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

By Joyce Sadka, Enrique Seira, and Christopher Woodruff *

Draft: March 4, 2019

Well-functioning courts are essential for upholding the rule of law and underpinning markets. Using data from Mexico's largest labor court, we document delays, overconfidence, and low settlement rates. In this context we conduct an experiment, providing personalized outcome predictions and conciliation services to a random subset of plaintiffs. The treatment doubles settlement rates, but only when the worker is present to receive the information. This suggests that information asymmetry and agency between plaintiffs and their lawyers are important. We show evidence that the treatment increases the discounted payout to workers. The results replicate across two separate implementations.
JEL: K31, K41, K42, J52, J83
Keywords: firing disputes, labor courts, settlement, overconfidence, statistical information

* Sadka: ITAM, jsadka@itam.mx; Seira: ITAM, enrique.seira@itam.mx; Woodruff: University of Oxford, christopher.woodruff@qeh.ox.ac.uk. Acknowledgements: We would like to thank Sebastian Garcia, Andrea Fernandez, Isaac Meza, Diana Roman, and Monica Zamudio for superhuman research assistance, Vince Crawford, Jonas Hjort and Bentley MacLeod for helpful discussions, and seminar and conference participants at Cambridge, Columbia, ITAM, Monash, NYU, Abu Dhabi, Oxford, Tinbergen, the Latin American Workshop on Law and Economics, and the American Law and Economics Association for comments. All errors are ours. We acknowledge financial support from the Government Partnership Initiative of the Abdul-Latif Jameel Poverty Action Lab (JPAL) at the Massachusetts Institute of Technology, the Economic Development and Institutions program funded by the UK government through UK Aid, and the Asociacion Mexicana de Cultura. We also acknowledge crucial institutional, operational, and human resource support from the Mexico City Labor Court and its president, Darlene Rojas Olvera. The research reported in the paper was carried out with the approval of the ITAM Institutional Review Board. The trial registration number is AEARCTR-0002339.

I. Introduction

Well-functioning courts underpin markets and constrain private power in developed markets, but courts function poorly in most developing countries. Outcomes are unpredictable, parties are misinformed, and inefficient processes lead to slow decisions and large case backlogs. In addition to hampering the functioning of markets, poorly functioning courts raise concerns for justice. Although the ineffectiveness of courts in developing countries is widely understood and anecdotal evidence of corruption and inefficiency in the legal systems is widespread, little rigorous evidence exists regarding the underlying causes of the courts' ineffectiveness. Formulating effective reforms leading to better-functioning institutions is impractical without a more detailed understanding of why the institutions fail.

With this knowledge gap in mind, we work with the Mexico City Labor Court (MCLC), using data from historical case files and a randomized field experiment conducted with ongoing cases to examine underlying causes of inefficiency in the court. The MCLC receives more than 30,000 filings per year from workers who claim to have been involuntarily separated from their jobs and seek severance pay due to them according to Mexican labor law.

Courts are a disciplining device for a bargaining game between the parties to the case. Parties settle most labor law disputes before they reach a court judgment, international data show. One notable feature of our data is that settlement rates are low by international standards, even though a large share of the cases involve low stakes and last many years. This suggests potential inefficiencies in bargaining between the parties. An extensive literature examines bargaining breakdowns and delays. The canonical Rubinstein (1982) bargaining framework shows that bargaining outcomes are immediate and efficient where information is complete, delays are costly, and parties bargain by making alternative offers. 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 breakdown or be delayed if parties are overly optimistic.[1] Our data indicate 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 the lawyers representing either side. This raises potential agency issues that, theoretically, may result in either more or less efficient outcomes. Gilson and Mnookin (1994) model court cases as prisoners' dilemmas in which the parties play once and the lawyers play repeatedly. As such, the lawyers may cooperate when the players would not, generating more efficient outcomes. Ashenfelter and Dahl (2012) examine data from arbitration cases involving emergency services unions and municipalities in New Jersey. In a context in which the parties sometimes represent themselves and sometimes are represented by lawyers, they show

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

that lawyer-agents provide positive benefits to the party they represent.

But agency may also lead to inefficiencies if the incentives of the agent and principal diverge. In the cases we study, lawyers on both sides typically specialize 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. 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 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] Even though private lawyers in the MCLC cases almost always receive a share of the award collected by their plaintiffs, we argue analytically that differences in discount rates and the assessment of risk will cause them to have very different preferences over settlement options. These differences provide an underlying rational for potential agency issues.

We begin by analyzing the information environment of the court. We digitize more than 5,000 completed cases from the court's historical records, 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 stipulates 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: fewer than 60% of cases are settled in Mexico, compared with 80% to 90% in higher-income countries. A large majority of dismissed workers (plaintiffs) are using the court for the first time. Our surveys show that a typical worker has little knowledge of her legal entitlements; more surprisingly, plaintiffs - particularly those represented by private lawyers - are often uninformed even about the contents of their own 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 expected size of the award are optimistic relative to predictions based on historical cases.

With the goal of identifying the role of misinformation and overconfidence in generating low settlement rates, we conduct an experiment that changes the information available to the parties in randomly selected ongoing cases.[3] The experi-

[2] Hubbard (1998) and Hubbard (2000) suggests that reputation is effective in controlling agency in the automobile emissions testing market in California. The fact that plaintiffs typically use the labor court only once and that users are seldom connected to other users undermines the development of reputation by lawyers.

[3] Most of the research on agency in relationships between principals and expert agency is all based

ment is carried out in two phases in ongoing cases. The Mexico City Labor Court allocates the 30,000 cases it receives each year to one of 20 subcourts. Between March and May 2016, we worked with active case files from a single subcourt, randomly assigning each case to one of two treatments or to a control group. In the first treatment, the "conciliator treatment," parties were asked to confer with a conciliator employed by the court. The conciliator acts as a neutral, non-binding, mediator. One conciliator works in each subcourt of the MCLC. In the second treatment, the "calculator treatment," all parties present at the hearing received customized statistical predictions of their case's most likely outcomes.[4] We used machine-learning techniques and 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 that are still ongoing four years after being filed, which served as a measure of expected duration; and the peso value of the awards collected by the plaintiffs in similar 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 calculator treatment. 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 differed from phase 1 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 focus in the second phase on cases holding their initial hearing. We noticed in the first phase that the effect on settlement was largest in recently-filed cases. Finally, we worried that the presence of our research assistants might be affecting outcomes. So, in Subcourt 7, where we conducted phase 1 of the experiment, we added a placebo treatment just after the end of phase 2.

Our main experimental outcome is whether the case was settled on the day of the intervention or before December 2018, the latter based on administrative records 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 recently-dismissed, low-wage worker with a high discount rate, she benefits from receiving payment earlier.

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,

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, Kerschbamer and Sutter (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.

[4]Each of the two treatments is relevant for current policy discussions in Mexico: in 2017, Mexico passed a constitutional reform mandating a conciliation hearing before bringing labor suits to court. Providing statistical information is being discussed in current labor reforms.

the settlement rate increases only in the 18% of hearings in which the worker is present at the court on the day of the intervention.[5] Conditional on the worker being present, settlement rates increase from around 24% in the control group to 40% in each of the treatment groups. Administrative data from December 2018, 18 to 30 months after the day of treatment, indicate that an additional 35% of the cases in the control group are settled after the day of our intervention. But the treatment effects remain constant over that time. That is, all of the treatment effect occurs on the day of the treatment. The constant and persistent nature of the treatment effect has two implications. First, the fact that the treatments have a fairly precisely measured zero effect for those cases in which we delivered the treatment only to the plaintiff's lawyer, both on the day of the intervention and two years later, is consistent with lawyers not conveying the information to their clients. Second, 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. The results from the calculator treatment suggest that the plaintiff's lawyers retain influence over decision making by controlling the flow of information to clients.

We show that the settlements induced by treatment likely leave the plaintiff's between off compared with the counterfactual of continuing to a judge's ruling. We match the cases on observables to a set of cases ending in a judge's decision in our historical files. We find that the plaintiffs in our treated cases recover larger awards in present-value terms than those in the matched historical cases.

Our finding that information affects decision making resonates with results found in wide range of other contexts. Improved information has been shown to improve decision making and the functioning of private markets (Andrabi, Das and Khwaja (2017) ; Belot, Kircher and Muller (2018)), and to improve schooling decisions (Jensen (2010); Dizon-Ross (2019)). Information has also been shown to be useful in improving political institutions (Chong et al. (2015); Reinikka and Svensson (2011)). Our results show that it is important that the information be conveyed directly to the party affected by the decisions.

The paper also relates to 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 the use of variation in historical circumstances (e.g., Acemoglu, Johnson and Robinson (2001)). More recently, attention has shifted to the question of how to reform poorly functioning institutions, with a particular interest in the value of making incremental reforms.[6] The results here suggest that a modest, scalable and transparent change has a significant effect on the functioning of the courts.[7] The experiment also illu-

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

[6]See, for example, Khan, Khwaja and Olken (2016) and Khan, Khwaja and Olken (2019) for examples of experiments testing incremental reforms in the collection of taxes in Pakistan.

[7]There is a very small literature focused on reforms in courts in particular. Kondylis and Stein

minates 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 an important area for future research.

Finally, 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) show 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 a 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 lead to increased 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 II. We then detail the data from both administrative records and surveys of litigants and lawyers in Section III. Section IV uses those data to describe a set of stylized facts that motivate our experiment. Section V describes the experimental protocol and result. Finally, we discuss the implications of the results and conclude.

II. 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. 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 three 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 four years to conclude its current cases even if no new cases were filed. Worker dismissal lawsuits make up over 95% of filings. The MCLC has 20 ”special labor courts” (which we refer to as “subcourts”), each with a jurisdiction based on the industry in which the defendant firm operates. In the first phase of the project, we worked with the subcourt that deals mainly with the services side of automotive, transport and

(2018) examine the effect of an administrative reform on civil case duration in Senegal; and Ponticelli and Alencar (2016) use variation in court enforcement across states in Brazil to study the effect of bankruptcy reform on access to credit.

retail gasoline industries. In phase 2, we expanded to four additional subcourts specializing 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. The law provides few legal bases for "justified dismissal", so that firing a worker due to low productivity or poor market conditions is considered unfair dismissal and requires 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.[8] 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.[9] 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.[10] 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 to 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 officers of the court. This is followed by adjudication of liquid assets or sale of non-liquid assets to pay the worker the awarded amount. Given that an interval of six-months between hearings is typical, the fact that there are more than four hearings held in an average case, and the frequent postponement of scheduled hearings due to lack of notification,

[8] 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.

