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

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ABSTRACT

While observers agree that courts function poorly in developing countries, a lack of data has limited our understanding of the causes of malfunction. We combine data from administrative records on severance cases filed in the Mexico City Labor Court with interventions that provide information to parties in randomly selected cases on predicted case outcomes and conciliation services. We first use the data to document a set of stylized facts about the functioning of the court. The interventions nearly double the overall settlement rate, but only when the plaintiff herself is present to receive the information directly. Administrative records from six months after the treatment indicate that the treatment effects remain unchanged over that period, even though an additional one in three cases in the control group settle in that period. The post-treatment results indicate that lawyers do not convey the information provided in the intervention to their clients. A simple analytic framework rationalizes the experimental results. Analysis of settlements induced by the interventions suggests that the provision of information is welfare-improving for the plaintiffs. The experimental results replicate over two phases conducted in different sub-courts, showing robustness.

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1 Introduction

Courts function poorly in most developing countries. Outcomes are unpredictable, parties are misinformed, and inefficient processes lead to slow decisions and large case backlogs. Poorly functioning courts hamper the functioning of markets and raise concerns for justice. Although this is widely understood and there is anecdotal evidence of corruption and inefficiency in the legal systems of developing countries, there is little rigorous evidence on the underlying causes of the courts' ineffectiveness. With the aim of filling this gap, we work with the Mexico City Labor Court (MCLC). The MCLC receives more than 30,000 filings per year from workers who claim to have been involuntarily separated from their jobs and are seeking severance pay due to them according to Mexican labor law. We use data from historical case files and a randomized field experiment conducted with ongoing cases to understand sources of inefficiency in the court.

One notable feature of our data is the high proportion of the cases involving low stakes, inexperienced plaintiffs, and settlement rates that are low by international standards. This suggests potential inefficiencies in bargaining between the parties. There is an extensive literature examining bargaining breakdowns and delays. The canonical Rubinstein (1982) bargaining framework shows that where information is complete, delays are costly, and parties bargain by making alternative offers, the bargaining outcome is immediate and efficient. However, the efficiency result breaks down if parties have asymmetric information, a point made in the seminal paper by Myerson and Satterthwaite (1983). Bargaining may also be delayed or break down if parties are overly optimistic.[1] Our data will show that both misinformation and excess 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 lawyers representing either side. This raises potential agency issues that, theoretically, may either result in either more or less efficient outcomes. Gilson and Mnookin (1994) model court cases as prisoners dilemmas where the parties play once and the lawyers play repeatedly. As such, the lawyers may cooperate in ways that the players would not, generating more efficient outcomes. But agency may also drive a wedge between either party and her lawyer if the incentives of the two diverge. Lawyers on both sides of the cases we study are typically specialized in labor cases, and hence have extensive experience in the labor courts. This gives them informational advantages over their clients who, especially on the plaintiff's side, are typically first-time users of the labor courts.

Previous research suggests that, where interests diverge, expert-agents may use informational advantages to make recommendations or decisions not fully in the interest of the principal. For example Schneider (2012) reports on an audit study showing that one-third of auto repair shops

[1] See Yildiz (2011) for a review of the theory on this point. Overoptimism may also reflect self-serving bias (Babcock and Loewenstein (1997)) if parties may feel that they deserve to win the case. We discuss potential causes of overoptimism below, but do not attempt to clearly distinguish between fairness and other causes for optimism in the data.

in Canada recommend unnecessary repairs for vehicles, Emons (1997) cites evidence showing doctors in Switzerland are less likely to undergo surgery, and Levitt and Syverson (2008) show that real estate agents sell houses they own for higher prices than houses of clients.[2] We argue analytically that the incentives of the plaintiffs and their own lawyers may diverge in our context, providing an underlying rational for potential agency issues.

Improving the efficiency of the court thus requires a deeper understanding of both the bargaining environment and agency issues. With these issues in mind, we begin by analyzing the information environment of the court. We digitize more than 5,000 completed cases from the court's historical record, and conduct surveys with parties in currently-active cases. These data allow us to document a set of stylized facts about the functioning of the court. We show first that, although the law provides that suits should be adjudicated within three months, the court has a backlog of about four years. Among all cases filed in the court between 2009 and 2012, about a third were unresolved in early 2016. The backlog of cases is in part driven by settlement rates that are low by international standards: less than 60% of cases are settled in Mexico, compared with 80-90 percent in higher income countries (OECD). A large majority of dismissed workers (plaintiffs) are using the court for the first time. A typical worker has little knowledge of her legal entitlements and, more surprisingly, is often uninformed even about what she herself is claiming in her lawsuit. Lawyers are much better informed, being at least aware of the details of the filed case. We also document that parties are overconfident: the sum of the two parties' probabilities of winning far exceeds 100% and, particularly on the plaintiff's side, both the probability of winning and the award are optimistic relative to predictions based on historical cases.

With the goal of identifying the role misinformation and overconfidence play in generating low settlement rates, we conduct an experiment that changes the information available to the parties in randomly selected ongoing cases.[3] The experiment is carried out in two phases in on-going cases filed in the Mexico City Labor Court. Mexico's largest labor court, the MCLC receives 30,000 cases per year in 20 separate subcourts. In the first phase of the experiment, carried out between March and May 2016, active case files from a single subcourt were randomly assigned to receive either encouragement to conduct a session with a conciliator (the "conciliator treatment"); statistical predictions on the probability of winning their case and on the expected award conditional on winning (the "calculator treatment,"); or neither of the two (the control group). In the first treatment, we use conciliators employed by the court. Each subcourt is assigned a conciliator who acts

[2]Hubbard (1998) & Hubbard (2000) suggests that reputation is effective in controlling agency in the automobile emissions testing market in California. The fact that plaintiff's typically use the labor court only once in their life and users are not often connected to other users makes undermines the development of reputation by lawyers.

[3]Most of the research on agency in relationships between principals and expert agency is all based on very clever variation in observational data, or on lab experiments. (See Domenighetti et al. (1993) and Levitt and Syverson (2008) for examples of evidence using observational differences, and Dulleck et al. (2011) for an example of lab experiments on the issue. Schneider (2012) and Das et al. (2016) conduct audit studies among auto repair providers in the U.S. and public- and private-sector medical clinics in India, respectively.) We are unaware of other field experiments providing evidence on the effect of expert agency issues on the efficiency of markets or institutions.

as a neutral, non-binding, mediator. Our intervention required parties to sit with the conciliator on the day of the intervention. An additional conciliator was provided to subcourt 7, where we implemented this intervention, during our experimental window.

In the second arm of the treatment, we provided predictions on case outcomes. We use machine learning techniques on data from 5,000 concluded cases filed in 2011 to estimate predictive models based on the characteristics of each individual case. We describe how we used the historical data in more detail below and in Appendix A. This intervention provided two key pieces of information: the percentage of similar cases filed four years previous that are still on-going, as a measure of expected duration; and the awards collected by the plaintiffs in cases that were settled by agreement between the parties.

The second phase of the experiment scaled the initial work to five subcourts, but narrowed the focus to the intervention based on the predicted case outcomes. Scaling up the experiment provides a replication of the initial results in a different sample. The population from which the phase-two sample was drawn is different in two ways. First, each subcourt handles cases for workers from different industries, and hence the additional subcourts provide cases from different industries. Second, we adjusted the design somewhat based on what we learned in the first phase of the experiment. In particular, we noticed in the first phase that the effect on settlement was largest in recently-filed cases. Hence, in the second phase we focus on cases holding their initial hearing. Finally, we worried that the presence of our research assistants might be affecting outcomes. So, in the subcourt where we conducted phase one of the experiment, we added a placebo treatment just after the end of phase two. In the placebo treatment, we provided a randomly-selected subset of the control group with a leaflet and a one-minute speech explaining that the court had conciliators that could help them to conciliate their case.

Our main experimental outcome is whether the case was settled on the day of the intervention or within six months of the date of the treatment, the later based on administrative data from the court. Settlement is an important outcome for the court, given its large backlog. But in a typical case, settlement also provides the plaintiff with an award as much as three to four years earlier than a decision by the judge. Because the median plaintiff is a low-wage worker who discounts the future at a high rate, the earlier payment may be beneficial to them.

We find that the calculator and conciliator treatments both lead to a near doubling of the rate of settlement on the day the treatment is provided. However, the settlement rate increases only in the 18 percent of hearings where the worker is present at the court on the day of the intervention.[4] Conditional on the worker being present, settlement rates increase more than sixfold, from around 3 percent in the control group to around 20 percent in each of the treatment groups. Administrative data from up to six months after the day of treatment indicate that settlement rates in the control group increase by 30 percentage points in the half-year after the treatment. But the treatment

[4]The employee's presence is likely endogenous. However, the treatment is orthogonal to the employee's presence. We discuss this issue in more detail below.

effects remain constant over that time. That is, all of the treatment effect occurs on the day of the treatment. The fact that the treatment effect is both constant and persistent has two implications. First, the fact that the treatments have a fairly precisely measured zero effect for those cases where we delivered the treatment only to the employee's lawyer, both on the day of the intervention and six months later, is consistent with lawyers not conveying the information to their clients. The fact that the treatment effect persists even as the number of settlements in the control group increases substantially suggests that the intervention did more than just speed up settlements: it appears to have led to settlement of cases that would not have otherwise been settled, at least over the following six months. The results from the calculator treatment suggest that the plaintiffs retain control of decision-making when they are fully informed, but that their lawyers may retain influence over decision-making by controlling the flow of information to clients.

Settlement is an outcome that the law promotes and that the court is interested in. But we should also be concerned with the welfare consequences, particularly for workers. We show in a simple agency framework that the plaintiff and her lawyer may have different incentives with regard to settlement, potentially impeding settlement in cases where settlement would be beneficial to the plaintiff. We also compare the settlement amounts received by treated plaintiffs to two counterfactuals. The first is the settlement amount among cases in the control group that settled on the same day. For the second couneterfactual, we compare discounted awards recovered by the plaintiffs from a set of cases in our historical files matched on observables and ending in a judge's decision. The average settlement in the treatment arms is similar to that in the control group. Moreover, compared with the matched historical cases we show that, if anything, the plaintiffs in our treated cases recover larger awards in present value terms.

The two treatments are relevant from a policy perspective. While there is little evidence on the effectiveness of non-binding arbitration, in 2017 Mexico passed a constitutional reform mandating a conciliation hearing before a labor suit is brought to court. The statistical information arm directly addresses the issues of informational asymmetries and overconfidence, and its implementation is scalable and transparent. When faced with the concern about inefficient legal institutions, the standard policy advice is very general: Improve the quality of the court system. The results from the experiment allow us to provide much more specific advice: providing information about past outcomes results in settlement of cases that would not otherwise have settled. The experiment also illuminates the importance of the agency issue between the plaintiff and her lawyer. While this experiment does not allow us to offer a solution to that issue, the data do suggest that better understanding the market for reputation among lawyers is another potentially fruitful area that may lead to specific policy advice.

In addition to contributing to the empirical literature on bargaining in legal proceedings, the paper relates to two other literatures. The first is the very large literature on institutions and institutional reform in developing countries. The link between institutional quality and economic

growth is now well established, most convincingly by use of variation in historical circumstances (e.g., Acemoglu et al. (2001). More recently, attention has shifted to the question of how to reform poorly-functioning institutions. There is a very small literature focused on courts in particular. Two notable efforts in this area are recent work by Kondylis and Stein (2018) examining the effect of an administrative reform on civil case duration in Senegal and Ponticelli and Alencar (2016), who use variation in court enforcement across states in Brazil to study the effect of bankruptcy reform on access to credit. The paper also relates to the literature on the efficiency of labor markets in low- and middle-income countries. This literature has focused mainly on the wedges created by labor market regulations. For example, Besley and Burgess (2004) shows that pro-worker labor regulations lower employment, investment and productivity in formal sector manufacturing in India, and Almeida and Carneiro (2009) show that increased enforcement of labor regulations in Brazil chases employment to the informal sector. In Mexico, severance pay represents a substantial potential cost to employers and benefit to workers. Unpredictability in enforcement may create a wedge between the expected cost to (risk averse) employers and the expected benefit to (risk averse) employees. While we are not able to say anything about the effect of our experiment on hiring by the firms involved, this literature would lead us to expect that reforms making severance payments more predictable would increase hiring at the margin.

The rest of the paper proceeds as follows. We begin by describing the relevant part of the Mexican labor law and the labor courts charged with enforcing that 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 result. Finally, we discuss the implications of the results and conclude.

2 Labor Courts and Labor Law in Mexico

A single federal labor law governs all labor relationships in the private sector. The majority of cases are assigned to state-level labor courts, with disputes in a few "strategic" industries named in the Mexican constitution (for example, oil and gas, social security, pharmaceuticals and auto manufacture) handled by a federal-level labor court.[5] We use data from the court serving Mexico City. With over 100,000 active cases, this is the largest state labor court in Mexico and Latin America. About 30,000 new cases are filed each year, and in the last 3 years, the court has concluded fewer than 25,000 cases per year. Thus, it faces a large and growing backlog: it would take more than 4 years for it to conclude all of its current cases even if no new cases were filed. Worker dismissal lawsuits make up over 95% of all filings. The MCLC has 20 "special labor courts" with the jurisdiction of each based on the industry in which the defendant firm operates. In the first

[5]Labor courts manage a large number of cases. As of 2016, there were about 225,000 open labor lawsuits, more than all open federal lawsuits in areas of civil, commercial, family, and penal law put together.

phase of the project, we worked with one of the subcourts that deals mainly with the services side of automotive, transport and retail gasoline industries. In phase two, we expanded to four additional subcourts that specialize in industries such as private education and security, restaurants and other retail food business, retail banking, department stores, and medical services.

The labor Law: Mexican labor law is very protective of workers. In the area of firing, the law provides few legal bases for "justified dismissal", so that firing a worker due to low productivity, poor market conditions or redundancy are all considered unfair dismissal and require severance pay. By law, the severance payment is a minimum of three months' wages, including benefits. At-will workers are also entitled to 20 days wages for each year they worked a the firm.[6] Other workers can access this entitlement if they ask for reinstatement in their job, they win the lawsuit, and the firm refuses to reinstate them.

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.[7] 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. Firms most often respond to the suit in one of three ways: denying the existence of a labor relationship (this can be a successful strategy due to high levels of informality as well as a thriving industry of out-sourcing); offering reinstatement; or claiming the worker resigned voluntarily and producing a letter of resignation signed by the employee.[8] In this context, firing lawsuits will be successful only when the worker and her lawyer have access to solid evidence about the employment relationship.

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 a administrative assistant to the judge. Hearings are oral, but are transcribed into the case file. The file is passed on the judge, who writes the final 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 notifiers. 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

[6] 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. The severance provided by the law is more generous as a result.

[7] In practice notification involves substantial corruption, as the lawsuit cannot proceed without it. In ongoing work Kaplan and Sadka (2015) shows that when notifiers' work load is assigned randomly and control of case files is taken away from notifiers, success in notification can be more than doubled.

[8] 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.

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 these 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 public labor prosecutor's office in each state. 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 no more than 50 cases. So while public lawyers are generally well qualified, their incentive is to finish cases quickly in order to reduce their workload. The general perception is that public lawyers do not explain details to their clients or 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. In surveys carried out in a related project, we find that 38% of plaintiffs with 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. In other words, workers - 82% of whom are suing for the first time, according to our survey data - have poor access to information about where to find a good private lawyer. Therefore, they very often opt for the first one that they run into, with little notion of that lawyer's reputation or previous record.

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.[9] 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

[9]Private lawyers may have incentives to convince a worker to sue and not settle even when it is not in her interest to do so. The most common way of doing this is to inflate the claim by adding elements to it, even if in the end they won't stand up in court (e.g. lost wages, vacation pay, end of year bonus, and overtime), and by exaggerating the worker's salary, which is often paid by the firm totally or partially in an informal way.

the effectiveness of our intervention.

