Enforceability of Labor Law: Evidence from a Labor Court in Mexico*

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Abstract

We analyze lawsuits involving publicly-appointed lawyers in a labor court in Mexico to study the enforcement of a law that nominally provides high levels of worker protection. We show that, even after a judge rules in favor of the worker, the judgment goes uncollected 56% of the time due to the costs associated with the excessive formalism of the enforcement process. Differences in the probability of receiving compensation after trial, both across lawyers and across workers with different levels of tenure, are not due to differences in win rates at trial, but rather are entirely attributable to post-trial differences in the probability of enforcing the judgment. This paper is the first in the literature that demonstrates the importance of post-trial collection costs on litigation outcomes. We then develop a simple model of litigation that includes costs of collecting awards after trial and show how differences in lawsuit outcomes across lawyers can be rationalized theoretically.
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1 Introduction

There is little dispute that Mexican labor law is extremely protective of workers. Botero, et. al. (2004), for example, perform an international comparison of labor law in which Mexico figures as one of the countries with the most onerous labor regulation from the point of view of firms. An open question, however, is to what extent this extremely protective legislation is actually enforced.

In this paper, we look inside the black box of enforcement and study how labor law is applied to individual lawsuits. Specifically, we analyze alleged unjust-dismissal lawsuits from a labor tribunal in Mexico and study the process through which these suits go to trial, reach out-of-court settlements, or are dropped. Conditional on going to trial, we analyze both court rulings and whether or not the workers manage to collect what has been awarded to them.

One institutional feature we document is that it can be very costly for a worker to collect money that has been awarded at trial by a judge. Consistent with this observation, we find that it is common for trial awards to go uncollected, particularly for cases in which the worker had not worked for long at the firm. In this sense, it can be said that the enforcement of labor law is particularly lax for workers with low (but not trivially low) levels of tenure. The differential enforcement between low- and high-tenure cases is one example of how the highly formalistic process for enforcing judgments, in addition to generating a lax application of labor law on average, also leads to an inconsistent application of labor law.

The previous literature on litigation costs has focused on the costs of going to court, rather than on the costs of enforcing court awards. In order to explain the large percentage of uncollected awards, we need to model **post-trial** collection costs explicitly. We use a simple one-sided incomplete information model and add post-trial collection costs in order to develop testable hypotheses on how outcomes should differ depending on the accuracy of the plaintiff's information and on the plaintiff's costs of collecting an award after the judge has made a ruling. We show that plaintiffs with better information should drop fewer small-stakes cases and more high-stakes cases. We also show that plaintiffs with high costs of collecting awards settle fewer low-stakes cases and may settle more high-stakes cases. We find empirical support for both hypotheses.

We show that informational differences across lawyers affect lawsuit outcomes and that differences in the costs of collecting awards across lawyers affect lawsuit outcomes. Since much of the differences in lawsuit outcomes can be attributed directly to legal formalism, we show that the enforcement of judgments in judicial systems with high degrees of formalism may depend crucially on the lawyer. Those with superior knowledge will be able to recognize the cases that are likely to be enforced, and therefore pursue worthwhile cases at the expense of those that are unlikely to be enforced. Furthermore, enforcement depends in part on whether the lawyer (and plaintiff) believes it is worthwhile to invest the time and effort required to get a judgment enforced. Although our econometric models use variation across lawyers, these insights no doubt also apply to the workers themselves.

Our empirical methodology, in addition to exploiting the fact that we have multiple observations for a given lawyer, exploits the fact that the assignment of cases to public lawyers is essentially random. Assignment of cases to lawyers is based on a short questionnaire that contains only basic characteristics of the case such as the plaintiff's gender and tenure, which we can control for in the econometric models.

We therefore argue that selection of cases to lawyers based on unobservables is quite unlikely. In fact, when we focus on the 19 public lawyers whom we observe at least once in a trial and at least once not in a trial, we do not even find evidence that selection of cases to lawyers is correlated with observables. This essentially random assignment of cases to public lawyers allows us to examine differences in outcomes across lawyers and attribute these differences to the lawyers themselves, not to the unobservable characteristics of these cases.

The outline of the rest of the paper is as follows. In section 2, we review the papers that are most related to what we study. In section 3, we discuss in some detail the legal framework related to alleged unjust-dismissal lawsuits in Mexico. In section 4, we discuss the data we use and present evidence that a significant fraction of tried cases result in an award going uncollected. We also present in section 4 evidence that supports our argument that the assignment of cases to public lawyers is essentially random. In section 5, we present a simple model in which a worker anticipates the possibility that it will be too costly to collect what the judge awards. This possibility affects the entire bargaining process between the worker and the firm and therefore generates several testable implications. In section 6, we present the main empirical results of the paper and relate them to the theoretical model. In section 7, we offer our final conclusions.

2 Related literature

Our paper is related to four strands of literature in law and economics. The first of these strands analyzes how litigation costs affect the bargaining and litigation process. Previous work in this strand has generally shown that the costs of going to court affect the probability of settlement as well as the characteristics of the cases that end up in court.

Bebchuk (1984) develops one of the early theoretical models of bargaining in this strand. He shows that as the costs of going to trial of either party increase, the probability of reaching a settlement before trial increases. Spier (1992) builds a one-sided asymmetric information model in which the plaintiff and defendant negotiate for a finite number of periods before a trial occurs. An important implication of this model is that as litigation costs increase, the probability of settlement before trial increases and time to settlement is reduced.

Fenn and Rickman (1999) estimate Spier's (1992) model and confirm that lower litigation costs imply longer delays in reaching a settlement. Eisenberg and Farber (1997) develop and test a model in which the distribution of plaintiff's litigation costs affects her win rate at trial and time to settlement. Consistent

with their model, they show that individuals, who have more variable litigation costs than corporations, have higher trial rates and lower win rates at trial.

The above strand of the literature has focused exclusively on the costs of going to trial. Our paper contributes to this literature by showing that the costs of collecting judgments after trials are also empirically important. Indeed we show that more than half of judgments are not collected and that differences in post-trial collection probabilities are the main source of heterogeneity across lawyers in terms of lawsuit success. Our paper is the first to investigate, empirically or theoretically, the importance of post-trial collection costs.

The second strand of the law and economics literature to which we contribute is the strand that analyzes how lawyers affect lawsuit outcomes. The main focus in this literature is to investigate whether some lawyers are better than (or at least different from) others. Szmer, et. al. (2007), for example, study lawyer effects in Canadian Supreme Court cases and find that more experienced lawyers obtain more favorable outcomes conditional on going to trial.

Papers like Szmer, et. al. (2007), and many others, are subject to a significant methodological critique. Since cases are generally not randomly assigned to lawyers, it is difficult to know whether the observed differences across lawyers are really attributable to the lawyers themselves rather than to unobserved differences in the cases the lawyers receive.[1] For this reason, Abrams and Yoon (2007) is particularly noteworthy because they do exploit the random assignment of public defenders in Las Vegas felony cases. They find substantial heterogeneity in attorney performance.

Cases are not randomly assigned to lawyers in our data. Nevertheless, the assignment process of cases to public lawyers is quite close to random. Cases are assigned to public lawyers on the basis of a short questionnaire that does not contain any information that is unobservable to us as econometricians. We can therefore rule out that possibility that unobservable differences in cases across lawyers may be driving our results. In fact, there is not even evidence that the information from the questionnaire was used in the assignment of cases to the lawyers used for the main empirical tables. The differences we find in lawsuit outcomes across lawyers can therefore be attributed to the lawyers themselves, which is a significant improvement over much of the literature.

The third strand of the law and economics literature to which we contribute is the strand that analyzes the differences between *de facto* rather than *de jure* regulatory environments. Many papers show that analyses of written laws give an incomplete picture of the true regulatory environment. That is, the manner in which laws or regulations are applied must be incorporated into a complete analysis of the legal or regulatory environment.

Along these lines, Lerner and Schoar (2005) find that private equity investments have higher valuations and returns in countries with good enforcement mechanisms. Djankov, et. al. (2006) show that the development of debt markets is highly correlated with the efficiency of debt enforcement. Djankov et.

[1] See Abrams and Yoon (2007) for an extensive list of papers that analyze differences across lawyers, all of which are subject to the same methodological critique.

al. (2008), La Porta et. al. (2006), and Safavian and Sharma (2007) all show that both *de jure* legal rules and the quality of enforcement affect economic outcomes.

Papers that analyze the interaction between labor-market laws and their enforcement are particularly related to our paper since we are analyzing data from a labor court. Almeida and Carneiro (2007) examine the effects of differential enforcement across municipalities of Brazilian national labor regulations and find that increased enforcement causes formal-sector employment and unemployment to rise and causes self employment to fall. Montes Rojas and Santamaría (2007) examine new survey evidence for Mexico showing that going through the labor courts in a firing dispute increases firms' labor adjustment costs. Caballero, et. al. (2006), Haltiwanger, et. al. (2006), and Micco and Pagés (2007) all find that the negative effects of labor-market regulation are particularly strong in countries where the regulations are likely to be enforced.

We provide a first-hand look at why *de facto* and *de jure* labor-market regulations might be so different. In particular, we provide direct evidence that the enforcement of labor law in Mexico is extremely ineffective. Specifically, we show that labor law is unlikely to be enforced even after the worker wins a case in a labor court. Additionally, we show that the court's inability to enforce judgments has an important impact on the entire bargaining process, tilting the *de facto* regulatory environment towards the interests of the firm. Our finding that labor law is more stringently enforced for high-tenure workers may be important for future work on the effects of labor-market regulation.

The fourth and final strand of the law and economics literature to which we contribute is the strand that analyzes how procedural formalism affects the legal system. A legal system would be classified as being very formalistic if numerous in-person notifications are required at each stage in the process or if a plaintiff has the ability to delay the legal process for an unreasonable amount of time by filing appeals with no merit.

One of the most highly-cited papers in this strand of the literature is Djankov, et. al. (2003). They construct an index of formalism for a large group of countries. Some of the measures they consider are exactly the type of post-trial collection costs that are the focus of our paper. They consider, for example, whether the notification of a court judgment requires the participation of a court officer. They also count the minimum number of procedural actions required to enforce a court's judgment. We will show in next section that the enforcement of judgments in Mexico is indeed quite formalistic.

One of the main findings of Djankov, et. al. (2003) is that French style civil-law legal systems like Mexico's are more formalistic on average than other legal systems. Djankov et. al state that legal formalism "is associated with higher expected duration of judicial proceedings, *less consistency*, less honesty, less fairness in judicial decisions, and more corruption".

