the results of regressing the net present value of the amount collected against treatment. For cases with a private lawyer, we reflect legal fees by multiplying the recovery by 70 percent to reflect the plaintiff's share and then subtracting the 2000 MXN up-front fee. We use the amount awarded for both settlements and court judgments. While settlements are collected in all but a very few cases, judgments are not. So for judgments, this will overstate the amount recovered. For the 12 percent of cases that were continuing at the time of the latest update, we assume either that the cases recover the amount that the calculator predicts when their case goes to judgment[28] (columns 1 through 3), or that they recover nothing (columns 4 and 5). We discount all payments to the date of filing using the discount rate reported reported by the median plaintiff in the Phase 1 survey, which is 50 percent per year.[29] We revisit the assumption on the discount rate below.

In Table 7, we regress the net present value of the amount awarded against treatment, controlling for subcourt fixed effects, year of filing, number of plaintiffs in the case, and use of a public lawyer. In all the regressions, we interact treatment with the presence of the plaintiff on the day of treatment. The outcomes have a very long right-hand tail, and so we have to

[28]That is, the recovery conditional on winning the case times the probability they win the case, conditional on the case ending by a court ruling.

[29]Figure C.10 in Appendix C shows the discount rate data elicited from surveys, along with comparable data from the Mexican Family Life Survey for 2009. Microcredit interest rates in Mexico are closer to 100% per year.

decide how best to handle these. On the one hand, there is no reason to believe they do not represent real outcomes, though in fact many of the largest awards are reduced through post-trial bargaining, or are not collected by plaintiffs. In columns 1 and 4, we measure the NPV in levels, but winsorize the data at the 95th percentile to reduce the influence of the upper tail of the data. In levels, we find no significant effects of treatment on the NPV of outcomes, either when we use the calculator to impute values for unresolved cases (column 1) or when we impute 0's for those cases (column 4). The measured effect of the treatment when the plaintiff is not present is negative, and the measured effect when the plaintiff is present is positive. However, both effects are highly insignificant. Column 2 instead uses the inverse hyperbolic sine of the outcomes. In the IHS specification, we find that treatment when the plaintiff is not present has no effect, while treatment when the plaintiff is present has a significant positive effect. The pattern in column 2 is similar to the effects of treatment on settlement, showing that the treatment left plaintiffs better off only when the plaintiff was present to receive the treatment directly. The IHS results put increased weight on the left-hand part of the distribution of outcomes. Table 6 shows that the treatment shifts some losing cases to settlement; these are likely to be settlements of modest value. In Figure C.7 in Appendix C, we show the distribution of the amount awarded when the plaintiff was present at the hearing, by treatment round. The distribution of awards in the treated sample has less mass in both tails, the left-hand tail reflecting the results from Table 6, and the right-hand tail perhaps reflecting settlements of cases that would have yielded a higher award if they had proceeded to judgment. Because the IHS specification raises the weight of observations in the left-hand tail and lowers the weight of those in the right-hand tail, the results are more favorable for treatment.

Columns 3 though 5 of Table 7 show that the estimated effects are robust to changes in two key assumptions. First, in column 3, we discount all payments at an annual rate of 30 percent - lower than a typical credit card rate in Mexico during this period. Comparing columns 2 and 3, we see that the discount rate has almost no effect on the estimated treatment effect. Second, although almost 90 percent of the cases were resolved by 2020, the analysis of the effects of treatment on plaintiff outcomes still depends on assumptions about the remaining unresolved cases. In the first three columns, we assumed that the unresolved cases would all result in plaintiff being awarded the amount predicted by the calculator, conditional on the case proceeding to judgment. In Columns 4 and 5, we instead assume that the plaintiff loses the case and collects nothing. Consistent with this, columns 4 (levels) and 5 (IHS) show that the

assumption on recovery from the unresolved cases has little effect on the estimated treatment effects, though the effect of treatment when the plaintiff is not present is now marginally significantly negative in the IHS specification. The general robustness to how we impute values for the unresolved cases is perhaps not surprising given that, as we noted earlier, treatment has less effect on settlement among plaintiffs with the highest-value cases (see Figure 5). Further, figure 6 shows that, among the unresolved cases, the calculator predicts much larger recoveries for the treatment cases than for the control cases, conditional on winning a court judgment.

The cases of the workers involved in Phase 3 of the experiment are not far enough along to carry out a similar analysis. However, in the survey conducted two months after the treatment in Phase 3, we asked workers about well being more directly. In particular, we asked them a standard question about general happiness and two questions about difficulties in paying bills. We examine the effect of treatment on responses to these questions on Table 8. We find no effect on happiness, though the measured effect is positive. However, we do find that the calculator treatment reduced the likelihood they report having had trouble paying bills or not having money for food. These are, of course, short-term impacts, but they are consistent with the higher settlement rates for Phase 3 treatment we observed on Table 3, and also consistent with the importance of cash from settlement while the dismissed workers search for a new job.

In sum, the data provide evidence that the treatments left the plaintiffs are better off when they were present to receive the treatment directly. The gains come both from receiving payment earlier and from receiving payments in cases that would otherwise have been lost. Factoring in risk and the likelihood that the additional settlements come from cases with relatively weak unobservable characteristics, we believe the data show that the typical plaintiff is benefited by the treatment.

# 8 Conclusion

There is substantial evidence that delays in court proceedings are detrimental to the economy. The Mexico City Labor Court is emblematic of dysfunctional courts in lower- and middle-income countries. The MCLC suffers from large backlogs and parties to cases face uncertain outcomes. We show that a straightforward information intervention increases settlement rates both on the day of treatment and in the longer run. The induced settlements remove cases from the court docket more quickly. This contributes to reducing case backlogs – though of course it is possible

that, at scale, this effect would be partially offset by an increase in the use of the courts by dismissed workers.

Comparing the treatment- and control-group outcomes of the ongoing cases involved in the experiment, we find that the increase in settlements comes almost entirely from cases that would have been lost by the plaintiff. Given the counterfactual of obtaining nothing in these cases, the plaintiffs are clearly better off. There are three reasons to believe that the intervention also improves overall welfare. First, the increase in settlements contributes to reducing the court backlog. We should expect this to have a positive effect on the speed of other cases in process. Evidence from other settings shows positive effect on financial (Ponticelli and Alencar (2016)) and other markets (Boehm and Oberfield (2020); Chemin (2020)) from more rapid resolution of cases. Second, the worker-dismissal cases filed in the MCLC involve a payment from one party (the firm) to other parties (the plaintiff and her lawyer). These payments themselves are zero sum. In this setting, it is very likely that the discount rates of firms are lower than those of plaintiffs; in those circumstances, earlier payment will improve the collective welfare of the parties. Finally, reaching and enforcing agreements consume resources of the court; faster settlement reduces these administrative costs. Intervening before cases are filed would appear to maximize these administrative benefits.

The experiment also illuminates several underlying causes of the delays. First, parties to the case, and plaintiffs, in particular, show excessive levels of optimism and limited knowledge of the law. Second, lawyers take advantage of the naivete of the plaintiffs, extending cases when workers would prefer settlement. There are undoubtedly other issues, particularly those related to enforcement of court rulings and delivery of summons leading to longer lags between hearings. Indeed, the results suggest the need for further work to better understand the functioning of the market for lawyers in this context.

More broadly, the results of the experiment suggest the need to merge the insights of the bargaining literature with those from the literature on expert agents. The literature on bargaining and settlements has focused on the relationship between the plaintiff and defendant, and de-emphasized the importance of agency issues between a party and her lawyer. Given the importance of the employee being present to receive the information directly, we should view the bargaining game as one that involves more than two parties. In our case, for example, this is most apparent on the plaintiff's side, where lawyers are informed experts, and plaintiffs are mostly first-time users of the court. The results suggest the need to explore ways to create and

disseminate information on the quality of lawyers, allowing higher-quality lawyers to develop a reputation.

Finally, the experiment provides a window on the functioning of the court as an institution. In conducting the experiments, we worked closely with the court, and provided officials there with evidence about effective and easily scalable policies; thus, the research has contributed to the policy dialogue on general policies at the court. Indeed, as part of a major constitutional reform of labor law, the court proposed that federal labor law include both statistical information customized to the case, and pre-filing conciliation hearings  proposals that were grounded in the evidence from the experiment.[30]

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[30]These proposals were passed by the Federal Senate in mid-2019. See text at: https://goo.gl/9AZ6H7.

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# 9 Tables & Figures

Table 1: Description of Treatment Arms

| | Types of cases | Court | # cases | # treated cases | Treatment | Meassurements | Dates |
|---|---|---|---|---|---|---|---|
| Phase 1 | All hearings | Subcourt 7 | 636 | 315 | Point estimates on the probability, time, and monetary returns of case outcomes. | Baseline survey and admin data 42 month followup | 02/03/2016 to 27/05/2016 |
| Phase 2 | 1st hearings | 5 subcourts | 1096 | 768 | Point estimate on settlement amount, conditional on settling. Point estimate of not receiving any compensation, conditional on court ruling. | Baseline survey and admin data 42 month followup | 14/10/2016 to 10/02/2017 |
| Phase 3 | Dismissed workers before finding a lawyer | All workers approaching the information booth. | 1929 | 925 | 95% confidence intervals regarding expected returns and duration of the lawsuit. | Baseline, 2w and 2m followups, admin data for 30 months | 15/05/2017 to 17/08/2018 |

*Notes:* This table presents the main characteristics of the three treatment arms.

Table 2: Summary Statistics

| Variable | HD | HD Subcourt 7 | Phase 1 | Phase 2 | Phase 3 |
|---|---|---|---|---|---|
| | Panel A: Outcomes | | | | |
| Win | 0.65 | 0.69 | | | |
| | (.48) | (.46) | | | |
| Amount won | 23917 | 20358 | | | |
| | (56302.54) | (47488.41) | | | |
| Total asked | 343060 | 301566 | 598315 | 644064 | |
| | (655168.15) | (551154.03) | (1438490.59) | (3860993.2) | |
| Conciliation | 0.63 | 0.67 | 0.22 | 0.2 | |
| | (.48) | (.47) | (.41) | (.4) | |
| Losing court ruling | 0.07 | 0.01 | | | |
| | (.25) | (.1) | | | |
| Winning court ruling | 0.02 | 0.02 | | | |
| | (.14) | (.12) | | | |
| Duration (years) | 1.02 | 0.98 | | | |
| | (.96) | (.71) | | | |
| | Panel B: Basic variables | | | | |
| Public Lawyer | 0.1 | 0.15 | 0.11 | 0.06 | |
| | (.3) | (.36) | (.32) | (.24) | |
| Female | 0.48 | 0.35 | 0.41 | 0.46 | 0.45 |
| | (.5) | (.48) | (.49) | (.5) | (.5) |
| At will worker | 0.07 | 0.06 | 0.18 | 0.08 | |
| | (.25) | (.23) | (.39) | (.27) | |
| Tenure (years) | 4.17 | 3.72 | 153.44 | 4.33 | 3.71 |
| | (4.99) | (4.72) | (725.65) | (5.74) | (4.74) |
| Daily wage | 470 | 455 | 740 | 605 | 323 |
| | (1100.8) | (656.22) | (1360.94) | (1007.75) | (634.99) |
| Weekly hours | 57.33 | 57.36 | 57.37 | 56.15 | 52.88 |
| | (15.47) | (15.57) | (16.87) | (13.38) | (15.52) |

*Notes:* The table presents summary statistics of different samples in columns. Column 1 uses the complete sample of cases that started 2011 and were finished by December 2015, for 5 subcourts chosen to represent the majority of the industries at the Mexico City labor court. Column 2 limits these cases to only Subcourt 7, where we ran the phase 1 experiment. Column 3 uses Subcourt 7 casefiles that were subjects of the phase 1 experiment. Column 4 does the same for phase 2. Panel A shows outcomes: the fraction win for workers, the amount won (includes zeros, actually recovered which may not coincide with what the judge ordered), the total amount asked by the worker in the initial filing, the fraction that settled over the lifetime of the lawsuit, the fraction that reached a court ruling with positive recovery for the worker, the fraction that reached a court ruling with zero recovery, and the duration of the lawsuit in years (a case can have one of 4 types of endings: court ruling, settlement, expiry, and case dropped). Panel B shows some of the main characteristics of the case from the initial filing. These include the fraction that are represented by a public lawyer, the fraction of women, fraction of at will worker (who cannot be reinstated but receive higher severance pay under the law), worker tenure at the firm, daily wage and total hours per week. We include these variables since they are essential for calculating the amount of money that the worker is owed under the law for unfair dismissal.

Table 3: Effect of Treatment on Settlement

| | Months after treatment | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Same day settlement | | | | | | Long run | |
| | Phase 1 | | Phase 2 | | Phase 1/2 | | | Phase 3 |
| | OLS | | | | | CF OLS | OLS | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Calculator | 0.052** | 0.026 | 0.039 | 0.006 | 0.016 | 0.002 | -0.022 | 0.064** |
| | (0.022) | (0.020) | (0.025) | (0.023) | (0.015) | (0.015) | (0.023) | (0.027) |
| Emp present (EP) | | 0.144** | | 0.148* | 0.147*** | 0.698*** | 0.055 | |
| | | (0.054) | | (0.069) | (0.043) | (0.198) | (0.053) | |
| Calculator#EP | | 0.129 | | 0.135 | 0.134** | 0.122** | 0.109* | |
| | | (0.084) | | (0.088) | (0.059) | (0.058) | (0.064) | |
| Control Function | | | | | | -0.312*** | | |
| | | | | | | (0.105) | | |
| Control group mean | 0.0623 | 0.0623 | 0.165 | 0.165 | 0.114 | 0.114 | 0.444 | 0.387 |
| Interaction control group mean | | 0.194 | | 0.275 | 0.230 | 0.230 | 0.496 | |
| Observations | 636 | 636 | 1,096 | 1,096 | 1,732 | 1,732 | 1,732 | 1,927 |
| R-squared | 0.057 | 0.145 | 0.056 | 0.117 | 0.139 | 0.144 | 0.112 | 0.007 |
| Calculator p-value | | 0.0539 | | 0.120 | 0.0101 | 0.0255 | 0.156 | |

*Notes:* This table estimates the main treatment effects for both experimental phases. Columns 1 through 5 measure settlement on the same day as the treatment, with the sample in columns 1 and 2 limited to phase 1, the sample in columns 3 and 4 limited to phase 2, and the sample in column 5 combining data from the two phases. Column 6 shows the effect of treatment on settlement around 42 months after the treatment, using the combined sample from the first two phases. Column 7 uses treatment-day settlements but adds a control for potential endogeneity of the presence of the employee, as described in the text and shown in Table C.6. Column 8 shows the effect on settlement two months after treatment in phase 3, using data from the follow-up phone surveys. Note that in phase 1, treatment is at the casefile level, while in phase 2 it is at the day level. "Calculator" is a dummy indicating that the casefile or day was randomly assigned to receive the calculator. "Emp present" is an indicator for the employee being present on the treatment day, and "Calculator#EP" is the interaction between the employee present and treatment dummies. Columns including Phase 2 observations include subcourt dummies.