3 Data

As described above, the first phase of the experiment was conducted in subcourt 7 of the MCLC. Through an agreement with the court, we collected all of the case files for suits assigned to the court in 2011 and 2012 that were concluded as of December 2015. We also conducted surveys of parties to cases that had an assigned hearing between March 2, 2016 to May 27, 2016. The second phase was carried out in subcourt 7 and four additional subcourts - 2, 9, 11, and 16 - between October 2016 to March 2017.

We use a combination of historical administrative data coded from concluded case files, administrative data from the ongoing case files, and surveys of the parties to the ongoing cases. The case file registers all the legally relevant information in the lawsuit. We describe here in more detail each of the data sources and the variables used. Given the scarcity of evidence on the functioning of courts, we view the construction of this data itself as an important contribution of this paper.

3.1 Administrative data

Historical cases: We begin by digitizing data from historical case files with the goal of building predictive models of case outcomes, as described below. Given the duration of the average lawsuit, we chose the earliest year for which the court had digitized all initial case filings: 2011. For phase 1, we digitize 2,158 lawsuits that were filed in 2011 or 2012, assigned to subcourt 7, and concluded by December 2015. We faced the issue that only 55 of the cases filed in 2011 and 2012 that were concluded before 2016 were concluded by a judge's decision. 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 case files concluded by a judge's decision. These were used in addition to the 2011 and 2012 cases to calibrate the likelihood of winning and amount collected at trial.

For the second phase of the experiment, our aim was to use data from 1,000 concluded cases in each of the five participating subcourts. We used all of the subcourt 7 cases filed in 2011 and concluded by December 2015, and a random sample of approximately 1,000 cases in each of subcourt 2, 9, 11, and 16 with the same two criteria. Thus, the calculator for Phase 2 was calibrated with historical data covering 5005 cases, all filed in 2011 and concluded by December 2015.[10]

Our intention in the first phase of the experiment was to intervene in lawsuits at all stages, including those arriving for their first hearing. Thus, the predictive model could only use infor-

[10]For phase 1, the calculator used the full set 2,158 cases from subcourt 7. We also include the 2012 cases from subcourt 7 for descriptive purposes.

mation included in the initial 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 basic formula for severance payment in the law is in large part a function of the wage, tenure and hours worked. The variables are defined in Table C1 in Appendix C. We also record when the suit ended and how: by settlement, judge's ruling, being dropped by the worker, or by expiry of the right to continue the suit. Finally, we record the amount recovered by the worker at the end of the proceedings. Note that the amount recovered may be 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, given very high costs to enforce payment, the worker may accept a significantly lower payment than the judgment amount, which is equivalent to a post-judgment settlement; and third, the worker may be unable to collect from the firm, since the firm may no longer exist or may have no assets that can be seized by the time the judgment is enforced. The details of the judge's decision are often complex and somewhat opaque and hence difficult to code. We do not record the details of the decision in the dataset.

In addition to providing the raw material for the case 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 that won, 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 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 data from the case file to determine who attended the hearing on the day the parties to the case participated in the experiment, whether the lawsuit ended on that day by being dropped or through a settlement, and any amount of money recorded for the settlement. We then repeated this data coding in November 2016, 8 months after the start of the experiment, to record outcomes for any experimental cases ended by that time. Note that even cases that settle out of court are registered in court files, since this is the only way the firm can ensure that the employee can not sue again for that same cause.

3.2 Survey Data

We collect survey data for the active cases that are included in the experiment. In the first phase of the experiment, we selected a set of hearings conducted between March and May 2016 in subcourt 7 from among those cases where both parties were officially notified of the hearing and were legally obligated to appear at the court. We conducted an 8-10 minute survey with each side

appearing at the court for these hearings.[11] The survey was conducted before parties were aware of their treatment status, and was kept brief so as not to interfere with the hearings themselves. We asked parties their expected probability of winning, and conditional on winning, the most likely amount of the award; the number of months they expected the trial to last if it went to a judge's decision; and for how much they would be willing to settle. When the plaintiff was present, we asked for age, education, gender, feelings about how the firm treated them, how they found their lawyer, and if they already had a new job or were they actively searching for one. Finally, we also included questions intended to test the plaintiff's knowledge of the law and of her own case. These included the amount of severance pay provisioned by law and facts about which claims she was making in her suit. Since we could only survey people who showed up to the hearings, we have many more surveys of lawyers than of plaintiffs and defendants. Among lawyers, 399 representing defendants and 376 representing plaintiffs filled out the main survey. We surveyed 162 workers but only 13 firm representatives. [12]

After the survey the parties were treated according to protocol described in Section 5 below, and then went to the regular hearing if they did not settle. After the hearing, we conducted an exit survey, intended to measure changes in expectations after the treatments. The main purpose for the exit survey was to update expectations on outcomes. This was not relevant for those who settled. Though we kept the exit survey to about two minutes, attrition was an issue even among those who did not settle. None of the main results rely on the survey data, which are used mainly for descriptive purposes. [13]

Table C2 in Appendix C summarizes the data from these surveys. We find that 19.9 percent of employees did not complete middle school. A large proportion (29 percent) of workers who showed up had public lawyers (whereas only 10 percent of the case files in the experiment had a public lawyer). Of those workers who showed and had a private lawyer, most (nearly 82 percent) pay a fraction of the award (30 percent, on average) to their lawyer. Almost three-quarters (73.3 percent) say they are mad or very mad with their employer. Only 7.6 percent of plaintiffs showing up were currently employed, and for those not currently working who were searching for a job, the average reported likelihood of finding a job in the next three months is 58 percent.

[11] We usually surveyed the plaintiff if she was present, and her lawyer if not. As we will see, lawyers for the plaintiff and defendant are almost always there, whereas the plaintiff is only there around 18% of the time, and the defendant only about 1.4% of the time. The lawyers for either side are legally authorized to make decisions on behalf of their clients.

[12] Note that when both the lawyer and client were present, we chose to survey only one of them, in the interest of time. At least one party completed the baseline survey in 71 percent of the cases. Survey compliance rates are detailed in Table C5 in Appendix C.

[13] At least one party completed the exit survey only 54 percent of the time.

3.3 Construction of the calculator

The experimental design called for providing to a subset of plaintiffs and defendants personalized predictions on important case outcomes based on characteristics of their lawsuit. 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 if the calculator is described in some detail in Appendix A, but we summarize the main points here. The main continuous outcome variables we wanted to predict were amount received by the plaintiff conditional on receiving a positive amount of compensation, and the duration of the case. The main discrete variables were case outcome, which included settling the case, dropping it, expiry of the right to continue the lawsuit due to inactivity in the proceedings, judgment with zero recovery, and judgment with positive recovery. The main explanatory variables we used were: gender, hours worked per week, tenure at the firm, salary, type of lawyer, whether or not the worker was registered with Social Security, if he/she is 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. For each outcome, we chose the model and variables that had the best fit on the verification sample, measured by the correlation between predicted and actual values. Tables A2 & A1 in Appendix A present goodness of fit measures for all these models and highlight the models we selected in each case.

The models allow us to produce individualized predictions that we shared with parties in cases randomized into the calculator treatment. Figure 5 displays the template we used in phase 1. The template shows the compensation provided by law and the probability the case will end in each of five possible endings. For each of these five endings, we showed the expected amount recovered by the plaintiff. We then aggregated these data to produce the overall expected payout across all endings. We also provided the percentage of cases that were still unresolved after three years, and the minimum legal entitlement based on the law if the plaintiff were to win on the issue of unfair dismissal. In phase 1, we provided the exact same information to both sides of the case. We adjusted the format for phase two, first to simplify the information so that it could be explained to parties more quickly, and second to address concerns raised by the conciliators. In particular, the conciliators requested that we drop the information on minimum legal entitlement, and that we develop separate templates for the plaintiffs on the one hand and defendants on the other. These are shown in Figure 6. The conciliators suggested that we provide the expected settlement amount, conditional on characteristics, and then provide each side with data indicating the contingency they faced in case they failed to settle. For the worker, this 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 potential sources of bias in the predictions based on our historical data. One is that our sample is composed of cases that have concluded, and 29% of cases filed in 2011 and 2012 were still ongoing when we estimated the models on which the calculator is based. 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 cases. Note that if cases end in settlement, they almost always do so within the first 24 months of the filing. Since our data cover more than 24 months after filing, this implies that almost none of the unconcluded cases would end in settlement. Therefore, the projecting average payment for cases ending in settlement is not affected by this censoring issue. The amount received in settlement is the most important variable in the calculator information.

For cases dropped by the plaintiff, and those ending in ending judgment or expiry, 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 A1 we show that the two sets of cases are similar on observables.[14] 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 (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 A2 reports the results. There are few differences.

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

4 Outcomes and Expectations: Stylized facts

We use the administrative and survey data from the first phase to document a set of stylized facts about the court. These serve as a motivation for the experiment we implement, but may also be interesting in their own right as a picture of the functioning of the court. We note whether 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% of the amount claimed on average, and 50% of what the law mandates.*

[14] An exception is that the experimental cases have a higher rate of claiming reinstatement. We believe 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.

Figure 1 uses the sample of concluded cases to show the amounts claimed and paid for the 4 main outcomes: settlement, drop, judgment, and expiry. First, note that 63 percent of cases end by settlement, 20 percent are dropped, 8 percent end because the time limit for the case runs out, and 9 percent end with a judge's decision. For each outcome, the first bar shows the average amount of money claimed by the plaintiff. The second and third bars show two estimates of the minimum compensation by law based on the details of the cases. The first estimate includes 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 second estimate includes these same items and, in addition, back pay, which by law should be paid in full from the date of firing to the date the lawsuit ends. The fourth 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. The average amount received in judgments is slightly higher than the average received in settlements, but in either case, the amount received is a small percentage of the amount claimed. Note that while every single settlement results in a positive compensation to the worker, in cases ending with a judgment the worker recovers a positive amount only 24 percent of the time.

In a court judgment in which the worker recovers a positive payment, she receives on average 37.5% of her claim; however, 76% of judgments have zero payments to workers. This means that in expected value terms, the worker recovers only 8% of her claim in a court judgment.[15] Figure 1 shows that the amount plaintiffs receive in judgments is less than the minimum compensation according to the law. Among plaintiffs whose cases continue to a court judgment, the 24% who recover a positive amount receive on average 126,664 pesos, about 170 percent of the minimum legal compensation for their case without back pay, but only 49.9% percent of the minimum compensation including back pay.

In addition to low recoveries, a significant share of the cases filed through private lawyers have negative (discounted) payoffs. Private lawyers typically charge a fee of around 2000 MXP (USD 100) to file a case at the labor court. They also receive 30% of any amount collected by the plaintiff. Figure 2 shows realized recoveries from our 5,000 historical casefiles, net of filing and contingency fees. After subtracting filing and contingency fees, around 40% 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 dropped or expire, and hence have zero recovery, but around 5 percent of the settlements are also for amounts that imply a negative net present value for the

[15]For the overall statistics quoted in Fact 1 above, we do not include back pay in the claim since these are never negotiated in a settlement, and settlements are the majority of case terminations. In court judgments, however, we include back pay in the claim, since 98% of case filings demand back pay, and the law mandates that in court judgments they should be granted. In practice, however, back pay are rarely recovered by the worker. In fact, the imputed back pay exceeds the total amount recovered in the majority cases ending in judgment with positive recovery.

plaintiff.

Fact 2. Long suit duration (Historical Data): *30% of trials started in 2011 had not finished by December 2015; among those that had concluded in a judgment, the average duration was 2.5 years. But even conditional on reaching a settlement, the average duration is almost 1 year.*

Figure 3 shows the distribution of case length by type of trial ending. Even cases that settle take a long time to do so, with an average time between filing and settlement of 10 months. Trials ending in a court judgment take 2.4 years on average even conditional on being concluded in December 2015. Given that 30 percent of cases filed in 2011 were still open in 2016, and many of those are likely to end in a judgment, the unconditional average will be higher. Moreover, settlement rates are low by international comparison. In sub-court 7 of the MCLC, only 63 percent of cases are settled over the lifetime of a lawsuit. 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.[16] The low settlement rate occurs in spite of a labor law that mandates a conciliation hearing at the beginning of each lawsuit.[17]

These long delays and low settlement rates help to explain the large backlog of cases in the court. Even if there were no further incoming cases, the Mexico City Labor Court would take about four years to conclude the current stock of cases. Delay has direct costs in the form of court staff time, lawyer fees and the opportunity cost of litigants time. If, as is likely the case, the plaintiffs discount the future at a higher rate than the defendants, then delays also lead to decay in the collective value of any payments. Most of these costs represent pure efficiency losses, which is one of the reasons why the MCLC is very interested in barriers to settlement.[18]

Fact 3. Misinformation (Survey data): *Only one-third of plaintiffs (employees) understand what their main legal entitlement is. 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 4 indicates that only 27% of plaintiffs responding to the survey know the number of days covered by this entitlement.[19] Knowledge of the entitlement to severance pay is increasing in the education level of the plaintiff. 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

[16] See Ebisui and Fenwick (2016).

[17] Article 876 of the Federal Labor Law mandates that each labor court promote settlement between the parties during the entire process, through court officials, including the court's conciliators. But implementation is lacking.

[18] Interestingly, concurrent with our work, Mexico has passed a constitutional reform of labor law creating separate conciliation bodies that are to oversee a compulsory conciliation process before lawsuits can be filed.

[19] This is from an answer to our following survey question: *"In case of unfair dismissal the law entitles you to a constitutional severance pay, this represents _____ days of salary"*. They have to fill in the blank.

you are asking for in your suit among the following...".[20] We assess accuracy by comparing the responses to the case file. Panels (c) to (f) of Figure 4 shows the proportion of time the plaintiffs responded correctly to questions regarding elements of their claim. We see that between 20 percent and 50 percent of respondents answered each element incorrectly. Again, knowledge of the case increases somewhat in the level of education.

Fact 4. Inflated expectations (Survey data): *The subjective probabilities of winning for plaintiffs and defendants (in the same cases) sum to 1.47*[21]*, 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.[22] 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 phase 1, the average expected probability of winning for workers in is 0.79, while for firm lawyers it is 0.68. These probabilities sum to 1.47. Workers in the phase 2 cases are equally optimistic, with an average probability of winning of 0.80, but the firms' lawyers are somewhat less optimistic, expressing a probability of winning of 0.40. The sum of the probabilities far exceeds 1 in both phases, being 1.47 in phase 1 and 1.20 in phase 2. For comparison, the probability of the worker winning predicted by our calculator in the same cases is 41% in phase 1 and 33% in phase 2. There are also large differences in the expected amount of the award in case of the worker wins. Both the worker and her lawyer estimate average amounts more than twice those of defendants. We can build a proxy of overconfidence as the difference between the subjective expectation and the calculator's prediction: $\gamma(X_i, u_i) \equiv E^s[y_i|X_i, u_i] - \widehat{y(X_i)}$.[23] Figure C2 plots the distribution of $\gamma(X_i, u_i)$ for different parties for peso amounts conditional on winning and probabilities of winning. The distribution is centered above zero for expectations, displaying average overconfidence.

Table 2 uses regressions to compare expectations across parties to the lawsuit. We regress the measures of expectations against dummy variables indicating the firm's lawyer and the employee's lawyer, and a constant that captures the plaintiff's mean. We report these regressions as summary statistics and do not place any causal interpretation on the estimates. The regressions

[20] Constitutional payment, reinstatement, overtime, holiday bonus, Sunday bonus, and insurance.

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

[22] (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.

[23] If unobservables for the case u_i are important in any given case, the calculator prediction will be biased for that case. But under the assumption that the unobservables are, on average, the same in the new experimental cases and the cases used to estimate the calculator, the calculator's prediction will be an unbiased estimate of the *average* outcome, and if the individual subjective expectations are unbiased, then the calculator's predictions should on average coincide with the subjective expectation.

use data from phase 1 and phase 2 combined, and include casefile fixed effects.[24] The first two columns show the raw expectations of the probability of winning and the amount parties expect to collect (pay) conditional on winning. The constant should be interpreted as the plaintiff's probability of winning the case, while the coefficients for the two lawyers can be interpreted as each party's expected outcome relative to the plaintiff's expected outcome. The results in column 1 confirm the findings in the raw data shown above: The plaintiff expects to win 74% of the time, and the firm's lawyer 51% of the time, expectations that sum to 125%. The second column shows that the expectations of the parties are also inconsistent with regard to payment amounts: plaintiffs expect to receive almost MXN$76,000 while the firm's lawyer expects to pay MXN$46,000.