[9] 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, rates of successful notification more than double.

[10] 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.

the average lawsuit continuing to a judge's decision takes over three years. This in spite of the law stipulating that lawsuits should have a maximum duration of 100 days.

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

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

Private plaintiff's lawyers typically charge an initial fee of about MXN$2000 pesos (USD 100) to file the lawsuit and a contingency fee of about 30% of any amount collected by the plaintiff. In spite of the contingency fee, their incentives are not perfectly aligned to those of their clients. First while plaintiffs are party to a single case, the lawyers manage a portfolio of many firing lawsuits with widely differing characteristics, against many different firms. With diversified risk, they may be more willing to take risks on any given case. Second, filing a low-quality suit is cheap and easy and the lawyers may profit from collecting the filing fee even with no expectation of recovering anything on behalf of the worker. The plaintiff must ratify settlements of the lawsuit in person, as well as acceptance or rejection of an offer of reinstatement, should the plaintiff's side receive one. Lawyers typically do not bring the plaintiffs to hearings nor do they provide them much detail on the developments in the lawsuit. As will be shown below, the physical presence of the worker at the hearing in which we intervene in the field experiment is crucial for the effectiveness of our intervention.

III. 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, where both parties had been notified and were therefore obligated to attend the hearing. 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.

A. 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 judges decision. In order to increase the sample of cases concluded by the judges decision, we reached back to lawsuits filed in Subcourt 7 in 2009 and 2010, identifying 241 case files concluded by a judges 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.[11]

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 information 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 workers 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,

[11]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.

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, the parties may reach a post-judgment settlement, with the worker accepting a lower payment to avoid the high costs of enforcing payment; and third, the worker may be unable to collect from the firm, since the firm may have no assets that can be seized by the time the judgment is enforced. The details of the judges 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 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 on-going 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 the data coding about six months after the start of each phase of the experiment, and in December 2018. 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.

B. Survey Data

We collect survey data for the active cases that are included in the experiment. In the first phase of the experiment, we conducted an 8-10 minute survey with each side appearing at the court for these hearings.[12] 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

[12]We usually surveyed the plaintiff if she was present, and her lawyer if not. Lawyers for the plaintiff and defendant were almost always present, but the plaintiff was present only 18% of the time, and the defendant only 1.4% of the time. 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% of the cases. Survey compliance rates are detailed in Table C5 in Appendix C.

or were actively searching for one. Finally, we also included questions intended to test the plaintiffs 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.

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% of employees did not complete middle school. Plaintiffs with public lawyers were more likely to attend the hearing: 29% of workers present had a public lawyer, while only 10% of the case files in the experiment had a public lawyer. Of those workers who showed up and had a private lawyer, most (nearly 82%) said they would pay a fraction of the award (30%, on average) to their lawyer. Almost three-quarters (73.3%) say they are mad or very mad with their employer. Only 7.6% 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 was 58%.

C. Construction of the calculator

In the experiment, we provide 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 of the calculator is described in detail in Appendix A, but we summarize the main points here. The main outcome variables we wanted to predict were amount received by the plaintiff conditional on receiving a positive amount of compensation, the duration of the case and probability the case ends in settlement, judgment with zero recovery judgment with positive recovery, expiry, or by being dropped. 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 s/he is an at-will worker, the specific claims in the case (reinstatement, overtime, back pay, vacation pay,

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

Christmas bonus, statutory profit sharing, severance pay) and the industry of the firm. For each outcome, we used cross-validation to chose the model and variables with 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 the models and highlight those 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 ends in each of the five possible endings. For each of these five endings, we showed the expected amount recovered by the plaintiff. We then use 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 sheet to both sides of the case. We adjusted the format for phase 2, 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 suggested that we provide the expected settlement amount, conditional on characteristics, and then provide each side with data indicating the contingency they faced if they did not settle. We developed separate templates for the plaintiffs and defendants which are shown in Figure 6. For the worker, the no-settlement contingency was the percentage of cases where workers collected nothing, and for firms it was the average amount collected by plaintiffs that won judgments. For the firms, we also showed the recovery amount implied by the law. In addition to using the calculator as a treatment in the experiment, we use it to build a proxy of average overconfidence, as we describe below.

There are 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 the historical data used in the calculator models cover more than 24 months after filing, few of the 29% of historical cases that were unresolved would end in settlement. Therefore, the projected average payment for cases ending in settlement - the most important variable in the calculator information - is not affected by this censoring issue.

For cases 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 for any specific case. Parties may have information about the strength of their case that is unobservable to us. Again, we made clear to the parties that the predictions were based on average outcomes, and outcomes of individual cases will vary depending on the circumstances of the case.

IV. 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 when the source of data for each stylized fact is the historical administrative data or survey data.

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

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% of cases end by settlement, 20% are dropped, 8% end because the time limit for the case runs out, and 9% end with a judges decision.[15] 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

[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.

[15]Defendants have a strong incentive to record all settlements with the court because, otherwise, plaintiffs may continue to press their case even after receiving the settlement payment.

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 settlement results in a positive compensation to the worker, in cases ending with a judgment the worker recovers a positive amount only 24% 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. 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% of the minimum legal compensation for their case without back pay, but only 49.9% 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 MXN$2000 (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 7% of the settlements are also for amounts that imply a negative net present value for the plaintiff.

Fact 2. Long suit duration (Historical Data): *30% 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 case ending. Even cases that settle take some time to do so, with the average settlement occurring 10 months after filing. Trials ending in judgment take 2.4 years on average even conditional on being concluded in December 2015. Given that 30% 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 Subcourt 7 of the MCLC, only 63% of cases are settled over the lifetime of a lawsuit. By way of comparison firing disputes are settled after filing in 79% of the cases in Australia, in 80% of the cases in the United States, and in 90% of the cases in Sweden (Ebisui and Fenwick (2016)).

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

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. 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 you are asking for in your suit among the following...", listing: Constitutional payment, reinstatement, overtime, holiday bonus, Sunday bonus, and insurance. We assess accuracy by comparing the responses to the case file. Panels (c) to (f) of Figure 4 shows the proportion of time the plaintiffs responded correctly to questions regarding elements of their claim. We see that between 20% and 50% 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*[16]*, 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.[17] 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

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

[17]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.

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, $E^s[y_i|X_i, u_i]$, and the calculator's prediction, $y(\widehat{X_i})$. That is: $\gamma(X_i, u_i) \equiv E^s[y_i|X_i, u_i] - y(\widehat{X_i})$. 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, taking the plaintiff as the base group. We report these regressions as summary statistics and do not place any causal interpretation on the estimates. The regressions use data from phase 1 and phase 2 combined, and include casefile fixed effects.[18] 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 subjective probability of winning the case, while the coefficients for the two lawyers can be interpreted as each party's subjective expected outcome relative to the plaintiff's subjective 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 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 from the calculator, $y(\widehat{X_i})$. 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.

[18]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.

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

Private lawyers charge a fee of MXN$2000 (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. Indeed, Figure C6 shows that plaintiffs represented by private lawyers are significantly less knowledgeable about the content of their cases than those presented by public lawyers. With regard to case outcomes we find that, conditioning on five variables coded from the initial filing[19], private lawyers ask for 86% 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 course, the selection of lawyers is endogenous, and the data we report here is only descriptive, making no attempt to adjust for this selection beyond the five control variables described above.

V. 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.

A. The treatments

The first phase of the experiment compares the effects of two treatments - the provision of statistical predictions of case outcomes and the use of court-employed conciliators - against a control group; the second phase compares the statistical predictions against control. During the experimental window, hearings for which both parties were formally notified were assigned to either a treatment arm or a control group. We describe the treatments here, and also a describe a placebo

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

treatment that was implemented in Subcourt 7 during a later period and that was designed to show that experimenter effects are not driving any outcomes.

The Calculator: Subjects in the calculator arm received the personalized prediction of their case's expected outcomes based on the statistical model described above and the covariates of their own case. The predictions were presented in a single sheet of paper like the ones shown in Figures 5 and 6. We extracted the data needed to customize the calculator predictions from the initial filing, and the data were typed into a user interface in the presence of the parties. The predictions were 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. Enumerators gave no additional legal advice. After explaining the calculator information, they asked the parties if they wanted to delay the start of their hearing for a few minutes to negotiate with the assistance of a conciliator. In phase 2, because there was less time available for the treatment, we pre-filled the information needed to customize the calculator 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 additional conciliator to Subcourt 7. 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. 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 prespecified 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 they were interested in discussing a settlement.

Placebo: 13 months after the end of the calculator/conciliator treatments in phase 1 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 the leaflet shown in the Appendix C Figure C7 which describes the role of conciliators in the process. It was provided in format similar to the calculator information, but rather than quantitative predictions it said: "Do you know that you could resolve this conflict today? Conciliation is fast, free, confidential and impartial. Subcourt 7 has conciliators. Ask for help!". Unlike the conciliator treatment, the administrative judge did not stop the hearing to request the parties sit with the conciliator. However, if a party receiving the placebo treatment asked for the conciliators, our enumerators told them where they were situated.

B. Implementation

The implementation of the experiment differed slightly in phase 1 and phase 2. There are two important difference. The first 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 second is we intervened in cases at all stages of the process during the first phase, but focused on cases holding their first hearing in the second phase. All first hearings are held on Fridays.

The first phase of the experiment started in Subcourt 7 on March 2, 2016 and continued daily for 12 weeks. The "subcourt" is not a single courtroom, but rather a room with a waiting area and eight counters conducting simultaneous hearings. Subcourt 7 manages about 55 hearings per day. Each night the court gave us a list of hearings scheduled for the following day, along with their notification status. We worked with the subset of hearings for which both parties were duly notified and therefore required to be present. Among the 20 case files meeting this criterion on a typical day, we excluded hearings scheduled to start at the court's opening hour of 9 AM because the court did not want to delay the start time of the first hearings of the day for fear of causing cascading delays through the day.[20] Note that cases are assigned to hearing times randomly, so our agreement not to consider 9 AM hearings does not compromise the validity of the experiment. On a typical day, this reduced our sample by around 1.5 cases. In what follows we focus on the remaining sample of roughly 18.5 cases per day. The sample cases were at different stages of the process - that is, not all were 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 subcourt 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 assignment for both the parties and our enumerators. Survey tables were separated from treatment tables to avoid contamination across experimental groups. This was

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

possible because our sample was only about 18 cases per day. All the parties present were asked to complete the survey, but compliance was optional and 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 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 assigned experimental condition and given the appropriate treatment protocol described above.