Table 4: Treatment Effects conditional on type of lawyer

| | Private | | Public | |
|---|---|---|---|---|
| | Same day | Long run | Same day | Long run |
| | Phase 1/2 | | | |
| | (1) | (2) | (3) | (4) |
| Calculator | 0.020 | -0.000 | 0.014 | 0.023 |
| | (0.017) | (0.027) | (0.049) | (0.134) |
| Emp present (EP) | 0.143*** | 0.052 | 0.251** | 0.274** |
| | (0.044) | (0.065) | (0.111) | (0.135) |
| Calculator#EP | 0.192*** | 0.154* | -0.056 | -0.048 |
| | (0.066) | (0.089) | (0.125) | (0.181) |
| Control group mean | 0.115 | 0.419 | 0.160 | 0.420 |
| Control group interaction mean | 0.229 | 0.446 | 0.269 | 0.538 |
| Observations | 1,554 | 1,554 | 132 | 132 |
| R-Squared | 0.143 | 0.086 | 0.085 | 0.079 |
| Calculator p value | 0.002 | 0.07 | 0.709 | 0.825 |

*Notes:* The table reproduces the regressions in columns 5 through 5 of 3. Columns 1 and 2 use the sample of cases in which the plaintiff uses a private lawyer and the columns 3 and 4 the sample of cases where the plaintiff uses a public lawyer. We are unable to determine whether the lawyer is public or private in 60 of the cases. The dependent variable is a dummy indicating the case was settled by the time indicated on the column heading. All regressions include sub-court dummy variables as controls. See the notes to 3 for further details.

Table 5: Immediate expectation updating

| **Information Experiment: Immediate updating** | | | | |
|---|---|---|---|---|
| | Probability | | Amount | |
| | Dummy (lowered) | Continous | Dummy (lowered) | Continous (log difference) |
| | (1) | (2) | (3) | (4) |
| Calculator | 0.30*** | -0.061*** | 0.26*** | -0.0027 |
| | (0.021) | (0.0077) | (0.028) | (0.035) |
| Constant | -0.000044 | -0.0056 | 0.0023 | -0.0032 |
| | (0.0098) | (0.0041) | (0.014) | (0.014) |
| Source | 2m | 2m | 2m | 2m |
| Observations | 1274 | 1274 | 578 | 578 |
| Obs per group | 663/611 | 663/611 | 315/263 | 315/263 |
| Days per group | 90/103 | 90/103 | 82/90 | 82/90 |
| R-squared | 0.19 | 0.058 | 0.16 | 0.0060 |

*Notes:* The table uses the Calculator treatment to estimate its effect on immediate expectation updating. Column (1) regresses an indicator for "decreased expectations" (a variable a dummy = 1 if the the subjective belief of the probability of winning measured in the immediate follow up survey is lower than in the baseline survey) on treatment arms dummies and basic variable controls. On the other hand, Column (2) regresses the amount by which expectations changed with the same regressors. Recall that the immediate follow up survey was implemented less than 3 minutes after the treatment was given. Using the immediate survey instead of the 2w or 2m survey avoids attrition, however since it was implemented only in the Information Experiment, this table does not have a Panel B. Column (3) is the same as Column (1) except that the dummy refers to amount to be won conditional on winning instead of probability of winning, while column (4) mimics column (2) referring to the quantitative difference of the amount the subject thought she might win.

Table 6: Case outcomes by treatment arm

| | Control | Calculator | Difference |
|---|---|---|---|
| **Resolved with recovery:** | | | |
| Settled | 41.33% | 47.82% | 6.49% |
| Court ruling with payment | 6.19% | 5.76% | -0.44% |
| **Resolved with no recovery:** | | | |
| Court ruling without payment | 26.93% | 19.59% | -7.34% |
| Expired / Dropped | 13.62% | 13.74% | 0.12% |
| **Unresolved:** | | | |
| Continues | 11.92% | 13.09% | 1.17% |
| N | 646 | 1077 | 1723 |

*Notes:* The table shows the status of the experimental cases in Phases 1 and 2 as of January 2020 (for Phase 1) or July 2020 (for Phase 2). These outcomes are taken from the administrative records of the court. "Court ruling with payment" indicates the plaintiff was awarded a judgment by the court. However, XX% of those awards were uncollected as of January / July 2020 and it is likely that some will never be collected.

Table 7: Recovery after 42 months, Phase 1/2 samples

| Net Present Vaue of Amount Awarded | | | | | |
|---|---|---|---|---|---|
| | Calculator imputed | | | 0s imputed | |
| | (1) NPV | (2) IHS NPV | (3) IHS NPV | (4) NPV | (5) IHS NPV |
| Calculator | -404.6 | -0.447 | -0.423 | -952.0 | -0.825* |
| | (670.4) | (0.508) | (0.513) | (598.0) | (0.431) |
| Employee Present (EP) | 3,023** | -0.175 | -0.215 | 2,940** | 0.245 |
| | (1,461) | (0.965) | (0.992) | (1,347) | (0.922) |
| Calc#EP | 511.1 | 2.632** | 2.679** | 1,313 | 2.818** |
| | (1,685) | (1.040) | (1.070) | (1,533) | (1.109) |
| Control group mean | 5382 | 0.193 | 7123 | 2.205 | 2.477 |
| Control group interaction mean | 7818 | 1.149 | 9803 | 2.716 | 2.945 |
| Observations | 1,698 | 1,698 | 1,698 | 1,698 | 1,698 |
| R-squared | 0.067 | 0.116 | 0.110 | 0.071 | 0.123 |
| Calculator p-value | 0.817 | 0.0664 | 0.950 | 0.0277 | 0.0253 |

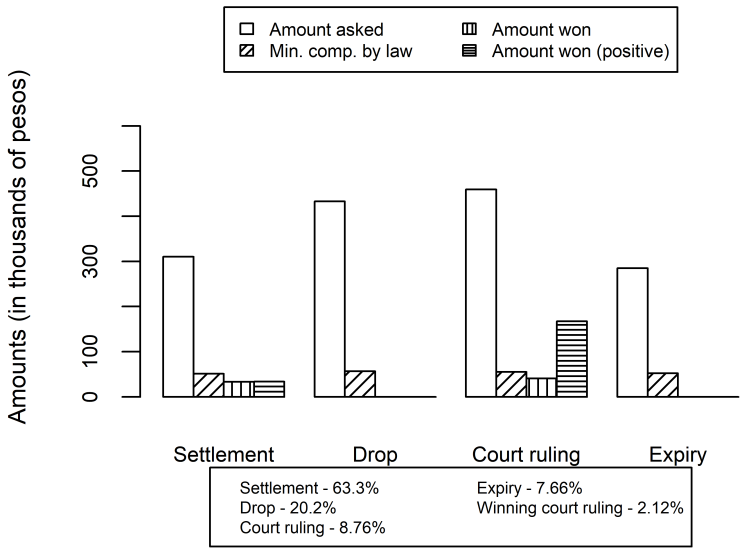
*Notes:* We measure the net present value of the amount awarded to plaintiffs. The dependent variable in columns 1 and 4 is the NPV, winsorized at the 95th percentile, and in columns 2, 3, and 5 is the inverse hyperbolic sine of the NPV. For cases not yet resolved, columns 1 through 3 impute the calculator predicted value for a court judgment, while columns 4 and 5 impute zeros to unresolved cases. In columns 1, 2, 4 and 5 we discount payments to the date of filing using a discount rate of 3.43 percent per months (50 percent per year). In column 3, we use a discount rate of 2.22 percent per months (30 percent per year). Standard errors are clustered on the day of treatment. All regressions include dummies for the phase, the subcourt, the year the case was filed, the number of plaintiffs in the case and an indicator some information was missing from the case file data.

Table 8: Phase 3: Effects on welfare

| | Happiness | Stopped paying serv. | Lack of money | Works |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Calculator | 0.13 | -0.070*** | -0.067*** | 0.019 |
| | (0.12) | (0.024) | (0.025) | (0.025) |
| Source | 2m | 2m | 2m | 2m |
| Observations | 1459 | 1474 | 1476 | 1476 |
| R-squared | 0.0079 | 0.0073 | 0.0095 | 0.021 |

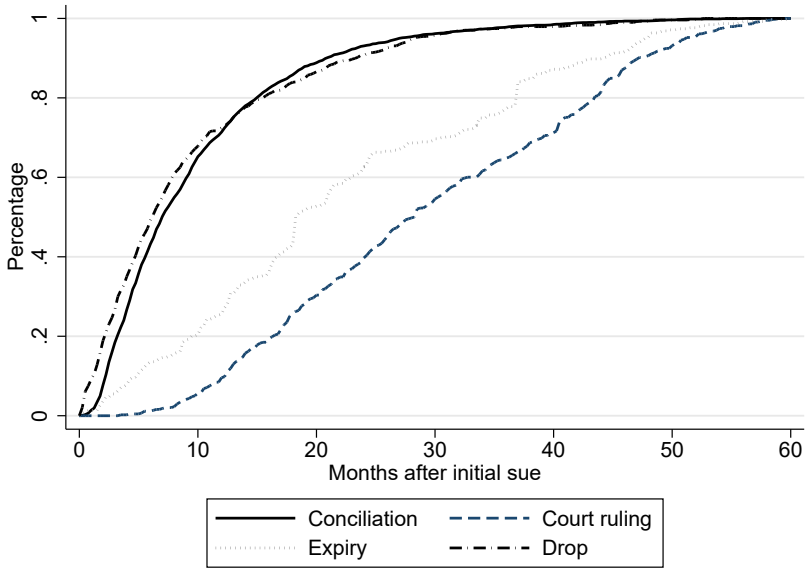
*Notes:* The 2 month survey for Phase 3 included questions measuring proxies for welfare. We asked (Happiness) *On a scale of 1 t 10, where 1 means "not happy at all" and 10 means "totally happy", in general, how happy do you feel about your life lately?* (Stopped paying serv.) *In the past three months have you had to stop paying for a basic service such as electric power, water, or rent due to lack of money?* (Lack of money for food) *In the past three months, have you lacked money to spend on food one or more days?* (Works) *Are you currently working?*

Figure 1: Differences in Claims and Compensation by case file outcome - Historical Data



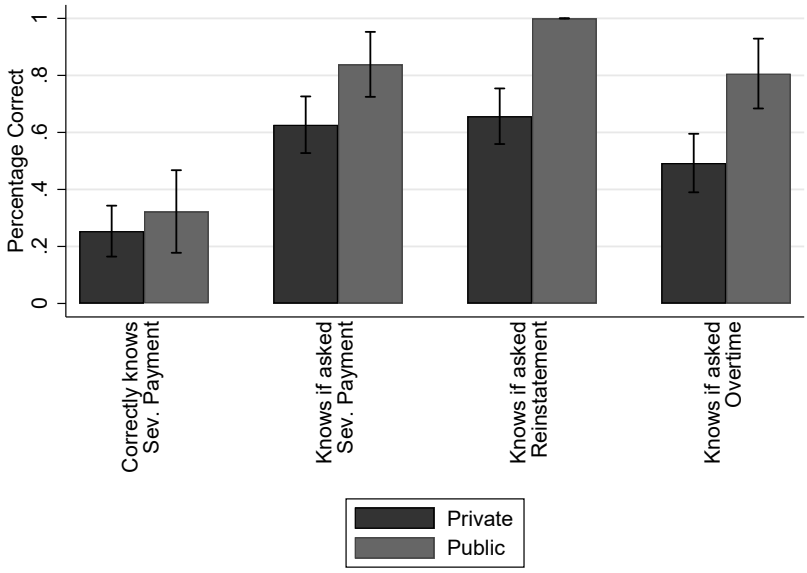

*Notes:* The figure shows the average amount asked for in the filing, the minimum legal compensation based on the case characteristics, and the amount actually received at the end of the process (overall and conditional on recovering a positive amount). Data are displayed in thousands of pesos by type of case ending, using the 5,005 historical case files. The amounts are discounted at the rate of 50 percent per year (3.43 per month). Cases end in any one of four ways: *settlement*; being *dropped* by the plaintiff; *court ruling*; or *expiring* from lack of activity. The share of completed cases in the historical case files by type of ending is shown at the bottom. Note that these do not match the share of all cases by type of ending because these data exclude cases that were unresolved at the end of 2015. Workers recover nothing when cases are dropped or expire. We define settlements as an agreement followed by payment, so settlements always imply a positive recovery for the worker. Workers recover a positive amount only 24 percent of the time when the case goes to judgment. These data do not distinguish between the judge ruling against the worker and the judge ruling in favor but the worker being unable to collect anything from the firm.

Figure 2: Time Duration




The figure uses the historical data (5005 casefiles) to plot the cumulative distribution of the duration of the case in months, by type of ending, for the 70 percent of cases concluded by the end of 2015.

Figure 3: Knowledge about Law and their Own Claims in Lawsuit




*Notes:* Data are from baseline survey of Phase 1. The figure shows averages of correct answers for several questions, grouped into: knowledge of the law (panel 1) and knowledge of the content of their own lawsuit (panels 2 to 4). The question for panel (1) is *"In the case of unjustified dismissal, law gives you a constitutional indemnification: Do you know how many days of salary this represents?"*. Panels (2) to (4) correspond to the question: *"Mark the benefits that you claimed in this suit: 1.Constitutional indemnification, 2.Medical insurance, 3.Reinstatement, 4.Overtime, 5.Premium for working Saturdays, 6. Aguinaldo (bonus), 7.Don't know"*. The figure indicates whether the respondent correctly answered items 2, 3 and 4.

Figure 4: Calculator Treatment Format (example) - Phase 1

**CÁLCULO DE COMPENSACIÓN TRABAJADOR**

MARZO 2016

*Datos del Trabajador*

**Género:** Hombre **Salario diario:** $350.00 mxn diarios **Antigüedad:** 6.23 años

En caso de despido injustificado, la Ley Federal del Trabajo le otorga al trabajador las siguientes prestaciones mínimas:

| | |
|---|---|
| **1.- Indemnización Constitucional -** consistente en 90 días de salario diario integrado: | $31,500.00 |
| **2.- Prima de Antigüedad -** 12 días por año laborado a razón del salario base con tope de 2 veces el salario mínimo: | $9,687.39 |
| **3.- Aguinaldo -** Parte proporcional de 15 días por año, a razón del salario base, del último año calendario laborado: | $1,861.80 |
| **4.- Vacaciones -** Parte proporcional de vacaciones del último periodo laborado, a razón del salario base: | $1,060.50 |

*SU COMPENSACIÓN DE LEY:* **$44,109.68**

**¡IMPORTANTE! Después de 3 años, el 48% de los juicios NO ha concluido.**

**Ahora le mostramos resultados de juicios concluidos y que son SIMILARES al suyo.**
*Nos basamos en 4500 expedientes de 2009, 2010, 2011 y 2012.*

| | % | Tiempo estimado | Cantidad pagada |
|---|---|---|---|
| **Convenio** | 65.11% | 0.86 años | $26,052.29 |
| **Desistimiento** | 25.62% | 0.65 años | $0.00 |
| **Caducidad** | 3.40% | 2.94 años | $0.00 |
| **Laudo con pago** | 3.41% | 2.39 años | $50,925.21 |
| **Laudo sin pago** | 2.46% | 1.01 años | $0.00 |

Tomando en cuenta las posibilidades de ganar y el proceso de ejecución, los datos estadísticos indican:

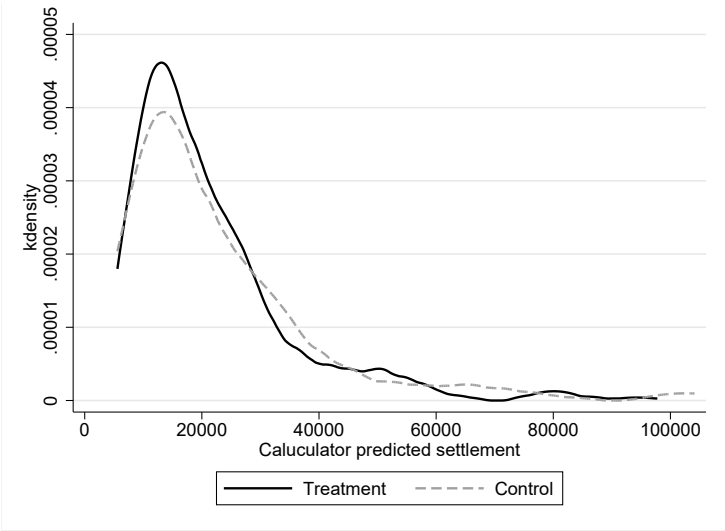
*SU COMPENSACIÓN ESPERADA:* **$18,699.32**

Recibí impresión. Entiendo que son datos estadísticos que no influyen en mi proceso ni afectan mis derechos.