In Columns 3 and 4, we examine the expectations relative to predictions from the calculator prediction for the case. We construct the dependent variable by dividing the expectations gap, $\gamma(X_i, u_i)$, by the predicted probability, $\widehat{y(X_i)}$. We refer to the resulting value as the "relative OC". Results in Column 3 show the worker is also overconfident of the likelihood of winning the case, with an average predicted win percentage 1.8 times that of the calculator. Firm lawyers, on the other hand have expectations of winning that are actually slightly less (0.90) than those predicted by the calculator. Finally, in column 4, we see that workers expect to win 75% more than the amount predicted by the calculator, while the firm's lawyer expects to pay only 20% less that the calculator predicts. For all four regressions, note that the employee's lawyers is insignificantly more optimistic than the employee herself.

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. The average plaintiff therefore recovers much less with a private lawyer.*

Private lawyers charge a fee of 2000 MXP (100 USD) to file a case. Given the ability to copy and paste filing documents, the marginal cost of the filing is much lower than the fee received. This gives private lawyers an incentive to inflate claims in order to convince workers to file a suit. We find that, conditioning on five variables coded from the initial filing[25], 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% lower for private lawyers. The result is that the average recovery is insignificantly lower (by 0.5%) for private lawyers. We verify that this is the case in Table 3.

While the amount recovered is the same for public and private lawyers, the recovery is split between the plaintiff and the lawyer in cases filed by private lawyers, while all of the recovery goes to the plaintiff when she uses a public lawyer. This implies that plaintiffs receive much larger amounts with public lawyers than with private lawyers, conditioning on the characteristics. Of

[24]Since we don't have expectations for all the parties for all the cases, the fixed effects results are preferable as they make comparisons across parties for the same set of cases. However, the results are very similar without the fixed effects.

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

course, the selection of lawyers is endogenous, and the data we report here is only descriptive, making no attempt to adjust for this selection.

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: Can the provision of personalized statistical predictions increase settlement rates? Additionally, we also ask whether requiring parties to meet with conciliators affects settlement rates. Understanding the effect conciliators is important because a recent constitutional reform mandates a compulsory conciliation process before a lawsuit can be filed.

5.1 The treatments

The experiment compares the effects or two treatments - the provision of statistical predictions of case outcomes and the use of court-employed conciliators - against a control group. As detailed below, in the first phase we work with hearings scheduled in subcourt 7. During the experimental window, hearings for which both parties were formally notified were assigned to one of two treatment arms or a control group. 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 Hawthorne effects are not driving any outcomes.

The Calculator: Subjects in the calculator arm received the personalized statistical 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 Figures 5 and 6. We extracted the data for the calculator from the initial filing, and the data were typed into a user interface in the presence of the parties. The calculator was then printed and given to all of the parties present at the hearing. 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. The enumerators gave no additional legal advice, but asked the parties if, after receiving the calculator information, they wanted to delay the start of their hearing for a few minutes to negotiate with the assistance of a conciliator. In phase two, because there was less time available for the treatment, the information was pre-filled to save time in administering the treatment.

Conciliators: The conciliator treatment was implemented only in the first phase of the experiment, and thus only in subcourt 7. In the normal course of operations, the court assigns one conciliator to each subcourt. During the first phase of our experiment, the court assigned an addi-

tional conciliator to subcourt 7.[26] Cases assigned to the conciliator arm (and any other cases that wished to do so) used either of the conciliators working in subcourt 7 during the experiment. Both of the conciliators assigned to subcourt 7 during the experiment had at least 5 years of experience as a conciliator. The conciliators had a desk where parties sat to negotiate. If a casefile was assigned to the conciliator treatment, project research assistants requested parties to go to one of the conciliator tables for a discussion before the hearing started. Actually negotiating with the conciliator was optional for the parties. They could stop the discussion at anytime they wished. The conciliators rely on soft-skills to aid settlement based on a pre-specified protocol. They explain the key aspects of the conciliation process and the rights of each party. The conciliator had no access to the calculator. After an introduction of about five minutes, conciliators continued talking to parties if a real negotiation between them started to take place.

Placebo: 13 months after the end of the calculator/conciliator treatments in phase one of the project, we implemented an additional treatment arm in subcourt 7. We were concerned that the presence of project research assistants and the surveys might change behavior of the parties. We therefore implemented a "placebo" treatment in which we provided only a leaflet that provided information about the role of conciliators in the process. (The leaflet is shown in the Appendix C Figure C5). The leaflet was provided in format similar to the calculator information, but rather than quantitative predictions it said: "Do you know that you could fix this conflict today? Subcourt 7 has conciliators that can help you! Settlement advice is fast, free, confidential and impartial". As in the calculator treatment, if a subject asked for the conciliators our enumerators told them where they were situated.

5.2 Implementation

The implementation of the experiment differed slightly in phase 1 and phase 2. The most important difference is that randomization was at the case level in the first phase and at the day level in the second phase. This change was made for logistical reasons, given that during the second phase we were working with a larger number of the subcourts.

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. Sub-court 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 since the

[26]The additional conciliator appears sufficient to relieve any capacity constraints generated by the additional conciliation engendered by our treatments, but we return to the issue of possible spillovers when we discuss the results below.

court needed to start the day with all hearing counters full to avoid delays in the schedule. Hence, whenever counters were not filled at 9 AM with cases not meeting our notification criterion, we randomly selected fully notified cases to fill the counters and erased those from our list of treatments for that day. 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. Note that different suits are in different stages of the process (i.e. not all are new suits).

After receiving the list of cases for the following day, we randomized the eligible cases in equal proportions to the two treatment groups and a pure control group. Control cases followed business as usual, except for the surveys we administered. Each morning we set up a survey table, a calculator module, and conciliator desk in the waiting area just outside the hearings counters. The hearings were displayed on a screen and parties were called up by the sub-court judge's assistants. Except for the 9 am hearing slot, most hearings were somewhat delayed, and we carried out surveys and treatments during parties' waiting time.

Table C4 shows details of the treatments. We began by administering the baseline survey. The survey was conducted blind to the experimental conditions for both the parties and our enumerators. All the parties present were asked to complete the survey, but compliance was optional and the completion rate is about 75% at the case level. Those completing the survey were told that they would be asked to complete a followup survey after their hearing[27] and were informed they would receive a prize if they did. The experimental treatment was revealed after the baseline survey, and parties were channeled to their appropriate experimental condition. Those in the calculator group were given their personalized calculator results as described above. Research assistants explained the information and then asked parties if they wanted to conciliate. If the answer was in the negative the person (plaintiff or defendant) went to her hearing. If the answer was positive for both plaintiff and defendant, they were sent to the conciliation table to agree on the terms and sign a settlement document. If they were assigned to the conciliator group they were asked to talk to the conciliator before their hearing.

Phase 2 of the experiment involved only the calculator treatment and, as we noted above, treatment was randomized at the day level rather than case level. First hearings are scheduled on Fridays, and hence we randomize across Fridays in each of the 5 subcourts during the experimental window. Otherwise, the protocol was changed only slightly from phase 1. To save time, we shortened the survey and we pre-filled and pre-printed the calculator. The sub-courts did not agree to allow us to delay the hearings, so if after receiving the calculator the parties wanted to negotiate with the help of a conciliator, they themselves had to request a delay in the hearing.

For convenience, the placebo treatment was randomized at the week level, two weeks had the placebo treatment and two adjacent weeks a control group where we did not intervene at all. For both groups we coded the variables in the case file and recorded whether there was a settlement

[27]We did not implement the exit survey if the parties settled during the hearing because the main topic was the elicitation of beliefs about the outcomes of continuing the suit, which was not relevant for them.

on the day of the hearing.

5.3 Integrity of the experiment

In phase 1 of the experiment, we assigned 351 case files to the calculator treatment, 360 to the conciliator treatment, and 365 to the control. Table C5 shows compliance rates by treatment assignment for the treatment and the surveys. Table C6 in the Appendix C shows that the variables are balanced across the experimental groups: only 3 out of 23 tests are significant at the 10 percent level. For phase 1, randomization was done within day, but we avoided contamination by separating the survey tables from the treatment tables. This was doable because our sample was only about 18 cases per day. Compliance rates were very similar in the second phase of the experiment. We use the intention to treat in all reported results.

6 Results

Theory has shown that asymmetric information between two bargaining parties can generate rational delay as a screening or as a signaling device, and that overconfident expectations can lead to delay even without asymmetric information. Our interventions aim to increase information and reduce overconfidence, so that we can observe the resulting effects on settlement. The experiment also measures the effect of requiring parties to use person-to-person advice provided by professional conciliators, as a recent constitutional reform in Mexico proposes to do in the future.

6.1 Effects on settlement

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

$$y_{it} = \alpha_t + \sum_{j=1,2} \beta_{tj} T_{ij} + \epsilon_{it} \tag{1}$$

The constant α_t estimates the mean for the control group, while T_j indicates assignment to the calculator and conciliator (in phase one only) treatment arms. Thus, β_t estimates the intent-to-treat (ITT) effect at a given point in time t. A separate regression is estimated for each t, with t indicating the day of the hearing, or two or five-to-seven months after treatment. 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 calculator or conciliator, while controlling for EP itself. In phase 2, we add subcourt fixed effects.[28]

[28]We concentrate on plaintiff side interactions, since there is little variation in the defendant side. The defendant himself was present in just over 1% of hearings, while the employee is present 18% of the time.

The first four columns of Table 4 focus on same-day conciliation, i.e. the dependent variable is a dummy for whether there was a settlement on the day of the intervention, a very short term outcome. The first two columns use data from phase 1 of the experiment and columns 3 and 4 use data from the second phase of the experiment. Column 1 shows that about 6% of the control cases in phase 1 settle, while the settlement rates in each of the two treatments are approximately 5 percentage points higher, a near doubling of the settlement rate. The calculator effect is significant at the five percent level and the conciliator effect at the one percent level. We cannot reject that the calculator and conciliator have the same effect (p-value=0.88). Thus, in terms of settlement, providing statistical information has a similar effect to the alternative of face-to-face conciliation.

Column 3 shows that in the second phase of the project, 11 percent of the cases settled on the day of the hearing. Recall that the second phase was conducted with cases holding their first hearing, so the higher settlement rate likely reflects this fact. However, the effect of the calculator treatment is quite similar to that in the first phase: settlement rates on the day increase by 4.5 percentage points in the treatment group compared with control.

Columns 2 and 4 show our second main result: the treatment effect occurs *only* when the employee is present. In these regressions, we interact a variable indicating the plaintiff herself was present with each of the two treatments. We also include a variable indicating that the plaintiff was present. First, note that in both phase one (column 2) and phase two (column 4), settlement on the day is much more likely when the employee is present. In the control group, 17 percent of the phase one cases and 24 percent of the phase two cases are settled on the day of the intervention when the employee is present. But the treatment effects are also much larger when the employee is present. Settlement rates are 16 percentage point higher with either treatment when the employee is present in phase one, both effects significant at the 5 percent level. The measured effect of the employee present - treatment interaction is as large in the second phase, 16 percentage points, but the effect is significant only at the 10 percent level. Moreover, there is no effect of the calculator treatment in either phase of the project when the employee is not present. The level effects of treatment are close to zero and highly insignificant once the interaction is added to the regression.[29] 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. But 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 the effect of the calculator treatment when the employee is present now significant at the 1 percent level, and the effect of the calculator when the employee is not present remaining a fairly precisely-estimated zero.

[29] Table C7 in Appendix C examines balance in key variables in the subsample of cases where the employee is present. Table C13 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 less likely to attend where they had a long tenure at the firm.

The treatments induce parties to use the conciliators. For phase 1 of the experiment, where randomization is at the case level, we might be concerned that in doing so, the treated cases use up the available conciliation capacity and crowd out the control group cases, in violation of the SUTVA assumptions. Given that the court assigned an additional conciliator to subcourt 7 during the experiment, that conciliators typically only talk with parties for about five minutes unless they are serious about settling, that the percentage of cases that settle is relatively low, and that the subcourt judge can also act as a conciliator if necessary, we do not expect tat spillovers should be an issue. But we check this by regressing a dummy for settlement in the control group against the number of treated cases in the same half-hour hearing slot. Indeed, we find no significant spillover effect.[30]

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 further, delayed, effect on settlement? The court's administrative records allow us to track cases over time. Column 6 shows the effect of treatment two months after the treatment hearing. In a typical dismissal case at the MCLC, hearings are scheduled about three months apart. The two-month window, then, occurs prior to the next hearing. Column 7 shows the effect of treatment measured in November 2016, five to seven months after the treatment hearing, after at least one additional hearing in most cases.

Our third main result is that the effect of treatment does not change materially during the six months after the intervention. Focusing first on cases where the employee was not present at the hearing, comparing column 5 with column 6 or 7, we see that in the control group, the settlement rate increases from 9.5 percent to 15 percent two months later and to 39 percent six months later. But the effect of the calculator when the employee was not present (row 2) remains zero after either two or six months. Where the employee was present to receive the treatment, the treatment effect remains unchanged over time. The 16 percentage point effect on the day of the hearing increases (insignificantly) to 18 percentage points after two months and then returns to 16 percentage points after six months. Not that the effect of the employee being present on the day by itself (row 4) falls from 14 to 8.9 percentage points, with the difference between these has a p-value of 0.11. In Appendix C Table C11, we show results separately for clients with private and public lawyers. The calculator - employee present is significant only in the private lawyer subsample.[31] Moreover, the effect of the employee being present falls from 11 percentage points on the day of the hearing to 4 percentage points six months later, the drop being significant at the .10 level. These results suggest that, while the employee being present on the day of the hearing is clearly potentially endogenous to settlement, her presence does not appear to affect settlement

[30] Results available from the authors on request.

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

rate in the longer term, even though the treatment does retain effects in the longer run.

Taken together, the results imply that, where the employee was not present at the time of our intervention, 29.5 percent of cases settled after the day of the intervention but within six months (comparing the top lines of columns 5 and 7). But the increase in the settlement rate was *the same* in the calculator and control group (comparing the second row of columns 5 and 7). And the effect of the employee being present at the treatment hearing diminishes over time. The lack of any future treatment effect in the face of substantial increases in settlement rates among the untreated suggests that the lawyers do not share information from the calculator with clients. On the other hand, where the employee *was* present on the day of the intervention, the effect of treatment does not change over time. That is, there is no strong evidence that the interventions simply sped up settlements that would have happened soon in any case. We read these results as indicating that plaintiff-lawyer agency issues are important in this context. Lawyers appear not to transmit evidence to plaintiffs who are not present to receive it directly. Of course, this might reflect the difficulty lawyers have in explaining the calculator to the plaintiffs. But even in that case, the lack of any effect when the plaintiff is not present indicates agency issues in that the plaintiff does not trust her lawyer to make decisions on her behalf.[32]

A final result, shown in Appendix C Table C10 is that the placebo has no effect on conciliation. The placebo provides parties with information about the conciliation process, but 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 Effects on overconfidence

Our calculator treatment provides information on likely outcomes of the case, while the conciliator treatment induces parties to talk with an expert with extensive in conciliating labor cases. Either of these treatments might make their expectations of outcomes more realistic. Ideally, we would be able to measures the effect of treatment on overconfidence of parties that were initially overconfident, to isolate the overconfidence channel. However, we faced operational limitations in being able to measure the impact on expectations. First only 266 workers showed at the hearing. We were able to administer the baseline survey to 162 of those workers, 53 of whom were overoptimistic. But in the exit survey, the expectations questions were relevant only for 133 workers who did not settle at the hearing. Of these, we were able to re-survey 94 after the hearing. This leads to both a power and a selection issue: we might expect those most affected by the calculator to be the most likely to settle. So while re present results on updating of expectations, given these issues we view the the evidence as only suggestive.