Phase 2 of the experiment involved only the calculator treatment and treatment was randomized at the day level rather than case level. Because first hearings are scheduled only on Fridays, 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 subcourts 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, with cases during two weeks given the placebo treatment daily and cases in two adjacent weeks serving as a control group without any intervention. For both groups we coded the variables in the case file and recorded whether there was a settlement on the day of the hearing.

C. Integrity of the experiment

Table C5 shows compliance rates by treatment assignment for the treatment and the surveys for each phase of the experiment. We define compliance as the parties being present and willing to receive the treatment. The table shows compliance for each party and at the case level. Compliance rates were just over 70% in the both phases. We estimate the intention to treat (ITT) in all reported results. Table C6 in the Appendix C shows that the variables are balanced across the experimental groups in both phases: only 3 out of 23 tests are significant at the 10 percent level. Compliance rates were very similar in the first and second phase of the experiment.

VI. 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 effect 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.

A. 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 (in phase 1 only) conciliator treatment arms. Thus, β_t estimates the ITT effect at a given point in time t. We estimate separate regressions for each t, with t indicating the day of the hearing, or two, six or 24 to 30 months after treatment (the latter measured in December 2018). 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 treatment, while controlling for EP itself. In phase 2, we add subcourt fixed effects.[21]

The first five columns of Table 4 focus the short-term outcome of same-day settlement. The dependent variable is a dummy for whether there was a settlement on the day of the intervention.[22] The first two columns of Table 4 use data from phase 1 of the experiment; columns 3 and 4 use data from the second phase of the experiment; and column 5 combines data from the two phases. Column 1 shows that 6% of the control cases in phase 1 settle on the day of the hearing, while the settlement rates in each of the two treatment groups are approximately 5 percentage points higher, a near doubling of the same-day settlement rate.[23] 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% of the cases settled on the day of the hearing. Recall that the second phase was conducted with cases holding their first hearing, and the higher settlement rate likely reflects this fact. However, the effect of the calculator treatment is quite similar to that in the first phase: settlement rates on the day increase by 4.7 percentage points

[21]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.

[22]We use a linear probability model throughout, but the results are robust to other specifications. Column 1 of Table C12 reports the results of column 5 using a probit specification to show robustness.

[23]Because the treatments induce parties to use the conciliators, we might be concerned that the treated cases use up the available conciliation capacity and crowd out the control group cases, violating the SUTVA assumptions. Because the court assigned an additional conciliator to Subcourt 7 during the experiment, and because the percentage of cases that settle is low, we do not expect spillovers to be an issue. 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. We find no significant spillover effect. These results are available from the authors on request.

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 1 (column 2) and phase 2 (column 4), settlement on the day is much more likely when the employee is present. In the control group, 17% (0.034 + 0.14) of the phase 1 cases and 24% of the phase 2 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 points higher with either treatment when the employee is present in phase 1, 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 between treatment and employee presence is added to the regression (row 2).[24] The effect of the treatment when the employee is present increases settlement rates by enough to significantly close the gap with those of developed countries referenced above.

The phase 2 results provide a replication within the experiment, and the similarity of results in phases 1 and 2 is reassuring. Combining the samples increases statistical power. We do that in Column 5 using the specification from columns 2 and 4. Not surprisingly, we find very similar treatment effects, with the 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.

The regressions in the first five columns measure the effect of treatment on immediate settlement. The results suggest that lawyers do not act on the calculator information in the absence of their client. But might they share the information with their client after the hearing, producing a delayed effect on settlement? The court's administrative records allow us to track cases over time. Columns 6 and 7 show the effect of treatment two and roughly six months, respectively, after the treatment hearing. Column 8 shows the effect of treatment as of December 2018, around 30 months after treatment in phase 1 and around 24 months after treatment in phase 2. 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, while the six-month window occurs after at least one additional hearing and the 24-30 month period after several further hearings in most cases.

Our third main result is that the effect of treatment does not change materially

[24]Table C7 in Appendix C examines balance in key variables in the subsample of cases where the employee is present. Table C17 in the Appendix C tries to predict EP using case characteristics with mild success. Employees are more likely to attend in cases with public lawyers and when they had a long tenure at the firm, and less likely to attend when they worked longer hours at the firm.

at any point up to 30 months after the intervention, even though the number of cases settled overall increases substantially. Focusing first on cases where the employee was not present at the hearing, comparing column 5 with columns 6 through 8, we see that in the control group, the settlement rate increases from 9.5% to 15% two months later, 39% six months later, and 45% by December 2018. But the effect of the calculator when the employee was not present (row 2) remains a precisely estimated zero at all three of the follow-ups. Where the employee was present to receive the treatment, the treatment effect also remains unchanged over time. The 16 percentage point effect on the day of the hearing increases (insignificantly) to 18 percentage points after two months, returns to 16 percentage points after six months and falls insignificantly to 14 percentage points in December 2018.

Treatment is random conditional on the presence of the employee at the hearing, but we might be concerned with the endogeneity of employee presence itself. While this is fundamentally an external validity issue, it is relevant for how we interpret the null treatment effect when the employee is not present. Linking this finding to plaintiff-lawyer agency issues implies that settlement rates would have been higher had the employee been present in the subset of cases where she was not present. We might be concerned that plaintiffs are present when there is potential for the case to be settled, and not present when there is little potential for settlement. However, the December 2018 data suggest that the plaintiff's presence on the day is not determinant of settlement in the longer run in the control group. First, among cases in the control group where the employee *was* present on the day, the effect of the employee's presence dissipates over time; by December 2018 the effect in the control group is no longer significant (column 8 of Table 4).[25] Second, among the control group cases where the employee was not present on the day, an additional 35% of the cases settled before December 2018. Taken together, the results imply that neither the presence nor absence of the plaintiff on the day of the intervention determined settlement in the longer run among the control group cases. On the other hand, settlement in the treatment group was affected by the presence of the employee, both on the day of treatment and in the longer run.

Collectively, these results suggest that the lawyers do not share the calculator information with clients, and hence, that agency issues may be important in this context. We provide one further piece of evidence on this by separating plaintiffs according to whether they are represented by public or private lawyers. We would expect agency to be particularly important where plaintiffs use private lawyers. Supporting this assumption, Figure C6 shows that plaintiffs using private lawyers are significantly less informed about the contents of their case than are plaintiffs using public lawyers. In Appendix C Table C14, we repeat the regressions in columns 4-8 of 4 separately for plaintiffs with private and public

[25] The fall in the effect of the employee's presence from 0.14 on the day of treatment to 0.07 in December 2018 is significant at the 0.09 level. Results available on request.

lawyers. The calculator - employee present interaction is driven entirely by the sample of cases with private lawyers.[26] In cases where the plaintiff is represented by a private lawyer, the calculator treatment increases settlement by 24 percentage points when the plaintiff is present, and not at all when the plaintiff is not present. Settlement is also more likely in the control group when the employee is present, with 11% more of the control groups cases settling on the day. These effects change over time even more starkly than they do in the full sample. By December 2018, the effect of employee presence on the day of the treatment falls to a highly insignificant 1.5 percentage points while the effect of the calculator when the plaintiff was present remains unchanged. The results for the subsample represented by private lawyers are consistent with agency being particularly important for plaintiffs with private lawyers, and are also reassuring with regard to the endogeneity of the plaintiff's presence at the treatment hearing.

To address any residual concerns with the endoegeneity of the plaintiff's presence, in column 9 we use a control function approach (Wooldridge (2015)).[27] Employees are more likely to be present when their hearings are scheduled for one of the first two hearing times (9:00 or 9:30) or the last hearing times (12:00 or 12:30). Hearing times are assigned to cases randomly, and we find that a dummy variable indicating the two early / two late hearing times is highly significant in predicting employee presence.[28] The results indicate that the control function variable itself is not statistically significant at customary levels, a finding consistent with the suggestion from column 8 that the employee's presence is not determinant in the longer run. Moreover, while the control function increases both the magnitude and the standard error of the employee presence variable, it has little effect on the interaction between employee presence and treatment.

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 the information directly. Even if this simply reflects the difficulty lawyers have in explaining the calculator to the plaintiffs, the lack of a treatment 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.[29]

A final result, shown in Appendix C Table C11 is that the placebo has no effect on conciliation. The placebo provides parties with very general information about the conciliation process, but no information on their own case. We interpret the

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

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

[28] The first stage regression is shown on Table C12. We might worry that the time of the hearing affects settlement for reasons other than the plaintiff's presence. We can not rule this out, though we note that in the control group, the time-of-hearing dummy does not significantly predict settlement when the employee is not present (p=0.65)

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

lack of any effect of the placebo treatment as evidence that the content of the calculator information matters.

B. *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 experience 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 came to the hearing. We were able to administer the baseline survey to 162 of those workers, 53 of whom were overoptimistic relative to their calculator prediction. 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 we present results on expectations updating, these issues lead us to view the the evidence as only suggestive.

We measure the change in expectations as 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".Table C16 regresses relative updating against treatment, focusing on the sample of those who were overconfident at baseline.[30] We find weak evidence of de-biasing among plaintiffs, the group we expect would most affected, given their lack of experience with the court. Column 1 shows that overconfidence decreases by around 30% (40%) from the calculator (conciliator), significant at the 10% level. As noted, given the small and selected sample, we view these effects as only suggestive.

VII. 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, uninformed, and almost always the only party using the court for the first time. We therefore focus on the plaintiffs in our empirical analysis.

[30]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. 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.

To aid in interpretation, we write down a simple model that 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, *even with* a contract under which the lawyer is paid a share of the settlement. 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 discussing the framework, we examine empirically the effect of the increased settlements on plaintiffs, focusing on the amount they receive. 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.

A. A framework for Understanding Incentives

Our main empirical findings so far are: (1) workers and firms have biased expectations on average; (2) the fraction of filed cases that are settled increases when the calculator information is provided; (3) but only when the employee is present. These facts could be explained by several models, and we do not claim that the simple framework we write down here is the only way to rationalize them. 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 three utility functions, $(U_w(x), U_l(x), U_f(x))$, defined over the amount awarded to plaintiffs for the plaintiff (w), the plaintiff's lawyer (l), and the firms(f)[31], and subjective distributions $g_k(x)$ for $k = \{w, l, f\}$ for the amount that party k recovers (pays) in a trial. We define the *settlement range* between the plaintiff and defendant as $I_{w,f} := \{x \mid \mathbb{E}_w U_w \leq x \leq \mathbb{E}_f U_f\}$, where expectations are taken with respect to $g_k(x)$ and the utility functions incorporate attitudes toward risk over uncertain outcomes. If the utility over outcomes differs for plaintiffs and their lawyers, then we can also define a similar settlement range between firms and plaintiff's lawyers as $I_{l,f} := \{x \mid \mathbb{E}_l U_l \leq x \leq \mathbb{E}_f U_f\}$.