No. Expediente / Año : *9999/2013* Firma:

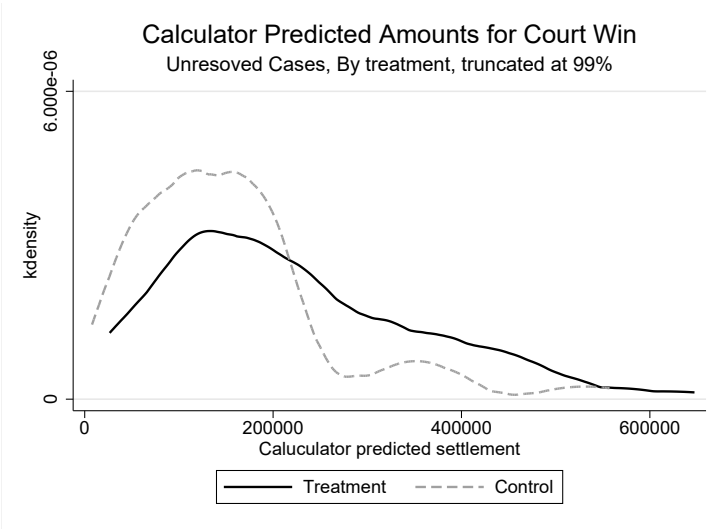
*Notes:*The figure shows an example of the calculator use in phase 1 treatments. The top half described the entitlement by law if the judge rules in favor of the plaintiff, based on data in the case filing. The second half shows what fraction of cases end which way, the average duration and amount for each ending, and the expected value ex-ante saliently in red in the bottom box. The worker and firm name are removed from the example shown here. Parties were told that this information comes from a statistical exercise based on completed historical cases, and that it gives average prediction based on variables of the initial lawsuit described in the calculator treatment.

Figure 5: Calculator predictions for Settled Cases




*Notes: This figure shows the distribution of settlement amounts predicted by the calculator for treatment and control cases ending in settlement. Data are truncated at the 99th percentile to compress the scale.*

Figure 6: Unresolved Cases: Calculator Prediction Conditional on Court Win




*Notes:* This figure presents the distribution of recovery amounts predicted by the calculator, conditional on winning a judgment, for treatment and control cases that were unresolved at the time of last data access. Data are truncated at the 99th percentile to compress the scale. Note that the differences between predicted outcomes in treatment and control reflect in part the fact that more of the unresolved cases in the treatment group are from phase 2, and hence higher-value cases. Figure C.8 in Appendix C shows the distributions by phase. The patters are similar though somewhat more muted.

# Appendix A

# Mexican Labor Law, Description of Data, and the Calculator

The appendix is divided into four sections. The first describes the Mexican labor law and court procedures, the second provides some additional details on the administrative data, the third describes our surveys and response rates, and the fourth describes the calculator we use to make predicted outcomes for the cases in which we intervene.

## 9.1 Mexican Labor Law

**Mexican labor Law:** On paper, Mexican labor law is protective of workers. For any employer-initiated separation that is not a "justified dismissal," the law requires severance payment of a minimum of 90 days' wages and benefits. The law provides very few legal bases for a justified dismissal. For example, workers are owed severance if they are fired for low productivity or because of poor market conditions. As an element of their lawsuit, workers may ask the court to order the firm to rehire them. If they win their case and the firm refuses to rehire them, then they are entitled to an additional severance payment equal to 20 days' wages for each year of tenure at the firm.[31]

**The Process:** In a firing lawsuit, an initial claim is filed by the worker or her lawyer, and an initial hearing date is set, generally two to three months after the date of filing. The defendant(s) must be notified of the filing and hearing date by a formal court summons that must be delivered in person by an employee of the court. The notification process typically takes 6 months.[32] Once notification of all defendants has taken place, a "conciliation, demands, and answers" hearing takes place. The principal demand in most firing lawsuits is either the base severance pay of 90 days or reinstatement of employment.

If a settlement cannot be reached then the defendant must answer the lawsuit. Additional hearings are scheduled for presentation and viewing of evidence, after which the written record is closed. All proceedings are conducted by an administrative assistant to the judge. Hearings are transcribed into the case file. The file is passed on to the judge, who writes the final

[31] Certain categories of workers (those defined as "at-will") are also entitled to to 20 days' wages per year of tenure. An at-will worker is one who is employed in a position of confidence, for example, a driver or a personal security guard. The law recognizes that the employer may need to dismiss the worker if that confidence is broken.

[32] In practice, notification involves substantial corruption, as the lawsuit cannot proceed without it. Aldeco Leo, Kaplan and Sadka (2014) shows that when notifiers' work load is assigned randomly and control of case files is taken away from notifiers, rates of successful notification more than double.

decision. Enforcement of judgments involving payments to the plaintiff is often challenging. A large proportion of firms do not pay the judgment voluntarily, and a seizure of assets must officially be performed by officers of the court. This is followed by adjudication of liquid assets or sale of non-liquid assets to pay the worker the awarded amount. Given that an interval of six-months between hearings is typical, the fact that there are more than four hearings held in an average case, and the frequent postponement of scheduled hearings due to lack of notification, the average lawsuit continuing to a judge's decision takes over three years. This in spite of the law stipulating that lawsuits should have a maximum duration of 100 days.

**Lawyers:** Once a case is filed, lawyers control the lawsuits almost completely. The presence of the parties themselves at the hearings is not compulsory, unless they are to be deposed as part of the evidentiary hearings. By law, workers who are not able to hire a private lawyer must be provided with free public legal assistance from labor lawyers in the public prosecutor's office. Public lawyers are paid a flat wage by the court and may not charge clients anything further. A public lawyer handles as many as 400 cases concurrently, while administrative data suggest that a normal load for a private lawyer is at most 50 cases. So while public lawyers are generally well qualified, they have an incentive to finish cases quickly in order to reduce their workload. The general perception is that public lawyers do not use creative or aggressive litigation strategies that may have a positive payoff but imply longer cases or more work.

Private lawyers must be licensed, but obtaining the license is fairly easy and otherwise lawyers are unregulated. Survey data indicate that 82 percent of workers are suing for the first time. Workers have little access to information about where to find a good private lawyer, and often opt for the first one that they run into, with little notion of that lawyer's reputation or previous record. The surveys show that 38 percent of plaintiffs using a private lawyer say that they found the lawyer either just outside the court or in one of the court corridors. Plentiful anecdotal evidence suggests that these '"informal lawyers" are low-quality and may not serve their clients' best interests.

Private plaintiff's lawyers typically charge an initial fee of about MXN$2000 pesos (USD 100) to file the lawsuit and a contingency fee of about 30 percent of any amount collected by the plaintiff. In spite of the contingency fee, their incentives are not perfectly aligned to those of their clients. First, while plaintiffs are party to a single case, the lawyers manage a portfolio of many firing lawsuits with widely differing characteristics, against many different firms. With diversified risk, they may be more willing to take risks on any given case. Second,

filing a low-quality suit is cheap and easy and the lawyers may profit from collecting the filing fee even with no expectation of recovering anything on behalf of the worker. The plaintiff must ratify settlements of the lawsuit in person, as well as acceptance or rejection of an offer of reinstatement, should the plaintiff's side receive one. Lawyers typically do not bring the plaintiffs to hearings nor do they provide them much detail on the developments in the lawsuit. As will be shown below, the physical presence of the worker at the hearing in which we intervene in the field experiment is crucial for the effectiveness of our intervention.

## 9.2 Administrative Data

The main source of administrative data are the initial case filings for the historical and experimental cases. For the experimental cases, we also collected data on the phase 1 and 2 case outcomes in December 2018 and in January and July 2020. The historical data are used to develop a predictive model of case outcomes. We then use this model and the data from each experimental case filing to predict outcomes for each case, as described in subsection 9.4 below. Because we intervene in many cases at the time of their first hearing, we use only data from the initial case filing. The case files are available in hard copy, and we digitize the variables shown on Table A.1.

Table A.1: Casefile Variable

| VARIABLE | DESCRIPTION |
|---|---|
| **Tenure** | Employee's tenure with the employer. |
| **Weekly hours** | Number of hours that the plaintiff worked on a weekly basis. |
| **Reinstatement*** | The plaintiff claims reinstatement. |
| **Severance pay*** | The plaintiff claims constitutional indemnity (three months of integrated salary) that the law dictates for unjustified dismissal. |
| **Lost wages*** | The plaintiff claims lost wages/back pay. |
| **Vacation pay*** | The plaintiff claims accrued vacation days not taken. |
| **Overtime*** | The plaintiff claims overtime pay. |
| **Twenty days compensation*** | The plaintiff claims the payment of compensation (20 days per year worked) that the law dictates for unjustified dismissal for a worker who has the right to be reinstated but the employer refuses to reinstate, or for an at-will employee who cannot ask for reinstatement. |
| **Insurance-IMSSIN-FOSAR*** | The plaintiff claims the payment of employer contributions that were not made to these institutions, or retroactive registration in the institutions (SAR: retirement savings, IMSS: social security, INFONAVIT: worker's housing fund). |
| **Co-defendant*** | At least one of the codefendants is the IMSS or INFONAVIT or SAR. |
| **Total asked** | The total quantifiable peso amount of the worker's claim. |
| **Minimum legal entitlement** | The quantifiable peso amount of the sum of severance pay, vacation and end of year bonuses of the last year of tenure at the firm. It is a conservative estimate of the minimum amount of money the worker is entitled to if she wins the lawsuit. |

*Notes:* Detailed description of variables used throughout the paper. Dummy variables are marked with *.

### 9.3 Survey Data

We conducted surveys with participants at the hearings in Phases 1 and 2, and with dismissed workers approaching the court in Phase 3. The surveys in the first two phases had to be very short so that we did not delay the start of hearings. We use one survey instrument for plaintiffs, and another for the lawyers representing both plaintiffs and employers. Table A.2 shows summary statistics for workers and lawyer from phase 1. Table A.3 shows compliance rates for both the surveys and treatments for the first two phases. We have no measure of noncompliance in phase 3. Every worker who completed the survey and was assigned to treatment received the calculator treatment. However, there were surely dismissed workers who approached the court but did not stop at our information booth. Since the workers approaching the court had no way of know if the given day was assigned to treatment or control, we have no reason to believe there is differential non-compliance across treatment groups on either observable or unobservable characteristics. Hence, non-compliance in phase 3 is an issue of external rather than internal validity.[33]

[33]Even if every dismissed worker approaching the court had fully complied with the experimental protocol, the sample would not be representative of all aggrieved workers dismissed from Mexico City employers, because many dismissed workers obtain information from public or private lawyers without coming to the court. However, we believe the sample is relevant for understanding the effect of information provision on the decision to file case or settle.

Table A.2: Survey variables summary statistics

| Panel A: worker characteristics | | | |
|---|---|---|---|
| | Mean<br>(SD)<br>N | | Mean<br>(SD)<br>N |
| Age | 44.37<br>(13.18)<br>102 | Have you changed lawyer during trial? | 0.09<br>(0.29)<br>102 |
| Less than 50 employees | 0.59<br>(0.49)<br>96 | Probability of winning trial | 79<br>(21.3)<br>102 |
| Percentage of what is obtained | 28.3<br>(11.11)<br>57 | Most probable amount | 97116.66<br>(355749)<br>102 |
| Probability of winning | 48.68<br>(34.06)<br>102 | Most probable time | 3.64<br>(9.76)<br>102 |
| More than secondary education | 0.58<br>(0.5)<br>102 | How well were you treated? | 0.45<br>(0.5)<br>102 |

| Panel B: lawyer characteristics | | |
|---|---|---|
| | Worker's lawyer | Employer's lawyer |
| | Mean<br>(SD)<br>N | Mean<br>(SD)<br>N |
| Age | 37.79<br>(12.43)<br>231 | 36.56<br>(10.97)<br>223 |
| Tenure | 9.25<br>(8.88)<br>181 | 18.65<br>(143.32)<br>193 |
| More than 100 historical cases | 0.49<br>(0.5)<br>231 | 0.61<br>(0.49)<br>223 |
| More than 30 current lawsuits | 0.6<br>(0.49)<br>231 | 0.68<br>(0.47)<br>223 |
| Less than 50 employees | 0.65<br>(0.48)<br>173 | 0.46<br>(0.5)<br>151 |
| Percentage of what is obtained | 29.69<br>(7.79)<br>172 | |
| Probability of winning trial | 72.19<br>(20.14)<br>231 | 70.48<br>(21.53)<br>223 |
| Most probable amount | 237957.65<br>(1482119.93)<br>231 | 66884.79<br>(285657.2)<br>223 |
| Most probable time | 3.94<br>(2.3)<br>231 | 4.17<br>(2.54)<br>223 |

*Notes:* Summary statistics of survey variables.

Table A.3: Compliance Rate

(a) Phase 1

| Group | N | Compliance with treatment | | | | Compliance with baseline survey | | | | Compliance with exit survey | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Plaintiff | Defendant | Both | Any | Plaintiff | Defendant | Both | Any | Plaintiff | Defendant | Both | Any |
| Control | 321 | - | - | - | - | 50.78 | 35.51 | 8.72 | 77.57 | 39.88 | 24.3 | 94.39 | 58.57 |
| Calculator | 315 | 77.65 | 73.03 | 75.81 | 79.72 | 48.25 | 33.65 | 12.38 | 69.52 | 38.63 | 25.86 | 88.47 | 54.83 |

(b) Phase 2

| Group | N | Compliance with treatment | | | | Compliance with survey | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Plaintiff | Defendant | Both | Any | Plaintiff | Defendant | Both | Any |
| Control | 329 | - | - | - | | - | - | - | - |
| Calculator | 762 | 72.7 | 63.78 | 49.08 | 87.4 | 54.33 | 48.29 | 49.08 | 87.4 |

(c) % Shows up

| | Phase 1 | | Phase 2 | |
|---|---|---|---|---|
| | Control | Calculator | Control | Calculator |
| Employee | 0.193 | 0.197 | 0.122 | 0.165 |
| Emp Lawyer | 0.847 | 0.832 | 0.85 | 0.853 |
| Firm Lawyer | 0.738 | 0.762 | 0.434 | 0.444 |

*Notes:* Compliance rate for each phase, both for treatment and survey. Each panel shows the percentage compliance with treatment and survey for plaintiff' side, defendant's side, both, and any. In phase 1, we did not measure compliance with treatment at the party level but rather at the casefile level, so that for example the 74.16 under the column "both" for compliance with treatment means that among the casefiles for which both parties showed up to the hearing, we were able to give the calculator to at least one of the parties in 74.16 percent of the cases. Also, in phase 1 we had an exit survey in addition to the baseline survey. In phase 2, we did measure compliance with the treatment by party. On the other hand, note from panel (b) that since in phase 2 control days were days that no implementing personnel were present in the subcourts, we have neither treatment nor survey compliance for the control group. Panel (c) shows from the court's administrative data who shows up to the hearings for each of the two phases, split by employee, employee lawyer, and firm lawyer.