Our paper shows clearly how an overly-formalistic legal system can generate less consistency in legal outcomes. We show, for instance, that workers who had accumulated substantial tenure at their firms before separating are more likely to collect a judgment after a trial. We also show that there is heterogeneity across

publicly-appointed lawyers in terms of their probabilities of collecting awards for their clients after trials. In both cases, the differences in the probabilities of collecting awards after trials are not due to differences in the probabilities of receiving favorable rulings from the judge. Rather these differences arise because of differences in the willingness or ability to go through the costly and excessively formal process of enforcing a judgment after the judge has made the ruling. In other words, the observed differences across types of workers and across lawyers in the enforcement of labor law are directly attributed to legal formalism.

3 Legal Framework

As we mentioned earlier, Mexican labor law is highly protective of workers. The law regulates hours and working conditions, health risks, fringe benefits, and firing. In this paper we analyze firing lawsuits, so a discussion of the regulation of firing is in order. Under Mexican law, firing can either be considered justified or unjustified. In order for firing a worker to be justified under the law, the worker must have engaged in wrongful behavior such as deliberately destroying the firm's machinery or materials, physically attacking a supervisor, showing up to work under the influence of alcohol or drugs, or being absent from work repeatedly without justification. Remarkably, firing a worker for lack of productivity or laying off a worker during downturns is not considered to be justified.[2]

In order to fire a worker, a firm must notify the worker in writing, stating the cause for firing the worker. Given that firms must state one of the causes specified in the labor code, they often fabricate causes for firing a worker who is simply unproductive, and this often results in a lawsuit in which the worker claims the dismissal was not justified. When sued by a worker, the firm is considered to carry the burden of proof in relation to the cause of firing.

Certain components of firing costs do not depend on whether the firing was justified or not. In particular, any worker who is fired is entitled to unpaid overtime and wages, fringe benefits up to the date of firing, as well as severance pay equivalent to 12 days' wage per year worked at the firm. This daily wage for this calculation, however, is capped at two times the minimum wage.

Firing costs include several additional elements when the dismissal is unjustified under the law. First, a worker fired without just cause can sue for reinstatement. The firm may only refuse to reinstate for certain categories of workers such as temporary workers, those with less than one year's tenure, and at-will (typically white-collar) employees.[3]

[2] The discussion of Mexican labor law in this section is based on the *Ley Federal del Trabajo* (LFT), Title II, Chapter IV, as well as on the *Reglamento Interior de la Junta Federal de Conciliación y Arbitraje* (Internal Regulations of the Federal Labor Board).

[3] This category of workers, called *trabajadores de confianza,* are only "at will" in the sense that the firm cannot be forced to re-instate them, not in the sense that it need not give them severance pay when they are fired. In fact, these (usually managerial) workers receive higher severance pay under the law.

Second, in addition to the compensation due to a worker under any type of firing, an unjustly-dismissed worker receives two additional payments. She receives back pay including benefits from the date of firing to the date of payment of the court award. She also receives three months' wage with benefits per year worked at the firm, as well as an additional 20 days' salary per year worked at the firm if she is an at-will employee. Wages for these calculations are not capped.

We now describe the mechanisms through which labor law is enforced. In the first place, labor code in Mexico is federal, so that private employees in any state have access to the same legally-mandated protections. The labor courts are called *Juntas de Conciliación y Arbitraje*. They are administrative courts that belong to the executive branch of government at both the federal and state levels. Federal labor courts resolve disputes in a number of industries listed in the labor code. All other labor disputes fall under local jurisdiction, so all states have at least one local *junta,* and large states will often have several tribunals with jurisdiction defined by the geographical location of the dispute.

These tribunals are intended to serve both mediation and adjudication functions. The law mandates that they hold at least one conciliation hearing before proceeding to a court judgment. If the conciliation hearing concludes without a settlement, another hearing similar to a trial is held. Evidence such as expert testimony, depositions, and other documents is submitted to the judge during this hearing. After the conclusion of this hearing, the judge produces a draft ruling on matters of fact as well as matters of law and submits it to the labor board, consisting of the judge, a lay magistrate who represents firms, and a lay magistrate who represents workers. In order for the proposed draft to become a valid ruling, at least one of the magistrates must vote along with the judge in favor of the decision. Finally a hearing is scheduled in which the court's decision is read publicly in the presence of the parties to the dispute.

Should the firm send a legal representative to the hearing in which the court's decision is made public, then according to the law the firm has already been duly notified of the decision. However, firms often do not send a representative to the hearing, and in this case, the firm must be notified by a court clerk. In practice, in order for this notification to be carried out in a timely fashion, the plaintiff must participate in the process by making a motion to request immediate notification, as well as accompanying or having her lawyer accompany the court employee to the firm's place of business. This notification often takes some time, and firms, especially smaller ones, may do their best to avoid being notified properly.

Once due notification has taken place, the firm has 72 hours to send payment to the tribunal. If the firm does not pay within 72 hours, another hearing must be scheduled in which the judge should order a court actuary to appraise the firm's assets, seize a sufficient number of assets to pay the judgment the firm owes, and proceed to a sale of these assets, after which the court pays the judgment amount to the worker directly.[4] This process is akin to putting the

[4]This procedure is governed by Title 15 of the LFT, Articles 939-975.

firm through bankruptcy and therefore can be very costly, especially because the firm may block proper notification, move its place of business, or hide its assets. The court's order of an appraisal and sale of assets should be part of the same original lawsuit file from which we extract our data, however we find very few such orders. Discussions with both public and private lawyers have led us to believe that once firms have been duly notified, they generally do pay the award amount, but that firms very often take actions to block or avoid proper notification of the court award.

The parties may resolve their dispute by settlement at any point in the litigation process. However, unlike many other areas of law in Mexico and elsewhere, labor courts must both approve and record settlements. Unratified settlements, including settlements reached after the court has issued its ruling, are not legally binding. For this reason, we believe it is unlikely that the parties reach private settlements without notifying the court.

Apart from the protections in the federal labor code, the federal government and the states provide workers under their jurisdictions with free legal representation through public agencies generally called *Procuradurías de la Defensa del Trabajador.* The public prosecutors who work for these agencies are typically licensed lawyers, but may also be interns in their fourth year of law school. Public lawyers are not allowed to receive any compensation from their clients, who are assigned to them by the agency. Public lawyers are paid a salary that does not depend, at least not explicitly, on their performance. For methodological reasons that will be explained later, these public lawyers will be the focus of our empirical work.

We want to stress at this point why we believe that, at least compared to the U.S., the enforcement of judgments is Mexico is highly formalistic. Although we suspect that the enforcement of judgments is difficult in any legal system, we believe that the excessive use of formal notifications in Mexico represents a significant hurdle faced by plaintiffs that would not be faced by plaintiffs in the U.S. What is strikingly different about the enforcement of a judgment in Mexico is that the defendant is typically not present when the judge makes the ruling. Furthermore, before any attempt at collection can be made, the defendant must be notified **in person** by a court clerk. Anecdotal evidence leads us to believe that failed notifications are a common cause for the plaintiff giving up. Concerns about the notification process were expressed specifically for Mexican courts in World Bank (2006), which states that notifications can be delayed "by simply refusing to answer the door." For this reason, the World Bank is pursuing legal reforms in civil commercial courts that would allow electronic notifications to replace in-person notifications.

We are not the only ones to rank the Mexican judicial system as formalistic, particularly in the area of enforcing judgments after trials. Using the methodology described in Djankov, et. al. (2003), the 2008 Doing Business rankings place Mexico 49th out of 178 countries ranked in terms of how quickly a contract can be enforced. This time is counted from the moment the plaintiff files the lawsuit in court until payment. In terms of time to enforce a judgment, however,

Mexico's rank is 121. We therefore see that the Mexican judicial system is particularly inefficient at enforcing judgments.[5]

4 Data and Preliminary Statistics

We have assembled a data set comprised of all lawsuits filed in the *Junta Local de Conciliación y Arbitraje del Estado de México, Valle de Cuautitlán-Texcoco*, during 2000 and 2001.[6] This tribunal is located in an industrial region in the northern part of the Mexico City metropolitan area. Overall 718 cases were initiated in 2000 and 1,850 cases were initiated in 2001. Cases involving public lawyers, which will be the focus of this paper, account for 174 cases initiated in 2000 and 491 cases initiated in 2001. There were many more lawsuits filed in 2001 because of the dramatic decline of the *maquiladora* sector, which represents a large fraction of cases filed in this tribunal.

For all lawsuits, we observe the motive for filing, which is typically the allegation of an unjust dismissal, as well as the date of filing. From the initial filing made by the worker's lawyer, we observe a description of the job held, the dates the worker started and stopped working for the firm, the salary with and without fringe benefits, hours per week, the worker's gender, date of birth, and demands. In firing law suits, workers generally demand reinstatement, back-pay, overtime, fringe benefits, and severance pay.

In terms of the lawsuits' outcomes, we observe three modes of termination: dropped suits, settlements, and trials leading to a judgment by the court. We record the date of conclusion of the procedure and the payment received by the worker under a settlement or a court judgment. For trials, we observe a trial result stated by the court. This result classifies the decision as being in favor of the firm, in favor of the worker, or mixed in the sense that the court only concedes part of the worker's claim. We also observe the votes of the judge and the magistrates representing labor and management in favor of or against the judgment, and the facts of the case as recognized by the judge, including any payments that the firm previously made to the worker. Often a court ruling will result in constitutional appeals by one or both parties, and in these cases, we record the number of constitutional appeals, who files the appeals, and we extract data on the first and last court ruling.

We now present some descriptive statistics from the data set. Table 1 presents summary statistics for lawsuits in our sample separately for lawsuits involving private lawyers, lawsuits involving the 49 public lawyers observed in the data at least once, and for lawsuits involving the 19 public lawyers who we observe going to trial at least once and not going to trial (dropping or settling) at least once. The main difference we see between lawsuits involving public and

[5] The data on total time to enforce a contract are available from http://www.doingbusiness.org/ExploreTopics/EnforcingContracts/. The data on time to enforce a judgment, which is a component of the total time to enforce a contract, was provided to us by the Doing Business staff and are available upon request.

[6] These data were obtained by the authors using a new law governing freedom of governmental information in Mexico.

private lawyers is that final payoffs are substantially bigger in cases involving private lawyers. We also see that private lawyers tend to go to trial more often.