We show three claims that follow from this simple framework.[32]

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

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

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 says that even with the same subjective $g(x)$ for all parties, if plaintiffs are more risk averse than defendants[33] then the settlement range between the worker and firm is non empty. Moreover, if the worker is more risk averse than her lawyer, then the settlement range between the firm and the plaintiff's lawyer is a subset of the settlement range between the firm and the plaintiff. Therefore, the worker will be willing to accept some settlement offers that her lawyer will want to reject. The difference in the circumstances governing risk creates a potential conflict of interest between the plaintiff and her own lawyer.

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

Claim 2 relaxes the assumption of the same expectations over outcomes to allow for overconfidence. We show that if the worker is overconfident (i.e. g_w^o first-order stochastically dominates g_w) then the settlement range shrinks. Indeed, if the parties are sufficiently optimistic, the settlement range will be empty. 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. 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 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

[33]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.

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 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 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.[34] 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.

B. Empirical Evidence on the Induced Settlements and Plaintiff Welfare

Our framework shows that, under reasonable assumptions, the rate of settlement is suboptimally low from the perspective of the plaintiffs. But the 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, we next carry out an empirical exercise to provide evidence on the welfare of the plaintiffs induced to settle through the treatments.

Our aim is to estimate what the settled cases in the treatment groups would have collected had the treatment not induced them to settle. Because the treatment effects remain very stable for 24 to 30 months after the day of treatment, the relevant counterfactual is continuance to a court judgment. This counter-factual amount is, of course, never observed in actual data. Our approach is to construct the counter-factual by matching the treated cases that settle on the day with cases having similar measured characteristics in the historical data that ended in

[34] 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.

a court judgment.[35] 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.

We match on seven case characteristics: the minimum entitlement according to law, the worker's daily wage, working hours and tenure, whether the worker is represented by a public lawyer, is female and was an "at will" worker. The monetary variables (daily wage, legal entitlement and the recovery by the plaintiff) in all of the historical and experimental cases are first adjusted by inflation to reflect real values as of June 2016. We then calculate the present value of the plaintiff's recovery as of the date each case was filed, using the discount rate reported by the median plaintiff in our surveys (50 % annually).[36] We show results using both covariate and propensity score matching.

Panel A of Table 5 shows the average treatment effect (ATE) using the cases in the treatment sample that conciliated together with matched samples constructed with nearest-neighbor matching. Columns 1 and 2 show results using covariate matching for the 1 and 3 nearest neighbors, respectively. The matching produces samples that are well balanced on covariates and that pass unconfoundedness tests suggested by Imbens (2015).[37] The NPV of the settlements in the treatment sample is larger than that in the matched historical data, with the result in the second column significant at the 10 percent level. Columns 3 and 4 repeat the same process but use the bias-adjustment suggested by Imbens (2015) to correct for any remaining differences in covariates between the treatment and matched samples. The bias adjustment increases the measured effects slightly, such that the treated settlements have NPVs that are significantly larger than the historical counterfactual when matching to either one (at the 10 percent level) or 3 (at the 5 percent level) neighbors. Finally, columns 5 and 6 of Table 5 show that the results are robust to matching on propensity scores rather than the covariates.

As with any matching analysis, we can not rule out the possibility that there are unobservable differences between the set of settlements induced by treatment and the matched historical cases. Note first that the results on Table 4 indicate that it is reasonable to assume that the cases induced to settle by the treatment would have continued to judgment - the treatment effect is stable for 24 to 30 months, and settlements that long into to processing of a case are very rare.[38]

[35]This imputation is subject to bias to selection on unobservables. We discuss below the results of a series of tests on the reasonableness of the counterfactual suggested by Imbens (2015).

[36]Figure C5 in Appendix C shows the discount rate data elicited from surveys, along with comparable data from the Mexican Family Life Survey for 2009. Microcredit interest rates in Mexico are closer to 100% per year. The results shown below in column 4 of Table 5 remain significant with discount rates as low as 42% and remain positive with discount rates as low as 25%, the latter rate lower than credit card rates in Mexico.

[37]Panel A of Appendix table Table C8 shows that the treatment and counterfactual samples are balanced on the case variables. Imbens (2015) suggests testing uncounfoundedness by examining whether treatment affects outcomes that pre-date treatment ("pseudo-outcomes"). In panel B of Table C8, we show that treatment does not predict legal entitlement, daily wage or tenure in the matched sample, providing some reassurance that the matched historical outcomes reflect a valid counterfactual.

[38]This is the best-case scenario for the plaintiffs in these cases. Table C13 shows that, as of December

Table 5 would show and upward bias if plaintiffs with stronger than average cases chose to settle as a result of treatment. We believe the opposite is more likely: that plaintiffs with the strongest cases are more likely ignore the calculator and continue with their case. But we have no way of testing this supposition. We also note that the analysis in 5 is conservative in that the comparison ignores the added uncertainty inherent in proceeding to judgment. Risk averse plaintiffs will benefit from the reduced risk settlement provides.[39]

In sum, under reasonable discounting assumptions, the matching analysis shows that the treatments leave the plaintiffs either better off. The gains come from receiving payment earlier. Factoring in risk and the likelihood that the additional settlements come from cases with relatively weak unobservable characteristics, we conclude that is it highly likely the plaintiffs are benefited by the treatment.

VIII. Conclusion

Data from case filings 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 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 for 24-30 months following the treatment. These results are consistent with agency issues between the plaintiff and her lawyer. That these agency issues occur in a setting in which the agent receives a share of the court award may seem surprising. But we show that even a reward-sharing contract will not align incentives of the principal and the agent when the two parties assess the cost of risk differently or have different discount rates, both of which are likely in our context. These results have important policy implications for bargaining impasses that happen in many similar contexts. The calculator is easily scalable 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 dis-

2018, the treatment with employee present group has 8.4 percentage points fewer court judgments, but also 2.3% fewer dropped cases. Table C9 shows that instead using the full set of treatment group cases that settled before December 2018 implies somewhat larger benefits for plaintiffs.

[39] An alternative approach would compare cases in the treatment group with those in the control group, taking averages of the settlement amounts for cases settling on the day. In effect, this gives a local average treatment effect driven by those who settle given treatment but would not have settled without treatment. Ex ante, it is not clear what we should expect from this comparison. But results from this exercise - available on request - show a small and statistically negative effect of treatment on the average settlement amount.

pute 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 results also point to opportunities for future research. In particular, the results 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.

Finally, the results highlight 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 scalable 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.[40]

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

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IX. Tables & Figures

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

Note: 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

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

Table 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

Note: 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	Long run	Same day
	Phase 1		Phase 2		Phase 1/2				
	OLS					OLS			CF OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Control (constant)	0.060***	0.034***	0.11***	0.10***	0.094***	0.15***	0.39***	0.45***	0.053
	(0.013)	(0.011)	(0.030)	(0.030)	(0.026)	(0.043)	(0.039)	(0.049)	(0.040)
Calculator	0.051**	0.019	0.047**	0.0077	0.018	0.0035	-0.0069	-0.0025	0.0084
	(0.022)	(0.019)	(0.021)	(0.019)	(0.014)	(0.021)	(0.024)	(0.025)	(0.014)
Conciliator	0.054***	0.033*			0.016	-0.0028	-0.030	-0.053	0.023
	(0.019)	(0.018)			(0.019)	(0.023)	(0.028)	(0.036)	(0.019)
Emp present (EP)		0.14***		0.14*	0.14***	0.11**	0.094*	0.070	0.47**
		(0.050)		(0.072)	(0.041)	(0.046)	(0.048)	(0.050)	(0.21)
Calculator#EP		0.16**		0.16*	0.16***	0.18***	0.16**	0.14**	0.16***
		(0.079)		(0.089)	(0.056)	(0.061)	(0.064)	(0.061)	(0.054)
Conciliator#EP		0.16**			0.16**	0.21***	0.27***	0.20**	0.17**
		(0.074)			(0.071)	(0.079)	(0.075)	(0.078)	(0.074)
Control Function									-0.19
									(0.12)
Observations	1074	1074	1092	1092	2166	2166	2166	2166	2166
R-squared	0.0072	0.12	0.051	0.11	0.13	0.12	0.11	0.087	0.135
Court dummies	NO	NO	YES	YES	YES	YES	YES	YES	YES
DepVarMean	0.095		0.20		0.15	0.19	0.32	0.43	0.15
InteractionVarMean		0.18					0.18		
Calc=Conc	0.88	0.53	-	-	0.94	0.82	0.79	0.40	0.47
Calc#EP=Conc#EP	-	0.98	-	-	1.00	0.58	0.68	0.085	0.91

Note: This table estimates the main treatment effects for both experimental phases. Columns 1 through 5 measure settlement on the same day as the treatment, with the sample in columns 1 and 2 limited to phase 1, the sample in columns 3 and 4 limited to phase 2, and the sample in column 5 combining data from the two phases. Columns 6 through 8 show the effect of treatment on settlement 2 months, 6 months and 24-30 months after the treatment, respectively, using the combined sample from the two phases. Column 9 adds a control for potential endogeneity of the presence of the employee, as described in the text and shown in Table C12. Note that in phase 1, treatment is at the casefile level, while in phase 2 it is at the day level. The 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, a treatment assigned only in 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 hypothesis: Calculator = Conciliator & Calculator*EP = Conciliator*EP. Phase 2 columns include subcourt dummies.