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

To measure changes in expectations we use the proportional difference between expectations at baseline and expectations at the exit survey, relative to initial expectation: $(\frac{exitsurvey-initialsurvey}{initialsurvey})$. We refer to this as "relative updating".[33] Table C12 regresses relative updating against the treatment arms, focusing on the sample of those who were overconfident at baseline.[34] We find weak evidence of de-biasing among employees, the group we would expect to be most affected given their lack of experience with the court. Column 1 shows about a 30% (40%) decrease in overconfidence from the calculator (conciliator), significant at the 10% level. As noted, given the small and selected sample, we view these effects as only indicative but not conclusive.

7 Is the Increased Settlement Rate Beneficial?

The increased settlement rate generated by both treatments helps the court meet its goals of reducing the case backlog. But we should also be concerned about the effects on the parties. There are four parties involved in the case: the plaintiff, the defendant, and the lawyers on either side. The parties are likely to have different ability to diversify risks and different discount rates. Arguably, it is most important to understand the effects on the plaintiffs, who are the most vulnerable[35], uninformed, and almost always the only party using the court for the first time. We therefore focus on the plaintiffs in our empirical analysis.

To aid in interpretation we write down a simple model which clarifies how the differences in the primitives of the parties (in the ability to bear risk for instance, or in time preference) might lead to inefficiently low settlement rates, given that the lawyer's influence on the decision to settle. A necessary condition for the increase in settlements to be beneficial is that the untreated settlement rate is inefficiently low. Our framework explains why this might be the case, and helps us interpret the results of the experiment from the perspective of the welfare of the parties.

After going through this framework, we examine the effect of the increased settlements on plaintiffs empirically, focusing on the amount they receive. First we compare settlement amounts in the treatment and control groups. Then we compare the amounts received by the plaintiff in our treatment arms to what they would have received if they had gone to a court judgment, using matched cases from the historical files that proceeded to judgment as a counterfactual. While the model highlights reasons that the plaintiff's incentives may be misaligned with those of her lawyer, the empirical exercise shows that under assumptions we believe are reasonable, the plaintiffs are made better off, on average, by the increase in settlement rates.

[33] We use data only from phase 1 of the experiment because, for logistical reasons, we were not able to implement the expectations questions in the second phase.

[34] We focus on the overconfident sample both because they are the majority of the sample and because theta has a simpler interpretation if we know the sign of the numerator.

[35] 60 percent of plaintiffs say the do not have enough money to take care of their family.

7.1 A framework for Understanding Incentives

Our main empirical findings so far are: (1) workers and firms have biased expectations on average; (2) a fraction of filed cases are settled and this fraction increases when the calculator is provided; (3) but only when the employee is present. These facts could be explained by several models. Our aim here is to write down a simple framework that can rationalize them, without claiming that this is the only way to do so. We focus on the plaintiff and her lawyer, and combine the firm and its lawyer as a single party. Given repeated interactions between the firms and their lawyers, we believe it is reasonable to assume that their objectives are more closely aligned. In any case, we have almost no variation in employee presence in our data.

The framework is stylized and used only to exemplify some of the mechanisms. We start with a utility function defined over the amount awarded to plaintiffs for the plaintiff (w), the plaintiff's lawyer (l), and the firms (f) $(U_w(x), U_l(x), U_f(x))$[36], and subjective distributions $g_k(x)$ for $k = \{w, l, f\}$ for the amount that party k gets 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.[37]

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 willing to settle than her lawyer.*

Claim 1 is that even with the same subjective $g(x)$ for all parties, if plaintiffs are more risk averse than defendants[38] 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

[36] We will assume that $U(0) = 0$, $U'(x) > 0$ and $U''(x) \leq 0$, so that U is risk averse or risk neutral. Lawyers typically get about 30% of the winnings, we can think of this as being already reflected in their utility function.

[37] The proofs are straightforward, and shown in Appendix B.

[38] 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.

will want to reject. The difference in the circumstances governing risk creates a potential conflict of interest between the plaintiff and her own lawyer.

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

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^o_w first-order stochastically dominates g_w) then the settlement range shrinks. Indeed, if the parties are sufficiently optimistic, the settlement range will be empty. 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 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.*[39] *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.*

Claim 3 introduces the calculator by modelling it 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.

Thus claims 1-3 together say that the incentives of plaintiffs and their lawyers may diverge, that this divergence provides an incentive for the lawyer to inflate the expectations of the plaintiffs, and that the calculator (and conciliator treatment) works by reducing the degree of overconfidence, particularly in the plaintiffs, who are the most naive. Together, these explain why the lawyer may not have the incentive to show the calculator information to the plaintiff, and hence provide a rationale for the calculator treatment being effective only if the plaintiff is present to receive the information directly. These claims, and the assumptions on which they are based, are also sufficient to produce a settlement rate which is sub-optimally low from the perspective of the plaintiff and the firm.

We have focused on differences in risk aversion because we believe the difference between parties is clearest with regard to risk. Lawyers are able to diversify risk across a large number of cases, while plaintiffs are affected by the outcome of a single case. But the parties may also differ in other respects. Notably, they may differ in how much they discount the future. Because plaintiffs will have recently lost a job and will generally have lower incomes even when working, we might expect them to be more impatient than either of the lawyers, or indeed, in most cases, the defendant. For any expected payment made at the point of the judge's decision several years later, the higher discount rate would lead the plaintiff to accept a lower settlement today than her

[39] We say that the agent is Bayesian if he follow a *Bayesian rule* according to definition in *Shmaya & Yariv. Foundations for Bayesian Updating.*

lawyer would be willing to accept. The differences in either the ability to diversify risk or the rate of discount would lead the plaintiff to have a larger settlement range than her lawyer, creating conflicting incentives. The lawyer may manage these incentives by inflating the plaintiff's beliefs over outcomes in the case.[40] Again, our objective here is not to be exhaustive in the modelling, but to show that there are reasons that the incentives of the plaintiff and her lawyer are plausibly misaligned, and that the misalignment results in too few settlements from the perspective of the plaintiff.

7.2 Empirical Evidence on the Induced Settlements and Plaintiff Welfare

In a simple model with rational agents, access to free and truthful statistical information does not harm the parties receiving the information. Our framework re-enforces this by showing that under reasonable assumptions, the rate of settlement is suboptimally low from the perspective of the plaintiffs.[41] Moreover, our framework shows only that the plaintiffs *could* be better off with more settlement, not that they *are* made better off with the additional settlements our treatments induce. So, ideally we would also show empirical evidence that the welfare of the plaintiffs increases with the additional settlements.

With this in mind, we carry out two empirical exercises. The first estimates differences in settlement amounts between the control and treatment arms, conditional on having settled. This is, of course, subject to a selection problem since we know that our treatment had an extensive margin effect, although the cases that settled across the treatment and control arms are similar on observables. (See Table C8 in Appendix.) Panel (a) of Table 5 shows the results of a regression using the sample of cases that settled on the day with the following specification:

$$S_{it} = \alpha_t + \sum_{j=1,2} \beta_{tj} T_{ij} + \epsilon_{it} \tag{2}$$

where $S_i t$ is the settlement amount in pesos and $\beta_t j$ are dummies for the calculator and conciliator treatments. The first three columns of Table 5 show results from the first phase of the experiment and the last three results from the second phase. We see no statistically significant differences between the settlement amounts in the treatment and control groups in either phase of the experiment, though the measured effects are negative in each specification.[42] However, the coefficients are economically small in all of the regressions (less than 1/30 of the legal entitlement, and an order of magnitude less than what the control group receives in the main experiment). The

[40] This could be offset by differences in the opportunity cost of time of the lawyer. But since a lawyer typically manages many cases in the same court building, the opportunity cost of time spent on a given case is arguably modest.

[41] In a context with both agency and differentially naive parties, it may be possible to write down a model showing that some parties are made worse off by the provision of information.

[42] Table C8 in Appendix C shows differences in characteristics of cases that settle in the control vs. treatment groups. The treatment cases that settle on the day are statistically similar to the control cases except that plaintiffs in the calculator treatment group who settle have slightly higher daily wage wages.

estimates change little when controlling for the case's basic characteristics (columns 2 and 5) or for either the calculator's estimate of the amount that would be collected after judgment or the amount of the legal entitlement (columns 3 and 6).

The second exercise estimates what the settled cases in the two treatment groups would have collected had they continued to a court judgment. We re-iterate that the judgment amounts are not what the court ruled should be paid, but what plaintiffs were actually able to collect, which is the relevant measure given that settlements are always paid. This counter-factual amount is, of course, never observed in actual data. We construct the counter-factual with a matching exercise using cases with similar measured characteristics in the historical data that ended in a court judgment.[43] Panel B shows the result of a regression on the settlement amount using the cases in the treatment sample that conciliated together with matched samples constructed with nearest-neighbor matching. The matching is done in the basic variables (described in the notes on Table 5) including the calculator prediction for the case, and the dependent variable is the present value of the amount recovered, discounted at the rate reported by the median plaintiff in our surveys (50 % annually).[44] The comparison ignores the added uncertainty inherent in proceeding to judgment, so with risk-averse plaintiffs, this again works against the more certain settlements generated by the interventions. Using our complete sample of judgments, we find that the settlement amounts in the calculator treatment are significantly larger than the average discounted settlement amount. The effect of the timing of the settlement is crucial in this exercise, given the long delay before judgment and the high discount rates of plaintiffs. However, the judgments are characterized by a very long right-hand tail, with 15 of the matched cases collecting more than 250,000 pesos. There may be characteristics of cases unobservable to us but observable to plaintiffs that characterize the cases with high-value awards. If those with stronger cases are less likely to settle, then a more appropriate counterfactual would remove the largest judgments. With this in mind, we show results with outcomes winzorised at the 99th percentiles (Column 3 of Panel B). Here we find that the calculator settlements leave plaintiffs significantly better off. In sum, at worst we find that the calculator treatment leaves the plaintiffs no worse off. If the additional settlements come from cases with relatively weak unobservable characteristics, then they may be significantly better off.

8 Conclusion

Data from case filings of participants in the Mexico City Labor Court show that worker dismissal lawsuits have long duration even though many have very low values. Around 40% of plaintiffs

[43] This imputation is subject to bias to selection on unobservables. We note that the observables of cases that settle in the control group are similar to the obseverables of cases that settle in the treatment groups.

[44] Figure C3 in Appendix C shows how the results in Column 1 of Panel B are affected by the choice of discount rate. A discount rate of around 10%, some 20 points lower than credit card rates, is sufficient to generate positive measured gains. Figure C4 in Appendix C shows the discount rate data elicited from surveys, along with comparable data from the Mexican Family Life Survey for 2009.

pay more in legal fees than they recover from the case. At the same time, survey data indicate that parties are overly-optimistic and workers are ill-informed about both the relevant law and their own cases. Though our data are unusually detailed, the patterns they reveal are widely viewed as common across courts more generally in low- and middle-income countries.

Results from our randomized experiment show that providing simple information on expected outcomes of the case nearly doubles the rate of settlement on the day of the treatment. The treatment effect is evident only when the worker is present. Both the treatment effect and the importance of the employee's presence are persistent over the six months following the treatment. These results have important policy implications for bargaining impasses that happen in many contexts. The calculator is easily scaleable to dispute settlement situations with a large volume of cases similar enough to allow for accurate prediction of expected outcomes. The provision of information is likely to be particularly relevant in contexts where at least one side to the dispute is not a repeat player, and hence is likely to be misinformed, such as divorce or civil cases.

The experiment provides a window on the functioning of the court as an institution. 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. The literature on bargaining and settlements has focused on the relationship between the plaintiff and defendant, and de-emphasized the importance of agency issue between either party and their lawyer. The results of our experiment suggest the need to merge the insights of the bargaining literature with those from the literature on expert agents. 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.

One story that weaves together our results contains five elements reflecting plaintiff-lawyer agency issues: First, employees are misinformed about their entitlements and the importance of particular types of evidence. Second, employees have little access to information on the quality of lawyers, with many using low-quality informal lawyers first encountered on the steps of the court building. Third, many private lawyers inflate claims, possibly to inflate the expectations of workers to convince them to sue. Fourth, perhaps because of the second and third element, two in five plaintiffs using private lawyers realize negative net returns. Fifth, information provided to the lawyer is not transmitted to the employee.

Our evidence is thinner that we would like on some of these elements, pointing to opportunities for future research. In particular, the results and story suggests the need to understand more about how plaintiffs choose their lawyer, and to explore ways to create and disseminate information on the quality of lawyers, allowing higher-quality lawyers to develop a reputation.

Our experiment also points to ways in which courts in developing countries can be incrementally improved. By working closely with the court and providing it with evidence about effective and easily scaleable policies, we have been able to contribute to the policy dialogue on general policies at the court. In the context of a major constitutional reform of labor law, the court has

proposed that federal labor law include both pre-filing conciliation hearings and statistical information customized to the case, grounded in the evidence from the experiment. [45].

[45]This proposal is under consideration in the Federal Senate, see text at: https://goo.gl/9AZ6H7

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9 Tables

Table 1: Summary Statistics

Variable	HD	HD Subcourt 7	Phase 1	Phase 2
	Panel A: Outcomes			
Win	0.65	0.69		
	(.48)	(.46)		
Amount won	23526	20298		
	(54298.25)	(46612.46)		
Total asked	343060	301566	651620	611740
	(655168.15)	(551154.03)	(1501129.1)	(3151612.79)
Conciliation	0.63	0.68	0.09	0.13
	(.48)	(.47)	(.29)	(.34)
Losing court ruling	0.07	0.01		
	(.25)	(.09)		
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	0.09	0.07
	(.3)	(.36)	(.29)	(.25)
Female	0.48	0.35	0.41	0.45
	(.5)	(.48)	(.49)	(.5)
At will worker	0.07	0.06	0.13	0.08
	(.25)	(.23)	(.34)	(.27)
Tenure (years)	4.17	3.72	4.82	4.05
	(4.99)	(4.72)	(6.01)	(5.44)
Daily wage	470	455	953	622
	(1100.8)	(656.22)	(2498.66)	(908.99)
Weekly hours	57.33	57.36	58.14	56.76
	(15.47)	(15.57)	(17.06)	(14.22)
Observations	5005	857	1097	1838

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 2: Expectations Relative to Prediction

	Expectation		Relative OC	
	Probability	Amount	Probability	Amount
	(1)	(2)	(3)	(4)
Employee's Lawyer (β_1)	4.36	5834.2	0.22	0.16
	(3.35)	(12516.9)	(0.14)	(0.54)
Firm's Lawyer (β_2)	-22.8***	-29459.8**	-0.79***	-0.56
	(3.24)	(12048.4)	(0.13)	(0.52)
Constant (employee α)	74.1***	75602.4***	1.69***	0.75*
	(2.78)	(10240.7)	(0.12)	(0.44)
Observations	2529	2169	2192	1878
File Fixed Effects	YES	YES	YES	YES
R-squared	0.80	0.88	0.97	0.89
p-value:Emp Law	0	0	0	0
p-value:Firm Law	0	0	0	0.3

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 the firm or firm's lawyer are accurate in their predictions. 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 3: 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***	0.0049	0.057**	1.67
	(0.033)	(0.22)	(0.024)	(2.33)
Constant	2.76***	5.33***	1.02***	74.4***
	(0.19)	(0.97)	(0.095)	(9.67)
Observations	4866	4864	4864	4866
BVC	YES	YES	YES	YES
Dummy Industry	YES	YES	YES	YES
R-squared	0.74	0.036	0.042	0.020
DepVarMean	11.7	6.39	0.20	65.3

	Panel B : Settlement			
	Total asked	Amount Won	Won/asked	Prob winning
Public Lawyer	-0.67***	-0.19***	0.087***	0.090
	(0.041)	(0.053)	(0.020)	(0.38)
Constant	2.63***	6.43***	1.52***	97.8***
	(0.25)	(0.33)	(0.11)	(2.50)
Observations	3081	3080	3080	3081
BVC	YES	YES	YES	YES
Dummy Industry	YES	YES	YES	YES
R-squared	0.74	0.35	0.11	0.0065
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***	-0.16	0.039	-0.11
	(0.12)	(0.80)	(0.29)	(7.57)
Constant	2.13***	-0.87	1.48**	-0.29
	(0.70)	(2.96)	(0.75)	(26.7)
Observations	417	416	416	417
BVC	YES	YES	YES	YES
Dummy Industry	YES	YES	YES	YES
R-squared	0.78	0.13	0.056	0.13
DepVarMean	11.7	2.67	0.34	23.8