Some of our empirical models will be identified by lawyers for whom we observe both lawsuits that go to trial and lawsuits that do not go to trial. Restricting the data set to these lawyers essentially removes interns (those who have not yet completed their law degrees) from the data set. We see from table 1 that this restriction does not substantially affect the descriptive statistics. The 30 lawyers eliminated by this restriction account for only 85 observations.

Perhaps the most important feature we see from table 1 is that, both for cases involving private lawyers and cases involving public lawyers, it is quite common for positive awards at trial to go uncollected. In the case of private lawyers we see that, of 202 lawsuits in which a positive amount was awarded at trial, this amount was left uncollected 123 times. Similarly in the case of public lawyers we see that of the 45 lawsuits in which a positive amount was awarded at trial, the award was left uncollected 25 times. It is important to note that these are not judgments that were overturned on appeal. These are cases in which the workers abandoned their cases instead of initiating or continuing the costly processes of collecting judgments awarded at trials.

The main reason for focusing on public lawyers is that we believe the assignment of lawsuits to these lawyers was not based on unobservable characteristics. Court personnel assured us that case assignment was based on a short questionnaire that contained only basic information such as tenure and gender which we observe, and that assignment took place before the plaintiff met any public lawyer and before the actual filing of the lawsuit. In fact, we were told that gender and tenure were the only possible criteria for case assignment to public lawyers, but that tenure was more likely to be used since it was a better proxy for how large the worker's claim would be.

This essentially random assignment of cases to lawyers will allow us to attribute differences in lawsuit outcomes to the lawyers themselves. Although we cannot estimate similar models for the workers, we conjecture that the same types of differences we find across lawyers would exist across workers as well.

We attempt to verify this view of the assignment process in table 2. We estimate linear models with lawyer fixed effects for two characteristics of the case: a female worker dummy and years of tenure. Table 2 presents the results of the F-tests of the null hypothesis that there is no heterogeneity across lawyers. The results for private lawyers are quite strong; both gender and years of tenure are strongly correlated (at the 0.01 level) with the lawyer fixed effects. That is, case assignment is far from random. When we use all public lawyers, we see that years of tenure is strongly correlated (at the 0.01 level) with the lawyer fixed effects, but gender does not appear to be correlated with these lawyer fixed effects. These results are consistent the assertions of court personnel that tenure was more likely to be taken into account in the assignment of cases to lawyers. When we restrict our analysis to the 19 public lawyers for which we observe at least one case that went to trial and at least one that did not, we no longer see any evidence of non-random assignment. That is, neither gender nor years of tenure appear to be correlated with the lawyer fixed effects.

We believe that the results from table 2 are encouraging for our analysis. The assignment of lawsuits to lawyers could not have been based on things like the strength of the worker's claim because there would be no way to read such information from the short questionnaire filled out by the plaintiffs. When we restrict our analysis to the 19 lawyers for whom we observe at least one lawsuit that goes to trial and at least one that does not, we do not even observe a significant correlation between the observable characteristics and the lawyer fixed effects. These 19 lawyers can be viewed as the basic staff of the court.

We now turn to the issue of whether different lawyers indeed appear to act differently. In table 3 we investigate whether there are significant differences across lawyers in their probabilities of ending a lawsuit by dropping, settling, or going to trial. We estimate random-effects logit models with no independent variables in which the dependent variable is one of the three possible modes of termination. We present the chi-bar-square statistics of the test of the null hypothesis that all lawyers have equal probabilities that the case will be dropped, settled, or go to trial.

Looking first at the models for private lawyers, we reject the null hypothesis at the 0.01 level for all three termination modes. One may suspect, however, that these results are strongly affected both by differences in observable and unobservable characteristics of the cases across lawyers. When we use all public lawyers, we reject the null hypothesis that lawyers have the same probabilities of dropping and settling their cases at the 0.01 level. We only reject the null hypothesis that all public lawyers have the same probabilities of going to trial at the 0.10 level. Using only the 19 public lawyers with one trial and one non-trial outcome, we again reject the null hypothesis that lawyers have the same probabilities of dropping and settling their cases at the 0.01 level and find no significant differences in their probabilities of going to trial. We will exploit the fact that we find strong differences in settling and dropping probabilities in the subsequent analyses.

Since cases in which lawyers do not collect a positive trial award will be a key focus of our analysis, we want to explore these cases a bit more. The cases in which a positive award is left uncollected do not appear to be of trivial stakes. In the case of private lawyers, a judge awarded a positive amount to the worker in 202 cases. In the 123 cases in which the positive award was left uncollected, average years tenure was 3.76 and the median was 2.46. The analogous figures for the 79 cases in which a positive award was collected are 3.43 and 1.59. Surprisingly the cases in which a positive award is not collected appear if anything to be higher stakes cases. We note, however, that in at least 92 of the 123 cases in which the award was uncollected, the worker did not win the case outright. That is, in the vast majority of the cases in which the worker left an award uncollected, the judge failed to recognize at least some aspects of the worker's claim.[7] To the extent that the judge may be disputing years

[7] The worker won the case outright in at least 27 of the 79 cases in which the worker collected a positive award from a case involving private lawyers. There were 18 cases in which we did not observe whether the court's ruling was in favor of the worker, in favor of the firm, or mixed. In 9 of these cases a positive award was collected and in 9 of these cases a positive

of tenure as stated by the worker, tenure comparisons across cases involving private lawyers may be suspect.

When we analyze the data for public lawyers, we see that the judge awarded a positive amount in 45 cases. In the 25 cases in which the award was not collected, average tenure was 1.92 with a median of 1.51. In the 20 cases in which a trial award was collected, average tenure was 5.98 with a median of 2.59.[8] Another way to see that awards in high-tenure cases get collected is to note that there were seven cases in which a worker with more than seven years of tenure was awarded a positive amount at trial. In all seven of these cases the award was collected. It is also noteworthy that the judge's ruling was in favor of the worker, that is, the judge essentially accepted the worker's statement of the facts, in 15 of the 25 cases in which the worker left the award uncollected The judge's ruling was in favor of the worker in 15 of the 20 cases in which the worker did collect the award. Since the majority of cases in which the worker is awarded something result in an outright win for the worker, the tenure comparisons for cases involving public lawyers seem more reliable.

It is clear that, at least in the case of public lawyers for whom we believe that the assignment of cases to lawyers is close to random, cases in which a positive award is not collected tend to be lower-stakes cases. Nevertheless, these uncollected awards do to appear to be from trivially small cases. Razú (2006), for example, finds that 75% of newly-hired workers in Mexico do not stay continuously with the employer for one year. Kaplan, et. al. (2007) find that about 38% percent of formal-sector workers in Mexico were hired within the past year. We therefore see that a substantial fraction of employment at any given time has tenure below the median tenure observed for uncollected awards.

5 Simple Bargaining Model with Collection Costs

In order to derive testable implications about the bargaining process, we consider a model in which a plaintiff brings a lawsuit against a firm. We assume that the plaintiff maximizes her expected payment net of legal costs. We assume that, if the case goes to trial, the judge will award $V\varepsilon$. For simplicity we assume that the firm has perfect information both about the lawsuit and about the plaintiff. We will further assume that the plaintiff always observes V, and observes ε with probability λ. The timing of the game is as follows:

1. The plaintiff observes V. With probability λ, the plaintiff also observes ε. With probability $1-\lambda$, the plaintiff does not observe ε. In this case, the plaintiff simply knows that ε is drawn from a uniform distribution on the unit interval.

award was not collected.

[8] The results from the 19 public lawyers with at least on trial and one non-trial outcome look nearly identical to the results for all public lawyers.

2. The plaintiff decides whether to drop the case or not. If the case is dropped, the payoff to the plaintiff is zero. If the case is not dropped, the plaintiff pays a cost of C_O to proceed to the offer stage.

3. If the case has not been dropped, the plaintiff makes a take-it-or-leave-it offer to the firm. The plaintiff asks to receive a payoff of S. If the firm accepts the offer, payment is made and the game ends. If the firm rejects this offer, the case goes to trial and the judge awards $V\varepsilon$ to the plaintiff.

4. If the plaintiff pays a cost of C_C, she receives the award. If not, the plaintiff receives nothing. We will assume that $C_C > C_O$.

The model can be solved quite simply. First, consider the cases in which the plaintiff observes ε. If $V\varepsilon < C_C$, the case will be dropped. If not, the plaintiff will make a settlement offer of $V\varepsilon$, that will always be accepted by the firm. Hence, when the plaintiff observes the true value of the case, the lawsuit will never end up in court.

Now consider the case in which the plaintiff does not observe ε. If $V\varepsilon < C_C$ and the parties have reached the offer stage, the firm will not accept any offer since the firm knows that the judge's award will not be collected. At this stage in the game the difference between costs of simply going to trial and post-trial collection costs is most transparent. A cost for the plaintiff of simply proceeding to the trial stage would certainly affect the plaintiff's settlement offer, but would not affect the firm's decision rule on accepting or rejecting settlement offers. Post-trial collection costs for the plaintiff do, however, affect the firm's decision to accept the settlement offer. Even holding constant the plaintiff's settlement offer (which of course would not occur in equilibrium), the firm is more likely to reject the offer and proceed to trial when matched against a plaintiff with a high post-trial collection cost.

Conditional on $V\varepsilon \geq C_C$, an offer C_C or less will be accepted with probability one. Therefore the plaintiff will never offer less than C_C. The expected payoff (excluding the cost of making an offer which would have already been paid by this point) to the plaintiff can be written as:

$$E(\pi) = \left[-C_C + \left(\frac{S + C_C}{2}\right)\right]\left(\frac{S - C_C}{V}\right) + S\left(\frac{V - S}{V}\right). \tag{1}$$

How do we arrive at this expression? With probability $\frac{C_C}{V}$, the judge's award would be too small to be collected, so any offer will be rejected and the payoff to the plaintiff will be zero. With probability $\frac{S-C_C}{V}$, the offer will be rejected by the firm even though the award will be large enough to be collected. The expected judgment conditional on being in this situation is $\frac{S+C_C}{2}$, but the plaintiff will be forced to pay a cost of C_C to collect the award. With probability

$\frac{V-S}{V}$, the offer will be accepted and the payoff is simply S. It is straightforward to show that the optimal offer made by the plaintiff is[9]

$$S^* = \begin{cases} V - C_C \text{ if } V \geq 2C_C \\ C_C \text{ if } V < 2C_C. \end{cases} \tag{2}$$

We consider two potential sources of heterogeneity across plaintiffs in order to derive testable implications of the model. The first form of heterogeneity is that the plaintiffs differ in their values of λ, that is, plaintiffs differ in the accuracy of their information about the case. The second form of heterogeneity we consider is that plaintiffs differ in their values of C_C, that is, that plaintiffs differ in their costs (or disutilities) of collecting awards after the judgment is made.