Table 5—: Comparison of Settlement Amounts

Treatment effect. Nearest-neighbor matching						
	Phase 1/2					
	Variable matching				PSM	
	(1)	(2)	(3)	(4)	(5)	(6)
ATE	2580	3536**	3242*	3939**	3698*	3995**
	(1939)	(1715)	(1934)	(1725)	(1900)	(1554)
% ATE	34	47	43	52	49	53
Baseline mean	7598					
Obs	377					
Obs HD	415					
Bias adjustment	NO	NO	YES	YES	-	-
Matches	[1-1]	[1-3]	[1-1]	[1-3]	[1-1]	[1-3]

Note: This table investigates whether our treatments affected settlement amounts adversely for the workers. The table compares the settlement amount for cases that settled on the day of treatment in either the calculator or conciliator treatment with the settlement amount in a matched sample from the historical dataset (HD) that were decided by a judge's ruling. This panel combines phase 1 and 2. In columns 1 through 4, we match nearest-neighbors using direct covariate matching. Columns 1 and 3 use samples of one match for each treatment observation, while columns 2 and 4 match the three nearest neighbors. Columns 2 and 4 include the adjustment for bias induced by any remaining imbalance in continuous covariates as described in Abadie and Imbens (2011) and Imbens (2015). Columns 5 and 6 instead match on propensity score, with column 5 using one nearest neighbor and column 6 using three nearest neighbors. Variables used in matching are: public lawyer, gender, at will worker, tenure, daily wage & weekly hours, and the amount of the plaintiff's entitlement according to the law. Counterfactual and settlement quantities are brought to present value at the time the case was filed using a monthly interest rate of 3.43. Recovery amounts in cases using private lawyers are adjusted for the median lawyer share of 30% of the award, and an initial fee of MXN$2000 pesos.

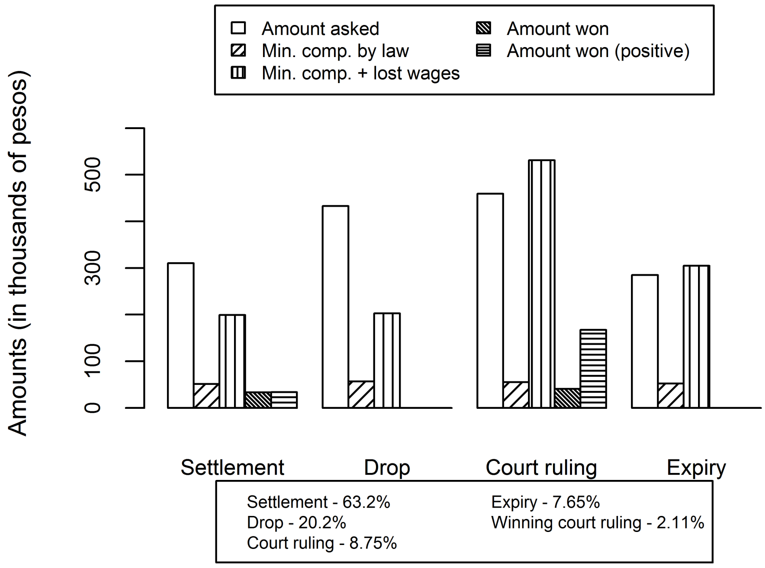


Figure 1. : Differences in Claims and Compensation by case file outcome - Historical Data

Note: 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 in the 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.

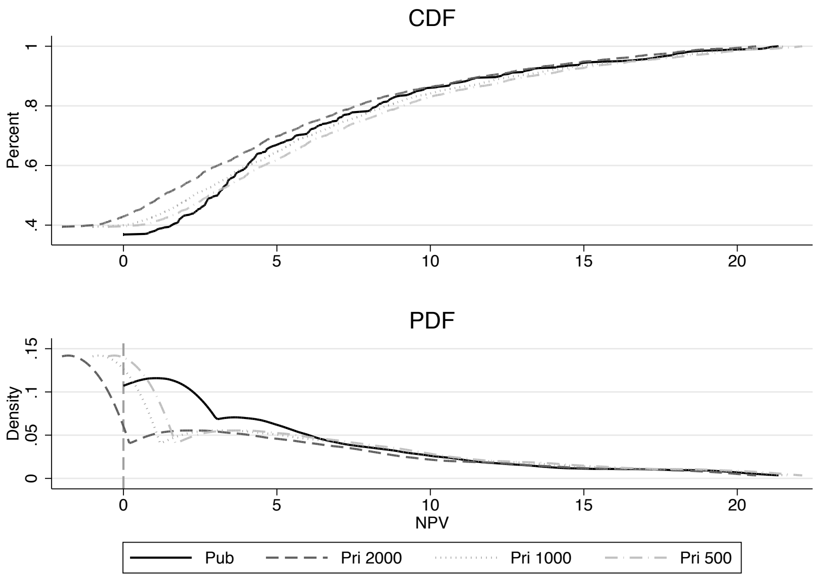


Figure 2. : Distribution of Amount Collected, by Type of Lawyer

Note: 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.

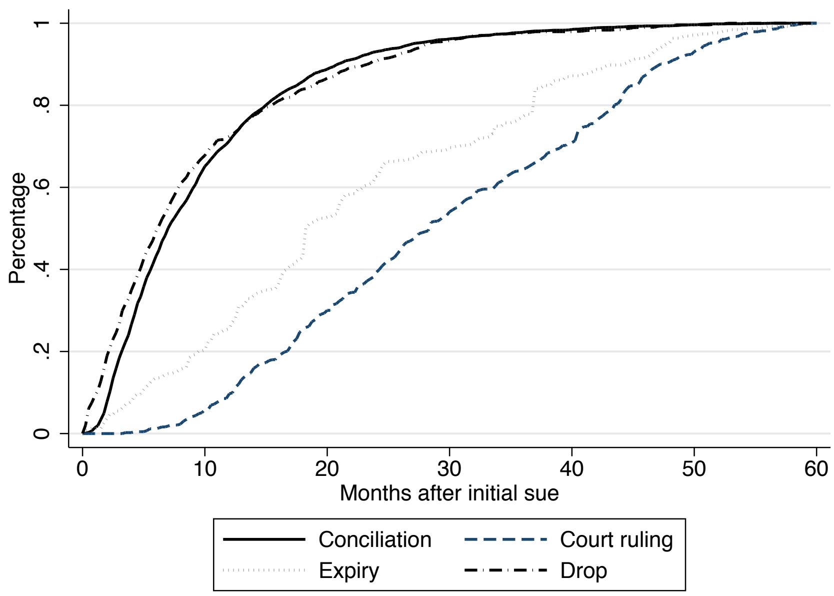
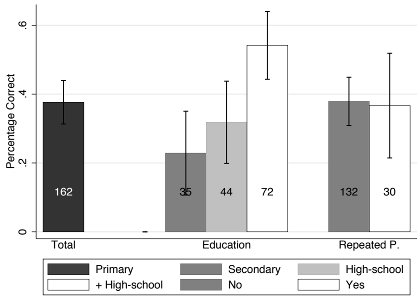


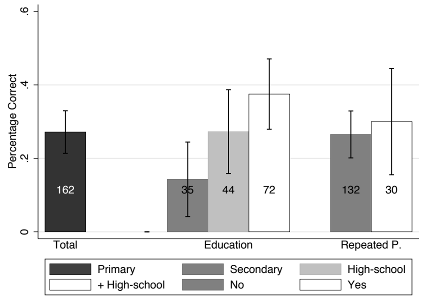
Figure 3. : Time Duration

Note: 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% 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.

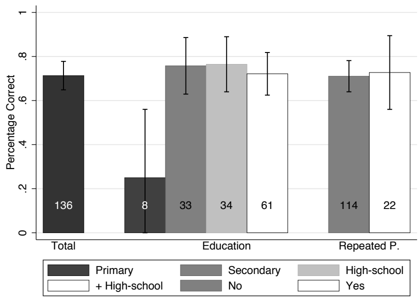
(a) Employee claims to know constitutional days of severance



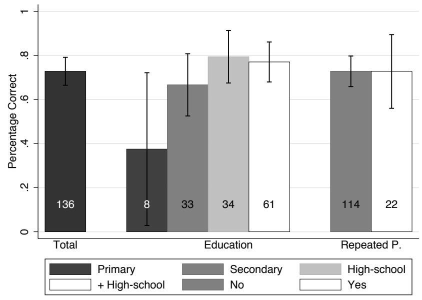
(b) Employee correctly knows constitutional days of severance



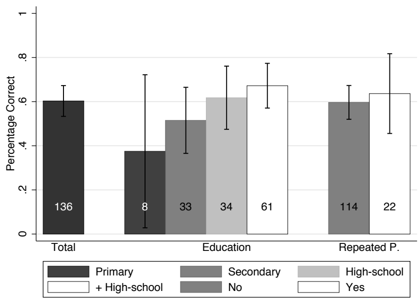
(c) Know if asked for constitutional days of severance



(d) Know if asked for Reinstatement



(e) Know if asked for Overtime



(f) Know if asked for end of year bonus

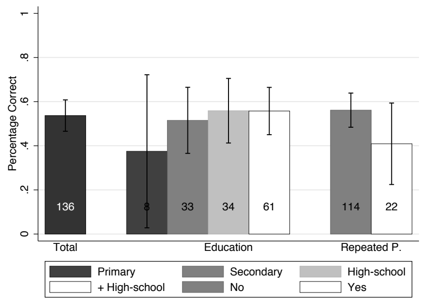


Figure 4. : Knowledge about Law and their Own Claims in Lawsuit

Note: 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"*.

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:

Figure 5. : Calculator Treatment Format (example) - Phase 1

Note: 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.

(a) Plaintiff

JLCA CDMX

CÁLCULO DE COMPENSACIÓN TRABAJADOR

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

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

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

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

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

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

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

(b) Defendant

JLCA CDMX

CÁLCULO DE PAGO DEMANDADO

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

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

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

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

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

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

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

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

Note: 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,.

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

Joyce Sadka, Enrique Seira and Christopher Woodruff *

March 4, 2019

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

*Sadka: ITAM. Seira: ITAM. Woodruff: University of Oxford.

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.[1]

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 dont 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.[2] We therefore estimated models for four bivariate outcomes, using each the following methods: (a) Logistic Regression, (b) Probit, (c) Random Forest[3], (d) Single-hidden-layer Neural Network (20 nodes in the hidden layer and 10% weight decay), (e) Gradient Boosting.[4] Models were estimated in a random sample training set made of 70% of the

[1]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.

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

[3]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.

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

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 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.[5] 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.

[5]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.[6] The four models were: (a) OLS regression, (b) GLM Boosting, (c) Random Forest[7], (d) Ridge Regression[8] [9]. 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 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.

[6] 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.

[7] 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.