Table A.4: Balance table

| | Phase 1 | | | Phase 2 | | | Phase 1/2 | | |
|---|---|---|---|---|---|---|---|---|---|
| | C | T | p-value | C | T | p-value | C | T | p-value |
| Female | 0.44 | 0.39 | 0.26 | 0.51 | 0.45 | 0.06* | 0.47 | 0.43 | 0.09* |
| At will worker | 0.18 | 0.18 | 0.93 | 0.1 | 0.07 | 0.2 | 0.14 | 0.1 | 0.04** |
| Weekly hours | 56.58 | 58.05 | 0.28 | 54.97 | 56.64 | 0.05* | 55.77 | 57.04 | 0.09* |
| Tenure at firm | 104.05 | 236.84 | 0.04** | 4.97 | 4.11 | 0.07* | 56.96 | 69.32 | 0.57 |
| Public Lawyer | 0.1 | 0.12 | 0.3 | 0.06 | 0.06 | 0.76 | 0.08 | 0.08 | 0.85 |
| % Reinstatement | 0.45 | 0.44 | 0.83 | 0.48 | 0.51 | 0.29 | 0.46 | 0.49 | 0.24 |
| % Severance pay | 0.81 | 0.83 | 0.39 | 0.81 | 0.79 | 0.54 | 0.81 | 0.8 | 0.87 |
| Daily wage | 842.02 | 628.04 | 0.05* | 617.35 | 600.52 | 0.85 | 728.57 | 608.02 | 0.08* |
| % Backpay | 0.97 | 0.96 | 0.61 | 0.99 | 0.98 | 0.47 | 0.98 | 0.98 | 0.78 |
| % Tenure bonus | 0.78 | 0.83 | 0.08* | 0.8 | 0.78 | 0.55 | 0.79 | 0.79 | 0.69 |
| % Extra hours | 0.68 | 0.71 | 0.43 | 0.71 | 0.77 | 0.04** | 0.7 | 0.76 | 0.01** |
| % 20 days | 0.31 | 0.32 | 0.72 | 0.35 | 0.34 | 0.7 | 0.33 | 0.33 | 0.86 |
| % Sunday bonus | 0.18 | 0.19 | 0.78 | 0.18 | 0.19 | 0.67 | 0.18 | 0.19 | 0.57 |
| % Weekly rest | 0.2 | 0.19 | 0.67 | 0.17 | 0.22 | 0.05* | 0.19 | 0.21 | 0.18 |
| %Mandatory rest | 0.32 | 0.29 | 0.47 | 0.26 | 0.3 | 0.2 | 0.29 | 0.3 | 0.72 |
| % Social security codef | 0.53 | 0.53 | 0.98 | 0.63 | 0.64 | 0.73 | 0.58 | 0.61 | 0.23 |
| % Earnings | 0.36 | 0.35 | 0.86 | 0.37 | 0.35 | 0.54 | 0.36 | 0.35 | 0.57 |
| % Nulity | 0.62 | 0.57 | 0.23 | 0.61 | 0.57 | 0.22 | 0.61 | 0.57 | 0.08* |
| Entitlement | 97065.2 | 71560.51 | 0.03** | 63189.71 | 68890.8 | 0.5 | 78994.5 | 69581.42 | 0.18 |
| Presence employee | 0.19 | 0.19 | 0.94 | 0.16 | 0.19 | 0.14 | 0.17 | 0.19 | 0.31 |
| Presence emp law | 0.85 | 0.84 | 0.7 | 0.92 | 0.89 | 0.14 | 0.88 | 0.87 | 0.58 |
| Presence firm | 0.04 | 0.02 | 0.16 | 0.01 | 0.02 | 0.22 | 0.02 | 0.02 | 0.65 |
| Presence firm law | 0.74 | 0.77 | 0.41 | 0.79 | 0.78 | 0.76 | 0.77 | 0.78 | 0.54 |
| Observations | 330 | 326 | | 327 | 768 | | 657 | 1094 | |

*Notes:* Balance table on basic and strategic variables. The first three columns show phase 1, with means for each variable in the control, calculator, and conciliator treatment groups in the three columns. The next two columns show the control and treatment for phase 2, and finally the last two columns combine both phases. *, **, and *** indicates a difference that is significant at the 10%, 5%, and 1% level, respectively, as compared with the control group.

## 9.4 The Calculator

As described in the paper, one of the treatment arms required giving workers information about predicted outcomes for their case. In this subsection we describe the variables and machine-learning models we use to develop these predictions. Since the objective of the project was to provide the parties with the most accurate predictions possible, we considered several different models for each outcome of interest. The models were estimated using 70 percent of the data and then tested on the remaining 30 percent. We based the predictions on the model that yielded the lowest error in the test sample. Among the models we considered are the most common machine learning models, since these have shown in other settings to be very flexible and improve prediction accuracy.

We want to predict a series of outcomes, some of which are continuous and some of which are discrete. Different models are appropriate for these two types of outcomes, so we organize our discussion by first describing the discrete outcomes of interest and the models tested for those

outcomes, and then describe the continuous outcomes of interest and the models we tested for those outcomes. We then describe the calculator templates used in both the first and second phase of the project.

For the calculators used in both phases, and for both the continuous and discrete models, we fed the models with the same set of input variables, all from the initial case filing. This is because, for operational reasons we could not use procedures that occur after the filing of the lawsuit, such as evidence submission. Also, the court wanted us to develop a parsimonious calculator that could be used in pre-judicial conciliation meetings prior to the plaintiff filing suit.

**Discrete outcome models**

The expected payment made to the plaintiff is a function of which party prevails and the amount transferred conditional on the outcome. There are five ways a case can end:

1. **Settlement:** The case may end with a voluntary agreement between the parties where the the plaintiff accepts a sum of money to cease the lawsuit and renounce the legal right to sue again for the same reason. To be valid, these settlements must be registered at the court, and therefore included in our administrative data.

2. **Court ruling with positive compensation:** Cases may proceed to a ruling by a judge that decides which side is wins the lawsuit and how much should be paid to the plaintiff. We classify an outcome as Court ruling with positive compensation if the case ends in a ruling by the judge and the worker actually collects a positive amount.[34]

3. **Court ruling with zero compensation:** The judge may also rule in the defendant's favor. In that event the worker receives nothing. However, the worker may also receive nothing if the judge rules in her favor, but she is unable to recover any of the judgment amount from the defendant. Defendants use a variety of strategies to avoid paying judgments, so the win but collect nothing outcome is not uncommon. The court records that we digitized did not allow us to differentiate between these two outcomes. We do not see this as an important shortcoming since from the point of view of the plaintiff what matters is the amount she receives.

[34]Because court rulings with positive collection are uncommon (3.3 percent of cases) we face the the problem of unbalancedness of our court rulings sample. To deal with this problem we used a Synthetic Minority Over-Sampling Technique (see Chawla et.al., 2002) and did a 80-20 train vs. test split on our data.

4. **Dropped:** A case can be dropped by the plaintiff at any time during the legal proceedings. The difference between dropped and settlement is that it can be done unilaterally by the plaintiff and no payment to the defendant is registered. Our understanding is that when cases are dropped it is because the plaintiff has little evidence to support the case. It is a decision of the plaintiff and not a mandate by the court.

5. **Expiry:** Finally, a labor suit may expire if the court requires information to continue the procedure, including items of evidence presented by one of the parties that the court needs to view before concluding the hearings procedure if proofs are not provided in a span of 4 months.

In our historical data of cases filed in 2011 and completed by the end of 2015, 63.3 percent end in settlement, 2.0 percent in a court ruling with positive collection, 6.6 percent with a court ruling with nothing collected, 20.3 percent expire and 7.6 percent are dropped.

We want to estimate the probability that a case with characteristics $X_i$ ends in each of the five ways described above. We have a choice of estimating a single multinomial model or separate bivariate outcome models. We chose the bivariate option for simplicity. But to ensure that the probabilities summed to one, we set the probability of expiry equal one minus the sum of the probabilities of the other four outcomes.[35] We therefore estimated models for four bivariate outcomes, using each the following methods: (a) Logistic Regression, (b) Probit, (c) Random Forest[36], (d) Single-hidden-layer Neural Network (20 nodes in the hidden layer and 10% weight decay), (e) Gradient Boosting.[37] Models were estimated in a random sample training set made of 70 percent of the observation. The remaining 30 percent was used as a test set. For each model hyperparameters were chosen to minimize mean square error in the test set (MSE-T) using cross-validation. Once hyperparameters for each model were chosen, we chose among those optimized models bases on their mean square error. For each of the 5 discrete outcomes we want to predict we kept the model with the highest correlation between Y and $\hat{Y}$ in the test set.[38] Table A.5 shows the accuracy rate for each of these 5 outcomes and for each of the 5 models, highlighting in grey the one chosen. The correlations range from 0.61

[35] We also estimated a multinomial model. The results were similar, which is why we chose the bivariate models for simplicity.

[36] We performed grid search in order to find the best hyperparameter setting. We compared 7 different models with the number of trees ranging between 900 and 1500. Our final model resulted in a Random Forest of 1200 CARTs, which yielded an 86 percent accuracy rate on test classification.

[37] This was implemented with off the shelf models in R : e1071, randomForest, neuralnet, caTools, mboost.

[38] For the outcome of settlement, we find that the correlation between Y and $\hat{Y}$ is very slightly higher for Random Forest than for Logit, but we use the Logit model because it was simpler to implement in the field.

to 0.93 for the models selected.

Table A.5: Fit assessment of discrete calculator models

Phase 1: Discrete outcomes

| | N | Logit | Probit | Random Forest | Neural Network | Gradient Boosting |
|---|---|---|---|---|---|---|
| Settlement | 2075 | 0.61 | 0.61 | 0.62 | 0.57 | 0.40 |
| Losing court ruling | 2075 | 0.67 | 0.69 | 0.74 | 0.65 | 0.62 |
| Winning court ruling | 2075 | 0.67 | 0.69 | 0.75 | 0.70 | 0.67 |
| Expiry | 2075 | 0.93 | 0.93 | 0.93 | 0.93 | 0.93 |
| Drop | 2075 | 0.78 | 0.78 | 0.78 | 0.74 | 0.78 |

Phases 2 and 3: Discrete outcomes

| | N | Logit | Probit | Random Forest | Neural Network | Gradient Boosting |
|---|---|---|---|---|---|---|
| Accuracy | 432 | 0.71 | 0.72 | 0.89 | 0.80 | 0.75 |

*Notes:* Some statistics on the predictive power of the models considered for both -Phase 1 and Phase 2- calculator calibration processes. Statistics for models chosen for each problem are shown in bold. We show $Corr(y, \hat{y})$ for continuous outcomes. We considered random forests both for continuous and categorical outcome variables, using the algorithm's regression and classification methods, respectively.

## Continuous outcome variables

Several relevant outcomes are continuous variables. We focus on three continuous variables to be provided to the parties:

1. *Amount collected conditional on a positive payment:* This is the peso amount actually collected by the plaintiff in the each of the two outcomes where they payments are positive: settlement and judgment in favor of the plaintiff, with recovery. The other endings all result in zero payments.

2. *Probability of Positive Recovery:* From the historical data for each casefile we know if the worker in fact was paid a positive amount or not (i.e. won the case in the court ruling and could indeed collect, or settled and got a positive amount).

3. *Duration of the case:* The number of months from the filing of the case to the date when it ended. As a significant share of the cases were not resolved by the end of 2015, we discuss censoring below.

For the continuous outcomes, we estimated a set of four different models for each of the two prediction problems.[39] The four models were: (a) OLS regression, (b) GLM Boosting, (c)

[39]Considering both regular and logarithmic models that would be eight different models for each outcome. The logarithmic models help to tackle the skewness of our dependent variable.

Random Forest[40], (d) Ridge Regression[41] [42]. As with the discrete variables, for each model the hyperparameters were chosen to minimize mean square error in the test set (MSE-T) using cross-validation. Once hyperparameters for each model were chosen, we chose among those optimized models based on their Mean Absolute Percentage Deviation (MAPD).

Finally we also wanted to predict the duration of the trial from initial suit to termination. We used the data with models similar to those described for the amount collected. However, for phase 2 the predictions from these models were very noisy: the models could not beat a simple average within each type of ending, so we decided to present the simple average within each type of termination rather than a model as a function of observables.

As with the discrete outcomes, Table A.6 shows the accuracy rate for settlement and duration outcomes and for each of the six models, highlighting in grey the ones chosen. The correlations range from 0.61 to 0.76 for the models selected for total compensation, but from 0.08 to 0.93 for the five duration outcomes.

[40]We perfomed grid search in order to find the best hyperparameter setting. We compared 7 different models with the number of trees ranging between 900 and 1500. Our final model resulted in a Random Forest of 1200 CARTs, which yielded an 86% accuracy rate on test classification.

[41]This was done with the libraries in R: e1071, randomForest, neuralnet, caTools, mboost.

[42]We also run the OLS and Random Forest models using logged data. In phase 2, we changed the models we estimated to include Kernel regression, running both OLS and Kernel in levels and logs. We do not present here the Boosted, Random Forest and Ridge regressions. The $corr(\hat{y^j}, \hat{y^k})$ between the predicted values of model $j$ and model $k$ for the union of all models is above 0.8 for all models.

Table A.6: Fit assessment of continuous calculator models

Phase 1: Continuous outcomes

| Ending type | N | Regression | Log-regression | Boosted regression | Random Forest | Log-Random Forest | Ridge Regression |
|---|---|---|---|---|---|---|---|
| | | | | Total Compensation | | | |
| Settlement | 1236 | 0.61 | 0.55 | 0.61 | 0.63 | 0.61 | 0.61 |
| Court Ruling | 66 | 0.70 | 0.76 | 0.70 | 0.49 | 0.28 | 0.67 |
| | | | | Duration | | | |
| Settlement | 1236 | 0.07 | 0.07 | 0.08 | 0.05 | 0.07 | 0.07 |
| Losing court ruling | 49 | 0.93 | 0.94 | 0.93 | 0.87 | 0.81 | 0.93 |
| Winning court ruling | 66 | 0.64 | 0.65 | 0.64 | 0.47 | 0.44 | 0.65 |
| Expiry | 118 | 0.35 | 0.34 | 0.37 | 0.29 | 0.39 | 0.42 |
| Drop | 468 | 0.20 | 0.16 | 0.19 | 0.13 | 0.17 | 0.05 |

Phases 2 and 3: Continuous outcomes

| Ending type | N | Regression | Kernel regression | Log-regression | Log-kernel regression |
|---|---|---|---|---|---|
| | | | Total Compensation | | |
| Settlement | 3130 | 0.59 | 0.46 | 0.69 | 0.63 |
| Court ruling | 105 | 0.44 | 0.30 | 0.18 | -0.06 |

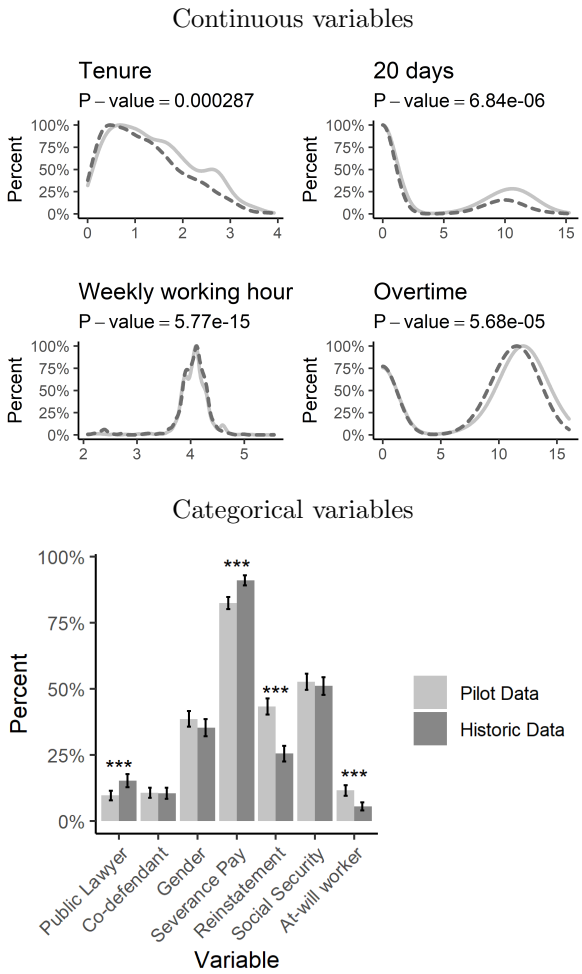
*Notes:* Some statistics on the predictive power of the models considered for both -Phase 1 and Phase 2- calculator calibration processes. Statistics for models chosen for each problem are shown in bold. We show $Corr(y, \hat{y})$ for continuous outcomes. We considered random forests both for continuous and categorical outcome variables, using the algorithm's regression and classification methods, respectively.