We begin by considering the testable implications of the hypothesis that some plaintiffs have better information than others. If this were true, plaintiffs with better information (high values of λ) would be less likely to drop low-stakes (low V) cases and more likely to drop high-stakes (high V) cases. To see that this statement is true, note first that a plaintiff who never observed ε ($\lambda = 0$) would have a cutoff level of V below which she will always drop the case and above which she will never drop the case. A plaintiff who always observed ε ($\lambda = 1$), on the other hand, would drop cases if and only if $V\varepsilon < C_C$. This means that, even if V is very close to C_C, the perfectly-informed plaintiff will have a positive probability of not dropping the case. Furthermore, even if V is extremely large, the perfectly-informed plaintiff will have a positive probability of dropping the case.

What other predictions do we have about plaintiffs if we assume they only differ in terms of the quality of their information (λ)? Since all cases get settled when $V\varepsilon \geq C_C$ and the plaintiff observes ε, plaintiffs more likely to observe ε should always have settlement probabilities that are at least as high as those of plaintiffs with worse information. Further, better-informed plaintiffs should always have lower probabilities of a trial than plaintiffs with worse information, but since we observe relatively few trials in the data, this hypothesis will be difficult to test.

As mentioned in the introduction, these testable implications are comparisons of the outcomes of different cases for the same plaintiff. Since we do not observe workers multiple times in the data, we cannot use workers to test these implications. We do, however, observe lawyers multiple times in the data. We will therefore test these hypotheses using lawyers, implicitly making the reasonable assumption that the information used by the worker-lawyer team is a combination of worker information and lawyer information. The essentially random assignment of cases to lawyers guarantees that there should be no correlation between the quality of worker information and the quality of lawyer information. Nevertheless, the effects of differences in worker information and differences in lawyer information should be the same.

[9] It is very easy to add a cost of going to trial to the model. Assume, for example, that the plaintiff has to pay a cost of C_T if the case goes to trial. The resulting optimal offer would be $S^* = V - C_C - C_T$ if $V - C_C - C_T \geq C_C$ and $S^* = C_C$ if $V - C_C - C_T < C_C$.

We will not, unfortunately, observe any proxy for the quality of the lawyer's information (λ). We will, however, observe a proxy for the stakes of the case (V). In particular, we argue that the tenure at the firm of the dismissed worker is a good proxy for the stakes of the case. Assuming that lawyers only differ in terms of the quality of their information, we can rewrite the testable hypotheses in the following way:

i) Lawyers with **high probabilities** of dropping low-stakes cases will have **low probabilities** of dropping high-stakes cases.

ii) Lawyers with **high probabilities** of settling low-stakes cases will have **high probabilities** of settling high-stakes cases.

Some discussion of the above testable implications may be in order. First, we believe that the first testable implication is quite intuitive. Lawyers with accurate information will be able to recognize those few "small-stakes" cases that are indeed worth handling due to the high likelihood of success. Lawyers with accurate information will also be able to discard those apparently very high-stakes cases that have such a small likelihood of success that they are not worth pursuing.

We believe that the second testable implication is also intuitive. If we interpret our stylized model strictly, we would interpret a high probability of settling as evidence that the lawyer has better information. This result is a product of our assumption that the firms have better information than plaintiffs. If on the other hand plaintiffs tended to have better information than firms, the result would be that a lawyer with better information would be **less** likely to settle. Regardless of which party may have better information, however, lawyers with high probabilities of settling low-stakes cases should also have high probabilities of settling high-stakes cases.

The other potential source of heterogeneity that we consider in this paper is that plaintiffs differ in their costs of collecting awards (C_C). The first (trivial) testable implication in this case is that, conditional on any value of V, plaintiffs with high collection costs will have dropping probabilities that are at least as high as those for plaintiffs with lower costs.

We now turn to settlement probabilities assuming plaintiffs differ in their collection costs. As λ (the probability of observing the true value of the suit) approaches 1, all cases that are not dropped will settle, since both parties will know the true value of the case. Also, since the plaintiff knows the true value of the case, for any value of V, the case will be dropped with a higher probability when collection costs are higher. Since settling and dropping are the only two case outcomes, this means that for any value of V, a plaintiff with higher costs of collection is less likely to settle. As λ approaches zero, however, the story is more complicated.

Note first that, conditional on the suit not being dropped and conditional on ε not being observed, settlement will occur whenever the true value of the suit ($V\varepsilon$) is greater than the settlement offer (S^*) given by equation 2. Simple inspection of equation 2 reveals that the optimal settlement offer is higher for

high-cost plaintiffs when V is low and is lower for high-cost plaintiffs when V is high. We therefore see that if $C_O = 0$, which would imply that no suits are dropped, plaintiffs with high collection costs would have lower probabilities of settling low-V suits and higher probabilities of settling high-V suits.

The intuition behind the above result is straightforward. When the stakes of the case are high, a firm views offers from high- and low-cost plaintiffs similarly since, conditional on going to trial, all plaintiffs will collect with probability close to one. In the bargaining stage, however, a high-cost plaintiff will ask for less money and therefore settle more often since she is more anxious to avoid the trial. Hence for high-stakes cases, a high-cost plaintiff is more likely to settle. This is exactly how a standard cost of going to trial operates in the literature. When the stakes of the case are high, which implies that awards will almost never be left uncollected, a cost of going to trial and a cost of collecting an award are effectively the same thing.

When the stakes of the case are low, however, the firm anticipates that a high-cost plaintiff will not collect the award. Therefore in a low-stakes case a high-cost plaintiff is less likely to settle because the firm views a trial as a good outcome. In our model this translates into settlement occurring whenever the true value of the case exceeds the plaintiff's collection costs. This implies a lower probability of settling for plaintiffs with high collection costs and low values of V. The possibility that a high-cost plaintiff will settle with a lower probability, even if cases are never dropped, differentiates our model from those with costs of simply going to trial.[10]

To make this issue clear, consider figure 1, which gives a graphical representation of the analysis on settlement probabilities presented above. We plot the optimal offers, conditional on V (and conditional on the worker's lawyer not observing ε) for a lawyer with a low value of C_C and a lawyer with a high value for C_C. Assuming the case has not been dropped, all settlement offers will be accepted when $V\varepsilon \geq S$. We see from the figure that high-cost lawyers ask for more money from low-stakes cases and ask for less money in high-stakes cases.

How do we incorporate dropped cases into our analysis of the effect of collection costs on settlement probabilities? The fact that a high-cost plaintiff will have a higher cutoff level of V required to not drop the case only reinforces the result that, when λ is small, high-cost plaintiffs will have lower settlement probabilities for low-V cases. To see this, one only has to note that the high-cost plaintiff will have a higher cutoff value of V in order to proceed with the case. If the high-cost plaintiff is below her cutoff value of V, her probability of settling will be zero. Once V is high enough, dropped cases cease to be an issue and

[10] Note that this explanation does not rely crucially on the fact that the firm has better (indeed perfect) information. In order to obtain the result that a high-cost plaintiff will settle high-stakes cases at a higher rate, it is crucial that the informational differences between the plaintiff and the firm be large enough. Trials will never occur if the plaintiff and the firm have similar enough information, which means that high-cost plaintiffs will settle high-stakes cases with lower probabilities simply because they will drop them more often rather than settling them.

our previous analysis that high-cost plaintiff will settle with higher probabilities remains intact.

Once again, we will use lawyers as a way of informing us about the effects of these costs on lawsuit outcomes. We do not observe any proxy for collection costs, but we will continue to use tenure as a proxy for the stakes of the case. Assuming that lawyers only differ in terms of their collection costs, we therefore summarize out testable implications in the following way:

iii) Lawyers with **high probabilities** of dropping low-stakes cases will have **high probabilities** of dropping high-stakes cases.

iv) Lawyers with **high probabilities** of settling low-stakes cases may have **low probabilities** of settling high-stakes cases.

Although the relation between the settlement probability for low-stakes cases and the settlement probability for high-stakes cases is theoretically ambiguous if lawyers only differ in terms of collection costs, settlement probabilities will still be central to our empirical analysis. If we find evidence that lawyers who settle low-stakes cases tend not to settle high-stakes cases, we will be able to reject the hypothesis that lawyers differ only in terms of their information. Such a finding would therefore imply that differences in collection costs across lawyers affect lawsuit outcomes. Since it can be also extremely costly for the workers in terms of time to collect awards after trials, it seems likely that differences across workers in collection costs should have similar effects on outcomes.

The primary goal of the empirical section will be to test the above hypotheses empirically. We will begin, however, by presenting evidence that these costs of collecting awards significantly impact the bargaining and trial outcomes we study. We will also present some results we believe are interesting, although not strictly related to the theoretical model.

To anticipate results, we will neither be able to reconcile our empirical results by assuming that lawyers differ only in terms of the quality of their information nor by assuming that lawyers differ only in terms of their collection costs. We can, however, reconcile our empirical results with our model if lawyers differ both in terms of their information and in terms of their collection costs.

6 Empirical Analysis

Our first goal in this section is to demonstrate that costs associated with collecting awards must be taken into account in order to understand how lawsuits are resolved. For the rest of the paper, we will only use data from the 19 public lawyers with at least one trial and one non-trial outcome. We will do this because some of our models, like the one we present below, compare outcomes of lawsuits that go to trial to outcomes of lawsuits that do not go to trial for the same lawyer. Lawyers who do not have at least one lawsuit that goes to trial and at least one that does not go to trial contribute very little to these estimations. The inclusion of these lawyers, however, would require the estimation of many

more parameters in a non-linear model. As we mentioned earlier, the inclusion of these lawyers would require the estimation of an additional 30 lawyer effects while only adding an additional 85 observations.

Consider now the following model:

$$\begin{aligned} pos_{il} = \quad & \beta_1 tenure_{il}\left(1 - trial_{il}\right) + \beta_2 tenure_{il} * trial_{il} + \\ & \beta_3 gender_{il}\left(1 - trial_{il}\right) + \beta_4 gender_{il} * trial_{il} + \beta_5 trial_{il} + \\ & \alpha_l\left(1 - trial_{il}\right) + \gamma\alpha_l trial_{il} + \varepsilon_{il} \end{aligned} \tag{3}$$

where the subscript i denotes the case and the subscript l denotes the lawyer. The dependent variable pos_{il} is a dummy variable equal to one if the worker recovers a positive award. If the case ended in a trial ruling, the dummy will be equal to one if the worker was awarded a positive amount at trial **and** if this award was in fact collected. If the lawsuit did not end in a trial ruling, then pos_{il} is simply a dummy variable for whether the case was settled (all settlements are for positive amounts) as opposed to being dropped. We consider two observable characteristics: gender and tenure, and allow the effects of these variables to be different for trial and non-trial outcomes. We also allow trials to have a different intercept than do lawsuits that do not end as trials.