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

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

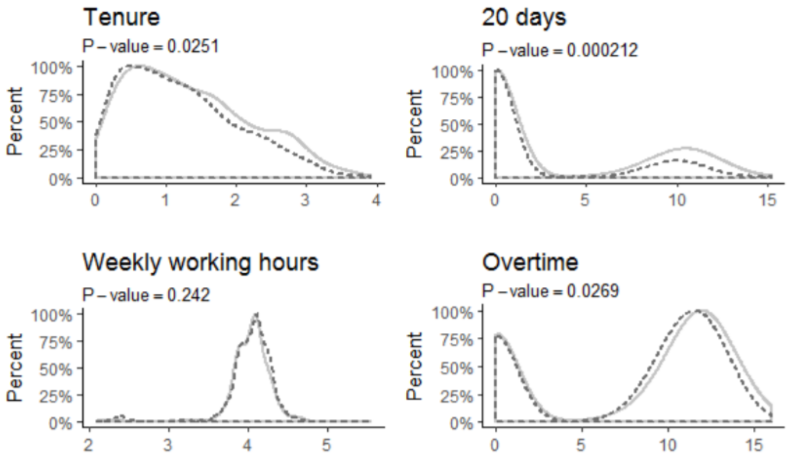
Other issues: Censoring

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.

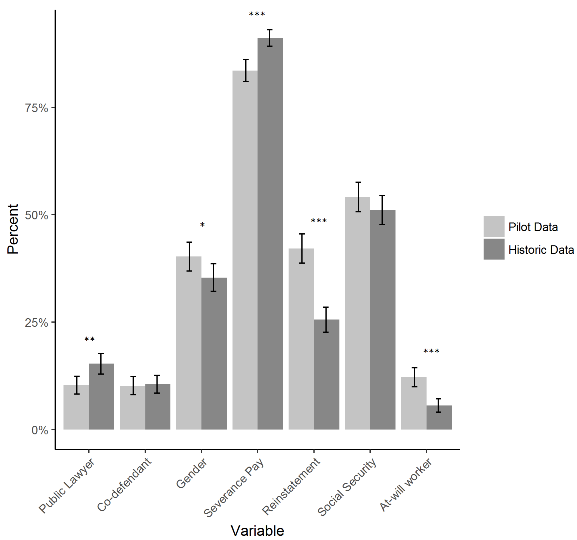
We are aware of this bias and it was communicated to the parties when the calculator information was provided. Although we cannot know how large the bias is, we performed two exercises that suggest it is not large. First, we compare characteristics of ongoing cases with those of the historical cases 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



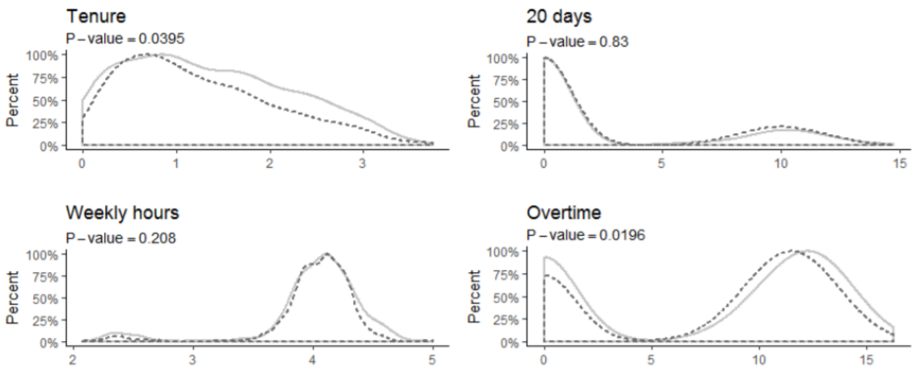
Categorical variables



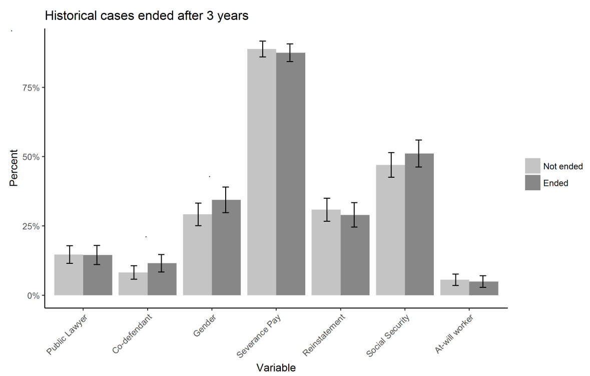
Notes: Covariate distributions comparisons. We compare between -used to calibrate our calculator- and Phase 1 data. All continuous variables are plotted in logs. Color guide is the same for both variable types. Plots for continuous variables show the p-value of a KolmogorovSmirnov test in the subtitle.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

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

Continuous variables



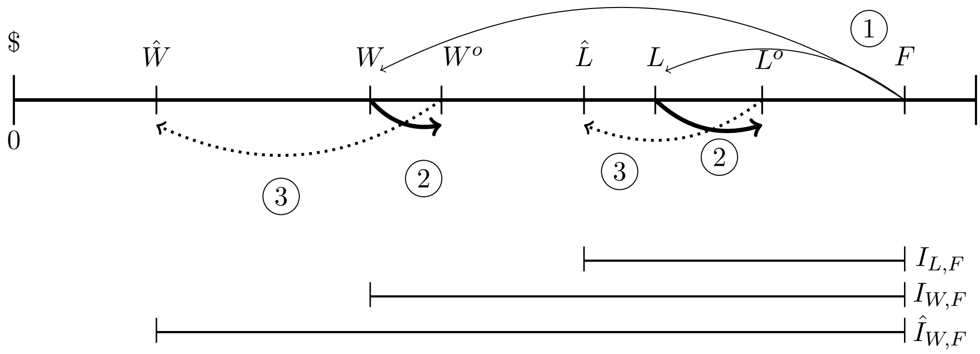
Categorical variables



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 KolmogorovSmirnov test in the subtitle. In the categorical covariates plot, we show significance of a two-sided t-test of differences in means between samples.

Appendix B

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,

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

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.

$\square$

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

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

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

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

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.
Insurance-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.

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

Table C7: Balance regression on characteristics conditional on employee present

	Control	Calculator	Conciliator	Observations	R-squared	Controls
Public Lawyer	-0.013	0.097	-0.27	370	0.041	Subcourt
	(0.096)	(0.11)	(0.16)			
Gender	0.13	-0.24*	-0.084	370	0.077	Subcourt
	(0.088)	(0.12)	(0.16)			
At will worker	-0.055	0.022	0.29	370	0.039	Subcourt
	(0.086)	(0.100)	(0.19)			
Tenure	0.20	-0.32**	-0.14	364	0.058	Subcourt
	(0.13)	(0.15)	(0.19)			
Daily wage	-0.053	0.011	0.37	364	0.021	Subcourt
	(0.053)	(0.060)	(0.35)			
Weekly hours	0.010	-0.020	0.086	361	0.11	Subcourt
	(0.066)	(0.11)	(0.21)			

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

Table C8: Unconfoundedness assessment

(a) Balance in matching covariates

	Control	Treatment	p-value
Entitlement by law	60234.96	57567.95	0.62
	(3400.38)	(4098.63)	
Public lawyer	0.08	0.09	0.77
	(0.01)	(0.01)	
Woman	0.45	0.45	0.86
	(0.02)	(0.03)	
At will worker	0.07	0.06	0.44
	(0.01)	(0.01)	
Tenure	3.82	3.47	0.29
	(0.25)	(0.22)	
Daily wage	535.31	514.78	0.7
	(32.76)	(40.85)	
Weekly hours	58.5	56.79	0.12
	(0.81)	(0.73)	
Observations	416	377	

(b) Pseudo treatment effect

Pseudo treatment effect. Nearest-neighbor matching			
	Phase 1/2		
	Entitlement	Daily wage	Tenure
	(1)	(2)	(3)
ATE	28.3	-2.3	-0.2
	(1162.4)	(11)	(.2)
% ATE	0.05	-0.43	-5.22
Baseline mean	60342.9	536.2	3.8
Obs		377	
Obs HD		415	
Bias adjustment	YES	YES	YES
Matches	[1-3]	[1-3]	[1-3]

Notes: Panel (a) shows the balance in basic variables in the matched sample constructed using 1:3 matching on covariates. This corresponds to the sample used in the regression reported in columns 3 and 4, panel (a) of Table 5. Panel (b) uses the same sample and tests for unconfoundedness by measuring the effect of treatment on three pre-determined variables. If the matched sample represents a valid counterfactual for the treatment group, we should expect that the ATE is not significantly different from zero, which is what all three columns show. Results from both panels are robust to using instead the sample obtained from matching on the propensity score.

Table C9: Comparison of Settlement Amounts

Treatment effect. Nearest-neighbor matching						
	Phase 1/2					
	Variable matching				PSM	
	(1)	(2)	(3)	(4)	(5)	(6)
ATE	4472	4815**	5026*	5056**	6838*	6529**
	(2011)	(1830)	(2042)	(1836)	(1938)	(1648)
% ATE	57	61	64	64	87	83
Baseline mean	7870					
Obs	478					
Obs HD	416					
Bias adjustment	NO	NO	YES	YES	-	-
Matches	[1-1]	[1-3]	[1-1]	[1-3]	[1-1]	[1-3]

Notes: This table is a robustness check considering all cases settled up to December 2018, contrary to cases settled on the same day as Table 5. See the notes to 5 for further details.

Table C10: 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: Each column regresses a characteristic of the case against a dummy variable indicating that the plaintiff recovered an amount less than the MXN$2000 initial filing fee. The sample is all cases from the historical data using private lawyers.

Table C11: 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: The table reports regressions using the full sample and including variables indicating that the case received a placebo treatment, as described in the text.