### Other issues: Censoring

All of the models are estimated on a sample of cases filed in 2011 and completed by the end of 2015. The fact that the sample contains only the cases that were resolved could introduce a bias in the prediction of outcomes of ongoing cases. For example, if we are interested in the probability of winning *eventually* and if cases with larger expected payouts take longer to resolve, then excluding the unresolved cases may produce an underestimate of the average payments in all cases.

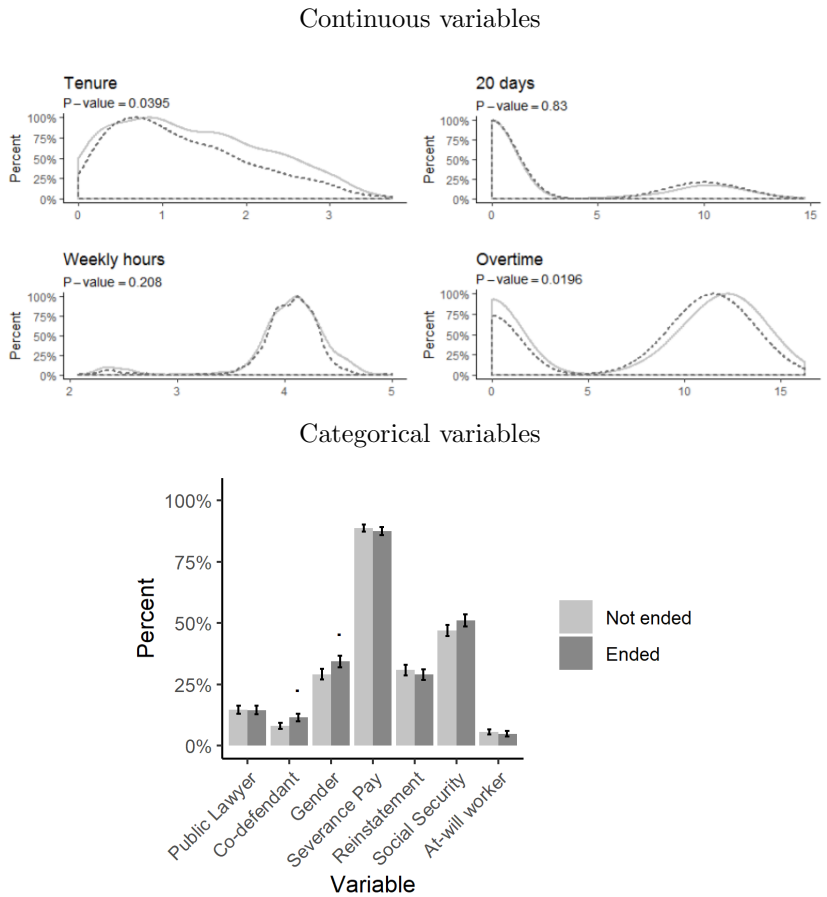
We are aware of this bias and it was communicated to the parties when the calculator information was provided. Although we cannot know how large the bias is, we performed two exercises that suggest it is not large. First, we compare characteristics of ongoing cases with those of the historical cases used in the models. In Figure A.1 we show that the two sets of cases are similar. The second exercise compares the characteristics of completed and continuing lawsuits within the historical data. To do this we drew a random sample of 956 cases filed in 2011 that were not finished by 2015 (i.e. this represents the complement of our historical dataset). We compare these 956 cases to the completed cases used to develop the models. Figure A.2 reports the results. There are no significant differences between these two samples, and the magnitude of the measured differences are all very small.

Figure A.1: Covariate distribution comparison : Historical and Phase 1 data




*Notes:* Covariate distributions comparisons. We compare between -used to calibrate our calculator- and Phase 1 data. All continuous variables are plotted in logs. Color guide is the same for both variable types. Plots for continuous variables show the p-value of a KolmogorovSmirnov test in the subtitle. In the categorical covariates plot, we show significance of a two-sided t-test of differences in means between samples.

Figure A.2: Covariate distribution comparison : Historical data: Ended and not ended cases




*Notes:* Covariate distributions comparisons between historical data cases: ended and not ended after 3 years. All continuous variables are plotted in logs. Color guide is the same for both variable types. Plots for continuous variables show the p-value of a KolmogorovSmirnov test in the subtitle. In the categorical covariates plot, we show significance of a two-sided t-test of differences in means between samples.

# Appendix B

## The Effect of Agency on Settlement Rates

We aim to write down a simple model that aims to illuminate why agency issues plausibly the underlying cause of the patterns in treatment we find on Tables 3 and 4. The treatment is effective only when plaintiffs are present to receive it directly. Moreover, this pattern is driven by cases where the plaintiff is represented by a private lawyer. These basic facts could be explained by several models, and we do not claim that the simple and highly stylized framework we write down here is the only way to rationalize them.

Private lawyers almost always receive a share of whatever the plaintiff collects. We should expect the contingency payment contract to align incentives with regard to effort at least partially. We focus instead on differences in incentives over the decision to settle or proceed with the case. The parties differ in their ability to diversify risks and are also likely to have different discount rates. Our framework focuses on these differences, and shows why they may lead to differences in preferences over taking a certain payoff now (i.e., a settlement) or an uncertain payoff later (i.e., a court judgment).

We start with three utility functions, $(U_w(x), U_l(x), U_f(x))$, defined over the amount awarded to plaintiffs for the plaintiff $(w)$, the plaintiff's lawyer $(l)$, and the firms$(f)$[43], and subjective distributions $g_k(x)$ for $k = \{w, l, f\}$ for the amount that party $k$ recovers (pays) in a trial. We define the *settlement range* between the plaintiff and defendant as $I_{w,f} := \{x \mid \mathbb{E}_w U_w \leq x \leq \mathbb{E}_f U_f\}$, where expectations are taken with respect to $g_k(x)$ and the utility functions incorporate attitudes toward risk over uncertain outcomes. If the utility over outcomes differs for plaintiffs and their lawyers, then we can also define a similar settlement range between firms and plaintiff's lawyers as $I_{l,f} := \{x \mid \mathbb{E}_l U_l \leq x \leq \mathbb{E}_f U_f\}$.

We show three claims that follow from this simple framework.

**Claim 1** *Suppose* $g_w(x) = g_l(x) = g_f(x) := g(x)$ *and*

$$A_{U_w}(x) \geq A_{U_l}(x) \geq A_{U_f}(x) \quad ; \forall\, x \in \operatorname{supp} g$$

*where* $A_U(x) - \frac{U''(x)}{U'(x)}$ *is the Arrow-Pratt measure of risk aversion. Suppose further that* $U'_w(x) \leq U'_l(x) \leq U'_f(x)$ *in a neighbourhood of* $0$. *Then* $\emptyset \neq I_{l,f} \subseteq I_{w,f}$. *That is, the worker is more*

[43] We will assume that $U(0) = 0$, $U'(x) > 0$ and $U''(x) \leq 0$, so that $U$ is risk averse or risk neutral. We can think of the 30 percent of any recovery that the plaintiffs' lawyers typically receive as being already reflected in their utility function.

*willing to settle than her lawyer.*

*Proof of claim (1):* It will suffice to prove that $\mathbb{E}_w U_w \leq \mathbb{E}_l U_l \leq \mathbb{E}_f U_f$.

Now,

$$\begin{aligned}
A_{U_w}(x) \geq A_{U_l}(x) &\iff \frac{U_w''(x)}{U_w'(x)} \leq \frac{U_l''(x)}{U_l'(x)} \\
&\iff \frac{d}{dx}\ln(U_w'(x)) \leq \frac{d}{dx}\ln(U_l'(x)) \\
&\iff \int_0^y \frac{d}{dx}\ln(U_w'(x))dx \leq \int_0^y \frac{d}{dx}\ln(U_l'(x))dx \;\; \forall\, y \in \text{ supp}\, g \\
&\iff \ln(\frac{U_w'(y)}{U_w'(0)}) \leq \ln(\frac{U_l'(y)}{U_l'(0)}) \;\; \forall\, y \in \text{ supp}\, g \\
&\iff \frac{U_w'(y)}{U_w'(0)} \leq \frac{U_l'(y)}{U_l'(0)} \;\; \forall\, y \in \text{ supp}\, g \\
&\implies U_w'(y) \leq U_l'(y) \;\; \forall\, y \in \text{ supp}\, g
\end{aligned}$$

As $U(0) = V(0)$ the last inequality tells us that

$$U_w(x) \leq U_l(x) \;\; \forall\, x \in \text{ supp}\, g$$

which together with the fact that all parties follow the same subjective distribution leads to

$$\mathbb{E}_w U_w \leq \mathbb{E}_l U_l$$

The other inequality is proved analogously.

Claim 1 says that even with the same subjective $g(x)$ for all parties, if plaintiffs are more risk averse than defendants[44] then the settlement range between the worker and firm is non empty. Moreover, if the worker is more risk averse than her lawyer, then the settlement range between the firm and the plaintiff's lawyer is a subset of the settlement range between the firm and the plaintiff. Therefore, the worker will be willing to accept some settlement offers that her lawyer will want to reject. The difference in the circumstances governing risk creates a potential conflict of interest between the plaintiff and her own lawyer.

[44] Since the workers typically have much lower incomes and have recently lost a job, and since both the plaintiff's lawyers and the firms typically manage several cases simultaneously, we believe this is a reasonable assumption. With a slightly more complex model, we can get the same result even with the same utility function for the lawyer and the worker, if lawyers are able to diversify across multiple cases. Since, on average, lawyers surveyed in phase 1 reported having more than 100 open cases, we think it is safe to say that lawyers are far more diversified than workers.

**Claim 2** *Assume additionally that $g_w^o \succeq_{FOSD} g_w$. Then $I_{w^o,f} \subseteq I_{w,f}$. That is, overconfidence shrinks the settlement range.*

*Proof of claim (2):* As $g_w^o$ first-order stochastically dominates $g_w$ then

$$\mathbb{E}_{g_w^o} U \geq \mathbb{E}_{g_w} U$$

for all weakly increasing $U$. From this the claim follows directly.

Claim 2 relaxes the assumption of the same expectations over outcomes to allow for overconfidence. We show that if the worker is overconfident (i.e. $g_w^o$ first-order stochastically dominates $g_w$) then the settlement range shrinks. Indeed, if the parties are sufficiently optimistic, the settlement range will be empty. Our surveys in phase 3 indicate that workers are wildly optimistic before filing a case. Because most workers are suing for the first time, while their lawyers have typically handled a large number of cases, the lawyers have the ability to guide workers toward realistic expectations or maintain their initial overconfidence. The misalignment between the worker and her lawyer generated by Claim 1 gives an incentive for the lawyer to inflate the expectations of the worker.

**Claim 3** *Let the worker have a prior distribution $g_w$, and let the calculator be a signal with distribution $S$. Suppose further that the signal satisfies $g_w \succeq_{FOSD} S$ and that the agent is Bayesian. If we denote the posterior by $\hat{g}_w$, then $I_{w,f} \subseteq I_{\hat{w},f}$. That is, after updating the settlement range increases.*

*Proof of claim (3):* We will prove that $g_w \succeq_{FOSD} \hat{g}_w \succeq_{FOSD} S$, combining this fact with Claim 2 will yield the desired result.

As the agent is Bayesian, then by Theorem 1 in *Foundations for Bayesian Updating* we can write $\hat{g}_w = \alpha g_w + (1-\alpha)S$ for some $\alpha \in [0,1]$. Then,

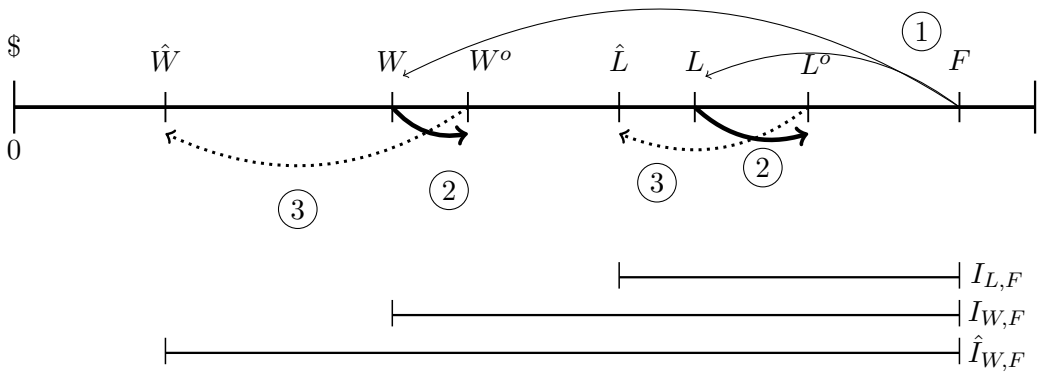
$$\mathbb{E}_{\hat{g}_w} U = (1-\alpha)\mathbb{E}_{g_w} U + \alpha \mathbb{E}_S U \leq (1-\alpha)\mathbb{E}_S U + \alpha \mathbb{E}_S U = \mathbb{E}_S U$$

for all weakly increasing $U$. As $S$ is dominated by $g_w$, this yields the desired inequality.

Claim 3 introduces the calculator as a signal received by Bayesian updaters, and shows that when initially overconfident workers get the calculator, their settlement range increases to $\hat{I}_{w,f}$, leading to more settlements.

The effect of the three claims can be illustrated in the following figure, which shows the effect of overconfidence and differences in the ability to diversify risk on the settlement range over which parties might bargain a resolution to the case.

Figure B.1: Illustration of claims




*Notes:* To simplify the notation we will simply write the subindex $i \in \{W, L, F\}$ to denote $\mathbb{E}_i U_i$. The arrows describe the conclusion on each of the claims: Initially with the same subjective probability but with different degrees of risk aversion, the expected utility of the different parties differ. This is Claim 1. Claim 2 tells us that for overconfident individuals (here denoted by $W^o$), their expected utility increases. Finally, Claim 3 is about the updating process for lawyers and workers, assuming that the calculator reduces the overconfidence. The conclusion is that lawyers are *weakly* more overconfident than workers, so that they update less than workers. The last intervals denote the *settlement range*, the first one for the lawyer and the last two before and after updating, when the employee is present.