We estimate the parameter α_l for each lawyer, which among other things captures the differences in settlement probabilities across lawyers, controlling for gender and tenure and conditional on the case not going to trial. Note also that γ, through the parameter α_l, also affects the probability of recovering a positive amount at trial. Since the two parameters γ and α_l enter multiplicatively in the last term of the equation, we estimate this model with non-linear least squares.[11]

The first column of table 4 presents the results of the estimation of equation 3. The first result to point out is that the estimate of β_2 is 0.04 and statistically significant at the 0.01 level. This result tells us that, conditional on going to trial, workers with high tenure tend to collective positive awards. We also see that the estimate of γ is -1.06 and is significant at the 0.05 level. This means that lawyers who tend to settle cases that do not go to trial (ones with high values for α_l) tend not to collect positive awards for cases that go to trial.

We therefore see that cases that go to trial are more likely to end with the worker collecting something when the worker was employed for a long time at the firm and when the worker's lawyer drops a high fraction of cases that do not go to trial. One simple explanation for these results is that these types of cases receive more favorable rulings at trial. Another explanation is that these types of cases do not receive more favorable rulings, but that awards in these cases are more likely to be collected. The results from columns 2-5 of table 4 support the latter explanation.

In column 2 we estimate a model similar to equation 3 in which the dependent variable continues to be pos_{il} for lawsuits that do not go to trial. For lawsuits that do go to trial, however, we use as the dependent variable a dummy for

[11] Some cases are grouped together in the same lawsuit. We calculate the standard errors of our estimated parameters allowing for arbitrary heteroscedasticity and allowing for an arbitrary correlation of the error terms among cases grouped together in the same lawsuit.

whether the judge declares her ruling to be favorable for the firm. Columns three and four present the analogous estimations examining the trial rulings of favorable for the worker and mixed respectively. Since the none of the estimated values of β_2 or γ from columns 2-4 are statistically significant, we see no evidence that high-tenure cases are more likely to receive favorable rulings at trial and no evidence that lawyers who drop a lot of cases receive more favorable rulings at trial.

In column 5, however, we use a dummy variable for "not collecting a positive amount awarded at trial" as the trial outcome measure. Since the estimate of β_2 is -0.03 and significant at the 0.01 level, we see that awards from high-tenure cases tend to be actually collected. Furthermore, since the estimate of γ is 1.49 and significant at the 0.05 level, we see that lawyers who tend to settle many cases also tend to leave positive awards uncollected at trial.

Combining the information from all columns of table 4, we see that workers in high-tenure cases tend to collect something at trial, not because they do better in terms of trial outcomes but rather because the awards are actually collected. Similarly, lawyers who settle many cases tend not to collect positive amounts for their clients at trial not because they do worse in terms of trial outcomes, but rather because they simply tend not to collect positive awards for their clients. In both cases, we see how an inconsistent application of labor law is directly attributable to the costly and excessively formalistic system of collecting judgments.

The results on tenure are obviously consistent with the theoretical model if we view tenure as a proxy for the stakes of the case (V). But what do the results on lawyers have to do with the theoretical model? Perhaps the easiest interpretation of the results on lawyers from table 4 is that the lawyers who drop a lot of cases do so because they have better information. Consistent with the model, these lawyers should tend not to go to trial when the amount awarded is likely to be too small to bother collecting. Indeed, we will present further evidence in favor of this hypothesis.

Can the results on lawyers from table 4 be consistent with the idea that lawyers simply differ in their costs of collecting trial awards? If we made the (ridiculous) assumption that gender and tenure were the only variables observed by the lawyer, the results in table 4 would seem to contradict the predictions of this hypothesis. According to this hypothesis, lawyers who settle (do not drop) a lot of (non-trial) cases should have low costs of collecting trial awards, and therefore should tend to collect positive amounts with **higher** probabilities. Furthermore, lawyers who settle (do not drop) a lot of (non-trial) cases should have **lower** probabilities of not collecting positive awards at trial.

Of course the lawyer should observe many things that we do not observe as econometricians. Suppose, for instance, that we observe two cases with different lawyers in which tenure has a low value. Suppose further that neither of these cases is dropped. If we know that one lawyer has low costs of collecting trial awards, the fact that we observe that she did not drop the case might not be surprising. If, however, we know that the other lawyer has a high cost of collecting a trial award, it is surprising to see that the case has not been

dropped. It is therefore likely that some unobservable (to the econometrician) characteristics of the case are quite favorable.

We could therefore rationalize the results in table 4 in the following way. Lawyers with high costs of collection end up dropping many cases. Conditional on going to trial, these lawyers with high costs therefore have cases that are stronger for unobservable reasons. Since the tried cases for high-cost lawyers are in fact stronger, it is quite natural to see that the high cost lawyers tend to recover positive awards for their clients. If you thought that lawyers with high costs of collecting awards also had high costs of proceeding with the case in the first place (that is, not dropping the case early on), this "selection effect" would be even stronger. We will in fact present evidence in favor of the hypothesis that differences in collection costs also play an important role.

In summary, we do not believe the results on lawyers from table 4 are particularly helpful in testing our hypotheses. We do believe, however that table 4 demonstrates that the collection costs associated with the excessively formalistic enforcement procedures, which are the emphasis of our entire paper, are important factors both for explaining why low-tenure cases do not collect awards after trial and for explaining differences across lawyers. We now turn to empirical exercises that are more closely linked to our hypotheses.

If we believe that plaintiffs differ in the quality of their information, the theoretical model makes a clear prediction on dropped cases. Plaintiffs with better information should drop **fewer** low-stakes (low V) cases, because they will be able to separate out the few low stakes cases that are very likely to lead to a judgment that is worth collecting. Plaintiffs with better information should also drop **more** high-stakes (high V) cases since they will be able to recognize the few high V cases that are not worth the effort. The model therefore predicts that plaintiffs who are more likely to drop small cases should be less likely to drop large cases. As mentioned earlier, since we do not have multiple observations on the same worker and since the empirical implications of our model apply to the worker-lawyer team, we use multiple observations on the same lawyers to test the general hypothesis that informational differences are important determinants of lawsuit outcomes.

In order to test this prediction, we estimate the following equation:

$$drop_{il} = \alpha_l + \beta_1 female_{il} + \beta_2 tenure_{il} + \gamma \alpha_l * tenure_{il} + \varepsilon_{il}. \qquad (4)$$

Equation 4 also has to be estimated by non-linear least squares. The parameter α_l measures the lawyer's propensity to drop a lawsuit when tenure is equal to zero. A negative value for the parameter γ would imply that, for a large enough value of tenure, lawyers who are more likely to drop when tenure is low are less likely to drop when tenure is high. We present the results of estimating equation 4 in the first column of table 5.

A brief discussion of the validity of this estimating equation may be in order. We argued in section 4 that there were ex-ante reasons to suspect that the assignment of cases to lawyers might be related to gender and worker tenure, since these variables were on the simple questionnaire used to assign cases to

lawyers. In other words, there were ex-ante reasons to suspect that α_l might be correlated with $female_{il}$ and $tenure_{il}$. We note that, for the lawyers used in the estimation of equation 4, we found no evidence of correlations between gender and tenure and lawyer assignment. If, however, such correlations existed, they would not invalidate our technique.

We also argued in section 4 that there were strong ex-ante reasons to believe that the assignment of cases could not be related to the unobservables in equation 4 because, when the assignment took place, there was no additional information available from the questionnaire apart from gender and tenure. In other words, there are strong ex-ante reasons to believe that $corr(\alpha_l, \varepsilon_{il}) = 0$, $corr(female_{il}, \varepsilon_{il}) = 0$, and $corr(tenure_{il}, \varepsilon_{il}) = 0$. These are our key identification assumptions. We do not require, for example, that average tenure of the worker be the same for all lawyers. We do require that differences in tenure within and between lawyers not be related to ε_{il}, which again seems reasonable since no other information was available at the moment of case assignment.

As predicted by the theoretical model when lawyers differ in the accuracy of their information, our estimate of γ is negative (-0.14) and statistically significant at the 0.01 level. According to this estimation, lawyers would be predicted to have the same dropping probabilities when tenure is 7.20 years. This figure is a bit worrisome since tenure of 7.20 years corresponds to the 90th percentile of the tenure distribution in our data, that is, there are very few observations with a tenure level higher than 7.20. To address this concern, we estimated an equation similar to equation 4, but with a more flexible functional form for tenure. Specifically we estimated

$$\begin{aligned} drop_{il} = \quad & \beta_1 female_{il} + \alpha_l + \beta_2 tenure_{il} + \gamma_1 \alpha_l * tenure_{il} + \\ & \beta_3 tenure_{il}^2 + \gamma_2 \alpha_l * tenure_{il}^2 + \beta_4 tenure_{il}^3 + \\ & \gamma_3 \alpha_l * tenure_{il}^3 + \beta_5 tenure_{il}^4 + \gamma_4 \alpha_l * tenure_{il}^4 + \varepsilon_{il}. \end{aligned} \tag{5}$$

We present the results of estimating equation 5 in column 2 of table 5. Importantly, lawyers with high probabilities of dropping when tenure is low are now estimated to have lower probabilities of dropping when tenure is greater than 3.58, which is at the 75th percentile of the tenure distribution. To make the results of table 5 more transparent, we plot in figure 2 the estimated values of the derivative of the dropping probability with respect to α_l, for all tenure values up until 23.98 which is the 99th percentile of the tenure distribution. We do this both for the equation in which tenure is entered linearly and for the case in which tenure is entered as a quartic.

Since we believe this "switching point" in the probabilities of dropping is a crucial test of the hypothesis that lawyers differ in terms of the accuracy of their information, we explore this issue further. In column 3 of table 5, we present estimates of the following equation:

$$\begin{aligned} drop_{il} = \quad & \beta_1 female_{il} + \alpha_l * (tenure_{il} < 3.58) + \beta_2 (tenure_{il} \geq 3.58) + \\ & \gamma \alpha_l * (tenure_{il} \geq 3.58) + \varepsilon_{il}. \end{aligned} \tag{6}$$

The cutoff value of 3.58 to separate low and high tenure was chosen because our estimation of equation 5 indicated that lawyers with high probabilities of dropping when tenure is low were estimated to have lower probabilities of dropping when tenure is greater than 3.58. The estimated value for γ is -1.42 and is statistically significant at the 0.01 level.