Table C12: First stage and robustness for the control function regression

	Probit	First Stage (OLS)	First Stage (probit)		CF
	(1)	(2)	(3)	(4)	(5)
Calculator	0.11	0.030*	0.11	0.11*	0.0090
	(0.083)	(0.018)	(0.068)	(0.069)	(0.014)
Conciliator	0.079	-0.023	-0.088	-0.089	0.023
	(0.15)	(0.020)	(0.081)	(0.082)	(0.021)
Emp present (EP)	0.66***				0.45**
	(0.17)				(0.19)
Calculator#EP	0.39**				0.16***
	(0.19)				(0.057)
Conciliator#EP	0.51**				0.17**
	(0.26)				(0.071)
Control Function					-0.18*
					(0.11)
Time instrument		0.069***	0.26***		
		(0.015)	(0.053)		
09:30				-0.12	
				(0.14)	
10:00				-0.36***	
				(0.092)	
10:30				-0.16	
				(0.12)	
11:00				-0.28***	
				(0.093)	
11:30				-0.48***	
				(0.17)	
12:00				-0.078	
				(0.12)	
12:30				-0.0036	
				(0.44)	
Constant	-1.34***	0.095***	-1.25***	-0.94***	0.055
	(0.12)	(0.027)	(0.11)	(0.16)	(0.037)
Observations	2166	2166	2166	2166	2166
R-squared	.	0.013	.	.	0.135
Court dummies	YES		YES	YES	YES
Calc=Conc	0.82	0.018	0.019	0.47	0.53
Calc#EP=Conc#EP				0.91	0.89

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

Table C13: Treatment Effects with different end mode outcomes

	Phase 1/2 (Long run)			
	Settlement	Court ruling	Expiry	Drop
	(1)	(2)	(3)	(4)
Control (constant)	0.45***	0.15***	0.0063	0.18***
	(0.049)	(0.023)	(0.0060)	(0.030)
Calculator	-0.0025	-0.017	-0.0067	0.0068
	(0.025)	(0.025)	(0.0074)	(0.017)
Conciliator	-0.053	0.057	-0.012*	-0.0028
	(0.036)	(0.040)	(0.0062)	(0.022)
Emp present (EP)	0.070	0.0072	-0.015***	0.013
	(0.050)	(0.040)	(0.0053)	(0.029)
Calculator#EP	0.14**	-0.084*	0.011	-0.023
	(0.061)	(0.046)	(0.0083)	(0.035)
Conciliator#EP	0.20**	-0.16**	0.011*	-0.019
	(0.078)	(0.069)	(0.0063)	(0.046)
Observations	2166	2166	2166	2166
R-squared	0.087	0.064	0.011	0.013
Court dummies	YES	YES	YES	YES
DepVarMean	0.43	0.20	0.0088	0.099
IntVarMean	0.18	0.18	0.18	0.18
Calc#EP=Conc#EP	0.38	0.22	0.96	0.93

Notes: This table indicates the treatment effects for both experimental phases taking different type of ending outcomes. See the notes to 4 for further details.

Table C14: Treatment Effects conditional on type of lawyer

	Private				Public			
	Same day	2 months	5 months	Long run	Same day	2 months	5 months	Long run
	Phase 1/2							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control (Constant)	0.092***	0.14***	0.40***	0.47***	0.12	0.14	0.100	0.21
	(0.026)	(0.046)	(0.040)	(0.050)	(0.15)	(0.19)	(0.19)	(0.22)
Calculator	0.015	0.0039	-0.012	-0.017	0.019	0.0067	0.078	0.041
	(0.016)	(0.024)	(0.028)	(0.028)	(0.052)	(0.082)	(0.093)	(0.13)
Conciliator	0.011	-0.0055	-0.039	-0.055	0.023	-0.017	0.014	-0.087
	(0.023)	(0.026)	(0.032)	(0.039)	(0.076)	(0.10)	(0.12)	(0.15)
Emp present (EP)	0.11**	0.091*	0.044	0.015	0.22**	0.18	0.29**	0.32**
	(0.043)	(0.052)	(0.060)	(0.063)	(0.11)	(0.13)	(0.14)	(0.13)
Calculator#EP	0.24***	0.25***	0.25***	0.21***	-0.0082	0.021	-0.15	-0.069
	(0.065)	(0.074)	(0.080)	(0.080)	(0.12)	(0.14)	(0.16)	(0.17)
Conciliator#EP	0.17**	0.20**	0.27***	0.20**	0.28	0.47**	0.29	0.20
	(0.081)	(0.086)	(0.086)	(0.087)	(0.23)	(0.23)	(0.24)	(0.25)
Observations	1811	1811	1811	1811	154	154	154	154
R-squared	0.14	0.12	0.11	0.083	0.13	0.14	0.12	0.12
Court dummies	YES	YES	YES	YES	YES	YES	YES	YES
DepVarMean	0.16	0.21	0.35	0.35	0.18	0.20	0.30	0.30
InteractionVarMean	0.16	0.16	0.16	0.16	0.53	0.53	0.53	0.53
Calc#EP=Conc#EP	0.35	0.49	0.84	0.82	0.19	0.034	0.050	0.25

Notes: The table reproduces the regressions in columns 5 through 5 of 4. Columns 1 through 4 use the sample of cases in which the plaintiff uses a private lawyer and the columns 5 through 8 the sample of cases where the plaintiff uses a public lawyer. We are unable to determine whether the lawyer is public or private in 201 of the cases. The dependent variable is a dummy indicating the case was settled by the time indicated on the column heading. All regressions include sub-court dummy variables as controls. See the notes to 4 for further details.

Table C15: 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: We test for heterogeneity of treatment effects by interacting treatment with the variable shown in the column header. For each test, the first column uses the sample from phase 1, the second from phase 2, and the third from the combined sample. For example, the third column in the top panel shows that, in the combined sample, plaintiffs with higher daily wages (as reported in the case file) were marginally significantly less likely to settle when the plaintiff was present and provided the calculator information.

Table C16: 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 $\frac{exitsurvey - initialsurvey}{initialsurvey}$.

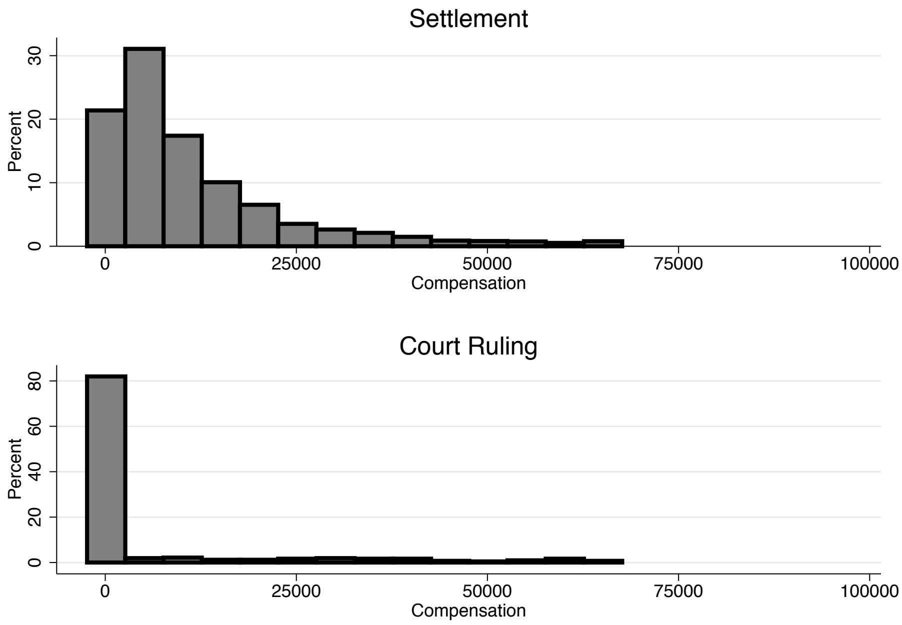
Table C17: Employee presence

	Employee Present		
	Phase 1	Phase 2	Phase 1/2
	(1)	(2)	(3)
Female	-0.043	0.00012	-0.019
	(0.027)	(0.023)	(0.018)
Public Lawyer	0.26***	0.51***	0.37***
	(0.055)	(0.059)	(0.042)
At will worker	-0.061	0.011	-0.028
	(0.039)	(0.042)	(0.029)
Tenure (years)	0.0035	0.0046**	0.0044***
	(0.0023)	(0.0021)	(0.0016)
Daily wage	0.000010	-0.0000073	0.0000049
	(0.0000083)	(0.0000070)	(0.0000076)
Weeky hours	-0.0017**	-0.0029***	-0.0023***
	(0.00081)	(0.0010)	(0.00065)
Age		0.00061	
		(0.0037)	
Constant	0.26***	0.30***	0.27***
	(0.054)	(0.065)	(0.041)
Observations	836	1021	1857
R-squared	0.054	0.12	0.081
DepVarMean	0.19	0.18	0.19
p-value	0.0000015	5.5e-19	9.1e-21
p-value w/o PL	0.018	0.0020	0.000095

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.

C02 Figures

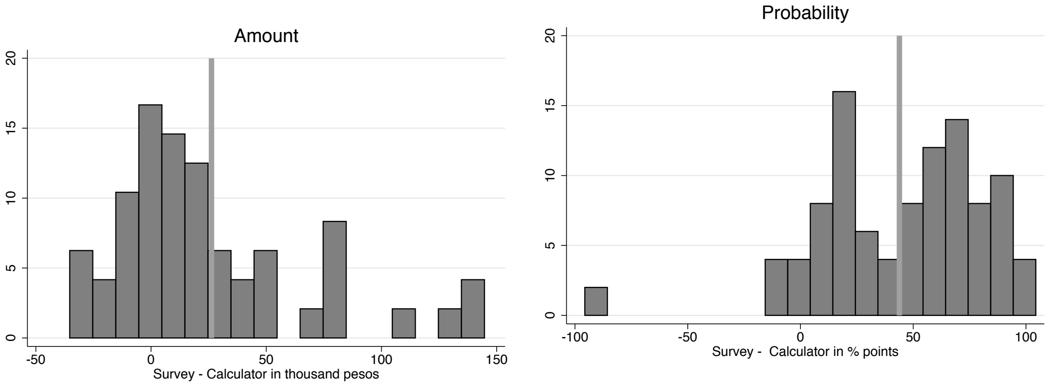
Figure C1: Compensation histograms - Historical Data



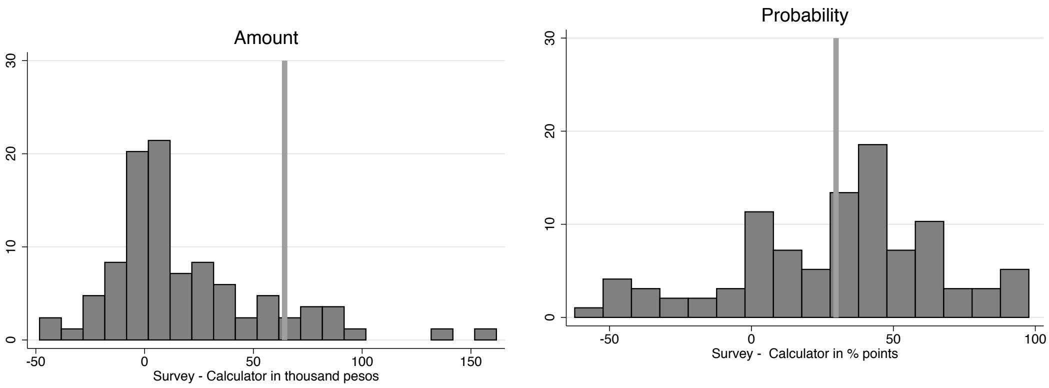
Notes: Distribution of compensation in present value at the time of suing with a monthly interest rate of 3.43, with a 30% cost, and an initial fee of MXN$2000 pesos for private lawyers and deflated into June 2016 pesos.