# Appendix C

## 9.5 Tables

Table C.1: Amount asked (log), amount won (log), and probability of winning - Historic Data

| | Panel A : All cases | | | |
|---|---|---|---|---|
| | Total asked | Amount Won | Won/asked | Prob winning |
| Public Lawyer | -0.62*** | 0.0011 | 0.055** | 1.65 |
| | (0.033) | (0.22) | (0.024) | (2.33) |
| Constant | 2.76*** | 5.48*** | 1.06*** | 75.6*** |
| | (0.19) | (0.97) | (0.10) | (9.67) |
| Observations | 4866 | 4866 | 4866 | 4866 |
| BVC | YES | YES | YES | YES |
| Dummy Industry | YES | YES | YES | YES |
| R-squared | 0.74 | 0.036 | 0.043 | 0.020 |
| DepVarMean | 11.7 | 6.40 | 0.21 | 65.3 |

| | Panel B : Settlement | | | |
|---|---|---|---|---|
| | Total asked | Amount Won | Won/asked | Prob winning |
| Public Lawyer | -0.67*** | -0.20*** | 0.087*** | 0.018 |
| | (0.041) | (0.053) | (0.020) | (0.37) |
| Constant | 2.63*** | 6.59*** | 1.52*** | 99.4*** |
| | (0.25) | (0.31) | (0.11) | (2.15) |
| Observations | 3077 | 3077 | 3077 | 3077 |
| BVC | YES | YES | YES | YES |
| Dummy Industry | YES | YES | YES | YES |
| R-squared | 0.74 | 0.37 | 0.11 | 0.0068 |
| DepVarMean | 11.7 | 9.70 | 0.27 | 99.6 |

| | Panel C: Court ruling | | | |
|---|---|---|---|---|
| | Total asked | Amount Won | Won/asked | Prob winning |
| Public Lawyer | -0.44*** | -0.22 | 0.027 | -0.59 |
| | (0.12) | (0.81) | (0.29) | (7.59) |
| Constant | 2.14*** | 0.63 | 1.86** | 7.23 |
| | (0.68) | (3.14) | (0.84) | (27.5) |
| Observations | 422 | 422 | 422 | 422 |
| BVC | YES | YES | YES | YES |
| Dummy Industry | YES | YES | YES | YES |
| R-squared | 0.78 | 0.13 | 0.065 | 0.13 |
| DepVarMean | 11.7 | 2.74 | 0.37 | 24.2 |

This table shows OLS regressions of log total amount asked in the initial labor suit, the amount actually won, the ratio of these two, and the probability of the worker recovering a positive amount. Panel A includes all our historical data files (i.e. for all types of case ending), Panel B focuses on cases ending in settlement, and Panel C on those ending in court ruling. All regressions control for our basic variable controls (gender, at will worker, tenure at the firm, daily wage, weekly hours worked), as well as industry dummies in which firm operates in. We have 4866 observations instead of 5005 since there are some missing values in the controls.

Table C.2: Balance of casefiles having negative recovery amount.

| | Gender | At will worker | Tenure | Daily wage | Weekly hours |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Negative NPV | -0.035** | -0.034 | -0.006*** | -0.000 | 0.002*** |
| | (0.014) | (0.028) | (0.001) | (0.000) | (0.001) |
| Constant | 0.417*** | 0.404*** | 0.428*** | 0.402*** | 0.279*** |
| | (0.017) | (0.016) | (0.017) | (0.016) | (0.035) |
| Observations | 4503 | 4492 | 4470 | 4484 | 4425 |
| R-squared | 0.014 | 0.013 | 0.017 | 0.013 | 0.017 |
| F stat | 13.022 | 12.109 | 15.139 | 11.902 | 15.373 |
| Court dummies | YES | YES | YES | YES | YES |

*Notes:* Each column regresses a characteristic of the case against a dummy variable indicating that the plaintiff recovered an amount less than the MXN$2000 initial filing fee. The sample is all cases from the historical data using private lawyers.

Table C.3: Balance regression on characteristics conditional on employee present

| | Control | Calculator | Observations | R-squared | p-value (calculator = 0) |
|---|---|---|---|---|---|
| Public Lawyer | 0.223*** | 0.034 | 312 | 0.042 | 0.492 |
| | (0.043) | (0.049) | | | |
| Gender | 0.162* | -0.256** | 312 | 0.080 | 0.0395 |
| | (0.0890) | (0.121) | | | |
| At will worker | 0.0197 | -0.0351 | 312 | 0.049 | 0.742 |
| | (0.0965) | (0.106) | | | |
| Tenure | 0.189 | -0.286* | 306 | 0.068 | 0.0814 |
| | (0.133) | (0.161) | . | | |
| Daily wage | 0.0659 | -0.0975 | 307 | 0.006 | 0.493 |
| | (0.141) | (0.141) | | | |
| Weekly hours | 0.0230 | -0.0203 | 306 | 0.136 | 0.863 |
| | (0.0755) | (0.117) | | | |

*Notes:* The table shows results of regressions with the specification $y_i = \alpha_t + \sum_{j=1,2} \beta_j T_j + \epsilon_i$, where j refers to the treatment assignment $calculator, conciliator$. The sample is limited to the cases in which the plaintiff was present at the hearing. All right-hand side variables are standardized so that the constant reflects balance in the control group. Each row represents a different regression with the indicated independent variable. The regressions all include subcourt fixed effects.

Table C.4: Employee presence

| | Employee Present | | |
|---|---|---|---|
| | Phase 1 | Phase 2 | Phase 1/2 |
| | (1) | (2) | (3) |
| Female | -0.066* | 0.000 | -0.023 |
| | (0.035) | (0.023) | (0.019) |
| Public Lawyer | 0.317*** | 0.508*** | 0.416*** |
| | (0.066) | (0.059) | (0.045) |
| At will worker | -0.063 | 0.013 | -0.016 |
| | (0.049) | (0.042) | (0.032) |
| Tenure (years) | 0.004 | 0.005** | 0.005*** |
| | (0.003) | (0.002) | (0.002) |
| Daily wage | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) |
| Weeky hours | -0.002** | -0.003*** | -0.003*** |
| | (0.001) | (0.001) | (0.001) |
| Age | | -0.001 | |
| | | (0.003) | |
| Constant | 0.313*** | 0.303*** | 0.298*** |
| | (0.062) | (0.066) | (0.045) |
| Observations | 520 | 1026 | 1546 |
| R-squared | 0.090 | 0.124 | 0.105 |
| DepVarMean | 0.208 | 0.183 | 0.191 |
| p-value | 0.000 | 0.000 | 0.000 |
| p-value w/o PL | 0.014 | 0.001 | 0.000 |

*Notes:* OLS of employee presence against basic variable controls as predictors. Last two rows shows the p-value testing the quality of all basic variables ( & without public lawyer) respectively.

Table C.5: Expectations Relative to Prediction

| **Panel A** | | | | |
|---|---|---|---|---|
| | Expectation | | Relative OC | |
| | Probability | Amount | Probability | Amount |
| | (1) | (2) | (3) | (4) |
| Employee's Lawyer ($\beta_1$) | 0.070 | 11410.9 | 0.28 | -0.17 |
| | (0.044) | (13314.9) | (0.18) | (0.71) |
| Firm's Lawyer ($\beta_2$) | -0.25*** | -21804.2* | -0.86*** | -0.51 |
| | (0.042) | (12614.9) | (0.17) | (0.67) |
| Constant (employee $\alpha$) | 0.74*** | 66755.3*** | 1.70*** | 1.00* |
| | (0.036) | (10791.5) | (0.15) | (0.58) |
| Observations | 1274 | 1104 | 1177 | 1032 |
| File Fixed Effects | YES | YES | YES | YES |
| R-squared | 0.79 | 0.91 | 0.96 | 0.89 |
| p-value:Emp Law | 0 | 0 | 0 | 0 |
| p-value:Firm Law | 0 | 0 | 0 | 0.04 |

| **Panel B** | | | | |
|---|---|---|---|---|
| | Expectation | | Relative OC | |
| | (1) | (2) | (3) | (4) |
| | Probability | Amount | Amount | Amount |
| Mean | 0.885*** | 58,077*** | 1.713*** | 1.315*** |
| | (0.00288) | (1,794) | (0.0729) | (0.0619) |
| Standar deviation | 0.166 | 74471 | 3.026 | 2.570 |
| Observations | 3,330 | 1,723 | 1,723 | 1,723 |
| Ignores prob. (%) | 13.08 | | | |
| Standard deviation | 0.166 | 74471 | 3.026 | 2.570 |
| Reference | | | Mid prediction | Max prediction |

*Notes:* The table regresses measures of expectation elicited in the baseline survey on dummies of who is the respondent of the survey. For some cases we could elicit the expectation of more than one party (employee, employee's lawyer, firm's lawyer). The omitted variable is the employee dummy, so the interpretation of the employee's lawyer and firm's lawyer coefficients are relative to the employee who is captured in the constant. It combines two phases in one singled pooled dataset. Each column represents a different regression. The first column use elicited probability of winning as a dependent variable. The exact question is: *"How likely is it that you will win the lawsuit if it ends in a court judgment?"*. The second column use the peso amount (undiscounted) that they expect to recover conditional on winning. The exact wording of the survey question is: *"in case you win the lawsuit, what amount are you most likely to win?"*. All columns include casefile fixed effects, avoiding the comparison across casefiles. The bottom of the table present the p-values of two null hypothesis: $\alpha + \beta_1 = 0$, and $\alpha + \beta_2 = 0$, telling us whether the employee's laywer, or firm's lawyer are more or less confident. Last 2 columns use a measure of "overconfidence", which compares the subjective expectation vs the personalized calculator prediction. Relative OC is computed as $\frac{expectation - prediction}{prediction}$, where expectation refers to the expectation measured in the baseline survey.

Table C.6: First stage and robustness for the control function regression

| | Probit | First Stage (OLS) | First Stage (probit) | | CF |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Calculator | 0.122 | 0.028 | 0.0281 | 0.110 | 0.002 |
| | (0.091) | (0.017) | (0.0183) | (0.069) | (0.015) |
| Emp present (EP) | 0.726*** | | | | 0.698*** |
| | (0.174) | | | | (0.199) |
| Calculator#EP | 0.253 | | | | 0.122** |
| | (0.202) | | | | (0.059) |
| Control Function | | | | | -0.312*** |
| | | | | | (0.104) |
| Time instrument | | 0.055*** | 0.0550*** | | |
| | | (0.018) | (0.0178) | | |
| 09:30 | | | | -0.133 | |
| | | | | (0.159) | |
| 10:00 | | | | -0.327*** | |
| | | | | (0.113) | |
| 10:30 | | | | -0.157 | |
| | | | | (0.134) | |
| 11:00 | | | | -0.224** | |
| | | | | (0.105) | |
| 11:30 | | | | -0.504*** | |
| | | | | (0.184) | |
| 12:00 | | | | -0.139 | |
| | | | | (0.144) | |
| 12:30 | | | | -0.031 | |
| | | | | (0.441) | |
| Constant | -1.969*** | -0.072 | -0.0721 | -2.134*** | 0.067 |
| | (0.510) | (0.049) | (0.0500) | (0.464) | (0.061) |
| Observations | 1,709 | 1,732 | 1,732 | 1,732 | 1,732 |
| R-squared | . | 0.034 | . | . | 0.144 |
| Casefile controls | YES | YES | YES | YES | YES |

*Notes:* The first column repeats the regression in column 5 of 3 using a probit specification rather than a linear probability model. The second column is a OLS first stage for employee present included just for comparison with columns 3 and 4. The third column is the first stage of the control function regression shown in column 9 of 3. Columns 4 and 5 repeat the control function regression using individual dummy variables for each-hour hearing time. Column 4 shows the first stage and column 5 the outcome regression including the residual from the regression in column 4. See the notes to 3 for further details.

Table C.7: Heterogeneity in treatment effects

| Interaction var | Daily wage | | | Tenure | | | Weekly hours | | |
|---|---|---|---|---|---|---|---|---|---|
| | Phase 1 | Phase 2 | Phase 1/2 | Phase 1 | Phase 2 | Phase 1/2 | Phase 1 | Phase 2 | Phase 1/2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Control (Constant) | 0.054*** | 0.074 | 0.079** | 0.055*** | 0.10** | 0.074** | 0.067*** | 0.100** | 0.10*** |
| | (0.018) | (0.045) | (0.036) | (0.019) | (0.040) | (0.035) | (0.023) | (0.044) | (0.033) |
| Calculator | 0.096** | 0.044 | 0.0013 | 0.089** | 0.034 | 0.0068 | 0.068 | 0.027 | 0.0023 |
| | (0.041) | (0.027) | (0.020) | (0.038) | (0.022) | (0.019) | (0.047) | (0.038) | (0.027) |
| Interaction Var (Int) | 0.045 | 0.084 | 0.039 | 0.036 | 0.034 | 0.048* | 0.019 | 0.019 | -0.023 |
| | (0.034) | (0.060) | (0.040) | (0.031) | (0.037) | (0.025) | (0.033) | (0.044) | (0.025) |
| Calculator#Int | -0.070 | -0.00019 | 0.027 | -0.065 | 0.0054 | 0.012 | -0.0091 | 0.035 | 0.027 |
| | (0.065) | (0.066) | (0.046) | (0.056) | (0.040) | (0.030) | (0.059) | (0.063) | (0.042) |
| Emp present (EP) | | | 0.045 | | | 0.15*** | | | -0.011 |
| | | | (0.050) | | | (0.047) | | | (0.067) |
| Calculator#EP | | | 0.32*** | | | 0.22*** | | | 0.30*** |
| | | | (0.079) | | | (0.067) | | | (0.095) |
| EP#Int | | | 0.14* | | | -0.0041 | | | 0.19** |
| | | | (0.077) | | | (0.072) | | | (0.091) |
| Calculator#EP#Int | | | -0.24** | | | -0.13 | | | -0.16 |
| | | | (0.10) | | | (0.093) | | | (0.13) |
| Observations | 507 | 1067 | 1574 | 506 | 1041 | 1547 | 502 | 1074 | 1576 |
| R-squared | 0.015 | 0.060 | 0.13 | 0.012 | 0.053 | 0.14 | 0.011 | 0.054 | 0.13 |
| DepVarMean | 0.10 | 0.21 | | 0.10 | 0.21 | | 0.11 | 0.20 | |

| Interaction var | Gender | | | Public Lawyer | | | Entitlement | | |
|---|---|---|---|---|---|---|---|---|---|
| | Phase 1 | Phase 2 | Phase 1/2 | Phase 1 | Phase 2 | Phase 1/2 | Phase 1 | Phase 2 | Phase 1/2 |
| | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) |
| Control (Constant) | 0.074*** | 0.092** | 0.092*** | 0.068*** | 0.11*** | 0.095*** | 0.056*** | 0.054 | 0.064* |
| | (0.022) | (0.040) | (0.030) | (0.018) | (0.029) | (0.027) | (0.021) | (0.042) | (0.034) |
| Calculator | 0.086** | 0.056** | 0.0017 | 0.060* | 0.045* | 0.015 | 0.069* | 0.060* | 0.0051 |
| | (0.040) | (0.024) | (0.017) | (0.030) | (0.023) | (0.016) | (0.037) | (0.030) | (0.020) |
| Interaction Var (Int) | 0.0033 | 0.049 | 0.013 | 0.065 | 0.041 | -0.029 | 0.033 | 0.11** | 0.066* |
| | (0.036) | (0.041) | (0.023) | (0.060) | (0.11) | (0.042) | (0.035) | (0.049) | (0.036) |
| Calculator#Int | -0.063 | -0.025 | 0.029 | -0.0059 | -0.032 | 0.0015 | -0.018 | -0.022 | 0.019 |
| | (0.062) | (0.039) | (0.027) | (0.083) | (0.12) | (0.036) | (0.057) | (0.063) | (0.043) |
| Emp present (EP) | | | 0.095* | | | 0.12** | | | 0.093 |
| | | | (0.055) | | | (0.046) | | | (0.057) |
| Calculator#EP | | | 0.27*** | | | 0.23*** | | | 0.25*** |
| | | | (0.064) | | | (0.069) | | | (0.089) |
| EP#Int | | | 0.070 | | | 0.077 | | | 0.063 |
| | | | (0.089) | | | (0.096) | | | (0.092) |
| Calculator#EP#Int | | | -0.22* | | | -0.24** | | | -0.12 |
| | | | (0.11) | | | (0.11) | | | (0.13) |
| Observations | 514 | 1081 | 1595 | 514 | 1081 | 1595 | 499 | 1081 | 1580 |
| R-squared | 0.014 | 0.051 | 0.13 | 0.014 | 0.050 | 0.13 | 0.012 | 0.064 | 0.14 |
| DepVarMean | 0.11 | 0.21 | | 0.11 | 0.21 | | 0.10 | 0.21 | |

*Notes:* We test for heterogeneity of treatment effects by interacting treatment with the variable shown in the column header. For each test, the first column uses the sample from phase 1, the second from phase 2, and the third from the combined sample. For example, the third column in the top panel shows that, in the combined sample, plaintiffs with higher daily wages (as reported in the case file) were marginally significantly less likely to settle when the plaintiff was present and provided the calculator information.