In column 4 of table 5 we re-estimate equation 6, but only using tenure values in the bottom quartile (tenure $\leq$ 0.55) or tenure values in the highest quartile (tenure $\geq$ 3.595). The idea behind this estimation is to throw out the observations from tenure ranges in which the differences between lawyers are estimated to be small. Our estimate of γ is now -1.61 and is significant at the 0.05 level. Overall we believe that there is considerable empirical support for the model's prediction that lawyers who drop a high percentage of low-stakes cases will drop a low percentage of high-stakes cases. In other words, the results from table 5 support the hypothesis that lawyers differ in terms of the accuracy of their information. It is also worth noting that we could not rationalize the results of table 5 if we thought that lawyers differ only in their costs of collecting awards. A high-cost lawyer would be more likely to drop all cases.

The results from table 5 are therefore consistent with the hypothesis that informational differences affect lawsuit outcomes. Although we confirmed this hypothesis using heterogeneity across lawyers, there is no doubt enormous heterogeneity across workers in terms of their information. In this sense, the results from table 5 almost certainly indicate that labor law will be enforced less strictly for workers who lack the information necessary to defend their rights.

Now that we have presented evidence that informational differences are important determinants of lawsuit outcomes, we turn to evidence that the costs of collecting awards are also important determinants of lawsuit outcomes. Recall that if plaintiffs differ in terms of their collection costs, it is possible for plaintiffs with high probabilities of settling low-stakes cases to have low probabilities of settling high-stakes cases. Such a result, however, would be inconsistent with the hypothesis that plaintiffs only differ in terms of the quality of their information. We therefore estimate models like in table 5 (equations 4, 5, and 6), but use settlement as the dependent variable instead of the case being dropped. Once again, we use differences across lawyers to establish that differences in collection costs are important determinants of lawsuit outcomes.

We present the results of estimating settlement probabilities in table 6. In column one we present the results of estimating an equation analogous to equation 4, but with settlement as the dependent variable instead of dropped cases. Once again we estimate γ to be negative (-0.13) and statistically significant at the 0.01 level, implying that those lawyers with high settlement probabilities when tenure is low have lower settlement probabilities when tenure is high. This "switching point" occurs when tenure is 7.97 years, which is the 91st percentile of the tenure distribution. When estimating the analogy of equation 5 for settlement probabilities, we estimate that the switching point occurs at a tenure of 3.46 years, which is the 74th percentile of the distribution of tenure. In figure 3, we plot the estimated values of the derivative of the probability of settling with

respect to α_l, both for the equation in which tenure is entered linearly and for the case in which tenure is entered as a quartic.

We again present the results of some "less parametric" models like equation 6, this time using 3.46 years as the cutoff between high and low tenure. When we use all of the data, we estimate γ to be negative (-0.46) but not statistically significant (p-value of 0.106). When eliminating observations from the middle two quartiles of the tenure distribution, we now estimate γ to be negative (-0.79) and statistically significant at the 0.01 level. Overall table 6 presents evidence that lawyers may also differ in terms of their costs of collecting awards. In particular, lawyers who settle with high probabilities when the stakes of the case are low (lawyers with low collection costs in the theoretical model) settle with lower probabilities when the stakes of the case are high.[12]

The results in table 6, therefore, support the hypothesis that heterogeneity in terms collection costs affects lawsuit outcomes. Since we have found evidence for heterogeneity in terms of collection costs across lawyers, it seems extremely likely that this same sort of heterogeneity exists across workers. In fact, the main cost of collection is that both the worker and the lawyer accompany the court clerk when she attempts to notify the firm about the judge's ruling. Certainly the value of time varies across workers much as it does across lawyers. In this sense, it seems likely that workers with high collection costs do not receive the full benefits to which they are entitled. They will drop many cases when they have a legitimate case, they may accept low settlement amounts in order to avoid trying to collect, or they may leave awards uncollected after trials.

Recall that we presented evidence in table 5 that some lawyers had better information. That is, the results from table 5 can be reconciled with our theoretical model if the main source of differences among lawyers is that some have more accurate information about what the judge will award. Table 6 presents evidence that lawyers also differ in their collection costs. That is, the results from table 6 can be reconciled with our theoretical model if the main source of differences across lawyers is that some lawyers find it more costly (or more unpleasant) to collect awards after trials.

In light of the evidence from both tables 5 and 6, one might conjecture that the results from both tables might be reconciled with our theoretical model if both forms of lawyer heterogeneity are incorporated into the model simultaneously. We indeed show in the appendix that, if the lawyers with more accurate information also tend to be those with lower post-trial costs of collections, the results from both tables 5 and 6 can be reconciled by our theoretical model.

We view the results in tables 5 and 6 as the results that are most directly linked to our model. In table 7, however, we present some models that we believe

[12] Since the results of analyzing equations like equations 4, 5, and 6 for trial outcomes do not give clear empirical results and do not relate to the theoretical model in an obvious way, we do not report the results of these models. We are happy to provide these results upon request.

are interesting although not related in a clear way to our theory. In particular, we estimate the following equation in column one of table 7:

$$\begin{aligned} pos_{il} = \quad & \beta_1 tenure_{il}\,(1 - trial_{il}) + \beta_2 tenure_{il} * trial_{il} + \\ & \beta_3 gender_{il}\,(1 - trial_{il}) + \beta_4 gender_{il} * trial_{il} + \beta_5 trial_{il} + \\ & \alpha_l\,(1 - trial_{il}) + \gamma_1 \alpha_l trial_{il} + \gamma_2 \alpha_l tenure_{il} * trial_{il} + \varepsilon_{il}. \end{aligned} \tag{7}$$

The parameters α_l capture (much like in equation 3), among other things, the differences in settlement probabilities across lawyers, controlling for gender and tenure and conditional on the case not going to trial. The parameter γ_1 now captures how settlement probabilities conditional on not going to trial (α_l) affect the probability of recovering a positive amount at trial when tenure equals zero. The key feature of this model is that, through the parameter γ_2, the differences in recovering something at trial between lawyers who settle or drop most of their non-trial cases can vary with tenure.

We see from column one that the estimate of γ_2 is -0.18 and statistically significant. That is, lawyers who drop a lot of cases do **comparatively worse** in low tenure cases, which one may argue is consistent with the theoretical model although we certainly have not resolved the selection issues that made our interpretation of the results from table 4 difficult. We think, however, that the more interesting results come from analyzing the rulings of the judge.

Our theoretical model has an exceedingly simple view of a trial. In the model, the judge simply reveals the truth and does not need to communicate with the two litigants. One might conjecture, however, that a more complex model would predict that lawyers with high costs of collecting awards would tend to exaggerate their claims for low-stakes cases. After all, why would a lawyer ask for a "reasonable" amount if the lawyer would not bother collecting a "reasonable" amount?

In column two of table 6 we estimate a model similar to equation 7 in which the dependent variable continues to be pos_{il} for lawsuits that do not go to trial. For lawsuits that do go to trial, however, we use as the dependent variable a dummy for whether the judge declares her ruling to be favorable for the firm. The parameter α_l continues to measure, among other things, the lawyer's propensity to settle cases as opposed to dropping them. Since we do not estimate a significant coefficient for γ_2, we find no evidence that lawyers who settle a high fraction of non-tried cases have differential propensities to lose high- or low-stakes cases outright.

In column three, however, we analyze the outcome of the judge's ruling being favorable to the worker. Our estimate of γ_2 is -0.34 and significant at the 0.01 level, which implies that lawyers who drop a lot of cases (presumably those with high costs of collecting trial awards) are comparatively less likely to win low-stakes cases outright. Finally, we analyze the probability of a "mixed" ruling in column 4. Since we estimate that γ_2 is 0.25 and statistically significant at the 0.01 level, we find evidence that lawyers who drop a lot of cases (presumably those with high costs of collecting trial awards) are comparatively more likely to get mixed rulings.

Our interpretation for these results on trial outcomes is the following. The results on rulings that are favorable to the firm tell us that, when the stakes of the case is low, judges do not tend to rule that lawyers who drop a lot of cases (presumably those with high costs of collecting awards) bring for cases with no merit. The results on rulings favorable to the worker tell us that judges tend not to accept the entire claims of lawyers who drop a lot of cases when the stakes of the case are low. Rather, the results on mixed rulings tell us that the judges tend to say that, for low-stakes cases, lawyers who drop a lot of cases tend to be exaggerating their claims.[13]

7 Conclusions

Government regulations, combined with the mechanisms through which regulations are enforced, have a crucial impact on a country's business climate. In this paper, we analyzed the interaction between an extremely rigid labor law and a court system that is inefficient at enforcing the law. In particular, we used data from lawsuits assigned to public lawyers in a labor tribunal in Mexico to show that 56% of awards "won" by workers were not collected. This never occurred in cases in which the worker had more than seven years of tenure with the firm.

Although we could not analyze worker heterogeneity in lawsuit outcomes directly, we could analyze heterogeneity across the lawyers representing them. We showed empirically that those lawyers who drop a lot of cases tend not to leave trial awards uncollected. One interpretation for this result is that better-informed lawyers anticipate cases in which they would be unlikely to collect the amount awarded at trial and drop these cases at earlier stages. Another interpretation is that lawyers with high costs of collecting awards drop all low-stakes cases and only go to trial with high-stakes cases.

In order to sort through these two interpretations, we developed a simple theoretical model to help interpret the effects of a cost of collecting awards after a trial. The model generated distinct testable hypotheses of how workers (and the lawyers representing them) would act differently depending on differences in the accuracy of their information and on differences in their costs of collecting awards. With respect to the cost of collecting awards, our model's implications differ from the implications of previous models focusing on the costs of going to court. We verify the model's testable hypotheses with our data, and find evidence that lawyers are different both in terms of the accuracy of their information and in terms of their collection costs.

We therefore see that the distinction between *de facto* and *de jure* labor regulation is a complex one. We show that differences in the information available to the worker affect the application of labor law. We also show that when the worker is more willing to defend her rights, either because the potential benefits are high or because her costs are low, labor law is applied more strictly. More

[13] The possibility that plaintiffs with high costs of collection might exaggerate their claims may provide a rationale for the results of Kaplan, et. al. (2008) who find that some workers exaggerate their claims despite apparently being punished on average for doing so.

generally, we show that the worker herself is a crucial determinant of the degree to which labor law is enforced.