Figure C2: Subjective expectation minus prediction - Phase 1

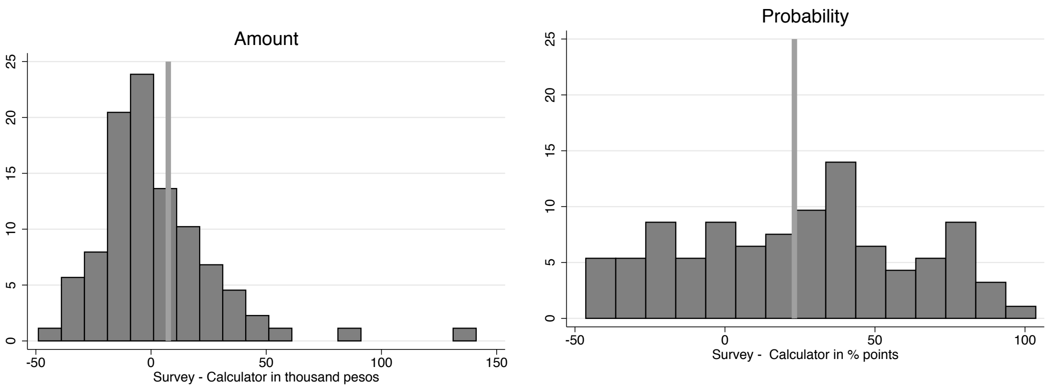
(a) Employee



(b) Employee's Lawyer

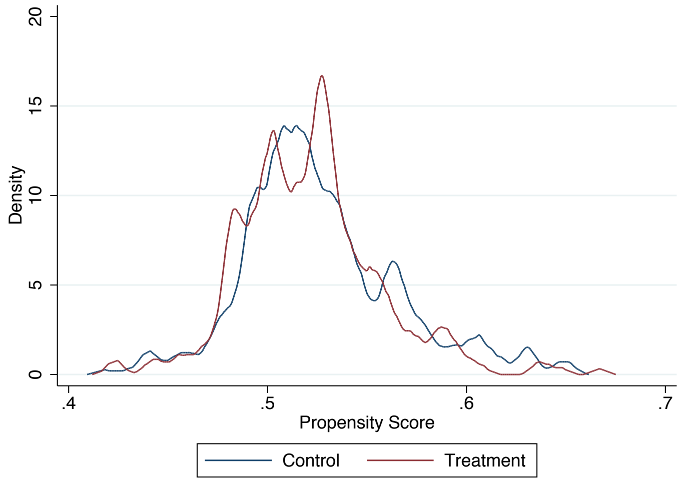


(c) Firm's Lawyer



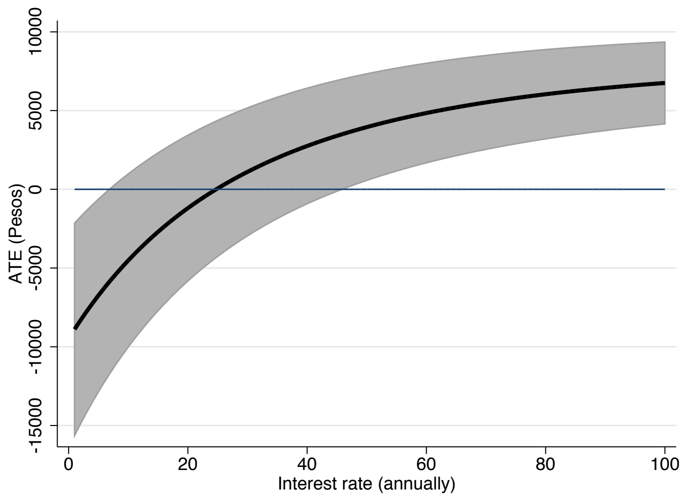
Notes: Difference in thousand pesos for amounts (panel on left) and percentage points for probabilities (panel on right) from what the subject expects vs. what our models predict. Note how, for Employee and employee lawyer, the distribution for amounts is much more skewed to the right than for firm lawyers.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

Figure C3: Propensity score overlap



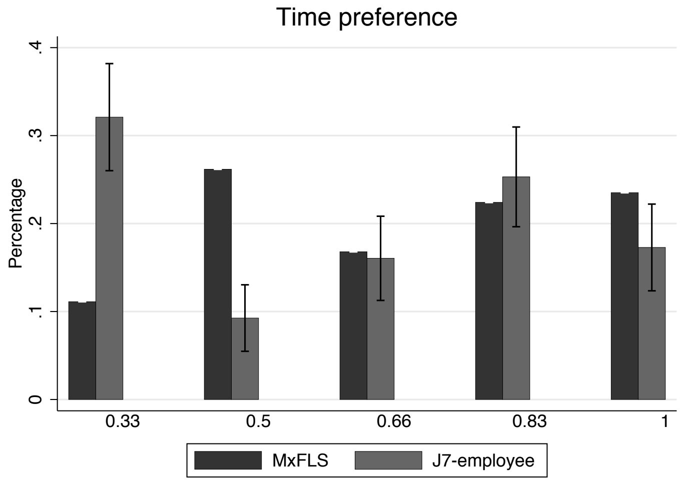
Notes: The graph shows the overlap between the propensity scores after the trimming procedure suggested by Crump et al. (2009) and Imbens (2015) when performing the analysis for table 5.

Figure C4: Discount rate against welfare effects



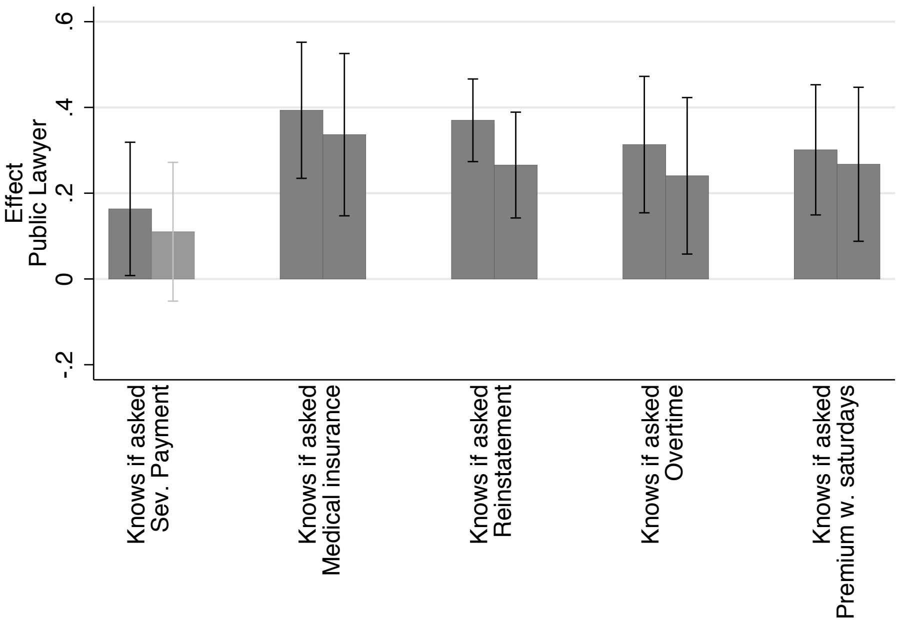
Notes: The graph shows the ATE effect of Table 5 column (4), when using different discounting annual interest rates.

Figure C5: Discount rate for Phase 1 and MxFLS



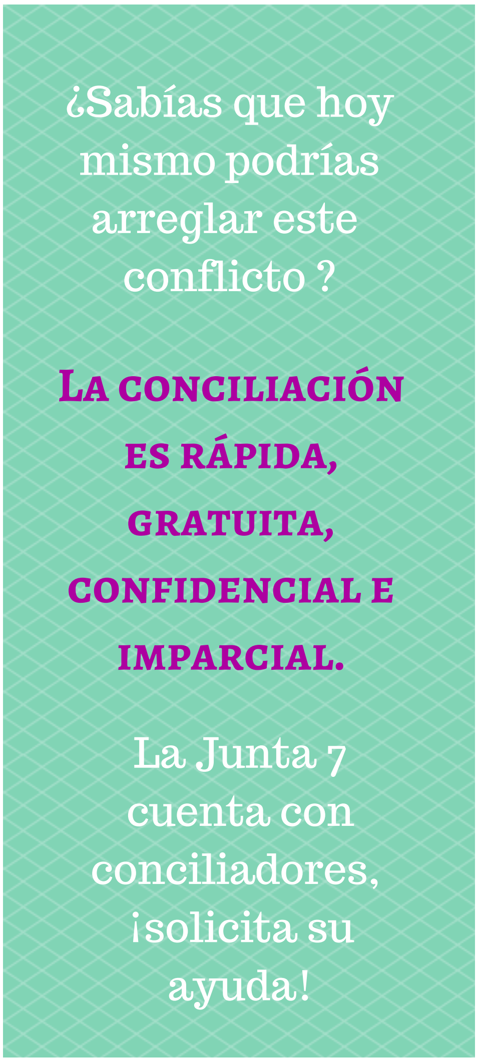
Notes: Comparison of discount rates for Phase 1 data and survey data from the MxFLS (Mexican Family Life Survey- a longitudinal survey in Mexico that follows individuals across rounds).

Figure C6: Prob plaintiff correctly knows what is in the lawsuit



Notes: We regress a dummy variable indicating the plaintiff correctly answered a question about the contents of her case against a variable indicating the plaintiff is represented by a public lawyer. Each pair of bars shows the coefficient from the regression on the case characteristic shown in the label on the x axis. In each pair, the left-hand bar is the coefficient from a regression without any control variables and the right-hand bar is the coefficient from a regression including the six basic case controls. The whisker lines represent the 95 % confidence limits. The data indicate that clients of public lawyers are significantly more knowledgeable about the contents of their cases than are clients of private lawyers for four of the five case characteristics.

Figure C7: 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.

References

Crump, Richard K., V. Joseph Hotz, Guido W. Imbens, and Oscar A. Mitnik. 2009. "Dealing with limited overlap in estimation of average treatment effects." *Biometrika*, 96(1): 187–199.

Imbens, Guido W. 2015. "Matching Methods in Practice: Three Examples." *Journal of Human Resources*, 50(2): 373–419.