Table C.8: Treatment Effects with placebo arm - Phase 1

| | Months after treatment | |
|---|---|---|
| | Same day | |
| | (1) | (2) |
| Control (Constant) | 0.072*** | 0.041*** |
| | (0.015) | (0.013) |
| Calculator | 0.044* | 0.016 |
| | (0.024) | (0.020) |
| Placebo | -0.011 | 0.0058 |
| | (0.020) | (0.018) |
| Placebo ctrl | -0.032* | -0.017 |
| | (0.017) | (0.014) |
| Emp present (EP) | | 0.16*** |
| | | (0.053) |
| Calculator##EP | | 0.15* |
| | | (0.086) |
| Plaecbo##EP | | -0.050 |
| | | (0.077) |
| Placebo ctrl##EP | | 0.047 |
| | | (0.076) |
| Observations | 1668 | 1668 |
| R-squared | 0.012 | 0.096 |
| DepVarMean | 0.064 | 0.064 |
| InteractionVarMean | | 0.13 |
| Calc=Placebo | 0.017 | 0.62 |
| Calc=Conc=Placebo=0 | 0.070 | 0.43 |
| =Placebos | 0.10 | 0.12 |

*Notes:* The table reports regressions using the full sample and including variables indicating that the case received a placebo treatment, as described in the text.

Table C.9: Treatment generated updating in probability - Phase 1

(a) Overconfidents

| | Relative updating | | |
|---|---|---|---|
| | Employee | Emp Lawyer | Firm Lawyer |
| | (1) | (2) | (3) |
| Calculator | -0.45** | 0.63 | -0.48 |
| | (0.21) | (3.92) | (0.62) |
| Constant | -1.63** | -15.9 | 1.24* |
| | (0.58) | (11.1) | (0.72) |
| Observations | 30 | 79 | 58 |
| Basic Variable Controls | YES | YES | YES |
| Other controls | YES | YES | YES |
| R-squared | 0.45 | 0.11 | 0.095 |
| Update (mean) | -0.18 | 2.58 | 0.65 |
| Update (SD) | 0.46 | 16.2 | 2.07 |

(b) Underconfidents

| | Relative updating | | |
|---|---|---|---|
| | Employee | Emp Lawyer | Firm Lawyer |
| | (1) | (2) | (3) |
| Calculator | 14.5 | -2.59 | 0.042 |
| | (15.6) | (1.97) | (0.36) |
| Constant | -167.5* | 3.11 | -2.46** |
| | (72.1) | (5.50) | (1.07) |
| Observations | 20 | 56 | 32 |
| Basic Variable Controls | YES | YES | YES |
| Other controls | YES | YES | YES |
| R-squared | 0.97 | 0.15 | 0.16 |
| Update (mean) | 18.1 | 2.20 | -0.23 |
| Update (SD) | 77.9 | 9.27 | 0.98 |

*Notes:* This table presents the treatment effect of the calculator on relative expectation updating, defined as $\frac{exitsurvey-initialsurvey}{initialsurvey}$. Sample is restricted to over-confident cases (i.e. *initial survey¿calculator prediction*) for Panel A, while it is restricted to under-confident cases for Panel B. For both panels, column 1 presents the results for surveyed employees, column 2 the results for surveyed employee's lawyer and table 3 the results for the firm's lawyer. All regressions include basic variables as controls (Public lawyer, Gender, At will worker, Tenure, Daily wage & Weekly hours).

Table C.10: Duration of Cases by Treatment

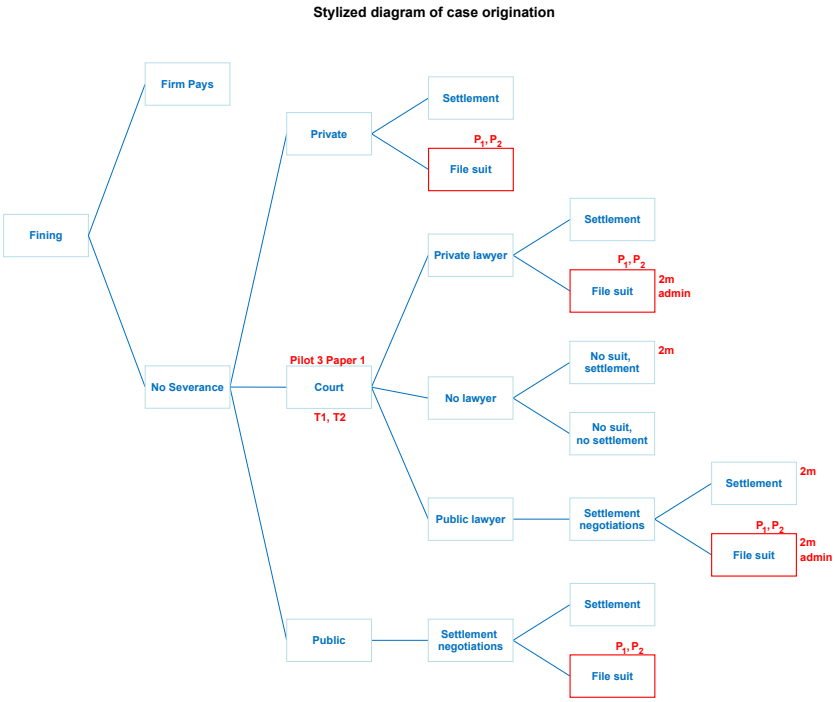
| | Case duration in months | | |
|---|---|---|---|
| | OLS | COX | COX |
| | (1) | (2) | (3) |
| Calculator | 17.480 | -0.073 | -0.041 |
| | (26.680) | (0.065) | (0.101) |
| Emp present (EP) | -59.887 | 0.133 | 0.013 |
| | (51.101) | (0.132) | (0.210) |
| Calcularot#EP | -111.731* | 0.318* | -0.071 |
| | (63.117) | (0.183) | (0.277) |
| Observations | 1,549 | 1,549 | 816 |
| R-squared | 0.427 | | |
| DepVarMean | 0.174 | | |
| Casefile Controls | Yes | Yes | Yes |
| Includes settled | Yes | Yes | No |

*Notes:* The table shows the duration of cases in phases 1 and 2, measured in days from the date of filing to the date of the final resolution. All regressions control for the age of the case at the time of treatment, and include dummies for phase 2 and the subcourt where the case was heard. Column 1 is an OLS regression and Columns 2 and 3 are Cox proportional hazards models, showing the coefficients rather than proportional hazards. Columns 1 and 2 include all cases and Column 3 removes from the sample cases that were settled. The sample in all regressions is limited to cases with private lawyers. The sample is slightly smaller than that in Table 4 because we eliminate a few cases with a missing date of filing.

## 9.6 Figures

Figure C.1: Stylized Depiction of the Labor Justice Process

Diagram of Case Origination.pdf Diagram of Case Origination.pdf




*Notes:* This figure is a stylized representation of the process that casefiles follow in Mexico City's Labor Court.

Figure C.2: Calculator Treatment Format (example) - Phase 2

(a) Plaintiff

JLCA CDMX

**CÁLCULO DE COMPENSACIÓN TRABAJADOR**

*ALEJANDRA [redacted] vs [redacted] SA DE CV*
*Expediente J11 / [redacted] / 2016*

- **La Calculadora se basa en 5000 expedientes concluidos del año 2011.**
- **A continuación le da información de cómo terminaron juicios similares al suyo.**
- **¡IMPORTANTE! 16% de los juicios duran más de 4 años.**

| De los juicios concluidos más similares al suyo... | |
|---|---|
| → Los que llegaron **a un convenio**, en promedio obtuvieron: | $22,494 |
| → De los que continuaron hasta un **laudo**, el **70%** **NO** obtuvieron pago. | |

- **Como referencia, le damos los datos básicos de su demanda**

| Datos básicos de la demanda | | | | | |
|---|---|---|---|---|---|
| **Género:** Mujer | **Salario diario:** $700.00 mxn diarios | | **Antigüedad:** 1.85 años | | |
| **Prestaciones Solicitadas** | Reinstalación | ✓ | | | |
| | Indemnización Constitucional (90 días de salari | ✓ | | | |
| | Horas Extras | ✓ | | | |
| | Indemnización de 20 días por año | ✓ | | | |

**La Calculadora provee datos estadísticos de juicios similares. No afecta sus derechos ni influye en su juicio.**

No. Folio : J11 / [redacted] 2016 A

(b) Defendant

JLCA CDMX

**CÁLCULO DE PAGO DEMANDADO**

*ALEJANDRA [redacted] vs [redacted] SA DE CV*
*Expediente J11 / [redacted] / 2016*

- **La Calculadora se basa en 5000 expedientes concluidos del año 2011.**
- **A continuación le da información de cómo terminaron juicios similares al suyo.**
- **¡IMPORTANTE! 16% de los juicios duran más de 4 años.**

| De los juicios concluidos más similares al suyo... | |
|---|---|
| → En caso de **perder en el laudo**, en promedio pagan | $174,197 |
| → En caso de **llegar a un convenio**, en promedio pagan | $22,494 |

- **Como referencia, le damos el cálculo del 100% de las prestaciones básicas, en caso de que gane el trabajador por despido injustificado**

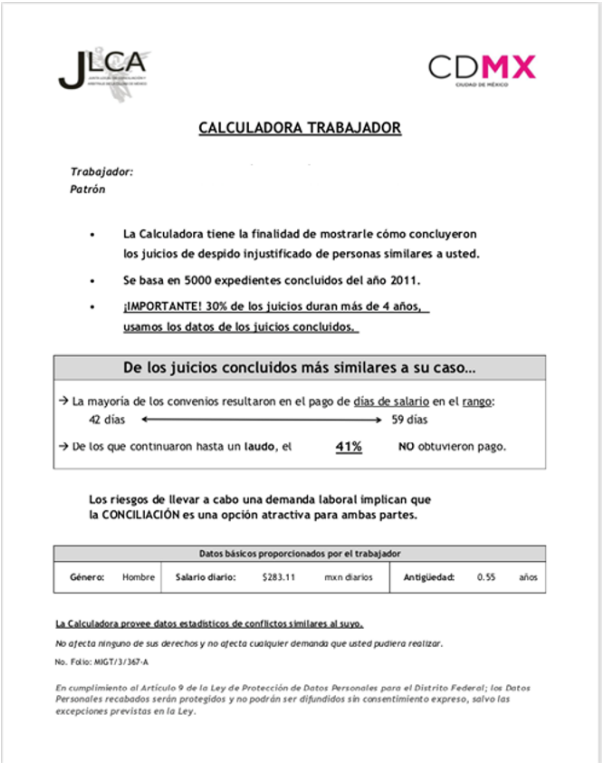
| Datos del trabajador | | | | | |
|---|---|---|---|---|---|
| **Género:** Mujer | **Salario diario:** $700.00 mxn diarios | | **Antigüedad:** 1.85 años | | |
| **Indemnización Constitucional** | 90 días de salario diario integrado | | $63,000.00 | | |
| **Prima de Antigüedad** | 12 días por año laborado | | $3,246.58 | | |
| **Aguinaldo** | Parte proporcional de 15 días del último año | | $7,312.33 | | |
| **Vacaciones** | Parte proporcional del último año laborado | | $4,815.12 | | |
| **100% DE LAS PRESTACIONES BÁSICAS** | | | **$78,374.03** | | |

**La Calculadora provee datos estadísticos de juicios similares. No afecta sus derechos ni influye en su juicio.**

No. Folio : J11 / [redacted] / 2016 D

*Notes:* The figure shows examples of the calculator treatment used in phase 2. The left panel format was given to the plaintiff / dismissed worker and the right panel format to the defendant. The calculator shows two numbers: first, for cases with similar characteristics that settled, the average amount obtained in settlement; second, each party's contingency in case they did not settle and proceeded to a judge's ruling. This is the likelihood of not obtaining any payment for the plaintiff and the recovery amount conditional on positive recovery for the defendant. As in phase 1, parties were told that this information comes from a statistical exercise based on completed historical cases, and that it gives average prediction based on variables of the initial lawsuit described in the calculator treatment.

Figure C.3: Calculator Treatment Format (example) - Phase 3



JLCA

CDMX

**CALCULADORA TRABAJADOR**

***Trabajador:***
***Patrón***

- **La Calculadora tiene la finalidad de mostrarle cómo concluyeron los juicios de despido injustificado de personas similares a usted.**
- **Se basa en 5000 expedientes concluidos del año 2011.**
- **¡IMPORTANTE! 30% de los juicios duran más de 4 años, usamos los datos de los juicios concluidos.**

**De los juicios concluidos más similares a su caso...**

→ La mayoría de los convenios resultaron en el pago de días de salario en el rango:
42 días ⟷ 59 días

→ De los que continuaron hasta un **laudo**, el **41%** **NO** obtuvieron pago.

**Los riesgos de llevar a cabo una demanda laboral implican que la CONCILIACIÓN es una opción atractiva para ambas partes.**

| Datos básicos proporcionados por el trabajador | | | | | | | |
|---|---|---|---|---|---|---|---|
| **Género:** | Hombre | **Salario diario:** | $283.11 | mxn diarios | **Antigüedad:** | 0.55 | años |

**La Calculadora provee datos estadísticos de conflictos similares al suyo.**

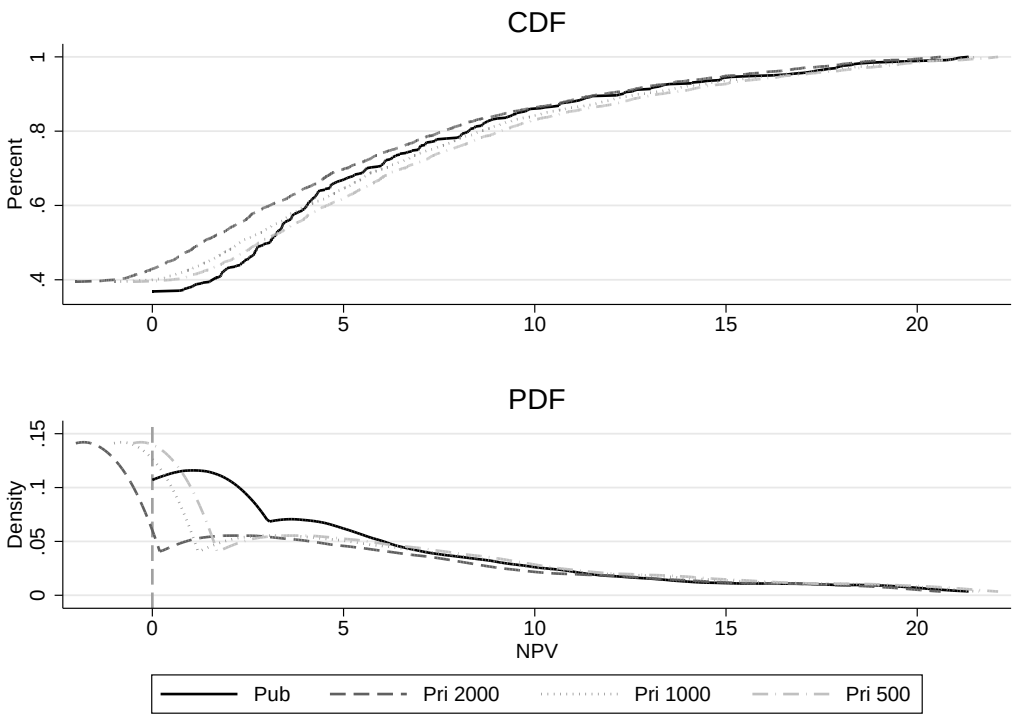
*No afecta ninguno de sus derechos y no afecta cualquier demanda que usted pudiera realizar.*

No. Folio: MIGT/3/367-A

*En cumplimiento al Artículo 9 de la Ley de Protección de Datos Personales para el Distrito Federal; los Datos Personales recabados serán protegidos y no podrán ser difundidos sin consentimiento expreso, salvo las excepciones previstas en la Ley.*

*Notes:* The figure shows an example of the calculator treatment provided to dismissed workers in phase 3. The calculator shows the inter-quartile range of settlement amounts expressed as the number of days paid and the percentage of cases continuing to court judgment where the plaintiff recovered nothing. The information provided is specific to the worker's characteristics. As in phases 1 and 2, parties were told that this information comes from a statistical exercise based on completed historical cases, and that it gives average prediction based on variables of the initial lawsuit described in the calculator treatment.