Notes: 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 4: Treatment Effects

	Months after treatment						
	Same day settlement					2 months	5+ months
	Phase 1		Phase 2		Phase 1/2		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Control (Constant)	0.060***	0.034***	0.11***	0.10***	0.095***	0.15***	0.39***
	(0.013)	(0.011)	(0.030)	(0.030)	(0.026)	(0.043)	(0.039)
Calculator	0.051**	0.019	0.045*	0.0067	0.017	0.0024	-0.0082
	(0.022)	(0.019)	(0.021)	(0.019)	(0.014)	(0.021)	(0.024)
Conciliator	0.054***	0.033*	-	-	0.016	-0.0028	-0.032
	(0.019)	(0.018)	-	-	(0.019)	(0.023)	(0.028)
Emp present (EP)		0.14***		0.14*	0.14***	0.10**	0.089*
		(0.050)		(0.072)	(0.041)	(0.047)	(0.049)
Calculator##EP		0.16**		0.16*	0.16***	0.18***	0.16**
		(0.079)		(0.088)	(0.056)	(0.061)	(0.065)
Conciliator##EP		0.16**			0.16**	0.22***	0.28***
		(0.074)			(0.071)	(0.081)	(0.077)
Observations	1074	1074	1098	1098	2172	2172	2164
R-squared	0.0072	0.12	0.050	0.11	0.13	0.12	0.11
Court dummies	NO	NO	YES	YES	YES	YES	YES
DepVarMean	0.095	0.095	0.20	0.20	0.15	0.19	0.32
InteractionVarMean		0.18		0.18	0.18	0.18	0.18
Calc=Conc	0.88	0.53	-	-	0.94	0.82	0.37
Calc=Conc=0	0.0083	0.18	-	-	0.42	0.97	0.50

Notes: This table estimates the main treatment effects for both experimental phases. Columns 1 through 4 measure conciliation on the same day as the treatment (dummy variable), while 5 through 7 use later time periods measuring from the treatment date, pooling data on phase 1 and phase 2 together. For this latter columns we pooled data from both phases. Each column is a different regression. Recall that in phase 1 treatment is at the casefile level, while in phase 2 it is at the day level. The control group in phase 1 only answered the survey while in phase 2 control means there was neither treatment nor survey. Calculator is a dummy indicating that the casefile or day was randomly assigned to receive the calculator. Conciliator is a dummy indicating that the casefile was assigned to speak to a conciliator (this exists only for phase 1). Employee present is an indicator for the employee being present on the treatment day, and the next two rows are interactions terms between the presence of the employee and the treatment dummies. The bottom panel shows p values of the hypotheses: Calculator = Conciliator, Calculator = Conciliator = 0. Phase 2 columns are conditional on being notified and include subcourt dummies.

Table 5: Comparison of Settlement Amounts

(a) Settlement amount by treatment

Settlement amount						
	Phase 1			Phase 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Calculator	-988	-315	-1310	-856	-731	-1140
	(1753)	(1822)	(1793)	(1180)	(1208)	(1216)
Conciliator	-572	-182	-1299			
	(1850)	(1872)	(2050)			
Counter factual-court ruling amount (thousands)			2			103***
			(55)			(29)
Entitlement by law (thousands)			198			17
			(273)			(64)
Observations	173	160	146	292	274	263
R-sq	0	0.06	0.14	0	0.23	0.28
Basic Variable Controls	NO	YES	YES	NO	YES	YES
DepVarMean	10880	11187	11236	10942	10829	10970

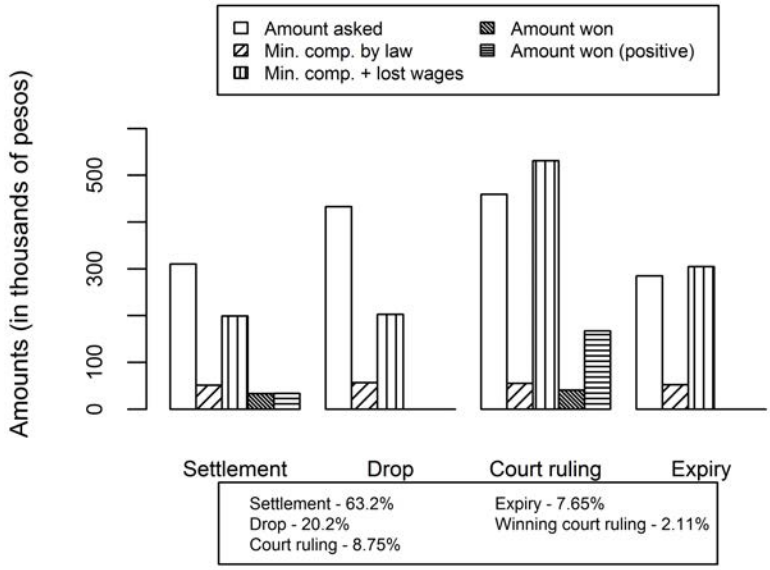
(b) Settlement amount vs Judgement amounts

Treatment effect. Nearest-neighbor matching			
	Phase 1/2		
	(1)	(2)	(3)
ATE	4926***	4946***	6170***
	(1009)	(876)	(886)
% ATE	141	141	319
Baseline mean	3502	3502	1936
Obs	344	344	341
Obs HD	377	377	365
Matches	[1,1]	[1,3]	[1,1]

Notes: This table investigates whether our treatments affected settlement amounts adversely for the workers. Panel A focuses on our workers that conciliated within our experimental samples in the same day as treatment. It regresses the (undiscounted) settlement amount against treatment dummies. When the worker is served by a private lawyer we subtract MXN$2000 from the initial fee, as well as 30% of the settlement amount. These numbers are the median responses from our baseline survey on how the private lawyers are paid. Each column is a different regression. Column 1 for instance shows that 173 settled on the same day as the treatment. Column 2 controls for the BVC's (observations differ since there are some missing values in the basic variable controls), while Column 3 also includes the calculator prediction ("counter factual-court ruling amount") as well as the entitlement by law for unfair dismissal assuming the worker wins the case. Panel (b) compares the settlement amount for those that conciliated in the the calculator or conciliator treatment against a matched sample from the historical dataset (HD) that went all the way to a court ruling. This panel combines phase 1 and 2 (hence the difference in observations in both panels). The matching is a nearest-neighbors matching between casefiles from Phase 1/Phase 2 and Historical Data (HD) with the basic variables public lawyer, gender, at will worker, tenure, daily wage & weekly hours, entitlement by law, and calculator prediction for court ruling amount. Counterfactual and settlement quantities are brought to present value at the time of suing with a monthly interest rate of 3.43, with a 30% cost, and an initial fee of MXN$2000 pesos for private lawyers. The ATE is calculated via different models are as follows: Column 1 is 1 to 1 match, with exact matching on the public lawyer dummy, has bias adjustment correction on continuous variables, and no trimming. Column 2 is the same except that the match is 1 to 3. Column 3 is the same as column 1 but trimming the top 1 percent of the dependent variable.

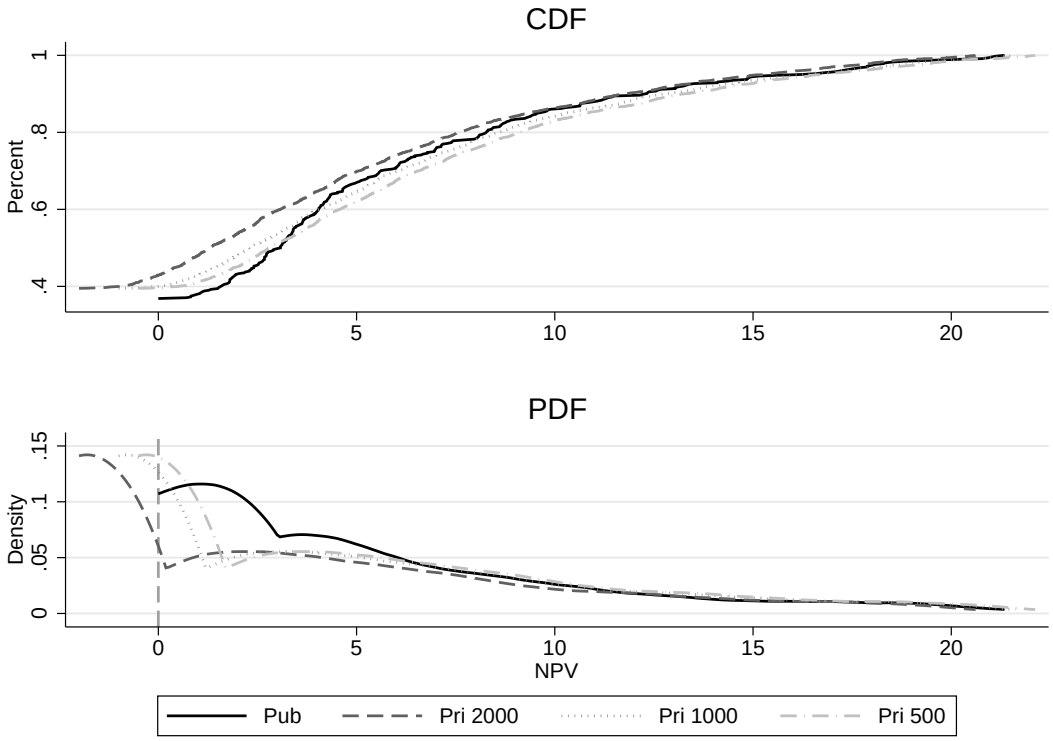
10 Figures

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



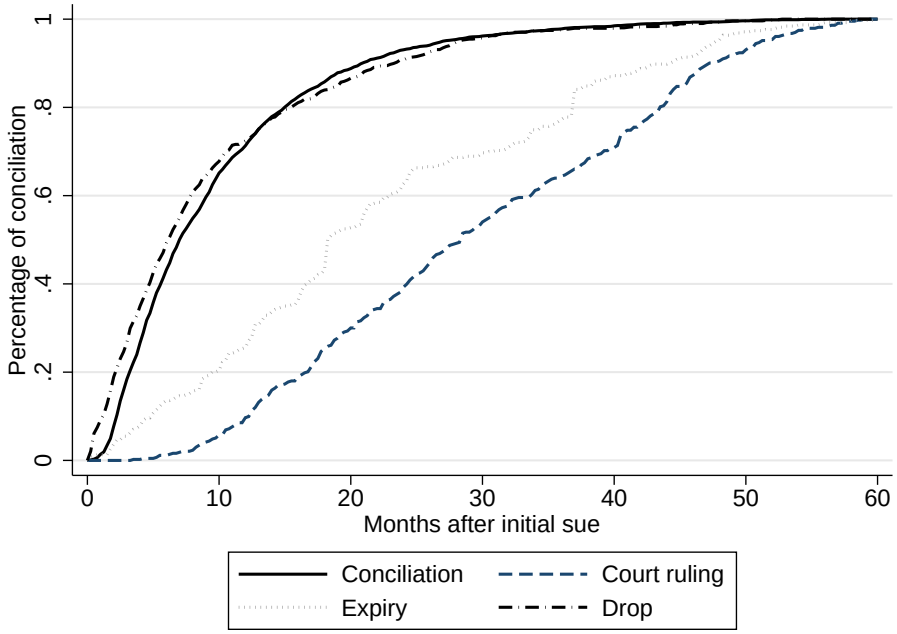
Notes: The figure shows the average amount asked in the filing initiating the suit, the amount actually received at the end of the process (overall and conditional on recovering a positive amount), the minimum legal compensation based on the case filing if the judge rules it he worker's favor, and the minimum legal compensation plus lost wages. 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 3.43 per month. Cases may end in any one of four ways: *settlement*, when the parties agree on a compensation for the worker and register that settlement at the court; *drop*, when that the case was dropped by the worker; *court ruling* when the judge issues a ruling in the case; *expiry*, when the case was closed by the court from lack of activity. The bottom legend shows the percentage of each outcome for the cases in the historical case files. By definition, workers recover nothing when cases are dropped or expire. In our data, all settlements imply a positive recovery for the worker. In court rulings, workers recover a positive amount only 24% of the time. We cannot distinguish between the judge ruling against the worker vs ruling in favor but the court being unable to collect anything from the firm.

Figure 2: 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.

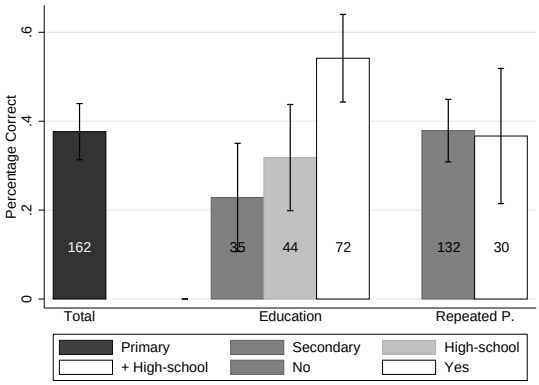
Figure 3: Time Duration



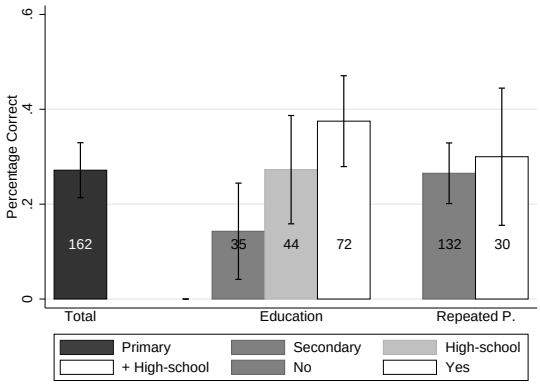
Notes: The figure uses the historical data (5005 casefiles) to plot the cumulative distribution of the duration of the case in months, by type of ending, for the 70 percent of cases concluded by the end of 2015. Many last for several years. In particular, around two-thirds of cases that are settled have a duration of less than one year, and there are very few settlements more than two years after filing. Those ending by court ruling or expiry have much longer average duration.