Finally, our paper allows us to peer inside the black box of courts to see a mechanism through which legal formalism generates inefficient and inconsistent enforcement. The heterogeneity we find in lawsuit outcomes, both in terms of the differences between high- and low-tenure workers, and in terms of differences across lawyers, can be directly attributed to the excessively formalistic way in which judgments are enforced.

Appendix

The purpose of this appendix is to demonstrate that the main empirical results of the paper can be reconciled with our theoretical model if lawyers with lower costs of collection also have more accurate information. Specifically, the lawyers with lower collection costs and better information will drop low-stakes cases with lower probabilities, drop high-stakes cases with higher probabilities, settle low-stakes cases with higher probabilities, and settle high stakes cases with lower probabilities.

Tables 5 and 6 present evidence that neither of the two sources of heterogeneity across lawyers on their own can explain our empirical results. Recall that table 5 told us that the lawyers who drop low-stakes cases tend not to drop high-stakes cases. Recall further from table 6 that lawyers who settle low-stakes cases tend not to settle high-stakes cases. Since trials form a relatively small percentage of outcomes, it would appear that those lawyers who drop low-stakes cases (and tend not to drop high-stakes cases) are also those who tend **not** to settle low-stakes cases (and do tend to settle high-stakes cases). We confirm this fact by looking at the correlation of the estimated values for α_l for the 19 lawyers across tables 5 and 6. The correlations are -0.93, -0.93, -0.87, and -0.81 using the estimates from columns 1, 2, 3, and 4 respectively.

We will now argue that the two sources of heterogeneity that we consider, when taken into account simultaneously, can be reconciled with the empirical evidence. Let us suppose, for example, that the lawyers who disproportionately drop high-stakes cases and disproportionately do not drop low-stakes cases have better information. The fact that these lawyers disproportionately settle low-stakes cases is perfectly consistent with having better information. The question then becomes how we can reconcile the fact that these lawyers also disproportionately do not settle high-stakes cases? This result could only be reconciled with our theory if the better-informed lawyers also had lower costs of collecting awards. Indeed, as we show below, it is easy to find parameters for the theoretical model that make this possible.

Consider two possible values for V: $V = 0.5$ and $V = 2$. For ease of exposition we will assume that the cost of reaching the offer stage C_O is zero for all lawyers but that lawyers will drop lawsuits when there is no chance that making an offer will yield a positive payoff. Alternatively you may assume that C_O is positive but negligible.

Suppose that there are two types of lawyers. Suppose that type 1 (the bad lawyers) observe ε with probability zero ($\lambda = 0$) and have collection costs $C_C = 0.5$. When $V = 0.5$, it is trivial to see that type 1 lawyers will drop cases with probability one and therefore settle cases with probability zero. When $V = 2$, type 1 lawyers will drop with probability zero. They will make a settlement offer $S^* = 1.5$, which means that they will settle with probability 0.25 and go to trial with probability 0.75.

Now suppose that $\lambda = 0.1$ and $C_C = 0.1$ for type 2 lawyers. When $V = 0.5$, they will only drop the case in the event that they observe the true value of ε and $\varepsilon < 0.2$. This occurs with probability 0.02. They will go to trial in the

event that they do not observe ε and that their settlement offer $S^* = 0.4$ is rejected, which occurs with probability 0.72. Type 2 lawyers therefore settle with probability 0.26 when $V = 0.5$.

Now consider type 2 lawyers when $V = 2$. They will only drop the case in the event that they observe the true value of ε and $\varepsilon < 0.05$. This occurs with probability 0.005. They will go to trial in the event that they do not observe ε and that their settlement offer $S^* = 1.9$ is rejected, which occurs with probability 0.855. Type 2 lawyers therefore settle with probability 0.14 when $V = 2$. We summarize all of these results in the following table:

	$V = 0.5$	$V = 2$
$C_C = 0.5$, $\lambda = 0.0$	drops with prob 1 settles with prob 0	drops with prob 0 settles with prob 0.25
$C_C = 0.1$, $\lambda = 0.1$	drops with prob 0.02 settles with prob 0.26	drops with prob 0.005 settles with prob 0.14

We can therefore see that we can reconcile our empirical results with those of the theoretical model if the lawyers with higher collection costs also have lower-quality information. In the above example, the lawyers with higher collection costs and lower-quality information:

i) drop low-stakes cases with higher probabilities.

ii) drop high-stakes cases with lower probabilities.

iii) settle low-stakes cases with lower probabilities.

iv) settle high-stakes cases with higher probabilities.

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Figure 1: Settlement offers from workers assuming case is not dropped

Figure 2: Derivative of probability of dropping with respect to the probability of dropping when tenure equals zero

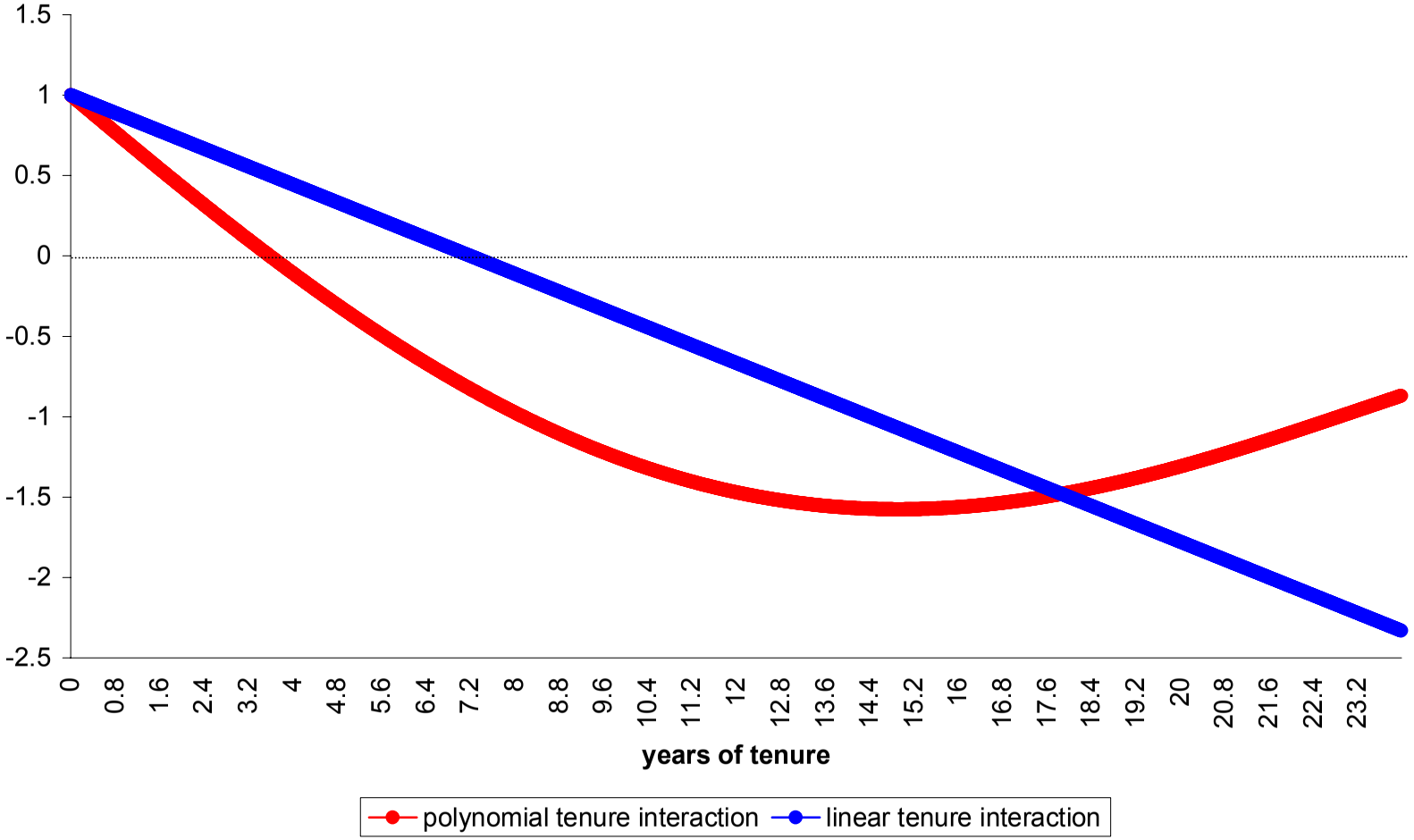

Figure 3: Derivative of probability of settlement with respect to the probability of settling when tenure equals zero