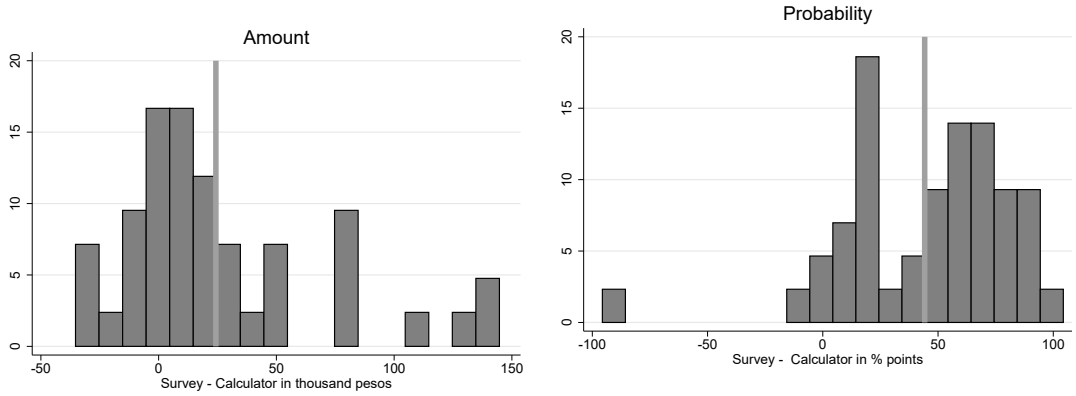
Figure C.4: Distribution of Amount Collected, by Type of Lawyer



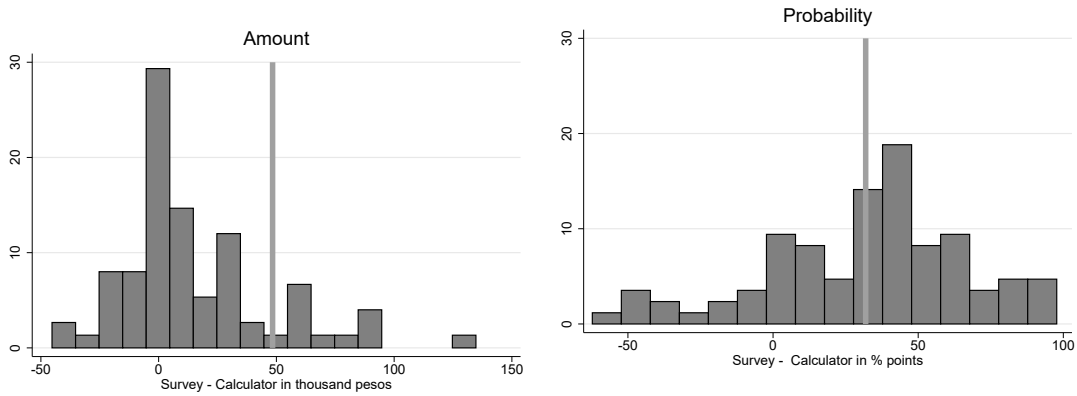

*Notes:* This Figure uses the historical data to show cumulative distributions and densities of the amount received in the historical data. It uses all casefiles endings (court ruling, settlements, drop and expiry). Amounts on the x-axis are in thousand pesos and brought to present value to the time of suing, with a monthly interest rate of 3.43. Since we care about what the worker actually receives, when they use a private lawyer we subtract a 30% of the recovery and the initial filing feel. The graph shows outcomes for three different initial fees (indicated in the legend): MXN$2,000 pesos, MXN$1,000 pesos, and MXN$500 pesos. The modal fee in the survey data is MXN$2,000 pesos. To the left of the vertical line at zero the worker loses money (from the initial fee). The figure indicates that there are a large fraction of cases where the worker has a negative net recovery.

Figure C.5: Subjective expectation minus prediction - Phase 1

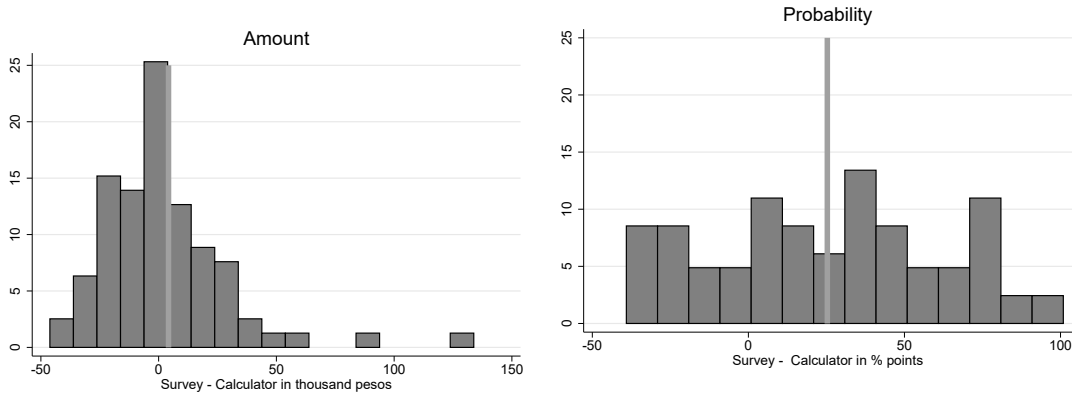
(a) Employee




(b) Employee's Lawyer

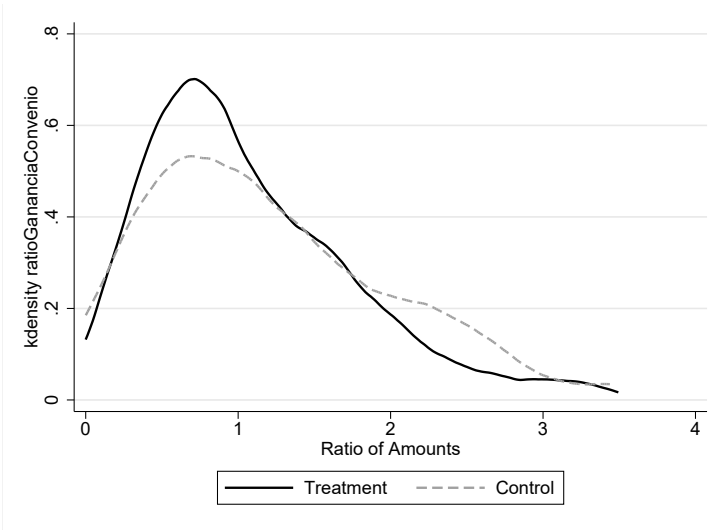



(c) Firm's Lawyer




*Notes:* Difference in thousand pesos for amounts (panel on left) and percentage points for probabilities (panel on right) from what the subject expects vs. what our models predict. Note how, for Employee and employee lawyer, the distribution for amounts is much more skewed to the right than for firm lawyers. This is only natural, since the former are thinking about expected wins and the latter about expected losses.

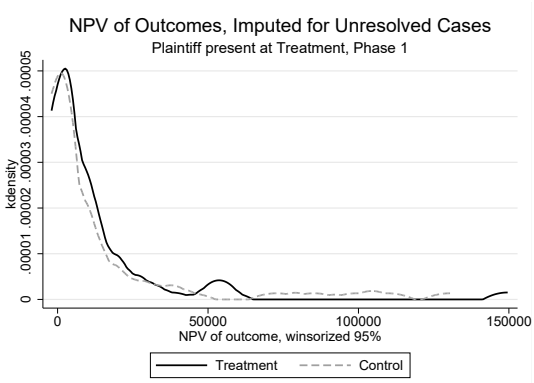
Figure C.6: Settlement Amount vs. Calculator



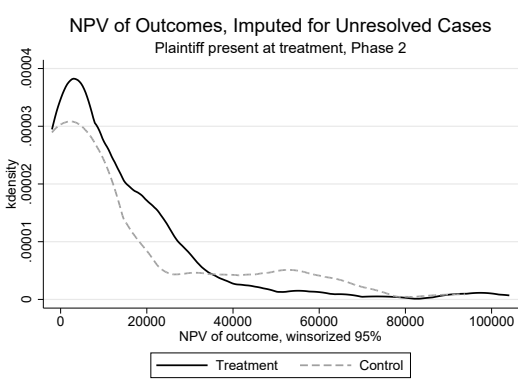

*Notes:* This figure presents the distribution of actual settlement amount over the calculator prediction. Data are truncated at the 99th percentile to compress the scale.

Figure C.7: Outcomes when Plaintiff was Present, by Treatment

(a) Phase 1



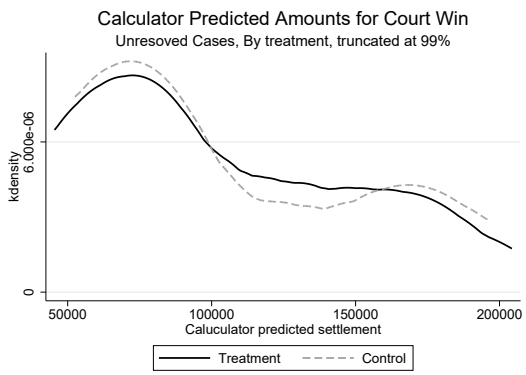

(b) Phase 2



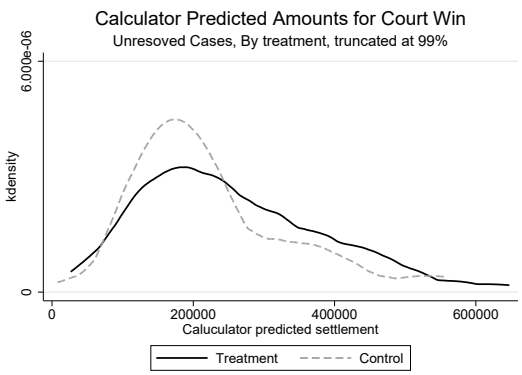

*Notes:* This figure shows the kernel density of the NPV of the amount awarded by treatment and phase of the intervention. NPV is discounted at an annual rate of 50 percent and the value of unresolved cases is imputed from the calculator prediction for outcome by court judgment. Data are winsorized at the 95th percentile.

Figure C.8: Calculator Predictions for Plaintiff Court Judgment, By Phase

(a) Phase 1

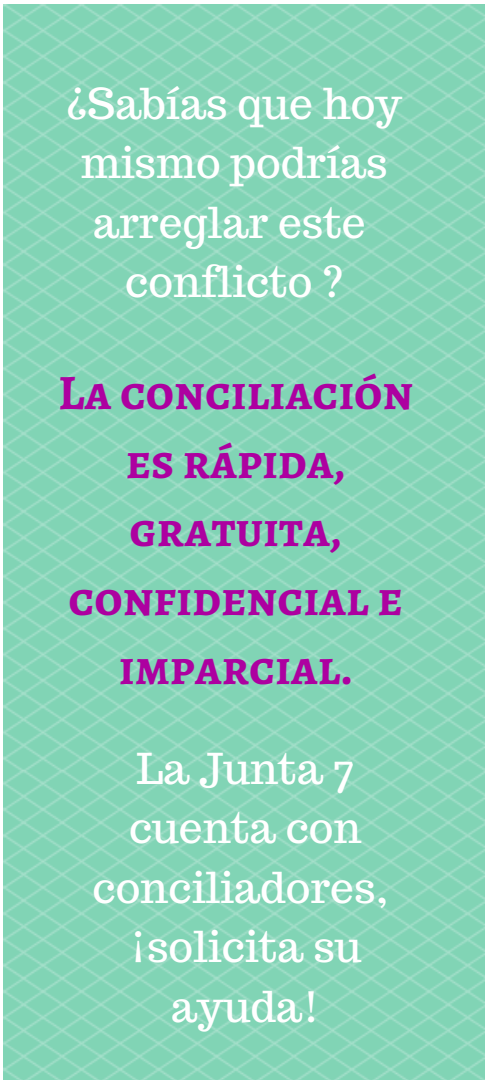



(b) Phase 2



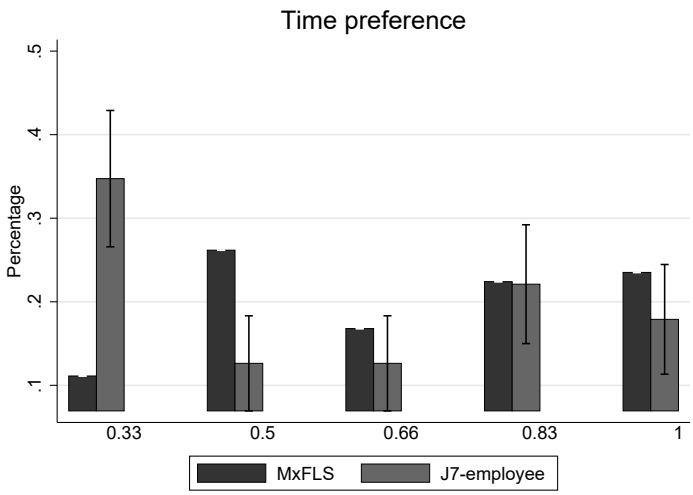

*Notes:* This figure presents the calculator prediction for amount recovered conditional on the plaintiff winning a court judgment, for treatment and control cases that were unresolved as of 2020. The cases in phase 1 and phase 2 are shown separately. Data are truncated at the 99th percentile to compress the scale.

Figure C.9: Information handed out in placebo test




*Notes:* This is the information brochure handed out to subjects in the placebo test. Essentially, it is a reminder of the existence of the conciliation process, which is free, confidential and unbiased.

Figure C.10: Discount rate for Phase 1 and MxFLS




*Notes:* Comparison of discount rates for Phase 1 data and survey data from the MxFLS (Mexican Family Life Survey- a longitudinal survey in Mexico that follows individuals across rounds). In both surveys individuals are asked whether they would choose to recieve $1,000 dolars today or an array of different values in the future ($1,200, $1,500, $2,000 or $3,000 dollars). The monthly discount rate is calculated by dividing the $1,000 dollars over the answered choice made by individuals. This was done accordingly to the MxFLS.