Figure 4: Knowledge about Law and their Own Claims in Lawsuit

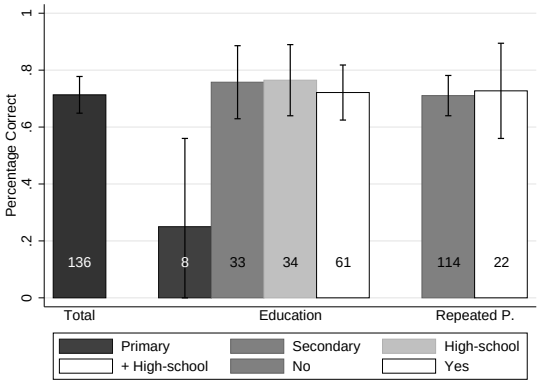
(a) Employee claims to know constitutional days of severance



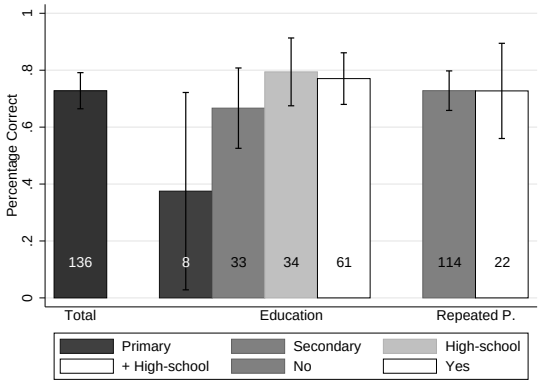
(b) Employee correctly knows constitutional days of severance



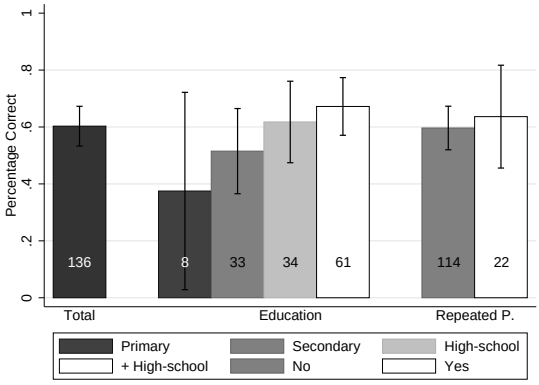
(c) Know if asked for constitutional days of severance



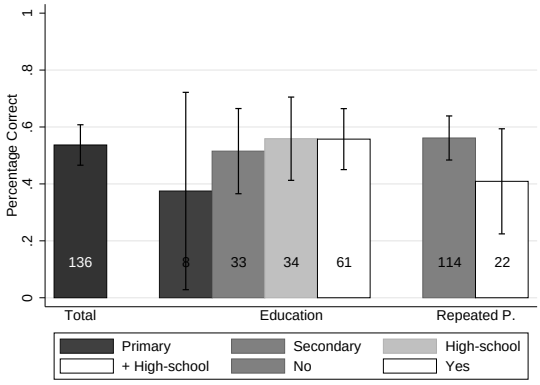
(d) Know if asked for Reinstatement



(e) Know if asked for Overtime

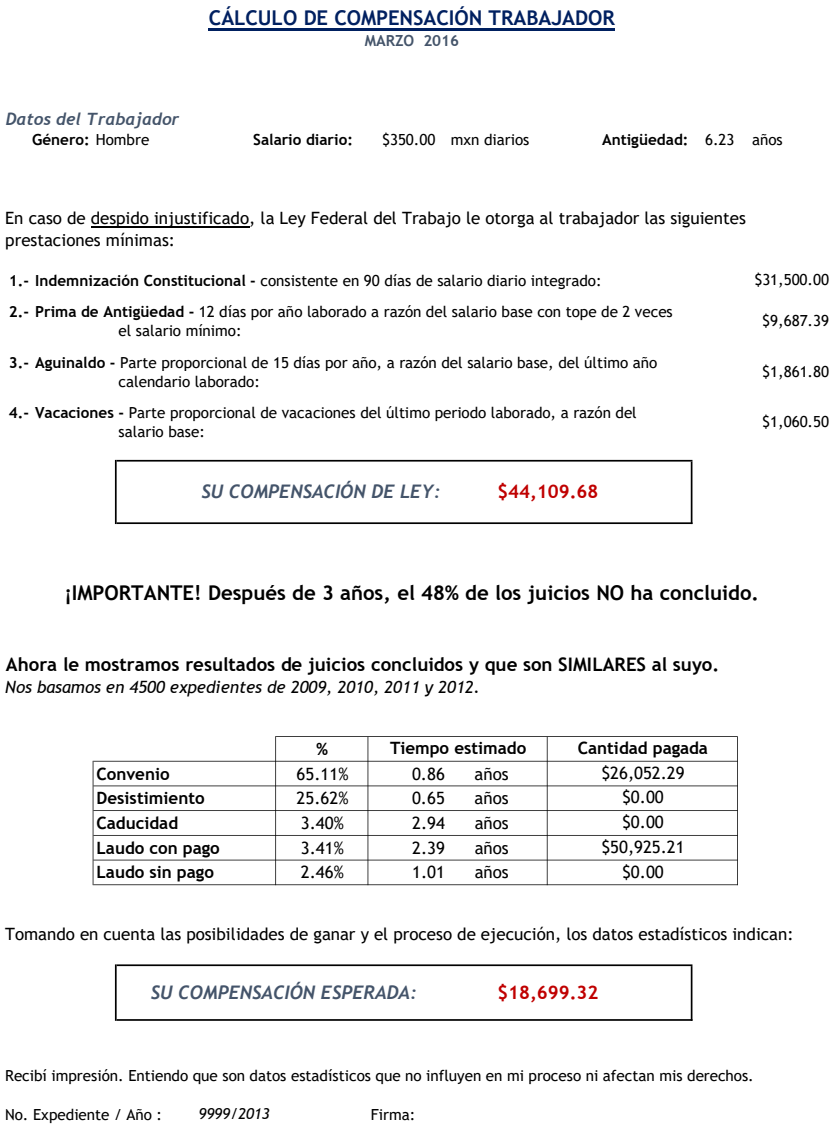


(f) Know if asked for end of year bonus



Notes: Data is from baseline survey of Phase 1. The figure plots averages of correct answers for several questions, grouped into: knowledge of the law –panel (a) and (b)–, and knowledge of the content of their own lawsuit –panels (c) to (f). We shows the average of the 136 survey responses and also averages by education level. The question for panel (a) is *"In the case of unjustified dismissal, law gives you a constitutional indemnification. (a) did you know this? and (b) This represents XX days of salary"* (162 obs). Panels (c) to (f) 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"*.

Figure 5: 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**

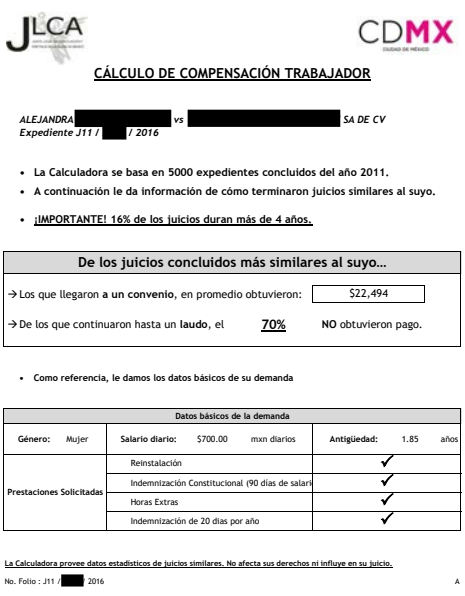
Recibí impresión. Entiendo que son datos estadísticos que no influyen en mi proceso ni afectan mis derechos.

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

Notes: The figure shows an example of Phase 1 calculator treatment formats. We explained that this is a statistical exercises based on historical finished cases, and that it gives average prediction based on variables of the initial lawsuit described in the calculator treatment. The top half of Phase 1's format described their entitlement by law if the judge ruled in their favor and took the facts from the initial suit at face value. 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.

Figure 6: Calculator Treatment Format (example) - Phase 2

(a) Plaintiff



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.

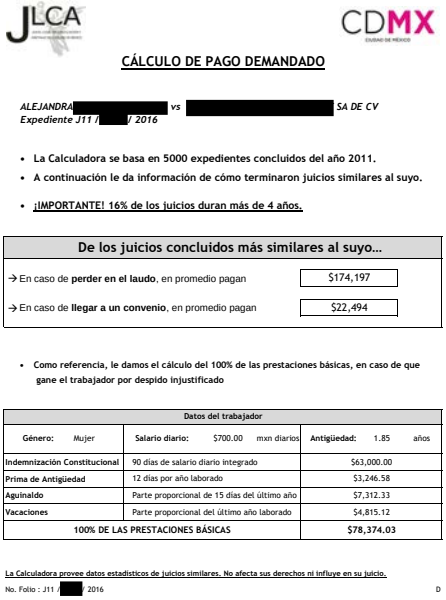
- Como referencia, le damos los datos básicos de su demanda

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



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 Phase 2 calculator treatment formats. The left panel format was given to the plaintiff and the right panel format to the defendant. We explained to parties that this is a statistical exercises based on historical finished cases, and that it gives average prediction based on variables of the initial lawsuit described in the calculator treatment. The format for phase was made altered from that used in phase 1 at the request of the court and because we had less time to explain the data to the parties. We show 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,.

Appendix A

As described in the paper, one of the treatment arms required giving workers information about predicted outcomes for their case. In this Appendix 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 predicted with 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 both experiments and for 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 to have 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.[46]

[46] 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.

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 have digitized do not allow us to differentiate between these two outcomes. We don't see this as an important shortcoming since from the point of view of the plaintiff what matters is the amount she receives.

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.

As described in the paper, in our historical data of cases filed in 2011 and completed by the end of 2015, 63.3% end in settlement, 2.0% in a court ruling with positive collection, 6.6% with a court ruling with nothing collected, 20.3% expire and 7.6% 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.[47] We therefore estimated models for four bivariate outcomes, using each the following methods: (a) Logistic Regression, (b) Probit, (c) Random Forest[48], (d) Single-hidden-layer Neural Network (20 nodes in the hidden layer and 10% weight decay), (e) Gradient Boosting.[49] Models were estimated in a random sample training set made of 70% of the observation. The remaining 30% 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

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

[48] 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% accuracy rate on test classification.

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

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.[50] The following table 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 to 0.93 for the selected.

Table A1: Fit assessment of discrete calculator models

Phase 1: Probability models

	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

Phase 2: Probability models

	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 accuracy rate for this models. 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.

[50] 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.

For the continuous outcomes, we estimated a set of four different models for each of the two prediction problems.[51] The four models were: (a) OLS regression, (b) GLM Boosting, (c) Random Forest[52], (d) Ridge Regression.[53][54] 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).

Table A2: 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

Phase 2: 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.

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

[51] 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.

[52] 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.

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

[54] We also run the OLS and Random Forest models using logged data. In phase 2, we changed the models we estimated to include Kernal regression, running both OLS and Kernal 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.

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.

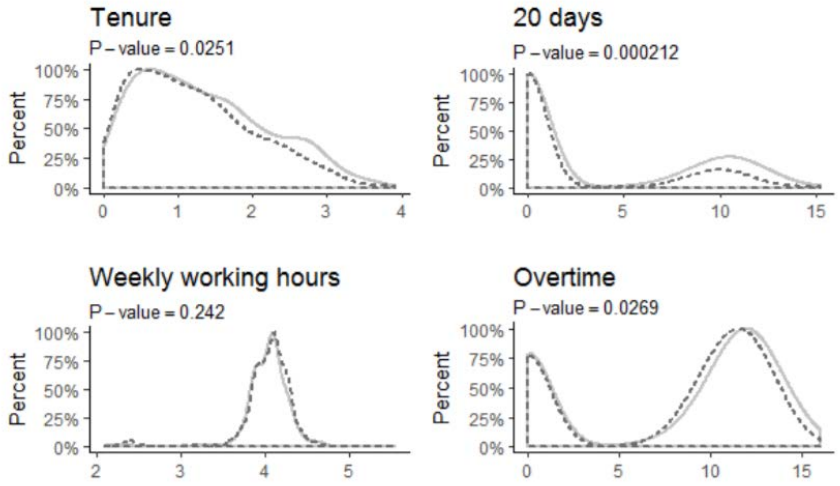
Other issues: Censoring

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.

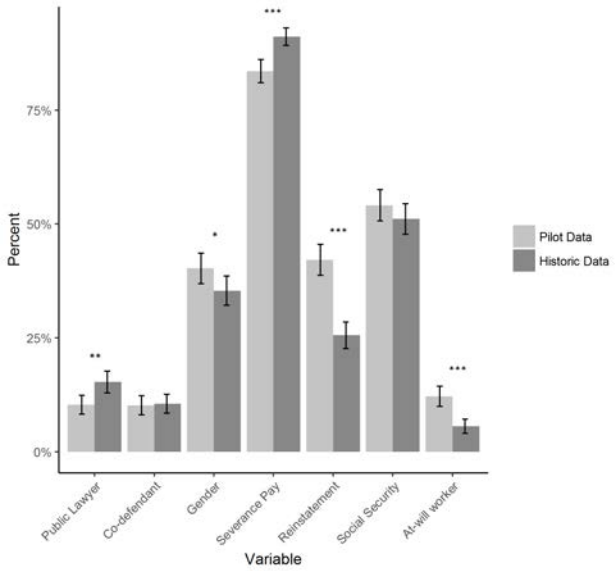
We are aware of this bias and it was communicated to the parties when the calculator information was provided. Although we cannot know how large the bias is, we performed two exercises that suggest it is not large. First, we compare characteristics of ongoing cases with those of the historical cases used in the models. In Figure A1 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 A2 reports the results. There are few differences.

Figure A1: Covariate distribution comparison
Historical and Phase 1 data

Continuous variables



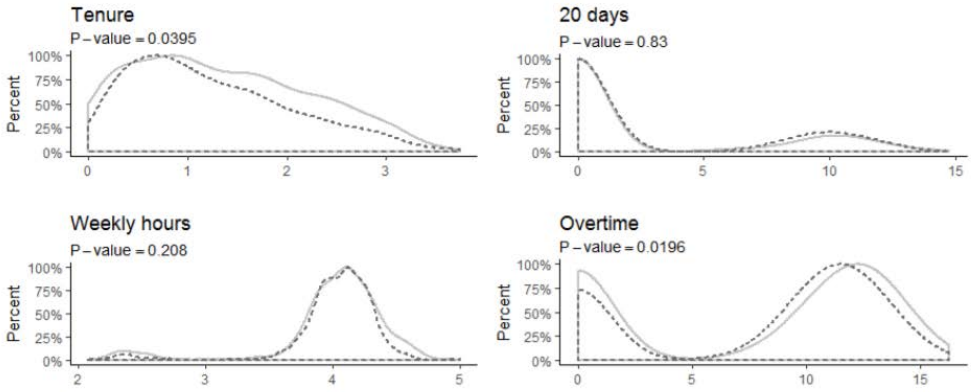
Categorical variables



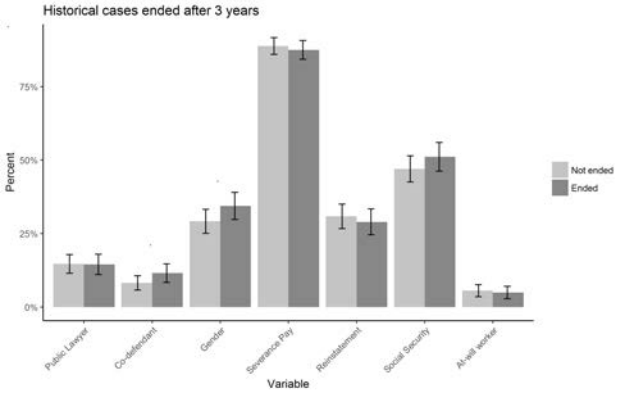
Notes: Covariate distributions comparisons. We compare between -used to calibrate our calculator- and Phase 1 data. All continuous variables are plotted in logs. Color guide is the same for both variable types. Plots for continuous variables show the p-value of a Kolmogorov–Smirnov test in the subtitle.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

Figure A2: Covariate distribution comparison
Historical data: Ended and not ended cases

Continuous variables



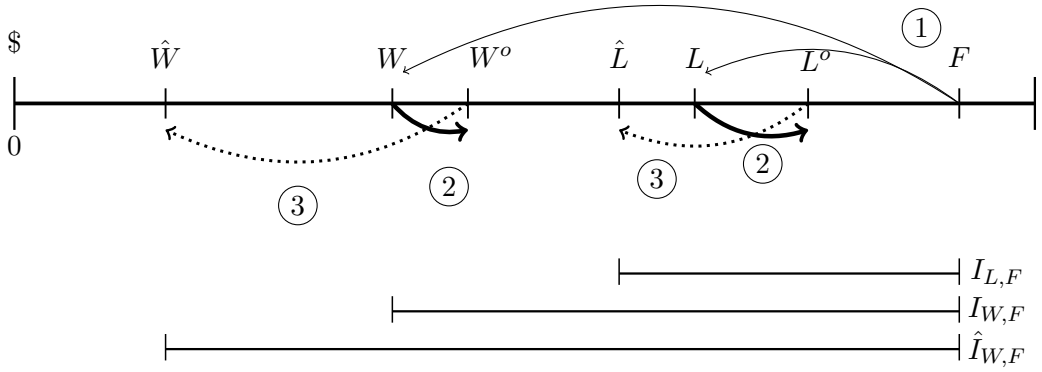
Categorical variables



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 Kolmogorov–Smirnov test in the subtitle. In the categorical covariates plot, we show significance of a two-sided t-test of differences in means between samples. *Script:* `covariate_plots.R , covariate_plots_hd.R`

Appendix B

Figure B1: 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.