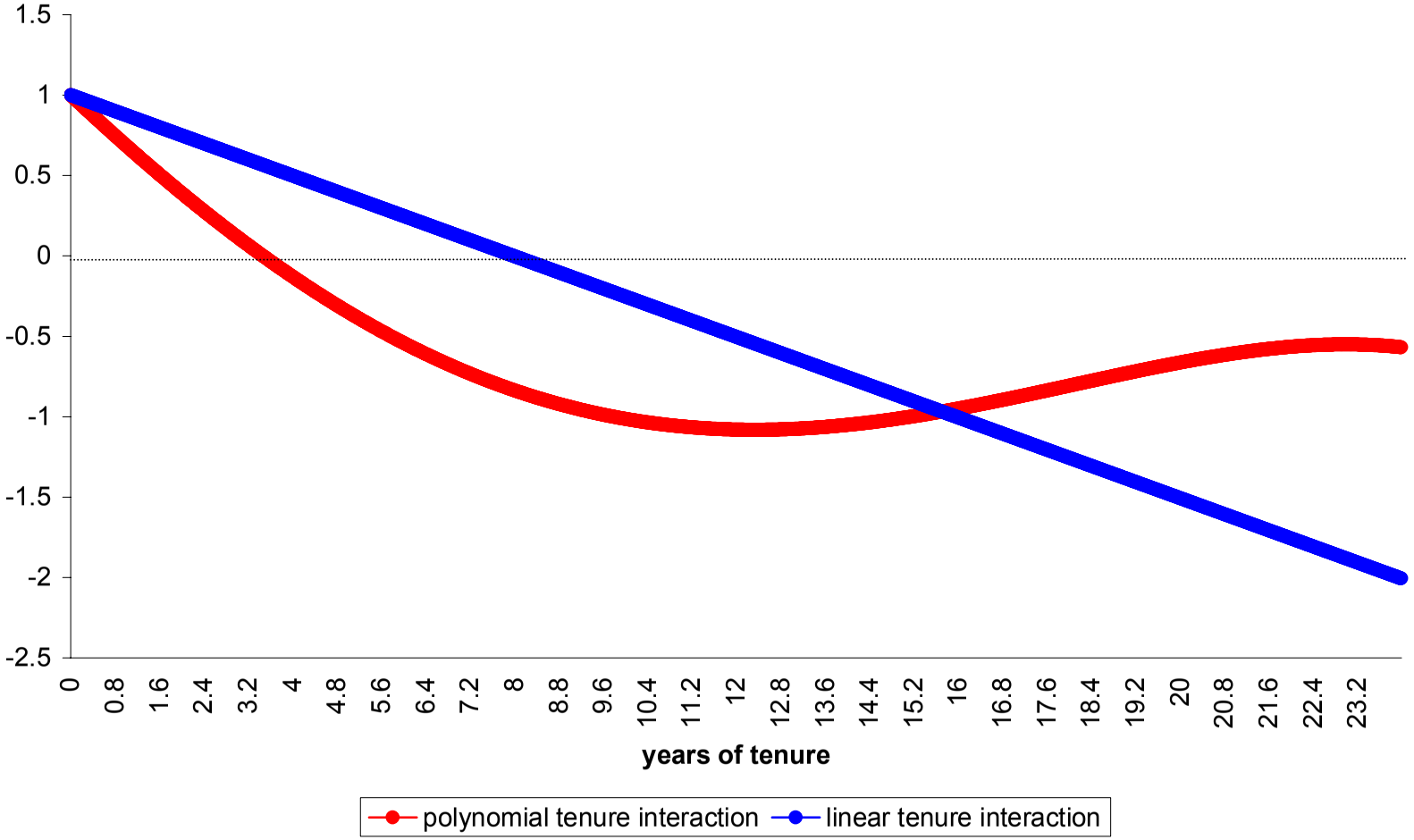

Table 1: Descriptive Statistics

All suits with private lawyers

	N	Mean	Std Dev	Min	Max
tenure	1,903	3.76	4.85	0	39.86
gender	1,903	0.32	0.47	0	1
final payment (2000 pesos)	1,903	15,989	74,574	0	1,683,751
case settles	1,903	0.50	0.50	0	1
case dropped	1,903	0.28	0.45	0	1
case goes to trial	1,903	0.22	0.41	0	1
positive award at trial uncollected	202	0.61	0.49	0	1

All suits with publicly-appointed lawyers

	N	Mean	Std Dev	Min	Max
tenure	665	3.12	4.86	0	47.08
gender	665	0.34	0.48	0	1
final payment (2000 pesos)	665	6,779	21,914	0	385,212
case settles	665	0.63	0.48	0	1
case dropped	665	0.26	0.44	0	1
case goes to trial	665	0.11	0.31	0	1
positive award at trial uncollected	45	0.56	0.50	0	1

Only publicly-appointed lawyers with at least one trial and at least one non-trial

	N	Mean	Std Dev	Min	Max
tenure	580	3.02	4.60	0	34.91
gender	580	0.35	0.48	0	1
final payment (2000 pesos)	580	6,751	22,972	0	385,212
case settles	580	0.63	0.48	0	1
case dropped	580	0.26	0.44	0	1
case goes to trial	580	0.11	0.31	0	1
positive award at trial uncollected	42	0.57	0.50	0	1

Table 2: Assignment of Cases to Lawyers
(F-statistics on joint significance of lawyer fixed effects)

	Dependent Variable	
	female	tenure
All suits with private lawyers: N=1903, F(987, 915)	1.677 ***	2.580 ***
All suits with public lawyers: N=665, F(48, 616)	1.255	3.214 ***
Public lawyers with at least one trial and at least one non-trial: N=580, F(18, 561)	1.141	1.157

Notes: The F-statistics correspond to tests of the joint significance of the lawyer fixed effects in models with no other independent variables. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level. See text for details.

Table 3: Lawyers and Modes of Termination
(Chi-bar-square statistics on joint significance of lawyer random effects)

	Dependent Variable		
	drop	settle	trial
All suits with private lawyers: N=1903, 988 lawyers	118.44 ***	174.69 ***	192.84 ***
All suits with public lawyers: N=665, 49 lawyers	11.79 ***	12.79 ***	1.69 *
Public lawyers with at least one trial and at least one non-trial: N=580, 19 lawyers	6.59 ***	5.74 ***	0.35

Notes: The chi-bar-square statistics correspond to tests of the joint significance of the lawyer random effects in random-effects logit models with no independent variables. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level. See text for details.

Table 4: Models Relating Settlement Rates to Trial Outcomes

	Recover Something at Trial	Firm Wins	Worker Wins	Mixed Ruling	Positive Award Not collected
tenure*(trial)	0.04 ***	-0.01	0.00	0.01	-0.03 ***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
tenure*(not trial)	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
female*(trial)	-0.09	0.00	0.10	-0.10	0.22 *
	(0.10)	(0.12)	(0.13)	(0.13)	(0.12)
female*(not trial)	0.10 **	0.10 **	0.10 **	0.10 **	0.10 **
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
trial	0.90 **	0.07	0.44	0.57	-0.59
	(0.37)	(0.27)	(0.40)	(0.37)	(0.42)
(lawyer's settlement fraction)*(trial)	-1.06 **	0.30	-0.11	-0.31	1.49 **
	(0.51)	(0.40)	(0.59)	(0.53)	(0.59)

Notes: Standard errors in parentheses. All models are estimated with non-linear least squares using 580 observations from 19 lawyers. For observations in which the outcome is not a trial, the dependent variable is a dummy equal to one if the case is settled, zero if the case is dropped. Standard errors are calculated allowing for heteroscedasticity and for the possibility that the outcomes in cases that have been grouped into the same proceeding may be correlated. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

Table 5: Models Predicting Dropped Cases

	case is dropped	case is dropped	case is dropped	case is dropped
female	-0.09 **	-0.09 **	-0.08 *	-0.06
	(0.04)	(0.04)	(0.04)	(0.06)
tenure	0.03 ***	0.07		
	(0.01)	(0.05)		
$tenure^2$		0.00		
		(0.01)		
$tenure^3$		0.000		
		(0.001)		
$tenure^4$		0.00000		
		(0.00002)		
tenure >= 3.58			0.63 ***	0.73 ***
			(0.15)	(0.24)
(tenure)*(lawyer's dropping fraction when tenure=0)	-0.14 ***	-0.29 **		
	(0.03)	(0.13)		
(tenure2)*(lawyer's dropping fraction when tenure=0)		0.00		
		(0.03)		
(tenure3)*(lawyer's dropping fraction when tenure=0)		0.001		
		(0.002)		
(tenure4)*(lawyer's dropping fraction when tenure=0)		-0.00002		
		(0.00004)		
(tenure >= 3.58)*(dropping fraction when tenure < 3.58))			-1.42 ***	-1.61 **
			(0.51)	(0.74)
tenure level when lawyers have same probability of dropping	7.20	3.58		
Only tenure in lowest (<.55) or highest quartiles (>=3.595)	No: N=580	No: N=580	No: N=580	Yes: N=289

Notes: Standard errors in parentheses. All models are estimated with non-linear least squares using 19 lawyers. The dependent variable is a dummy equal to one if the case is dropped, zero if the case is not dropped. Standard errors are calculated allowing for heteroscedasticity and for the possibility that the outcomes in cases that have been grouped into the same proceeding may be correlated. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

Table 6: Models Predicting Settlement

	case is settled	case is settled	case is settled	case is settled
female	0.08 *	0.07	0.07	0.04
	(0.04)	(0.04)	(0.04)	(0.06)
tenure	0.08 ***	0.21 **		
	(0.02)	(0.08)		
$tenure^2$		-0.01		
		(0.02)		
$tenure^3$		0.000		
		(0.001)		
$tenure^4$		0.00001		
		(0.00002)		
tenure >= 3.46			0.91 ***	1.14 ***
			(0.18)	(0.19)
(tenure)*(lawyer's settlement fraction when tenure=0)	-0.13 ***	-0.32 **		
	(0.03)	(0.13)		
($tenure^2$)*(lawyer's settlement fraction when tenure=0)		0.01		
		(0.03)		
($tenure^3$)*(lawyer's settlement fraction when tenure=0)		0.001		
		(0.002)		
($tenure^4$)*(lawyer's settlement fraction when tenure=0)		-0.00002		
		(0.00004)		
(tenure >= 3.46)*(settlement fraction when tenure < 3.46))			-0.46	-0.79 ***
			(0.28)	(0.30)
tenure level when lawyers have same probability of settling	7.97	3.46		
Only tenure in lowest (<.55) or highest quartiles (>=3.595)	No: N=580	No: N=580	No: N=580	Yes: N=289

Notes: Standard errors in parentheses. All models are estimated with non-linear least squares using 19 lawyers. The dependent variable is a dummy equal to one if the case is settled, zero if the case is not settled. Standard errors are calculated allowing for heteroscedasticity and for the possibility that the outcomes in cases that have been grouped into the same proceeding may be correlated. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

Table 7: Models Relating Settlement Rates to Trial Outcomes

	Recover Something at Trial	Firm Wins	Worker Wins	Mixed Ruling	Positive Award Not collected
tenure*(trial)	0.17 **	-0.04	0.24 ***	-0.17 **	-0.12 *
	(0.07)	(0.05)	(0.08)	(0.07)	(0.07)
tenure*(not trial)	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
female*(trial)	-0.11	0.01	0.07	-0.07	0.24 **
	(0.10)	(0.12)	(0.12)	(0.13)	(0.12)
female*(not trial)	0.10 **	0.10 **	0.09 **	0.10 **	0.10 **
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
trial	0.45	0.17	-0.54	1.06 ***	-0.20
	(0.42)	(0.29)	(0.44)	(0.38)	(0.52)
(lawyer's settlement fraction)*(trial)	-0.41	0.16	1.31 **	-1.03 **	0.92
	(0.59)	(0.42)	(0.60)	(0.52)	(0.75)
(lawyer's settlement fraction)*(trial)*tenure	-0.18 **	0.04	-0.34 ***	0.25 ***	0.13
	(0.09)	(0.07)	(0.11)	(0.09)	(0.09)

Notes: Standard errors in parentheses. All models are estimated with non-linear least squares using 580 observations from 19 lawyers. For observations in which the outcome is not a trial, the dependent variable is a dummy equal to one if the case is settled, zero if the case is dropped. Standard errors are calculated allowing for heteroscedasticity and for the possibility that the outcomes in cases that have been grouped into the same proceeding may be correlated. We use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.