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,

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

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

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

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. □

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.

□

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, where the inequality is achieved as S is dominated by g_w. □

Appendix C

C01 Tables

Table C1: 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.
IMSSINFOSAR*	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 *.

Table C2: Survey variables summary statistics

Variable		Employee	Employee Lawyer	Firm Lawyer
Age		44.84	37.38	36.29
		(13.32)	(12.05)	(11.29)
		162	377	420
Tenure			27.19	30.15
			(247.12)	(204.10)
			303	354
Number of lawsuits				
	<10		8.31	6.18
	(10,50)		23.43	17.66
	(50,100)		18.39	17.66
	>100		49.87	58.5
			377	420
Current number of lawsuits				
	<5		9.57	7.06
	(5,10)		14.61	11.04
	(10,30)		17.88	17.22
	>30		57.93	64.68
			377	420
Number of employees				
	(1,10)	14.29	27.45	24.29
	(11,50)	41.76	39.87	20.19
	(51,100)	12.09	17.97	19.56
	>100	31.87	14.71	35.96
		154	289	420
Percentage of what is obtained		28.66	29.96	
		(12.29)	(7.64)	
		95	291	
Probability of other part of winning		45.39	52.65	55.64
		(33.38)	(25.08)	(27.82)
		162	377	420
Education				
	Elementary	12.57		
	Secondary	19.9		
	High-School	29.32		
	+ High-School	38.22		
		162		
Have you changed lawyer during trial?		9.95		
		162		
Probability of winning trial		80.19	71.42	68.69
		(21.79)	(20.64)	(21.00)
		162	377	420
Most probable amount		141427.6	203085.8	4.03
		(325872.5)	(1172272)	(2.47)
		162	377	420
Most probable time		55.88	3.85	25531.36
		(723.28)	(2.25)	(105448.6)
		162	377	420
How well were you treated?				
	Very good	17.8		
	Good	26.7		
	Not so good	29.84		
	Nothing good	25.65		
		162		
How common is the company mistreat its employees?				
	Very common	52.88		
	Common	20.42		
	Not so common	14.14		
	Nothing common	12.57		
		162		

Level of anger with company		
	A lot	50.26
	Moderately	23.04
	Little	13.09
	Nothing	13.61
		162
Repeat player		18.85
		163
Currently employed		7.64
		162
Looking for a job		43.46
		162
Probability of finding a job in next 3 months		58.19
		(26.31)
		73

Notes: Summary statistics of survey variables.

Table C3: Settlements against duration - Historical Data

	Settlement amount		Settlement amt discounted		Calculator prediction	
	(1)	(2)	(3)	(4)	(5)	(6)
Months	1202.9***		-166.3**		180.0***	
	(166.8)		(70.37)		(30.82)	
2 Quintil		4052.7**		1754.3		1576.3***
		(1859.3)		(1676.1)		(460.2)
3 Quintil		1622.4		-1767.8		1451.0**
		(2015.3)		(1705.6)		(565.7)
4 Quintil		8663.2***		44.58		3029.1***
		(2348.7)		(1808.5)		(595.2)
5 Quintil		21982.6***		-3474.0*		3990.9***
		(3299.9)		(1882.0)		(718.8)
Constant	10907.8***	15396.9***	15841.8***	14649.2***	9544.0***	9331.9***
	(4229.2)	(4326.9)	(2953.9)	(3018.0)	(1031.9)	(1076.7)
Observations	3080	3080	3080	3080	3081	3081
R-squared	0.24	0.23	0.22	0.22	0.66	0.65
BVC	YES	YES	YES	YES	YES	YES

Notes: This table focuses on the sample of cases from the historical data (HD) that settled at any point in time (3080 cases out of 5005). Each column is a different regression. All regressions control for the basic variables of the case (BVC) which are described in Panel B of Table 1. Columns 1 and 2 have as a dependent variable the settlement amount in pesos the worker actually got in the settlement. Column 1 regresses this amount on a counter for the number of months that elapsed from the initial filing until the settlement. Column 2 breaks this elapsed time in quintiles dummies. Columns 3 and 4 discount the peso amount at a 3.43 monthly rate. This rate was calculated by matching our sample to that of the MxFls survey and obtaining the elicited time discount rate for similar individuals (using gender, age, education, daily wage, and weekly hours), and taking the median. Columns 5 and 6 use the prediction from our "calculator", that is, the amount they would get on average given the initial case characteristics, with no discounting.

Table C4: Treatment description - Phase 1

	Control	Calculator	Conciliator
Baseline survey	All treatment and control groups were required to complete the baseline survey.		
Calculator prediction	-	Subjects were assisted by project personnel to input variables from their casefile to the calculator, and the resulting predictions were explained.	Subjects did not have access to the calculator information.
Conciliator mediation	-	Subjects could choose to talk to a conciliator.	Subjects were required to talk to the conciliator, whether or not their counterpart was present
Exit survey	All treatment and control groups were required to complete the exit survey.		

Notes: This is a short description of the kind of treatment received by each treatment group.

Table C5: 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	365	-	-	-	-	49.86	34.25	9.04	75.07	39.45	24.11	6.03	56.71
Calculator	351	76.35	71.69	74.18	75.9	47.29	34.19	12.54	68.95	37.26	25.21	9.86	52.33
Conciliator	358	71.8	72.03	72.91	76.29	43.02	41.9	15.92	68.99	35.07	27.4	11.51	50.14

(b) Phase 2

Group	N	Compliance with treatment				Compliance with survey			
		Plaintiff	Defendant	Both	Any	Plaintiff	Defendant	Both	Any
Control	386	-	-	-	-	-	-	-	-
Treatment	890	74.16	64.61	50.45	88.31	53.26	46.97	28.76	71.46

(c) % Shows up

	Phase 1			Phase 2	
	Control	Calculator	Conciliator	Control	Calculator
Employee	0.191	0.194	0.159	0.122	0.165
Emp Lawyer	0.844	0.835	0.846	0.85	0.853
Firm Lawyer	0.746	0.772	0.799	0.434	0.445

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. *Scripts:* `table_compliance.do, balance_compliance.R, showed_up.do`

Table C6: Balance table

	Phase 1			Phase 2		Phase 1/2	
	C	T1	T2	C	T	C	T
Female	0.43	0.37	0.37	0.51	0.45*	0.48	0.43
At will worker	0.12	0.1	0.14	0.1	0.07	0.11	0.08*
Weekly hours	55.89	57.57	58.67**	55.04	56.66*	55.41	56.86**
Tenure at firm	4.82	4.57	4.83	4.98	4.14*	4.91	4.24**
Public Lawyer	0.11	0.13	0.08	0.06	0.06	0.08	0.08
% Reinstatement	0.43	0.42	0.41	0.48	0.51	0.46	0.49
% Severance pay	0.82	0.83	0.84	0.81	0.79	0.82	0.8
Daily wage	855.84	630.16*	798.88	617.55	603.86	722.73	609.86
% Backpay	0.97	0.96	0.96	0.99	0.98	0.98	0.98
% Tenure bonus	0.8	0.83	0.82	0.8	0.78	0.8	0.79
% Extra hours	0.66	0.71	0.72	0.71	0.78**	0.69	0.76***
% 20 days	0.33	0.33	0.33	0.35	0.34	0.34	0.33
% Sunday bonus	0.19	0.21	0.21	0.18	0.19	0.18	0.2
% Weekly rest	0.21	0.17	0.22	0.17	0.22**	0.19	0.21
%Mandatory rest	0.31	0.29	0.3	0.26	0.3	0.28	0.3
% Social security codef	0.55	0.55	0.53	0.63	0.64	0.59	0.62
% Earnings	0.35	0.34	0.37	0.37	0.35	0.36	0.35
% Nulity	0.64	0.58	0.52***	0.61	0.57	0.62	0.57**
Entitlement	95251.61	71962.36*	90374.76	63265.84	69315.22	76900.06	69910.89
Presence employee	0.19	0.18	0.17	0.16	0.19	0.17	0.19
Presence emp law	0.86	0.83	0.83	0.91	0.89	0.89	0.87
Presence firm	0.03	0.02	0.01	0.01	0.02	0.02	0.02
Presence firm law	0.72	0.75	0.78*	0.79	0.78	0.76	0.78
Observations	300	288	301	328	768	628	1056

Notes: Balance table on some basic and strategic variables. The first three columns show phase 1, with control, calculator, and conciliator treatments in the three columns. The next two columns show the control and treatment of 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. *Do file:* `balance.do`

Table C7: Balance regression on characteristics conditional on employee present

	Control	Calculator	Conciliator	Observations	R-squared	F-stat
Public Lawyer	0.10	0.040	-0.11	371	0.040	4.69
	(0.068)	(0.046)	(0.068)			
Gender	0.56***	-0.12**	-0.042	371	0.075	6.93
	(0.097)	(0.059)	(0.080)			
At will worker	0.028	0.0059	0.084	371	0.039	2.85
	(0.041)	(0.028)	(0.054)			
Tenure	5.35***	-1.77**	-0.79	365	0.055	2.32
	(1.00)	(0.88)	(1.06)			
Daily wage	586.6***	19.7	680.4	365	0.021	1.21
	(135.7)	(110.1)	(653.4)			
Weekly hours	61.0***	-0.26	1.39	362	0.10	7.68
	(2.50)	(1.67)	(3.29)			

Notes:

Table C8: Balance regression on characteristics conditional on settlement

	Public Lawyer	Gender	At will worker	Tenure	Daily wage	Weekly hours
	(1)	(2)	(3)	(4)	(5)	(6)
Control (Constant)	0.073	0.62***	0.053	2.95***	510.0**	60.8***
	(0.049)	(0.10)	(0.053)	(0.75)	(216.2)	(1.32)
Calculator	-0.0071	-0.098	-0.022	-0.29	109.9*	-1.04
	(0.040)	(0.063)	(0.034)	(0.67)	(60.0)	(1.66)
Conciliator	-0.033	-0.050	-0.015	0.98	31.5	0.55
	(0.066)	(0.089)	(0.050)	(1.22)	(66.2)	(2.90)
Observations	315	315	315	303	312	312
R-squared	0.055	0.018	0.044	0.028	0.043	0.13
F stat	4.72	1.00	2.02	1.86	1.23	9.09
Court dummies	YES	YES	YES	YES	YES	YES

Notes:

Table C9: Balance of casefiles having negative recovery amount.

	Gender	At will worker	Tenure	Daily wage	Weekly hours
	(1)	(2)	(3)	(4)	(5)
Negative NPV	-0.037**	-0.0090	-0.66***	-1.21	1.88***
	(0.015)	(0.0080)	(0.16)	(35.9)	(0.48)
Constant	0.46***	0.070***	4.72***	459.0***	60.8***
	(0.017)	(0.0086)	(0.18)	(61.7)	(0.46)
Observations	4503	4492	4470	4484	4425
R-squared	0.023	0.0040	0.014	0.0010	0.067
F stat	22.1	2.89	10.5	1.00	53.3
Court dummies	YES	YES	YES	YES	YES

Notes:

Table C10: Treatment Effects with placebo arm - Phase 1

	Months after treatment	
	Same day	
	(1)	(2)
Control (Constant)	0.060***	0.034***
	(0.012)	(0.011)
Calculator	0.051**	0.019
	(0.021)	(0.017)
Conciliator	0.054***	0.033*
	(0.021)	(0.018)
Placebo	0.00077	0.013
	(0.018)	(0.016)
Placebo ctrl	-0.020	-0.010
	(0.014)	(0.012)
Emp present (EP)		0.14***
		(0.047)
Calculator##EP		0.16**
		(0.076)
Conciliator##Ep		0.16**
		(0.081)
Plaecbo##EP		-0.031
		(0.073)
Placebo ctrl##EP		0.066
		(0.072)
Observations	2154	2154
R-squared	0.014	0.11
DepVarMean	0.071	0.071
InteractionVarMean		0.14
Calc=Conc	0.89	0.49
Calc=Conc=0	0.0096	0.17
Calc=Placebo	0.018	0.73
Calc=Conc=Placebo=0	0.0066	0.31
=Placebos	0.23	0.38

Notes:

Table C11: Treatment Effects conditional on type of lawyer

	Private			Public		
	Same day settlement	2 months	5+ months	Same day settlement	2 months	5+ months
	Phase 1/2					
	(1)	(2)	(3)	(4)	(5)	(6)
Control (Constant)	0.093***	0.15***	0.40***	0.12	0.14	0.098
	(0.026)	(0.046)	(0.040)	(0.15)	(0.19)	(0.19)
Calculator	0.015	0.0028	-0.014	0.019	0.0067	0.079
	(0.016)	(0.024)	(0.028)	(0.052)	(0.082)	(0.093)
Conciliator	0.010	-0.0056	-0.042	0.023	-0.017	0.014
	(0.023)	(0.026)	(0.033)	(0.076)	(0.10)	(0.12)
Emp present (EP)	0.11**	0.093*	0.044	0.22**	0.18	0.32**
	(0.043)	(0.052)	(0.060)	(0.11)	(0.13)	(0.14)
Calculator##EP	0.24***	0.24***	0.24***	-0.0082	0.021	-0.17
	(0.065)	(0.073)	(0.080)	(0.12)	(0.14)	(0.16)
Conciliator##EP	0.17**	0.20**	0.27***	0.28	0.47**	0.27
	(0.081)	(0.086)	(0.086)	(0.23)	(0.23)	(0.24)
Observations	1817	1817	1812	154	154	153
R-squared	0.14	0.12	0.10	0.13	0.14	0.13
Court dummies	YES	YES	YES	YES	YES	YES
DepVarMean	0.16	0.20	0.34	0.18	0.20	0.30
InteractionVarMean	0.16	0.16	0.16	0.53	0.53	0.53
Calc=Conc	0.86	0.75	0.36	0.95	0.78	0.55
Calc=Conc=0	0.64	0.95	0.44	0.91	0.96	0.65

Notes:

Table C12: Heterogeneity in treatment effects

	Dep var: Settlement on the same day								
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)	0.048***	0.070	0.077**	0.047***	0.10**	0.075**	0.056***	0.10**	0.10***
	(0.016)	(0.044)	(0.033)	(0.017)	(0.040)	(0.034)	(0.019)	(0.044)	(0.031)
Calculator	0.084**	0.049*	0.0050	0.083**	0.033	0.0074	0.064	0.027	0.0032
	(0.037)	(0.026)	(0.018)	(0.032)	(0.022)	(0.018)	(0.041)	(0.037)	(0.025)
Conciliator	0.061*		0.020	0.056*		0.018	0.048		0.0053
	(0.032)		(0.034)	(0.029)		(0.026)	(0.032)		(0.034)
Interaction Var (Int)	0.037	0.093	0.042	0.034	0.034	0.045*	0.021	0.019	-0.020
	(0.029)	(0.060)	(0.037)	(0.028)	(0.037)	(0.024)	(0.029)	(0.044)	(0.023)
Calculator#Int	-0.043	-0.013	0.019	-0.050	0.0052	0.012	0.0015	0.033	0.025
	(0.059)	(0.064)	(0.042)	(0.050)	(0.039)	(0.028)	(0.055)	(0.062)	(0.041)
Conciliator#Int	-0.0051		-0.021	-0.0027		-0.012	0.024		0.011
	(0.052)		(0.057)	(0.051)		(0.046)	(0.047)		(0.049)
Emp present (EP)			0.048			0.14***			-0.013
			(0.048)			(0.045)			(0.059)
Calculator#EP			0.30***			0.23***			0.30***
			(0.075)			(0.063)			(0.088)
Conciliator#EP			0.22**			0.19*			0.20*
			(0.097)			(0.11)			(0.11)
EP#Int			0.12			0.0083			0.19**
			(0.071)			(0.071)			(0.084)
Calculator#EP#Int			-0.19*			-0.14			-0.16
			(0.10)			(0.092)			(0.12)
Conciliator#EP#Int			-0.017			-0.065			0.021
			(0.17)			(0.14)			(0.16)
Observations	875	1074	1949	869	1048	1917	855	1081	1936
R-squared	0.012	0.060	0.14	0.010	0.053	0.14	0.012	0.053	0.13
DepVarMean	0.11	0.20		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)	0.062***	0.093**	0.091***	0.059***	0.11***	0.095***	0.048**	0.056	0.067**
	(0.019)	(0.040)	(0.029)	(0.015)	(0.028)	(0.027)	(0.018)	(0.042)	(0.033)
Calculator	0.089**	0.055**	0.0032	0.062**	0.043*	0.015	0.069**	0.058*	0.0020
	(0.035)	(0.024)	(0.016)	(0.027)	(0.023)	(0.016)	(0.033)	(0.028)	(0.018)
Conciliator	0.054*		0.0083	0.056**		0.0079	0.026		0.0098
	(0.028)		(0.025)	(0.025)		(0.023)	(0.025)		(0.027)
Interaction Var (Int)	0.0099	0.049	0.014	0.066	0.041	-0.025	0.032	0.11**	0.063*
	(0.032)	(0.041)	(0.021)	(0.057)	(0.11)	(0.039)	(0.031)	(0.049)	(0.034)
Calculator#Int	-0.070	-0.026	0.025	-0.016	-0.030	-0.00017	-0.016	-0.025	0.023
	(0.055)	(0.039)	(0.026)	(0.077)	(0.12)	(0.034)	(0.052)	(0.063)	(0.042)
Conciliator#Int	0.012		0.0089	0.046		0.019	0.062		-0.00087
	(0.043)		(0.042)	(0.087)		(0.075)	(0.044)		(0.049)
Emp present (EP)			0.086*			0.11**			0.084
			(0.051)			(0.043)			(0.053)
Calculator#EP			0.28***			0.24***			0.26***
			(0.060)			(0.065)			(0.084)
Conciliator#EP			0.21**			0.17**			0.14
			(0.087)			(0.081)			(0.094)
EP#Int			0.082			0.086			0.072
			(0.087)			(0.095)			(0.088)
Calculator#EP#Int			-0.23**			-0.25**			-0.14
			(0.11)			(0.11)			(0.12)
Conciliator#EP#Int			-0.057			0.14			0.085
			(0.15)			(0.24)			(0.15)
Observations	883	1088	1971	883	1088	1971	861	1088	1949
R-squared	0.012	0.051	0.13	0.014	0.049	0.14	0.018	0.063	0.15
DepVarMean	0.11	0.20		0.11	0.20		0.10	0.20	

Notes:

Table C12: Treatment generated updating in probability - Phase 1

(a) Overconfidents

	Relative updating		
	Employee	Emp Lawyer	Firm Lawyer
	(1)	(2)	(3)
Calculator	-0.30*	0.61	-0.17
	(0.17)	(3.47)	(0.44)
Conciliator	-0.39*	-1.86	-0.11
	(0.19)	(1.92)	(0.39)
Constant	-0.76	-8.01	0.53**
	(0.51)	(5.63)	(0.27)
Observations	40	125	97
Basic Variable Controls	YES	YES	YES
Other controls	YES	NO	NO
R-squared	0.44	0.077	0.15
Update (mean)	-0.29	1.55	0.33
Update (SD)	0.42	12.9	1.70

(b) Underconfidents

	Relative updating		
	Employee	Emp Lawyer	Firm Lawyer
	(1)	(2)	(3)
Calculator	6.27	-4.69	0.055
	(10.7)	(5.07)	(0.28)
Conciliator	7.68	27.9	-0.18
	(14.7)	(20.5)	(0.17)
Constant	-4.11	64.3	-1.15***
	(45.7)	(43.4)	(0.31)
Observations	28	75	65
Basic Variable Controls	YES	YES	YES
Other controls	YES	NO	NO
R-squared	0.92	0.18	0.20
Update (mean)	13.2	9.76	-0.31
Update (SD)	65.8	48.9	0.77

Notes: All regressions include basic variables as controls (Public lawyer, Gender, At will worker, Tenure, Daily wage & Weekly hours) and sample is restricted to over-confident cases, i.e. where $initialsurvey > P$. Other controls refer to repeated, player, level of anger and education level. It is important to note that scpecification is not robust to other controls. Relative updating is computed as $\frac{exitsurvey - initialsurvey}{initialsurvey}$.

Do files: `update_reg_theta_rel_oc.do, update_reg_theta_rel_uc.do`

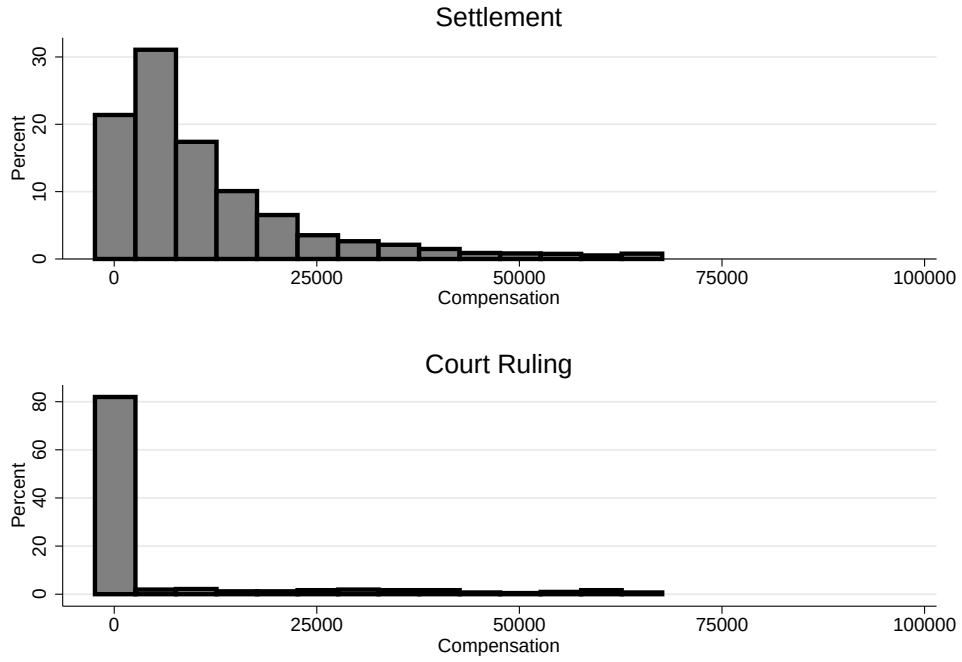
Table C13: Employee presence

	Employee Present	
	Phase 1	Phase 2
	(1)	(2)
Female	-0.045*	-0.015
	(0.027)	(0.014)
Public Lawyer	0.26***	0.43***
	(0.055)	(0.038)
At will worker	-0.061	0.0086
	(0.039)	(0.026)
Tenure (years)	0.0034	0.0025**
	(0.0023)	(0.0012)
Daily wage	0.0000099	-0.0000073
	(0.0000083)	(0.0000069)
Weeky hours	-0.0017**	-0.0017***
	(0.00081)	(0.00056)
Age		0.00010
		(0.0022)
Constant	0.27***	0.22***
	(0.054)	(0.036)
Observations	837	2504
R-squared	0.054	0.11
DepVarMean	0.20	0.15
p-value	0.0000015	7.0e-31
p-value w/o PL	0.016	0.00013

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.
Do file: `pactor_reg.do`

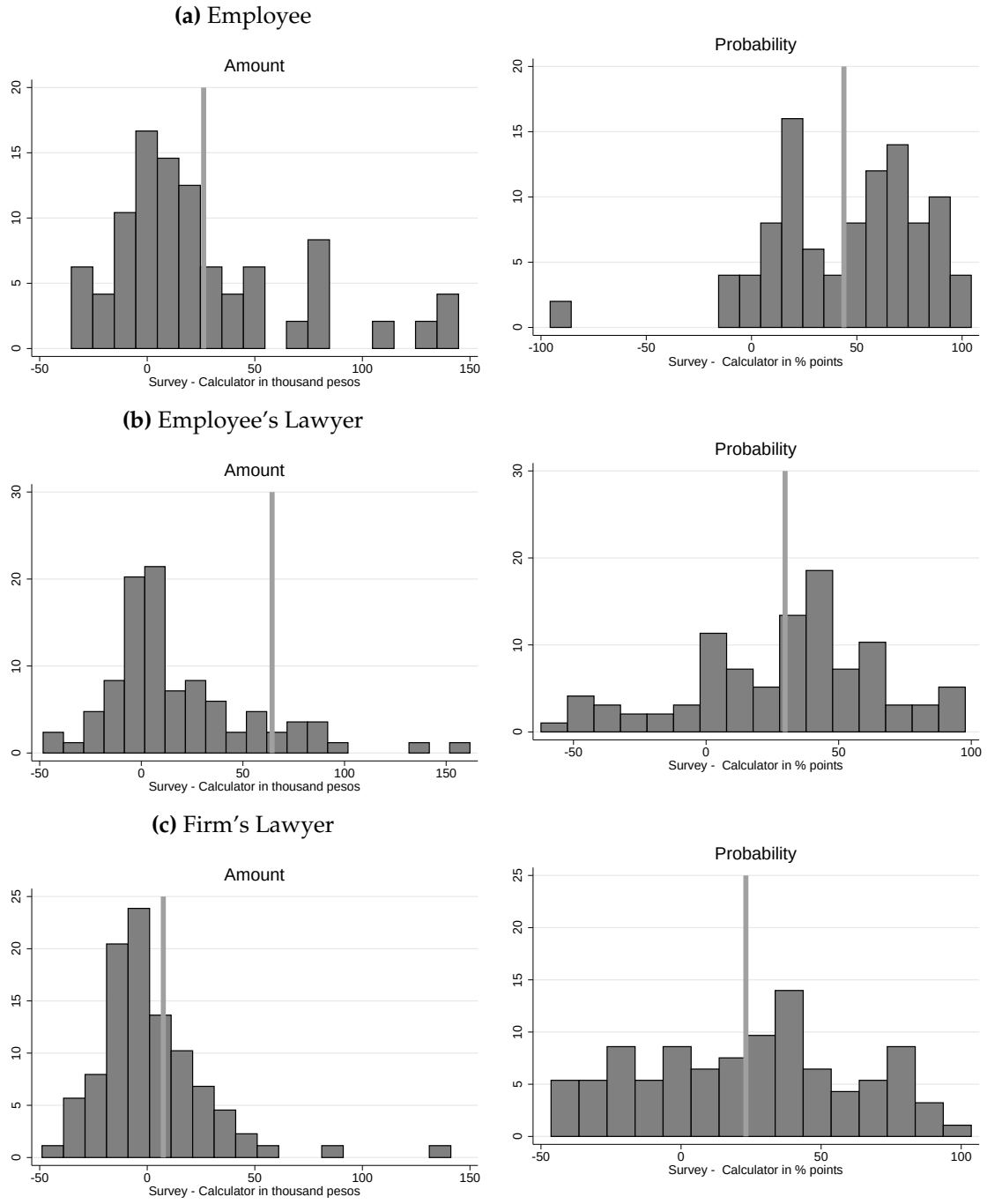
C02 Figures

Figure C1: Compensation histograms - Historical Data



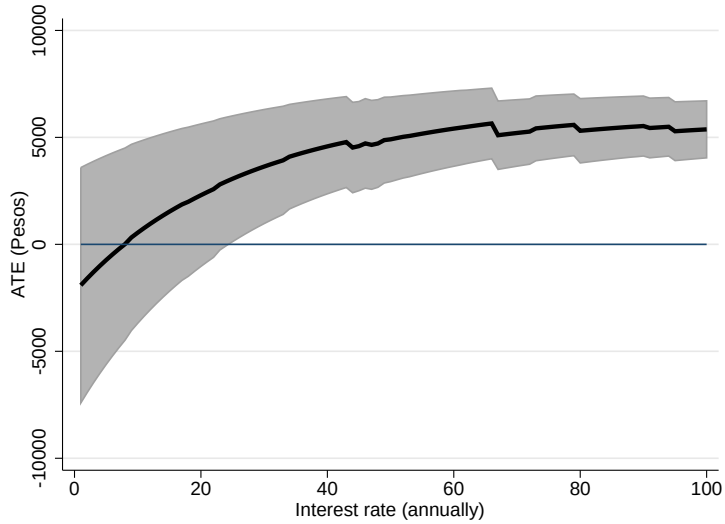
Notes: Distribution of compensation in present value at the time of suing with a monthly interest rate of 3.43, with a 30% cost, and an initial fee of MXN$2000 pesos for private lawyers and deflated into June 2016 MXN pesos. *Do File:* `histograms_npv_hd.do`

Figure C2: Subjective expectation minus prediction - Phase 1



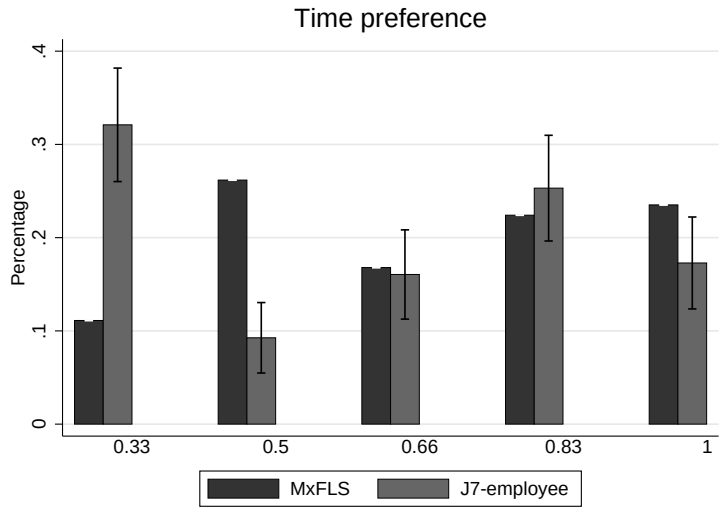
Notes: Difference in thousand pesos for amounts (panel on left) and percentage points for probabilities (panel on right) from what the subject expects vs. what our models predict. Note how, for Employee and employee lawyer, the distribution for amounts is much more skewed to the right than for firm lawyers.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

Figure C3: Discount rate against welfare effects



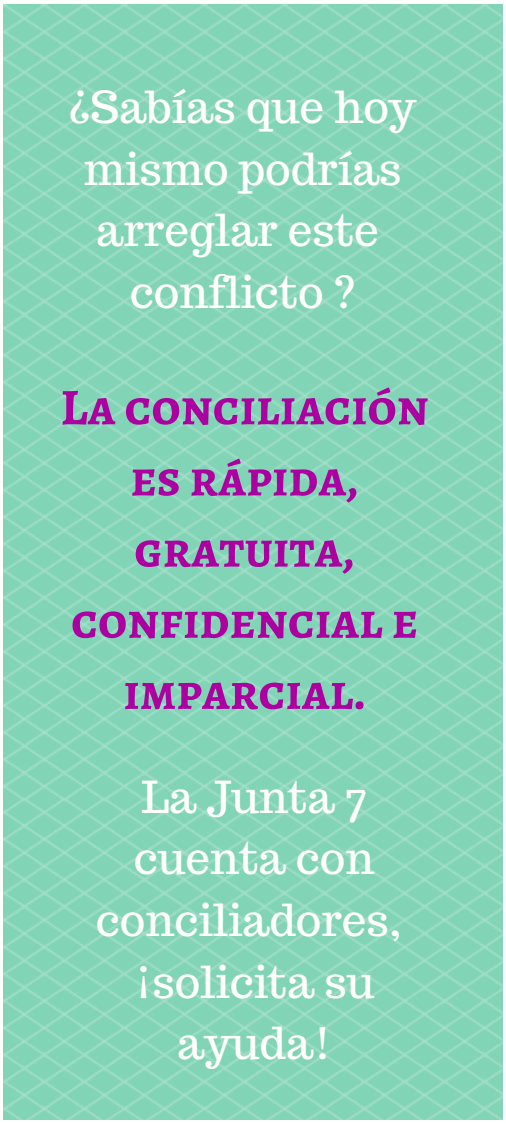
Notes: The graph shows the ATE effect of Table 5 when using different discounting annual interest rates.

Figure C4: 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).

Figure C5: 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.